

Rethinking the microfinance model: Returns to subsidized microcredit for male and female entrepreneurs in Uganda¹

Nathan Fiala²

December 2015

Abstract

Experimental tests of microcredit programs have consistently failed to find effects on business and household income. Does the current microfinance model and targeting of clients miss important effects from finance? I present results of a randomized experiment with microenterprise owners in Uganda that sought to expand access to finance for men and women who generally did not qualify for finance under normal circumstances with the goal of increasing business profits and employment. Participants were offered either capital with repayment (subsidized loans) or without (grants) and were randomly chosen to receive or not receive business skills training in conjunction with the capital. Consistent with existing literature, I find no effect for female enterprises from either form of capital or the training. However, I find large effects for men with access to loans combined with training. There is no effect for men or women from the grants, suggesting repayment requirements can increase the likelihood of productive investment. I also find little evidence that investing capital and training in a few enterprises crowds out other businesses. The results indicate that cash-constrained male-owned enterprises—a sample that is not well targeted by microcredit organizations or researchers—can benefit from subsidized finance, and that this may have larger, positive income and employment growth effects for an economy.

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

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

¹ Acknowledgments: Funding for data collection and analysis came from the Youth Employment Network (YEN), the International Growth Center, and a Marie Curie European Fellowship. I would like to thank PRIDE Microfinance of Uganda and the Ugandan ILO office for implementing the interventions. I also thank Christopher Blattman, Susan Steiner, Silvia Paruzzolo, Susana Puerto-Gonzalez, and participants of the NEUDC conference for helpful comments. Filder Aryemo, Fang Duan, Laurin Janes, Xi He, Cormac Mangan, and Stephen Okot provided superb research and data collection assistance. All findings and interpretations in this paper are those of the author and do not necessarily represent the views of YEN, PRIDE, or the ILO.

² University of Connecticut, 1376 Storrs Road, Storrs, CT 06269, nathan.fiala@uconn.edu.

1 Introduction

Microenterprises are common in countries with limited formal employment options. While there is often hope that informal enterprises can provide much-needed employment opportunities for communities and income for business owners, the evidence on enterprise expansion in developing countries suggests these businesses do not develop into larger enterprises or provide much more than subsistence-level income. Research on business expansion shows only a small number of firms upgrade into larger businesses, leading to doubts that microenterprises can generate general economic or employment growth (Berner, Gomez, and Knorringa 2012; Fajnzylber, Maloney, and Rojas 2006; Fajnzylber, Maloney, and Rojas 2009; and Mead 1994).

There are a number of reasons posited by researchers and policy makers for this lack of growth. Missing human capital, specifically management skills needed to handle increasing cash flow, can make expansion difficult for many business owners. However, the majority of studies on business skills training fail to find an effect on sales and profits from these trainings (Mobarak et al. 2012; Karlan and Valdivia 2011; Bjorvatn and Tungodde 2012; Gine and Mansuri 2011; Karlan, Knight, and Udry 2012).

Credit constraints, a major problem in almost all developing countries, may also constrain business development. Firms that don't have access to finance cannot properly invest in their enterprises and may be trapped in a lower-growth path. Cash transfers, which inject capital without repayment requirements, produce large returns to businesses, though recent evidence suggests cash can be difficult for businesses to use effectively (de Mel, McKenzie, and Woodruff 2008; Fafchamps et al. 2013; Berge et al. 2012). A more common approach is microcredit,

which, being a self-sustaining private-market solution, has penetrated many extremely poor areas. However, recent experimental work fails to find an effect from microcredit on enterprise and income growth (Augsburg et al. 2015; Banerjee et al. 2015; Fischer 2012; Gine and Mansuri 2011).

I present evidence that the lack of effect from microcredit may be partially due to the mission of microcredit organizations. These organizations traditionally target poor women, a group that does not often operate businesses that can easily be expanded. The women in the present sample, similar to women in other developing economies (Clark 1994), expressed during qualitative interviews how they often have strongly defined community and household roles—such as child care and household chores—that severely limit their ability to utilize cash for their businesses. This focus on women has been the standard from the beginning of the microcredit movement and is normally justified for two reasons: (1) they are the poorest, most underserved group in many developing countries, and (2) they are seen by investors as more likely to repay loans³ (Yunus 2003). Researchers have likewise focused for the most part on women borrowers.

In this study I explore how capital and skills constraints affect business growth while also focusing on an understudied population: male business owners. From a selection of semi-urban businesses across Uganda that expressed interest in accessing finance, I randomly selected a sample of business owners to receive capital. Participants were offered either loans (capital requiring repayment) or grants (capital not requiring repayment). Furthermore, some participants were offered a loan paired with business skills training or a cash grant paired with business skills

³ The men and women in this sample do not have different repayment rates. While it is taken as a general truism that women repay more reliably, to my knowledge there is no empirical evidence that this is true.

training. In addition, a random sample was chosen to be part of a control group whose members were offered neither capital nor training. Unknown to the study participants, the loans were subsidized in order to induce the microcredit organization to take on clients with whom they would not normally work, including primarily men and those without credit history or full collateral.

This design allows for a test of the effects of infusions of capital on business outcomes depending on whether the capital does or does not require repayment, as well as the marginal effects of business skills training on a capital drop. In a simple model, I discuss how applying conditionality to capital can improve the likelihood of investment in a business over providing unconditional capital. I also discuss whether business skills training can affect this investment, either through improved knowledge or attitudes; the role of individual attributes that may affect the efficiency of how capital is used, including individual patience, ability, and risk preferences; and how these effects may differ by gender.

The sample is composed of 1,550 microenterprise owners selected from a sample of owners who expressed interest in expanding their business and receiving trainings and loans. This selection was done to ensure the businesses are directly comparable across treatments and to increase take-up rates, which are generally very low in microcredit studies. The businesses come from a wide range of sectors, including hair salons, retail shops, and tailors. They are representative of the types of microenterprises one would find across Uganda and sub-Saharan Africa, though not necessarily representative of the owners of such businesses as selection into the sample was driven by interest in the programs. The sampling procedure thus produces a

comparable sample that is appropriate to determine the effect of capital on business outcomes for those who are interested in obtaining capital.

Data on business returns are noisy. In order to test for changes over time and improve on statistical power, the survey team conducted multiple data collections on the businesses.⁴ The analysis presented here utilizes two baseline surveys before the programs were delivered, along with two follow-ups conducted six and nine months after the treatment completion.

The program was successful in expanding access to loans. Men and women offered finance have on average 50% more loans than those not offered the loans. Consistent with a well-developed literature, I find no effects on business profits from any of the treatments during any of the data collections for female-owned enterprises. However, I find that men with access to loans—especially when paired with training—report 54% greater profits in the last month. The effects increase slightly over time and are strongest for those with higher ability, lower risk preferences, and no prior history of loans. I find an initial impact for men from the loans without the training, but this effect disappears by the second follow-up. There is no effect on business profits from grant treatment, with or without training. A differential effects test of the programs strongly rejects equality.

The growth in business profits for men is due to increased revenues, which appears to come from a combination of increased employment and capital accumulation. An estimate of the returns to employment and capital suggests that increasing employment has large and significant

⁴ This was done following de Mel, McKenzie, and Woodruff (2009) who show that, when autocorrelation across periods is low, increased number of data collections can greatly improve statistical power. They cite microenterprises in Africa as an example of a good case for multiple follow-up data collections.

implications for the profits of the business. I check the sensitivity of these results to attrition, spillover, and Hawthorne effects and find the results are unlikely to be sensitive to these issues.

The results for men, especially those who do not have a history of taking loans, are consistent with credit constraints. The grants' lack of effect suggests repayment requirements can increase the likelihood of productive investment in the business compared to receiving free cash. While loans paired with training had large effects on male-run businesses, this did not translate into observed impacts in households: I find no changes in spending on child health, general savings, or household consumption.

This paper presents three main contributions. First, I confirm the lack of effects for microcredit on female-run business outcomes but show that microcredit can present significant benefits for male-run businesses. There is a well-developed literature on the effect of microcredit on existing businesses for enterprise and household outcomes. While Field et al. (2013) find that a grace period for loan repayment for women leads to a positive and significant effect on profit, the majority of work has found little or no effect of microcredit on enterprise growth or income (Banerjee et al. 2015; Fischer 2012; Augsburg et al. 2015; Gine and Mansuri 2011) or on household consumption growth (Crepon et al. 2015; Desai, Johnson, and Tarozi 2013; Angelucci, Karlan, and Zinman 2013). However, these studies have focused almost exclusively on female-owned enterprises, with few if any male-owned enterprises in the samples. Of the seven studies included in a special issue of the *American Economic Journal: Applied* on microcredit experiments, only three include a substantial number of men. Of those three, Augsburg et al. (2015) is the closest to this study as they include existing male and female

businesses. They find no effect on total individual or household income. However, they do not look at the difference in impact between men and women and their sample is also from Bosnia and Herzegovina. In the current study I look at the differences between men and women and find that the differential impacts are important.

Second, I am able to directly compare the effect of offering capital with repayment (loans) and without (grants).⁵ While the literature on microcredit is well developed, there is less evidence as to whether grants can impact business growth. Cash appears to have large effects for starting a business, especially for women (Blattman, Fiala and Martinez, 2014). For existing businesses, de Mel et al. (2008) find large returns from cash grants for male-led businesses, while Haushofer and Shapiro (2013) find more modest effects on enterprise income. I find no results from cash grants for male- or female-owned enterprises. Comments during qualitative interviews suggest both groups used the money for short-term consumption needs rather than investment. The requirement for repayment appears to have induced men to use the money for investment.

Finally, I am able to explore how human capital development can affect business outcomes in the presence of relaxing capital constraints. There is reason to believe business owners are missing a number of skills and leaving profits on the table. For instance, Bloom et al. (2013) find large returns for Indian textile firms when given management services. The majority of studies on micro and small businesses, though, find positive impacts of business skills

⁵ To the best of my knowledge, this is the first paper to directly compare the effects of *business* loans versus cash grants. The only other study I am aware of that tests the concept is Beaman et al. (2014), who compare grants and loans for farmers.

trainings only on knowledge and attitudes, with little or no impact on profits and sales (Mobarak et al. 2012; Karlan and Valdivia 2011; Bjorvatn and Tungodde 2012; Gine and Mansuri 2011; Karlan and Valdivia 2011; Berge et al. 2012). However, Calderon et al. (2013) do find large profit effects from an intensive training program in Mexico. There is thus plenty of reason to doubt the value of business skills trainings for existing microenterprises. I find that training does have positive marginal impacts for male business owners. While I am unable to explicitly test for why this is the case, the results suggest that training can improve business performance when combined with loan capital.

The results suggest small-scale, subsidized capital can improve business returns and lead to economic growth, even among the smallest enterprises. However, the results presented here and from the existing literature suggest this effect is limited to the group that is currently underserved in such finance: male-owned enterprises. In the majority of sub-Saharan households, women face significant restrictions on how they can use capital, while men do not face these restrictions. In order for microcredit to improve welfare and reduce poverty, it needs to expand beyond traditional clients and target those who are better able to utilize business capital.⁶

This paper proceeds as follows. I discuss the experimental design in Section 2. In Section 3, I describe how male- and female-owned enterprises may differentially benefit from loan, grant, and training programs. I present the data in Section 4 and the results in Section 5. I explore whether there are spillover effects from the treatments in Section 6 and conclude in Section 7.

