

DO CREDIT CONSTRAINTS LIMIT ENTREPRENEURSHIP? HETEROGENEITY IN THE RETURNS TO MICROFINANCE

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ABSTRACT. Can entrepreneurship help households escape poverty? We examine subjects in urban Hyderabad, India, six years after microfinance—an intervention commonly believed to lower the cost of credit and spark business creation—was randomly introduced to a subset of neighborhoods. We find large benefits both in business scale and performance from giving “seasoned entrepreneurs”—those who started a business before microfinance entered—more access to microfinance. Notably, these effects persist two years after microfinance was withdrawn from Hyderabad. However, any persistent benefits to “novices,” those without prior businesses, are much more meager and generally indistinguishable from zero. A structural model of the production functions of seasoned vs. novice entrepreneurs suggests that marginal returns to capital for the former are 3-4 times as large as for the latter. These results suggest that heterogeneity in entrepreneurial ability is important and persistent; and that lenders entering a new market may be better off by focusing on borrowers at the intensive rather than extensive margin. We also provide some of the first evidence on the relationship between formal and informal credit from an individual’s social network. While microfinance crowds out informal finance for the novices, the informal financial relationships of seasoned entrepreneurs exhibit complementarities with microfinance.

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1. INTRODUCTION

One striking stylized fact about low-income countries is the firm size distribution. Many researchers have noted the high density of very small firms and the absence of medium and large enterprises relative to more developed countries (Hsieh and Olken (2014)). One explanation for this pattern is binding credit constraints that limit firm growth (Banerjee and Duflo (2005)). In this paper, we tackle credit constraints and limits to entrepreneurship at the very bottom of the firm size distribution using a field experiment that randomly introduced microfinance across neighborhoods in a large city in India. We focus on both the extensive margin of firm creation as well as the intensive margin of existing firm growth in response to the relaxation of credit constraints (see Kerr and Nanda (2009) and Bertrand et al. (2007) for the case of the US and France). We further investigate the role of entrepreneurial heterogeneity, which may be especially important in the case of microfinance, which by design does not perform any screening of potential borrowers.¹ Namely, we implicitly ask whether entrepreneurs are indeed credit constrained and if so whether relaxing those constraints removes costly barriers to entry (such as barriers to experimentation as in Kerr et al. (2013)) or instead encourages low quality firms to enter.

One of the most exciting ideas in the microfinance movement is that the lending does not have to be selective; essentially almost any poor person can get a small loan from an MFI. Behind this is the thought articulated most clearly by Muhammad Yunus when he said that the poor are natural entrepreneurs—it follows that they can always make good use of the loan—there is always a profitable business opportunity available to take. The only reason why their businesses haven't already been able to grow is credit constraints. We call such entrepreneurs gung-ho entrepreneurs (henceforth GE). Formally, we will assume that these entrepreneurs have access to high productivity technologies and that they would be willing to borrow more, even at high interest rates. When microfinance becomes available, credit supply increases and entrepreneurs are able to expand their businesses (or start new ones).

An alternative view is that even though a large fraction of households use their new-found access to credit to start a business, they do so because of labor and capital market imperfections that prevent them from getting a job. Presumably if they could simply work longer hours without setting up the business they would do so—but that might not be an option. We call such entrepreneurs reluctant entrepreneurs (henceforth RE). Formally we will assume that these people have access only to low productivity business technologies. When microfinance becomes available, the cost of capital falls to a level such that they are willing to select into entrepreneurship.

¹Because microfinance is non-collateralized and because the legal institutions in developing countries are weak, microfinance is designed to be informationally insensitive. Loan monitoring and screening are both outsourced to borrowing groups, and repayment rates hover close to 100%. Therefore, even if lenders have the ability to screen at a low cost, they have no incentives to do so.

While these are clearly extreme types, the basic distinction is quite robust and can be expanded to cover a variety of other settings. The purpose of this paper is to study the impact of microfinance through the lens of this particular distinction. We focus on whether microfinance can help businesses at the extreme end of the firm size distribution to grow and if so, whether the biggest impacts are found at the intensive or extensive margin of entrepreneurship.

The average impact of microfinance has been the subject of a recent wave of rigorous experimental research (including [Angelucci et al. \(2013\)](#), [Augsburg et al. \(2012\)](#), [Atanasio et al. \(2011\)](#), [Banerjee et al. \(2014\)](#), [Crépon et al. \(2011\)](#), and [Tarozzi et al. \(2013\)](#)) in a diverse set of countries and settings. This body of short to medium-run evidence paints a consistent picture of moderate impacts. Access to microfinance is generally found to cause moderate business creation and business expansion, but the evidence on growth in revenues and profits is at best weak. There is some evidence that borrowers do purchase more household durables and business assets, but almost no evidence of an impact on non-durable consumption or on other indicators of welfare such as health, education, or most measures of women's empowerment.

This paper uses a new round of data from the randomized experiment in the city of Hyderabad, India, reported in [Banerjee et al. \(2014\)](#) to document that the modest average impact (which we again find) masks very large differences between seasoned entrepreneurs who are those who had started businesses before microfinance entered the Hyderabad market (intensive margin) and novices, who started their business after microfinance became available (extensive margin). The former had opted to start their businesses when credit constraints were severe and interest rates likely high. The latter started the business in response to an offer of relatively inexpensive credit without any real screening.

Our hypothesis is that the former group, whom the market had selected either through screening on eligibility for credit or by setting a high interest rate, are likely to be more like what we call GEs, while the latter who did not face as much of either form of screening might be more like what we call REs.

We find the evidence fits very well with this simple theory. The evidence is based on the comparison of the original treatment neighborhoods that were first exposed to microfinance 6 years before our survey in 2012 and original control neighborhoods, which also got access to microcredit about four years ago.² About two years before the survey we use here, in 2010, both treatment and control neighborhoods were exposed unexpectedly to an ordinance by the AP government that forced the withdrawal of all microfinance

²In 2005, an MFI, Spandana, selected 104 areas within Hyderabad in which it was willing to open branches. Half of the areas were randomly selected to receive branches, while the remainder were allocated to control. Spandana then progressively began operating in the 52 treatment areas between 2006 and 2007. After an endline survey in 2007-8, Spandana moved into the control areas starting in mid-2008. A second endline survey was conducted in early to mid 2010. The results of these two studies are discussed in [Banerjee et al. \(2014\)](#).

from the state. Perhaps because of this break in the flow of credit, we do not see full convergence in access to microcredit in these two sets of neighborhoods. Households in the original treatment neighborhoods were significantly more likely to have ever borrowed from a microfinance institution (MFI) and in 2010, their outstanding MFI loans were worth on average Rs. 1,000 more.³The comparison therefore measures the *persistent* effects of longer exposure to microcredit.

Our average treatment effects echo the results from the previous literature showing that microfinance access promotes business growth: there more businesses in treatment neighborhoods, and business asset stocks and durable purchases are larger, but so are wage bills of businesses. Households in treatment neighborhoods also work more hours in self-employment activities, and their businesses have (marginally) significantly higher revenues and expenses. These findings suggest that effects from microfinance access are both increasing in length of exposure and persist even when microfinance is no longer available. We again find no evidence of overall increases in consumption or in spending on health or education.

However most of the business impacts are driven almost entirely by those who had a business before microcredit became available. For these firms, asset stocks, investment, self-employment hours, business expenses and revenues, are significantly higher in treatment neighborhoods. Moreover, the magnitudes are substantial: self-employment hours increase almost 20%, the stock and flow of business assets increase by 35-40%, business expenses increase by 80% and revenues more than double, relative to seasoned entrepreneurs in control.

We also find positive and significant effects on the profits of the top tercile of the seasoned entrepreneurs, and positive and significant effects on per-capita consumption for much of the top half of the distribution of the same group. Household durables also appear to increase for the seasoned entrepreneurs.⁴

In contrast, for the rest of the population almost all the effects are insignificant and small in magnitude. However as far as business outcomes are concerned this is mostly driven by the fact that not many households start businesses after 2006 either in treatment areas or in control.

To look at the selection issue more closely we compare the households in treatment and control that set up a business during the roughly 24 month period when microcredit was available in treatment areas but not in control. This comparison mixes the treatment effect of getting better access to credit, which ought to be positive, with the selection

³Note that this “intent to treat” or ITT estimate is driven both by the fact that treatment households had longer access to microfinance, and hence larger loans (given the fact that loan sizes increase with each completed loan cycle) and by the fact that treatment households are more likely to have ever borrowed.

⁴If household durables, which include both items like gold and those like television, are a combination of savings and consumption, this suggests that the income gains experienced by seasoned entrepreneurs are partly saved and partly consumed.

effect of starting business without the extra credit, which we expect to be negative. The net effect turns out to be almost always negative (though only sometimes significantly so) which tells us that there is a strong selection effect.

We also see the predicted differences on the borrowing side. While we see no differences in informal borrowing on average, an index of whether the household has an informal loan (which is the typical recourse for this financially constrained population) goes up for the seasoned entrepreneurs (the p-value is 0.14 i.e. it just misses being significant at the 10% level) but goes down significantly with treatment for the rest of the population. The difference of the two is very significant. The aggregate amount of informal borrowing also goes up by a fifth of the control mean for the seasoned entrepreneurs (and the increase is significant at the 5% level) while it goes down for the rest of the population, though the effect is not significant.

The fact that the seasoned businesses in treatment were the only ones who also expanded their informal borrowing, is consistent with another striking fact. A unique feature of our data is that we collect data on eight dimensions of network ties for all of the respondents in our sample. We find on average households in treatment have fewer links to other households (lower average degree). However this crowdout effect is missing for the seasoned business households and is driven by the rest. In other words, the seasoned entrepreneurs seem to have been careful to keep their options open in terms of being able to access other households for credit or other help, whereas the rest seem to have neglected to cultivate those links. This seems consistent with the arguments in [Ligon et al. \(2000\)](#) suggesting that giving individuals access to savings or credit may crowd out network transactions by increasing the temptation to “renege” on loan repayment or reciprocal transfers. On the other hand, [Feigenberg et al. \(2010\)](#) provide experimental evidence showing that the social aspects built into microfinance itself can help to foster enhanced risk-sharing relationships between borrowers. However we do not find that treatment households are more likely to list members of their former MFI borrowing groups in our network elicitation than control households. Only the seasoned business households also name more individuals from their previous microfinance groups as members of their networks, though these effects are concentrated among non-financial links.

Overall the two groups of borrowing households seem to be on quite different trajectories, consistent with our characterization of them as GEs and REs. Of course there are other possible interpretations—in particular it could be in principle a transitional phenomenon. Perhaps it just takes a long time to get started and that eventually the original selection will not matter and these firms will become like the old businesses. Or, these firms are learning about their own types by entering and most of them will exit eventually leaving only the right firms—microcredit is valuable because it encourages experimentation (See [Kerr et al. \(2013\)](#). [Karlan et al. \(2012\)](#) also suggest a theory along these lines.). The

fact that our results are at least 4 years after these firms were started suggests that if this is to be the case, transitions have to be very slow. We also provide some other evidence arguing against the transitions view but this possibility cannot be entirely ruled out.

If our interpretation is correct then the non-selective nature of microcredit is less of an advantage than it is usually claimed. While we have no reason to question the fact that even the REs benefit from the loan (see [Angelucci et al. \(2013\)](#) who carefully explore the possibility that some groups end up doing worse off from microcredit), there may be a case for focusing more of the energy on identifying the GEs and helping them grow.⁵

The idea that there may be heterogeneity in the response to microcredit is not new. [Angelucci et al. \(2013\)](#) and [Banerjee et al. \(2014\)](#), are evaluations of microcredit which examine potential heterogeneity in the outcomes and [Karlan et al. \(2012\)](#) make the general point that heterogeneity may be a central piece of the story. Both [Angelucci et al. \(2013\)](#) and [Banerjee et al. \(2014\)](#) find more positive results for old business owners than for the rest of the population; this paper is in a sense a follow up of [Banerjee et al. \(2014\)](#). However the results for old business owners are much stronger and positive four years later than they were in 2008 pointing to a continuing divergence as a result of getting the original credit shock. The results for the new business owners, on the other hand, who were doing (weakly) worse in treatment areas than in control in 2008, do not get any more positive—these firms continue to do no better than firms that, at best, got access to microcredit a year or more later. Our results confirms that this is not simply a transitional phenomenon in the conventional sense.

We also look at a set of outcomes not emphasized in the literature. In particular we show that the divergence also shows up in borrowing behavior. The average household borrows less in treatment from informal sources but the seasoned business owners borrow more; they also do not become less connected as a result of getting microcredit access, unlike the average household, which does. This is potentially important from the policy point of view—if microcredit crowds out informal connections and these links are costly to reestablish after microcredit is gone (we see that people in treatment have fewer links several years after microcredit is shut down—then policies need to take this into account.

A natural question is how much better microfinance could do at allocating capital if it screened for GEs ex ante. To make progress toward answering this question, we present a structural model which we plan to use to conduct counterfactual analysis. In our model, the GEs and REs differ in the production technologies available to them—GEs have access to both a high- and low- productivity project, while REs only can take the low project. Further, we allow the high productivity project to operate only after a fixed cost has been paid. In this world, credit constrained GEs may not have the resources to take the high productivity project, and saving to accumulate enough wealth might take a very long time.

⁵[La Porta and Shleifer \(2008\)](#) make the case that most of the firms in the informal economy are marginal to main story of growth.

When microfinance enters, the credit constraints are relaxed for both the GEs and the REs. For preliminary parameter estimates, the model is consistent with our key results.

The remainder of the paper is organized as follows. In section 2, we describe the experimental setup of Banerjee et al. (2014), details of the AP microfinance ordinance, and our 2012 survey activities. Section 3 displays our main results. In Section 4, we present our model, and in Section 5, we conclude.

2. DATA AND EXPERIMENTAL DESIGN

2.1. Setting and Previous Work. We build upon two existing rounds of panel data collected by Banerjee et al. (2014). As discussed in that paper, 104 neighborhoods in Hyderabad were randomized so that 52 received access to credit from Spandana, a large lender that was then moving into Hyderabad, starting in 2006. The remaining neighborhoods only received access in mid-2008 after a round of data collection conducted in 2007 - early 2008. A second round of data collection was conducted in mid-2010 to examine longer-term impacts of access to microfinance. Coincidentally, this second endline was completed just a few months before the microfinance landscape abruptly changed, as we discuss below. Figure 1 shows the timeline of the data collection as it relates to the timing of this change.

Banerjee et al. (2014) examined the effects of the intervention on outcomes measured in 2007-8 and in mid-2010.⁶ Key outcomes examined in that work include borrowing from various sources, consumption, business creation, and business income, as well as measures of human development outcomes such as education, health, and women's empowerment.

