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

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Abstract. Can improved access to credit jump-start microenterprise growth? 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 "gung-ho entrepreneurs" (GEs)–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 Notably, these effects persist two years after microfinance was withdrawn from Hyderabad.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 "reluctant entrepreneurs" (REs), those without prior businesses, are much more meager and generally indistinguishable from zero. A model of technology choice in which REs can only access a diminishing-returns technology, while GEs can also access a technology with high fixed costs but high returns, can generate dynamics matching those observed in the data. 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 access to formal credit.

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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). Alternatively, while running a small business may augment household income, many entrepreneurs may be incapable of growing their firms or unwilling to pay the cost of doing so. In this paper we empirically explore a model that combines these two points of view. We have in mind a setting where heterogeneity is central–while there are indeed some firms that, were it not for credit constraints, could be much larger than they currently are, others have very limited growth potential. In such a world, the impact of improved access to credit would be heterogeneous in a specific way. Those who are content with a small business (we call them reluctant entrepreneurs or REs) might channel some of the newly available cheaper credit into their business or start a new business, but their target business size is small and therefore the revenue and profit effects will be small. In contrast, those who we call gung-ho entrepreneurs (henceforth GEs) have a large target business size and therefore they will take full advantage of the additional credit and the revenue and profit effects will be large.

Specifically our model has two testable predictions: One is that access to credit will have a much bigger effect on the business outcomes of the GEs than on the REs. Second, while the GEs will put all of the extra credit into business and perhaps leverage it to borrow even more from others, the REs will use some or all of it to pay down their other loans, so that their total non-microcredit borrowing may actually go down.

To identify the GEs, we use a simple economic insight. In the absence of microcredit, interest rates faced by small businesses in developing countries are high. Therefore, among those who are still willing to start a business, a large fraction are likely to be GEs. In contrast, those who only start businesses when microcredit becomes available will tend to be REs.

Our empirical exercise uses a new round of data from the randomized experiment in the city of Hyderabad, India used in Banerjee et al. (2015a) to estimate the average impact of microcredit.[1] That paper finds that the average impact of microcredit on business

[1]In 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 2010. The results of these two waves are discussed inBanerjee et al. (2015a). Due to the design, what these studies measure is the average effect of the two year head start in accessing microcredit.

and consumption outcomes is very modest.[2] Our results on the average impact confirm that access to microcredit continues to have a modest average impact six years after the treatment neighborhoods were first exposed to microcredit (and four years after the control neighborhoods got access to microcredit).

As in Banerjee et al. (2015a), we find that microfinance access does promote business growth: there are more businesses in treatment neighborhoods, and business asset stocks and durable purchases are larger, and 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, consistent with the simple selection story we tell above, most of the business impacts are driven almost entirely by the GEs, those who had a business before microcredit became available. For these firms, asset stocks, investment, self-employment hours, business expenses and revenues are all 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 GEs in control. We also find positive and significant effects on the profits of the top tercile of the GEs, 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 GEs.[3] In contrast, for the rest of the population (the REs) almost all the effects are insignificant and small in magnitude, which in the case of business outcomes is largely driven by the fact that not many households start businesses after 2006, either in treatment areas or in control, and the businesses that are started by this group remain small.

We also see the predicted differences on the borrowing side. While we see no differences in informal borrowing on average, an indicator 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 ($p<.05$). The difference of the two is highly significant. The aggregate amount of informal borrowing also goes up by a fifth of the control mean for the seasoned entrepreneurs (and

[2] Angelucci et al. (2015), Augsburg et al. (2015), Attanasio et al. (2015), Crépon et al. (2015), and Tarozzi et al. (2015) find similarly modest impacts in other countries.

[3] If 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.

the increase is significant at the 5% level) while it goes down for the rest of the population, though the effect for the REs is not significantly different from zero.

The fact that the GEs' businesses in treatment areas 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 have data on eight dimensions of network ties for all of the respondents in our sample. We find on that average households in treatment have fewer links to other households (lower average degree). However this crowdout effect is missing for the GE households and is driven by the rest. In other words, the gung-ho 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 GE 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. Examining the paths of treatment effects over time for GEs vs REs confirms this impression. Figure 1A shows the treatment effects on the stock business assets at EL1, EL2 and EL3, separately for GEs, who had a business before 2006, when Spandana entered Hyderabad, and REs, i.e. the rest of the population. The treatment effects for GEs are insignificantly different from zero at EL1 and EL2, but at EL3 those exposed to microcredit in 2006 have stocks of business assets ~Rs. 4,200 greater than those exposed later; the effect is significantly different from zero at 1%, and different from the EL1 and EL2 effects at the 10% and 1% levels, respectively. The effects for the REs are never significant and show no tendency to increase over time.

Figure 1B shows a similar result for expenditure on durable assets (for business and household use). For GEs, the EL1 treatment effect is small and insignificant; at EL2 the effect is roughly Rs. 1,000 (significantly different from zero at 10%), and at EL2 ~Rs. 1,300 (significant at 5%). Again, the effects for the REs are never significant and do not increase over time.