⁶ It is also possible that microcredit may work for women who do not face constraints to capital usage. However, as I discuss in Fiala (2015), at least for the context of Uganda, this constraint appears to be strongly grounded in the family system and so is not easily relieved.

2 Experimental design

2.1 Sample selection

I selected the enterprises I study here from a census of businesses operating in four districts in Uganda. I hired and trained a survey team that interviewed 4,637 business owners spread across the central and northern regions of the country. The team gathered information on profit levels, business size, entrepreneurial ability, interest in training and loans, and general owner demographics. I selected those business owners who expressed an interest in the International Labour Organization (ILO) training and loan program for an additional short baseline survey in which they were asked about their interest a second time. The final sample is thus composed of the 1,550 individuals who twice expressed interest in trainings and loans. Expression of interest is important, both to increase take-up of the loans, which is often low, and to ensure the sample receiving the cash grants is directly comparable to the loan group. I describe these individuals and how they are different from the census population in detail in section 4.2.

I randomly sorted individuals into five categories, presented in figure 1: 406 were assigned to the loans treatment; 401 to the loans and training; 167 to grants; 219 to grants and training; and 357 to the control group. The sample sizes were based on power calculations assuming multiple data collections and taking into account implementation budget limitations. Stratification was done by region but not for any other characteristic due to program implementation beginning before the baseline data was fully analyzed. The loan programs were run in both the north and central regions of the country, but the grant programs, due to budget limitations, were only run in the central region.

Figure 2 presents the business types by frequency for the study population, divided by male and female owners. The majority of businesses were hair salons, followed by retail shops and tailors. The sample businesses are of the general types of businesses found in Uganda, as well as much of sub-Saharan Africa, though are not representative. The distribution of businesses among men and women is not equal but follows similar patterns. Importantly, female and male business owners are present in the majority of business types.

2.2 Treatments

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 of \$200, (4) those who were offered business skills training and a loan of between \$180 and \$220, and (5) a control group. The same divisions were made in the northern region but without the grant groups.

A local microfinance organization, PRIDE Microfinance, provided the loans. Unknown to the participants, the loans were guaranteed by the ILO. Only the director and top main office staff of PRIDE knew about the guarantee. Businesses that expressed interest in a loan, including over half that had never taken a loan before, made up the sample; however, these businesses did not always fit the lending requirements of PRIDE. A guarantee helped mitigate this risk. Many NGOs and governments are taking interest in this way of expanding access to microcredit.⁷

⁷ The African Guarantee Fund for Small and Medium-sized Enterprises (<http://www.afdb.org/en/topics-and-sectors/initiatives-partnerships/african-guarantee-fund-for-small-and-medium-sized-enterprises/>) is one example of large funds being developed by governments and cross-national institutions to expand financial coverage.

PRIDE normally provides loans with an interest rate of 26% and requires 100% collateral. For this study, the lender 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 the ILO saw as a potential design for future treatments. Individuals were required to repay the loan in a year in monthly installments, starting in the first month, per standard microcredit requirements. There were some delays in the rollout of the loan program. The distribution of loan density by month is presented in the online appendix. A regression on loan disbursement dates and profits suggests that the date of loan delivery is not correlated with outcomes. However, all analysis includes dummies for when the treatment was delivered.

The ILO conducted the trainings in August and September 2012 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). Researchers evaluated these trainings experimentally at least twice before. Mano et al. (2012) looked at the effect of giving training to 53 business owners. In keeping with other training results, they found business survival rates increased, as did the incidence of good business practices such as keeping budgets, but found no consistent effects on business profit. de Mel et al. (2008) also used the SIYB training in a study that offered business training and cash grants to women in Sri Lanka. They found no effect from the training on profits for those already in business, but there was some increase in the number of women entering business who had not

owned a business previously. Due to the lack of effect from training on business profits, the main outcome of interest here, I decided to pursue trainings as a potential augmenting effect on capital, not as a stand-alone treatment.

For this study, the ILO delivered the cash grants through PRIDE bank accounts from the middle of October to early November. The ILO then contacted individuals to attend information meetings explaining how the cash grant program worked. They were then asked to open a free savings account where the money would be deposited. It is not possible as part of this design to separate the effect of receiving a cash grant from that of having a savings account.

2.3 Regional differences

The design takes advantage of regional differences in Uganda to explore both the generalizability of the results and the potential differential effects of the programs by area. There are significant differences 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 experienced significant growth, with expanded trade from both Sudan (north of Uganda) and Kampala, Uganda's capital, helping to fuel this expansion. Increased income has also meant increased interest in market interactions, with the main trading town in the north, Gulu, growing quickly.

In contrast, the central region experienced 25 years of peace. Economic and market growth have been steady throughout this time period, with robust trade with Kenya, Uganda's main access to seaports. The businesses in the central region are also larger than those in the north. From the baseline census, central-region 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.

3 The role of capital in male and female microenterprises

Business owners everywhere face a number of constraints to expansion of their enterprises.

Access to capital is a major problem for most, especially in developing economies.

Microenterprises, in particular, are heavily reliant on the characteristics and whims of the owner.

Thus, possible individual constraints for microenterprises can come from an owner lacking necessary business skills and ability or having family pressure to spend money outside of the business. In this section I discuss these constraints through a simple self-employment model, similar to de Mel et al. (2008), and explore how gender affects each. The model is presented formally in the online appendix.

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 complements, such that $f_{Kl} > 0$. The entrepreneur maximizes utility, $U(c, L-l)$, 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 investment constraints, 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 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 is then 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 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.

Individuals may also be impatient or face strong pressures for spending outside of the business. Family pressures to spend on extended household consumption are especially strong in Africa and developing countries in general (Townsend 1994; Kocherlakota 1996; Jakiela and Ozier 2013; Grimm et al. 2013). If an individual under such pressures receives a shock to capital K , she will not invest the money into the business optimally. Instead, some, or perhaps the entirety, of the windfall will be taken for immediate purchases or to fulfill household needs.

Therefore, investment in the business will be suboptimal and equilibrium returns will not be reached. However, if this money is constrained in a way that it needs to be invested, such as a conditional transfer or as a loan that must be repaid, individual business owners may be forced into committing to an investment in the business. Whether this commitment is enough to overcome the effects of pressures to spend quickly will depend on the relative size of these pressures and the ability of the individual to resist them.

If ability to resist such pressures, θ , is a strong constraint, the right training may be able to increase the entrepreneur's ability in this area and thus increase returns to the business. Ability changes could lead to better management of the business, such as improved cash flow or employee oversight, meaning investment into the business could be better optimized. Training may not only affect ability, but could also lead to changes in attitudes regarding investment in the business. However, improved ability or attitudes toward the business, though necessary, may not be sufficient if there is not enough investment capital available to take advantage of the new skills. That is, θ and K are not separable. In this case, trainings alone may not have much effect on business outcomes, but instead work only in conjunction with increased capital availability.

As the objective of the business owner is to optimize utility, there may also be a constraint to business expansion due to effects on leisure time, $L-l$. The owner may thus purposefully restrict investment if it leads to greater time commitments. One solution could be to expand the number of employees working in the business, though for some business owners this could be difficult if they do not have access to reliable workers or do not know how to manage that labor.

Finally, it is possible that capital, family, ability and time constraints may work differently for men and women. During qualitative interviews, women in the present sample often expressed their role in the household as being key for providing food, clothing, and other household needs for themselves and their children, while men often didn't worry as much about day-to-day operations of the household. In most societies in Africa, Asia, and Latin America, men and women have strongly defined roles in the household. Men are often relatively unconstrained to conduct business activities and can take family members as workers and household cash for the business. Women, on the other hand, are frequently very constrained: when they are able to work outside the home, they may still be responsible for household chores; spending on family needs such as clothing, schooling and health; and are last to be able to use household assets for their own business.⁸

The discussion on business constraints presented here leads to a number of questions that we can test in the current experimental design. The first question is whether this population faces credit constraints that restrict investment into their business. If so, we would expect to see returns from the cash grant and loan treatments, especially for those who are the most capital constrained (i.e., those who have not had access to finance previously). However, if there are constraints to utilizing capital, either individual or family, it is possible conditionality to capital can improve the likelihood of investment in the business over unconditional capital. If capital and ability are not separable constraints, we may expect to see an effect from the inclusion of business skills training on business outcomes, either through improved ability or attitudes. Finally, this

⁸ Clark (1994) presents an interesting discussion about how female market sellers in Ghana balance household constraints while managing their business.

discussion suggests these constraints may affect men and women differently. The common finding in the literature shows little effect for women. However, because men are freer to use capital as they want, this group is more likely to be positively affected by capital.

4 Data

Baseline surveys of the business owners were conducted in February 2012 (wave 1) and May 2012 (wave 2). Individuals then received the treatments 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 treatments, respectively. All data collection was conducted by an independent research team led by the author. No one on the data collection team was affiliated in any way with the ILO or PRIDE Microfinance.

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 program take-up and 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 who are included in the final sample are presented in Table 1, 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 keeping 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 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. I designed the survey questions for profits and revenue after the findings of McKenzie (2012) who shows that directly asked profits and revenues are less biased than other measures, such as calculating profits from revenue and expenses or mark-up rates. The survey team asked business owners for the last month’s their total profits and revenues. I have adjusted each for inflation across all of the data collections, with the first baseline being the base period.

The survey team also asked business owners 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 could repeat four numbers back. Finally, the team asked four math questions, though most business owners were able to respond correctly to all four. 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. I then normalized the index again.

Before asking the business owner whether he or she wanted loans and training, business owners answered whether 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 having reported the same (53%), likely reflecting that microcredit is traditionally targeted toward women.

To develop an asset index, the respondent reported a range of assets. This was used in principal component analysis and normalized at 0. There is significant variation in the number of items people own, with men having greater assets than women.

I present the results of a balance test for treatment assignment in the final columns of Table 1. The results suggest that randomization worked well. In expectation, 10% of the variables should be significant at the 90% level or better while, 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, sex of business owner and change in baseline values is presented in the Online Appendix. The results of these balance tests suggest there was strong balance across all groups.

I discuss in detail selection into the sample 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 treatments. 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 Program take-up

As is common in the literature, while businesses expressed strong interest in the programs, take-up was not universal⁹. I briefly describe here take-up into the programs. A full analysis of 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. While every effort was made to make people comfortable with the program, 30% of people were not convinced¹⁰.

Selection for take-up into the programs, also shown in the Online Appendix, does not appear to be strong. The largest predictor of take-up for loans and grants is whether the person

⁹ See Karlan, Morduch and Mullainathan (2010) and McKenzie and Woodruff (2012b) for discussions of take-up rates in microcredit and training studies, respectively.

¹⁰ I have also explored using baseline values to predict who becomes a borrower and interacting this with the grant program. These results (not shown) do not change the effect of the grant. While there is certainly selection into grant take-up, it is likely not affecting the ability to observe differences in effects from the loan and grant programs.

was offered and attended the trainings. Only past experience with having attended trainings predicts whether a person attended the offered training.