At the first (2007-8) endline, households do borrow more from microcredit institutions (though fewer than a third of treated households borrow). No significant difference was found on consumption, but there were significant positive impacts on investment in durables. Treated households start more businesses, and invest more in the businesses that were already in existence before microcredit. The average profits of these existing businesses increased, with particularly large gains at higher quantiles, while the median marginal new business is both less profitable and less likely to have even one employee in treatment than in control areas.

At the second (mid-2010) endline, when microcredit was available both in treatment and control groups but treatment group households had the opportunity to borrow for a longer time, businesses in the treatment group have significantly more assets. But the average business is still small and not very profitable, though, once again, a tail of businesses appear to experience gains from longer microfinance access. There is still no difference in average consumption. No effect was found on women's empowerment or human development outcomes either 18 or 36 months after the initial treatment.

⁶As described below, the survey instrument for this paper is based on that used in Banerjee et al. (2014), to facilitate comparisons across time, although new modules were added.

These results, as well as those of other papers such as [Angelucci et al. \(2013\)](#); [Crépon et al. \(2011\)](#), hint at important heterogeneity. However, many unresolved issues remain. Since during the 2006-2010 period, treatment households always had access to microfinance, one question is whether the impacts seen, particularly those on business outcomes, are sustainable in the absence of continued access to new loans. Another question is whether newly created businesses would, given more time, catch up to the existing businesses, or whether they are on permanently different trajectories. These are among the questions we address in this paper.

2.2. Andhra Pradesh Microfinance Ordinance. The second round of endline data analyzed in [Banerjee et al. \(2014\)](#) was collected in mid-2010, only a few months before the Andhra Pradesh (AP) state government put forth a sweeping new regulation of the microfinance sector. On October 15, 2010, the AP government unexpectedly issued an emergency ordinance (The Andhra Pradesh Micro Finance Institutions Ordinance, 2010) to regulate the activities of MFIs operating in the state. The government was worried about widespread over-borrowing by its citizens and alleged abuses by microfinance collection agents. The provisions of the Ordinance (promulgated as a law in December 2010) brought the activities of the MFIs in the state to a complete halt. Under the law (which still stands), MFIs are not permitted to approach clients to seek repayment and are further barred from disbursing any new loans.⁷ In the months following the ordinance, almost 100% of borrowers in AP defaulted on their loans.⁸ Furthermore, Indian banks pulled back tremendously on their willingness to lend to any MFI across the country, and MFIs even outside of Andhra Pradesh were forced to contract their lending activities, at least temporarily. In mid-2011, the Reserve Bank of India (RBI) issued new guidelines for the microfinance sector and established itself as the national regulator for the industry. While the environment for MFIs in the rest of India has improved since 2010 in large part due to the RBI's actions, MFIs in AP still are not permitted to operate under state law and have been unable to collect on their loans or issue new credit.

The respondents surveyed for the [Banerjee et al. \(2014\)](#) study experienced the direct consequences of the AP ordinance. Approximately one third of respondents reported having a loan outstanding at the time of the second endline survey in mid-2010, and close to 50% had taken at least one microloan from any lender between 2004 and 2010.⁹ During October 2010, the respondents became aware of the Ordinance through widespread television and print advertising campaigns. In informal conversations during 2011 and 2012, many respondents told members of the research team that they had not seen any

⁷However, it is not illegal for borrowers to seek out their lenders to make payments.

⁸We investigate the effects of this “windfall” in a companion paper ([Banerjee et al., 2013a](#)).

⁹See Table 4, columns 3 and 4, respectively.

loan officers since 2010. In compliance with the law, none of the respondents had been given the opportunity to take a new loan.

Currently, the Government of India is at a crucial juncture in the debate about the regulation of microfinance. There has been a shortage of rigorous empirical evidence on the effects of the AP government’s actions on India’s credit markets specifically, and guidance for regulators in general. The RBI guidelines that were released in 2011 did apply new regulations to the entire microfinance sector. In order to be eligible to receive a priority sector designation,¹⁰ MFIs should charge no more than 26% interest and earn no more than 12% margin¹¹ on their loans.¹² The regulations also stipulate that “total indebtedness of the borrower not to exceed [Rs.] 50,000,” and borrowers cannot borrow simultaneously from more than two MFIs.¹³¹⁴ This study aims to provide needed evidence to the government, policymakers and other stake-holders about the longer run, persistent implications of microfinance and the differential effects exposure microfinance has on different types of borrowers.

2.3. Follow-Up Data Collection. In mid-2012, we returned to the respondents of the 2010 survey round of Banerjee et al. (2014) and conducted a follow-up survey with 5,744 households located in 103 of the original 104 combined treatment and control neighborhoods.¹⁵ At the time of the survey, it had been 6 years since the original treatment group was first exposed to microfinance and 4 years since the control group had gained access to microfinance from Spandana, the implementing partner. All of the respondents experienced a simultaneous withdrawal of microfinance from Hyderabad in response to the AP ordinance shortly after the 2010 survey round. Therefore, when we compare outcomes between the original treatment and control groups, we measure the impacts of the intensity of *past* exposure to microfinance against a backdrop where microfinance is no longer available.

Table 1 provides a description of the households surveyed in the 2012 round. The table displays the means of demographic, consumption, and business outcomes for households in the control group. We also include information about the borrowing behavior of these

¹⁰The priority sector designation allows MFIs to obtain bank credit at lower interest rates.

¹¹i.e the spread between the interest rate and their own cost of funds

¹²It should be noted that the absolute interest cap was subsequently removed from the regulation, but that the margin cap still effectively caps interest rates.

¹³<http://www.rbi.org.in/scripts/NotificationUser.aspx?Id=6376&Mode=0>

¹⁴The rules on borrowing limits are enforceable due to the recent rise of microfinance credit registries in India.

¹⁵One (treatment) area was dropped because it was used for piloting. It was crucial to pilot in an area where past waves of surveying had taken place since familiarity with surveyors significantly increases households’ willingness to respond accurately. All our results below control for strata dummies from the original strata assignment and therefore also omit the control area assigned to the same stratum.

households at the time of the second endline (2010), which is a close proxy for the household borrowing right before the AP crisis. Note that approximately 30% of the control group had an outstanding microloan at that time.

In addition to the outcomes analyzed in [Banerjee et al. \(2014\)](#), we added survey questions about the respondent’s social network, a module to capture the household’s worries, happiness, and time preferences, and retrospective questions about the household’s exposure to the AP crisis and desire to borrow from MFIs in the future. Due to the size of Hyderabad and the high likelihood that household connections cross neighborhood boundaries, a complete network elicitation in the style of [Banerjee et al. \(2013b\)](#) was not feasible.¹⁶ Instead, we asked each respondent to list the individuals with whom they engaged in 8 different activities: (1) borrowing or lending cooking fuel (kerosene); (2) borrowing or lending milk or sugar; (3) borrowing or lending Rs. 100¹⁷; (4) giving or receiving advice about financial matters; (5) giving or receiving advice about a child’s schooling; (6) giving or receiving advice about finding housing; (7) giving or receiving advice about health concerns; and (8) watching television together. For each activity, we asked about hypothetical interactions in the future and about actual interactions in the past. For each name listed, we also asked about when the relationship began; we further ask if there is a third individual who engages in that same activity with both the respondent and the reported link.¹⁸ We classify the first four activities as financial and the last four activities as non-financial. After the respondent listed all of the names of the individuals relevant for these eight types of activities, we then randomly selected three of the financial and two of the non-financial links and asked a follow-up mini survey about each individual. This brief questionnaire included information on demographics, assets, income-generating activities, geographical proximity, and whether the respondent had ever been in a microfinance group, self help group (SHG), or rotating savings and credit association (RoSCA) with the individual. We included a supplemental set of questions to ascertain network position in the spirit of [Zheng et al. \(2006\)](#). Table 2 presents summary statistics of the network relationships for the original control group households. The average household in the control group has a degree (number of social connections) of approximately 6. Of these links, 4.4 are engaged in financial activities with the respondent. Of the 6 connections that the average household lists in the elicitation, only 16.4% percent of them were involved in microfinance with the respondent.¹⁹ Further, almost all of the friends that

¹⁶[Banerjee et al. \(2013b\)](#) collected network data for 75 villages by first taking a complete census of each village and subsequently revisiting each household to record information about their relationships with other. This type of survey method, while the gold standard, is extremely resource intensive even in rural areas.

¹⁷About \$5 at PPP-adjusted exchange rates [World Bank Group \(2012\)](#).

¹⁸The answer to this question provides a measure of network support. [Jackson et al. \(2012\)](#) have shown theoretically that supported links can be helpful in enforcing cooperation and favor-exchange in networks.

¹⁹We do not know, however, what fraction of former microfinance group members are still listed as network connections in 2012, as we do not have access to group rosters from before the AP ordinance.

the respondents listed who were also engaged in microfinance with the respondent (0.555 links) were also connected to the respondent *before* microfinance entered in 2006 (0.550 links).

2.4. Empirical Design and Threats to Validity. We aim to use the empirical setting to explore the long-run, persistent impacts of microfinance. As in [Banerjee et al. \(2014\)](#), we focus on intent to treat (ITT) comparisons between the initial treatment neighborhoods and control neighborhoods. We would like to interpret the results of such comparisons as the impacts of receiving microfinance for two additional years in the past. We consider a few important issues relating to the interpretation of these comparisons.

Recall that the implementing partner of the original study was Spandana, one of the largest MFIs in India at the time. The original treatment group received access to Spandana in 2006, but the control group was not permitted to borrow from Spandana until 2008. As discussed in [Banerjee et al. \(2014\)](#), other MFIs entered Hyderabad between 2006 and 2008, when the control group was treated. That the control group had access to microfinance before Spandana entered may make the initial treatment less powerful, but it does not invalidate the original experimental design.

We interpret the comparisons between treatment and control as measuring the effects of increased exposure to microfinance in general. The loans offered by Spandana were very similar to those of the competitors operating in Hyderabad at the time. Borrowers, who were organized into joint liability groups, met on a weekly basis and made weekly installment payments. At the successful completion of a loan cycle, borrowers were offered larger loan sizes for subsequent cycles. In fact, conversations with former borrowers in 2011 indicate that residents of Hyderabad viewed the lenders as exchangeable. Many borrowed from several lenders at a time. We will further discuss the treatment intensity in section 3.1.

It is also important to understand the differential repercussions of the AP ordinance on the treatment and control groups. Note that the effects were twofold. First, all households uniformly lost access to future credit. Second, households with outstanding loans received an implicit write-off of the remaining principal and interest. Thus borrowers who had received a new loan just before the ordinance received a large loan forgiveness, while those who were close to fully repaying the loan and obtaining a new loan received a small loan forgiveness. In this paper, we would like to interpret differences in the treatment versus the control group as coming through increased past exposure to microfinance and to nothing else. However, if individuals in the treatment group had different-sized windfalls when microfinance was withdrawn, then the comparison would be muddied. In [Table 3](#), we compare different measures of the loan forgiveness windfall between the treatment and control groups. These coefficients come from an OLS regression of windfall size (i.e. total loan amount outstanding as of the crisis) on an indicator for original treatment status.

We find no evidence that the likelihood of having a loan or the size of the windfall at the time of the crisis differed at all between the two groups. This supports our interpretation of the treatment effects we identify as coming solely through the length of past exposure to microfinance.

3. RESULTS

Following Banerjee et al. (2014), we estimate ITT impacts of increased access to microfinance on a range of outcomes. The average treatment effects regression takes the form

$$y_{ia} = \alpha + \beta \times Treat_{ia} + X'_a \gamma + \varepsilon_{ia}$$

where y_{ia} are outcome variables (generally measured in 2012), $Treat_{ia}$ is an indicator for treatment neighborhoods in the original study (where microfinance entered in 2006), and β is the coefficient of interest. X'_a includes area-level strata variables such as population, total number of businesses, availability of credit, literacy rates, and consumption per capita.²⁰ For all specifications, standard errors are clustered at the area level.

While we are interested in tracking the average impacts of microfinance over the entire population, we are especially keen to understand the differential impacts for seasoned and novice entrepreneurs. For these specifications, the regressions take the form

$$y_{ia} = \alpha + \delta SE_{ia} + \beta_1 \times Treat_{ia} + \beta_2 SE_{ia} \times Treat_{ia} + X'_a \gamma + \varepsilon_{ia}$$

Here, we indicate that household i in area a is a seasoned entrepreneur by setting $SE_{ia} = 1$. The coefficient β_1 can be interpreted as the treatment effect on the novice group, while the coefficient β_2 is the differential treatment effect for the seasoned entrepreneurs above and beyond the impact on the novices. Thus, the total treatment effect for the seasoned entrepreneurs is $\beta_1 + \beta_2$.

The following sections discuss results for intent-to-treat estimates of treatment effects on multiple sets of outcomes. For most, we present each set of results in a regression table with two panels: Panel A shows average treatment effects for each outcome variable (i.e. the first specification described above), while Panel B shows heterogeneous effects by entrepreneurial status (i.e. the second specification described above). We further show the p-values of the total treatment effect $\beta_1 + \beta_2$ for the seasoned entrepreneurs at the bottom of each table.

3.1. Exposure to Microfinance. We aim to identify the persistent, longer-run impacts of microfinance two years after the withdrawal of microfinance from the entire state of Andhra Pradesh. Before we can investigate the outcomes of interest such as business growth and consumption, it is important to understand how the exposure to microfinance

²⁰Altogether, there were 52 strata. Randomization was done at the matched pair level conditional on being in the same stratum.

was affected by the initial treatment status. Over the course of the three survey rounds, we have collected a number of measures that capture the exposure to microfinance. Table 4 presents the treatment effects for a set of these measures.

A natural measure of exposure is the likelihood of ever borrowing from any MFI. Panels A and B contain regressions of indicators for past borrowing at different points in time on treatment status. In column 1 of panel A, the outcome is an indicator for ever borrowing at endline 1 (in 2007/2008). As reported by Banerjee et al. (2014), treatment households were approximately 11 percentage points more likely to have ever borrowed than control households. Columns 2 and 3 measure the incidence of borrowing around the time of endline 2 (2010). Column 2 captures any borrowing from microfinance between endline 1 and endline 2, while Column 3 reports the effects of the initial treatment on having a loan outstanding at the time of the second endline. There are no detectable differential impacts on borrowing just before or at the time of endline 2. Recall that the AP Ordinance outlawed microfinance just months after endline 2 was administered. Thus, we interpret the endline 2 measures as a proxy for the credit outstanding that would eventually be affected by the regulation change. This evidence suggests that by 2010, the control group had caught up to the treatment group in terms of access to credit. However, the treatment group did get a head start. In column 4, we consider an indicator for whether the household ever reported borrowing at any time in any survey round. This is the union of the outcomes from Columns 1-3 and a retrospective question asked at the time of endline 3. We do see that while approximately 50% of the control group had ever borrowed before the AP ordinance, households in the treatment group were 4.4 percentage points (a 9% increase) more likely to have ever borrowed. Thus one interpretation is that the treatment increased exposure to microfinance along the extensive margin.

