Returns to microcredit, cash grants and training for male and female microentrepreneurs in Uganda

Nathan Fiala

University of Connecticut, Agriculture and Resource Economics, 1376 Storrs Rd, Storrs, CT 06269, United States

ABSTRACT

Experimental tests of microfinance programs have found little or no impacts on business and household income outcomes. I present experimental evidence that the gender of the individual receiving a loan matters for the impacts measured. Microenterprise owners were randomly offered either capital with repayment (discounted loans) or without (grants) and were randomly chosen to receive business skills training in conjunction with the capital. I find no short-run effects for female-owned enterprises from either form of capital or the training. However, I find large effects on profits and sales for male-owned enterprises that were offered loans. There is no effect for men from the grants, suggesting repayment requirements increased the likelihood of productive investment. The results indicate that cash-constrained men—a sample that is not traditionally targeted by microcredit organizations—can benefit from subsidized microfinance.

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1. Introduction

Microenterprises are common in countries with limited formal employment options. In Uganda, these businesses account for about 90% of private sector production and employ over 2.5 million people (National Small Business Survey of Uganda, 2015[1]). 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, & Knorringa, 2012; Fajnzylber, Maloney, & Rojas, 2006; Fajnzylber, Maloney, & Rojas, 2009).

In this study, I explore how capital and skills constraints affect business growth for both female and male business owners. From a selection of semi-urban businesses across Uganda that expressed interest in accessing finance, I randomly selected a sample of 1550 business owners to receive capital and business skills training or to be part of a control group. Participants were offered a loan (capital requiring repayment), a cash grant (capital not requiring repayment) or to be in a control group. A selection of these participants were also offered free business skills training. Unknown to the study participants, the loans were subsidized to reduce the normal interest rates and induce the microcredit organization to take on clients with whom they would not normally work, including primarily men and those without credit history or enough collateral to meet the minimum requirements. This design allows for a test of the effects of infusions of capital on business outcomes depending on whether men or women receive the money, whether the capital does or does not require repayment, as well as the marginal effects of business and management skills training on a capital drop.

The sample is selected from business owners who expressed interest in 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 process produced a sample that is appropriate to determine the effect of capital on business outcomes for those who are interested in obtaining capital, minimizes selection issues present in studies that rely on targeting larger groups, allows for a test of microloans on a sample that is very interested in loans, rather than a broader sample that includes many that are not, and increases the comparability of those that were offered grants to those that were offered loans.

E-mail address: nathan.fiala@uconn.edu
[1] See https://www.fsdafrica.org/knowledge-hub/documents/national-small-business-survey-of-uganda/.

Data on business returns are noisy. To test for changes over time and improve on statistical power, the survey team conducted multiple data collections on the businesses.[2] The analysis presented here utilizes two baseline surveys before the programs were delivered, along with two follow-up surveys conducted six and nine months after the treatments were delivered.

The program was successful in expanding access to loans. Men and women who were offered finance report having on average 50% more loans than those not offered the loan. 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 report up to 54% greater profits in the last month. The effects increase slightly over time and are strongest for those that were offered training, had higher measured levels of ability, lower risk preferences, and no prior history of loans. 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 heterogeneity results suggest that business owner characteristics are important determinants of capital usage. The results for men without a history of taking loans are also consistent with credit constraints, while the grants' lack of effect suggests repayment requirements can increase the likelihood of productive investment in the business compared to receiving unconditional cash.

This study is related to a large literature on microenterprise development. There are several reasons posited by researchers and policy makers for the lack of growth observed with microenterprises. Missing human capital, specifically management skills needed to handle increasing cash flow, can make expansion difficult for many business owners. However, most studies on business skills training fail to find an effect on sales and profits from these trainings (Bjorvatn & Tungodde, 2012; Gine & Mansuri, 2011; Karlan & Valdivia, 2011; Karlan, Valdivia, Knight, & Udry, 2012; Mobarak, Kalomba, Orozco, & Cho, 2013).

Credit constraints, a major problem in almost all developing countries, may also constrain business development. Cash transfers, which inject capital without repayment requirements, have been shown to produce large returns to businesses, though recent evidence suggests cash can be difficult for businesses to use effectively (Berge, Oppedal, & Tungodden, 2015; de Mel, McKenzie, & Woodruff, 2008; Fafchamps, McKenzie, Quinn, & Woodruff, 2014). A more common approach is microcredit, which, being a self-sustaining private-market solution, has penetrated many extremely poor areas. However, recent experimental work has found no or mixed effects from microcredit on enterprise and income growth (Augsburg, De Haas, Harmgart, & Meghir, 2015; Banerjee, Duflo, Glennerster, & Kinnan, 2015; Fischer, 2013; Gine & Mansuri, 2011).