Finally, I test for whether the programs increased the number of loans individuals take. On average both men and women, show statistically significant and positive effects for the loan only and loan with training programs on number of loans. Men report 0.47 and 0.56 more loans than the control group, respectively, while women report 0.52 and 0.39 more loans, respectively. The program was designed to increase access to finance among a group of people that generally do not qualify for finance, either due to a short credit history, missing collateral or concerns over the size of the loans relative to profit levels. The program appears to have successfully increased this access.

4.3 Survey attrition

The survey team made significant efforts to track businesses during the follow-up data collections. As the business owners were busy, the survey was kept short at approximately 30 minutes. 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 (93%). Not all of the business owners we found were willing to tell us their profits or other information: I 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 (e.g., Blattman et al., 2014; Baird et al. 2011).

Table 2 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. We were more likely to find older people at waves 3 and 4, as well as those with higher ability measures and more baseline employees. Selection is negatively associated with baseline assets. We had an easier time finding people in the grant-only, loan-only and grant-with-training treatments 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 is 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.

This analysis suggests 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 attriters that might bias the results. To help minimize the effects of this selection, I conduct a bounding exercise, described in the Online Appendix, to test the strength of the results on different assumptions about the missing sample. I find the results are robust to moderate assumptions about the attritted sample.

5 Results

In order to test the questions posed in Section 3, I estimate the following intention to treat (ITT) fixed effects regression model:

$$Y_{it} = \alpha + \beta T_{it} + \theta T_{it} * \gamma_t + \gamma_t + \eta_i + \varphi R + \mu W + \delta M + \varepsilon_{it} \quad (1)$$

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 robust¹¹. Following de Mel et al. (2008), I conduct fixed effects estimation to take maximum advantage of the high frequency of data collection.

All analysis is divided between male and female-owned enterprises. In addition, there are a number of heterogeneity analyses that I will conduct, as noted in Section 3. This includes analysis by region, whether the business owner had a loan in the past, patience, ability and risk preferences. Due to the complexity of the interactions already employed, these are estimated by splitting the sample, though analysis is done using joint significance tests.

¹¹ As some of the specifications have a relatively small sample size, there may be concern about parametric asymptotic assumptions for standard errors. I also utilize bootstrapped standard errors (results not shown) and obtain similar results in all specifications.

5.1 Business profit outcomes

The first, and main, outcome of interest is the effect of the treatments on the output of the businesses, specifically profits and revenue. Table 3 presents the results of estimating Equation 1 for the male and female-owned enterprises. I estimate the equation with and without wave interactions. I present a formal test of the equality of treatments and waves in the Online Appendix.

Columns 1 to 4 are for the male-only sample of business owners. Without the wave interactions, both loan programs have a large coefficient, significant at the 95% level. While the loan and training coefficient is larger, I cannot reject equality. There is no effect from either of the grant treatments. When I include wave dummies, there is still no effect of the grants, while 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 by wave 4 and are no longer statistically significant, while the effects for loans and training increases slightly. Compared to the control sample average of 428,000 USH at wave 3, men that were offered the loan and training program have an increase in profits of between 50% and 60%.

Columns 3 and 4 also explore the effect of the treatments on sales (revenue) and margins (profit/revenue). None of the coefficients are significant. However, the coefficient on loan and training is very large for wave 3 and increases in size by wave 4. This is jointly significantly different from zero.

For women (columns 5 to 8), 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

US\$ for wave 3. By wave 4 this effect disappears as the coefficient is of similar size, negative and significant at 282,000 US\$. While there may be an initial effect from the cash grants for women, this effect vanishes by wave 4.

The effect for women on sales and margins (columns 7 and 8) show an interesting effect from the loan only and grant only programs. These coefficients are large and negative, suggesting there was actually a decrease in revenues. Given that profits for these treatments did not change, this is suggestive that there may be an efficiency change. However, there is no change on margins.

In addition to the ITT effects, the Online Appendix presents the local average treatment effects for the main outcomes on the male and female subsamples. I estimate these results 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 women, 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 likely growing over time. Women experienced no effect from the programs on profits, with an increasing negative effect from the grants.

5.2 Heterogeneous effects

As discussed in Section 3, there are a number of heterogeneity tests based on location and individual characteristics that can be tested. Table 4 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 lack of consistent results found in Table 3. Please note that this analysis is largely exploratory as only the results for loan history and risk preferences hold when controlling for multiple hypothesis testing, due to low sample sizes.

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 treatments 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 3 column 2 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 treatment appear only 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 treatments for men. As can be seen from the plot, the results from the treatments are driven by higher profit businesses in wave 3. This is confirmed in Table 4 (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 initially 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%. A joint test of the difference between the loan and the loan-with-training treatments suggests there is no differential effect of the training initially. However, these differential results disappear by wave 4. While not individually significant, there is a jointly significant, 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 1, 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 4 presents the results of splitting the samples into those with high and low measures of ability, risk and patience. 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.

5.3 Treatment effects on employees and capital

In order to understand the channels the effects of the programs are having on men and women, Table 5 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 0.72 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 returned to the control group level. This is because the number of outside employees actually decreased overall, 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. 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 be 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 treatments 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 treatments relative to existing stock. Control men report almost 5 million USH in capital stock. The treatments 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.

5.4 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 6 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 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 household 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.

5.5 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 noisy or weak capital accumulation or possibly changes in productivity. To test for the returns to increased employment and capital, I estimate the following model for men only:

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

where either employees or employees and capital are instrumented by the treatments. This follows the linear specifications preferred by de Mel et al. (2008) and Field et al. (2013). The results are presented in Table 7 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. As I am using treatment as an instrument for both employees and capital, this violates the exclusion restriction. I thus include capital as both a control and an instrument for comparison purposes. 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 businesses¹².

6 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 therefore estimate the following model:

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

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 (as a proxy for demand). I use same district as this is the only reliable distance measure I have, though this is a large assumption that will likely overestimate the impact of the spillovers. I present the results in Table 8. Column 2 is for female-owned businesses, while columns 1, 3 and 4 are only for men and split the sample into only-treated or only-control businesses.

Including the number of firms located nearby does not change the main results reported in Table 3. The coefficients for having treated firm nearby are not significant for either men or women. The effect of having treated firms located nearby to control firms does not appear to change the income of control firms, and so spillovers from the program are very small, if any.

¹² 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.

This analysis is likely 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 as I utilize population density as a proxy.

7 Discussion

The question of what restricts existing businesses from expanding has been a pressing problem for researchers and policy makers. This experiment presents new evidence on the effect of capital (with and without repayment requirements) and training on business development for male- and female-owned microenterprises.

The results for women are consistent with existing experimental literature, and present a pessimistic picture of the effect of capital. None of the treatments led women to expand their businesses. In fact, some of the treatments appear to lead women to decrease investment in the business. In separate work (Fiala, 2015), I discuss how family constraints can explain much of this decrease in investment.

The results for men, however, suggest they can make good use of capital to expand their enterprises. Capital investment is strongest when there is a repayment requirement, and when finance is combined with trainings. The results are driven by increases in employment and capital stock, not through efficiency gains. The increase in profits is quite large and suggests there are substantial returns to increasing employment and capital. As the results are driven by men who have not had loans previously, this suggests a substantial credit constraint for many

businesses. I also find the effects are largest for those with higher baseline ability and patience, suggesting that business owner characteristics are important determinants of capital usage. The profits from the business are not being used to increase household welfare but appear to be used for further business investment and possibly other short-term consumption.

The impact from the loans stands counter to previous evidence on microcredit. I argue that this is because of the selection of businesses in this sample, specifically the inclusion of a large sample of men. Most studies have focused on women, who are the main group microcredit organizations prefer to target¹³.

I find no results from the grants. Unfortunately, I am not able to determine where exactly the cash was spent, though discussions from the qualitative interviews suggest the money was consumed quickly for household expenses and not used for other, productive investments. When compared with previous experimental literature, the lack of impact from the grants for men and women suggests a complex picture of how people invest in their enterprises and the constraints they face when doing so. Recent work on cash grants to start enterprises has found strong evidence they can shift young people from low income farm employment to skilled trades, especially women (Blattman et al., 2014). Other work on capital infusion into existing enterprises shows large returns, though the best evidence suggests that cash does not work well for existing female enterprises and other constrained businesses (de Mel, McKenzie and Woodruff, 2008; Fafchamps et al., 2013; Berge et al., 2012). The differential effects between starting and expanding enterprises suggest that capital constraints are even worse than expected:

¹³ Cull et al. (2007) find that over 65% of microcredit clients are women in a survey of large microcredit organizations. The ILO and UN have also put the number of women closer to 80% of all clients worldwide.

business owners cannot start highly profitable businesses through traditional finance. Once they have started, there are likely other constraints to expansion beyond capital, including pressure to spend on the household and a lack of desire to expand businesses.

While there may be substantial benefits to the businesses and employment growth for investment, two caveats are needed regarding the short-run welfare implications. First, for men that benefited the most from the loans, I find no increases in household welfare. All of the additional profits is thus either invested or consumed quickly. Second, while the business owners state they are interested in expanding their enterprises, this may not actually be the case. Microenterprises may have rapidly diminishing returns to scale and are simply used by households as a way of ensuring consistent cash income. Households may see little value to business expansion, and may even experience a welfare decrease from the need to repay the loans. Despite the lack of results for the grants, welfare could in fact be higher in the short-run for those who received cash compared to those who received loans.

The results presented here also suggest a mixed outcome from training. I do not find a differential effect of training for most of the treatments, except for men who received loans. The training appears to have led to increases in investment and overall employment in the short-run, with a substitution toward family employees over time. While I am unable to explicitly test for why training improved investment and employment, the results suggests that training can be effective for business growth but only when paired with capital that must be repaid.

These results suggest that small-scale, market-driven capital can improve business returns and lead to economic growth, though only for male-owned enterprises. The current pessimism in

the experimental literature with regard to the effects of microcredit are certainly true for the traditional model of microcredit, which focuses on a population, poor women, who in most cultures has very little control over business capital. In order for microcredit to affect welfare and potentially reduce poverty, it needs to expand beyond traditional clients and target those that are better able to utilize business capital.