The original treatment could have also affected households via the intensive margin, namely the number of loans taken over time, the number of MFIs from which the household borrower, and the total amount of credit taken. Panels C and D focus on this intensive margin. All outcomes in these panels are snapshots at the time of endline 2.²¹ Here, Column 1 is identical to Column 3 of Panels A and B, and captures whether a household had an active loan at the time of endline 2. Columns 2 and 3 explore the number and the total value of the MFI loans outstanding at the time of the second endline survey. While the number of MFI loans²² is no different in treatment and control neighborhoods, the overall amount of credit is larger in treatment areas. The average treatment household reports Rs. 946 more borrowing than the average control household. This amounts to a

²¹We would ideally also like to measure each household's total stock of microfinance taken between 2006 and October 2010 from all MFIs. However, we do not have access to this information. In Panels A and B we can construct a proxy for ever borrowing at any time. However, we trust available proxies for total amount taken over time much less.

²²This can be interpreted as the number of lenders.

14% increase in credit over the control group. Because treatment group borrowers had earlier access to microfinance through Spandana, this effect may capture the fact that most microlenders increase the loan size offered to clients over time.²³ In column 4, we report that treatment households are 50% more likely to have a Spandana loan than households in the control group, and that they also have Rs. 1,132 more credit from Spandana in 2010.

We next ask whether the exposure treatment effects vary between seasoned entrepreneurs and novices. Panels B and D capture the heterogeneous treatment effects. On the extensive margin, we cannot detect any significant differences in ever borrowing from a microfinance institution between seasoned entrepreneurs and novices. However, the point estimate for the differential impact on the total amount of MFI credit taken in 2010 is large, although insignificant. We do find a treatment effect on the amount borrowed from Spandana in 2010 of Rs. 800 for novices. This treatment effect is twice as large for the seasoned entrepreneurs.

Overall, households in the original treatment neighborhoods, started borrowing earlier and were more likely to ever borrow from an MFI. They also had more credit outstanding before the AP crisis. Though we cannot measure the total value of loans ever taken from microfinance, this evidence also suggests that treatment households borrowed for longer (more loan cycles) and had a larger overall stock of microfinance credit. We also find some suggestive evidence that the seasoned entrepreneurs took larger loans (but were equally likely to borrow) from lender.

Finally, we note that there is no single sufficient statistic that captures all of these effects. In the results that follow, we focus on the reduced form ITT treatment effects and do not attempt to include an IV or Wald statistic interpretation of the effects on other consumption and business outcomes.

3.2. Business Outcomes. Table 5 reports treatment effects on outcomes related to household businesses. We find that the effects of microfinance on business creation described in Banerjee et al. (2014) persist even in the absence of ongoing microcredit: treatment households were 3.8% more likely to have a business, and own 0.056 more businesses on average, than control households (Panel A, columns 1 and 2). (They were also just under 1% more likely to have closed a business in the last 12 months [column 3].) Moreover, treatment households's businesses are larger than those of the control group. Treatment households are 3% more likely to own a business with more than one employee (column 5) and have 0.21 more employees in their largest business (column 6); they also pay out Rs. 370 more in wages to employees each month, more than 100% of the control group mean (column 8). Businesses in the treatment group are larger along other dimensions as well. Households in the treatment group have over Rs. 2,000 more in business assets

²³Increases of between Rs. 2,000 and Rs. 5,000 are common each year.

than households in the control group (column 9), and report 31% higher expenses and 36% higher revenues from their businesses than the control group (columns 10 and 11).

Yet as Panel B shows, these results are driven almost entirely by effects on seasoned entrepreneurs alone. Seasoned entrepreneurs in the treatment group are 6.4% more likely to own a business and own, on average, 0.10 more businesses than seasoned entrepreneurs in the control group (columns 1 and 2). Their businesses are larger, as well: seasoned entrepreneurs in treatment are 5.7% more likely to own a business with multiple employees (column 5) and pay out Rs. 587 more in monthly wages to employees (column 8). They also own over Rs. 4500 more in business assets (column 9) and report spending 83% more in business inputs and receiving 104% more in business revenue (columns 9 and 10). In contrast, these same outcomes for novice entrepreneurs in the treatment group are no different than those for novices in the control group, with two exceptions: novices in the treatment group have .174 more employees in their largest business and pay out Rs. 275 more in wages than novices in the control group (columns 7 and 8).

These results for business inputs and revenues for seasoned entrepreneurs in the treatment group suggest that their businesses not only are larger, but also generating more profits than seasoned entrepreneurs in the control group. Figure 2, Panel A plots the results of bootstrapped quantile regressions for business profits on treatment status for seasoned entrepreneurs. As this figure shows, a large section of the distribution of households by business profits (from around the 75th to 95th percentiles) experienced significant positive treatment effects on their business profits. No portion of the distribution for novice households, on the other hand, experienced such results, as Figure 2, Panel B shows.

3.3. Household Labor Supply. Table 6 reports effects both on total household labor supply (column 1) and on household labor supply broken into three categories: self-employment (i.e. business) labor (column 2), wage labor (column 3), and casual labor (column 4). As Panel A shows, treatment households work 2.75 more hours per week in their businesses than do control group households. Although the estimates of treatment effects on total labor supply (2.17 hours), wage labor supply (0.351 hours), and casual labor supply (-0.937 hours) are not statistically significant, these results are suggestive, when taken together, of treatment households increasing their total labor supply by both increasing the number of hours they work in their business and substituting away from casual labor.

However, as Panel B reveals, there is significant heterogeneity in these treatment effects. Novice entrepreneurs in the treatment group show no significant differences in their labor supply relative to novices in the control group. Seasoned entrepreneurs, on the other hand, show multiple significant treatment effects: seasoned-entrepreneur households in the treatment group work an additional 6.65 total hours per week relative to the control group (column 1), of which 5.827 hours are in self-employment (column 2). Thus, not

only do seasoned entrepreneurs in treatment neighborhoods have larger businesses several years after the introduction of microcredit; they are also contributing more labor time to their businesses on a weekly basis.

3.4. Consumption. Table 7 shows intent-to-treat estimates for treatment effects on household spending. As Panel A, column 1 shows, we find no significant average effect of increased exposure to microfinance on monthly consumption per adult equivalent. Once again, this lack of a significant average treatment effect masks considerable heterogeneity, both between seasoned and novice entrepreneurs and *within* each group of households. We find no significant average treatment effects on consumption for either seasoned or novice entrepreneurs alone, as Panel B, column 1 shows. But as demonstrated in Panel A of Figure 3 (displaying the results of bootstrapped quantile regressions for per-capita consumption for seasoned entrepreneurs), more than half of the distribution of per-capita consumption (from around the 30th to the 85th percentile) experienced positive treatment effects on consumption. At the 75th percentile of the distribution, we find a gain of just under Rs. 350 in monthly household consumption per adult equivalent, an increase of 10.4% over the 75th percentile of consumption among seasoned entrepreneurs in the control group (Rs. 3325). However, at no point in the distribution of per-capita consumption for novice entrepreneurs (Figure 3, Panel B) do we find any significant positive treatment effects.

Columns 3, 4, and 5 report results for annual household spending on durable goods, both in total and broken into spending on durables for business use and non-business use. Because of outliers in these distributions, we Winsorize data of reported spending on durables in each category at the 95th percentile of each distribution.²⁴ We find a marginally significant average treatment effect of Rs. 560 in increased total spending on durable goods (Panel A, column 3) and a highly significant, though small, average treatment effect of Rs. 24 in increased spending on durable goods for household businesses (Panel A, column 5). These results, as Panel B reveals, are driven entirely by seasoned entrepreneurs. In the treatment group, seasoned entrepreneurs spent Rs. 1,937 more on durables and Rs. 61 more on business durables in the previous year than seasoned entrepreneurs in the control group, while novices in treatment and control show no differences in either of these outcomes (columns 3 and 5). Moreover, seasoned entrepreneurs show a large and highly significant increase in spending on non-business durables: Rs. 1,540, or 18.9% of the mean for seasoned entrepreneurs in the control group (Panel B, column 4).

²⁴As Online Appendix Table?? shows, our results are qualitatively robust to Winsorizing at higher percentiles of the distribution, taking the log of expenditures, and conducting bootstrapped median regressions.

Consistent with Banerjee et al. (2014)'s results for their second endline survey (in 2010), we find no difference between treatment and control households - whether among seasoned entrepreneurs or novice entrepreneurs - in spending on festivals (column 5). As column 2 shows, we also find no difference in spending on "temptation goods," goods that households in the baseline survey said that they would like to spend less on (alcohol, tobacco, betel leaves, gambling, and food consumed outside the home).²⁵ Additionally, there is no difference between treatment and control households in monthly spending on education (column 7) and health (column 8).

One of the most disappointing features of the first wave of microfinance impact evaluations is the the lack of a positive effect on household consumption. Banerjee et al. (2014) and others do find an initial increase in durable consumption which ostensibly is obtained using the proceeds of the loan. However, they do not observe positive effects in overall consumption or in longer-run household durable consumption. Our results point to some optimism, at least when microfinance is directed toward seasoned entrepreneurs. While in 2012, the seasoned entrepreneurs continued to invest their labor hours and capital in their businesses, we also observe that a sizable subset of the distribution does in fact enjoy a consumption increase, and that the average household is able to purchase more household durables. If the marginal returns to business capital are still high, then we might expect even larger consumption increases in the future. For these seasoned entrepreneurs, a high marginal value to an additional rupee of business investment may explain the absence of a short-run consumption effect.

3.5. Other Sources of Borrowing.

Table 9 reports treatment effects on households borrowing from sources other than microfinance. At the time of our survey, households in the treatment group did not borrow differentially from the control group from either banks (Panel A, columns 3 and 4) or from Self-Help Groups (SHGs)²⁶ or other savings group (Panel A, columns 5 and 6). There is also no significant average treatment effect on borrowing from informal sources, such as a moneylender or a relative or friend, on either the extensive or intensive margins (Panel A, columns 1 and 2).

²⁵The fact that we do not find an effect on temptation good spending may not be surprising in our setting where MFIs are no longer operating. One possible source of the initial Banerjee et al. (2014) temptation goods effect may have been individuals scaling back unnecessary consumption in order to make the weekly MFI loan repayment. When microfinance is no longer present, there is no need to come up with the weekly payment amount.

²⁶SHGs are groups of women who are organized around a shared bank account and joint access to subsidized credit. They save jointly in this bank account, and are given access to credit once sufficient savings have been accumulated. The SHG then decides how to allocate credit among the members and how to enforce repayment. While SHGs in AP were more widespread in rural areas, many households did report participating in an SHG at some point in time. During the time of the AP Ordinance, the state government hoped that former microfinance borrowers would instead use the SHGs, which tend to be attached to state-owned banks.

Here, we also find that these effects look different for seasoned entrepreneurs and novices. As Panel B, column 1 shows, novice entrepreneurs in the treatment group were 4.4% less likely than novice entrepreneurs in the treatment group to have an informal loan. Seasoned entrepreneurs in the treatment group, on the other hand, were no less likely to have an outstanding informal loan than seasoned entrepreneurs in the control group. These same entrepreneurs also had larger outstanding informal loans, as Panel B, column 2 shows: seasoned entrepreneurs in the treatment group had, on average, Rs. 12,400 more in outstanding informal debt relative to seasoned entrepreneurs in the control group. Conversely, there was no significant effect on informal loan size for novice entrepreneurs. Thus, on both the extensive and intensive margins, seasoned entrepreneurs tend to utilize more informal credit under greater exposure to microcredit, while novice entrepreneurs tend, on the extensive margin, to utilize less. These results are consistent with our model, where microcredit crowds out informal borrowing for novices but actually increases the demand for informal credit among seasoned entrepreneurs.

3.6. Social Network Change. We can also measure directly any changes to the household’s social network that resulted from enhanced exposure to microfinance. Results of this exercise are displayed in Table 10. Column 1 presents the impacts on the household’s degree (number of social connections) as elicited in our survey. Columns 2 and 3 report separately on the number of financial and non-financial links.²⁷ In the average population, we find evidence that access to microfinance does crowd out informal relationships, as models such as Ligon et al. (2000) predict. While on average, households have approximately 6 social connections, access to microfinance reduces this number by 0.37 links in the average population, a percent decrease of 6%. We find that the bulk of the effect is driven by financial links, as would be expected in a model of financial crowdout. The coefficient on non-financial links is much smaller in magnitude and is not statistically significant. This result may seem surprising given that at the time of the network elicitation, microfinance had not been available in Hyderabad for two years. To us, this suggests that link maintenance is indeed costly, and that once microfinance is no longer available, it is not free to re-establish connections with old friends. Further, the network measure used in Table 10 is based on links in hypothetical situations (i.e. “Who would you go to if you needed Rs. 100.”). Thus, even if treatment households experienced less of a need for informal credit, the networks questions are meant to capture the links would be available should the need for a social activity such as borrowing or advice arise.²⁸ This may suggest that when microfinance is functional in a community, the loss of links may be even greater.

²⁷Recall that the survey elicited information about relationships on eight different financial and non-financial dimensions. Some individuals were listed in both categories. Therefore overall degree, which is the union of the financial and non-financial links is smaller than the sum of the two categories.

²⁸We also ran a version of Table 10 using actual instances of borrowing or advice etc. The results look quite similar and are available upon request from the authors.

If, as our framework predicts, risk-sharing networks are more valuable to households with existing businesses, we would expect to see differential effects of the introduction of lower-cost financial capital (such as microcredit) on the size of networks for seasoned and novice entrepreneurs. This is in fact what we find, as the novices experience the full loss of network connections as a function of their exposure to microfinance. In each category, the loss in links is almost fully offset (in a statistically significant way) for the seasoned entrepreneurs. It is also interesting that on average, the seasoned entrepreneurs in the control group have no more network connections than the novices. The stark heterogeneous treatment effect also implies that the magnitude of the crowdout for the novices is even higher than in the average population. Novice entrepreneurs experience an overall loss of 0.50 connections, representing a greater than 8% drop.

In Table 11, we investigate whether individuals with more exposure to microfinance are more likely to report social connections who were in the same microfinance group as the borrower. Feigenberg et al. (2010) show that microfinance causes individuals in the same group to socialize more with one another (non-financial connections) and to engage in risk-sharing activities with one another (financial connections). Thus, if these relationships are durable, we may expect to see a compositional effect in the types of friends listed by treatment and control households. We find that in the average population, treatment households are no more likely to list MFI links and do not seem to differentially drop non-MFI links. We do, however, find that seasoned entrepreneurs in the treatment group are more likely to report MFI links. We find a statistically significant differential effect in almost all specifications. We also find an overall treatment effect for the seasoned entrepreneurs on the number of non-financial MFI links. The point estimates on financial MFI links are of the same order of magnitude, but estimated with much more noise. Two years after the withdrawal of microfinance, it does appear that some of the microfinance links are durable, but only for the seasoned entrepreneurs.