I present evidence that these mixed results for finance are likely due to who is being targeted by microfinance institutes (MFIs). MFIs traditionally target poor women, a group that often does not operate businesses that can easily be expanded. The women in the present sample 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. During quantitative interviews women reported spending six more hours a week than men at any household activity (32 h for women, 26 h for men). The focus on women has been the standard from the beginning of the microcredit movement and is normally justified for their higher

rates of poverty and higher likelihood to repay loans (Yunus, 2003). Researchers have likewise focused for the most part on women borrowers.

The results suggest that researchers should think differently about *who* receives microloans when looking at small enterprise growth. 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: men. In most sub-Sahara African households, women face significant restrictions on how they can use capital, while men do not face these restrictions. For microcredit to improve welfare and reduce poverty, it needs to expand beyond traditional clients and target those who are better able to utilize capital.[3]

This paper presents three main contributions to the literature. First, I find that microcredit has no effect on female-run business outcomes but does 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 presenting mixed results. While Field, Pande, Papp, and Rigol (2013) find that a grace period for loan repayment for women leads to a positive and significant effect on profit, they find this result is due to increases in the male spouse's business, not the woman's business. Banerjee et al. (2015) and Crépon, Devoto, Duflo, and Parienté (2015) find a growth in enterprise profits from the expansion of microcredit in India and Morocco, respectively, though this finance is delivered at the household level. Fischer (2013), Augsburg et al. (2015), Gine and Mansuri (2011), Desai, Johnson, and Tarozzi (2013) and Angelucci, Karlan, and Zinman (2013) all fail to find significant impacts from microcredit. However, these last studies focused almost exclusively on female-owned enterprises, with few if any male-owned enterprises in the samples. Of the six 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, though they do not look at the difference in impact between men and women. In the current study, I can test for the differential impact of finance for men and women and find that there is a significant difference.

Second, I can compare the effect of offering capital with repayment (loans) and without (grants).[4] Previous research suggests that cash can have large effects for starting a business, especially for women (Blattman, Fiala, & 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. More recent work, such as Berge et al. (2015), suggests that cash grants are often consumed quickly by business owners and not used for investment. 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. It is thus likely that the requirement for repayment induced men to use the money for investment.

Finally, I can explore how human capital development can affect business outcomes in the presence of relaxing capital con-

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[2] 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.

[3] 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.

[4] 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.

straints. There is reason to believe business owners are missing several skills. For instance, Bloom, Eifert, Mahajan, McKenzie, and Roberts (2013) find large returns for Indian textile firms when given management services. Most studies on micro and small businesses, though, find positive impacts of business skills trainings only on knowledge and attitudes, with little or no impact on profits and sales (Bjorvatn & Tungodde, 2012; Gine & Mansuri, 2011; Karlan & Valdivia, 2011; Mobarak et al. 2013). However, Calderon, Cunha, and De Giorgi (2013) do find large profit effects from an intensive training program in Mexico, while Berge et al. (2015) find that combining training with cash grants can have impacts for male-owned enterprises. 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.

There are some potential limitations to the current study. First, as will be discussed in detail, take-up of the loans and cash grants was not perfect. This means there is potentially some selection into the programs. It also means that direct comparison between the cash grants and microloan experiments may be biased. However, this is unlikely to be a large issue as the take-up rate is relatively high compared to similar studies and so any bias is not likely to be large. I also find that even among enterprise owners who are offered the cash grants and are predicted to be most likely to take the loan, there is no effect from the cash grant. If there is bias in the sample, it is likely no worse than other studies that find similar results.

Second, due to time and sample size constraints, I was unable to stratify the design by gender. However, tests of balance confirm that there is no imbalance between treatment and control arms for any of the most important variables, including gender. There is also good balance for the main variables within gender groups. I am also unable to answer some of the mechanisms behind what makes investment into enterprises difficult for women. Extensive previous research has shown that women and men face different constraints to running their businesses, and that selection into self-employment may differ by gender. This is a serious problem for anyone working with microenterprises in developing countries. Future research on this is obviously needed.