References

- Angelucci, Manuela, Dean Karlan, and Jonathan Zinman, “Win some lose some? Evidence from a randomized microcredit program placement experiment by Compartamos Banco,” NBER Working Paper No. 19119, 2013.
- Augsburg, Britta, Ralph De Haas, Heike Harmgart, and Costas Meghir, "The Impacts of Microcredit: Evidence from Bosnia and Herzegovina," *American Economic Journal: Applied Economics*, American Economic Association, vol. 7(1), pages 183-203, 2015.
- Baird, Sarah, Craig McIntosh, and Berk Ozler, “Cash or Condition? Evidence from a Cash Transfer Experiment,” *Quarterly Journal of Economics*, 126, 1709–1753, 2011.
- Bandiera, Oriana, Niklas Buehren, Robin Burgess, Markus Goldstein, Selim Gulesci, Imran Rasul, and Munshi Sulaimany, “Empowering adolescent girls: Evidence from a randomized control trial in Uganda,” working paper, 2012.
- Banerjee, Abhijit, Esther Duflo, Rachel Glennerster, and Cynthia Kinnan, "The Miracle of Microfinance? Evidence from a Randomized Evaluation." *American Economic Journal: Applied Economics*, 7(1): 22-53, 2015.
- Becker, Sascha and Hans Hvide, “Do entrepreneurs matter?,” IZA discussion paper No. 7146, 2013.
- Berge, Lars Ivar Oppedal, Kjetil Bjorvatn, and Bertil Tungodden, “Human and Financial Capital for Microenterprise Development: Evidence from a Field and Lab Experiment,” *Management Science*, forthcoming, 2015.
- Berner, Erhard, Georgina Gomez, and Peter Knorringa, “Helping a large number of people become a little less poor: The logic of survival entrepreneurs,” *European Journal of Development Research*, 2012, 24, 382396.
- Bjorvatn, Kjetil and Bertil Tungodde, “Teaching business in Tanzania: Evaluating participation and performance,” *Journal of the European Economic Association*, 2012, 8 (2-3), 561–570.

- Blattman, Christopher, Nathan Fiala, and Sebastian Martinez, “Generating skilled self-employment in developing countries: Experimental evidence from Uganda,” *Quarterly Journal of Economics*, Vol. 129 Issue 2, p697, 2014.
- Bloom, Nick, Benn Eifert, Aprajit Mahajan, David McKenzie, and John Roberts, “Does management matter: Evidence from India,” *Quarterly Journal of Economics*, February 2013, CXXVIII (1), 1–51.
- Bruhn, Miriam and Bilal Zia, “Stimulating managerial capital in emerging markets: The impact of business and financial literacy for young entrepreneurs,” Policy Research working paper series 5642, 2011.
- Calderon, Gabriela, Jesse Cunha, and Giacomo De Giorgi, “Business literacy and development: Evidence from a randomized controlled trial in Rural Mexico,” NBER working paper w19740, 2013.
- Clark, Gracia, “Onions are my Husband,” University of Chicago Press, 1994.
- Crépon, Bruno, Florencia Devoto, Esther Duflo, and William Parienté, "Estimating the Impact of Microcredit on Those Who Take It Up: Evidence from a Randomized Experiment in Morocco." *American Economic Journal: Applied Economics*, 7(1): 123-50, 2015.
- Cull, Robert, Asli Demirg-Kunt, and Jonathan Morduch, “Financial performance and outreach: A global analysis of leading microbanks,” *Economic Journal*, 2007, 117 (517), F107F133.
- de Mel, Suresh, David McKenzie, and Chris Woodruff, “Returns to capital in microenterprises: Evidence from a field experiment,” *Quarterly Journal of Economics*, November 2008, CXXIII (4), 1329–1372.
- _, ___, and Christopher Woodruff, “Measuring microenterprise profits: Must we ask how the sausage is made?,” *Journal of Development Economics*, 2009, 88 (1), 19–31.
- _, ___, and ___, “Business training and female enterprise start-up, growth and dynamics: Experimental evidence from Sri Lanka,” *Journal of Development Economics* Volume 106, Pages 199–210, 2014.
- Desai, Jaikishan, Kristin Johnson, and Alessandro Tarozzi, “On the impact of microcredit: Evidence from a randomized treatment rural Ethiopia,” working paper, 2013.

- Dupas, Pascaline and Jonathan Robinson, “Savings constraints and microenterprise development: Evidence from a field experiment in Kenya,” *American Economic Journal: Applied Economics*, 2013, 5 (1), 163–192.
- Fafchamps, Marcel, David McKenzie, Simon Quinn, and Chris Woodruff, “Microenterprise growth and the flypaper effect: Evidence from a randomized experiment in Ghana,” *Journal of Development Economics* Volume 106, Pages 211–226, 2014.
- Fajnzylber, Pablo, William Maloney, and Gabriel Montes Rojas, “Microenterprise dynamics in developing countries: How similar are they to those in the industrialized world? Evidence from Mexico,” *World Bank Economic Review*, 2006, 20 (3), 389–419.
- , —, and —, “Releasing constraints to growth or pushing on a string? Policies and performance of Mexican micro-firms,” *Journal of Development Studies*, 2009, 45 (7), 1027–1047.
- Fiala, Nathan, “Development after conflict: Making Ugandas growth more inclusive,” working paper, 2010.
- Fiala, Nathan, “Business is tough, but family can be worse: Experimental results on family constraints and enterprise development,” working paper, 2015.
- Field, Erica, Rohini Pande, John Papp, and Natalia Rigol, “Does the classic microfinance model discourage entrepreneurship among the poor? Experimental evidence from India,” *American Economic Review*, October 2013, 103 (6).
- Fischer, Greg, “Contract structure, risk sharing, and investment choice,” *Econometrica*, Volume 81, Issue 3, pages 883–939, 2013.
- Gine, Xavier and Ghazala Mansuri, “Money or ideas? A field experiment on constraints to entrepreneurship in rural Pakistan,” working paper, 2011.
- Grimm, Michael, Renate Hartwig, and Jann Lay, “Does forced solidarity hamper investment in small and micro enterprises?,” IZA Discussion Paper No. 7229, 2013.
- Haushofer, Johannes and Jeremy Shapiro, “Welfare effects of unconditional cash transfers: Evidence from a randomized controlled trial in Kenya,” working paper, 2013.
- Horowitz, Joel L. and Charles F. Manski, “Nonparametric analysis of randomized experiments with missing covariate and outcome data,” *Journal of the American Statistical Association*, 2000, 95, 77–84.

- Jakiela, Pamela and Owen Ozier, “Does Africa need a rotten kin theorem? Experimental evidence from village economies,” working paper, 2014.
- Karlan, Dean and Martin Valdivia, “Teaching entrepreneurship: Impact of business training on microfinance clients and institutions,” *Review of Economics and Statistics*, 2011, 93, 510–552.
- _, Jonathan Morduch, and Sendhil Mullainathan, “Take-up: Why microfinance take-up rates are low and why it matters,” financial access initiative working paper, 2010.
- _, Ryan Knight, and Chris Udry, “Hoping to win, expected to lose: Theory and lessons on micro enterprise development,” NBER working paper 18325, 2012.
- Kling, Jeffrey and Jeffrey Liebman, “Experimental analysis of neighborhood effects on youth,” *Econometrica*, Volume 75, Issue 1, pages 83–119, 2007.
- Kocherlakota, Narayana, “Implications of efficient risk sharing without commitment,” *Review of Economic Studies*, October 1996, 63 (4), 595–609.
- Lee, David, “Trimming for bounds on treatment effects with missing outcomes,” NBER Technical working paper 277, 2002.
- Mano, Yukichi, Al Hassan Iddrisu, Yutaka Yoshino, and Tetsushi Sonobe, “How can micro and small enterprises in sub-Saharan Africa become more productive? The impacts of experimental basic managerial training,” *World Development*, 2012, 40 (3), 458–468.
- McKenzie, David, “Beyond baseline and follow-up: The case for more T in experiments,” *Journal of Development Economics*, 2012, 99 (2), 210–221.
- _ and Chris Woodruff, “Experimental evidence on returns to capital and access to finance in Mexico,” *The World Bank Economic Review*, 2012, 22.
- _ and _, “What are we learning from business training and entrepreneurship evaluations around the developing world?,” *World Bank Research Observer*, 29 (1): 48-82, 2014.
- Mead, Donald, “The contribution of small enterprises to employment growth in southern and eastern Africa,” *World Development*, 1994, 22 (12).
- Mobarak, Ahmed Mushfiq, Davie Kalomba, Victor Orozco, and Yoonyoung Cho. “Gender differences in the effects of vocational training: Constraints on women and drop-out behavior,” Working paper, 2013.

- Ssewanyana, Sarah, “Chronic poverty and household dynamics in Uganda,” EPRC working paper, 2010.
- Townsend, Robert, “Risk and insurance in village India,” *Econometrica*, May 1994, 62 (3), 539–91.
- Udry, Christopher and Santosh Anagol, “The return to capital in Ghana,” *American Economic Review*, 2006, 96 (2), 388–393.
- Valdivia, Martin, “Training or technical assistance for female entrepreneurship? Evidence from a field experiment in Peru,” working paper, 2012.
- van Lieshout, Susanne, Merten Sievers, and Mirza Aliyev, “Start and improve your business global tracer study 2011,” International Labour Office working paper, 2012.
- Yunus, Muhammad, *Banker to the Poor: Micro-Lending and the Battle Against World Poverty*, Public Affairs, 2003.

Fig. 1: Experimental design with sample sizes

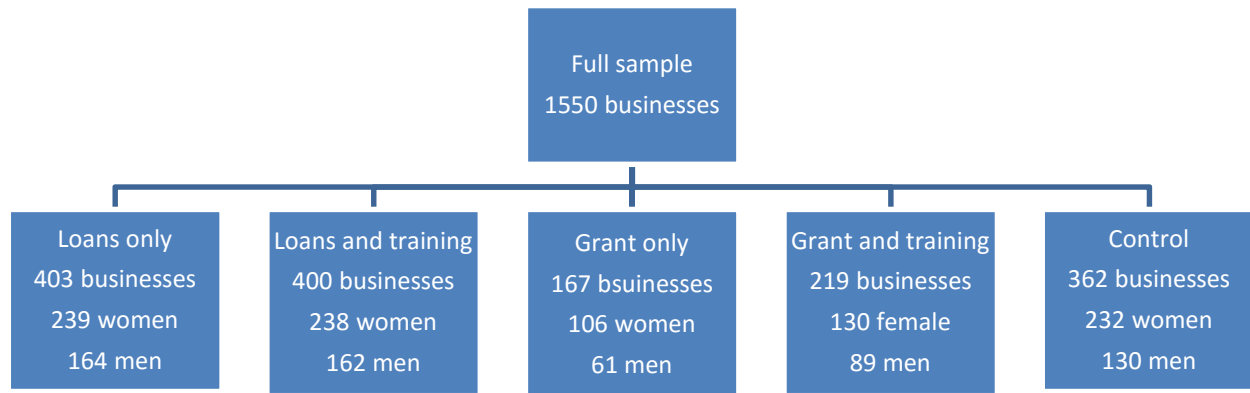


Fig. 2: Business types in the final sample for male and female-owned enterprises

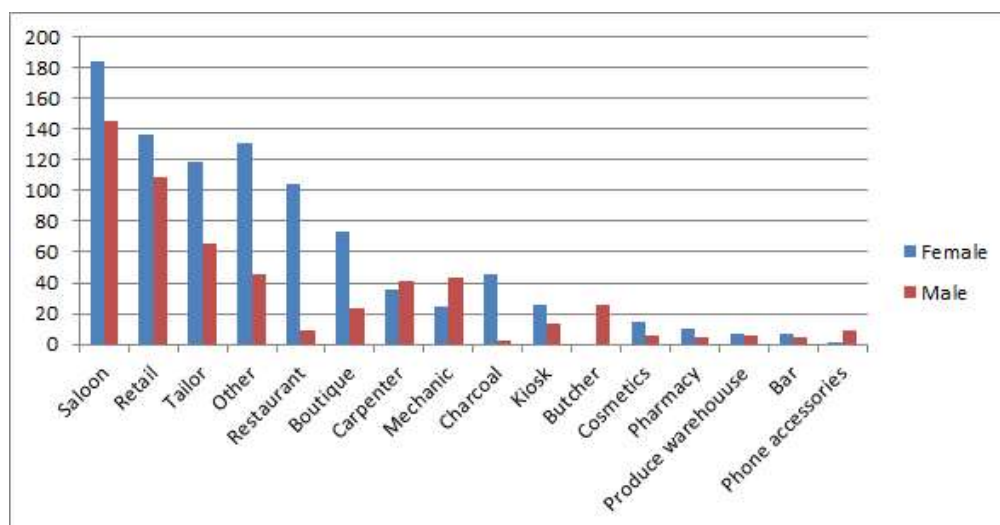


Fig. 3: CDF plot of the control (group 0), loan (group 1) and loan-with-training (group 2) for the male sample.