Of course there are other possible interpretations for the different path of treatment effects for the GEs vs. REs–in particular it could be a transitional phenomenon. Perhaps it just takes a long time to get started and that eventually the original selection will not matter and the RE firms will become like the old, GE, businesses. Or, these firms may be learning about their own types by entering and most of them will exit eventually, leaving only the "right" firms–in which case microcredit is valuable because it encourages experimentation (See Kerr et al. (2013). Karlan et al. (2012) also suggest a theory along these lines.)

We are able to rule out these explanations using the fact that Spandana did not enter all treated neighborhoods at exactly the same time: branches opened in treatment areas between April 2006 and April 2007.[4] As a result, we observe businesses in different treatment areas that opened up at the exact same time (say, August of 2006): some opened before Spandana opened in its area (e.g., Spandana's branch may have opened in October); others opened after Spandana opened in its area (e.g., Spandana may have opened in June). Moreover, because randomization was done at the matched pair level, for each treated area, we have a pre-identified control area which serves as a counterfactual. If the differential treatment effects found for GEs are simply due to the fact that GEs are older, more experienced, etc., then among this "overlapping" sample of firms that opened in the period during which Spandana was opening branches in treated areas, the firms that opened pre-Spandana (because Spandana opened relatively late in their area) should have indistinguishable treatment effects from those that opened post-Spandana (because Spandana opened relatively early in their area). If, however, microfinance induces businesses to enter that have lower returns than those who enter in the absence of microfinance, then the firms that opened pre-Spandana should have different (larger) long-term treatment effects than those that opened post-Spandana but at the same point in calendar time. In fact, this is precisely what we find, providing strong evidence that the differential long-term returns we find are due to selection rather than age or experience.

Given the very large difference in the estimated impact on the two sets of firms, it is reasonable to ask whether they could have arisen merely as a result of a head start in exposure to microcredit, especially given the small size of microcredit loans (of the order of $200-250). We therefore use the data to structurally estimate a simple model of firm growth, in the presence of technology shocks and credit constraints. The model allows for two different technologies, one with constant returns and one with diminishing returns with a fixed cost of adopting the former. There is also heterogeneity among the entrepreneurs–one group has access both technologies (we think of these as the GEs) while the other (the REs) can only access the diminishing returns technology. While the estimation of the model is ongoing, we are able to show that the model can generation a process in which the impact of temporary access to some additional credit cumulates over time and generates divergence among the GE firms (but this does not happen among the

[4]Figure 2 provides a timeline of Spandana's entry as it relates to the timing of the survey waves.

RE firms), thereby helping us explain why the impact is so much larger on the GEs. Once the estimation is complete, we will also be able to use the estimated model to study the effects of credit market interventions that differ from microcredit.*

If our interpretation is correct, it has a number of important implications for credit market policy. First, microcredit organizations often emphasize the non-selective nature of their lending as an advantage. But if most of the business growth comes from small minority of firms, then a more selective approach may be better. While we have no reason to question the fact that even the REs benefit from the loan (see Angelucci et al. (2015) who carefully explore the possibility that some groups end up doing worse from microcredit), there may be a case for focusing more of the energy on identifying the GEs and helping them grow. Second, it raises the issue of whether, from the point of view of growth, much bigger (and more selective) loans are desirable. [5]

The idea that there may be heterogeneity in the response to microcredit is not new. Angelucci et al. (2015) and Banerjee et al. (2015a) are evaluations of microcredit which examine potential heterogeneity in the outcomes and Karlan et al. (2012) makes the general point that heterogeneity may be a central piece of the story. Maitra et al. (2014) show that incentivized agents can identify productive and lower-risk borrowers in West Bengal. Karaivanov and Yindok (2015) estimate a model which makes a distinction between "voluntary" and "involuntary" entrepreneurship using data from urban Thailand and examines heterogeneous responses to credit. Beaman et al. (2015) explore the distinct but related phenomenon of heterogenous selection into credit markets, using an experiment in Mali. Both Angelucci et al. (2015) and Banerjee et al. (2015a) 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. (2015a). 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 receiving 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 confirm 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 GEs 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

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

microcredit is gone (we see that treated households have fewer links several years after microcredit is shut down)–then policies need to take this into account.

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. (2015a). 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 2 shows the timeline of the data collection as it relates to the timing of this change.

Banerjee et al. (2015a) 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.

These results 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

[6]As described below, the survey instrument for this paper is based on that used in Banerjee et al. (2015a), to facilitate comparisons across time, although new modules were added.

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. (2015a) 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. (2015a) 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 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

[7] However, it is not illegal for borrowers to seek out their lenders to make payments.
[8] We investigate the effects of this "windfall" in a companion paper (Banerjee et al., 2014).
[9] See Table 4, columns 3 and 4, respectively.