Finally, I was unable to collect quantitative data on what was done with the grant and loan money as people do not reliably report how they used the money. Qualitative interviews suggest the grants were consumed very quickly for household needs while the loans were invested into the business. As already mentioned, this is a similar finding (and problem) for other cash grant studies and is another area where future research would be welcomed. I am though able to present some evidence that the loans were invested in the enterprises.

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.

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. The focus on existing businesses is intentional. These are likely to be the most productive businesses and, in the literature discussed above, have been found to respond to capital infusions, though with mixed results. More

critically, they are the main types of consumers of loans as microfinance organizations prefer to lend to businesses that have a track record of some success and have collateral on hand.

I hired and trained a survey team that interviewed 4637 business owners spread across the central and northern regions on 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 1550 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 in studies like this, 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 the Appendix.

I randomly sorted individuals into five categories, presented in Fig. 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 done at the time of the evaluation design assuming multiple data collections and considering implementation budget limitations. Power with samples where outcomes are noisy, such as microenterprise profits, can be problematic. The use of multiple data collections helps to alleviate this issue, as discussed in McKenzie (2012). 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.

Fig. 2 presents the business types by frequency for the study population, divided by male and female owners. Most 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 necessarily 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 most business types.

The selection of businesses was done without specific identification criteria. The firms in this sample are more likely to be composed of fixed location shops as these are the predominate business type in the areas studied. Mobile location shops are more likely to be found at homes or in smaller villages. As will be shown in the data section, all businesses are informal and few report paying any kinds of taxes. Many people do have access to credit, but few men and about half of the women have never taken formal loans. There are informal lending methods, though these normally come with annual interest rates well above 200%.

Finally, the context for any evaluation matters for the interpretation of results. I include a fuller discussion of the context in the Appendix. Importantly for this study, the evaluation covers two areas in Uganda that are very different from each other. The central region is much better off and has a history of extensive trade with neighbors. The north is much poorer, has lower trading options, and experienced a civil war from 1984 to 2007. The country is also considered very entrepreneurial. Students often can take classes on entrepreneurship in secondary school. Microenterprises are seen by the government as a solution to the lack of formal employment options for most people.

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
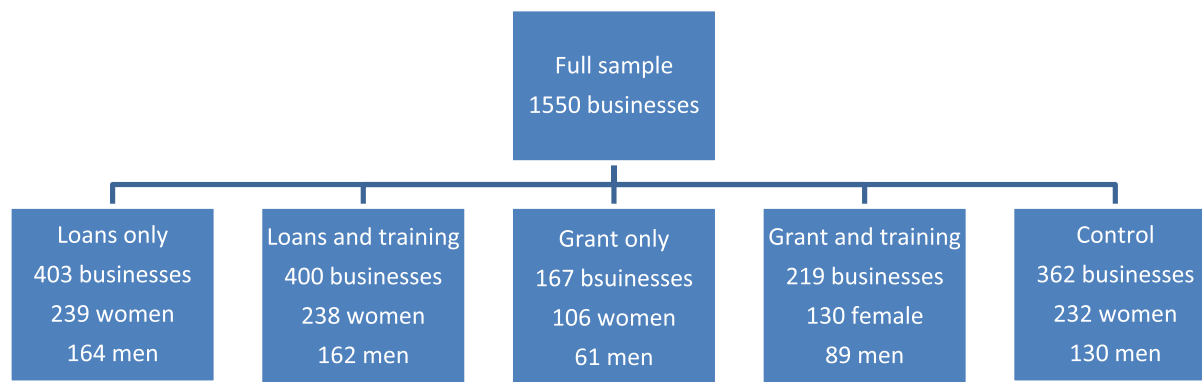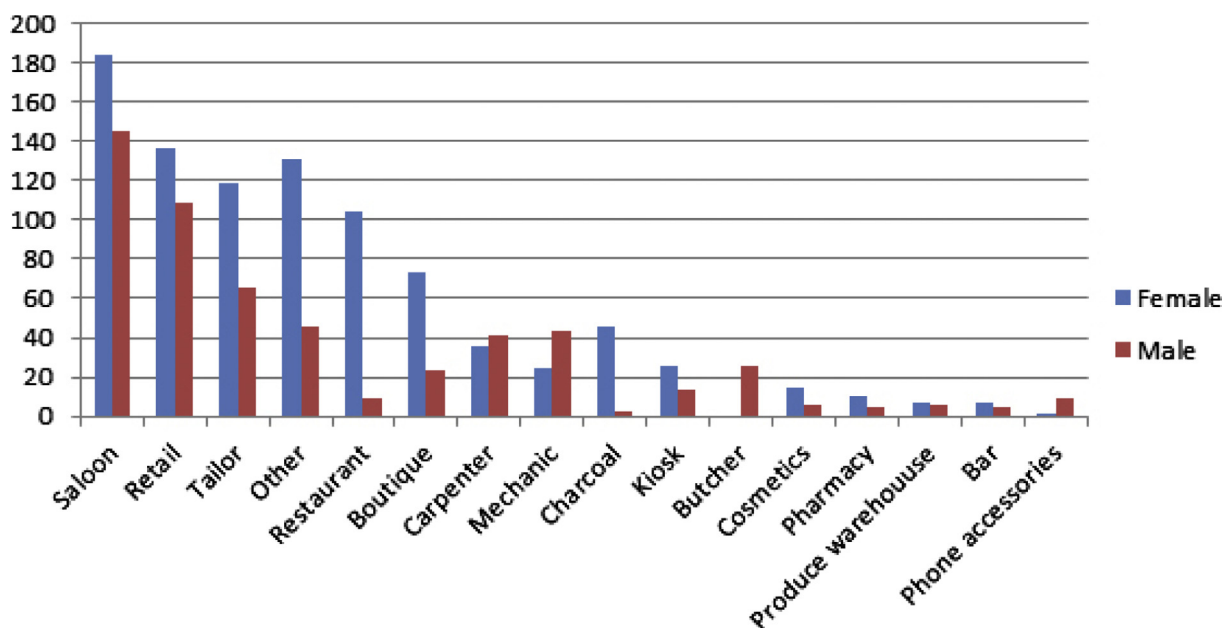
Fig. 1. Experimental design with sample sizes.