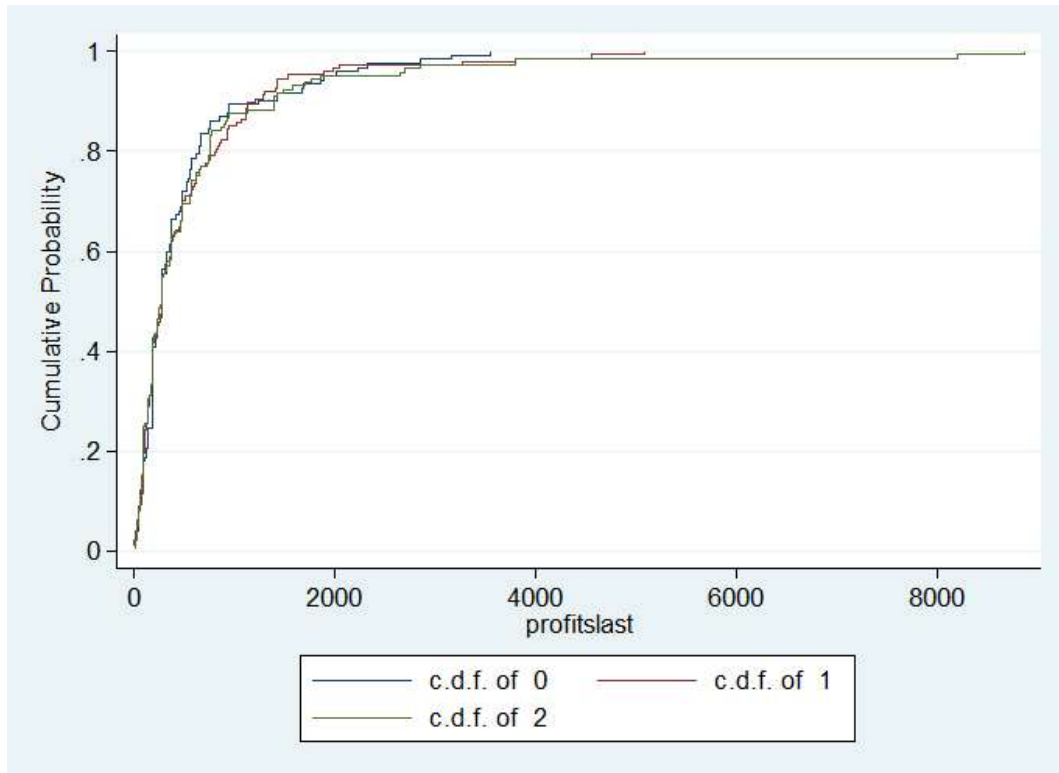


Table 1: Summary statistics and balance tests

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 2: Attrition analysis for follow-up surveys

	(1)	(2)	(3)
	Wave 3	Wave 4	Waves 3 and 4
Loan	0.065** (0.03)	0.059** (0.03)	0.059** (0.03)
Loan and Training	0.012 (0.03)	0.030 (0.03)	0.031 (0.03)
Grant	0.071** (0.04)	0.071* (0.04)	0.075** (0.04)
Grant and Training	0.049 (0.03)	0.092*** (0.03)	0.092** (0.03)
Female	0.00089 (0.02)	0.0075 (0.02)	0.0064 (0.02)
Age	0.037** (0.01)	0.027** (0.01)	0.027** (0.01)
Married	-0.0038 (0.02)	0.048** (0.02)	0.050** (0.02)
Ability	0.022** (0.01)	0.022** (0.01)	0.023** (0.01)
Total Employees	0.015*** (0.01)	0.0069 (0.01)	0.0082 (0.01)
Assets	-0.014** (0.01)	-0.0043 (0.01)	-0.0039 (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 clustered at the individual level are in parentheses below the coefficients. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Main treatment effects on business profits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male				Female			
	Profits	Profits	Sales	Margin	Profits	Profits	Sales	Margin
Loan	176.9*	260.6**	-11.9	2.60	-36.1	-40.9	-302.6*	-0.66
	(95.19)	(113.53)	(339.19)	(2.52)	(52.90)	(61.85)	(181.11)	(0.69)
Loan and Training	289.5*	249.4**	620.7	2.49	-39.0	-13.2	3.15	-0.13
	(147.97)	(118.62)	(446.08)	(2.53)	(53.90)	(60.34)	(184.30)	(0.11)
Grant	-11.9	33.8	-404.8	2.05	87.9	229.0**	-16.4	0.37
	(108.12)	(129.96)	(431.25)	(2.06)	(82.91)	(116.20)	(334.61)	(0.24)
Grant and Training	-79.9	-184.1	-74.9	1.90	-20.6	-6.99	111.3	0.18
	(171.12)	(238.04)	(424.27)	(1.99)	(63.28)	(83.42)	(351.42)	(0.21)
Loan * W4		-170.5	423.6	-0.30		10.4	60.4	0.014
		(116.10)	(429.01)	(0.27)		(62.23)	(196.30)	(0.07)
Loan and Training * W4		80.7	327.1	-0.14		-52.2	83.3	0.042
		(170.07)	(536.02)	(0.27)		(65.02)	(220.59)	(0.08)
Grant * W4		-89.1	367.2	-0.22		-282.4***	-582.3***	-0.23
		(139.26)	(507.06)	(0.34)		(99.23)	(209.61)	(0.14)
Grant and Training * W4		199.2	-145.0	0.14		-27.8	48.9	-0.12
		(202.33)	(472.60)	(0.30)		(79.57)	(316.47)	(0.11)
Control Mean	428.2	428.2	1527.1	0.56	342.9	342.9	1145.5	0.43
R ²	0.018	0.022	0.053	0.0053	0.016	0.021	0.026	0.0033
N	2069	2069	2065	2058	3261	3261	3253	3238

Notes: Columns (1) to (8) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four treatments on business profits, sales and margin. Columns (1) to (4) are for men only and columns (5) to (8) are for women only. The analysis reported here is with the sample split between men and women to make the results easier to read. The same model with men and women pooled and a dummy interaction for sex shows similar results. Robust standard errors clustered at the individual level 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. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Heterogeneity effects for men on profit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Central Region	Northern Region	Low Profit	High Profit	Previous Loans	No Previous Loans	Low Patience	High Patience	Low Ability	High Ability	High Risk	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 (12) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four treatments on business profits for men only. The results are divided by the cited heterogeneity category. Robust standard errors clustered at the individual level 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 5: Treatment effect on employment and capital outcomes

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

Notes: Columns (1) to (10) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four treatments on employment, capital and incidence of loans outcomes. Robust standard errors clustered at the individual level 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: Treatment effects on household outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Missed School		Child Health		Other Business		Savings		HH Consumption	
	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 Training	0.078	0.0048	-2.02	-15.3	0.096	-0.13	34.0	42.7	235.9	-229.3***
	(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 Training	0.031	0.075	-14.1	-17.8	0.0097	0.0069	-41.7	-6.67	-58.1	-152.0*
	(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 Training * W4	-0.12	-0.029	10.7	15.6	0.086	0.14	-61.3	-2.66	-394.8	181.5*
	(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 Training * W4	0.053	-0.15*	75.0	7.60	0.089	-0.028	-29.0	21.5	-40.9	113.6
	(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 (10) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four treatments on household outcomes. Robust standard errors clustered at the individual level 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: Returns to employment and capital

	(1)	(2)	(3)	(4)	(5)	(6)
	Family Employees			Hired Employees		
Family Employees	664.8 (419.55)	651.1 (404.64)	652.7 (406.98)			
Employees				259.2* (151.30)	208.4 (144.89)	212.4 (149.90)
Capital		0.043*** (0.01)	0.060 (0.05)		0.038*** (0.01)	0.034 (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 instrumented by assignment to one of the four treatments. Robust standard errors clustered at the individual level 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 8: Spillover effects

	(1) Male Firms	(2) Female Firms	(3) Treatment Firms	(4) Control Firms
Close Firms	0.041 (3.29)	0.44 (2.05)	1.27 (4.00)	-1.88 (4.27)
Close Firms * W4	-0.88 (3.37)	0.75 (1.88)	-0.91 (3.90)	0.70 (6.03)
Loan	276.9** (114.64)	-40.1 (62.34)		
Loan and Training	258.5** (118.99)	-11.5 (60.81)		
Grant	91.8 (135.02)	233.4** (117.31)		
Grant and Training	-131.7 (230.81)	-3.85 (84.27)		
Loan * W4	-184.2 (116.77)	11.9 (61.77)		
Loan and Training * W4	66.5 (169.13)	-49.4 (64.35)		
Grant * W4	-107.2 (139.15)	-277.3*** (99.30)		
Grant and Training * W4	182.4 (195.88)	-22.1 (79.19)		
Close firms * Density	-0.011 (0.01)	-0.0025 (0.01)	-0.014 (0.01)	-0.016 (0.02)
Close firms * Density * W4	0.0036 (0.01)	-0.0020 (0.01)	0.0023 (0.01)	0.0053 (0.02)
Control Mean	428.2	342.9	371.9	428.2
R2	0.030	0.021	0.022	0.057
N	2069	3261	1642	427

Notes: Columns (1) to (4) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four treatments on business profits. Robust standard errors clustered at the individual level 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

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, of which 29 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 days¹⁴, with some business owners reporting that they did not attend the training despite being invited. Business owners who attended the training said they appreciated the program and would recommend 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, record-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 institution was responsible for their dissemination. This type of complaint is common for individual microfinance organizations.

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 that they would consequently fail to repay. They thought the

¹⁴ 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.

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 claimed 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 be appreciated. At least two 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 it to 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 investing the money in their businesses as well as using it for short-term personal expenses. The majority of the loan beneficiaries reported investing the loan in their businesses because otherwise they would fail to finance the loan. Most of the cash grant beneficiaries admitted that they used the cash for short-term personal needs, not investment.

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 prevent the women from financing the loans. Some of the male respondents, on the other hand, stated 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 thought that, regardless of the gender, the successful use of a business grant or loan depends on how organized the beneficiary is.