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% margin[11] 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.[13][14] 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. (2015a) 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 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. (2015a), we added survey questions about the respondent's social network, a module to capture the household's worries,

[10]The priority sector designation allows MFIs to obtain bank credit at lower interest rates.
[11]I.e the spread between the interest rate and their own cost of funds.
[12]It 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
[14]The rules on borrowing limits are enforceable due to the recent rise of microfinance credit registries in India.
[15]One (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.

happiness, and time preferences, and retrospective questions about the household's exposure to the AP crisis and desire to borrow form 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. (2013) was not feasible.[16] Instead, we asked each respondent to list the individuals with whom they engaged in 8 different activities:[17] (1) borrowing or lending cooking fuel (kerosene); (2) borrowing or lending milk or sugar; (3) borrowing or lending Rs. 100[18]; (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.[19] 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.[20] Further, almost all of the friends that 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).

[16] Banerjee et al. (2013) 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.

[17] Measuring network degree in this way does not suffer from the sampled network issues discussed in Chandrasekhar and Lewis (2011).

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

[19] The 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.

[20] We 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.

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. (2015a), we focus on intent to treat (ITT) comparisons between the initial treatment neighborhoods and control neighborhoods. We interpret the results of such comparisons as the impacts of receiving microfinance for two additional years in the past. We consider a few issues which relate to the interpretation of these impacts.

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. (2015a), 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, i.e. a windfall equal to the amount they would otherwise have had to repay, while those who were close to fully repaying the loan and obtaining a new loan received a small loan forgiveness.[21] We would like to interpret differences in the treatment versus the control group we find in this paper 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, allowing the treatment effect to differ for GEs (those with an existing business at the time of Spandana entry) vs REs. These coefficients come from OLS regressions

[21] In a companion paper (Banerjee et al., 2014) we consider the effects of the windfall on household consumption and investment.

of three indicators of windfall receipt–having an MFI loan, the number of installments left to repay (with more installments outstanding representing a larger windfall, and receiving a "large" windfall (i.e. in the top quintile of total loan amounts outstanding as of the crisis)–on an indicator for original treatment status, GE status, and treatment interacted with GE 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 treatment and control groups, either among REs or GEs.[22] This supports our interpretation that the treatment effects we identify come solely through the length of past exposure to microfinance.

3. Results

Following Banerjee et al. (2015a), 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. [23]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 gung-ho vs. reluctant entrepreneurs. For these specifications, the regressions take the form

$$y_{ia} = \alpha + \delta GE_{ia} + \beta_1 \times Treat_{ia} + \beta_2 SE_{ia} \times Treat_{ia} + X_a^{'}\gamma + \varepsilon_{ia}$$

Here, we indicate that household i in area a is a gung-ho entrepreneur by setting $GE_{ia} = 1$. 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 GEs above and beyond the impact on the REs. Thus, the total treatment effect for the GEs 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

[22]Note that GEs are 3.5pp more likely to have an MFI loan on the eve of the crisis, but this is balanced between treatment and control.

[23]Altogether, there were 52 strata, or pairs. Pairs were formed to minimize the sum across pairs A, B (area A avg loan balance – area B avg loan balance)^2 + (area A per capita consumption – area B per capita consumption)^2. Within each pair one neighborhood was randomly allocated to treatment.

the p-values of the total treatment effect $\beta_1 + \beta_2$ for the gung-ho 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 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. (2015a), 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.[24] Here,

[24]We would ideally also like to measure each household's total stock of microfinance taken between 2006 and October 2010 from all MFIs. However, this is infeasible because the amount of loans taken and fully repaid between survey waves was not measured. However, the existence of such loans (though not the amount) was measured, so we can construct a proxy for ever borrowing at any time, presented in Panels A and B.

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 loans[25] 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 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.[26] 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 GEs and REs. 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 gung-ho and reluctant entrepreneurs. 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 GEs.

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 gung-ho entrepreneurs took larger loans (but were equally likely to borrow) from microlenders.

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 inBanerjee et al. (2015a) 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,

[25]This can be interpreted as the number of lenders.

[26]Increases of between Rs. 2,000 and Rs. 5,000 are common each year.

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 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 gung-ho entrepreneurs alone. GEs in the treatment group are 6.4% more likely to own a business and own, on average, 0.10 more businesses than those in control (columns 1 and 2). Their businesses are larger, as well: GEs 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 reluctant entrepreneurs in the treatment group are no different than those for those in the control group, with two exceptions: REs in the treatment group have .174 more employees in their largest business and pay out Rs. 275 more in wages than in the control group (columns 7 and 8).

These results for business inputs and revenues for GEs in the treatment group suggest that their businesses not only are larger, but also generating more profits than GEs in the control group. Figure3, Panel A plots the results of bootstrapped quantile regressions for business profits on treatment status for GEs. 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 RE households, on the other hand, experienced such results, as Figure3, 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. REs in the treatment group show no significant differences in their labor supply relative to the control group. Gung-ho entrepreneurs, on the other hand, show multiple significant treatment effects: GE 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 GEs 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 GEs and REs and *within* each group of households. We find no significant average treatment effects on consumption for either GEs or REs, as Panel B, column 1 shows. But as demonstrated in Panel A of Figure 4 (displaying the results of bootstrapped quantile regressions for per-capita consumption for gung-ho 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 GEs in the control group (Rs. 3325). However, at no point in the distribution of per-capita consumption for REs (Figure 4, 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.

