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Finance and Enterprise Growth in Developing Countries:

Evidence From An Experiment in Uganda

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Finance and enterprise growth in developing countries: Evidence from an experiment in Uganda[*]

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Abstract

Microenterprises are a major contributor to income and employment in developing countries. There is growing evidence though that they do not expand beyond their intitial start-up point. I present the results of a randomized experiment with microenterprise owners in Uganda designed to explore the constraints to this growth. Business owners were randomly selected to receive loans, cash grants, business skills training, or a combination of these programs. I find that men with access to loans and training report significantly higher profits. The loan-only intervention had some initial impact, but this does not last. There are no impacts from the grant intervention, and no effects for women from any of the interventions. While recent research has found little effect from microfinance, I argue this is because men are not included in the studies.

The results from this experiment suggest that male owned businesses can expand from microfinance.

JEL codes: O12, O16, C93, J16, L26, M53

Key words: Economic development; microenterprises; microfinance; cash grants; entrepreneurship training; credit constraints

1 Introduction

Microenterprises are very common in countries with limited formal employment options as they provide informal employment and income for business owners. Recent work suggests that when people start microenterprises, there can be increased economic returns and security for the household (Blattman, Fiala and Martinez (2013) and Bandiera et al. (2012)). However, once a business has been started, there appear to be a number of challenges to growth. Research on business expansion shows only a small number of firms upgrade into larger businesses, leading to doubts that microenterprises can generate general economic growth (Berner, Gomez and Knorringa (2012), Fajnzylber, Maloney and Rojas (2006), Fajnzylber, Maloney and Rojas (2009) and Mead (1994)).

Microenterprise development therefore appears to be difficult. If there are capital constraints, greater access to capital may be a solution. Recent experimental work in microfinance, though, has failed to find increased profits for existing business (Banerjee et al. (2013), Fischer (2012), Augsburg et al. (2012), Gine and Mansuri (2011)). de Mel, McKenzie and Woodruff (2008) find a large effect on profits for cash grants to existing male businesses in Sri Lanka, though recent evidence suggests cash alone is difficult for businesses to use effectively (Fafchamps et al. (2013), Berge et al. (2012)). If capital is not always used optimally, perhaps this is because business owners lack the skills to use the funds well. However, the majority of studies on business skills training fail to find an effect on profits and sales from trainings (Cho et al. (2012), Karlan and Valdivia (2011), Bjorvatn and Tungodde (2012), Gine and Mansuri (2011), Karlan, Knight and Udry (2012)). Individual characteristics such as ability and patience, along with family pressure to spend on extended household needs, may also make optimal investment into the business difficult (Townsend (1994), Kocherlakota (1996), Jakiela and Ozier (2013) and Grimm, Hartwig and Lay (2013)).

I experimentally test some of these constraints to understand what kind of financial

and training services have impacts on enterprise growth, for whom, and why. Business owners from semi-urban locations across Uganda were randomly selected to receive loans, cash grants, loans paired with business skills training or cash grants paired with business skills training, or to be part of a control group. The sample was surveyed twice after the programs to determine the effect of these interventions on business and household outcomes. To the best of the author's knowledge, this is the first study to randomize offers of credit to individuals from a broad population and the first to directly compare cash grants and loans in an experimental setting.

Unemployment and lack of formal sector jobs are significant problems in Africa. Governments and international NGOs have therefore begun focusing on micro and small enterprise development to help spur job and wealth creation. The experiments described in this paper include a number of popular options employed by governments and NGOs to aid business expansion. The International Labour Organization (ILO) conducted the training tested here using their "Start Your Business" (SYB) curriculum, a materials-based training program employed worldwide for microbusiness owners who want to improve the management processes of their businesses. Cash grants were $200 and framed to business owners as unconditional. PRIDE Microfinance, a local microfinance organization in Uganda, delivered loans of $180 to $220 at a discounted annual interest rate of 20% (reduced from the normal 26%). The size of the grants and loans is equal to approximately 1.5 times the monthly profits of the average business. This is comparable in relative size of business to a loan program evaluated by Field et al. (2013) in India and one-half of the relative size of the businesses in a study of cash grants by de Mel et al. (2008) in Sri Lanka.

The sample is composed of 1,550 microenterprise owners who expressed interest in expanding their business and receiving trainings and loans. The businesses in this sample are thus directly comparable across interventions. The businesses come from a wide range of sectors, including hair salons, retail shops and tailors. The owners are well off by Uganda

standards: 77% report being literate and 49% report having accessed a loan at some time in the past. They are more likely to be women (61%) and young (64% of the business owners are between the ages of 24 and 35). 35% of respondents report having at least one employee, with average stock values of 2.4 million USH ($960) and profits of 318,000 USH ($127) in the last month. These businesses are thus representative of the types of microenterprises one would find across Uganda and Sub-Saharan Africa, though not necessarily representative of the owners of these businesses.

To test for changes over time and improve on statistical power, the survey team conducted multiple data collections on the businesses (de Mel, McKenzie and Woodruff (2009)). The results presented here represent two baseline surveys with the first follow-up, conducted six months after the intervention completion, and a second follow-up conducted three months later. I find that men with access to loans-with-training report 54% greater profits. This effect increases slightly over time and is driven by men with higher baseline profits and higher ability. Men have an initial impact from the loans without training, but this effect is gone by nine months. There is no effect from any grant intervention. A test for the differential effects of the programs strongly rejects equality.

There are no effects from any of the interventions during any data collection for female-owned enterprises. Family pressure on women appears to have significantly negative effects on business investment decisions: married women with family nearby perform worse than the control group in a number of the interventions. Women without family nearby, married or not, initially benefit, but these results are gone nine months after the programs ended.

A test of the mechanisms behind the growth in business profits for men suggests that the effect of the loans and training is likely due to a combination of increased family employment and capital accumulation, though capital is not found to be statistically significant, perhaps due to issues of power. An estimate of the returns to employment and capital suggests that employing a family member has large and significant implications for the profits of the

5

business. I check the sensitivity of these results to attrition, spillover and Hawthorne effects and find the results are likely not sensitive to these issues.

The results for men are consistent with credit constraints as the loans led to large increases in business profits. That the grants did not have an effect is consistent with a control constraints problem: knowing that the loan had to be repaid appears to have led them to use the money more effectively in the business. The results for women are more ambiguous and are possibly consistent with there being no credit constraints for women, that women are in a local equilibrium given the businesses they run or the other constraints they face, or that women simply do not make good business use of capital.

While the loans-with-training program had large effects on male-run businesses, this did not translate into observed impacts in the households. I find no changes to spending on child health, general savings or household consumption.

This paper contributes to a number of literatures. First is the effect of microfinance on existing businesses for enterprise and household outcomes. Field et al. (2013) find that a grace period for loan repayment for women leads to a positive and significant effect on profit, but the majority of work has found little or no effect of microfinance on business growth (Banerjee et al. (2013), Fischer (2012), Augsburg et al. (2012), Gine and Mansuri (2011)). There also appears to be no effects on household consumption growth (Crepon et al. (2011), Desai, Johnson and Tarozzi (2013), Angelucci, Karlan and Zinman (2013)). However, these studies have not been able to randomize loans directly but rely on either variation in the expansion of microfinance organizations, or look at marginally rejected clients, either through a cutoff score or randomization at the margin. In an attempt to better identify individual business effects, I randomize at the individual level from a population of business owners who expressed interest in expanding their businesses. More importantly, this literature also focuses almost exclusively on women owned enterprises.

The second literature is the role of cash grants on business growth. Haushofer and Shapiro

(2013) find small effects from giving cash grants to households on business income, while de Mel et al. (2008) find large returns, but only for male-led firms. New female business entrants have been found to make good use of cash grants (de Mel, McKenzie and Woodruff (2012) and Blattman et al. (2013)), the results for female existing-business owners are not positive. Fafchamps et al. (2013) find poor self-control leads to a lack of effect for women in Ghana. In this study, I find some small initial effects of cash grants on women who do not have family living nearby, but these disappear quickly. Similar to Berge, Bjorvatn and Tungodden (2012), I do not find results from cash grants for men. The cash does not appear to have been spent into the business for either sex, but is instead either spent on family obligations or other consumption.

A third literature focuses on the effect of human capital development on business outcomes. There is reason to believe business owners are missing a number of skills and leaving profits on the table. Bloom et al. (2013) test the effects of management services given to large Indian textile firms and find very large effects on firm outcomes. The majority of studies on micro and small businesses, though, find positive impacts of business skills trainings only on knowledge and attitudes, with little or no impact on profits and sales (Cho et al. (2012), Karlan and Valdivia (2011), Bjorvatn and Tungodde (2012), Gine and Mansuri (2011), Karlan and Valdivia (2011), Berge et al. (2012)). An exception is Calderon et al. (2013), who find large profit effects from an intensive training program in Mexico. The value of such trainings for existing microenterprises is thus in doubt. This study pairs training with capital. The initial short-term results confirm the findings of the literature. Over time, however, there appears to be a positive effect from the trainings for those who received loans. For male-run businesses, training helps to ensure the short-term results of the loan interventions last.

Finally, I contribute to the literature demonstrating how the characteristics of entrepreneurs predict business outcomes. Recent research on firm owner deaths suggests that entrepreneurs have a big effect on the success of their businesses (Becker and Hvide (2013)). I look at the

7

effect of individual ability and patience on business outcomes and find some important effects. The positive effects from the loan and training program are concentrated among high ability males, but I find no effect from an indicator of patience.

2 Experimental design

2.1 Sample selection

Enterprises in both the central and northern regions were selected from a census of businesses in the area. In total, the survey team surveyed 3,216 businesses in the central region and 1,421 businesses in the northern region to gather information on entrepreneur ability, size of business and demographics of the owners. The survey team was hired and trained by the researcher. The selection of the final businesses to be in the samples was based on the criteria of expressed desire to grow and interest in the ILO training and loan program. After the baseline, 2,383 business owners met these criteria. Due to contracting delays with the implementing organizations, I conducted a second baseline survey to determine if individuals were still interested in the programs, further reducing the sample to 1,550 individuals. I describe these individuals and how they are different from the full population in detail in section 4.2.

I randomly selected individuals into the five categories, presented in Figure 1. 406 were assigned to the loans intervention (220 in central and 187 in the north), 401 to the loans and training (215 in the central and 186 in the north), 167 to grants (all central), 219 to grants and training (all central), and 357 to the control group (170 in central and 186 in the north). The sample sizes were based on power calculations after taking into account implementation budget limitations. Stratification was done by region but not for any other characteristic.

Figure 2 presents the business types by frequency in this study for men and women. The majority of businesses were hair salons, followed by retail shops and tailors. The general type

of business follows the types of businesses present in the survey areas, as well as SubSaharan Africa in general. The distribution of businesses among both men and women is not equal, but follows similar paterns. Women and men are both present in the majority of business types.

2.2 Interventions

In the central region, I randomly divided individuals into six groups: (1) those who were offered a cash grant of $200, (2) those who were offered a loan of between $180 and $220, (3) those who were offered business skills training with a cash grant equal to $200, (4) those who were offered business skills training and a loan of between $180 and $220 and (5) a control group. For the northern region, individuals were randomly divided into three groups: (1) those who were offered a loan of between $180 and $220, (2) those who were offered business skills training with a loan of between $180 and $220 and (3) the control group. There was not sufficient budget to include the grant interventions in the north.

A local microfinance organization, PRIDE Microfinance, provided the loans. Unknown to the participants, the loans were guaranteed by the ILO. As the sample came from all businesses that expressed interest in a loan, these businesses may not have fit the lending requirements of PRIDE, and so a guarantee helped to mitigate risk. PRIDE normally provides loans with an interest rate of 26% and requires 100% collateral. Lenders reduced the interest rate to 20% and described the program as a special promotion to individuals. For those who were not able to provide 100% collateral, PRIDE agreed to accept 50% collateral instead. This special promotion encouraged participation in the loan program and reflected what a subsidized loan program might be like if conducted in the future. Individuals were then required to repay the loan in monthly installments, starting in the first month, per standard microfinance requirements. There were some delays in the roll-out of the loan program. Figure B.2 in the Online Appendix presents the distribution of loan density by

9

month. There are three main spikes in the middle of August, early September and middle of October[1].

The ILO delivered the cash grants through PRIDE bank accounts. The ILO then contacted individuals to attend information meetings explaining how the cash grant program would work. They were then asked to open a free savings account where the money would be deposited. This was done from the middle of October to early November. It is not possible as part of this design to separate the effect of savings accounts versus cash grants. As I do not find any effects from the grants, it is unlikely, but not impossible, that the savings accounts negatively effected individuals.

The ILO conducted the trainings In August and September using their Start and Improve Your Business (SIYB) training modules. This training program reached 4.5 million people in 100 countries from 2003 to 2010 (van Lieshout, Sievers and Aliyev (2012)). At least twice before researchers have evaluated the trainings experimentally. First, Mano et al. (2012) looked at the effect of giving training to 53 business owners. In keeping with other training results, they found survival rates increased, as did the incidence of good business practices such as keeping budgets, with no consistent effects on business profit. de Mel et al. (2008) also use the SIYB training on female business training and cash grants in Sri Lanka. They found no effect on profits for those already in business for training, but some initial effect for the grants that disappears after the second year. There is also increased entry for those without business and some income growth. The trainings have thus been evaluated previously and have presented mixed and unpromising results. It was decided not to pursue a pure training treatment arm but instead use trainings as a potential augmenting effect on the use of cash grants and loans.

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[1]A regression (not shown) on loan disbursement date and profits suggests that the date of loan delivery is not correlated with outcomes.

2.3 Regional differences

There is a significant difference across Uganda in terms of market structure, development and institutional quality. From 1986 to 2007, a 20-year civil war between the Government of Uganda and rebels left the north of the country highly underdeveloped. Incomes and wealth are significantly lower in the north than in any other part of the country (Ssewanyana (2010)), and market development is severely limited (Fiala (2010)). Since 2007, the north has been experiencing a large amount of growth, with expanded trade from both Sudan (north of Uganda) and the capital to the south helping to fuel this expansion. Increased income has also meant increased interest in market interactions, with the main trading town in the north, Gulu, experiencing significant growth.

In contrast, the central region has experienced 25 years of peace. Economic and market growth has been steady throughout this time period, with robust trade with Kenya, Uganda's main access to sea-ports. The businesses are also larger than in the north. From the baseline census, central business owners report having 19% more stock value than businesses in the north, though they report having accessed a comparable number of loans and similar profits in the last month. It is therefore possible that businesses in the north may be above (or below) an efficient scale. I take advantage of this difference in context to test the differential effect of business expansion programs in these areas through a pre-program stratification.

3 What might loans and kin networks mean for businesses?

Business owners face a number of constraints to expansion of their enterprises, including business productivity, individual ability, patience and family pressure to spend money outside the business. I discuss these constraints in the context of a simple self-employment model

from de Mel et al. (2008) with endogenous labor choice and imperfect credit markets. The full model is presented in Online Appendix C.

Consider an entrepreneur with capital K, labor l and ability θ. She currently owns a business with a production function $f(K, l, \theta)$ that faces the standard production assumption of diminishing marginal returns, i.e. $f_K > 0 > f_{KK}$ and $f_l > 0 > f_{ll}$. Labor and capital are also compliments such that $f_{Kl} > 0$. The entrepreneur maximizes $U(c, L - l)$, where c is consumption and $L - l$ is leisure time. This maximization problem is subject to a number of constraints, detailed in the appendix.

In this model, the business owner can affect equilibrium profits through increasing labor supply and capital stock. If the entrepreneur does not face family, gender or market constraints, according to the model she will invest into the business until the marginal rate of return on investment is equal to the market interest rate. If the market interest rate is very high, as is often the case in developing countries, then investment will still be optimal given the market conditions, though investment in the business will be relatively low. A shock to the capital stock K, either through a cash grant or a relaxation of borrowing constraints, would then be invested in the business until the marginal return to investment is zero. The rest would then be consumed.

Credit constraints exist to different extents and depend on the ability of individuals to access extant credit markets. In markets where interest rates are high, optimal investment may mean there is no expansion for most businesses. In cases where collateral conditions are very high, people may not have access to the assets necessary to obtain credit. In both cases, while credit is available, it is not easily obtainable, meaning there is low investment in business.

Family constraints have been discussed extensively in the literature (Townsend (1994), Kocherlakota (1996), Jakiela and Ozier (2013), Grimm et al. (2013)). When household and family needs are given preference over business investment, they lead to suboptimal invest-

ment with money not being spent on the business but instead on the needs of the household. Extended family can also put pressure on cash in some societies where communities expect that people not just support their immediate family but also siblings and cousins, making optimal investment in the business difficult.