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

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.[5]

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. The distribution of loan density by month is presented in the Appendix. A regression on loan disbursement dates and profits suggests that the date of loan delivery is not correlated with outcomes.

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, Aliyev, & improve your business global tracer study, 2011) and has continued to expand. Researchers have evaluated these trainings experimentally at least twice before. Mano, Iddrisu, Yoshino, and Sonobe (2012) looked at the effect of giving training to 53 business owners. In keeping with other training results, they found business survival

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[5] Many NGOs and governments are taking interest in this way of expanding access to microcredit. 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.

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.

3. The differential role of capital in male and female microenterprises

Business owners everywhere face several constraints to expansion of their enterprises. Access to capital is a major problem for most, especially in developing economies. Microenterprises 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 and how gender can affect each.

Consider an entrepreneur with a fixed amount of capital, labor, and ability. She currently owns a business and seeks to maximize her profits. Following common assumptions for business returns, the business owner can affect her equilibrium profits through increasing labor supply or capital stock. If the entrepreneur does not face investment constraints, she will invest all of her resources 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 could still be optimal given the market conditions, though investment in the business will be relatively low. A shock to the capital stock, 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 (Grimm, Hartwig, & Lay, 2013; Jakiela & Ozier, 2014; Kocherlakota, 1996; Townsend, 1994). If an individual under such pressures receives a shock to capital, 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 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, ability and capital 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.

It is also possible that constraints to business investment, including capital, family, ability and other issues, 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 can 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.[6]

The discussion on business constraints presented here leads to several questions that I 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 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 may be 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 months and nine months after the treatments, respectively. All data collection was con-

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

ducted 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 1550 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 business skills training in the past.

Most 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.

There may still be concerns about whether men and women systematically report profits differently. However, as I am concerned with treatment effects, men and women would have to report profits differently by treatment status. This seems unlikely as individuals did not have incentives to overstate their incomes to the survey team.

The survey team also asked business owners several 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 could 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 responded 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. In addition to gender, age, marital status, whether a person had received training in the past, ability and assets are all correlated with having received a loan previously.

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 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 Appendix. Expressed interest in the loans and training programs from the full baseline sample is significantly associated with several 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.

4.2. Program take-up

As is common in the literature, while businesses expressed strong interest in the programs, take-up was not universal.[7] I briefly describe here take-up into the programs; a full analysis of take-up is presented in the 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, summarized in the 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 convinced.[8]

An analysis of selection for take-up of the programs, presented in the Appendix, does not show significant observable reasons for why some people took the trainings, loans or grants. The largest predictor of take-up for loans and grants is whether the person was offered and attended the trainings. Only experience with having attended trainings predicts whether a person attended the offered training. By the endline data collection 16% of the sample was behind in repayment of their loans in some way. This was balanced between men and women and is uncorrelated with whether an individual agreed to be surveyed.