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 gung-ho entrepreneurs. In the treatment group, GEs spent Rs. 1,937 more on durables and Rs. 61 more on business durables in the previous year than GEs in the control group, while REs in treatment and control show no differences in either of these outcomes (columns 3 and 5). Moreover, GEs show a large and highly significant increase in spending on non-business durables: Rs. 1,540, or 18.9% of the mean for GEs in the control group (Panel B, column 4).

Consistent with Banerjee et al. (2015a)'s results for their second endline survey (in 2010), we find no difference between treatment and control households - whether among GEs or REs - 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).[27] 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. (2015a) 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 gung-ho entrepreneurs. While in 2012, the GEs 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)[28] 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).

[27]The 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. (2015a) 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.

[28]SHGs 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 GEs and REs. As Panel B, column 1 shows, REs in the treatment group were 4.4% less likely than those in the treatment group to have an informal loan. Gung-ho entrepreneurs in the treatment group, on the other hand, were no less likely to have an outstanding informal loan than in the control group. These same entrepreneurs also had larger outstanding informal loans, as Panel B, column 2 shows: GEs in the treatment group had, on average, Rs. 12,400 more in outstanding informal debt relative to the control group. Conversely, there was no significant effect on informal loan size for REs. Thus, on both the extensive and intensive margins, seasoned entrepreneurs tend to utilize more informal credit under greater exposure to microcredit, while reluctant entrepreneurs tend, on the extensive margin, to utilize less. These results are consistent with our model, where microcredit crowds out informal borrowing for REs but actually increases the demand for informal credit among GEs.

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.[29] 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 that would be available should the need for a social activity such as borrowing or advice arise.[30] This may suggest that when microfinance is functional in a community, the loss of links may be even greater.

[29]Recall 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.

[30]We 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 gung-ho vs. reluctant entrepreneurs. This is in fact what we find, as the REs drive 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 GEs. It is also interesting that on average, the GEs in the control group have no more network connections than the REs. The stark heterogeneous treatment effect also implies that the magnitude of the crowdout for the REs is even higher than in the average population. Reluctant 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 GEs 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 GEs 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 gung-ho entrepreneurs.

3.7. **Worries and Happiness.** Lastly, we can measure whether access to microfinance in the past caused any differences in happiness and worries, as measured by responses to survey questions.[31] Table 8 shows that treatment households are both more worried and less happy than control households.[32] 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 RE households. While we cannot statistically distinguish between the effects on GEs and REs, we can only detect a significant impact for the REs.

[31]Haushofer and Shapiro (2013) show that responses to a set of questions similar to ours (and on which our questionnaire is modeled) correlate with levels of salivary cortisol, a physiological marker of stress.

[32]All of the worries, happiness and financial security indexes are scaled to have units of standard deviations. 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.

3.8. **A test of the selection mechanism: Overlapping sample results.** We have argued that the differences observed between the REs and GEs reflect that fact that households differ in their underlying potential productivity as entrepreneurs, and that when microlenders lend to households who have not demonstrated entrepreneurial potential, they screen in those who are less well-positioned to benefit (at least in terms of marginal product of capital). However, other explanations could be driving our results– namely, the GE businesses are older, on average, and the GEs have more experience. It could be that, with time, the REs would accumulate enough age/experience and would then look like the GEs.

To test these alternative experience-based explanations, we use the fact that Spandana did not enter all treated neighborhoods at exactly the same time: branches opened in treatment areas between April 2006 and April 2007.[33] As a result, we observe treatment-area businesses that opened at the exact same time, some of which opened before Spandana's entry to the area (because Spandana's branch in that area opened relatively late), while others opened after Spandana's entry (because Spandana's branch in that area opened relatively early). Of course, Spandana's decision of where to open early vs late is not random–Spandana may have opened first in the largest areas, those closest to its headquarters, etc. However, because randomization was done at the matched pair level, for each treated area, the control area in the same matched pair serves as a counterfactual. We refer to the sample of businesses that opened during the time that Spandana was opening branches as the "overlapping sample."

Figure 5 shows a schematic illustrating the idea behind this overlapping sample. In Matched Pair A, Spandana entered the treated area A^T, at t_1; in Matched Pair B, Spandana did not enter the treated area B^T, until t_3. In both pairs, Spandana did not enter the control areas, A^C and B^C, until after the first endline. In each of the 4 areas, there is a set of businesses that opened at time t_2, after Spandana entered A^T but before it entered B^T. Finally, at t_4, endline outcomes, y, are measured. The comparison $\bar{y}_{A^T} - \bar{y}_{A^C}$ identifies the treatment effect on businesses opened *after* Spandana's entry, while the comparison $\bar{y}_{B^T} - \bar{y}_{B^C}$ identifies the treatment effect on businesses of the same age, but opened *before* Spandana's entry.