Family can also help increase investment in a business by providing household assets, such as cash, tools and labor. Obtaining access to these can be difficult in some families, while in others they are considered natural to use in the business. In many societies this difference is determined by sex, with men utilizing household resources for business and women expected to contribute to the household over her business.

If a family constrained individual receives a shock to capital K, they will not invest the money into the business optimally. Instead, some or all of the windfall will be taken for immediate purchases or to fulfill family constraints. Investment will then be suboptimal and equilibrium returns will not be reached. If this money is constrained in some way, such as a conditional transfer or as a loan that must be repaid, this could lead to a forced commitment on the part of the business owner to invest in the business. Whether this commitment is enough to overcome the effects of family and patience will depend on the relative size of these pressures.

Finally, training may affect business owners by changing either ability, θ, or attitudes toward the business. Ability changes would lead to a better management of the business, meaning current investment into the business can be fully optimized. Changes in attitudes would be reflected in increased investment, though there may not be greater returns if the owner cannot take full benefit of the additional capital.

4 Data

Table 1 presents the timing and attrition rates of the different data collections. These include a main baseline survey (wave 1) conducted in February 2012, followed by a smaller follow-up baseline (wave 2) in May 2012 that collected only information on interest in the programs and business revenues, expenses and profits. The interventions were then given to individuals from August to October 2012. The first follow-up data collection (wave 3) was conducted in March 2013 and the second (wave 4) in June 2013, six and nine months after the interventions.

This section first details the characteristics of the businesses as measured in the main baseline survey of 1,550 business owners and tests the balance of characteristics for those selected into the different samples. I then discuss how these businesses differ from other businesses in the same areas. I end by presenting attrition analysis for each of the follow-up surveys.

4.1 Baseline data and balance tests

The summary statistics from the main baseline of the businesses and business owners that are included in the final sample are presented in Table 2, split between the male and female samples. The business owners interviewed are more likely to be female (61%) and predominantly range in age from 24 to 35. Most business owners are married (65% for men and 72% for women) and report being literate (87% men, 70% women). One fourth report having received some kind of business skills training in the past.

The majority of businesses (67% overall) report having at least one employee and keep written records of some kind (59%), though a significant number report only keeping the records "in their head" (32%). Average revenue in the last four weeks was higher for men than women: 807,000 USH (approximately $323) vs. 663,000 USH ($265), though this includes a

14

significant amount of variation, with some businesses reporting exceptionally high revenues. Last month profits for the businesses again significantly favored men, who averaged 388,000 USH ($155), while women had 260,000 USH ($104) and showed a much lower variation.

Business owners were also asked a number of basic intelligence and ability questions. In a number recall question, enumerators read off a list of eight numbers and asked owners to repeat the numbers back to them from memory. On average, the business owners were able to repeat four numbers back. Four math questions were also asked, though most business owners were able to respond correctly to them. I create an ability index by normalizing and summing the results from the number recall and math tests, along with years of education and literacy. This index is then normalized again.

Before asking the business owner whether he or she wanted loans and training, business owners were asked if they had ever taken loans (49% said yes). There is a large difference between the number of men that report having a loan (38%) and women (53%). This likely reflects the fact that microfinance is traditionally targeted towards women.

A range of assets questions were also asked with the intent of developing an asset index using principal component analysis. This is normalized at 0, and there is significant variation in the number of items people own, with men having greater assets than women.

The results of a balance test for treatment assignment is presented in the final columns of Table 2 and suggests that randomization worked well. In expectation, 10% of the variables should be significant at the 90% level or better. Of the 26 variables of interest collected during the baseline, only 1 is significant: the treated groups are more likely to have older individuals. This balance test is for any treatment selection. A balance test by treatment arm is presented in the Online Appendix. The interventions samples are also well balanced across most variables.

Selection into the sample is discussed in detail in the Online Appendix. Expressed interest in the loans and training programs from the full baseline sample is significantly associated

with a number of individual characteristics, most of them similar across the interventions. Younger people are more likely to be interested in the programs, as are those who are married and have had loans previously. Ability and assets are also correlated with interest in training. Baseline profits are negatively correlated with interest in loans or trainings, though the effect is small. These correlations suggest that there is some selection into the sample, though none of the coefficients are very large. A population average treatment effect is also presented in the Online Appendix and suggests the results are robust to observable population selection.

4.2   Sample and data attrition

As is common in the literature, while businesses expressed strong interest in the programs, take-up was not universal[2]. A full analysis of the take-up is presented in the Online Appendix.

Of those who were offered the loans, grants and training, actual take-up was 41%, 71% and 70% respectively. The rates for loans and trainings are similar to those found in the literature. Strikingly, grant take-up was not universal. Qualitative interviews presented in the Online Appendix suggest that many people simply did not believe the offer of the grants, thinking it too good to be true. Selection for take-up into the programs does not appear to be strong. The largest predictor of take-up for loans and grants is whether the person was offered and attended the trainings. Past experience with having attended trainings predicts whether a person attended the offered training.

The survey team made significant efforts to follow-up businesses during the endline data collections. As the business owners were very busy, the survey was kept short. Some business owners were also visited after business hours to ensure they had time to speak with an enumerator.

Of the 1,550 business owners we tracked for the first follow-up survey, we found 1,437

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[2]See Karlan, Morduch and Mullainathan (2010) and McKenzie and Woodruff (2012b) for discussions of take-up rates in microfiance and training studies, respectively.

(93%). Not all of the business owners we found were willing to tell us their profits or other information. As shown in Table 1, I thus have profit data on 87% of businesses. In the second follow-up (wave 4), this dropped to 86%. I have at least one follow-up data point for 1,468 businesses (95% of the sample). These rates are either comparable to or higher than a number of studies working with similar populations.

Table 3 presents the results of an attrition analysis on observable characteristics of individuals from the baseline survey to test for selection into attrition. The results suggest that some business and individual characteristics matter for attrition selection. Older people were more likely to be found at waves 3 and 4, as well as those with higher ability measures and more baseline employees. Selection is negatively associated with baseline assets. People in the grant-only, loan-only and grant-with-training interventions were easier to find than the control sample or loan-with-training sample. Additionally, there is significant selection into the districts. Buikwe, Jinja and Mukono (left out of the regression) are in the central region, while Gulu represents the northern region. People were much easier to find in Buikewe and Gulu than they were in Jinja and Mukono. Attrition in wave 4 is similar to wave 3, but baseline employees and assets are not statistically significant.

The results suggest that the characteristics of businesses that we are most interested in do not strongly predict attrition, though there are still a number of potential observable and unobservable characteristics of the attritters that might bias the results. To help minimize the effects of this selection, I conduct a bounding exercise in section 6.2 to test the strength of the results on different assumptions about the missing sample and find the results are robust to moderate assumptions about the attritted sample.

5 Results

To test the differential effects of each program, I run the following intention to treat (ITT) fixed effects regression model:

$$Y_{it} = \alpha + \beta T_{it} + \theta T_{it} * \gamma_t + \gamma_t + \eta_i + \varphi R + \mu W + \delta M + \epsilon_{it} \tag{1}$$

where t is time, i refers to an individual and Y_{it} is the outcome of interest. T_{it} is a matrix of dummy variables for which treatment an individual belonged to, γ_t are wave effects, and so $T_{it} * \gamma_t$ is the wave effects for each treatment. η_i are individual fixed effects, R is a matrix of region and sample dummies, W is a control for the time between surveys, M is the month of the data collection and ϵ_{it} is the error term. All standard errors are clustered at the individual level and are robust[3].

There are also a number of pre-specified heterogeneity analyses that I will conduct. These include the differential sex, family pressure, region, baseline profit levels, loan experience, patience, ability and risk measures. Due to the complexity of the interactions already employed, these are estimated by splitting the sample.

5.1 Business profit outcomes

The main variable of interest is the effect on the output of the business, specifically profits from the business. Business owners were asked what their total profits were in the last month[4]. The profits have been adjusted for inflation across all of the data collections, with the first baseline being the base period.

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[3]As the sample sizes in this study are relatively small, there could be concern about the parametric asymptotic assumptions for the standard errors. Bootstrapped standard errors obtain the same results in all specifications.

[4]The question was designed after the findings of McKenzie (2012), who show that directly asked profits are less biased than other profit measures.

Table 4 presents the results of estimating Equation 5 for the main outcome of interest, business profits. The equation is estimated in columns 1 and 2 for the full sample without and with wave 4 interactions, respectively. The results are not significant for any of the interventions, though there is a negative and significant effect of the grants for the fourth wave.

Columns 3 to 6 then split the sample into male and female samples with and without wave 4 interactions. For men, there is still no effect of the grants, but now there is a large positive and significant effect of the loans and loans with training programs of 260,000 USH ($104) and 249,000 USH ($100), respectively. The effects for the loans diminish somewhat by wave 4 but are still present, while the effects for loans and training increases slightly. Compared to the control sample average of 428,000 USH at wave 3, this represents an increase of between 50% and 60% in profits.

For women, the coefficients are small and none of the programs are significant in waves 3 or 4, except for the cash grant, which is significant and positive 229,000 USH for wave 3, but is of similar size, negative and significant in wave 4 at 282,000 USH. While there may have been an initial effect from the cash grants for women, this effect was gone by wave 4. A formal test of equality of treatments, presented in Appendix table D.1, confirms the differential effects in treatment and waves.

In addition to the ITT effects, Table D.2 in the Online Appendix looks at the local average treatment effects for the main sample and male and female subsamples. These results are estimated using an instrumental variables framework where actual up-take of the loans and grants are instrumented by whether and to which treatment group individuals were assigned. The results are consistent with the ITT estimates, though with larger effect sizes: compared to the control mean profits, the impact of the loans is 131% for men.

Overall, the results suggest that the programs did not have effects for the full population, though there are significant and substantial results for men. Men are seeing large increases in

profits for the loan-with-training treatment that are stable and possibly growing over time. Women experienced no effect from the programs on profits, with an increasing negative effect from the grants. A potential reason for this counter-intuitive effect will become more clear in the next section.

5.2 Family effects

The presence of family members can present a positive or negative force on businesses, especially those who may rely on family members for employment. Table 5 explores the effects of family proximity, which is used to proxy for family presence, on the profits of the business. Columns 1 and 2 divide the full sample by whether the majority of the family lives far away (outside the district) or nearby (in the same district). For those with family far away, there is an initial increase in profits from the loans, but this disappears by wave 4. The grant-only program is negative for close and far family for wave 4, though this is partially offset by positive but insignificant effects in wave 3.

Columns 3 to 6 divide the sample by men and women with close and far family. Men with family far away do not benefit from the programs, though the loan-with-training program is marginally insignificant, large and positive for wave 4. From column 4, it appears that the large positive effects of the loan programs come from men with family nearby. By wave 4, the effects are reduced but not substantially. These results are consistent with those found in the main specification in Table 4 and suggest there is no consistent effect of family for men.

The effects of the program on women with family far away are also consistent with the main results for women. There is an initial increase in profits for women for all of the interventions, but these effects disappear by wave 4. The effects of the programs on women with family nearby present a bleaker picture of the programs. The effects of the loan, loan-with-training and grant-with-training programs are all large, negative and significant. These

negative effects appear to stay with wave 4.

To explore the role of husbands in this effect, columns 7 and 8 further divide the women with close family sample into those who are married and unmarried, respectively. The sample sizes, and thus power, are significantly reduced. This is also an exploratory analysis as it was not specified before data collection was conducted. The results though are quite striking: the negative effects from family appear to only be present for married women. This could be due to either increased demands on cash from the husband, or from the husbands family. Dividing the far family sample for those married and unmarried does not change any of the results and so is omitted.

Overall, the results suggest that men benefit from having family nearby while the businesses of married women are strongly hurt by them. This is consistent with the model of family pressure presented earlier. Men are able to capture household labor to use in the business. Women without family nearby initially perform well but, over time, do no better in any of the programs than they do in the control group.

5.3   Additional heterogeneous effects

In addition to the role of family in business outcomes, a number of heterogeneity tests were developed as part of the research design, based in part on the model discussed in Section 3 and the existing literature on business development. Table 6 presents the results of splitting the sample of male enterprises to identify different heterogeneities in the population. Female results are presented in the Online Appendix, but are not significant for any specification, following the non-results found in Table 4.

Columns 1 and 2 present the results for the central and northern samples, respectively. As there was no grant program in the north, it is only possible to compare the results of the loan interventions between the two regions. Men in the central region show an initial increase in profits of 436,000 USH from the loan-only program. This is comparable to the

control sample mean, which suggests a large impact from the loans. The wave 4 interaction is negative and decreases this effect by roughly 44%, but the overall effect is still significantly different from 0. The effects from Table 4 column 3 for the loan-with-training program are not significant in the split regional sample, though they are of approximately the same size. The interaction with wave 4 for the central sample is large and positive, though still not significant, while it is small and negative for the north. Taken together, this suggests that the lack of significance is due to reduced power from splitting the sample. Thus, there is likely no regional difference by wave 3 for men with loans and training, though men in the central region who received training with the loans are doing significantly better, again close to the control mean.

These results suggest that there are some differences between the two regions for outcomes. The effects of the loan-only intervention appear to only be in the central region, and these are large. Men in the north benefit equally from the loans with training as those in the central region, though the effect increases over time in the central region, but not in the north. Overall, businesses in the central region appear to be doing better than those in the north.

Figure 3 presents a CDF plot of the business profits for the loan and loan-with-training interventions for men. As can be seen from the plot, the results from the interventions are being driven by higher profit businesses. This is confirmed in Table 6, columns 3 and 4, which presents the results of splitting the male sample by high and low baseline profits. Low profit is defined as those with baseline profits below the baseline mean of profits, while high is those above the mean. The results suggest that the positive effects of the loans are coming from the top businesses. These effects are larger than the results from the non-split samples in absolute size at 315,000 to 369,000 USH, though relative to the control mean they are consistent with the previous results of an effect size of about 58%. The interaction with wave 4 is not significant, though the signs are negative and reduce the effects by approximately

half. A joint test of the difference between the loan and the loan-with-training interventions suggests there is no differential effect of the training. While not significant, there is also a large, positive effect for low profit men from the loan-with-training program by wave 4, suggesting that low income men may be benefiting from the program but take longer to do so.

As can be seen in the summary statistics in Table 2, 38% of the men and 53% of the women in this sample had taken a loan previously. This suggests that, at least for some of this population, credit is not necessarily a constraint. In columns 5 and 6, I split the male sample into those who have taken a loan previously and those who have not to explore if credit history impacts these results. The impacts of the loans do not hold for those who have taken loans previously, and are instead only present in those who have never had a loan. The reasons for not having taken a loan in the past could be many, so interpretation of these results must be careful. If not having a loan previously is a proxy for a credit constraint, the results suggest that the more constrained were the ones that benefited from the loans. If instead having had a previous loan is a proxy for over-indebtedness, the results suggest that debt is a problem for realizing returns to loans.

To test the effects of individual characteristics - specifically baseline ability, risk and patience levels - on profit outcomes of businesses, Table 6 presents the results of splitting the samples into those with high and low measures of ability, risk and patience, divided for men and women. Columns 7 and 8 present the results for patience, columns 9 and 10 present ability and columns 11 and 12 present risk.

There appears to be no difference in effects based on patience, though the main results for loans-with-training appear to be concentrated with men in the highest half of ability. High-ability males appear to perform better in general as the control mean profits are 25% higher. There appears to be some effect for high-risk individuals, though the effects of the training programs are significantly higher for the low risk individuals.

5.4   Treatment effects on sales

Table 7 explores the effects of the programs on different sales (revenue) outcomes for men and women, including the last month sales, sales in a normal month, sales in a good month and sales in a bad month. The results are largely insignificant, though the few outliers are consistent with the main results found thus far. In wave 4, women report lower sales in the last month for the grant program. Men in the loan and training program initially report better sales in a good month, though this is gone by wave 4. Women initially report better sales in a bad month for the grant program, but this is also gone by wave 4.

There is also likely an issue of power in these results. A regression without wave effects (not shown) shows a positive and significant effect on last months sales for men in the loan-with-training program. The results in column 1 of Table 7 are not significant for either wave 3 or 4, but there are large and positive effects for wave 3. These results suggest there are some changes in sales from the programs, but this change appears to be very noisy and not well identified.

5.5   Treatment effects on employees and capital

In order to understand the channels the effects of the programs are having on men and women, Table 8 looks at the effects of the programs for men and women on the number of employees and working capital.

There are some important and large effects for the number of employees. Men with loans and training initially hired more employees. Men hire 0.68 more employees, a 234% increase over the control group. This initial effect comes from both employees hired outside the household (45% increase), and from inside the household (66%), as can be seen in columns 1, 3 and 5, respectively. By wave 4, the number of total employees has returned to the control group level. This is because the number of outside employees has actually decreased overall,

24

replaced instead with family employees. Family employees are not normally paid, or if they are paid, they are given a rate significantly below their marginal productivity. Consistent with the previous results of the significant effects of family proximity, this suggests that a major part of the effect of the loan program is to increase men's demand on household member time for working in the business, whether paid or unpaid.