3.7. Worries and Happiness. Lastly, we can measure whether access to microfinance in the past caused any differences in happiness and worries. Table 8 shows that treatment households are both more worried and less happy than control households.²⁹ This result cannot be explained by the withdrawal of microfinance from Hyderabad, because all neighborhoods experienced this equally. Further, we find the greatest evidence of negative effects on the novice households. While we cannot statistically distinguish between the effects on seasoned and novice entrepreneurs, we can only detect a significant impact for the novices.

²⁹All of the worries, happiness and financial security indexes are scaled by the standard deviation. Larger outcomes for the index and scale variables indicate less worried and happier households.

There are several clues from our other results that might help to explain this finding. First, treatment households are more likely to own a business. Entrepreneurship is stressful and may cause households to appear more worried. Second, there may be negative consequences of losing access to social connections. If the availability of informal credit decreases, then households may be left more vulnerable to a shock.

4. MODEL AND ESTIMATION

Our reduced form analysis suggests that relaxing credit constraints through microfinance has large, persistent effects for the GE entrepreneurs. Here, we present a simple dynamic model of wealth accumulation, business investment, and borrowing, where GEs and REs have access to different production technologies, as suggested by the reduced form treatment effects. Through counterfactual exercises, we then plan to ask what the increase in profits would be if the REs had access to the same projects as GEs when microfinance becomes available; or if all microfinance credit due to the Spandana intervention was targeted to GEs.

4.1. Model. We assume that households maximize the discounted sum of the utility from consumption

$$(4.1) \quad U(c_t)_{t=0}^{\infty} = \sum_{t=0}^{\infty} u(c_t)$$

subject to a wealth and borrowing constraint, introduced below. We further assume that every household earns non-stochastic labor market earnings of y each period and that in some, randomly selected, periods, households have the opportunity to become an entrepreneur. We denote $e_{it} \in \{0, 1\}$ as the realization of whether individual i can be an entrepreneur in period t . Allowing households to stochastically enter and exit entrepreneurship matches the observation in many studies that households start and close businesses between survey waves. When $e_{it} = 1$, the household has access to a *Low* production technology in this state with profits equal to:

$$\pi_L(K_t) = A_L K_t^\alpha - K_t$$

where K measures the rupee value of total inputs used in production. We assume that capital either depreciates fully (for working capital and labor) or is sold in the capital market every period (for fixed assets).³⁰ We also posit that this technology exhibits decreasing returns to scale with $\alpha < 1$.

When $e_{it} = 0$, the household is unproductive, and they only have access to an imperfect savings technology with return

³⁰Thus the revenue term $A_L K^\alpha$ can be thought of as the sum of the revenues from production plus the sale of all fixed assets.

$$\pi_U(K_t) = \rho K_t$$

We assume that $\rho < 1$, which is consistent with the fact that households often report finding it hard to save for a number of reasons (limited access to formal interest-bearing savings, present bias, demands from friends and relatives etc. .)

We have shown in the reduced form that returns to capital are much higher for seasoned entrepreneurs than novices. We model two types of entrepreneurs, *Gung Ho (GEs)* and *Reluctant (REs)* $\theta \in \{GE, RE\}$. The REs only have access to the *Low* technology when productive, while GEs also have access to a *High* technology when productive. This *High* technology requires a minimum investment \underline{K} , but it comes with improved productivity $A_H > A_L$. We further assume that this technology has constant, rather than decreasing, returns to scale.³¹

$$\pi_H(K_t) = A_H(K_t - \underline{K}) - K_t$$

Each productive ($e_{it} = 1$) period, GE households choose which technology to use, according to which has higher returns given their optimal feasible level of capital. (See Figures 5 through 7, discussed below.)

Finally, households have the ability to borrow from informal lenders in each period, and when applicable, from microfinance. We assume that all project returns are deterministic and all lenders have a claim to project proceeds (including savings), so we abstract away from both distressed and strategic default. In the absence of microfinance, households can choose to borrow B_t^{SN} from informal sources at an interest rate of r_{SN} each period. This “social network” borrowing comes from input suppliers, shop keepers, moneylenders, friends, and relatives. This credit line, however, is not infinite, and all households are restricted to choose $0 \leq B_t^{SN} \leq \bar{b}^{SN}$.

When microfinance is available, households can choose levels of borrowing $0 \leq B_t^{MF} \leq b_t^{MF}(\tau)$ at interest rate r_{MF} . The borrowing cap $b_t^{MF}(\tau)$ depends on both the year (pre- vs. post-microfinance entry) and also the treatment status τ of the household’s neighborhood. While control neighborhoods do have some access to microfinance post-2006, the treatment neighborhoods have greater access. Finally, we posit that $r_{MF} < r_{SN}$, a claim for which we find empirical support in our data.

Regimes. Our model is designed to capture the differential transition dynamics between the treated and control neighborhoods for GEs versus REs. The time frame of the intervention spans seven years (2006-2012) and four different regimes. We index regime by $g \in \{1, 2, 3, 4\}$. It is important to note that each regime change is a surprise to all

³¹The High technology may have a greater span of control, allowing for the use of hired labor and avoiding decreasing returns due to fixed household labor, or may correspond to a business facing a less localized market.

households. During any regime g households believe that this regime will continue to be the status quo forever.

In regime 1 (year 1), neither treatment nor control areas has access to microfinance. Thus borrowers for whom $e_{it} = 1$ must invest in their businesses out of accumulated wealth and informal borrowing only. In regime 2 (years 2 and 3), microfinance enters all areas, but the microfinance credit limits are higher in treated areas due to Spandana's entry. In regime 3 (years 4 and 5), the microfinance borrowing limit in the control areas is raised to equal that of the treatment areas as Spandana enters these areas. Finally in regime 4 (years 6 and 7), microfinance is no longer available and households must revert to the financing sources available to them in regime 1. While individuals in regime 4 may have the same borrowing technologies at their disposal, they are likely to differ in their levels of wealth. We are exactly interested in modeling how microfinance may accelerate a household's wealth accumulation, especially for the GEs.

Timing. The timing of the model is as follows. Households enter each period t with wealth W_t . At the beginning of the households receive their realization of their entrepreneurship shock, e_{it} . Given their wealth and state realization, a productive GE household first decides which project to undertake (*Low* or *High*), how much capital K_t to invest and how much to borrow $B_t = (B_t^{SN}, B_t^{MF})$. A productive RE household decides only how much capital K_t to invest and how much to borrow $B_t = (B_t^{SN}, B_t^{MF})$ in the Low project. Then at the end of the period, profits are realized (and fixed assets liquidated), loans are repaid, and the household chooses how to divide the profits between consumption c_t and future wealth, W_t .

Utility Maximization Problem. We reframe the standard utility maximization problem of Equation 4.1 into the recursive Bellman equation form::

$$V^e(W|e; \theta, \tau, g) = \max_{c, W', K, L, B, D^H} u(c) + \beta E_{e'}(V^{e'}(W')|e; \theta, \tau, g)$$

s.t.

$$\begin{aligned} W' + c &= D^H \pi_H(K) + (1 - D^U - D^H) \pi_L(K) + D^U \pi_U(K) - r_{SN} B^{SN} - r_{MF} B^{MF} \\ 0 &\leq B^{SN} \leq b^{SN} \\ 0 &\leq B^{MF} \leq b^{MF}(\tau, g) \\ W' &\geq 0 \\ K &\leq W + B^{SN} + B^{MF} \\ D^H, D^U &\in \{0, 1\} \\ 1 - D^U - D^H &\in \{0, 1\} \end{aligned}$$

where D^H, D^U are respectively indicators for operating the High project and for being unproductive, respectively. If $\theta = RE$

$$D^H = 0$$

and if $e = 0$,

$$D^U = 1$$

Finally, we assume that the states transition according to a Markov process that we allow to be different by type.

4.2. Estimation. We estimate four parameters of the model $(A_L, A_H, \alpha, \underline{K})$ using Simulated Method of Moments. The other parameters from the model are either calibrated or estimated separately from our survey data. We assume that the utility function takes the standard CRRA form $u(c) = \frac{1}{1-\sigma}c^{1-\sigma}$. We estimate the curvature parameter, σ using an incentivized time and risk preference module following Andreoni and Sprenger (2012) that was conducted as a part of the third endline survey. The discounting parameter, β is calibrated to 0.9.

We calibrate the return on savings ρ to be equal to be equivalent to $1 - Infl$, where $Infl$ is the average consumer price inflation rate in India over the sample period. The social network interest rate r_{SN} and borrowing cap \bar{b}^{SN} are calibrated from the control group EL1 surveys. The microfinance interest rate r_{MF} and borrowing cap $b_i^{MF}(\tau)$ are taken from administrative microfinance data. Finally, we assign type θ based on whether a household ever operates a business which is above the 75th percentile of the business size distribution by assets: if a household even operates a “large” business, they are classified as a GE; otherwise, as an RE.

In order to estimate the parameters, we match empirical moments from the survey data with predicted moments under the model. We consider four outcomes: consumption, business assets, social network borrowing, and microfinance borrowing, separately for GEs and REs and in treatment and control neighborhoods. Altogether this yields 20 moments. We are currently estimating the model. However, early estimates (from a coarse grid search) deliver many of the empirical predictions that are borne out in the reduced form analysis.

Sample Policy Functions. In Figures 5 through 7, we present policy and value functions as a function of household wealth and evaluated at the preliminary parameter estimates. Figure 5 captures the profits of each of the available technologies and the choice of social network borrowing in absence of microfinance. Note that GE entrepreneurs will be able to achieve the upper envelope of the profits of the low and high technologies. Further, under the high technology, the households max out their borrowing from their social networks. However, for the low technology, because of the decreasing returns, social

network borrowing decreases in wealth. Microfinance is unavailable, so all households are constrained to zero MF borrowing.

When microfinance enters, credit constraints are relaxed for both types. Figure 6 plots the resulting policy functions. Note that now households are able to borrow at a lower cost from microfinance. At low levels of wealth both GEs and REs choose to borrow. This enables GEs to invest in the productive technology at lower levels of wealth. Also, note that even after microfinance enters, the GEs are still credit constrained; they choose to borrow to their credit limit from all sources. However, for the REs this is not the case. While low wealth REs borrow from both types of creditors, as wealth increases and the decreasing returns set in, they start to substitute away from social network credit. This is consistent with our “crowding-out” result. It is also the case that eventually, the RE’s demand for microcredit also starts to diminish.

Finally, in Figure 7, we plot the resulting value functions for the GEs under both productive and unproductive states. Note that the steep increase in value stemming from shifting from the low to the high technologies happens at much lower levels of wealth.

4.3. (Planned) Counterfactual Exercise. After finalizing the estimation results, we plan to use them for counterfactual exercises. First, we plan to ask what would the effect on total business profits be if the REs were given the technologies available to the GEs. This is isomorphic to directing credit away from REs and toward “unbanked” GEs. In the second exercise, we plan to ask what would the effect on profits have been if the MFIs had instead directed credit away from the REs and toward the existing GE clients of the MFIs. This second exercise requires more assumptions about the global shape of the production functions.

5. CONCLUSION AND DISCUSSION

We study the longer-run, *persistent* effects of a randomized microfinance impact evaluation. The setting is unique due to the universal withdrawal of microfinance from the entire study area in 2010, two years before we surveyed respondents. We show that access to formal credit through microfinance can have lasting impacts, especially for those individuals who are suited for entrepreneurship. We find that all of the benefits accrue by increasing credit supply on the intensive margin. For those individuals with an existing business before the entry of microfinance, we find economically meaningful effects on all aspects of the household business. Further, these effects are larger than those found in the shorter-run microfinance impact evaluation literature. Indeed, the results are larger than those detected in the same sample of borrowers looking at shorter time horizons (see Banerjee et al. (2014)). We also begin to observe evidence on household consumption impacts for the seasoned entrepreneurs. In contrast, on the extensive margin, microfinance appears to induce low-productivity businesses to enter. We find no evidence of consistently

positive effects from access to credit on the sample of households without a business when microfinance entered.

We are also among the first to use experimental variation to study the interaction between access to formal finance and social network connections. We find evidence for the “crowdout” hypothesis among the novice entrepreneurs. However, we detect no such effect for the seasoned entrepreneurs. This channel of crowdout might explain why impacts of microfinance are minimal among certain subpopulations: while microfinance may reduce borrowing costs, overall demand for credit may change very little. It is essential for policymakers to understand these interactions when designing financial inclusion policies and when targeting financial products to specific subgroups.

Overall, it does appear that there are indeed sizable benefits from microfinance, but it takes time for these benefits to accumulate. And it is important to look for the impacts in the right place.