[33]The timing of the first endline was such that no area was surveyed less than 12 months after Spandana entered.

If the differential treatment effects found for GEs are simply due to the fact that GEs are older, more experienced, etc., then among this overlapping sample, those that opened pre-Spandana (because Spandana opened relatively late in their area) should have indistinguishable treatment effects from those that opened post-Spandana (because Spandana opened relatively early in their area). If, on the other hand, microfinance induces businesses to enter that have lower returns than those who enter in the absence of microfinance, then the firms that opened pre-Spandana should have different (larger) long-term treatment effects than those that opened post-Spandana but at the same point in calendar time.

Table 12 shows the results. Panel A shows the EL1 treatment effects for businesses opened pre-2006, before Spandana opened any branches in Hyderabad. The treatment effects for this sample are large and positive, though in some cases imprecisely estimated; however, the index of business incomes is 0.071 standard deviations higher in treatment than control, significant at 10%. Panel B shows EL1 treatment effects for businesses opened in 2006 or 2007, before Spandana had opened in their area (equivalent to B^T in Figure 5), compared to businesses opened in the same time frame in the control areas in the same matched pairs (equivalent to B^C in Figure 5). The treatment effects for these businesses are similar to those for pre-2006 businesses and, if anything, stronger: the effect on the index of business incomes is 0.148 standard deviations, significant at 5%.

Finally, Panel C shows EL1 treatment effects for businesses opened in 2006 or 2007, after Spandana had opened in the area (equivalent to A^T in Figure 5), compared to businesses opened in the same time frame in the control areas in the same matched pairs (equivalent to A^C in Figure 5). The treatment effects for these businesses, while imprecisely estimated, are uniformly negative. The effect on the index of business incomes is -0.183; while this is not significantly different from zero, it is significantly different from the effect of plus 0.148 seen for the pre-Spandana (but same-aged) businesses.

Figure 6 further investigates whether age or experience effects could be at play in generating the observed differences between the GE and RE samples. Businesses opened before 2006 are separated into quintiles of age (with quintile 1 being the oldest and 5 the newest), and treatment effects on the index of business outcomes are estimated separately for each quintile, using the corresponding quintile in control areas as the counterfactual. The dashed gray horizontal line shows the overall treatment effect for the pre-2006 businesses. Then, we again plot the effects for the pre- and post-Spandana businesses in the overlapping sample and, in the rightmost bar, the difference between the two.

Panel A shows the EL1 results, which were summarized above. Panel B shows the results at EL2, which are quite similar: all age quintiles of pre-2006 businesses show treatment effects which are indistinguishable from each other; pre-Spandana businesses in the overlapping sample show a significantly positive treatment effect, and post-Spandana businesses in the overlapping sample show a treatment effect which is negative and imprecise, but significantly different from that of the pre-Spandana businesses in the overlapping sample. In Panel C, the EL3 effects are plotted. There is now more loss of precision, in part because some entrepreneurial households have now closed their businesses (a phenomenon we model below), but the qualitative pattern remains the same.

The facts that, among businesses of the same age, those opened pre-Spandana show significant, positive treatment effects while those opened post-Spandana show insignificant, negative effects; and that there is no systematic tendency of older pre-2006 businesses to have larger treatment effects than newer pre-2006 businesses buttress our interpretation that the differential effects we observe are due to selection rather than age or experience.

3.9. **Attrition.** Given the long time horizon since the sampled households were first contacted, it is perhaps unsurprising that some individauls have attrited from the sample, i.e., could not be found. Appendix Table 13 shows that, relative to endline 1 (2007), approximately 84% of households were located at endline 3 in 2012. Relative to a census conducted in 2006, 62% of households were found.[34] Neither of these attrition rates is differential across treatment vs. control. Panel C of Table 13 tests whether attrition was correlated with household characteristics, measured at the census. Spandana borrowers are less likely to attrit, which is as expected since loan officers could help survey staff locate borrower households; and those in better-quality pucca houses were more likely to attrit. No other characteristics predict attrition.

To address whether our results might be sensitive to attitrition, we compute Lee (2009) bounds for key oucomes. Appendix Table 14 shows the results. With a few exceptions, the Lee bounds are informative, i.e. when the non-attrition-adjusted estimate is significantly different from zero, the bounds do not include zero.

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. Due to a fixed cost in accessing the high-return technology, access to even a small amount of additional credit pushes households over the adoption threshold. Moreover, it may take time for large gains to appear as households accumulate enough wealth to operate the technology at its optimal scale. Through counterfactual exercises, we next plan to ask what the increase in profits

[34] The recontact rate is much lower when the census is used as a basis because not all households identified in the census were even attempted to be recontacted in endline 1; see Banerjee et al. (2015a) for details.

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

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

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. We do not incorporate an endogenous entry decision because Banerjee et al. (2015a) do not find evidence that the extensive margin of business ownership responds to microfinance access.[35] When $e_{it}=1$, the household has access to production technologies, discussed below.