For women, there appears to have been an initial decrease in hired employees, though this effect is gone by wave 4. Consistent with a model of household control, women do not demand more labor from the family.

Columns 7 and 8 explore the effects of the interventions on capital stock. The coefficients are large and economically significant, but they are not statistically significant. This may be a result of the size of the interventions relative to existing stock. Control men report almost 5 million USH in capital stock. The interventions were on average 500,000 USH, only 10% of existing stock. While this is comparable in size to other work (e.g. Field et al. (2013)), it has important implications for power. It is thus not possible to definitively say whether men used the money from any of the programs on capital investment.

5.6 Household outcomes

As there is an increase in income and household labor for men and the programs had little or negative effects for women's income, it is possible there are important consequences for general household outcomes. Columns 1 and 2 of Table 9 explore whether any child missed school in the last month. There appears to be some effect from the grant programs for men and women to reduce children missing school in wave 4, though these do not cancel out the (insignificant) increase in missing school from wave 3.

From columns 3 and 4 we see no sustained changes in spending on child health for men, with a significant decrease for women who were part of the loan-only program.

It is common for people in Sub-Saharan Africa to engage in multiple activities, not just

one main business, in order to diversify risk. Columns 5 and 6 look at the number of other businesses for men and women outside the main business. There are no statistically significant effects from the program, suggesting that people did not spend the money to start new businesses but instead focused on the ones they already had.

Household savings and consumption for men (columns 7 and 9) are not affected by the programs, while there are large negative changes in household consumption for women in column 10. Initially, consumption drops by approximately a third. By wave 4 this has returned to control levels. There are also no effects on savings rates for women.

The results for household outcomes do not present a promising picture of the programs for either men or women. Six months after the programs there is an increase in missed school for household children, consistent with men demanding family support. The effects on women's short-run household consumption suggest that women may have initially shifted consumption to replace money lost from increased demands on money from the family. Both the effects on missed school and household consumption have disappeared by the 9 month follow-up. Children are no longer being pulled out of school to work in the business.

While the loan programs increased profits for men, there are no changes in savings or houshold consumption. This is consistent with the increase in investment shown previously, as well as consistent with the literature suggesting women, not men, invest income into the household.

5.7 Returns to employment and capital

The results thus far suggest that the channel of effects of the loans for men is happening mostly through changes in employment, specifically family employment, though the evidence is also suggestive of some very noisy or weak capital accumulation, or possibly changes in productivity. To test for the returns to increased employment and capital, I run the following regression with men only:

$$Y_{it} = \alpha + \beta Employees_{it} + \theta Capital_{it} + \epsilon_{it} \qquad (2)$$

where either employees or employees and capital are instrumented by the treatments. This follows the linear specifications prefered by de Mel et al. (2008) and Field et al. (2013). The results are presented in Table 10 for family employees and hired employees. For each specification, I report the employment effect instrumented by treatment, include capital as a control, or include capital as an instrumented variable. A weak identification test suggests the IV results for capital and labor are weakly identified, though consistent across instrument specifications.

The estimates for family employees in columns 1 to 3 are about 650,000 USH per family employee. The effect in column 2 for family employees, with a control for capital, is the same size while capital is very significant. Columns 4 to 6 report the same specifications for hired employees. The size of the returns to employment and capital both decrease substantially.

These results suggest substantially large effects for employees, especially family employees. This is consistent with the main results and suggests that a significant amount of the effects from the loan programs are going through family employees, who may be either more productive than hired employees, or are un- or under-paid. The returns to capital, while poorly identified due to limited effects from the first stage reported in Table 8, range from 3.4% to 6.0%. This is consistent with other work on existing businesses[5].

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[5]de Mel et al. (2008) find a return of 5.5% in Sri Lanka, Dupas and Robinson (2013) 5.9% in Kenya, and Udry and Anagol (2006) 4% in Ghana. There are also some larger estimates from McKenzie and Woodruff (2012a) of 20-30% in Mexico, and Field et al. (2013) of 13% in India.

6   Extensions and robustness checks

6.1   Spillovers

Following de Mel et al. (2008), I explore the effects of being in the proximity of other treated businesses of the same business type. I run the following spillovers regression:

$$Y_{it} = \alpha + \nu N_{it}^d + \vartheta N_{it}^d * P_t + \beta T_{it} + \theta T_{it} * \gamma_t + \gamma_t + \eta_i + \varphi R + \mu W + \delta M + \epsilon_{it} \qquad (3)$$

where N_{it}^d is a count of the firms in any treatment within the same district and industry as firm i at time t and P_t is the population density of the district. I use same district as this is the only reliable distance measure I have, though this is a very large assumption that will likely overestimate the impact of the spillovers. The results are presented in Table 11. Columns 1 to 4 are without the interaction with population density and columns 5 and 8 are with the interaction. Columns 2 and 6 are for female-owned businesses, while columns 1, 3, 4, 5, 7 and 8 are only for men and split the sample into only-treated or only-control businesses.

Since the interventions affected men predominately, it is not surprising that the results are only significant for male-run businesses. Male-run businesses with treated similar firms in their district have between 2,300 and 2,800 USH lower profits per treated firm by wave 4. This is significant for all male specifications. Within the sample, the average business has 20 treated similar firms in their district, which means a decrease in monthly profits of about 60,000 USH. This is not a large amount relative to the effect size of the loans, but it is quite large for control businesses and those who had no effect from the programs.

Once population density is included as an interaction though, the effects of having treated firms nearby are no longer negative or significant, which suggests that taking into account

potential demand can have significant effects on spillover estimates.

This analysis is likely to be biased and so needs to be taken with caution. As GPS data was not collected, a rough estimate of distance is used. Districts, while the main area of trade, are quite large and may overestimate the number of competitors. This analysis also inexactly controls for demand.

6.2 Bounding

While attrition rates are relatively low, there may still be some biases present from selection into attrition. Table 12 presents a bounding exercise similar to that conducted by Karlan and Valdivia (2011), who use a range of assumptions for bounding originally from Horowitz and Manski (2000), Lee (2002) and Kling and Liebman (2004).

New lower-effect bounds are created by imputing the outcomes for the missing male businesses based on decreasing the assumptions of treatment outcomes. Outcome means are imputed for the missing treated population, minus a predetermined standard deviation of the non-attrited sample in the treated population. The process is then repeated for the attrited control sample, but this time adding a pre-defined standard deviation from the found treated sample. This process then creates a range of outcomes that test how sensitive the results are to the condition of the attrited sample.

The results of the bounding test suggest that the main outcomes obtained earlier for men are robust for assumptions up to 0.5 standard deviations. After this, the significance levels disappear and the signs switch to negative returns. The results are thus not sensitive to low-level assumptions about the missing population, but are sensitive if there is attrition among control firms that have expanded and treatment firms that have contracted.

6.3 Hawthorne effects

When individuals are aware they are part of a study, they may change their behaviors, consciously or subconsciously, to please the research team. This could lead to either overestimation of effects if respondents in the treatment group overestimate outcomes to appear to be performing very well, or under estimation if treatment participants want to appear needy of additional programts. These concerns are commonly called Hawthorne effects, and any study relying on self-reported outcomes could face such problems. Research projects that deliver cash grants or trainings or assist with pairing individuals to microfinance institutions could be especially subject to such problems. There are a though reasons to believe that this project does not suffer from Hawthorne effects, or if there are any, they are likely small.

The survey team was kept separate from the implementation teams. Enumerators wore uniforms that identified them as belonging to a German research organization (DIW Berlin) and were unaware of the interventions offered to people, both at baseline and endline surveys. Enumerator effects could though still be present. During data collection, it is possible that the comfort of respondents is determined by the quality of the enumerator that is interviewing. When controling for enumerator effects (results in Online Appendix), the main results hold and increase slightly, most likely through a decrease in noise. There thus may be some effect of the enumerators on reported profits, though this effect does not change the overall results, and suggests that if there is a problem, it is one of underreporting. While there may be a chance that people are systematically misreporting, this misreporting would have to be very sophisticated.

7 Discussion

The question of what restricts businesseses from expanding has been a pressing problem for researchers and policy makers. This experiment presents some evidence on why business

owners fail to invest and expand, and which are more likely to utilize capital and trainings.

The results are consistent with commitment and skills problems for men: men who received the loan-with-training intervention perform significantly better than the control group or those who received cash grants or loans without training. These results are driven partially through increases in employment, with an initial impact of child time in school, along with increases in capital stock and efficiency. The increase in profits are quite large and suggest that there are substantial returns to increasing family employment and capital. The results are driven by men who have not had loans previously, have higher baseline profits, higher baseline ability and are strongest in the central region.

The results for women are significantly more pessimistic. None of the interventions helped the full sample of women in the short-run, and all appear to have led to a decrease in profits over time. This counter-intuitive result is due to family presence. Family pressure in developing countries has long been a problem for women. Keeping cash in hand is difficult when there is pressure to spend money on school fees, health care and funerals. The evidence presented here suggests that these pressures matter a lot for women who want to expand their business but have family members nearby. Men often do not face the same pressures, and, in fact, benefit from having family near to use as labor. The lack of results for women is due in part to loan repayment difficulty. Excluding those behind on the loans leads to a positive and sustained effect from the loan-with-training program for women with family who are outside of their district.

Counter to previous evidence on microfinance, loans have a dramatic and positive effect here, at least for men. Why might these results be so different than what has been found in the literature thus far? The most likely reason is the selection of businesses in this sample. These are business owners who have expressed an interest in growing their businesses further. Most have had loans in the past but are clearly looking for additional credit to expand their businesses. In addition, most studies have focused on women, who are the main group that

microfinance organizations prefer to target[6]. This study includes men, and in fact finds that only men benefit from microfinance though, consistent with other studies, men do not spend the profits on the household.

Finally, the results presented here suggest a mixed outcome from training. There is not a differential effect of training for most of the interventions, except for men who received loans. The effect of the training appears to have been to increase overall employment in the short-run, with a substitution toward family employees over time. This result, combined with the small but growing literature on business skills training, suggests that training needs to be paired with a committed capital infusion in order to be effective.

This research adds to the growing evidence on the experimental returns to capital and training for microenterprise development. The results of this literature are not encouraging. After numerous experiments, only this paper and Field et al. (2013) find effects for business returns from microfinance. I only find these effects for men. The evidence in the literature for cash grants is more positive, again though only for men. I find no effect here from a cash transfer. There is also no effect in other research of training existing entrepreneurs, for men or women. I find some evidence here for a positive effect of training when combined with a loan. While more research is needed to understand the constraints to business growth, current evidence suggests that female microenterprises do not grow from small interventions like the one described here, but instead stay at a local equilibrium. For men, there appears to be significant potential for growth.

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[6]Cull et al. (2007) find that over 65% of microfinance clients are women in a survey of large microfinance organizations. The ILO and UN have also put the number of women closer to 80% of all clients worldwide.

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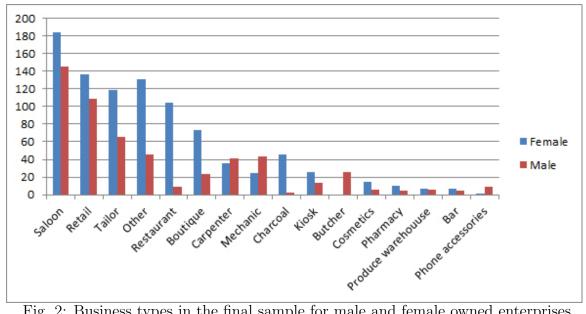Fig. 1: Experimental design with sample sizes

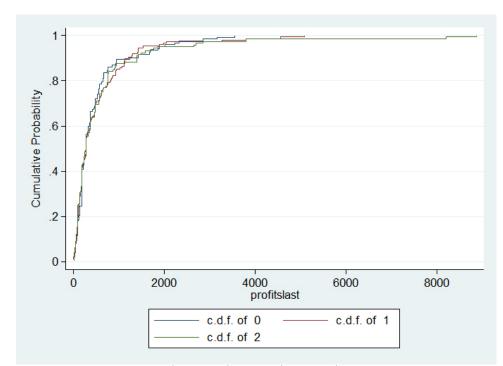Fig. 2: Business types in the final sample for male and female owned enterprises

Fig. 3: CDF plot of the control (group 0), loan (group 1) and loan-with-training (group 2) for the male sample.

Table 1: Timing of data collections and program implementation

Survey	Dates	Attrition rate
Baseline 1	Feb to March 2012	-
Baseline 2	May 2012	8%
Interventions	Aug to Oct 2012	-
Endline 1	March to April 2013	13%
Endline 2	June to July 2013	14%
Endline 1 or Endline 2		5%

Notes: The attrition rate for the second baseline is the percentage of business owners the survey team was not able to find from the first baseline. The attrition rates for the endline data collections refer to the percentage of business owners the survey team was unable to find from the final sample of 1,550 businesses selected from the second baseline. The final attrition rate is the number of business owners in the main sample without answering any endline surveys.

Table 2: Summary statistics and balance tests

Baseline Characteristic	Male sample			Female Sample			Means by Treatment Group: Full Sample		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Control	Treated	p-value
Female	604	0.00	0.00	942	1.00	0.00	0.630	0.595	0.25
Age 18-23	604	0.18	0.39	942	0.08	0.27	0.140	0.117	0.25
Age 24-29	604	0.37	0.48	942	0.32	0.47	0.350	0.366	0.58
Age 30-35	604	0.26	0.44	942	0.32	0.47	0.310	0.305	0.87
Age 36-41	604	0.10	0.30	942	0.16	0.37	0.150	0.127	0.26
Age 41-50	604	0.09	0.28	942	0.12	0.33	0.060	0.095	0.06
Married	604	0.65	0.48	942	0.72	0.45	0.650	0.638	0.68
Literate	604	0.87	0.33	942	0.70	0.46	0.810	0.807	0.90
Previous training	604	0.26	0.44	942	0.25	0.43	0.260	0.254	0.83
Number of employees	604	0.90	1.51	942	0.52	1.20	0.340	0.369	0.51
Employees hours worked	417	55.69	94.50	606	34.39	60.93	0.630	0.700	0.39
Does not keep records	601	0.04	0.20	937	0.07	0.25	43.200	50.150	0.21
Keeps records on computer	601	0.04	0.20	937	0.02	0.13	0.009	0.009	0.99
Keeps written records	601	0.67	0.47	937	0.55	0.50	0.025	0.037	0.22
Keeps record in head	601	0.24	0.43	937	0.35	0.48	0.600	0.605	0.86
Keeps money in separate bags	601	0.00	0.00	937	0.01	0.09	0.380	0.357	0.40
Last month's revenue (1000 USh)	604	807.72	774.11	942	662.94	643.75	715.100	663.600	0.23
Average months revenue (1000 USh)	593	1126.62	2112.66	932	1087.13	7257.18	759.300	1067.400	0.39
Last month's profit (1000 USh)	604	387.66	1032.37	942	259.89	533.24	341.900	320.000	0.64
Average month's profit (1000 USh)	583	543.91	2391.52	907	297.43	469.87	600.300	450.000	0.12
Stock value (1000 USh)	568	3662.82	10811.38	879	1519.77	3171.81	3336.600	2858.800	0.30
Value of liabilities (1000 USh)	437	252.07	936.50	680	136.29	534.77	145.400	179.500	0.52
Longest string of numbers recalled	604	4.59	2.20	942	3.83	1.98	3.800	3.790	0.94
Math questions answered correctly	604	3.65	0.52	942	3.47	0.61	3.540	3.558	0.61
Ability Index	604	0.29	0.88	942	-0.17	1.02	-0.005	0.009	0.82
Had a loan previously	599	0.38	0.49	934	0.53	0.50	0.440	0.478	0.21
Asset index	604	0.29	1.80	942	-0.16	1.45	-0.150	-0.061	0.37

Notes: Robust p-values from an OLS regression with baseline characteristic as the dependent and treatment status as the independent variable are reported in the final column. * denotes significance at the 10% level, ** at 5% and *** at 1%.