As a first step to testing whether analysis on the loan treatments is valid, I test for whether the programs increased the number of loans individuals take. The results, presented in the Appendix, shows that 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.

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%.

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 min. Some business owners were also visited after business hours to ensure they had time to speak with an enumerator.

Of the 1550 business owners we tracked for the first follow-up survey, we found 1437 (93%). Not all of the business owners we found were willing to tell us their profits or other information: I thus have profit data on 87% of businesses. In the second follow-up (wave 4), this dropped to 86%. I have at least one follow-up data point for 1468 businesses (95% of the sample). These rates are either comparable to or higher than several studies working with similar populations (e.g., Baird, McIntosh, & Ozler, 2011; Blattman et al., 2014).

Attrition analysis, presented in the Appendix, suggest that some business and individual characteristics matter for attrition selection in the six and none-month surveys, but the characteristics of businesses that we are most interested in do not strongly predict attrition. To help minimize the potential bias from selection I conduct a bounding exercise as part of the robustness checks and find the results are robust to moderate assumptions about the attritted sample.

5. Results

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

$$Y_i = \alpha + \beta T_i + \gamma_t + \eta_i + \delta R + \varepsilon_i \qquad (1)$$

where i refers to an individual, t is time and Y_i is the outcome of interest. T_i is a matrix of dummy variables for which treatment an individual was assigned and γ_t are wave effects. η_i are individual fixed effects, R is a matrix of region and sample dummies and ε_i is

the error term. All standard errors are clustered at the individual level and are robust.[9] Following de Mel et al. (2008), I conduct fixed effects estimation to take maximum advantage of the high frequency of data collection.[10]

All analysis is divided between male and female-owned enterprises. In addition, there are several heterogeneity analyses that I conduct. 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.

To maximize statistical power, I pool the six- and nine-month survey results. The estimated coefficients are thus the impacts averaged between these two surveys. This is done following McKenzie (2012), who shows that in certain circumstances, such as enterprise profit measures, high frequency data can significantly decrease noise in measurements and thus increase power.

5.1. Business profit outcomes

As the main outcome of interest is the effect of treatments on business profits, Table 2 presents the results of estimating Eq. (1) for the full sample, and divided by male and female-owned enterprises. The results present the pooled effects for the six and nine-month surveys to improve statistical power.

Column (1) is for the full sample. None of the coefficients for any of the treatments are significant, and the estimated coefficients are very small. Column (2) is for the male only sample. I find large and very significant effects on profits for the loan and the loan with

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[9] 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.

[10] I obtain the same general results when using a Fisher exact test for randomized inference.

Table 2
Main treatment effects on business profits.

	(1) Full sample Profits	(2) Men Profits	(3) Women Profits
Loan	38.26	337.8***	−151.4
	(79.70)	(111.9)	(103.4)
Loan and Training	47.62	382.0**	−156.5
	(98.72)	(181.0)	(107.1)
Grant	−1.113	267.9	−155.0
	(162.7)	(377.4)	(126.3)
Grant and Training	−90.89	69.05	−180.9
	(87.70)	(119.2)	(118.1)
Control mean	489.96	679.59	368.92
Observations	5696	2217	3464
R-squared	0.006	0.013	0.005

Notes: Columns (1) to (3) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four treatments on business profits. Column (1) is for the full sample, column (2) for the male only sample, and column (3) for women only. Robust standard errors clustered at the individual level are in parentheses below the ITT. All fixed effects analysis includes wave dummies. * denotes significance at the 10% level, ** at 5% and *** at 1%.

training treatments. The effects represent an increase over the control group of 50% and 56%, respectively. I find no effects from the cash grants: the coefficient on the grant treatment is large and positive, but not statistically significant, while the coefficient on the grant and training treatment is both small and not significant.

I find no statistically significant effects from any of the treatments for women in column (3). All of the coefficients are large and negative, but none are significant at the traditional levels. The loan, loan and training and grant and training treatments are significant at the 15% level and represent a decrease in profits of approximately 40%.

Table 3 presents a formal test of equality between treatment arms and across the six and nine-month surveys. 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.

Overall, the results suggest that the programs did not have effects for women, though there are significant and substantial results for men from the loans. Men are seeing large increases in profits that are sustained across both the six and none-month surveys for the loan only treatment, and some increase in profits with the loan and training treatment. Women experienced no effect from the programs on profits, though there is possibly a negative effect from treatment.