Appendix 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 US\$1.25 per day. GDP per capita in 2005 US dollars 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 treatments 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:

$$I_i = \alpha + \beta X_i + \gamma R + \varepsilon_i \quad (1)$$

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 B.1. 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 thousands of Ugandan shillings. These correlations suggest that there is some selection into the sample, though none of the coefficients is very large.

In Section D.6 I weight the experimental sample by selection into the programs to obtain the population-weighted average treatment effect and find that this does not have much effect on the main results.

B.3 Comparison of normal microcredit 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 that 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 that 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. To test for the characteristics of people who took the programs, I ran the following OLS regression:

$$P_i = \alpha + \beta X_i + \phi R + \varepsilon_i \quad (2)$$

where P is a dummy for whether person i participated in the particular treatment, 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 B.2 and are divided between the full (columns 1 and 2), central (columns 3–5) 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 microcredit 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 that they distrusted 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 predicts 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 Total loans taken

The loan treatments were designed to increase access to loans for those that did not previously have access to loans. I present here additional analysis on whether the loans led to increased take-up. Table B.3 explores the total number of loans individuals had at the time of the first and second endline data collections, as stated by the individual.

For both men and women, the coefficient on the number of loans is statistically significant and positive for the loan only and loan with training programs. Men report 0.47 and 0.56 more loans than the control group, respectively, while women report 0.52 and 0.39 more loans, respectively. The treatments do appear to have led to a significantly increased number of loans, as expected.

B.7 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.4 presents balance tests for the female sample, and Table B.5 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 controlled 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.3 and B.4 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.

Appendix C. Model of returns to capital and employment

The treatment is akin to an exogenous injection of capital (physical and human) 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 entrepreneur's 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:

$$\begin{aligned} & \underset{[K, l, B, A_k, I_k]}{\text{Max}} U(c, L - l) \end{aligned} \quad (3)$$

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

$$K \leq A_k + I_k + B \quad (5)$$

$$B \leq B^* \quad (6)$$

$$A_k \leq A \quad (7)$$

$$I_k \leq nw \quad (8)$$

$$l \leq L \quad (9)$$

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: $f_K(K, L, \theta) = r + \lambda$, 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 complements, 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:

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

The first-order conditions are thus

$$V_\lambda = f(K, l) - rK + r(A - A_k) + (nw - I_k) - c = 0 \quad (11)$$

$$V_c = U'_c(c, L - l) - \lambda = 0 \quad (12)$$

$$V_l = -U'_l(c, L, \lambda) + \lambda f'_l(K, l, \theta) = 0 \quad (13)$$

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:

$$U'_c(c, L - l) = U'_l(c, L - l) / f'_l(K, l, \theta) \rightarrow U'_c(c, L - l) \cdot f'_l(K, l, \theta) = U'_l(c, L - l) \quad (14)$$

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.

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:

$$\text{Max}_{[K, H, l, B, A_k, I_k]} U(c, L - l) \quad (15)$$

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

$$K + H \leq A_k + I_k + B \quad (17)$$

$$B \leq B^* \quad (18)$$

$$A_k \leq A \quad (19)$$

$$I_k \leq nw \quad (20)$$

$$l \leq L \quad (21)$$

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

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

and the first-order conditions are

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

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

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

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

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

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 products are equal to each other:

$$f'_K(K, H, l) = f'_H(K, H, l) \quad (28)$$

Appendix 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 the paper. Low p -values suggest a rejection of the hypothesis that the effects of the treatments 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 treatments and the grant and grant-with-training treatments are equal for men. However, the null is rejected for equality of any of the loan programs with the grant treatments. The test also suggests that the wave 3 plus wave 4 effects of the loan-with-training are equal to zero, but cannot reject for any of the other treatments.

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 :

$$Y_{it} = \alpha + \beta T_{it} + \theta T_{it} * \gamma_t + \gamma_t + \eta_i + \varphi R + \mu W + \delta M + \varepsilon_{it} \quad (29)$$

$$T_{it} = \alpha + \lambda A_{it} + \nu A_{it} * \gamma_t + \gamma_t + \eta_i + \rho R + \sigma W + \varsigma M + \varepsilon_{it} \quad (30)$$

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 $T_{it} * \gamma_t$ are 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.2. 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 treatments 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 Bounding

While attrition rates are relatively low, there may still be some biases present from selection into attrition. Table D.3 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.

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.4 explores the effects of this trimming on the two main results: the effect of the loan treatments 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 the effects for the loans and training program are consistent, if not significant, with the trimmed sample. A joint test of wave 3 plus wave 4 is statistically significant. 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, 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 obtained the PATE by first estimating the probability of being in the final sample using the characteristics of the broader population. I then used 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.5 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 model¹⁵. 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.6 looks at treatment 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 treatments appear to have no effect. However, the majority of businesses in the sample (95% of the control group) report they have a business plan of some kind. 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 a 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.

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.7 to D.12 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.

D.8 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 programs. 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 training or assist with pairing individuals with microcredit institutions could be especially subject to such problems. I discuss here why it is unlikely that Hawthorne effects are a serious issue in this study.

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 treatments offered to people, both at baseline and endline surveys. The survey also never mentioned the training and microcredit programs. By keeping the survey team separate from the program team, we sought to distance the program in the minds of the participants from the research project.

¹⁵ 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.

While not directly related to Hawthorne effects, enumerator effects could present some bias in reporting by participants. During data collection, it is possible that the comfort of respondents is determined by the quality of the enumerator that is interviewing them. When controlling for enumerator effects, presented in Table D.13, 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 under-reporting. While there may be a chance that people are systematically misreporting, this misreporting would have to be very sophisticated.