Table 3: Attrition analysis for follow-up surveys

	(1)	(2)	(3)
	Wave 3	Wave 4	Waves 3 and 4
Loan	0.065**	0.059**	0.059**
	(0.03)	(0.03)	(0.03)
Loan and	0.012	0.030	0.031
Training	(0.03)	(0.03)	(0.03)
Grant	0.071**	0.071*	0.075**
	(0.04)	(0.04)	(0.04)
Grant and	0.049	0.092***	0.092***
Training	(0.03)	(0.03)	(0.03)
Female	0.00089	0.0075	0.0064
	(0.02)	(0.02)	(0.02)
Age	0.037***	0.027**	0.027**
	(0.01)	(0.01)	(0.01)
Married	-0.0038	0.048**	0.050**
	(0.02)	(0.02)	(0.02)
Ability	0.022**	0.022**	0.023**
	(0.01)	(0.01)	(0.01)
Total	0.015***	0.0069	0.0082
Employees	(0.01)	(0.01)	(0.01)
Assets	-0.014**	-0.0043	-0.0039
	(0.01)	(0.01)	(0.01)
N	1550	1550	1550
R^2	0.03	0.02	0.02

Notes: Columns (1) to (3) report the results of an OLS regression on whether the business was found in wave 3, wave 4 or in waves 3 or 4, respectively. Robust standard errors are in parentheses below the coefficients. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Main treatment effects on business profits

	(1) Male and Female	(2) Male and Female	(3) Male	(4) Male	(5) Female	(6) Female
Loan	44.1	77.0	176.9*	260.6**	-36.1	-40.9
	(48.09)	(56.97)	(95.19)	(113.53)	(52.90)	(61.85)
Loan and	82.0	79.2	289.5*	249.4**	-39.0	-13.2
Training	(65.28)	(58.03)	(147.97)	(118.62)	(53.90)	(60.34)
Grant	41.4	139.0	-11.9	33.8	87.9	229.0**
	(66.95)	(89.40)	(108.12)	(129.96)	(82.91)	(116.20)
Grant and	-49.4	-86.8	-79.9	-184.1	-20.6	-6.99
Training	(78.85)	(110.27)	(171.12)	(238.04)	(63.28)	(83.42)
Loan * W4		-66.7		-170.5		10.4
		(58.53)		(116.10)		(62.23)
Loan and		4.95		80.7		-52.2
Training * W4		(74.92)		(170.07)		(65.02)
Grant * W4		-196.4**		-89.1		-282.4***
		(82.20)		(139.26)		(99.23)
Grant and		69.3		199.2		-27.8
Training * W4		(95.39)		(202.33)		(79.57)
Control Mean	371.9	371.9	428.2	428.2	342.9	342.9
R^2	0.0084	0.011	0.018	0.022	0.016	0.021
N	5345	5345	2069	2069	3261	3261

Notes: Columns (1) to (8) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. The results are for a trimmed sample where the top and bottom 0.5% outcomes are dropped as are the baseline values for individuals never found. Columns 1 and 2 are the full sample. Columns 3 and 4 are for men only and columns 5 and 6 are for women only. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Treatment effects on business profits by family proximity

	All		Male		Female		Female with close Family	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Far Family	Close Family	Far Family	Close Family	Far Family	Close Family	Married	Unmarried
Loan	148.5**	14.3	71.9	364.5**	143.9**	-191.8*	-261.9**	91.8
	(71.47)	(86.00)	(161.78)	(160.07)	(65.40)	(100.60)	(125.11)	(112.81)
Loan and	104.9	56.5	72.2	350.2**	112.8	-109.8	-155.5*	103.0
Training	(91.57)	(72.43)	(181.57)	(151.64)	(102.53)	(68.04)	(80.54)	(100.18)
Grant	154.8	152.1	-136.8	179.4	347.3**	128.1	223.1	30.0
	(106.66)	(152.62)	(166.90)	(185.33)	(139.47)	(199.17)	(282.86)	(115.46)
Grant and	-100.4	-18.4	-540.2	220.3	152.5	-177.9**	-205.8*	12.9
Training	(165.94)	(98.93)	(370.89)	(200.91)	(118.77)	(86.25)	(106.74)	(129.15)
Loan * W4	-125.8	-12.9	-32.9	-277.5*	-128.9*	138.9	138.2	152.4
	(82.27)	(82.67)	(202.54)	(153.62)	(72.70)	(92.83)	(108.75)	(158.74)
Loan and	50.8	-40.3	271.9	-69.4	-69.1	-42.2	-29.9	-92.3
Training * W4	(122.11)	(88.70)	(307.01)	(183.59)	(94.37)	(85.32)	(103.54)	(113.24)
Grant * W4	-204.0*	-209.7*	44.2	-258.9	-365.9***	-191.6	-273.6	-8.66
	(109.32)	(122.89)	(205.25)	(226.07)	(132.22)	(141.97)	(197.54)	(111.88)
Grant and	-13.9	176.4	159.5	286.4	-114.8	80.2	110.9	8.34
Training * W4	(136.52)	(118.88)	(317.55)	(225.17)	(102.43)	(116.30)	(159.90)	(135.04)
Control Mean	424.8	430.3	424.8	430.3	232.4	417.8	450.9	265.9
R^2	0.015	0.016	0.040	0.035	0.025	0.030	0.034	0.10
N	2688	2657	1025	1044	1652	1609	1214	395

Notes: Columns (1) to (8) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. The results are divided by the proximity of family. Individuals that report the majority of their family living within the same district are coded as having close family. All others are coded as far family. Columns 7 and 8 are further divided for women with close family by whether they are married or not. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Heterogeneity effects for men on profit

	(1) Central Region	(2) Northern Region	(3) Low Profit	(4) High Profit	(5) Previous Loans	(6) No Previous Loans	(7) Low Patience	(8) High Patience	(9) Low Ability	(10) High Ability	(11) High Risk	(12) Low Risk
Loan	436.1** (193.59)	73.6 (98.78)	181.2 (137.53)	315.3* (168.36)	-62.6 (147.33)	398.7*** (151.88)	294.4** (121.85)	217.0 (199.50)	71.8 (105.66)	371.4** (185.89)	117.0 (180.37)	405.7*** (127.98)
Loan and Training	244.9 (199.03)	251.7** (112.80)	86.2 (163.75)	368.7** (168.12)	190.9 (165.56)	234.7 (156.64)	203.8* (111.90)	312.6 (216.38)	0.41 (110.45)	398.4** (194.59)	136.7 (178.07)	453.7** (194.58)
Grant	191.4 (181.76)		-170.9 (123.43)	196.6 (219.07)	-58.2 (184.63)	40.6 (166.21)	45.6 (86.73)	26.7 (291.64)	-238.7 (153.48)	226.4 (208.04)	30.0 (245.43)	66.1 (103.78)
Grant and Training	-60.7 (231.38)		23.0 (146.73)	-411.5 (441.37)	-106.9 (184.41)	-337.5 (399.33)	9.13 (101.92)	-411.9 (551.26)	-757.1 (578.28)	130.7 (202.87)	-424.1 (537.84)	32.3 (119.51)
Loan * W4	-206.5 (177.95)	-189.7 (154.24)	-203.1 (163.17)	-188.0 (165.61)	-52.7 (193.41)	-256.7* (142.39)	-192.0 (148.47)	-139.4 (179.06)	75.3 (135.41)	-299.1 (183.10)	-0.24 (164.58)	-310.1 (208.77)
Loan and Training * W4	199.7 (275.12)	-103.6 (202.13)	434.3 (293.60)	-220.8 (177.27)	-126.7 (218.97)	164.7 (247.23)	113.2 (214.83)	-45.9 (223.51)	144.5 (186.06)	86.9 (272.08)	150.3 (165.10)	123.1 (440.19)
Grant * W4	-114.3 (175.50)		32.9 (165.43)	-245.0 (230.51)	-1.65 (217.80)	-235.3 (184.42)	-267.8 (169.01)	61.0 (238.09)	365.2* (194.47)	-411.0** (207.46)	88.6 (199.52)	-223.6 (211.29)
Grant and Training * W4	207.5 (201.56)		-85.7 (192.57)	441.4 (350.69)	243.9 (266.89)	128.9 (304.25)	-207.1 (153.46)	665.9 (422.03)	661.0 (469.49)	-57.3 (193.55)	462.5 (390.65)	7.91 (211.38)
Control Mean	448.1	403.2	301.5	535.3	471.1	367.3	320.7	533.5	372.7	478.2	517.9	325.0
R^2	0.032	0.044	0.077	0.036	0.058	0.028	0.033	0.040	0.042	0.035	0.023	0.053
N	1364	705	899	1170	799	1250	1154	915	848	1221	1029	840

Notes: Columns (1) to (14) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits for men only. The results are divided by the cited heterogeneity category. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Treatment effects on sales

	Last Sales		Normal Sales		Good Sales		Bad Sales	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male	Female	Male	Female	Male	Female	Male	Female
Loan	135.9	-207.3	64.6	-193.0	212.7	-328.2	4.96	-65.0
	(341.79)	(160.29)	(320.51)	(165.56)	(443.97)	(299.37)	(218.33)	(81.52)
Loan and	660.3	-50.3	513.9	38.3	1160.5*	-60.4	337.6	33.1
Training	(432.86)	(174.30)	(401.03)	(209.58)	(620.19)	(295.59)	(279.17)	(90.62)
Grant	-229.8	449.1*	-176.2	265.4	232.9	375.0	-85.1	339.0**
	(429.62)	(250.25)	(374.88)	(234.27)	(587.06)	(387.81)	(251.06)	(153.68)
Grant and	188.4	99.7	363.9	-152.3	454.2	260.3	153.8	12.4
Training	(436.61)	(321.59)	(447.46)	(242.41)	(579.90)	(591.88)	(278.74)	(119.60)
Loan * W4	-51.8	102.6	-94.2	324.4	-519.9	476.6	-104.9	-35.9
	(526.19)	(233.69)	(585.01)	(329.31)	(850.07)	(430.04)	(403.45)	(128.65)
Loan and	-119.9	60.9	-481.1	267.5	-1186.8	-68.7	-422.3	147.6
Training * W4	(604.64)	(259.38)	(613.64)	(481.55)	(916.88)	(387.68)	(435.61)	(221.87)
Grant * W4	21.2	-817.7**	-770.0	-647.6*	-1700.6*	-674.4	-398.2	-623.0***
	(597.30)	(341.15)	(616.90)	(369.58)	(898.49)	(552.71)	(428.36)	(207.07)
Grant and	-393.1	-2.53	-873.4	255.3	-885.2	88.9	-466.6	76.5
Training * W4	(563.92)	(410.01)	(662.80)	(379.61)	(936.48)	(747.98)	(484.82)	(211.42)
Control Mean	1490.6	1158.8	1615.7	1169.9	2511.8	1932.8	977.3	658.7
R^2	0.24	0.11	0.18	0.090	0.18	0.12	0.19	0.12
N	919	1482	918	1480	911	1474	907	1469

Notes: Columns (1) to (8) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business sales. The outcome variables are different measures of sales, including last month, sales in a normal month, sale in a good month and sales in a bad month. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8: Treatment effects on employment and capital outcomes

	Total Employees		Hired Employees		Family Employees		Capital		Number of loans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
Loan	0.24	-0.14	0.12	-0.16	0.080	0.100*	1571.7	-256.5	-0.048	0.074
	(0.25)	(0.19)	(0.15)	(0.12)	(0.08)	(0.06)	(2008.20)	(770.77)	(0.10)	(0.07)
Loan and	0.72**	-0.026	0.54***	-0.11	0.20**	0.034	1753.0	-258.2	0.22	-0.015
Training	(0.29)	(0.16)	(0.20)	(0.12)	(0.09)	(0.05)	(1974.67)	(369.34)	(0.22)	(0.07)
Grant	0.38	-0.24	0.13	-0.15	0.026	0.025	1608.8	411.3	0.20	0.49
	(0.27)	(0.19)	(0.20)	(0.14)	(0.10)	(0.07)	(2077.00)	(620.03)	(0.17)	(0.31)
Grant and	0.53	0.013	0.26	-0.18	0.19**	0.16**	1335.3	254.5	0.33	0.43*
Training	(0.32)	(0.18)	(0.20)	(0.13)	(0.09)	(0.07)	(2071.04)	(564.00)	(0.45)	(0.24)
Loan * W4	-0.57	-0.030	-0.51	0.077	-0.11	-0.028	-512.3	-211.3	0.11	-0.070
	(0.37)	(0.16)	(0.34)	(0.17)	(0.12)	(0.08)	(1063.43)	(722.25)	(0.12)	(0.10)
Loan and	-0.75*	-0.19	-0.69*	-0.043	-0.11	-0.066	-202.7	54.9	-0.22	0.016
Training * W4	(0.42)	(0.17)	(0.37)	(0.17)	(0.15)	(0.08)	(1131.66)	(350.45)	(0.23)	(0.09)
Grant * W4	-1.17**	0.15	-0.91**	0.0077	-0.31**	0.0092	-226.1	-785.8	-0.24	-0.60*
	(0.46)	(0.21)	(0.43)	(0.19)	(0.15)	(0.10)	(1303.04)	(567.51)	(0.18)	(0.32)
Grant and	-0.72	-0.074	-0.65	0.071	-0.18	-0.12	-133.2	-175.0	-0.41	-0.57**
Training * W4	(0.47)	(0.19)	(0.42)	(0.18)	(0.15)	(0.10)	(1149.95)	(552.26)	(0.46)	(0.25)
Control Mean	1.22	1.02	1.10	0.81	0.32	0.20	4310.4	2096.9	0.42	0.56
Controls	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Fixed Effects	Yes	Yes	No	No	No	No	Yes	Yes	No	No
R^2	0.082	0.055	0.18	0.10	0.060	0.054	0.012	0.020	0.37	0.38
N	1502	2358	899	1433	899	1433	1475	2340	618	1034

Notes: Columns (1) to (8) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on employment and capital outcomes. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 9: Treatment effects on household outcomes

	Missed School		Child Health		Other Business		Savings		HH Consumption	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
Loan	0.020	0.063	-5.84	-21.2*	-0.021	-0.15*	-62.6	53.5	-105.3	-187.2**
	(0.06)	(0.05)	(16.14)	(11.35)	(0.10)	(0.09)	(81.74)	(39.05)	(164.52)	(91.92)
Loan and	0.078	0.0048	-2.02	-15.3	0.096	-0.13	34.0	42.7	235.9	-229.3***
Training	(0.06)	(0.05)	(15.98)	(12.53)	(0.12)	(0.09)	(105.87)	(29.38)	(346.63)	(87.39)
Grant	0.11	-0.060	22.6	-8.95	-0.075	-0.062	-33.9	50.9	-25.3	-154.9**
	(0.08)	(0.05)	(19.72)	(23.58)	(0.12)	(0.12)	(120.27)	(34.93)	(171.92)	(75.68)
Grant and	0.031	0.075	-14.1	-17.8	0.0097	0.0069	-41.7	-6.67	-58.1	-152.0*
Training	(0.06)	(0.06)	(14.06)	(17.15)	(0.11)	(0.14)	(84.27)	(23.44)	(164.89)	(79.68)
Loan * W4	-0.058	-0.049	-0.44	24.1*	0.078	0.091	23.4	-16.6	-18.1	168.6*
	(0.09)	(0.07)	(16.30)	(13.75)	(0.13)	(0.11)	(130.44)	(51.08)	(185.93)	(101.22)
Loan and	-0.12	-0.029	10.7	15.6	0.086	0.14	-61.3	-2.66	-394.8	181.5*
Training * W4	(0.09)	(0.07)	(23.26)	(14.82)	(0.15)	(0.11)	(146.95)	(40.19)	(369.83)	(93.53)
Grant * W4	-0.28**	0.015	-14.8	13.8	0.16	-0.026	-47.3	-41.0	-211.3	185.3*
	(0.12)	(0.08)	(19.62)	(28.82)	(0.19)	(0.15)	(158.65)	(42.46)	(204.78)	(99.69)
Grant and	0.053	-0.15*	75.0	7.60	0.089	-0.028	-29.0	21.5	-40.9	113.6
Training * W4	(0.11)	(0.08)	(61.45)	(19.60)	(0.15)	(0.17)	(125.94)	(31.20)	(190.63)	(88.70)
Control Mean	0.19	0.27	29.3	34.6	0.69	0.59	291.9	145.8	674.1	605.0
R^2	0.061	0.058	0.056	0.045	0.23	0.30	0.44	0.10	0.087	0.16
N	684	1318	589	1108	674	951	832	1315	920	1483

Notes: Columns (1) to (8) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on household outcomes. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 10: Returns to employment and capital

	Family Employees			Hired Employees		
	(1)	(2)	(3)	(4)	(5)	(6)
Family Employees	664.8	651.1	652.7			
	(419.55)	(404.64)	(406.98)			
Employees				259.2*	208.4	212.4
				(151.30)	(144.89)	(149.90)
Capital		0.043***	0.060		0.038***	0.034
		(0.01)	(0.05)		(0.01)	(0.05)
Control Mean	465.8	468.8	468.8	465.8	468.8	468.8
Labour Instrumented	Yes	Yes	Yes	Yes	Yes	Yes
Capital Instrumented	No	No	Yes	No	No	Yes
Underidentification	0.024	0.015	0.16	0.022	0.012	0.18
Weak Identification	2.33	2.49	1.32	2.30	2.50	1.30
Hansen	0.84	0.90	0.84	0.83	0.78	0.69
R^2	-0.13	-0.050	-0.062	-0.023	0.060	0.057
N	946	935	935	946	935	935