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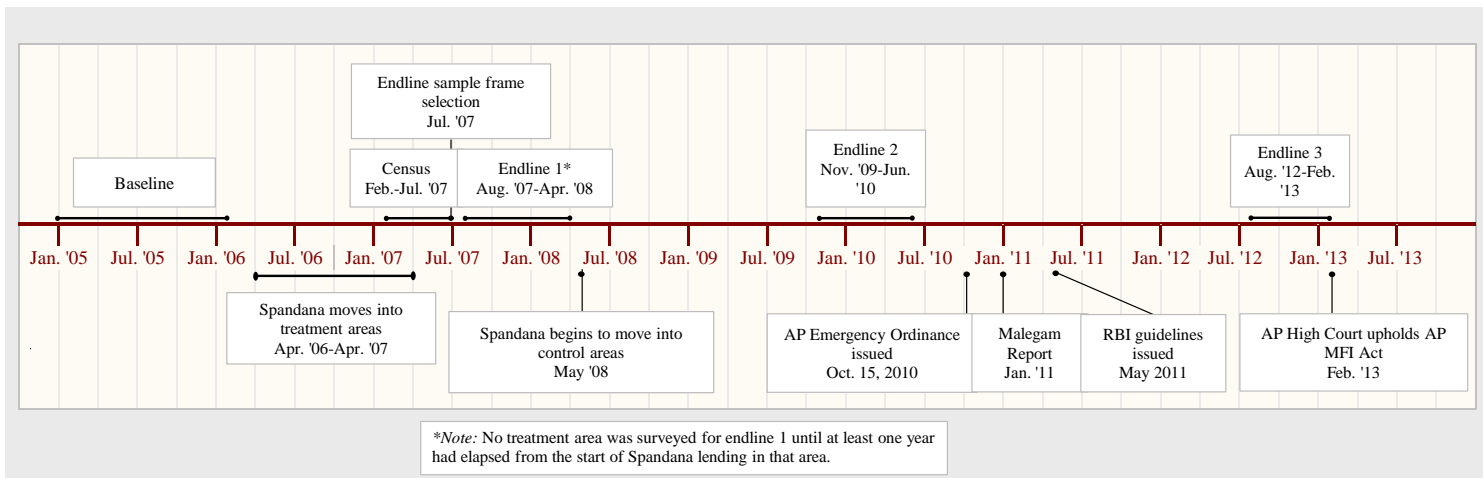
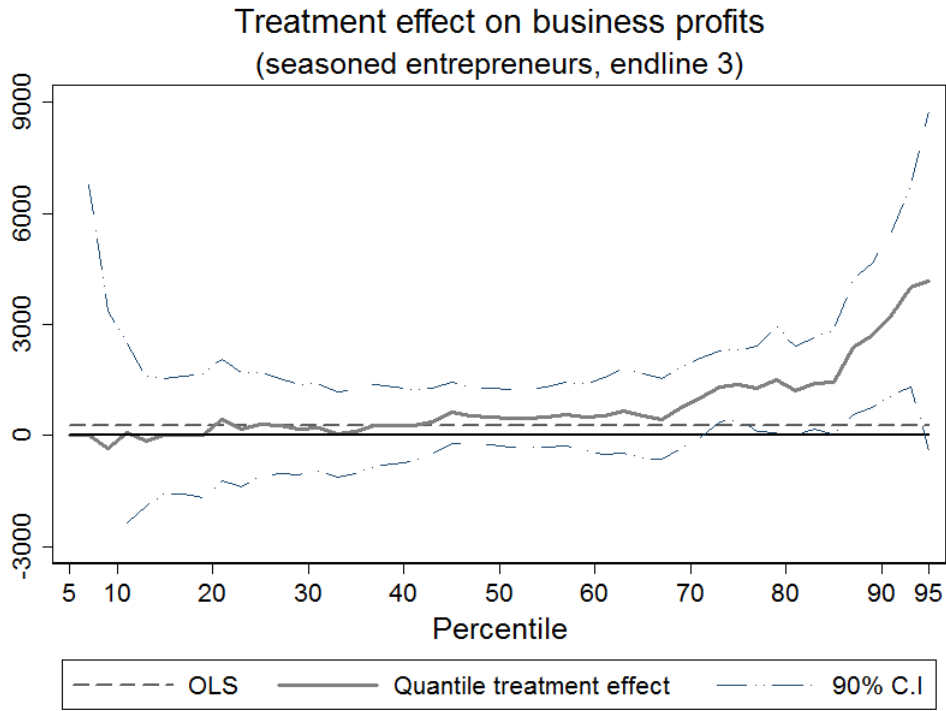


FIGURE 1. Timeline of Survey Activities and Microfinance Crisis

Panel A: Seasoned Entrepreneurs



Panel B: Novice Entrepreneurs

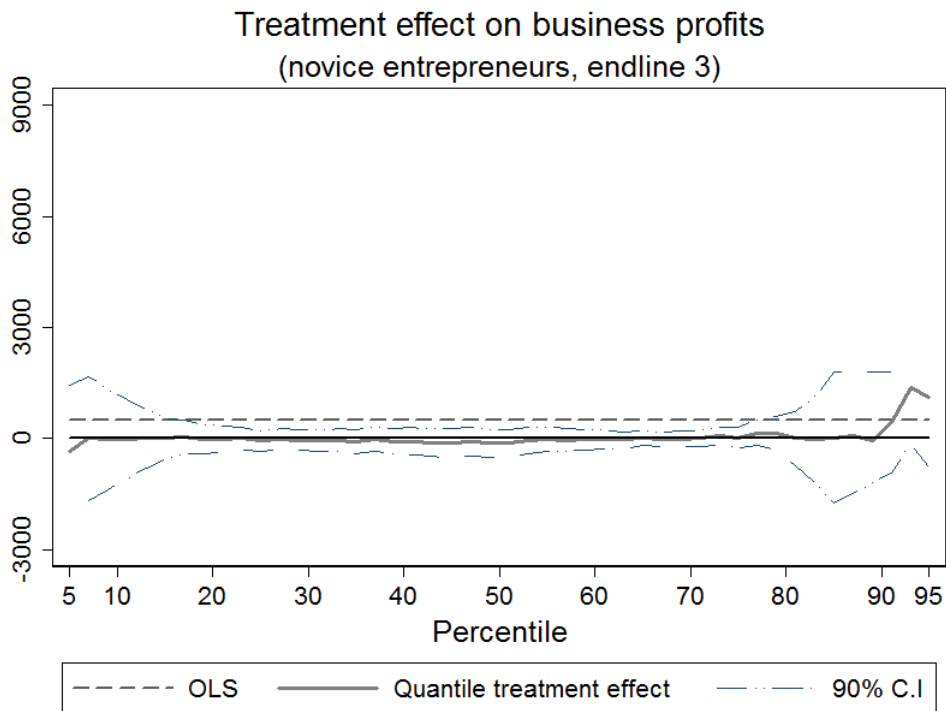
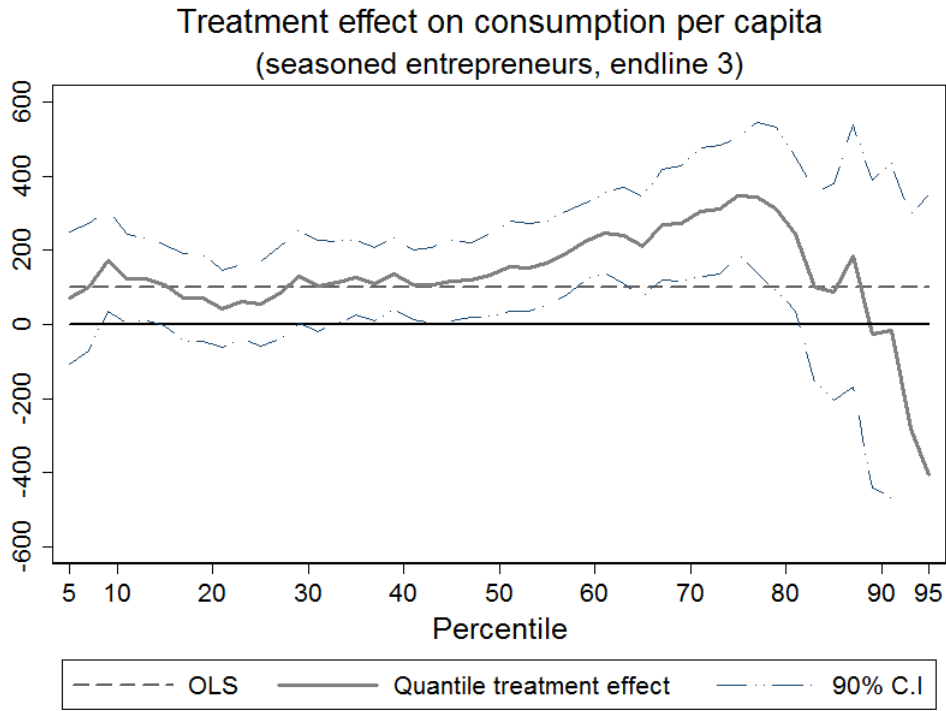


FIGURE 2. Quantile treatment effects for business profits

Panel A: Seasoned Entrepreneurs



Panel B: Novice Entrepreneurs

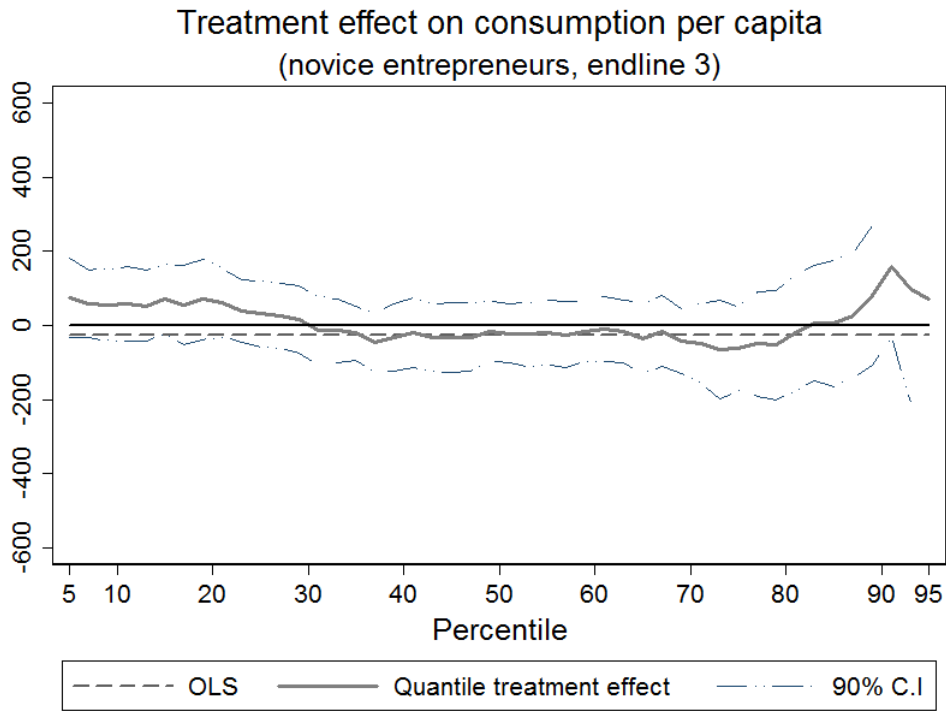


FIGURE 3. Quantile treatment effects for monthly consumption per adult equivalent

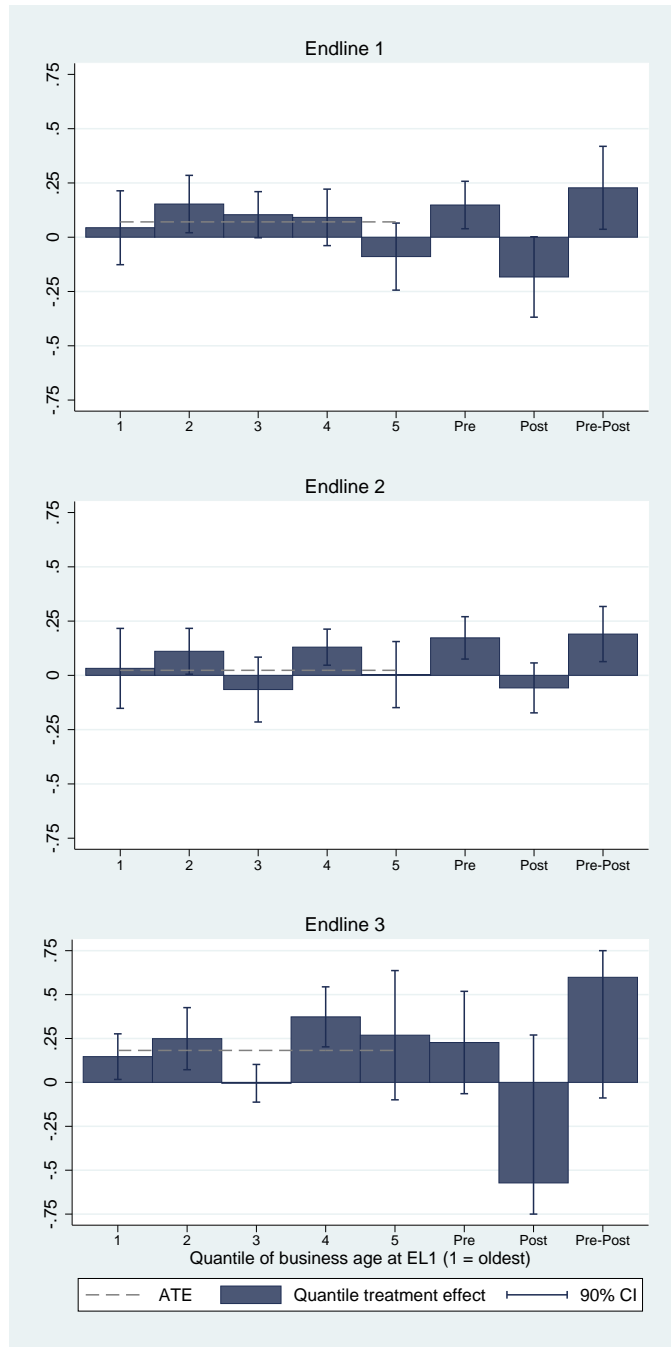


FIGURE 4. Experience vs. Selection

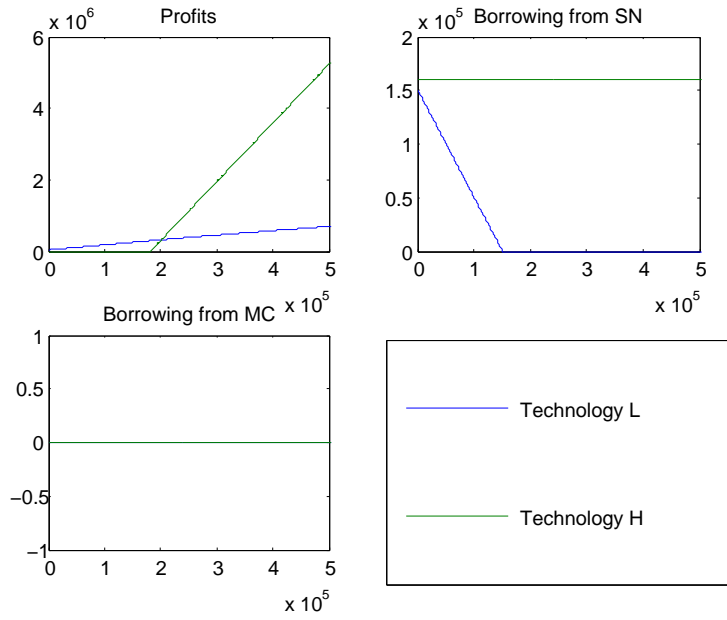


FIGURE 5. Profits and Borrowing: No MF

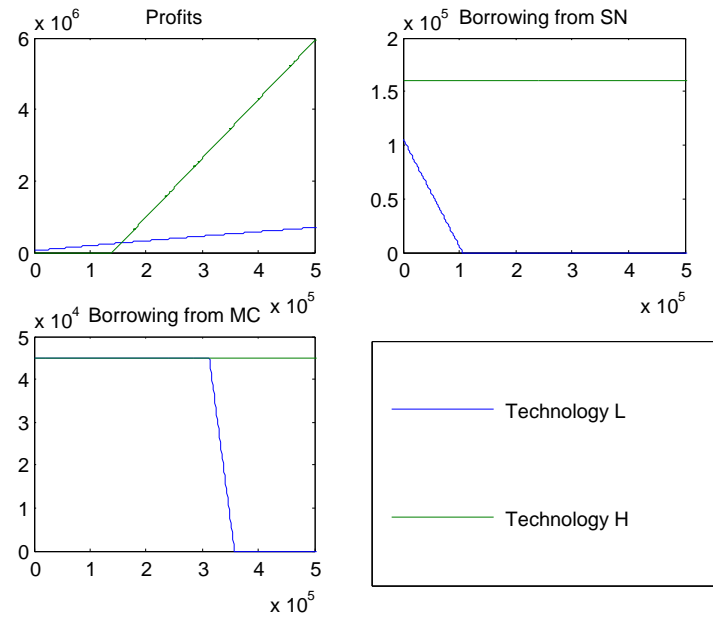
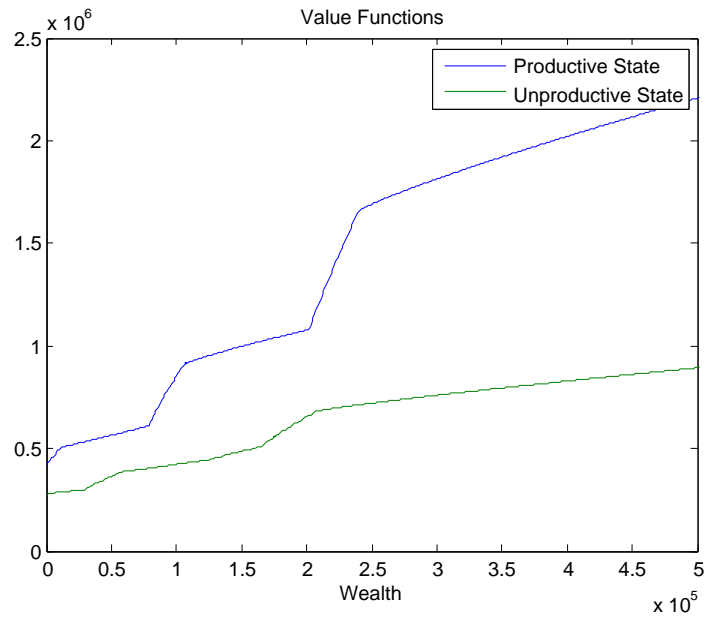