5.2. Heterogeneous effects

The main treatment effects presented in Table 2 show that there is a significant heterogeneity in effects between men and women. There are potentially additional important heterogeneities in results that may be significant. In Table 4 I present the results from dividing the male sample among a set of potentially important heterogeneities. As these heterogeneities were not all prespecified before analysis, these results should be interpreted as exploratory. Some interesting differences in treatment effects do appear to be present.

Columns (1) and (2) present the results for the central and northern samples, respectively. As there was no grant program in

Table 3
F-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

Notes: This table reports tests for equality between treatments for the male and female samples across follow-up waves using an F-test.

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 increase in profits of 446,500 USH from the loan-only program, or 67% over the control group mean. There is a similar sized coefficient on the loan and training treatment, though it is not individually significant. Joint tests, however, show no significant difference in outcomes between these two groups. I do not find significant treatment effects in the northern sample, though I am not able to reject equality between the central and northern loan treatments.

Columns (3) and (4) 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 treatment impacts are statistically significant for the loan treatments for the high profit sample only, though again I am not able to reject quality between the high and low profit samples.

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 matters for impacts. The impact of the loan only treatment does not hold for those who have taken loans previously, but are only present in those who have never had a loan. The impacts for the loan and training treatment is not significant for either sample, likely due to power issues. I cannot reject that these two treatments are equal. 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 businesses benefited from the loan only treatment. 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. However, the impacts for loans when paired with training are equal, suggesting that there is not a difference in treatment effects whether the man has had a loan in the past or not.

To test the effects of individual characteristics—specifically baseline ability, patience and risk levels—on profit outcomes of businesses, Table 4 presents the results of splitting the samples into those with high and low baseline measures of ability (columns 7 and 8), patience (columns 9 and 10) and risk (columns 11 and 12). The ability index is a normalized summation of baseline measures of years of education, whether the person reported being literate, whether they have had training previously and results from a digit span recall and simple math questions. The risk measure comes from a coin flip game played with participants where they could either choose a guaranteed amount or flip a coin for a chance

Table 4
Heterogeneity effects for men only.

	(1) Central	(2) Northern	(3) High profit	(4) Low profit	(5) Previous loan	(6) No previous loan	(7) Low ability	(8) High ability	(9) Low patience	(10) High patience	(11) High risk	(12) Low risk
Loan	446.5***	197.8	419.2**	218.4	83.05	419.5***	432.5**	275.1*	495.7***	206.2	184.3	593.1**
	(150.5)	(164.4)	(162.0)	(137.6)	(175.8)	(129.3)	(195.3)	(142.5)	(180.8)	(145.2)	(113.4)	(250.1)
Loan and Training	478.5	257.0	358.8**	414.4	287.7	374.5	117.3	558.3*	418.6	396.1*	236.0	638.0**
	(309.2)	(170.1)	(180.6)	(351.8)	(196.4)	(268.8)	(145.0)	(288.1)	(285.1)	(230.8)	(236.3)	(273.2)
Grant	358.5		702.1	−259.3	723.8	−205.8	−348.9	777.3	−171.1	856.8	277.6	−2.861
	(388.3)		(684.0)	(168.3)	(800.8)	(157.6)	(258.8)	(641.1)	(164.8)	(852.8)	(450.1)	(118.3)
Grant and Training	159.5		−29.29	138.8	194.0	−114.2	−34.59	128.1	16.97	172.4	−77.13	354.8**
	(144.2)		(177.4)	(140.1)	(184.4)	(135.6)	(231.2)	(139.1)	(116.1)	(234.9)	(161.5)	(163.0)
Control mean	662.19	713.24	803.45	516.82	835.28	581.48	500.89	800.90	651.04	754.77	673.45	701.01
Observations	1465	752	1253	964	853	1344	930	1287	1045	943	1406	696
R-squared	0.011	0.031	0.013	0.048	0.026	0.022	0.019	0.022	0.034	0.019	0.009	0.046

Notes: Dependent variable is last month's profit. 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 dummies. * denotes significance at the 10% level, ** at 5% and *** at 1%.

to win nothing or twice as much. It is a binary indicator of whether they chose to play the game.

Like previous heterogeneity results, I am not generally able to reject equality of treatment effects based on ability or patience. However, there is some difference between low and high ability individuals who receive the loan and training treatment. There is also a significant difference between those that have high versus low risk preferences.