Notes: Columns (1) to (6) report the instrumental variables estimate of the impact of different employment categories and capital on profits for male-owned businesses. Employment and capital are instumented by assignment to one of the four interventions. Robust standard errors are in parentheses below the coefficients. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Spillover effects

	(1) Male Firms	(2) Female Firms	(3) Treatment Firms	(4) Control Firms	(5) Male Firms	(6) Female Firms	(7) Treatment Firms	(8) Control Firms
Close Firms	-3.00***	-0.26	-2.71***	-6.09	0.041	0.44	1.27	-1.88
	(1.08)	(0.59)	(0.95)	(4.55)	(3.29)	(2.05)	(4.00)	(4.27)
Close Firms * W4	0.091	0.18	-0.23	1.92	-0.88	0.75	-0.91	0.70
	(1.17)	(0.64)	(1.45)	(2.80)	(3.37)	(1.88)	(3.90)	(6.03)
Loan	277.2**	-41.2			276.9**	-40.1		
	(114.61)	(61.87)			(114.64)	(62.34)		
Loan and Training	261.9**	-13.9			258.5**	-11.5		
	(119.21)	(60.23)			(118.99)	(60.81)		
Grant	67.6	228.6**			91.8	233.4**		
	(133.08)	(116.14)			(135.02)	(117.31)		
Grant and Training	-143.0	-8.92			-131.7	-3.85		
	(236.65)	(84.11)			(230.81)	(84.27)		
Loan * W4	-180.4	10.6			-184.2	11.9		
	(116.45)	(62.22)			(116.77)	(61.77)		
Loan and Training * W4	68.5	-51.7			66.5	-49.4		
	(168.16)	(64.92)			(169.13)	(64.35)		
Grant * W4	-94.3	-282.0***			-107.2	-277.3***		
	(138.86)	(99.30)			(139.15)	(99.30)		
Grant and Training * W4	187.1	-26.4			182.4	-22.1		
	(199.48)	(79.44)			(195.88)	(79.19)		
Close firms * Density					-0.011	-0.0025	-0.014	-0.016
					(0.01)	(0.01)	(0.01)	(0.02)
Close firms * Density * W4					0.0036	-0.0020	0.0023	0.0053
					(0.01)	(0.01)	(0.01)	(0.02)
Control Mean	428.2	342.9	371.9	428.2	428.2	342.9	371.9	428.2
R^2	0.029	0.021	0.020	0.055	0.030	0.021	0.022	0.057
N	2069	3261	1642	427	2069	3261	1642	427

Notes: Columns (1) to (8) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. The number of firms that received treatment of some kind within the same district is included in columns 1 to 4. Columns 5 to 8 include an interaction with the population density of the districts. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** p<0.01, ** p<0.05, * p<0.1.

Table 12: Bounding effects for attrition for the male sample

	(1) Original Sample	(2) +/- 0.1 SD	(3) +/- 0.2 SD	(4) +/- 0.3 SD	(5) +/- 0.5 SD
Loan	260.6**	178.0*	171.9*	165.9*	153.8
	(113.53)	(95.34)	(95.45)	(95.62)	(96.11)
Loan and	249.4**	146.4	140.0	133.5	120.5
Training	(118.62)	(93.87)	(94.00)	(94.18)	(94.72)
Grant	33.8	-263.1	-268.4	-273.7	-284.4
	(129.96)	(193.70)	(193.77)	(193.87)	(194.12)
Grant and	-184.1	27.7	21.5	15.3	2.84
Training	(238.04)	(115.05)	(115.12)	(115.27)	(115.75)
Loan * W4	-170.5	-46.6	-67.6	-88.6	-130.6
	(116.10)	(150.22)	(150.86)	(151.81)	(154.64)
Loan and	80.7	-12.4	-33.5	-54.7	-97.0
Training * W4	(170.07)	(158.59)	(159.16)	(160.09)	(163.00)
Grant * W4	-89.1	922.0	902.6	883.2	844.4
	(139.26)	(877.32)	(877.45)	(877.63)	(878.13)
Grant and	199.2	-53.4	-70.9	-88.4	-123.4
Training * W4	(202.33)	(149.71)	(150.35)	(151.29)	(154.03)
Control Mean	428.2	428.2	428.2	428.2	428.2
R^2	0.022	0.021	0.021	0.021	0.020
N	2069	2237	2237	2237	2237

Notes: Columns (1) to (5) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits after conducting a bounding exercise. New lower-effect bounds are created by imputing the outcomes for the missing male businesses based on decreasing the assumptions of treatment outcomes. Outcome means are imputed for the missing treated population, minus a predetermined standard deviation of the non-attrited sample in the treated population. The process is then repeated for the attrited control sample, but this time adding a pre-defined standard deviation from the found treated sample. This process then creates a range of outcomes that test how sensitive the results are to the condition of the attrited sample. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** p<0.01, ** p<0.05, * p<0.1.

ONLINE APPENDIX

A Results of interviews on program take-up and usage

Interviewers followed up with a total of 48 randomly selected business owners from the central region. 29 of these people were given in-depth, one-on-one interviews by three trained interviewers using a qualitative question guide. Responses were audio recorded or written down in notebooks. The 19 missing individuals had either relocated to a far location or refused to be interviewed due to time constraints. The two sections of this appendix describe some of the reasons people reported for not taking the loan, grant or training, and a short description of what people said they used the money for and how they see men and women using the money differently. The results are overall consistent with the quantitative analysis described in the main text for both up-take and usage, though they reflect some individual biases about the sexes.

A.1 Take-up of programs

Respondents noted a number of issues relating to take-up and implementation of the programs. The duration of training attendance varied between 1 and 14 days[7], with some business owners reporting that they did not attend the training despite being invited. Business owners who attended the trainings said they appreciated the program and would recommend a similar training for other business people like them. Common topics remembered from the training include how to manage capital flow, budgeting, calculating profits and losses, savings, customer care, records keeping, managing employees, market research and how to handle competition.

Some of the reasons business owners reported for not attending the training included distance to the training venue, especially that they didn't have money for transport; lack of time since they did not want to close their businesses; did not see the training as something important - "you can't start a business if you don't know how to do it [already]" one of the respondent said; were discouraged by their spouse; did not trust that the training would take place for sure.

According to the respondents, the majority of those who refused to take the grants did so because of security reasons. They claim that it was hard for them to believe that the people who came to offer them money were honest - "who just gives you money? Why would they pursue someone who is not interested?" asked one of the participants. They report that they did not trust the people offering money given that there are so many *bafere*, or conmen, in Uganda.

Some of the business owners declined to take the loans because PRIDE Microfinance was disseminating them. According to them, PRIDE has a reputation for harassing its customers. Such respondents claimed they would have taken the loans if any other microfinance

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[7]Attendance information was not collected as part of the follow-ups and so is not used in the analysis. The ILO, who conducted the trainings, was to keep an attendance log but did not do so consistently.

institution was responsible for their dissemination. This type of complaint is common for individual microfinance orgnizations.

Other business owners reported declining to take the loans because they had a lot of personal problems at the time of the offer. They were afraid that these problems would take up the money and prevent them from investing it in their businesses and consequently fail to repay. They thought the interest was too high and loans would have to be paid within a short period of time, putting their businesses at risk.

Two of subjects claim they did not receive the full loan or grant. One participant was offered less money (300,000 USH vs. the original 500,000 USH) at the start and was told that some people from ILO had deducted the money. The full money was eventually given to her after complaining to a manager of PRIDE Microfinance.

Other grant beneficiaries reported that trainers or PRIDE staff asked them for money. Apparently, these individuals claimed that they worked hard to get the individuals free money so it was fair that they are appreciated. At least 2 business owners admitted that they gave some money while others said they were lucky the people who asked for this money did not come back at the time of disbursement; otherwise, they would have had no choice but to give them. The rest of the respondents said they were not asked and a few of them did not want to talk about it. Though some people were quite upset with the field staff for asking for money from them, others thought it was quite right to thank someone who helped them get what they would otherwise not get.

A.2   How the grants and loans were used

Business owners interviewed reported both investing the money in their businesses as well as personal expenses. The majority of the loan beneficiaries though reported investing the loan in their businesses because otherwise they would fail to finance the loan.

When asked about the potential difference in usage of the money between men and women, the responses were more focused on each gender's opinion about the other gender. Overall, women thought they would manage loans much better than men because they have to ensure that the kids are fed, clothed and sent to school, a fact that would keep them focused on the business while men mostly care about booze and more women. The male participants, on the other hand, thought that they would do better because unlike business women, men are focused on their businesses and would therefore invest the grant or loan wisely. Male participants with such opinions claimed that women's focus is their children so the grant or loan would first be spent on the kids and the business would always come second. They also thought that as opposed to men, women like to give out a lot to support the family. This would take a significant amount of the loan or grant money and eventually cripple the women from financing the loans. Some of the male respondents, on the other hand, admitted that women are cowards and much more disciplined than the males, a fact which helps them to stick to the terms of the loan and benefit from it. Men, however, are less trustworthy and more risk-seeking so they would gamble just about anything without thinking much about the consequences of failing to finance the loans. One male respondent described men as "thieves". Other respondents were neutral. These participants thought

that, regardless of the gender, the successful use of a business grant or loan depends on how organized the beneficiary is.

B   Context of the study

B.1   Uganda

Uganda is a landlocked country that borders Kenya, Tanzania, Rwanda, the Democratic Republic of the Congo and South Sudan. It is one of the poorest countries in the world. In 2009, 38% of the population lived on less than \$1.25 per day. GDP per capita in 2005 USD was \$384, ranking it in the bottom third of countries in Sub-Saharan Africa (WDI 2014).

A map of the country is presented in Figure B.1. The sample areas are highlighted. The central region includes Mukono, Buikwe and Jinja districts. During the baseline survey, the survey team attempted to interview all of the businesses in these districts, with a focus on businesses located near the main road-way that connects the capital, Kampala, to the border with Kenya. This corridor is the main trading network for Uganda.

The northern region is composed of Gulu district, specifically Gulu town, the main trading center in the north. Once the civil war in the north of the country finished in 2007 this town became the main trading center with Sudan and Congo. It is currently the second largest town in Uganda, behind only the capital. The survey team focused in Gulu on the main trading center in the town, which has grown significantly in size since the end of the conflict.

B.2   Selection into the final sample

Individuals in the sample come from people who answered yes to two questions: "The ILO is looking for people willing to take a class to help improve their businesses. It takes five days and is completely free. Would you be interested?" and "A local microfinance organization is looking for people who are interested in taking out loans to expand their businesses. These loans would be about 500,000 USH. Would you be interested?" These questions were asked twice: once during the baseline census and once during the second baseline survey. To be included in the sample, individuals had to answer yes to both questions both times they were asked. This then comprises my main sample of 1,550 businesses. Therefore, this sample potentially presents a select group of people. I next look at correlations between the interest of individuals in receiving the two interventions and some basic demographics to determine how unique this sample might be. The regression conducted on individual i uses an OLS specification on the following model:

$$I_i = \alpha + \beta X_i + \gamma R + \epsilon_i \tag{1}$$

where I is a person's expressed interest in the program, X is a range of characteristics, R is a region dummy, and ϵ is the error term. This regression is run on both of the baseline surveys. A person is coded as interested if they answer both times "yes" to the loan and training offers and coded as "no" otherwise.

The results are presented in Table DXX. Interest in a loan (column 1), interest in training (column 2) and interest in both a loan and training (column 3) are all significantly associated

with a number of individual characteristics, most them the same across the interest categories. Younger people are more likely to be interested in the programs, as are those who are married and have had loans previously. Ability and assets are also correlated with interest in training. Baseline profits are negatively correlated with interest in loans or trainings, though the effect is small considering these values are in '000s USH. These correlations suggest that there is some selection into the sample, though none of the coefficients are very large. In section 7.5 I weight the experimental sample by selection into the programs to obtain the population weighted average treatment effect and find this does not have much effect on the main results.

B.3   Comparison of normal microfinance and training clients

PRIDE Microfinance, which has been operating across Uganda for many years, administered the loans. The sample of participants was drawn from the populations in the two baseline surveys. Individuals had to express interest in expanding their business, taking a loan and receiving training from the ILO in each of the surveys. In practice, everyone who wanted a loan and training also said they wanted to expand their business.

In addition to expressing interest in the programs PRIDE also reviewed the full sample of business owners to ensure they would accept all of the participants as clients if selected. However, the information from the baseline surveys did not fully coincide with PRIDE intake surveys, so some information PRIDE thought important was missing. It was agreed in cooperation with the researcher that PRIDE would accept everyone that had a monthly profit high enough to cover the cost of the loan. This meant a few businesses were not included in the final sample due to low profits.

I chose the sample to reflect what a program from an international organization or government would look like if they were interested in expanding loan access. It may not necessarily reflect what PRIDE or other microfinance organizations normally do. Data from a separate ILO study of PRIDE clients in Uganda suggests there is at least one difference between the businesses in this sample and normal PRIDE clients: the profit level of the business. The average profit level of the businesses in this study at baseline is 307,000 USH, while the previous PRIDE study found business profits to be 835,000 USH. The difference is large and statistically significant, suggesting this program targeted much smaller businesses, as intended.

B.4   Take-up

Actual take-up of training and loan programs by those who have expressed interest in such programs has been problematic in the literature. This evaluation faced some issues as well, which I discuss in this section. A qualitative follow-up survey to better understand take-up issues was also conducted and is presented in Appendix A. To test for the characteristics of people who took the programs, I run the following OLS regression:

$$P_i = \alpha + \beta X_i + \varphi R + \epsilon_i \qquad (2)$$

where P is a dummy for whether person i participated in the particular intervention, X is a matrix of individual baseline characteristics and R is a matrix of region and sample dummies. The results of this regression are presented in Table DXX and are divided between the full (columns 1 and 2), central (columns 3 to 5) and northern samples (columns 6 and 7).

Of those who were offered the loans, 40% accepted. This is similar to the literature on loan take-up, which finds lower than expected take-up after people have expressed interest. Karlan, Morduch and Mullainathan (2010) document a number of microfinance studies that have take-up rates of between 2% and 80%. Columns 1, 3 and 6 present the take-up analysis for the loans. There are few significant correlations across individual characteristics, though older people were more likely to take the loan. The largest predictor of take-up for loans is whether the person was offered and attended the trainings. The results of the qualitative interviews suggest that many people who did not take the loans did so because they were either worried about repaying the money or distrust of the implementing agency. As the trainings were given just before the loans were offered, the differential take-up for those who attended the trainings is most likely due to either increased time with the implementing organizations, and thus increased trust, or a greater confidence due to the trainings in being able to repay the loans.

Most surprising was that grant take-up was not universal. This was money that was to be given to the businesses without a repayment requirement and with no strings attached and was framed to businesses as such. Still, only 71% of those selected took the money. Column 3 presents the take-up analysis for the grants. None of the individual characteristics tested predict take-up for the grants. Similar to the effect on loan take-up, whether the person attended the trainings has a large positive correlation with grant take-up. Qualitative interviews suggest that many people simply did not believe the offer of the grants, thinking it too good to be true. The trainings most likely increased confidence in individuals that the offer was real.

Despite the indication of interest, only 71% of people invited to attend the trainings actually attended. This is similar to other studies, as summarized in McKenzie and Woodruff (2012b). Out of 14 studies they survey, only four had attendance above 80%. Most vary from 39% to 75%. For instance, Bruhn and Zia (2011) and Valdivia (2012) worked only with businesses that expressed interest in training but only had attendance of 39% and 51%, respectively. Take-up analysis for training is presented in columns 2, 5 and 7. Only past experience with having attended trainings and age predicts whether the person attended the offered training. The effect of previous training is positive, significant and large, suggesting that people with training felt a strong interest in receiving more training. The qualitative surveys identified a number of other reasons people did not take the training. Most people reported that the time away from the business necessary for the training was too difficult for them. The ILO made efforts to schedule evening and half-day sessions, but this was still too onerous for some business owners.

The evidence presented here on grant take-up presents a cautionary tale for organizations interested in unconditional cash transfers. The ILO was to inform people of their selection to receive the grants. They decided to do this first by phone, which was not effective as people did not believe the caller. The ILO then organized information sessions, but not all people showed up due to suspicions that the offer was not to be believed. Proper implementation of such programs is difficult and should be approached with some caution.