FIGURE 6. Profits and Borrowing: MF

Panel A: No Microfinance



Panel B: with Microfinance

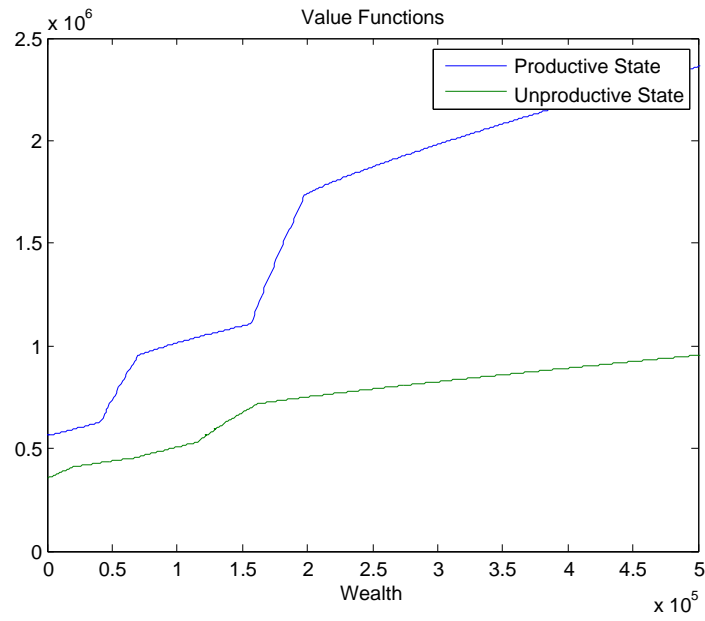


FIGURE 7. Profits and Borrowing: Value Functions GEs

TABLES

TABLE 1. Endline 3 summary household and business statistics (control group)

	Obs	Mean	St. Dev.
<i>Household composition</i>			
# members	2785	6.894	(2.978)
# adults (>=16 years old)	2785	4.221	(1.975)
# children (<16 years old)	2785	1.638	(1.368)
Male head	2784	0.765	(0.424)
Head's age	2784	44.379	(9.99)
Head with no education	2784	0.334	(0.472)
<i>Access to credit (endline 2)</i>			
Loan from Spandana	2946	0.112	(0.316)
Loan from other MFI	2946	0.268	(0.443)
Loan from a bank	2946	0.073	(0.26)
Informal loan	2946	0.603	(0.489)
Loan from Self-Help Group or other savings group	2946	0.092	(0.29)
Any type of loan	2946	0.905	(0.293)
<i>Amount borrowed at endline 2 from (Rs.):</i>			
Spandana	2946	1,898	(6,769)
Other MFI	2946	4,773	(10,731)
Bank	2946	5,951	(39,247)
Informal loan	2946	32,252	(76,606)
Self-Help Group or other savings group	2946	1,003	(5,223)
Total	2946	88,244	(144,194)
<i>Businesses</i>			
Has a business	2785	0.307	(0.461)
# of businesses	2785	0.371	(0.613)
# of businesses managed by women	2785	0.173	(0.417)
Share businesses managed by women	854	0.466	(0.475)
Sales (Rs.)	802	25,240	80,867
Expenses (Rs.)	849	16,300	70,729
Investment (Rs.)	854	3,496	30,499
More than 1 worker in any business	850	0.335	(0.472)
More than 2 workers in any business	850	0.115	(0.32)
# workers in largest business	850	1.660	(1.884)
Total work hours (hrs/week)	854	46.310	(47.898)
<i>Consumption (per household per month)</i>			
Consumption (Rs.)	2781	13,077	9,907
Non-durables consumption (Rs.)	2781	11,960	8,455
Durables consumption (Rs.)	2785	1,115	3,362
Asset index	2785	2.705	(0.831)

TABLE 2. Endline 3 summary social networks statistics (control group)

	Obs	Mean	St. Dev.
<i>All links</i>			
Degree (hypothetical)	2677	5.948	(3.722)
Financial links (hypothetical)	2677	4.372	(2.603)
Non-financial links (hypothetical)	2677	2.926	(2.569)
<i>Supported links</i>			
Degree (supported links only)	2677	2.755	(3.127)
Financial links (supported links only)	2677	2.286	(2.537)
Non-financial links (supported links only)	2677	1.360	(1.944)
Proportion of links that are supported	2677	0.402	(0.357)
<i>Non-supported links</i>			
Degree (non-supported links only)	2677	3.191	(2.699)
Financial links (non-supported links only)	2677	2.084	(1.893)
Non-financial links (non-supported links only)	2677	1.565	(1.751)
<i>Links from microfinance groups (MFI borrowers only)</i>			
Listed any MFI links	1343	0.340	(0.474)
Percent of links from MFI group	1343	0.164	(0.271)
Total MFI links	1343	0.555	(0.909)
Total MFI links (known before MFI group)	1343	0.550	(0.902)
Total MFI links (from financial links)	1343	0.470	(0.780)
Total MFI links (from non-financial links)	1343	0.217	(0.551)

TABLE 3. Lending balance in October 2010 (pre-ordinance)

	(1) MFI loan	(2) Installments	(3) Large windfall
Treatment	-0.009 (0.018)	0.033 (0.022)	0.005 (0.005)
Control mean	0.202 (0.402)	0.386 (0.297)	0.027 (0.161)
N	5745	1095	5745

Notes: Standard errors, clustered at the area level, reported in parentheses. * significant at the 10% level, ** at the 5% level, *** at the 1% level.

TABLE 4. Exposure to microfinance by treatment group

	(1)	(2)	(3)	(4)	
	Borrowed from MFI in last 3 years (endline 1)	Borrowed from MFI in last 3 years (endline 2)	Outstanding MFI loan in endline 2	Borrowed from MFI between 2004 and 2010	
Panel A: Cumulative exposure to microcredit					
Treatment	0.109*** (0.022)	0.032 (0.022)	0.008 (0.020)	0.044* (0.024)	
Control mean	0.256 (0.436)	0.420 (0.494)	0.332 (0.471)	0.498 (0.500)	
N	6804	6128	6143	5467	
Panel B: Cumulative exposure to microcredit by entrepreneurial status					
Seasoned entrepreneur	0.163*** (0.023)	0.112*** (0.022)	0.093*** (0.020)	0.110*** (0.022)	
Treatment	0.109*** (0.021)	0.029 (0.023)	0.003 (0.021)	0.036 (0.026)	
Treatment X Seasoned entrepreneur	-0.002 (0.030)	0.005 (0.030)	0.013 (0.031)	0.020 (0.032)	
Treatment + (Treatment X Seasoned entrepreneur)	0.107	0.034	0.016	0.057	
P(Treatment + (Treatment X Seasoned entrepreneur)!=0)	0.001	0.312	0.617	0.091	
Control mean (novice entrepreneurs only)	0.206 (0.404)	0.385 (0.487)	0.302 (0.459)	0.463 (0.499)	
Control mean (seasoned entrepreneurs only)	0.372 (0.483)	0.503 (0.500)	0.401 (0.490)	0.580 (0.494)	
N	6804	6128	6143	5467	
	(1)	(2)	(3)	(4)	(5)
	Any MFI loan	Number of MFI loans	Total MFI loan amount	Any Spandana loan	Total Spandana loan amount
Panel C: Microcredit exposure as of endline 2					
Treatment	0.008 (0.020)	0.026 (0.038)	946.417** (474.365)	0.061*** (0.014)	1132.643*** (257.510)
Control mean	0.332 (0.471)	0.530 (0.937)	6670.434 (13627.432)	0.112 (0.316)	1897.522 (6768.526)
N	6143	6143	6143	6143	6143
Panel D: Microcredit exposure as of endline 2 by entrepreneurial status					
Seasoned entrepreneur	0.093*** (0.020)	0.173*** (0.049)	2557.957*** (671.712)	0.052*** (0.018)	798.113** (388.901)
Treatment	0.003 (0.021)	0.000 (0.038)	677.234 (508.180)	0.050*** (0.014)	800.099*** (267.354)
Treatment X Seasoned entrepreneur	0.013 (0.031)	0.075 (0.073)	754.962 (929.289)	0.034 (0.024)	1036.985** (504.799)
Treatment + (Treatment X Seasoned entrepreneur)	0.016	0.075	1432.197	0.083	1837.084
P(Treatment + (Treatment X Seasoned entrepreneur)!=0)	0.617	0.299	0.102	0.001	0.000
Control mean (novice entrepreneurs only)	0.302 (0.459)	0.472 (0.878)	5812.723 (12661.459)	0.096 (0.294)	1629.648 (6782.720)
Control mean (seasoned entrepreneurs only)	0.302 (0.459)	0.472 (0.878)	5812.723 (12661.459)	0.096 (0.294)	1629.648 (6782.720)
N	6143	6143	6143	6143	6143

Notes: Standard errors, clustered at the area level, reported in parentheses. * significant at the 10% level, ** at the 5% level, *** at the 1% level.

TABLE 5. Reduced form: businesses (endline 3)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Has business	Number of businesses	Started a business in last 12 months	Closed a business in last 12 months	More than 1 worker in any business	More than 2 workers in any business	Workers in largest business	Total wages paid	Assets (stock)	Log expenses	Log revenue
Panel A: Treatment effects											
Treatment	0.038* (0.020)	0.056* (0.031)	0.006 (0.005)	0.008** (0.004)	0.030** (0.015)	0.016** (0.006)	0.208** (0.087)	373.747*** (133.018)	2012.651*** (515.398)	0.272* (0.140)	0.311* (0.157)
Control mean	0.307 (0.461)	0.371 (0.613)	0.032 (0.176)	0.027 (0.161)	0.102 (0.303)	0.035 (0.184)	0.507 (1.292)	348.367 (4700.427)	6811.262 (22049.316)	2.296 (3.786)	2.640 (4.188)
N	5744	5744	5744	5744	5738	5738	5738	5736	5744	5724	5589
Panel B: Treatment effects by entrepreneurial status											
Seasoned entrepreneur	0.422*** (0.020)	0.525*** (0.026)	0.025*** (0.008)	0.019** (0.008)	0.174*** (0.018)	0.055*** (0.009)	0.765*** (0.071)	488.639* (266.816)	8751.106*** (1044.605)	3.366*** (0.176)	3.900*** (0.181)
Treatment	0.024 (0.018)	0.031 (0.024)	0.009 (0.006)	0.006 (0.004)	0.017 (0.012)	0.009 (0.006)	0.174** (0.076)	275.264** (118.604)	827.149 (615.730)	0.101 (0.115)	0.127 (0.128)
Treatment X Seasoned entrepreneur	0.040 (0.028)	0.076** (0.035)	-0.011 (0.013)	0.006 (0.012)	0.040 (0.024)	0.023* (0.013)	0.102 (0.143)	311.864 (368.366)	3744.398** (1851.031)	0.502** (0.222)	0.588** (0.232)
Treatment + (Treatment X Seasoned entrepreneur)	0.064	0.107	-0.002	0.012	0.057	0.032	0.277	587.127	4571.547	0.603	0.715
P(Treatment + (Treatment X Seasoned entrepreneur)!=0)	0.008	0.008	0.849	0.228	0.025	0.010	0.060	0.093	0.004	0.004	0.001
Control mean (novice entrepreneurs only)	0.177 (0.382)	0.208 (0.483)	0.025 (0.155)	0.021 (0.144)	0.049 (0.215)	0.019 (0.135)	0.279 (0.865)	197.888 (2496.403)	4139.482 (19953.887)	1.259 (2.989)	1.452 (3.320)
Control mean (seasoned entrepreneurs only)	0.604 (0.489)	0.742 (0.712)	0.048 (0.215)	0.039 (0.194)	0.226 (0.419)	0.073 (0.261)	1.031 (1.842)	694.898 (7648.542)	12934.882 (25192.959)	4.687 (4.313)	5.416 (4.667)
N	5744	5744	5744	5744	5738	5738	5738	5736	5744	5724	5589

Notes: Standard errors, clustered at the area level, reported in parentheses. * significant at the 10% level, ** at the 5% level, *** at the 1% level.