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

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

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, inflation and risk of loss etc. .)

Types

We have shown in the reduced form that returns to capital are much higher for seasoned entrepreneurs than novices, and the overlapping sample results suggest that this return is a persistent characteristic of an individual. This we model two types of entrepreneurs, *Gung Ho (GEs)* and *Reluctant (REs)*: $\theta\in\{GE,RE\}$. An individual's type is persistent. When productive, the REs only have access to a *Low* technology, with profits equal to:

$$\pi_L(K_t)=A_L K_t^{\alpha}$$

where K measures the rupee value of total inputs used in production. Note that we do not subtract K_t from the measure of profits; this reflects our modeling assumption

[35]While Banerjee et al. (2015a) find a small but significant increase the in number of businesses per household, there is no effect on the likelihood that a household owns at least on business.

that capital is liquidated in the capital market every period.[36] We also posit that this technology exhibits decreasing returns to scale with $\alpha < 1$.

When GEs are productive, they also have access to a *High* technology.[37] 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.[38]

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

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 8 through 9, discussed below.)

Transitions in and out of entrepreneurship

Given that we observe frequent transitions in and out of entrepreneurship, we model a stochastic process that governs the chance an individual will be in business in period t as a function of their $t-1$ status. These transitions are allowed to differ for GEs vs REs. Let $\tau_1$be the probability of going from productive to unproductive for GEs, and ν_1 the probability of going from unproductive to productive for GEs. For REs, the analogous parameters are τ_2 and ν_2. Finally, λ is the fraction of gung-ho entrepreneurs. As discussed below, we estimate these parameters via MLE.

Borrowing

Households have the ability to borrow from informal lenders in each period, and when available, 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 an amount 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 also choose levels of microfinance borrowing $0 \leq B_t^{MF} \leq \bar{b}_t^{MF}(\tau)$ at interest rate r_{MF}. The borrowing cap $\bar{b}_t^{MF}(\tau)$ depends on both the year (pre- vs. post-Spandana 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, as shown by Banerjee et al. (2015a). Finally, we posit that microfinance borrowing has a lower effective interest rate

[36]Thus $A_L K^\alpha$ can be thought of as the sum of the profits (revenues less inputs) from production, plus the sale of assets.

[37]Below we discuss the Markov process that governs when an entrepreneur is productive.

[38]The 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.

than social network borrowing: $r_{MF} < r_{SN}$, a claim for which we find empirical support in our data.[39]

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 households. During any regime g households believe that this regime will continue to be the status quo forever, which is reasonable in this context given the rapid entry of microfinance and the unanticipated nature of the AP Crisis.

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 $B_t = \left(B_t^{SN}, B_t^{MF}\right)$. A productive RE household decides only how much capital K_t to invest and how much to borrow $B_t = \left(B_t^{SN}, B_t^{MF}\right)$in the Low project. Then at the end of the period, labor income 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 re-frame the standard utility maximization problem of Equation 4.1 into the recursive Bellman equation form:

[39] Karaivanov and Kessler (2015) and Lee and Persson (2013) discuss disadvantages of borrowing from the social network in terms of the risk of project failure and hence, damage to the social relationship. Collins et al. (2009) provide examples of implicit and explicit costs of social network borrowing such as guilt and unwanted intrusion from the lender.

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

s.t.

$$\begin{aligned}
W' + c &= D^{H}[\pi_{H}(K) + K] + \left(1 - D^{U} - D^{H}\right)[\pi_{L}(K) + K] + D^{U}[\pi_{u}(W)] \\
&\quad - r_{SN}B^{SN} - r_{MF}B^{MF} + y \\
0 &\leq B^{SN} \leq b^{SN} \\
0 &\leq B^{MF} \leq b^{MF}(\tau, g) \\
W' &\geq 0 \\
K &\leq W + B^{SN} + B^{MF} \\
D^{H}, D^{U} &\in \{0,1\} \\
1 - D^{U} - D^{H} &\in \{0,1\}
\end{aligned}$$

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 assume that the instantaneous utility function takes the standard CRRA form $u(c) = \frac{1}{1-\sigma}c^{1-\sigma}$. We estimate six parameters of the model $(A_{L}, \alpha, A_{H}, \underline{K}, \sigma, \beta)$ using Simulated Method of Moments and four parameters (the transition probabilities and share of GEs) via MLE using observed paths of entrepreneurship. The other parameters from the model are either calibrated or estimated separately from our survey data.

We calibrate the return on savings ρ to be equal to be equivalent to $1 - r^{MF}$, i.e. 0.76, given that households commonly used microloans at a rate of 24% as “savings in reverse”. 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 $b_{t}^{MF}(\tau)$ are taken from administrative microfinance data. The fixed labor earnings s are calibrated from the median EL1 labor market earnings of the control group.

We estimate the model in 3 steps: MLE estimation of the transition probabilities and type distribution; GMM estimation of the four production function parameters using EL1 data; and GMM estimation of the two parameters that govern the savings/consumption dynamic decisions. We discuss each in turn, then present the results.