B.5   Timing of loans and grants

The loans were not distributed at the same time due to the need to train some of the population first. The actual timing of the loans is presented in Figure B.2. The disbursement is tri-modal. Individuals selected to receive only the loans were given loans first, in August. The remaining individuals were given loans after the trainings had been completed, in September and October. Receiving a loan was not contingent upon taking the training, so tracking individuals over time became difficult. This is reflected in some individuals receiving the loans in early to mid November. A test for the effect of loan timing (available upon request) suggests that there is not a difference in outcomes due to loan timing.

The ILO delivered the grants. This delivery faced a serious problem of people feeling the program was too good to be true. Grant take-up was thus about 70%. The grants were initially to be distributed at the same time as the loans, but this was delayed. Grant disbursement was therefore to be done in October. Initially, the ILO called participants to offer the money. When it became apparent that people were not taking the calls seriously, the ILO sent teams to the field to speak directly with business owners, either face-to-face or in groups. Due to the delays, the grants were not delivered until November. All disbursement information was confirmed with the business owners during the first follow-up to ensure they received the money.

B.6   Additional balance tests

As the main analysis is conducted by program and gender, I present here balance tests for the individual treatment arms by gender. Table B.1 presents balance tests for the female sample, and Table B.2 for the male sample. There is generally very good balance across the variables, with 10% or less of the coefficients significant at the 90% or greater level.

The main analysis conducted on treatment effects is a fixed effects estimation. This method both improves power and means individual effects are controled for. Differences in level values of characteristics will therefore be less critical for the analysis. Of more importance will be understanding any systematic differences in changes over time for individuals. Thus, in addition to the balance levels, I also present the balance of *changes* between the two baseline surveys. The last two variables in Tables B.1 and B.2 are the changes for women and men by treatment arm for profit and revenue, the only values collected in both baseline surveys. The results are balanced for all of the samples, except for some imbalance in the male sample. Men in the loan only and grant only programs have greater revenue changes than the control group, as well as profit changes for men in the grant only program.
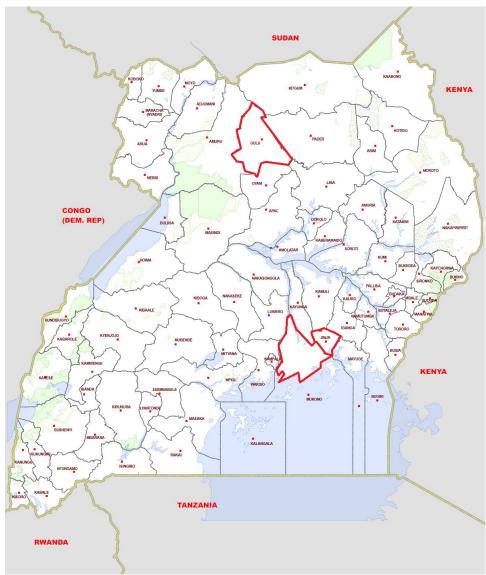
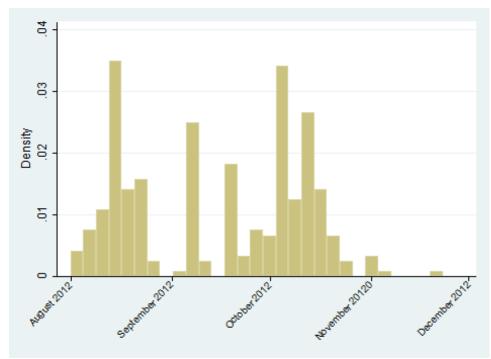Fig. B.1: Map of Uganda with treatment districts

Fig. B.2: Density of loans by date

Table B.1: Determinants of interest for the interventions

	(1) Interest in loan	(2) Interest in training	(3) Interest in both
Female	-0.0071	-0.0048	-0.011
	(0.01)	(0.01)	(0.01)
Age	-0.053***	-0.063***	-0.052***
	(0.01)	(0.01)	(0.01)
Married	0.033**	0.032**	0.035**
	(0.01)	(0.02)	(0.01)
Literacy	0.011	-0.028	0.0061
	(0.02)	(0.02)	(0.02)
Previous Training	0.016	0.0025	0.017
	(0.02)	(0.02)	(0.02)
Previous Loan	0.10***	0.089***	0.099***
	(0.01)	(0.01)	(0.01)
Revenues - 1 Lag	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)
Profits - 1 Lag	-0.000038***	-0.000041***	-0.000037***
	(0.00)	(0.00)	(0.00)
Ability	0.0035	0.024**	0.0049
	(0.01)	(0.01)	(0.01)
Assets	-0.0039	-0.0096***	-0.0049
	(0.00)	(0.00)	(0.00)
Control Mean			
R^2	0.38	0.30	0.38
N	4201	4201	4201

Notes: Columns (1) to (3) report the results of an OLS regression on whether the individual expressed interested in the loan, training or loan and training programs. Sample is from the first baseline. Robust p-values are in parentheses below the coefficients. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B.2: Take-up analysis

	All		Central			North	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Loans	Training	Loans	Grants	Training	Loans	Training
Received Training	0.10***		0.15***	0.21***		0.064	
	(0.04)		(0.05)	(0.05)		(0.05)	
Female	-0.0074	-0.0075	0.0073	-0.017	-0.050	-0.022	0.13
	(0.04)	(0.04)	(0.05)	(0.04)	(0.06)	(0.06)	(0.08)
Age	0.043**	0.045*	0.065**	0.018	0.052	0.025	0.047
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)
Married	0.026	-0.054	0.023	-0.0059	-0.069	0.045	-0.067
	(0.04)	(0.04)	(0.06)	(0.04)	(0.05)	(0.06)	(0.08)
Literacy	-0.023	-0.016	-0.11	-0.087	0.068	0.044	-0.21
	(0.06)	(0.07)	(0.09)	(0.07)	(0.09)	(0.08)	(0.13)
Previous Training	0.018	0.098*	0.014	-0.0020	0.19***	0.022	-0.083
	(0.05)	(0.05)	(0.07)	(0.05)	(0.07)	(0.07)	(0.09)
Total Employees	-0.012	-0.0072	-0.020*	-0.011	-0.0071	-0.0020	-0.049
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.04)
Revenues - 1 Lag	0.0069	-0.011	0.0077	0.0011	-0.0056	-0.0067	-0.065***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Revenues - 2 Lags	0.0029	-0.0035	0.035	0.050*	-0.015	-0.023	0.043
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Profits - 1 Lag	-0.023	0.0085	-0.014	0.010	-0.0065	-0.011	0.13
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Profits - 2 Lags	-0.017	-0.032***	-0.057*	-0.032***	-0.036***	0.015	0.015
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Ability	0.019	0.020	0.059	0.016	-0.038	-0.0096	0.11**
	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)
Assets	-0.0024	0.025	-0.0079	-0.0069	0.033	-0.0018	0.034
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)
Previous Loan	0.087**	0.064	0.047	0.062	0.066	0.13**	0.064
	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.07)
Control Mean							
R^2	0.18	0.053	0.24	0.45	0.065	0.043	0.100
N	695	514	358	324	348	337	166

Notes: Columns (1) to (3) report the results of an OLS regression on whether the invited individual took the program that was offered for the loans, grants and training programs, respectively. Robust standard errors are in parentheses below the coefficients. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B.3: Balance tests by intervention arm for women

Baseline Characteristic	Loan		Loans and Training		Grant		Grants and Training	
	Coef	P-value	Coef	P-value	Coef	P-value	Coef	P-value
Age 18-23	-0.01	(0.61)	-0.05	(0.04)	-0.05	(0.21)	0.01	(0.76)
Age 24-29	0.08	(0.08)	0.07	(0.10)	0.04	(0.56)	0.06	(0.39)
Age 30-35	-0.01	(0.83)	0.02	(0.67)	0.07	(0.34)	-0.05	(0.48)
Age 36-41	-0.01	(0.70)	-0.03	(0.38)	-0.06	(0.29)	-0.02	(0.68)
Age 41-50	-0.04	(0.10)	-0.01	(0.69)	-0.00	(0.39)	-0.00	(0.39)
Married	0.01	(0.72)	0.00	(0.94)	0.00	(1.00)	-0.02	(0.71)
Literate	-0.03	(0.54)	0.05	(0.25)	-0.02	(0.76)	-0.06	(0.39)
Previous training	-0.05	(0.25)	-0.01	(0.89)	-0.00	(1.00)	-0.01	(0.80)
Number of employees	0.16	(0.23)	0.01	(0.88)	0.21	(0.09)	0.19	(0.07)
Employees hours worked	6.30	(0.35)	0.92	(0.88)	6.19	(0.61)	3.70	(0.75)
Does not keep records	0.00	(0.95)	-0.02	(0.39)	0.01	(0.32)	0.01	(0.30)
Keeps records on computer	0.00	(0.89)	0.00	(0.92)	-0.02	(0.29)	0.00	(1.00)
Keeps written records	-0.02	(0.67)	0.04	(0.44)	0.06	(0.36)	0.08	(0.25)
Keeps record in head	0.00	(0.97)	-0.03	(0.55)	-0.05	(0.46)	-0.09	(0.19)
Keeps money in separate bags	0.02	(0.15)	0.01	(0.26)	0.00	(0.98)	-0.00	(0.07)
Last month's revenue (1000 USh)	-48.47	(0.44)	-76.15	(0.21)	-27.59	(0.77)	26.74	(0.80)
Average months revenue (1000 USh)	1106.02	(0.32)	-89.99	(0.28)	151.73	(0.41)	-4.01	(0.98)
Last month's profit (1000 USh)	5.96	(0.92)	42.32	(0.41)	-52.75	(0.24)	-9.28	(0.85)
Average month's profit (1000 USh)	-16.47	(0.64)	54.69	(0.28)	20.14	(0.75)	47.04	(0.38)
Stock value (1000 USh)	297.16	(0.38)	91.34	(0.69)	507.49	(0.32)	648.20	(0.24)
Value of liabilities (1000 USh)	91.99	(0.16)	46.82	(0.28)	73.51	(0.23)	82.73	(0.50)
Longest string of numbers recalled	-0.11	(0.56)	-0.12	(0.55)	-0.08	(0.72)	0.17	(0.49)
Math questions answered correctly	-0.00	(0.94)	0.01	(0.89)	0.13	(0.10)	-0.01	(0.93)
Ability Index	-0.06	(0.55)	0.06	(0.55)	0.06	(0.67)	0.02	(0.90)
Had a loan previously	0.04	(0.36)	0.09	(0.07)	0.07	(0.29)	0.05	(0.53)
Asset index	0.01	(0.95)	-0.05	(0.71)	-0.28	(0.08)	-0.06	(0.74)
Difference of Profit	-11.20	(0.86)	-11.46	(0.85)	156.52	(0.27)	165.16	(0.15)
Difference of Revenue	130.68	(0.48)	64.33	(0.71)	581.17	(0.45)	59.06	(0.86)

Notes: Robust p-values from an OLS regression with baseline characteristic as the dependent and treatment arm as the independent variable are reported for each treatment. * denotes significance at the 10% level, ** at 5% and *** at 1%.

Table B.4: Balance tests by intervention arm for men

	Loan		Loans and Training		Grant		Grants and Training	
Baseline Characteristic	Coef	P-value	Coef	P-value	Coef	P-value	Coef	P-value
Age 18-23	-0.06	(0.23)	0.01	(0.88)	-0.05	(0.46)	-0.05	(0.44)
Age 24-29	-0.01	(0.88)	0.00	(0.97)	-0.05	(0.56)	0.06	(0.50)
Age 30-35	0.06	(0.30)	-0.01	(0.88)	0.13	(0.10)	0.01	(0.88)
Age 36-41	0.03	(0.39)	0.02	(0.66)	-0.03	(0.54)	-0.02	(0.73)
Age 41-50	-0.02	(0.35)	-0.02	(0.50)	0.00	(0.00)	0.00	(0.86)
Married	0.02	(0.79)	-0.02	(0.77)	0.03	(0.70)	0.01	(0.93)
Literate	0.02	(0.54)	-0.00	(0.96)	-0.00	(0.99)	-0.01	(0.86)
Previous training	0.02	(0.69)	-0.00	(0.96)	0.02	(0.80)	0.07	(0.35)
Number of employees	0.02	(0.91)	0.05	(0.79)	0.35	(0.09)	0.35	(0.15)
Employees hours worked	12.73	(0.23)	17.50	(0.12)	30.58	(0.07)	54.00	(0.02)
Does not keep records	0.02	(0.39)	0.01	(0.60)	0.00	(0.93)	0.00	(0.99)
Keeps records on computer	0.04	(0.07)	0.02	(0.27)	0.00	(0.93)	0.04	(0.16)
Keeps written records	0.01	(0.86)	-0.03	(0.60)	0.12	(0.15)	0.12	(0.13)
Keeps record in head	-0.07	(0.13)	-0.01	(0.90)	-0.12	(0.13)	-0.16	(0.03)
Keeps money in separate bags	0.00	(.)	0.00	(.)	0.00	(.)	0.00	(.)
Last month's revenue (1000 USh)	-108.15	(0.28)	-122.38	(0.20)	-118.91	(0.31)	13.62	(0.91)
Average months revenue (1000 USh)	-20.26	(0.90)	29.19	(0.89)	2.37	(0.99)	373.80	(0.40)
Last month's profit (1000 USh)	-103.41	(0.27)	-131.86	(0.19)	-217.66	(0.20)	79.60	(0.80)
Average month's profit (1000 USh)	-415.64	(0.25)	-487.78	(0.20)	-740.75	(0.31)	-354.32	(0.64)
Stock value (1000 USh)	-542.56	(0.75)	-2141.20	(0.16)	-3037.04	(0.31)	-3577.94	(0.17)
Value of liabilities (1000 USh)	-120.26	(0.36)	-52.01	(0.73)	-148.65	(0.52)	-106.12	(0.60)
Longest string of numbers recalled	0.17	(0.53)	-0.15	(0.58)	0.12	(0.72)	0.02	(0.96)
Math questions answered correctly	0.05	(0.36)	-0.01	(0.88)	0.02	(0.85)	-0.00	(1.00)
Ability Index	0.08	(0.45)	-0.05	(0.67)	-0.03	(0.83)	0.02	(0.88)
Had a loan previously	-0.04	(0.48)	-0.02	(0.79)	0.09	(0.32)	0.01	(0.88)
Asset index	0.19	(0.30)	0.18	(0.32)	0.28	(0.15)	0.36	(0.02)
Difference of Profit	137.66	(0.26)	98.49	(0.31)	676.97	(0.02)	-117.58	(0.71)
Difference of Revenue	460.10	(0.10)	-7.12	(0.97)	1283.51	(0.06)	192.32	(0.64)

Notes: Robust p-values from an OLS regression with baseline characteristic as the dependent and treatment arm as the independent variable are reported for each treatment. * denotes significance at the 10% level, ** at 5% and *** at 1%.

C   Model of returns to capital and employment with endogenous labor choice and imperfect credit market

The intervention is akin to an exogenous injection of capital (human and physical) into an independent enterprise. To predict the effects on business returns and employment, I discuss a simple model of self-employment from de Mel et al. (2008) with endogenous labor choice and imperfect credit markets.

In a one-period model in which an entrepreneur has L hours to allocate between labor in her enterprise, l, and leisure, $L - l$ (I assume for simplicity she does not participate outside labor market). The household has an endowment of assets A and allocates the number of other household workers, n, to the labor market, where they earn wage, w. The entrepreneur runs a business using her labor and capital stock, K. This capital stock is financed through borrowing amount B on the formal credit market, as well as through household resources, either by allocating A_k of household assets or I_k of household earnings.

The entrepreneurs problem is to choose the amount of capital and own labor to invest in the business in order to maximize utility subject to budget and borrowing constraints:

$$Max_{[K,l,B,A_k,I_k]} U(c, L - l) \tag{3}$$

$$s.t. : c = f(K, l, \theta) - rK + r(A - A_k) + (nw - I_k) \tag{4}$$

$$K \leq A_k + I_k + B \tag{5}$$

$$B \leq B* \tag{6}$$

$$A_k \leq A \tag{7}$$

$$I_k \leq nw \tag{8}$$

$$l \leq L \tag{9}$$

where r is the market interest rate, and the production function of the business, $f(\cdot)$, depends on the capital stock, labor, and θ, the ability of the entrepreneur. Consumption and leisure are assumed separable in the utility function.

We can look at two cases. First, when constraint (9) binds, the household is sufficiently poor that the entrepreneur would like to work even more than her available time allows, in order to reap the utility from additional consumption. In this case, the full labor endowment L is applied to the business. With well-functioning credit markets, households will choose K to maximize expected profits, such that the marginal return to capital equals the market interest rate: $f_K(K, L, \theta) = r$.