Overall, there does appear to be some difference in treatment effects based on individual characteristics, though these are not always consistent. Impacts for the loan only treatment are not statistically different based on the ability or patience level of the business owner, though the impacts are concentrated in those with lower risk preferences. For the loan and training treatment, the effects are concentrated in individuals with higher ability and lower risk measures.

5.3. Treatment effects on employees and capital

In Table 5 I look at the treatment effects on working capital, number of employees and whether the business has any employees. These outcomes may be suggestive of mechanisms for the results obtained, or may, in the case of employees, suggest potential spillover effects.

However, I do not find significant or consistent treatment effects for either capital or employees. The coefficient for capital is large for men, but is not significant for any of the treatments. For women, capital is sometimes negative, sometimes positive, but never significant.

For employees, there appears to be some impact from the training interventions for men. The individual coefficients on the loan and training and grant and training treatments are 0.344 and 0.303, respectively. These represent a nearly 20% increase in number of employees over the control group. These coefficients are not individually statistically significant, but they are jointly significant. It is thus likely that the training treatment had some positive short-run impacts on employment for male owned enterprises. However, this does not explain profit treatment effects as I do not find business profit impacts from the grant and training treatment. For women, I find no significant treatment effects on employees, though the coefficient for the grant and training treatment is large.

In columns (5) and (6) I look at whether the business employs anyone. I do not find treatment effects for the loan, loan and training and the grant treatments, but I do find large and statistically significant impacts for the grant and training treatment for both

male and female owned enterprises of 0.291 and 0.160, respectively. Relative to the control group, these represent a 56% increase in the likelihood that a male owned enterprise has an employee, and 37% for women.

6. Robustness tests and spillovers

I next present robustness tests for the main impacts obtained in Table 2. In Table 6 I present the results of different model specifications and bounding assumptions.

To test for the importance of the fixed effects model, columns (1) and (2) recreate the male and female analysis using a random effects model while columns (3) and (4) are for an OLS specification. The results for either of these models are not substantially different than the preferred fixed effects model. The coefficients decrease slightly and the significance level for the loan and training treatment for men is now at the 10% level rather than the 5%, but overall the results are substantially the same.

While attrition rates are relatively low, there may still be some biases present from selection into attrition. In columns (5) to (10) I present 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 (2007).

New lower-effect bounds are created by imputing the outcomes for the missing 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. I start with 0.1 standard deviation changes and increase them to 0.2 and 0.3.

The results of the bounding test suggest that the main outcomes obtained earlier for men are robust for assumptions up to about 0.2 standard deviations. After this, the significance levels disappear though the coefficients are still positive and economically significant. 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.

Note that the female sample is not robust to the bounding exercise. After just the 0.1 standard deviation assumption, the

Table 5
Treatment effect on employment and capital outcomes.

	(1) Men Capital	(2) Women Capital	(3) Men Employees	(4) Women Employees	(5) Men Any employee	(6) Women Any employee
Loan	2130	−441.2	−0.0208	−0.178	0.0521	−0.0170
	(1997)	(463.0)	(0.217)	(0.182)	(0.0726)	(0.0520)
Loan and Training	1728	−328.8	0.344	−0.140	0.0341	0.0334
	(1783)	(279.4)	(0.225)	(0.147)	(0.0741)	(0.0517)
Grant	1517	−190.5	−0.109	−0.113	0.0347	0.0925
	(1768)	(453.4)	(0.234)	(0.167)	(0.0860)	(0.0616)
Grant and Training	1614	616.7	0.303	0.452	0.291***	0.160***
	(1711)	(754.7)	(0.260)	(0.435)	(0.0842)	(0.0599)
Control mean	4175	2127	1.455	1.017	0.521	0.431
Observations	1570	2459	1592	2471	1592	2471
R-squared	0.006	0.013	0.045	0.028	0.072	0.039

Notes: Columns (1) to (4) report the fixed effects intent-to-treat (ITT) estimate of the impact of assignment to the four treatments on employment and capital outcomes. Robust standard errors clustered at the individual level are in parentheses below the ITT. All fixed effects analysis includes wave dummies. * denotes significance at the 10% level, ** at 5% and *** at 1%.

Table 6
Robustness tests.