Figure B.1: Map of Uganda with treatment districts

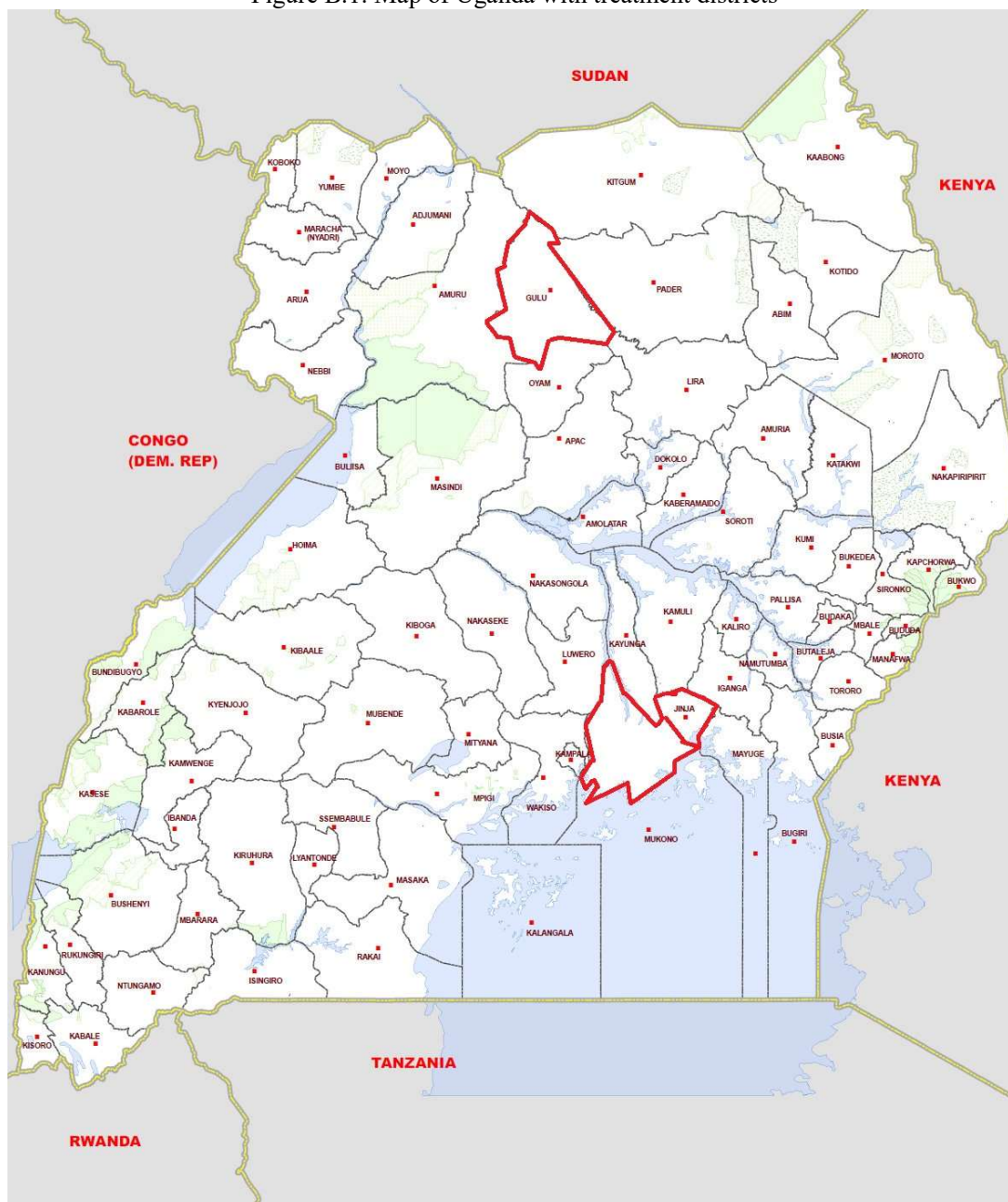


Figure B.2: Density of loans by date

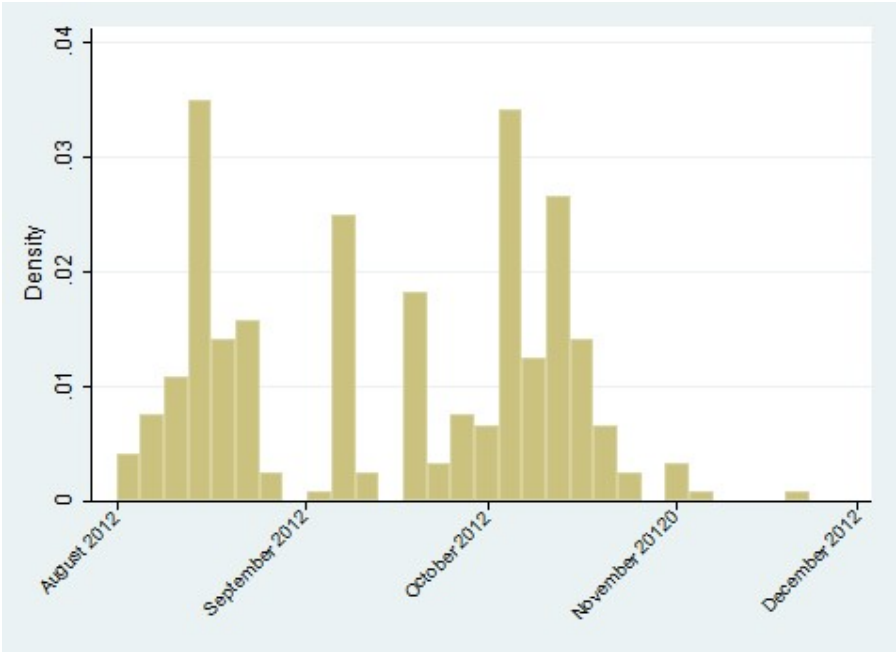


Table B.1: Determinants of interest for the treatments

	(1) Interest in loan	(2) Interest in training	(3) Interest in both
Female	-0.0071 (0.01)	-0.0048 (0.01)	-0.011 (0.01)
Age	-0.053*** (0.01)	-0.063*** (0.01)	-0.052*** (0.01)
Married	0.033** (0.01)	0.032** (0.02)	0.035*** (0.01)
Literacy	0.011 (0.02)	-0.028 (0.02)	0.0061 (0.02)
Previous Training	0.016 (0.02)	0.0025 (0.02)	0.017 (0.02)
Previous Loan	0.10*** (0.01)	0.089*** (0.01)	0.099*** (0.01)
Revenues - 1 Lag	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Profits - 1 Lag	-0.000038 (0.00)	-0.00041*** (0.00)	-0.000037 (0.00)
Ability	0.0035 (0.01)	0.024** (0.01)	0.0049 (0.01)
Assets	-0.0039 (0.00)	-0.0096*** (0.00)	-0.0049* (0.00)
Control Mean			
R2	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 interest in the loan, training or loan and training programs. Data is from the first baseline data collection. 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*** (0.04)		0.15*** (0.05)	0.21*** (0.05)		0.064 (0.05)	
Female	-0.0074 (0.04)	-0.0075 (0.04)	0.0073 (0.05)	-0.017 (0.04)	-0.050 (0.06)	-0.022 (0.06)	0.13 (0.08)
Age	0.043** (0.02)	0.045* (0.02)	0.065** (0.03)	0.018 (0.02)	0.052 (0.03)	0.025 (0.03)	0.047 (0.04)
Married	0.026 (0.04)	-0.054 (0.04)	0.023 (0.06)	-0.0059 (0.04)	-0.069 (0.05)	0.045 (0.06)	-0.067 (0.08)
Literacy	-0.023 (0.06)	-0.016 (0.07)	-0.11 (0.09)	-0.087 (0.07)	0.068 (0.09)	0.044 (0.08)	-0.21 (0.13)
Previous Training	0.018 (0.05)	0.098* (0.05)	0.014 (0.07)	-0.0020 (0.05)	0.19*** (0.07)	0.022 (0.07)	-0.083 (0.09)
Total Employees	-0.012 (0.01)	-0.0072 (0.02)	-0.020* (0.01)	-0.011 (0.02)	-0.0071 (0.02)	-0.0020 (0.02)	-0.049 (0.04)
Revenues - 1 Lag	0.0069 (0.00)	-0.011 (0.00)	0.0077 (0.00)	0.0011 (0.00)	-0.0056 (0.00)	-0.0067 (0.00)	-0.065*** (0.00)
Revenues - 2 Lags	0.0029 (0.00)	-0.0035 (0.00)	0.035 (0.00)	0.050* (0.00)	-0.015 (0.00)	-0.023 (0.00)	0.043 (0.00)
Profits - 1 Lag	-0.023 (0.00)	0.0085 (0.00)	-0.014 (0.00)	0.010 (0.00)	-0.0065 (0.00)	-0.011 (0.00)	0.13 (0.00)
Profits - 2 Lags	-0.017 (0.00)	-0.032*** (0.00)	-0.057* (0.00)	-0.032*** (0.00)	-0.036*** (0.00)	0.015 (0.00)	0.015 (0.00)
Ability	0.019 (0.03)	0.020 (0.03)	0.059 (0.04)	0.016 (0.04)	-0.038 (0.05)	-0.0096 (0.04)	0.11** (0.05)
Assets	-0.0024 (0.01)	0.025 (0.02)	-0.0079 (0.03)	-0.0069 (0.02)	0.033 (0.02)	-0.0018 (0.01)	0.034 (0.02)
Previous Loan	0.087** (0.04)	0.064 (0.04)	0.047 (0.05)	0.062 (0.04)	0.066 (0.05)	0.13** (0.05)	0.064 (0.07)
Control Mean							
R2	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. Note that training was always done before the offer for grant or loan, and is the biggest predictor of take-up. Robust standard errors clustered at the individual level are in parentheses below the coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.3: Total number of loans taken

	(1)	(2)
	Male	Female
Loan	0.47*** -0.07	0.52*** -0.06
Loan and Training	0.56*** -0.1	0.39*** -0.06
Grant	-0.016 -0.08	-0.035 -0.09
Grant and Training	-0.053 -0.11	-0.059 -0.08
Control mean	1.18	1.14
N	618	1034

Notes: Columns (1) and (2) report the results of an OLS regression on the total number of loans the invited individual took by the program that was offered for the loans, grants and training programs, respectively. Robust standard errors clustered at the individual level are in parentheses below the coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.4: Balance tests by treatment 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 US\$)	-48.47	(0.44)	-76.15	(0.21)	-27.59	(0.77)	26.74	(0.80)
Average months revenue (1000 US\$)	1106.02	(0.32)	-89.99	(0.28)	151.73	(0.41)	-4.01	(0.98)
Last month's profit (1000 US\$)	5.96	(0.92)	42.32	(0.41)	-52.75	(0.24)	-9.28	(0.85)
Average month's profit (1000 US\$)	-16.47	(0.64)	54.69	(0.28)	20.14	(0.75)	47.04	(0.38)
Stock value (1000 US\$)	297.16	(0.38)	91.34	(0.69)	507.49	(0.32)	648.20	(0.24)
Value of liabilities (1000 US\$)	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.5: Balance tests by treatment arm for men

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	-0.06	(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 US\$)	-108.15	(0.28)	-122.38	(0.20)	-118.91	(0.31)	13.62	(0.91)
Average months revenue (1000 US\$)	-20.26	(0.90)	29.19	(0.89)	2.37	(0.99)	373.80	(0.40)
Last month's profit (1000 US\$)	-103.41	(0.27)	-131.86	(0.19)	-217.66	(0.20)	79.60	(0.80)
Average month's profit (1000 US\$)	-415.64	(0.25)	-487.78	(0.20)	-740.75	(0.31)	-354.32	(0.64)
Stock value (1000 US\$)	-542.56	(0.75)	-2141.20	(0.16)	-3037.04	(0.31)	-3577.94	(0.17)
Value of liabilities (1000 US\$)	-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%.

Table D.1: Tests of equality of treatments

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 effects 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 (114.18)	209.0* (122.41)	562.6** (256.62)	617.3** (251.29)	-78.0 (109.26)	-54.2 (127.52)
Received Grant	3.98 (90.79)	52.0 (118.30)	-33.3 (187.33)	-80.2 (240.58)	48.8 (90.13)	156.5 (116.50)
Received Loan * W4		-113.7 (136.67)		-130.8 (287.98)		-56.8 (139.60)
Received Grant * W4		-103.9 (107.71)		83.8 (213.08)		-224.7** (107.68)
Control Mean	371.9	371.9	428.2	428.2	342.9	342.9
R2	0.0065	0.0061	-0.0095	-0.0073	0.011	0.012
N	5343	5343	2067	2067	3261	3261
Instrumented	Received Loan Received Grant	Received Loan Received Grant	Received Loan Received Grant	Received Loan Received Grant	Received Loan Received Grant	Received Loan Received Grant
Under identification Weak	9.9e-58	4.7e-50	1.1e-21	7.0e-20	3.3e-36	6.0e-31
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 treatments. 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: Bounding effects for attrition for the male sample

	(1)	(2)	(3)	(4)	(5)
	Original Sample	+/- 0.1 SD	+/- 0.2 SD	+/- 0.3 SD	+/- 0.5 SD
Loan	260.6** (113.53)	178.0* (95.34)	171.9* (95.45)	165.9* (95.62)	153.8 (96.11)
Loan and Training	249.4** (118.62)	146.4 (93.87)	140.0 (94.00)	133.5 (94.18)	120.5 (94.72)
Grant	33.8 (129.96)	-263.1 (193.70)	-268.4 (193.77)	-273.7 (193.87)	-284.4 (194.12)
Grant and Training	-184.1 (238.04)	27.7 (115.05)	21.5 (115.12)	15.3 (115.27)	2.84 (115.75)
Loan * W4	-170.5 (116.10)	-46.6 (150.22)	-67.6 (150.86)	-88.6 (151.81)	-130.6 (154.64)
Loan and Training * W4	80.7 (170.07)	-12.4 (158.59)	-33.5 (159.16)	-54.7 (160.09)	-97.0 (163.00)
Grant * W4	-89.1 (139.26)	922.0 (877.32)	902.6 (877.45)	883.2 (877.63)	844.4 (878.13)
Grant and Training * W4	199.2 (202.33)	-53.4 (149.71)	-70.9 (150.35)	-88.4 (151.29)	-123.4 (154.03)
Control Mean	428.2	428.2	428.2	428.2	428.2
R2	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 treatments 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 clustered at the individual level 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.4: Effects of trimming the sample

	Male		Female - Far Family		Female - Close Family	
	(1)	(2)	(3)	(4)	(5)	(6)
	No		No		No	
	trimming	Trimming	trimming	Trimming	trimming	Trimming
Loan	134.4 (225.48)	260.6** (113.53)	104.0 (73.01)	143.9** (65.40)	-143.6* (83.82)	-191.8* (100.60)
Loan and Training	142.1 (249.57)	249.4** (118.62)	96.6 (98.37)	112.8 (102.53)	-79.8 (65.97)	-109.8 (68.04)
Grant	-558.1* (291.23)	33.8 (129.96)	330.7** (151.76)	347.3** (139.47)	164.1 (199.13)	128.1 (199.17)
Grant and Training	-259.6 (286.64)	-184.1 (238.04)	243.7 (222.91)	152.5 (118.77)	-140.7* (82.68)	-177.9** (86.25)
Loan * W4	114.7 (253.72)	-170.5 (116.10)	-508.0 (443.21)	-128.9* (72.70)	34.0 (124.53)	138.9 (92.83)
Loan and Training * W4	256.1 (246.44)	80.7 (170.07)	-433.7 (426.97)	-69.1 (94.37)	-130.7 (110.21)	-42.2 (85.32)
Grant * W4	1278.4 (914.57)	-89.1 (139.26)	-834.2** (347.72)	-365.9*** (132.22)	-354.1* (208.77)	-191.6 (141.97)
Grant and Training * W4	258.9 (288.13)	199.2 (202.33)	-601.1 (414.27)	-114.8 (102.43)	-25.7 (145.25)	80.2 (116.30)
R2	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 treatments 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 clustered at the individual level 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: Population weighted treatment effects on business profits