With imperfect credit markets, however, the entrepreneur will set the marginal return equal to the market interest rate plus the shadow cost of capital. Solving the first order conditions for the optimal choices of B, A_k, and I_k yields: $f_K(K, L,) = r + \lambda$, where $\lambda = \mu_B = \mu A + r = \mu_I + 1$. μ_B, μ_A, and μ_I are the Lagrange multipliers on constraints (6),

(7) and (8), respectively. Thus credit constraints arise only if both the formal/external and informal/internal credit markets are binding.

In this case, λ depends on the availability of internal capital. If there is an exogenous increase in K, households with more access to capital, such as those with larger n and more liquid assets, will have a lower marginal return to capital compared to those more constrained. If ability and capital are compliments, the model also implies that higher ability individuals will be relatively more capital constrained relative to lower ability individuals, and so will have a higher marginal return to capital.

In the case where labor constraint (9) does not bind, and the entrepreneur has some leisure; we can predict the effect of an increase in K on employment hours.

The value function for equation (3) and constraint (4) is as follows:

$$V(c, l, \lambda) = U(c, L - l) + \lambda(f(K, l,)rK + r(A - A_k) + (nwI_k)c) \tag{10}$$

The first order conditions are thus:

$$V_\lambda = f(K, l,) - rK + r(A - A_k) + (nw - I_k) - c = 0 \tag{11}$$
$$V_c = U_c'(c, L - l) - \lambda = 0 \tag{12}$$
$$V_l = -U_l'(c, L - l) + \lambda f_l'(K, l, \theta) = 0 \tag{13}$$

This implies that the marginal utility of consumption is equal to λ, and the marginal utility of labor divided by the marginal return to labor is equal to λ. Solving for (12) and (13), we obtain

$$U_c'(c, L - l) = U_l'(c, L - l)/f_l'(K, l, \theta) \rightarrow U_c'(c, L - l) \cdot f_l'(K, l, \theta) = U_l'(c, L - l) \tag{14}$$

The individual thus chooses labor hours to set the marginal benefit from working an additional hour (the marginal utility of the additional consumption) times the marginal productivity of labor equal to the marginal cost of working an additional hour (the loss in utility from less leisure). The net effect on labor hours supplied is uncertain. A treatment that increases K also increases consumption, which lowers the marginal utility of consumption, reducing the incentive to work (the income effect on leisure). If capital and labor are complements in the production function, an increase in K raises the marginal product of labor, increasing the incentive to work in the business (a substitution effect). But if capital and labor are substitutes in production, increased K will lower hours worked.

Endogenous ability

An additional special case is when an individual can invest in human capital to increase her ability. In this case, we assume that investment in capital can be made to both human (H)

and physical (K) capital. The individual then faces the following maximization problem and constraints:

$$Max_{[K,H,l,B,A_k,I_k]} U(c, L - l) \tag{15}$$

$$s.t. : c = f(K, H, l, \theta) - r(K + H) + r(A - A_k) + (nw - I_k) \tag{16}$$

$$K + H \le A_k + I_k + B \tag{17}$$

$$B \le B* \tag{18}$$

$$A_k \le A \tag{19}$$

$$I_k \le nw \tag{20}$$

$$l \le L \tag{21}$$

In the case where the labor constraint (21) still does not bind, the value function becomes:

$$V(c, l, \lambda) = U(c, L - l) + \lambda(f(K, H, l) - r(K + H) + r(A - A_k) + (nw - I_k) - c) \tag{22}$$

And the first order conditions are:

$$V_\lambda = f(K, H, l) - r(K + H) + r(A - A_k) + (nw - I_k) - c = 0 \tag{23}$$

$$V_c = U'_c(c, L - l) - \lambda = 0 \tag{24}$$

$$V_l = -U'_l(c, L - l) + \lambda f'_l(K, H, l) = 0 \tag{25}$$

$$V_K = \lambda f'_K(K, H, l) - r = 0 \rightarrow f'_K(K, H, l) = r/\lambda \tag{26}$$

$$V_H = \lambda f'_H(K, H, l) - r = 0 \rightarrow f'_H(K, H, l) = r/\lambda \tag{27}$$

Equations (26) and (27) imply that the marginal product of human and physical capital will be equal to the interest rate of capital, weighted by the shadow cost of capital. Solving for these two equations, we obtain the result that an individual will invest in both human and physical capital until the marginal product is equal to each other:

$$f'_K(K, H, l) = f'_H(K, H, l) \tag{28}$$

D   Additional analysis

D.1   Formal test of equality of treatments

Table D.1 presents a formal test of equality between treatment arms for the main results presented in Table 4. Low p-values suggest a rejection of the hypothesis that the effects of the interventions are equal or the summation of the waves is equal to zero. The test fails to reject the null hypothesis that the loan and loan-with-training interventions and the grant and grant-with-training interventions are equal for men. The null is though rejected for equality of any of the loan programs with the grant interventions. The test also suggests the wave 3 plus wave 4 effects of the loan-with-training are equal to zero, but cannot reject for any of the other interventions.

D.2   Local average treatment effects

The main analysis presented in the paper explores the intention to treat effect of the programs. As there was not full take-up of the program, these results underestimate the effect of the loans on the treated population by including untreated individuals in the treatment groups. There was also differential take-up between the loan and grant programs, meaning comparisons of these two samples could be problematic if there was significant and important selection into the two groups. To control for these issues, I also conduct a local average treatment effects estimation.

The LATE estimator is a treatment-on-the-treated (TOT) estimate using assignment to treatment, A_{it}, as an instrument for treatment T_{it} for individual i at time t:

$$Y_{it} = \alpha + \beta T_{it} + \theta T_{it} * \gamma_t + \gamma_t + \eta_i + \varphi R + \mu W + \delta M + \epsilon_{it} \tag{29}$$

$$T_{it} = \alpha + \lambda A_{it} + \nu A_{it} * \gamma_t + \gamma_t + \eta_i + \rho R + \sigma W + \varsigma M + \epsilon_{it} \tag{30}$$

where Y_{it} is profit in the last month, T_{it} is a matrix of dummy variables for which treatment an individual belonged to, γ_t are wave effects, and so $T_{it} * \gamma_t$ is the wave effects for each treatment. η_i are individual fixed effects, R is a matrix of region and sample dummies, W is a control for the time between surveys, M is the month of the data collection and ϵ_{it} is the error term. All standard errors are clustered at the individual level and are robust. The instrument for treatment is whether a person was assigned to the cash grant or loan programs, but does not include assignment to training. This is done to simplify the analysis and would likely decrease any effects found.

The results of this estimation are presented in Table D.1. The effects are consistent with the main analysis. There is no effect from the programs on women and men in the grant programs. Men assigned to the loan interventions have a large, positive and significant coefficient. In column 3, where there are no wave interactions, the effect size is an increase in profits equal to 131% over the control group. In column 4, where wave interactions are included, this effect is larger in wave 3 and decreases to 114% of control levels in wave 4.

D.3 Targeting effects and idiosyncratic treatment response

By the time the second follow-up data collection was complete, 50 of the individuals (60% of them men) that had received loans were in arrears for their loan repayments. This represents approximately 15% of all people who had received a loan. While a relatively low number, especially considering that the ILO subsidized the loans, this is much higher than rates commonly found in group-lending models where repayments are often close to 98%. Those individuals who are behind in payments are likely a select group of individuals. Table D.2 estimates an OLS regression on whether a person is currently in arrears. None of the characteristics are significant except for age, which is significant and positive. Older individuals are thus more likely to be in arrears.

The individuals behind on payments may be driving the zero or negative effects for women. Table D.3 estimates the effect of family proximity, but with those who are behind on payments dropped from the sample. The results are largely consistent with Table 5 in the main text, except for women with family far away: the loan-with-training program is now positive and significant for waves 3 and 4. This suggests that the loan and training intervention had some positive effects for a subsample of women.

The results from dropping people in arrears is obviously biased as it drops what can only be assumed are the worst performing businesses. They could represent an idiosyncratic treatment response, which should thus not be dropped from any analysis. They could also represent targeting effects. The business owners offered loans were not the normal clients of PRIDE Microfinance but were instead selected from a population who had only expressed interest in getting a loan. These people are less likely to be accepted for a loan, on average. They were also offered discounted interest rates, meaning they may have felt more tempted than normal to take a loan.

The results in Table D.4 could thus suggest the effects of a better targeting system for loans. The goal of the experiment was to test the expansion of access to loans. While it is difficult to both expand access and target more carefully, better targeting here would have produced an improved social outcome. If the people who are currently in arrears had been excluded from the loan offer, the results would not change for men or women with close family, but there would be a positive and sustained impact from the loan and training program for women with families far away.

D.4 Effects of trimming the sample

The results presented in the main paper are for the sample that has been trimmed by dropping the highest and lowest 0.5% outcomes, as well as dropping the baseline values for individuals not found in either wave 3 or 4. Trimming is important in cases where there are concerns that extreme values do not reflect the actual situation but are instead due to reporting error. For survey based data, low levels of trimming are normal and generally considered a good idea to reduce noise and avoid overstating results.

Table D.5 explores the effects of this trimming on the two main results: the effect of the loan interventions on men and the effect of family proximity for women. Columns 1, 3 and

5 are the untrimmed sample, while columns 2, 4 and 6 are for the trimmed sample. The size of the effects from the loans programs for men decreases without trimming and is no longer significant for wave 3. By wave 4 though the effects for the loans and training program are consistent, if not significant, with the trimmed sample. Trimming does not change the results for women, and so does not appear to have a substantial effect on the main results.

D.5 Population weighted effects

As discussed in the main paper in Section 4.2, the final sample of businesses in this study are quantitatively different from the businesses found in the full baseline business listing. While these differences are not large, they could overstate the effect of such a program on larger samples and could impact the generalizability of the results. One way to test for this is to estimate the population average treatment effect (PATE). I obtain the PATE by first estimating the probability of being in the final sample using the characteristics of the broader population, as was conducted in Table 3. I then use the predicted probability to reweight the sample in the main fixed effects model so that the analysis better reflects the full population of business owners.

Table D.6 compares the results from the main ITT (columns 1 and 3) and new PATE (columns 2 and 4) estimations for the male and female samples. There is almost no difference between these results, though statistical significance does decrease for the PATE estimation. These results use the Probit model to estimate the probability of being in the sample, but are also consistent with estimating the probability of selection using a linear OLS model[8]. Overall, the PATE is suggestive that the main results would hold if the larger population of businesses in the baseline listing had greater interest in trainings and loans programs offered.

D.6 Business practices

To further explore some of the likely channel effects, Table D.7 looks at intervention effects on business practices, including whether the owner had a business plan, engaged in marketing, or sought advice from other people on the business.

The loan-only program increases the likelihood that men have a plan for their business (column 1, marginally not significant), and the grant program increases this likelihood for women (column 2). The other interventions appear to have no effect. The majority of businesses in the sample (95% of the control group) report they have a business plan of some kind though. Whether the person engages in marketing (columns 3 and 4) is not significant for men or women for any program. In column 5, men experience an initial effect from the programs on how many people they sought advice for their business. This ranges from a 30% to 100% increase in the number of people they spoke to over the control group. These effects disappear over time. There appear to be no effects for women.

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[8]The linear OLS estimation is not shown because, as is common with the linear OLS model, some of the probabilities are estimated as negative and must therefore be artificially bounded at 0.

Thus, there appears to be little effect from the programs on general business practices. There are, however, some short-run effects for men on seeking advice. It is not possible to know what these men spoke to other business owners about, but it suggests an increased engagement with their business.

D.7   Additional profit tests

The heterogeneity analysis in Table 6 presented only the results for men. Tables D.8 to D.13 present the full heterogeneity analysis for both men and women. The results for men are as described in the main paper, while there are no changes in results for women for any of the heterogeneity tests.

The test for including enumerator effects in the analysis is presented in Table D.14. The results, as described in the paper, are consistent if not a bit larger than the main results obtained without including enumerator effects.

Table D.1: Tests of equality of interventions

Tests for equality of treatment types	Male Sample	Female Sample
Loans = Loans and Training	0.913	0.674
Loans = Grants	0.046	0.020
Loans = Grants and Training	0.055	0.680
Loans and Training = Grants	0.064	0.046
Loans and Training = Grants and Training	0.064	0.950
Grants = Grants and Training	0.255	0.061
Tests for treatment effets over time		
Loans: Wave 3 + Wave 4 = 0	0.410	0.616
Loans and Training: Wave 3 + Wave 4 = 0	0.114	0.318
Grants: Wave 3 + Wave 4 = 0	0.662	0.452
Grants and Training: Wave 3 + Wave 4 = 0	0.921	0.600

Table D.2: Local average treatment effects on business profits

	(1)	(2)	(3)	(4)	(5)	(6)
	Male and Female	Male and Female	Male	Male	Female	Female
Received Loan	156.9	209.0*	562.6**	617.3**	-78.0	-54.2
	(114.18)	(122.41)	(256.62)	(251.29)	(109.26)	(127.52)
Received Grant	3.98	52.0	-33.3	-80.2	48.8	156.5
	(90.79)	(118.30)	(187.33)	(240.58)	(90.13)	(116.50)
Received Loan * W4		-113.7		-130.8		-56.8
		(136.67)		(287.98)		(139.60)
Received Grant * W4		-103.9		83.8		-224.7**
		(107.71)		(213.08)		(107.68)
Control Mean	371.9	371.9	428.2	428.2	342.9	342.9
R^2	0.0065	0.0061	-0.0095	-0.0073	0.011	0.012
N	5343	5343	2067	2067	3261	3261
Underidentification	9.9e-58	4.7e-50	1.1e-21	7.0e-20	3.3e-36	6.0e-31
Weak Identification	138.0	62.6	56.0	27.3	82.5	37.5
Hansen	0.41	0.097	0.59	0.25	0.48	0.21

Notes: Columns (1) to (4) report the instrumental variables fixed effects local average treatment effect (LATE). Whether the individual took the grant or loan is instrumented by assignment to the interventions. Robust standard errors are in parentheses below the LATE. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. Columns 1 and 2 are for the full sample. Columns 3 and 4 are for male-owned businesses only and columns 5 and 6 are for female-owned businesses only. *** p<0.01, ** p<0.05, * p<0.1.

Table D.3: Determinants of who is behind on loan repayment

	(1) Behind on loan
Loan	-0.0091
	(0.01)
Female	-0.015
	(0.02)
Age	0.018**
	(0.01)
Married	-0.016
	(0.02)
Ability	-0.014**
	(0.01)
Total Employees	-0.0046
	(0.00)
Assets	0.0011
	(0.00)
Profits - 3 Lags	0.000018
	(0.00)
Buikwe	0.065**
	(0.03)
Gulu	-0.080***
	(0.02)
Jinja	0.0034
	(0.03)
Additional Sample	-0.10***
	(0.03)
_cons	0.10***
	(0.03)
R^2	0.079
N	807

74

Notes: Column (1) reports the results of an OLS regression on whether the individual has an outstanding balance on their loan. All variables are from the baseline. Robust p-values are in brackets below the coefficients. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D.4: Treatment effects on business profits by family proximity excluding those who are late on loan repayments

	(1) Far Family	(2) Close Family	(3) Far Family - Male	(4) Close Family - Male	(5) Far Family - Female	(6) Close Family - Female
Loan	190.5**	11.8	191.5	338.9**	149.0**	-192.7*
	(75.96)	(86.39)	(173.74)	(160.54)	(67.65)	(98.54)
Loan and	189.8**	42.1	143.9	298.7*	198.6**	-110.6
Training	(96.47)	(76.21)	(211.39)	(159.14)	(93.74)	(72.45)
Grant	169.0	-4.34	-82.3	186.6	310.9**	-109.3
	(108.54)	(88.27)	(168.81)	(148.59)	(148.43)	(107.43)
Grant and	-80.1	-54.9	-437.3	230.4	124.0	-233.8***
Training	(149.22)	(79.21)	(335.13)	(160.28)	(128.84)	(75.56)
Loan * W4	-140.6	-27.8	-150.4	-215.9	-110.9	102.4
	(90.54)	(84.91)	(221.18)	(159.50)	(77.60)	(93.69)
Loan and	97.0	-31.1	368.9	27.3	-49.7	-74.5
Training * W4	(157.34)	(90.63)	(404.92)	(188.85)	(110.46)	(85.74)
Grant * W4	-197.0*	-180.0*	-12.8	-339.9	-315.2**	-107.9
	(113.60)	(91.98)	(214.75)	(224.47)	(140.46)	(82.54)
Grant and	-7.11	149.9	63.9	269.4	-46.2	50.1
Training * W4	(117.14)	(122.82)	(286.49)	(237.24)	(89.92)	(120.69)
Control Mean	453.3	471.7	453.3	471.7	236.5	435.0
R^2	0.024	0.014	0.042	0.031	0.027	0.028
N	2297	2623	858	1035	1435	1584

Notes: Columns (1) to (6) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. The sample is composed of those who are not behind on their loan repayments and the results are divided by the proximity of family. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys.*** p<0.01, ** p<0.05, * p<0.1.