Step 1: Estimation of the transition probabilities and type distribution. We estimate the transition probabilities and type distribution via MLE. There are four probabilities: τ_1, ν_1,τ_2 and ν_2. The type distribution is summarized byλ, the share of GEs. Let $T_k = 1$ for GEs and 0 for REs. Following the approach of Duflo et al. (2012), we estimate these 5 parameters, $\theta \equiv [\tau_1, \nu_1, \tau_2, \nu_2, \lambda]$, to match the observed probabilities of each of the observed paths of entrepreneurship. We have four data points on each household's entrepreneurship realizations: one year prior to EL1 (reported retrospectively at EL1), at EL1, at EL2 and at EL3. This generates $2^4 = 16$ possible histories (0000, 0001, 0010, etc.). Since the probabilities of the histories must sum to one for each type, we have up to $2^4 - 1 = 15$ degrees of freedom to estimate 5 parameters. The log likelihood function is

$$\begin{aligned} LLH(\theta) &= log\left(\prod_{i=1}^{N}\left[\sum_{k=1}^{2}\mathrm{P}(T_k|\theta)\prod_{t=1}^{4}\left(Pr(e_{it}=1|e_{i,t-1},\theta,T_k)^{e_{it}}\left[(1-Pr(e=1|e_{i,t-1},\theta,T_k))^{1-e_{it}}\right]\right)\right]\right) \\ &= \sum_{i=1}^{N}\log\left[\sum_{k=1}^{2}\mathrm{P}(T_k|\theta)\prod_{t=1}^{4}\left(Pr(e_{it}=1|e_{i,t-1},\theta,T_k)^{e_{it}}\left[(1-Pr(e=1|e_{i,t-1},\theta,T_k))^{1-e_{it}}\right]\right)\right] \end{aligned}$$

Step 2: Estimation of the production parameters. Next, using only the EL1 data, we estimate the four production function parameters $(A_L, A_H, \alpha, \underline{K})$. Estimation is via simulated method of moments. We use the following moments: capital, profits, informal borrowing and microfinance borrowing; interacted with indicators for treatment, seasoned (i.e., having a business before Spandana entry) and seasoned$\times$business. Thus the estimates are identified using variation from the randomization.

The state variable of the model is wealth. Because initial wealth is unobserved, we draw a starting value of wealth as an unconditional draw from a Pareto distribution whose parameters are calibrated based on a separate baseline sample of 2,800 households.[40] The fit of the Pareto distribution to the baseline data, shown in Figure 7, is quite good. We integrate over wealth draws in the estimation step.

Step 3: Estimation of the Dynamic Parameters

Next, using the EL2 data along with the production function estimates in step 1, we estimate the parameters (σ, β) that govern the savings/consumption dynamic decisions. Again, estimation is via simulated method of moments. To do this we solve the dynamic program given the transition parameters from step 1 and the production function parameters from step 2, and integrate over the wage income distribution. The resulting model moments are then matched to their empirical counterparts. The moments matched are Endline 2 values of capital, profits, and consumption.

Preliminary Results. We find that GEs who are in business at t have a 92% chance of remaining in business at $t+1$; GEs not in business have a 14% chance of entering business

[40]See Banerjee et al. (2015a) for a discussion of the baseline sample and why it does not form part of the panel.

the next period. REs in business have a 61% chance of remaining in business the next period, and REs not in business have just a 6% chance of entering business. The share of GEs, λ, is estimated to be 0.417.

In Figure 9, we plot the log gross revenues of the technologies as function of log capital. In Figures 8, we present investment policy functions as a function of household wealth and type, evaluated at the preliminary parameter estimates. Indeed we find a substantial fixed cost of 60,000 INR. Note that microfinance entry reduces the minimum wealth at which Type 1 households choose to pay the fixed cost and invest in the H technology.

Dynamics. We take the results from the estimated parameters and simulate the model across all 6 periods in our timeframe. We present wealth, the key state variable, in Figure 10, shown separately for each group: (treatment, seasoned), (treatment, novice), (control, seasoned), (control, novice). We find that, as expected, seasoned entrepreneurs' wealth grows more quickly, with divergence for the treated group. Novices do not experience divergence.

Next, we match the differential treatment effects for seasoned entrepreneurs for $log(K+1)$ across the three waves: comparing the model to the data. Figure 11 shows the results. Note that capital from waves 1 and 2 is a matched moment in the estimation, but wave 3 is out of sample. The model replicates the qualitative patterns observed in the data.

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 long-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 well-suited for entrepreneurship. We find that essentially all of the benefits of credit access accrue by increasing entrepreneurship on the *intensive* margin: for those individuals with an existing business before the entry of microfinance, we find economically meaningful, positive effects on all aspects of the household businesses. Further, these effects are are larger than those found in the shorter-run microfinance impact evaluation literature (see Banerjee et al. (2015b)). Indeed, the results are larger than those detected in the same sample of borrowers looking at shorter time horizons (see Banerjee et al. (2015a) for a survey of this literature). We also begin to observe evidence on household consumption impacts for the "gung-ho" 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 ("reluctant entrepreneurs").