	(1) Men RE model	(2) Women RE model	(3) Men OLS	(4) Women OLS	(5) Men ± 0.1 SD	(6) Women ± 0.1 SD	(7) Men ± 0.2 SD	(8) Women ± 0.2 SD	(9) Men ± 0.3 SD	(10) Women ± 0.3 SD
Loan	291.4***	−151.9	285.8**	−143.6	233.3**	−189.8**	181.1*	−242.4***	129.0	−295.1***
	(108.2)	(105.5)	(118.0)	(105.3)	(97.89)	(87.45)	(98.86)	(87.69)	(100.2)	(88.18)
Loan and Training	340.0*	−131.4	331.3*	−118.3	259.7*	−202.3**	197.0	−255.0***	134.3	−307.8***
	(178.0)	(108.9)	(187.6)	(109.2)	(152.2)	(88.54)	(153.0)	(88.81)	(154.1)	(89.32)
Grant	389.6	−122.5	429.6	−113.6	175.5	−209.2*	124.0	−260.4**	72.52	−311.6***
	(421.1)	(120.6)	(442.3)	(111.9)	(345.0)	(116.4)	(345.5)	(116.7)	(346.2)	(117.2)
Grant and Training	60.50	−118.8	60.62	−89.33	−47.01	−229.0**	−101.8	−276.5***	−156.5	−324.1***
	(91.95)	(127.2)	(103.7)	(122.1)	(130.7)	(102.2)	(132.2)	(102.4)	(134.0)	(102.9)
Observations	2217	3464	2217	3464	2416	3768	2416	3768	2416	3768
R-squared			0.012	0.006	0.011	0.009	0.010	0.010	0.009	0.012

Notes: Columns (1) to (10) report robustness tests for the specification presented in Table 2. Columns (1) and (2) are for a random effects model and columns (3) and (4) are an OLS model, compared to the fixed effects model. Columns (5) to (10) present a 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 businesses not found in a follow-up survey 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 dummies. * denotes significance at the 10% level, ** at 5% and *** at 1%.

coefficients for women are large, negative and statistically significant. This result is due to the fact that the initial results suggest no impact from the treatments, so the bounding assumptions here will mechanically lead to negative impacts.

Following de Mel et al. (2008), I also explore the effects of being in the proximity of other treated businesses of the same business type. I therefore estimate the following model:

$$Y_{it} = \alpha + \nu N_{it}^d + \theta N_{it}^d * P_t + \beta T_{it} + \tau T_{it} * \gamma_t + \gamma_t + \eta_i + \varphi R$$
$$+ \mu W + \delta M + \varepsilon_{it} \qquad (2)$$

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 the Appendix.

Including the number of firms located nearby does not change the main results reported in Table 3. The coefficients for having a 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.

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. I am also not able to fully control for demand, and so use population density as a proxy.

For an experiment like this, spillovers can sometimes be important. However, this concern is generally most important in studies that could produce general equilibrium effects, or where significant cooperation between study participants and control individuals is likely. Given the interventions are relatively small, GE effects are very unlikely. As there are many businesses in the areas where the research was conducted, and those in the treatment and control groups represent a small percentage of these businesses, significant interaction between treatment and control people seems very unlikely.

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 and business skills 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 interventions. 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. Why women owned enterprises may be experiencing a decrease in profit because of treatment is not clear. Quantitative surveys do not allow for an analysis of what was done with the loans or the grants. Qualitative discussions with women suggest that they faced several constraints to investment in their business, especially from family members. It is likely that they were unable to make productive investments in their business, and it is very likely that they drew more money from their business than the amounts they received from treatment.

The results for men, however, suggest they can make good use of capital to expand their enterprises. The loans, whether paired with training or not, led to large short-run increases in business profit. Heterogeneity analysis shows that prior experience with taking loans and risk preferences can matter for the results, suggesting that business owner characteristics are important determinants of capital usage.

The impact I find from the loans is a new finding. I argue here that previous studies have failed to account for the difference in the ability of men and women to utilize money for their business investment, and that the positive results obtained here are only due to the inclusion of a large sample of men. Most studies have focused on women, who are the main group microcredit organizations prefer to target.[11]

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 (Berge et al., 2015; de Mel et al., 2008; Fafchamps et al., 2014). The differential effects between starting and expanding enterprises suggest that capital constraints are important, but complicated. Once a business has started, there are probably important constraints to expanding beyond missing capital, including pressure to spend on the household and a lack of desire to expand businesses.

A caveat is needed regarding the short-run welfare implications of these results. 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.

These results suggest that small-scale, market-driven capital can improve business returns and lead to economic growth, though only for men. The current pessimism in the experimental literature about the effects of microcredit are likely true for the traditional model of microcredit, which focuses on a population - poor women - who in most cultures have very little control over investment

decisions. 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.

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.worlddev.2017.12.027.

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