TABLE 6. Reduced form: household labor (endline 3)

	(1)	(2)	(3)	(4)
	Total weekly labor hours	Total weekly hours in self- employment	Total weekly hours in wage labor	Total weekly hours in casual labor
Panel A: Treatment effects				
Treatment	2.170 (1.661)	2.752** (1.159)	0.351 (2.037)	-0.937 (1.166)
Control mean	87.490 (56.528)	15.400 (30.304)	56.918 (53.373)	15.120 (30.015)
N	5744	5744	5744	5744
Panel B: Treatment effects by entrepreneurial status				
Seasoned entrepreneur	4.798** (2.107)	23.537*** (1.587)	-14.248*** (2.257)	-4.480*** (1.347)
Treatment	0.150 (2.021)	1.259 (0.859)	-0.067 (2.226)	-1.011 (1.403)
Treatment X Seasoned entrepreneur	6.501* (3.321)	4.569** (1.962)	1.527 (3.279)	0.293 (1.747)
Treatment + (Treatment X Seasoned entrepreneur)	6.651	5.827	1.460	-0.719
P(Treatment + (Treatment X Seasoned entrepreneur)≠0)	0.017	0.004	0.618	0.626
Control mean (novice entrepreneurs only)	86.111 (55.490)	8.175 (22.456)	61.501 (53.817)	16.376 (30.954)
Control mean (seasoned entrepreneurs only)	90.652 (58.751)	31.957 (38.404)	46.415 (50.835)	12.239 (27.546)
N	5744	5744	5744	5744

Notes: Standard errors, clustered at the area level, reported in parentheses. * significant at the 10% level, ** at the 5% level, *** at the 1% level.

TABLE 7. Reduced form: consumption (endline 3)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Per capita consumption (monthly)	Temptation goods (monthly)	All durables	Non-business durables	Business durables	Festivals (annual)	Education (monthly)	Health (monthly)
Panel A: Treatment effects								
Treatment	-11.583 (58.735)	-19.609 (32.860)	559.362* (283.691)	351.696 (239.737)	23.900*** (8.242)	388.891 (304.751)	86.911 (88.338)	-45.121 (99.606)
Control mean	2791.712 (1964.732)	619.036 (971.999)	9264.343 (15748.713)	8482.853 (14264.700)	88.575 (351.011)	8932.731 (10950.345)	1724.557 (3105.393)	1775.509 (4107.905)
N	5738	5739	5744	5744	5744	5737	5738	5738
Panel B: Treatment effects by entrepreneurial status								
Seasoned entrepreneur	106.282 (77.229)	11.260 (29.245)	20.574 (658.347)	-513.234 (563.800)	92.895*** (16.248)	512.767 (551.631)	307.695** (153.525)	91.821 (177.110)
Treatment	-48.970 (61.965)	-21.350 (37.511)	-54.105 (385.449)	-175.322 (323.643)	6.847 (9.196)	351.483 (372.316)	111.138 (108.004)	-77.592 (122.781)
Treatment X Seasoned entrepreneur	120.166 (107.356)	5.530 (48.037)	1991.273** (850.072)	1716.980** (725.416)	54.254** (26.063)	114.824 (663.376)	-82.426 (211.525)	104.366 (239.184)
Treatment + (Treatment X Seasoned entrepreneur)	71.196	-15.821	1937.168	1541.658	61.101	466.308	28.712	26.775
P(Treatment + (Treatment X Seasoned entrepreneur)!=0)	0.491	0.721	0.004	0.007	0.007	0.394	0.869	0.891
Control mean (novice entrepreneurs only)	2759.209 (1886.292)	614.207 (986.241)	9266.641 (15716.467)	8635.084 (14350.628)	60.866 (293.414)	8781.591 (9151.870)	1631.834 (2663.565)	1736.854 (4262.700)
Control mean (seasoned entrepreneurs only)	2866.309 (2133.184)	630.105 (939.016)	9259.076 (15831.694)	8133.946 (14067.978)	152.080 (450.203)	9280.014 (14248.019)	1937.360 (3929.819)	1864.224 (3729.326)
N	5738	5739	5744	5744	5744	5737	5738	5738

Notes: Standard errors, clustered at the area level, reported in parentheses. * significant at the 10% level, ** at the 5% level, *** at the 1% level. Durables levels are Winsorized at the 5% level.

TABLE 8. Reduced form: worries and happiness (endline 3)

	(1)	(2)	(3)	(4)	(5)
	Overall worries index	Financial worries index	Happiness scale	Financial security scale	Beaten in last month
Panel A: Treatment effects					
Treatment	-0.052* (0.030)	-0.037 (0.031)	-0.082*** (0.027)	0.020 (0.053)	-0.002 (0.004)
Control mean	-0.000 (0.588)	-0.000 (0.658)	0.000 (1.000)	3.763 (1.270)	0.043 (0.204)
N	5717	5717	5716	5721	5702
Panel B: Treatment effects by entrepreneurial status					
Seasoned entrepreneur	0.039 (0.025)	0.045* (0.024)	0.015 (0.034)	0.076 (0.060)	-0.016* (0.009)
Treatment	-0.061* (0.032)	-0.049 (0.033)	-0.088*** (0.030)	0.009 (0.055)	-0.000 (0.006)
Treatment X Seasoned entrepreneur	0.029 (0.036)	0.041 (0.035)	0.018 (0.055)	0.033 (0.077)	-0.005 (0.011)
Treatment + (Treatment X Seasoned entrepreneur)	-0.033	-0.008	-0.069	0.042	-0.006
P(Treatment + (Treatment X Seasoned entrepreneur)≠0)	0.416	0.830	0.160	0.603	0.478
Control mean (novice entrepreneurs only)	-0.014 (0.587)	-0.016 (0.654)	-0.005 (1.010)	3.745 (1.285)	0.049 (0.217)
Control mean (seasoned entrepreneurs only)	0.033 (0.591)	0.037 (0.665)	0.012 (0.977)	3.805 (1.235)	0.030 (0.170)
N	5717	5717	5716	5721	5702

Notes: For indices and scales, lower numbers indicate worse outcomes (i.e. more unhappy, more worried).

Standard errors, clustered at the area level, reported in parentheses. * significant at the 10% level, ** at the 5% level, *** at the 1% level.

TABLE 9. Reduced form: borrowing (endline 3)

	(1)	(2)	(3)	(4)	(5)	(6)
	Has informal loan	Informal loan amount	Has bank loan	Bank loan amount	Has loan from SHG or savings group	SHG or savings group loan amount
Panel A: Treatment effects						
Treatment	-0.020 (0.015)	2668.157 (3545.218)	-0.008 (0.006)	-2994.125 (2322.857)	-0.003 (0.014)	-259.847 (434.327)
Control mean	0.627 (0.484)	57151.686 (113288.950)	0.056 (0.231)	11021.185 (89825.165)	0.032 (0.176)	787.648 (5497.976)
N	5744	5744	5744	5744	5744	5744
Panel B: Treatment effects by entrepreneurial status						
Old business						
Treatment	-0.030 (0.020)	3647.067 (5833.084)	0.014 (0.010)	3773.799 (3814.638)	-0.002 (0.005)	93.033 (200.672)
Treatment X Seasoned entrepreneur	-0.044** (0.018)	-1683.957 (4226.917)	-0.006 (0.007)	-1954.600 (2893.642)	-0.003 (0.015)	-285.879 (397.298)
	0.078*** (0.030)	14085.007* (7387.176)	-0.009 (0.014)	-3419.550 (4740.845)	0.002 (0.008)	83.402 (262.552)
Treatment + (Treatment X Seasoned entrepreneur)						
	0.035 (0.146)	12401.050 (0.046)	-0.014 (0.225)	-5374.151 (0.159)	-0.001 (0.952)	-202.478 (0.716)
Control mean (novice entrepreneurs only)						
	0.635 (0.482)	55097.667 (115425.793)	0.052 (0.221)	9923.156 (89924.523)	0.034 (0.181)	789.324 (5074.296)
Control mean (seasoned entrepreneurs only)						
	0.610 (0.488)	61859.421 (108151.800)	0.067 (0.251)	13537.825 (89599.334)	0.027 (0.163)	783.806 (6366.840)
N	5744	5744	5744	5744	5744	5744

Notes: Standard errors, clustered at the area level, reported in parentheses. * significant at the 10% level, ** at the 5% level, *** at the 1% level.

TABLE 10. Reduced form: social network change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Degree (hypothetical)	Financial links (hypothetical)	Non-financial links (hypothetical)	Degree (supported links only)	Financial links (supported links only)	Non-financial links (supported links only)	Proportion of links that are supported	Degree (non-supported links only)	Financial links (non-supported links only)	Non-financial links (non-supported links only)
Panel A: Treatment effects										
Treatment	-0.369*** (0.134)	-0.307*** (0.090)	-0.125 (0.091)	-0.323** (0.125)	-0.299*** (0.101)	-0.125* (0.066)	-0.037*** (0.014)	-0.045 (0.093)	-0.007 (0.067)	-0.001 (0.059)
Control mean	5.948 (3.722)	4.372 (2.603)	2.926 (2.569)	2.755 (3.127)	2.286 (2.537)	1.360 (1.944)	0.402 (0.357)	3.191 (2.699)	2.084 (1.893)	1.565 (1.751)
N	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492
Panel B: Treatment effects by entrepreneurial status										
Seasoned entrepreneur	0.195 (0.163)	0.145 (0.106)	-0.085 (0.110)	0.098 (0.128)	0.081 (0.100)	-0.083 (0.081)	-0.019 (0.014)	0.099 (0.111)	0.065 (0.085)	-0.002 (0.065)
Treatment	-0.492*** (0.136)	-0.382*** (0.094)	-0.226** (0.094)	-0.432*** (0.125)	-0.383*** (0.101)	-0.211*** (0.071)	-0.053*** (0.015)	-0.056 (0.103)	0.004 (0.076)	-0.014 (0.067)
Treatment X Seasoned entrepreneur	0.394* (0.228)	0.238 (0.144)	0.328** (0.165)	0.351* (0.205)	0.269 (0.164)	0.281** (0.130)	0.052** (0.024)	0.036 (0.172)	-0.035 (0.133)	0.043 (0.101)
Treatment + (Treatment X Seasoned entrepreneur)	-0.098	-0.144	0.101	-0.081	-0.114	0.070	-0.001	-0.020	-0.031	0.029
P(Treatment + (Treatment X Seasoned entrepreneur))=0	0.671	0.316	0.531	0.702	0.500	0.567	0.966	0.898	0.794	0.752
Control mean (novice entrepreneurs only)	5.903 (3.655)	4.328 (2.522)	2.971 (2.587)	2.727 (3.030)	2.256 (2.453)	1.392 (1.948)	0.407 (0.357)	3.174 (2.741)	2.070 (1.902)	1.578 (1.778)
Control mean (seasoned entrepreneurs only)	6.051 (3.872)	4.473 (2.778)	2.821 (2.526)	2.820 (3.342)	2.354 (2.722)	1.286 (1.936)	0.391 (0.356)	3.229 (2.599)	2.118 (1.873)	1.535 (1.687)
N	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492

Notes: Standard errors, clustered at the area level, reported in parentheses. * significant at the 10% level, ** at the 5% level, *** at the 1% level.

TABLE 11. Reduced form: microfinance groups and link formation

	(1)	(2)	(3)	(4)	(5)	(6)
	Listed any MFI links	Percent of links from MFI group	Total MFI links	Total MFI links (known before MFI group)	Total MFI links (from financial links)	Total MFI links (from non-financial links)
Panel A: Treatment effects						
Treatment	0.002 (0.018)	-0.002 (0.010)	-0.007 (0.034)	-0.008 (0.033)	-0.010 (0.029)	0.005 (0.015)
Control mean	0.188 (0.391)	0.091 (0.218)	0.308 (0.731)	0.305 (0.726)	0.261 (0.626)	0.118 (0.419)
N	5185	5185	5185	5185	5185	5185
Panel B: Treatment effects by entrepreneurial status						
Seasoned entrepreneur	0.021 (0.016)	0.004 (0.009)	0.021 (0.031)	0.020 (0.030)	0.008 (0.027)	0.011 (0.017)
Treatment	-0.007 (0.019)	-0.008 (0.011)	-0.033 (0.037)	-0.036 (0.037)	-0.030 (0.031)	-0.012 (0.018)
Treatment X Seasoned entrepreneur	0.028 (0.024)	0.017 (0.013)	0.084* (0.047)	0.090* (0.047)	0.064* (0.038)	0.055** (0.028)
Treatment + (Treatment X Seasoned entrepreneur)	0.021	0.010	0.051	0.054	0.034	0.043
P(Treatment + (Treatment X Seasoned entrepreneur)!=0)	0.403	0.472	0.277	0.246	0.400	0.081
Control mean (novice entrepreneurs only)	0.181 (0.385)	0.090 (0.219)	0.302 (0.732)	0.299 (0.727)	0.258 (0.632)	0.115 (0.412)
Control mean (seasoned entrepreneurs only)	0.206 (0.404)	0.095 (0.215)	0.323 (0.730)	0.319 (0.725)	0.269 (0.613)	0.124 (0.436)
N	5185	5185	5185	5185	5185	5185

Notes: Standard errors, clustered at the area level, reported in parentheses. * significant at the 10% level, ** at the 5% level, *** at the 1% level.

APPENDIX A. SUPPLEMENTAL APPENDIX

A.1. Details on Social Network Data.

A.1.1. *Social Network Variables.* The social networks module of our survey contained three main sections:

- The first, using the approach of Zheng, Salganik, and Gelman (2006), asked respondents how many people they knew with particular characteristics (e.g. named Aruna, having more than five children, working outside of India).
- The second asked respondents to name people with whom they would engage in a series of eight activities. Four of these activities (“financial activities”) were cases of explicit risk-sharing (e.g. borrowing kerosene or small amounts of cash in case the respondent ran out of either), while the others (“non-financial activities”) were not (e.g. getting health advice or watching television).
- The third asked respondents for more detailed information on a random subsample of the individuals whom they named in the second section. This information included demographic and occupational details for the individual and whether the respondent knew the individual from an MFI group, Self-Help Group, and/or ROSCA. The subsample consisted of up to two “non-financial” links and up to three “financial” links.

A.1.2. *Matching link information.* To match the individuals mentioned by respondents (“links”) in the second and third sections of the module, we used a two-step process. First, we matched links across the eight activities dealt with in the second section. Next, we matched the names of the subsample in the third section with the list of all links generated by the first step. All matching was performed in Stata using Michael Blasnik’s user-written `-relink-` command, which conducts fuzzy matches between datasets when identifiers (in our case, links’ names) may not exactly match. `-ReLink-` weights the output of a bigram string comparator based on user-provided match parameters to generate a “matching score” for each possible match.

We used the following process to match links across the eight activities in the second section of the module:

1. To improve precision, links’ names, as recorded by survey enumerators, were transformed as follows:
 - a. All non-alphanumeric characters were removed (e.g. “LAXMI(B)” to “LAXMI B”);
 - b. Components of the name were rearranged in increasing order of word length, with ties broken by alphabetical order (e.g. “LAXMI B” to “B LAXMI”); and
 - c. Since enumerators also occasionally used numerals to distinguish between unique links with the same name (e.g. “LAXMI” and “LAXMI 2”), a “1” was added to all names lacking a numeral (e.g. “B LAXMI” to “B LAXMI 1”).