Table D.5: Effects of trimming the sample

	Male		Female - Far Family		Female - Close Family	
	(1)	(2)	(3)	(4)	(5)	(6)
	No trimming	Trimming	No trimming	Trimming	No trimming	Trimming
Loan	134.4	260.6**	104.0	143.9**	-143.6*	-191.8*
	(225.48)	(113.53)	(73.01)	(65.40)	(83.82)	(100.60)
Loan and	142.1	249.4**	96.6	112.8	-79.8	-109.8
Training	(249.57)	(118.62)	(98.37)	(102.53)	(65.97)	(68.04)
Grant	-558.1*	33.8	330.7**	347.3**	164.1	128.1
	(291.23)	(129.96)	(151.76)	(139.47)	(199.13)	(199.17)
Grant and	-259.6	-184.1	243.7	152.5	-140.7*	-177.9**
Training	(286.64)	(238.04)	(222.91)	(118.77)	(82.68)	(86.25)
Loan * W4	114.7	-170.5	-508.0	-128.9*	34.0	138.9
	(253.72)	(116.10)	(443.21)	(72.70)	(124.53)	(92.83)
Loan and	256.1	80.7	-433.7	-69.1	-130.7	-42.2
Training * W4	(246.44)	(170.07)	(426.97)	(94.37)	(110.21)	(85.32)
Grant * W4	1278.4	-89.1	-834.2**	-365.9***	-354.1*	-191.6
	(914.57)	(139.26)	(347.72)	(132.22)	(208.77)	(141.97)
Grant and	258.9	199.2	-601.1	-114.8	-25.7	80.2
Training * W4	(288.13)	(202.33)	(414.27)	(102.43)	(145.25)	(116.30)
R^2	0.023	0.022	0.020	0.025	0.027	0.030
N	2253	2069	1896	1652	1668	1609

Notes: Columns (1) to (6) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. Columns 1, 3 and 5 are the full, untrimmed sample, while columns 2, 4 and 6 are the results after dropping the top and bottom 0.5% outcomes and dropping the baseline values for individuals never found. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D.6: Population weighted treatment effects on business profits

	(1) Male ITT	(2) Male PATE	(3) Female ITT	(4) Female PATE
Loan	260.6**	212.4*	-40.9	-53.5
	(113.53)	(113.50)	(61.85)	(65.49)
Loan and	249.4**	223.7*	-13.2	-35.6
Training	(118.62)	(117.57)	(60.34)	(64.60)
Grant	33.8	-2.90	229.0**	225.9*
	(129.96)	(129.87)	(116.20)	(118.91)
Grant and	-184.1	-246.1	-6.99	32.2
Training	(238.04)	(257.24)	(83.42)	(104.08)
Loan * W4	-170.5	-159.2	10.4	18.8
	(116.10)	(118.89)	(62.23)	(65.23)
Loan and	80.7	62.4	-52.2	-23.0
Training * W4	(170.07)	(173.06)	(65.02)	(69.82)
Grant * W4	-89.1	-119.4	-282.4***	-288.1***
	(139.26)	(139.72)	(99.23)	(101.48)
Grant and	199.2	220.6	-27.8	-49.6
Training * W4	(202.33)	(220.14)	(79.57)	(98.63)
Control Mean	428.2	405.8	342.9	343.2
R^2	0.022	0.023	0.021	0.021
N	2069	1871	3261	2874

Notes: Columns (1) to (4) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. Columns 1 and 2 are for men only while columns 3 and 4 are for women only. Columns 1 and 3 present the ITT effect while columns 2 and 4 are the population weighted effects, where the sample is reweighted based on predicted values of being in the sample from the full population of businesses in the baseline. The probability is estimated using a Probit model. *** p<0.01, ** p<0.05, * p<0.1.

Table D.7: Treatment effects on business practices

	Plan		Marketing		Advice from Others	
	(1)	(2)	(3)	(4)	(5)	(6)
	Male	Female	Male	Female	Male	Female
Loan	0.038	0.0060	-0.062	-0.026	0.26	-0.15
	(0.03)	(0.02)	(0.07)	(0.04)	(0.26)	(0.22)
Loan and	-0.0012	-0.019	0.089	0.055	0.42	0.092
Training	(0.03)	(0.02)	(0.07)	(0.05)	(0.30)	(0.33)
Grant	0.020	0.054*	-0.13	-0.033	0.37	-0.20
	(0.04)	(0.03)	(0.08)	(0.05)	(0.32)	(0.30)
Grant and	0.019	0.0067	0.075	0.040	1.07**	-0.29
Training	(0.04)	(0.03)	(0.08)	(0.05)	(0.44)	(0.26)
Loan * W4	-0.012	-0.013	0.11	-0.0078	-0.65	0.018
	(0.04)	(0.03)	(0.10)	(0.06)	(0.46)	(0.36)
Loan and	0.039	0.034	-0.011	-0.068	-2.24*	-0.27
Training * W4	(0.04)	(0.03)	(0.10)	(0.07)	(1.34)	(0.41)
Grant * W4	0.040	0.0049	0.088	0.13	-0.41	-0.28
	(0.05)	(0.03)	(0.12)	(0.08)	(0.74)	(0.44)
Grant and	0.044	0.0024	0.011	0.050	-0.81	0.19
Training * W4	(0.05)	(0.04)	(0.12)	(0.08)	(0.78)	(0.45)
Control Mean	0.95	0.96	0.36	0.27	1.34	1.39
R^2	0.057	0.044	0.039	0.076	0.031	0.024
N	913	1471	887	1418	920	1483

Notes: Columns (1) to (6) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business practices outcomes. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D.8: Treatment effects on business profits by region

	(1) Central	(2) North	(3) Central - Male	(4) North - Male	(5) Central - Female	(6) North - Female
Loan	204.2**	-32.4	436.1**	73.6	14.0	-78.2
	(90.16)	(73.19)	(193.59)	(98.78)	(67.67)	(102.15)
Loan and	91.6	85.2	244.9	251.7**	-18.0	6.44
Training	(98.58)	(64.14)	(199.03)	(112.80)	(95.75)	(74.91)
Grant	251.6**		191.4		282.6**	
	(104.70)		(181.76)		(121.33)	
Grant and	13.4		-60.7		40.5	
Training	(107.99)		(231.38)		(84.83)	
Loan * W4	-49.2	-86.2	-206.5	-189.7	85.7	-39.5
	(84.77)	(82.57)	(177.95)	(154.24)	(71.42)	(97.22)
Loan and	141.9	-140.7	199.7	-103.6	109.0	-188.1**
Training * W4	(115.74)	(94.22)	(275.12)	(202.13)	(83.58)	(93.39)
Grant * W4	-166.6*		-114.3		-207.8**	
	(91.96)		(175.50)		(99.92)	
Grant and	110.0		207.5		50.6	
Training * W4	(94.22)		(201.56)		(78.52)	
Control Mean	332.6	404.9	448.1	403.2	250.4	405.6
R^2	0.018	0.032	0.032	0.044	0.028	0.036
N	3377	1968	1364	705	2002	1259

Notes: Columns (1) to (6) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. The results are divided between the central region (Mukono, Buikwe and Jinja) and the northern region (Gulu). Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D.9: Treatment effects on business profits by baseline profit level

	(1) Male - Low Profits	(2) Male - High Profits	(3) Female - Low Profits	(4) Female - High Profits
Loan	181.2	315.3*	21.0	-130.2
	(137.53)	(168.36)	(48.09)	(129.82)
Loan and	86.2	368.7**	109.3*	-153.1
Training	(163.75)	(168.12)	(62.32)	(104.34)
Grant	-170.9	196.6	357.7**	67.9
	(123.43)	(219.07)	(167.64)	(153.23)
Grant and	23.0	-411.5	106.8	-109.9
Training	(146.73)	(441.37)	(102.01)	(127.88)
Loan * W4	-203.1	-188.0	-27.3	43.5
	(163.17)	(165.61)	(73.99)	(105.66)
Loan and	434.3	-220.8	-130.2	37.4
Training * W4	(293.60)	(177.27)	(87.57)	(96.10)
Grant * W4	32.9	-245.0	-400.8***	-139.5
	(165.43)	(230.51)	(135.05)	(141.78)
Grant and	-85.7	441.4	-24.5	-23.1
Training * W4	(192.57)	(350.69)	(101.35)	(122.45)
Control Mean	301.5	535.3	222.6	252.9
R^2	0.077	0.036	0.10	0.019
N	899	1170	1736	1525

Notes: Columns (1) to (4) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. The results are divided by level of profit. For each gender, businesses with the bottom 50% of profits in the baseline are called low profit, while those in the top 50% are called high profit. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D.10: Treatment effects on business profits by whether an individual had a loan previously

	(1)	(2)	(3)	(4)
	Male previous loan	Male no loans	Female previous loan	Female no loans
Loan	-62.6	398.7***	-48.0	-37.4
	(147.33)	(151.88)	(66.10)	(108.96)
Loan and	190.9	234.7	0.95	-74.1
Training	(165.56)	(156.64)	(82.81)	(79.68)
Grant	-58.2	40.6	347.4*	70.8
	(184.63)	(166.21)	(190.15)	(114.79)
Grant and	-106.9	-337.5	-14.1	-27.3
Training	(184.41)	(399.33)	(117.57)	(111.06)
Loan * W4	-52.7	-256.7*	42.4	-26.9
	(193.41)	(142.39)	(76.36)	(102.60)
Loan and	-126.7	164.7	-24.4	-40.1
Training * W4	(218.97)	(247.23)	(85.74)	(89.57)
Grant * W4	-1.65	-235.3	-386.0**	-140.9
	(217.80)	(184.42)	(163.81)	(93.69)
Grant and	243.9	128.9	-39.0	2.88
Training * W4	(266.89)	(304.25)	(103.89)	(124.03)
Control Mean	471.1	367.3	347.8	338.1
R^2	0.058	0.028	0.031	0.018
N	799	1250	1786	1450

Notes: Columns (1) to (6) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. The results are divided between individuals that had a loan previously and those who never had a loan. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D.11: Treatment effects on business profits by baseline ability and patience levels

	Low Ability		High Ability		Low Patience		High Patience	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male	Female	Male	Female	Male	Female	Male	Female
Loan	71.8	25.1	371.4**	-145.2	294.4**	-106.1	217.0	28.7
	(105.66)	(48.82)	(185.89)	(145.74)	(121.85)	(107.11)	(199.50)	(57.14)
Loan and	0.41	55.6	398.4**	-113.2	203.8*	-93.8	312.6	78.5
Training	(110.45)	(77.22)	(194.59)	(98.59)	(111.90)	(99.90)	(216.38)	(64.60)
Grant	-238.7	244.2*	226.4	183.8	45.6	305.1	26.7	108.6
	(153.48)	(135.87)	(208.04)	(196.72)	(86.73)	(202.15)	(291.64)	(97.44)
Grant and	-757.1	20.4	130.7	-60.3	9.13	-45.5	-411.9	-20.1
Training	(578.28)	(89.09)	(202.87)	(163.01)	(101.92)	(99.96)	(551.26)	(133.78)
Loan * W4	75.3	-32.9	-299.1	80.4	-192.0	31.1	-139.4	-13.4
	(135.41)	(65.43)	(183.10)	(122.10)	(148.47)	(105.59)	(179.06)	(67.61)
Loan and	144.5	-73.3	86.9	-18.3	113.2	-75.4	-45.9	-15.2
Training * W4	(186.06)	(84.91)	(272.08)	(103.81)	(214.83)	(105.39)	(223.51)	(82.16)
Grant * W4	365.2*	-286.7***	-411.0**	-248.1	-267.8	-380.7**	61.0	-179.0*
	(194.47)	(102.77)	(207.46)	(178.04)	(169.01)	(165.44)	(238.09)	(106.87)
Grant and	661.0	-46.8	-57.3	-2.08	-207.1	33.6	665.9	-115.0
Training * W4	(469.49)	(82.59)	(193.55)	(160.09)	(153.46)	(104.08)	(422.03)	(130.99)
Control Mean	372.7	292.7	478.2	416.9	320.7	350.3	533.5	336.0
R^2	0.042	0.031	0.035	0.022	0.033	0.026	0.040	0.034
N	848	1932	1221	1329	1154	1790	915	1471

Notes: Columns (1) to (8) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. The samples are split for the top and bottom half of business owner ability and patience measures. Ability was measured at the baseline while patience was measured during the first follow-up survey. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys.*** p<0.01, ** p<0.05, * p<0.1.

Table D.12: Treatment effects on business profits by risk preferences

	(1) Male - High Risk	(2) Male - Low Risk	(3) Female - High Risk	(4) Female - Low Risk
Loan	117.0	405.7***	2.41	-75.9
	(180.37)	(127.98)	(94.86)	(76.01)
Loan and	136.7	453.7**	86.0	-121.5
Training	(178.07)	(194.58)	(77.30)	(97.83)
Grant	30.0	66.1	330.3	128.8
	(245.43)	(103.78)	(226.41)	(139.73)
Grant and	-424.1	32.3	132.1	-108.4
Training	(537.84)	(119.51)	(179.08)	(95.20)
Loan * W4	-0.24	-310.1	-79.4	111.6
	(164.58)	(208.77)	(95.39)	(87.55)
Loan and	150.3	123.1	-157.5	46.6
Training * W4	(165.10)	(440.19)	(104.55)	(87.51)
Grant * W4	88.6	-223.6	-533.7***	-74.3
	(199.52)	(211.29)	(176.90)	(125.33)
Grant and	462.5	7.91	-215.6	120.7
Training * W4	(390.65)	(211.38)	(197.79)	(74.61)
Control Mean	517.9	325.0	317.1	383.7
R^2	0.023	0.053	0.022	0.030
N	1029	840	1458	1557

Notes: Columns (1) to (4) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. The results are divided between individuals that have risk preferences above (high risk individuals) and below (low risk) the median level. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** p<0.01, ** p<0.05, * p<0.1.

Table D.13: Treatment effects on business profits by age of firm

	(1) Male - Young Business	(2) Male - Old Business	(3) Female - Young Business	(4) Female - Old Business
Loan	502.4***	34.2	79.8	-141.1
	(184.93)	(137.94)	(80.49)	(91.38)
Loan and	338.3*	158.5	-37.3	1.77
Training	(175.67)	(153.64)	(91.93)	(79.92)
Grant	248.8	-150.3	98.5	361.6*
	(180.93)	(222.18)	(101.46)	(210.37)
Grant and	277.6	-578.9	109.1	-116.1
Training	(186.70)	(407.55)	(113.39)	(116.86)
Loan * W4	-251.3	-42.9	-86.3	83.1
	(178.02)	(157.80)	(108.47)	(76.52)
Loan and	-233.9	362.8	-84.6	-30.0
Training * W4	(167.00)	(255.04)	(104.58)	(82.88)
Grant * W4	-177.1	4.69	-153.7*	-416.3**
	(160.72)	(256.65)	(89.43)	(181.34)
Grant and	-305.4*	666.2*	-36.2	-26.5
Training * W4	(164.85)	(349.49)	(112.57)	(113.23)
Control Mean	367.8	478.4	297.9	374.1
R^2	0.037	0.048	0.047	0.025
N	917	1152	1401	1860

Notes: Columns (1) to (4) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions on business profits. The results are divided between individuals whose business age is less than (young business) or greater than (old business) two years. Robust standard errors are in parentheses below the ITT. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** p<0.01, ** p<0.05, * p<0.1.

Table D.14: Controlling for enumerator effects

	(1) Male	(2) Female
Loan	420.4**	75.2
	(190.53)	(68.06)
Loan and	483.2**	61.4
Training	(235.72)	(94.80)
Grant	232.5	301.9**
	(195.02)	(123.74)
Grant and	-83.6	130.0
Training	(250.13)	(88.75)
Loan * W4	-214.6	-27.5
	(130.77)	(62.34)
Loan and	-47.5	-58.4
Training * W4	(179.65)	(64.93)
Grant * W4	-99.4	-220.7**
	(147.43)	(103.29)
Grant and	175.2	-35.3
Training * W4	(198.46)	(78.81)
Control Mean	428.2	342.9
R^2	0.084	0.093
N	1642	2503

Notes: Columns (1) and (2) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four interventions and enumerator characteristics on business profits. Enumerator dummies, not shown, are included in both specifications. Robust standard errors are in parentheses. All fixed effects analysis includes wave and month dummies and a control for the time between surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.