	(1) Male ITT	(2) Male PATE	(3) Female ITT	(4) Female PATE
Loan	260.6** (113.53)	215.4* (114.48)	-40.9 (61.85)	-53.5 (65.49)
Loan and Training	249.4** (118.62)	229.8* (120.66)	-13.2 (60.34)	-35.6 (64.60)
Grant	33.8 (129.96)	-2.87 (131.39)	229.0** (116.20)	225.9* (118.91)
Grant and Training	-184.1 (238.04)	-273.3 (276.00)	-6.99 (83.42)	32.3 (104.08)
Loan * W4	-170.5 (116.10)	-162.3 (119.64)	10.4 (62.23)	18.8 (65.23)
Loan and Training * W4	80.7 (170.07)	75.9 (183.78)	-52.2 (65.02)	-23.0 (69.82)
Grant * W4	-89.1 (139.26)	-127.4 (141.21)	-282.4*** (99.23)	-288.1*** (101.48)
Grant and Training * W4	199.2 (202.33)	221.3 (231.38)	-27.8 (79.57)	-49.6 (98.63)
Control Mean	428.2	405.8	342.9	343.2
R2	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 treatments 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.6: 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 (0.03)	0.0060 (0.02)	-0.062 (0.07)	-0.026 (0.04)	0.26 (0.26)	-0.15 (0.22)
Loan and Training	-0.0012 (0.03)	-0.019 (0.02)	0.089 (0.07)	0.055 (0.05)	0.42 (0.30)	0.092 (0.33)
Grant	0.020 (0.04)	0.054* (0.03)	-0.13 (0.08)	-0.033 (0.05)	0.37 (0.32)	-0.20 (0.30)
Grant and Training	0.019 (0.04)	0.0067 (0.03)	0.075 (0.08)	0.040 (0.05)	1.07** (0.44)	-0.29 (0.26)
Loan * W4	-0.012 (0.04)	-0.013 (0.03)	0.11 (0.10)	-0.0078 (0.06)	-0.65 (0.46)	0.018 (0.36)
Loan and Training * W4	0.039 (0.04)	0.034 (0.03)	-0.011 (0.10)	-0.068 (0.07)	-2.24* (1.34)	-0.27 (0.41)
Grant * W4	0.040 (0.05)	0.0049 (0.03)	0.088 (0.12)	0.13 (0.08)	-0.41 (0.74)	-0.28 (0.44)
Grant and Training * W4	0.044 (0.05)	0.0024 (0.04)	0.011 (0.12)	0.050 (0.08)	-0.81 (0.78)	0.19 (0.45)
Control Mean	0.95	0.96	0.36	0.27	1.34	1.39
R2	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 treatments on business practices outcomes. Robust standard errors clustered at the individual level 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.7: Treatment effects on business profits by region

	(1)	(2)	(3)	(4)	(5)	(6)
	Central	North	Central - Male	North - Male	Central - Female	North - Female
Loan	204.2** (90.16)	-32.4 (73.19)	436.1** (193.59)	73.6 (98.78)	14.0 (67.67)	-78.2 (102.15)
Loan and Training	91.6 (98.58)	85.2 (64.14)	244.9 (199.03)	251.7** (112.80)	-18.0 (95.75)	6.44 (74.91)
Grant	251.6** (104.70)		191.4 (181.76)		282.6** (121.33)	
Grant and Training	13.4 (107.99)		-60.7 (231.38)		40.5 (84.83)	
Loan * W4	-49.2 (84.77)	-86.2 (82.57)	-206.5 (177.95)	-189.7 (154.24)	85.7 (71.42)	-39.5 (97.22)
Loan and Training * W4	141.9 (115.74)	-140.7 (94.22)	199.7 (275.12)	-103.6 (202.13)	109.0 (83.58)	-188.1** (93.39)
Grant * W4	-166.6 (91.96)		-114.3 (175.50)		-207.8 (99.92)	
Grant and Training * W4	110.0 (94.22)		207.5 (201.56)		50.6 (78.52)	
Control Mean	332.6	404.9	448.1	403.2	250.4	405.6
R2	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 treatments on business profits. The results are divided between the central region (Mukono, Buikwe and Jinja) and the northern region (Gulu). Robust standard errors clustered at the individual level 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 of business profits by baseline profit level

	(1)	(2)	(3)	(4)
	Male - Low Profits	Male - High Profits	Female - Low Profits	Female - High Profits
Loan	181.2 (137.53)	315.3* (168.36)	21.0 (48.09)	-130.2 (129.82)
Loan and Training	86.2 (163.75)	368.7** (168.12)	109.3* (62.32)	-153.1 (104.34)
Grant	-170.9 (123.43)	196.6 (219.07)	357.7** (167.64)	67.9 (153.23)
Grant and Training	23.0 (146.73)	-411.5 (441.37)	106.8 (102.01)	-109.9 (127.88)
Loan * W4	-203.1 (163.17)	-188.0 (165.61)	-27.3 (73.99)	43.5 (105.66)
Loan and Training * W4	434.3 (293.60)	-220.8 (177.27)	-130.2 (87.57)	37.4 (96.10)
Grant * W4	32.9 (165.43)	-245.0 (230.51)	-400.8*** (135.05)	-139.5 (141.78)
Grant and Training * W4	-85.7 (192.57)	441.4 (350.69)	-24.5 (101.35)	-23.1 (122.45)
Control Mean	301.5	535.3	222.6	252.9
R2	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 treatments 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 clustered at the individual level 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 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 (147.33)	398.7*** (151.88)	-48.0 (66.10)	-37.4 (108.96)
Loan and Training	190.9 (165.56)	234.7 (156.64)	0.95 (82.81)	-74.1 (79.68)
Grant	-58.2 (184.63)	40.6 (166.21)	347.4* (190.15)	70.8 (114.79)
Grant and Training	-106.9 (184.41)	-337.5 (399.33)	-14.1 (117.57)	-27.3 (111.06)
Loan * W4	-52.7 (193.41)	-256.7* (142.39)	42.4 (76.36)	-26.9 (102.60)
Loan and Training * W4	-126.7 (218.97)	164.7 (247.23)	-24.4 (85.74)	-40.1 (89.57)
Grant * W4	-1.65 (217.80)	-235.3 (184.42)	-386.0** (163.81)	-140.9 (93.69)
Grant and Training * W4	243.9 (266.89)	128.9 (304.25)	-39.0 (103.89)	2.88 (124.03)
Control Mean	471.1	367.3	347.8	338.1
R2	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 treatments on business profits. The results are divided between individuals that had a loan previously and those who never had a loan. Robust standard errors clustered at the individual level 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 effect 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 (105.66)	25.1 (48.82)	371.4** (185.89)	-145.2 (145.74)	294.4** (121.85)	-106.1 (107.11)	217.0 (199.50)	28.7 (57.14)
Loan and Training	0.41 (110.45)	55.6 (77.22)	398.4** (194.59)	-113.2 (98.59)	203.8* (111.90)	-93.8 (99.90)	312.6 (216.38)	78.5 (64.60)
Grant	-238.7 (153.48)	244.2* (135.87)	226.4 (208.04)	183.8 (196.72)	45.6 (86.73)	305.1 (202.15)	26.7 (291.64)	108.6 (97.44)
Grant and Training	-757.1 (578.28)	20.4 (89.09)	130.7 (202.87)	-60.3 (163.01)	9.13 (101.92)	-45.5 (99.96)	-411.9 (551.26)	-20.1 (133.78)
Loan * W4	75.3 (135.41)	-32.9 (65.43)	-299.1 (183.10)	80.4 (122.10)	-192.0 (148.47)	31.1 (105.59)	-139.4 (179.06)	-13.4 (67.61)
Loan and Training * W4	144.5 (186.06)	-73.3 (84.91)	86.9 (272.08)	-18.3 (103.81)	113.2 (214.83)	-75.4 (105.39)	-45.9 (223.51)	-15.2 (82.16)
Grant * W4	365.2* (194.47)	-286.7*** (102.77)	-411.0** (207.46)	-248.1 (178.04)	-267.8 (169.01)	-380.7** (165.44)	61.0 (238.09)	-179.0* (106.87)
Grant and Training * W4	661.0 (469.49)	-46.8 (82.59)	-57.3 (193.55)	-2.08 (160.09)	-207.1 (153.46)	33.6 (104.08)	665.9 (422.03)	-115.0 (130.99)
Control Mean	372.7	292.7	478.2	416.9	320.7	350.3	533.5	336.0
R2	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 treatments 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 clustered at the individual level 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 risk preferences

	(1) Male-High Risk	(2) Male-Low Risk	(3) Female-High Risk	(4) Female-Low Risk
Loan	117.0 (180.37)	405.7*** (127.98)	2.41 (94.86)	-75.9 (76.01)
Loan and Training	136.7 (178.07)	453.7** (194.58)	86.0 (77.30)	-121.5 (97.83)
Grant	30.0 (245.43)	66.1 (103.78)	330.3 (226.41)	128.8 (139.73)
Grant and Training	-424.1 (537.84)	32.3 (119.51)	132.1 (179.08)	-108.4 (95.20)
Loan * W4	-0.24 (164.58)	-310.1 (208.77)	-79.4 (95.39)	111.6 (87.55)
Loan and Training * W4	150.3 (165.10)	123.1 (440.19)	-157.5 (104.55)	46.6 (87.51)
Grant * W4	88.6 (199.52)	-223.6 (211.29)	-533.7*** (176.90)	-74.3 (125.33)
Grant and Training * W4	462.5 (390.65)	7.91 (211.38)	-215.6 (197.79)	120.7 (74.61)
Control Mean	517.9	325	317.1	383.7
R2	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 treatments 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 clustered at the individual level 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 age of firm

	(1)	(2)	(3)	(4)
	Male-Young Business	Male-Old Business	Female-Young Business	Female-Old Business
Loan	502.4*** (184.93)	34.2 (137.94)	79.8 (80.49)	-141.1 (91.38)
Loan and Training	338.3* (175.67)	158.5 (153.64)	-37.3 (91.93)	1.77 (79.92)
Grant	248.8 (180.93)	-150.3 (222.18)	98.5 (101.46)	361.6* (210.37)
Grant and Training	277.6 (186.70)	-578.9 (407.55)	109.1 (113.39)	-116.1 (116.86)
Loan * W4	-251.3 (178.02)	-42.9 (157.80)	-86.3 (108.47)	83.1 (76.52)
Loan and Training * W4	-233.9 (167.00)	362.8 (255.04)	-84.6 (104.58)	-30 (82.88)
Grant * W4	-177.1 (160.72)	4.69 (256.65)	-153.7* (89.43)	-416.3** (181.34)
Grant and Training * W4	-305.4* (164.85)	666.2* (349.49)	-36.2 (112.57)	-26.5 (113.23)
Control Mean	367.8	478.4	297.9	374.1
R2	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 treatments 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 clustered at the individual level 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: Controlling for enumerator effects

	(1)	(2)
	Male	Female
Loan	420.4** (190.53)	75.2 (68.06)
Loan and Training	483.2** (235.72)	61.4 (94.80)
Grant	232.5 (195.02)	301.9** (123.74)
Grant and Training	-83.6 (250.13)	130 (88.75)
Loan * W4	-214.6 (130.77)	-27.5 (62.34)
Loan and Training * W4	-47.5 (179.65)	-58.4 (64.93)
Grant * W4	-99.4 (147.43)	-220.7** (103.29)
Grant and Training * W4	175.2 (198.46)	-35.3 (78.81)
Control Mean	428.2	342.9
R2	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 treatments and enumerator characteristics on business profits. Enumerator dummies, not shown, are included in both specifications. Robust standard errors clustered at the individual level 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.