2. We conducted a fuzzy string match on links' names by respondent using `-relink-`, setting a minimum matching score of 0.985 for possible matches.³²

3. Possible matches between names were disqualified if any of the following conditions held:

a. One name contained a numeral that differed from the numeral in the other name (e.g. "B LAXMI 1" and "B LAXMI 2"); or

b. Any characters before the first word in the name with more than one letter (usually an abbreviated last name) did not match (e.g. "B LAXMI 1" and "C LAXMI 1" or "B LAXMI 1" and "LAXMI 1").

If a respondent listed the same name more than once for any activity, all but one of these observations were dropped, the observation was tagged, and the match process was followed as above. In all, this match process yielded a dataset of 31,864 unique links, with 53 names duplicated in any section.

Next, we matched the subsample of links included in the third section of the module (n=16,513) on this dataset. As before, if a respondent listed the same name more than once in the subsample, all but one of these observations were dropped and the observation was tagged; this yielded 16,492 names in the subsample, of which 21 had duplicates in the original dataset. We repeated the matching process described above, with the one exception that we used a lower cutoff for `-relink-` (0.915 rather than 0.985). (Since we knew that all names in the third section referred to unique individuals, we were more confident that lowering the cutoff would not lead to spurious matches.) Of the 16,492 unique links in the subsample, all but 14 matched successfully.

Because duplicated names in any section prevented us from uniquely matching a given respondent's links across all section, we dropped all links that had been tagged as duplicates in either round of matching. This yielded a final dataset of 31,805 unique links, of which 16,433 were matched with the third module.

³²Appendix Table 14 shows selected treatment effects for social networks using different minimum matching scores. As the table indicates, our results for treatment effects on social networks are robust to even fairly large changes in the cutoff used for matching.

A.2. Supplemental tables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Has MFI loan	MFI loan amount	Number of businesses	Workers in largest business	Total wages paid	Assets (stock)	Profit	Index of business variables
Endline 1 Outcomes								
<i>Panel A: Entered entrepreneurship post-2006, pre-Spandana</i>								
Treatment	0.019 (0.086)	3014.086 (2147.643)	0.101 (0.140)	0.212* (0.123)	227.928 (187.924)	900.734 (829.002)	2801.011** (1293.561)	0.148** (0.066)
Control mean	0.261 (0.439)	3629.300 (7461.592)	1.291 (0.721)	0.415 (1.751)	370.198 (3036.404)	2571.425 (4803.255)	2831.201 (14427.058)	0.002 (0.551)
N	133	133	133	133	133	119	128	133
<i>Panel B: Entered entrepreneurship post-2006, post-Spandana</i>								
Treatment	0.256** (0.117)	6014.120** (2501.411)	0.220** (0.107)	-0.265 (0.264)	-745.559 (723.061)	-1500.608 (1159.788)	-1400.672 (1286.628)	-0.183 (0.112)
Control mean	0.212 (0.412)	2969.697 (7548.754)	1.152 (0.504)	0.227 (1.107)	382.197 (2713.739)	2539.005 (4850.283)	1785.719 (6797.191)	-0.021 (0.490)
N	164	164	164	164	164	145	154	164
Endline 2 Outcomes								
<i>Panel A: Entered entrepreneurship post-2006, pre-Spandana</i>								
Treatment	0.050 (0.097)	3708.400 (3222.988)	0.105 (0.172)	0.242** (0.111)	692.515* (367.195)	2062.942 (1280.788)	4581.481 (3264.478)	0.173*** (0.059)
Control mean	0.378 (0.485)	6829.748 (12057.904)	1.134 (0.781)	0.434 (2.368)	1209.152 (7502.588)	2066.077 (9955.050)	1499.242 (22571.471)	0.002 (0.635)
N	119	119	119	119	119	119	117	119
<i>Panel B: Entered entrepreneurship post-2006, post-Spandana</i>								
Treatment	0.089 (0.082)	5401.525** (2453.763)	-0.297** (0.130)	-0.122 (0.133)	14.411 (397.486)	-465.098 (1533.396)	-2752.992 (1773.692)	-0.058 (0.070)
Control mean	0.411 (0.496)	6678.706 (10182.671)	0.982 (0.751)	0.196 (0.773)	441.071 (1730.977)	1312.500 (4291.220)	3942.742 (7404.038)	-0.022 (0.247)
N	145	145	145	145	145	145	145	145
Endline 3 Outcomes								
<i>Panel A: Entered entrepreneurship post-2006, pre-Spandana</i>								
Treatment			0.142 (0.173)	0.084 (0.222)	1457.127 (1200.091)	-1152.114 (5642.594)	6865.294** (3171.020)	0.227 (0.176)
Control mean			0.755 (0.700)	1.059 (1.457)	453.753 (4675.330)	13491.252 (26429.711)	4784.347 (9723.028)	-0.019 (0.557)
N			118	118	118	118	113	118
<i>Panel B: Entered entrepreneurship post-2006, post-Spandana</i>								
Treatment			-0.087 (0.171)	-1.539 (1.497)	-5355.478 (5021.004)	-2220.433 (5039.518)	-11157.855 (10160.503)	-0.572 (0.508)
Control mean			0.735 (0.785)	1.469 (5.112)	2520.408 (17136.085)	18816.531 (35892.715)	7805.367 (35351.198)	0.226 (1.813)
N			133	133	133	133	126	133

Notes: Business outcome index includes wages, assets, and profits.

TABLE 12. Experience vs. Selection Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Degree	Has informal loan	Informal loan amount	Has business	Number of businesses	Workers in largest business	Total wages paid	Assets (stock)	Log expenses	Log revenue	Profit	Index of business variables
Panel A: All Households												
Treatment effect estimate	-0.336*** (0.117)	-0.018 (0.013)	2311.846 (3055.914)	0.033* (0.018)	0.049* (0.027)	0.186** (0.078)	359.825*** (96.311)	1506.489*** (403.687)	0.235* (0.124)	0.271* (0.139)	231.501 (252.319)	0.076*** (0.029)
Lower Lee bound	-0.531 (0.114)	-0.044 (0.015)	-905.134 (3112.170)	0.009 (0.013)	0.026 (0.018)	0.132 (0.057)	290.887 (128.990)	948.636 (607.638)	0.109 (0.108)	0.121 (0.121)	-73.082 (310.501)	0.052 (0.022)
Upper Lee bound	0.105 (0.133)	0.001 (0.014)	18323.169 (3961.024)	0.061 (0.015)	0.110 (0.024)	0.339 (0.056)	683.682 (104.066)	4443.053 (706.111)	0.552 (0.133)	0.611 (0.145)	1582.681 (264.612)	0.151 (0.026)
N	5487	5745	5745	5745	5745	5739	5737	5745	5725	5591	5582	5745
Panel B: Seasoned Entrepreneurs Only												
Treatment effect estimate	-0.078 (0.214)	0.035 (0.023)	11532.067** (5450.123)	0.054** (0.026)	0.092** (0.039)	0.246 (0.152)	602.025** (288.464)	3067.072** (1258.001)	0.522** (0.216)	0.625*** (0.233)	508.709 (752.973)	0.112*** (0.026)
Lower Lee bound	-0.209 (0.213)	0.018 (0.026)	8876.946 (5901.261)	0.032 (0.026)	0.067 (0.038)	0.208 (0.120)	552.234 (306.917)	2576.493 (1334.861)	0.359 (0.233)	0.413 (0.256)	168.785 (880.219)	0.088 (0.038)
Upper Lee bound	0.233 (0.267)	0.047 (0.025)	22264.317 (8605.991)	0.066 (0.025)	0.142 (0.042)	0.415 (0.119)	1126.038 (244.743)	5754.794 (1534.685)	0.697 (0.241)	0.821 (0.256)	2709.655 (760.427)	0.162 (0.040)
N	1702	1781	1781	1781	1781	1778	1776	1781	1771	1704	1700	1781
Panel C: Novice Entrepreneurs Only												
Treatment effect estimate	-0.455*** (0.122)	-0.041** (0.017)	-1892.724 (3838.662)	0.019 (0.018)	0.024 (0.025)	0.151** (0.074)	245.371** (108.757)	708.063 (537.376)	0.065 (0.117)	0.088 (0.129)	74.153 (256.525)	0.059** (0.028)
Lower Lee bound	-0.675 (0.135)	-0.071 (0.018)	-5261.031 (3670.208)	-0.001 (0.013)	0.004 (0.017)	0.150 (0.061)	173.489 (126.172)	164.488 (621.235)	-0.028 (0.102)	-0.028 (0.114)	-193.931 (211.234)	0.035 (0.029)
Upper Lee bound	0.029 (0.153)	-0.023 (0.017)	16166.697 (4563.777)	0.056 (0.017)	0.089 (0.019)	0.281 (0.061)	450.740 (111.715)	3510.967 (636.526)	0.439 (0.144)	0.488 (0.157)	896.519 (214.770)	0.159 (0.035)
N	3785	3964	3964	3964	3964	3961	3961	3964	3954	3888	3883	3964

Note: Wages, assets, revenue, and expenses are all Winsorized at the 99th percentile of positive values. Profits are calculated from Winsorized revenues and expenses. Regressions include stratum fixed effects.

TABLE 13. Main Results with Lee Bounds

	Cutoff = 0.370			Cutoff = 0.375			Cutoff = 0.380			Cutoff = 0.385			Cutoff = 0.390			Cutoff = 0.395			Cutoff = 1.000		
	Degree	Financial links	Proportion of links supported	Degree	Financial links	Proportion of links supported	Degree	Financial links	Proportion of links supported	Degree	Financial links	Proportion of links supported	Degree	Financial links	Proportion of links supported	Degree	Financial links	Proportion of links supported	Degree	Financial links	Proportion of links supported
Panel A: Treatment effects																					
Treatment	-0.373*** (3.674)	-0.303*** (2.589)	-0.135 (2.563)	-0.377*** (3.698)	-0.309*** (2.595)	-0.133 (2.567)	-0.374*** (3.719)	-0.309*** (2.603)	-0.129 (2.571)	-0.369*** (3.722)	-0.307*** (2.603)	-0.125 (2.569)	-0.380*** (3.756)	-0.316*** (2.618)	-0.130 (2.579)	-0.387*** (3.788)	-0.324*** (2.639)	-0.127 (2.588)	-0.387*** (3.819)	-0.322*** (2.657)	-0.126 (2.597)
Control mean	5.917 (3.674)	4.361 (2.589)	2.890 (2.563)	5.934 (3.698)	4.370 (2.595)	2.932 (2.567)	5.950 (3.719)	4.378 (2.603)	2.934 (2.571)	5.948 (3.722)	4.372 (2.603)	2.926 (2.569)	5.991 (3.756)	4.400 (2.618)	2.941 (2.579)	6.039 (3.788)	4.427 (2.639)	2.947 (2.588)	6.071 (3.819)	4.445 (2.657)	2.952 (2.597)
N	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492
Panel B: Treatment effects by entrepreneurial status																					
Old business	0.185 (0.160)	0.149 (0.104)	-0.084 (0.109)	0.194 (0.160)	0.155 (0.104)	-0.096 (0.109)	0.191 (0.162)	0.150 (0.105)	-0.087 (0.110)	0.195 (0.163)	0.145 (0.106)	-0.085 (0.110)	0.189 (0.163)	0.145 (0.106)	-0.083 (0.110)	0.165 (0.164)	0.135 (0.166)	-0.085 (0.111)	0.159 (0.164)	0.135 (0.164)	-0.087 (0.111)
Treatment	-0.497*** (1.135)	-0.377*** (0.993)	-0.238** (0.994)	-0.495*** (1.136)	-0.380*** (0.993)	-0.235** (0.994)	-0.497*** (1.137)	-0.381*** (0.994)	-0.232** (0.995)	-0.492*** (1.136)	-0.382*** (0.994)	-0.228** (0.994)	-0.502*** (1.138)	-0.389*** (0.994)	-0.230** (0.995)	-0.517*** (1.137)	-0.399*** (0.993)	-0.230** (0.995)	-0.520*** (1.137)	-0.400*** (0.994)	-0.229** (0.995)
Treatment X Seasoned entrepreneur	0.398* (0.223)	0.238* (0.143)	0.332** (0.163)	0.379** (0.224)	0.227 (0.143)	0.330** (0.163)	0.396* (0.227)	0.231 (0.144)	0.334** (0.165)	0.394* (0.228)	0.238 (0.144)	0.328** (0.165)	0.391* (0.229)	0.231 (0.144)	0.327** (0.165)	0.416* (0.231)	0.240 (0.146)	0.333** (0.166)	0.439* (0.230)	0.249 (0.146)	0.335** (0.166)
Treatment+ (Treatment X Seasoned entrepreneur)	-0.099 (0.660)	-0.139 (0.330)	0.094 (0.556)	-0.117 (0.605)	-0.153 (0.285)	0.095 (0.554)	-0.101 (0.658)	-0.150 (0.296)	0.102 (0.529)	-0.098 (0.671)	-0.144 (0.316)	0.101 (0.531)	-0.091 (0.631)	-0.158 (0.274)	0.096 (0.554)	-0.101 (0.665)	-0.159 (0.273)	0.103 (0.526)	-0.092 (0.692)	-0.152 (0.297)	0.106 (0.517)
P(Treatment+ (Treatment X Seasoned entrepreneur)=0)	0.660	0.330	0.556	0.957	0.605	0.554	0.980	0.658	0.529	0.941	0.671	0.531	0.966	0.631	0.554	0.936	0.665	0.526	0.923	0.297	0.517
Control mean (novice entrepreneurs only)	5.875 (3.597)	4.316 (2.502)	2.875 (2.577)	5.890 (3.625)	4.323 (2.506)	2.977 (2.583)	5.907 (3.647)	4.333 (2.517)	2.980 (2.587)	5.903 (3.655)	4.328 (2.522)	2.971 (2.587)	5.949 (3.686)	4.356 (2.532)	2.986 (2.592)	6.004 (3.722)	4.386 (2.550)	2.992 (2.601)	6.038 (3.753)	4.405 (2.566)	2.998 (2.610)
Control mean (seasoned entrepreneurs only)	6.012 (3.846)	4.465 (2.777)	2.826 (2.530)	6.036 (3.861)	4.478 (2.786)	2.826 (2.530)	6.049 (3.879)	4.482 (2.790)	2.827 (2.534)	6.051 (3.872)	4.473 (2.793)	2.821 (2.526)	6.089 (3.911)	4.501 (2.805)	2.837 (2.547)	6.121 (3.937)	4.522 (2.832)	2.842 (2.557)	6.148 (3.966)	4.539 (2.857)	2.846 (2.564)
N	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492

TABLE 14. Social Networks: Alternative Cutoffs for Matching