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 reluctant entrepreneurs. However, we detect no such effect for the gung-ho entrepreneurs. This channel of crowdout might explain why impacts of microfinance are minimal among certain sub-populations: while microfinance may reduce borrowing costs, overall demand for credit may change very little for some groups. It is essential for policymakers to understand these interactions when designing financial inclusion policies and when targeting financial products to specific groups.

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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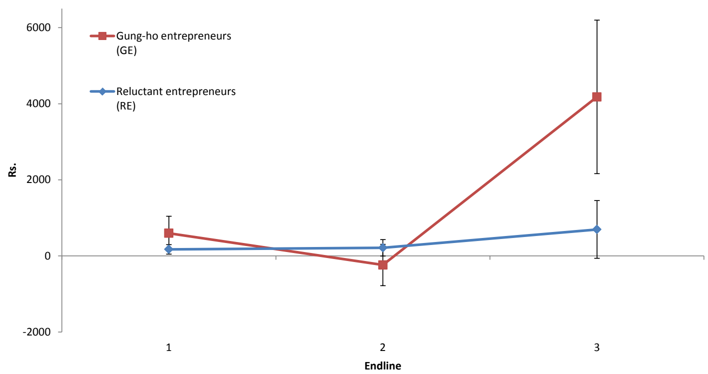
Figures

Panel A: Business assets



Panel B: Durable consumption

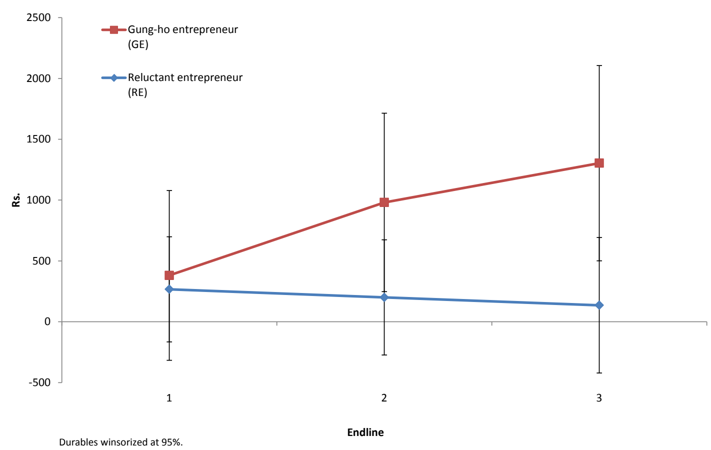


Figure 1. Treatment effects over time

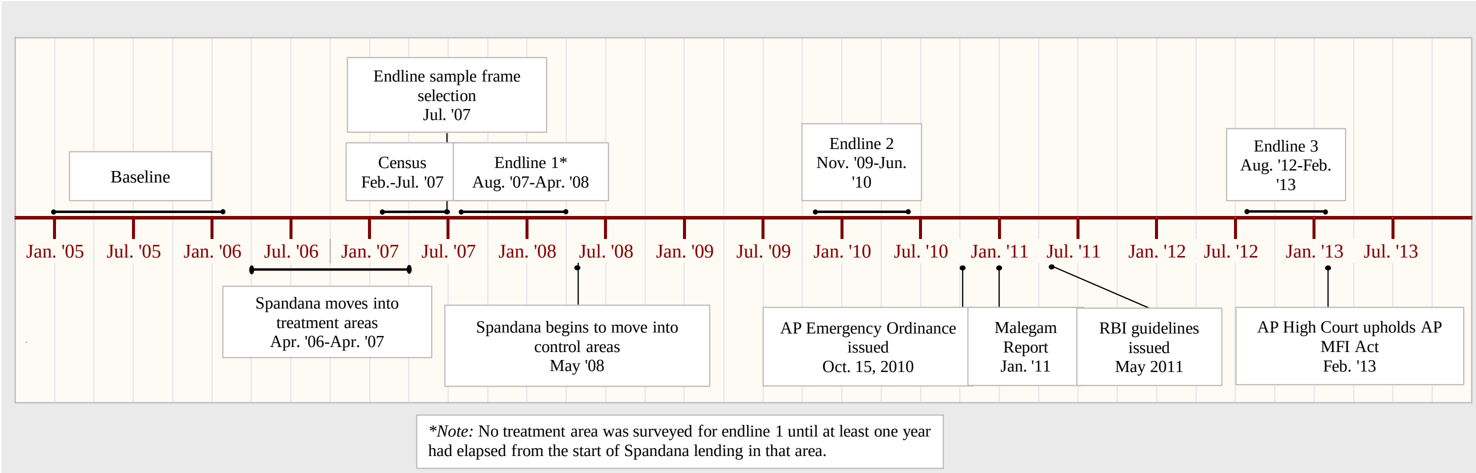
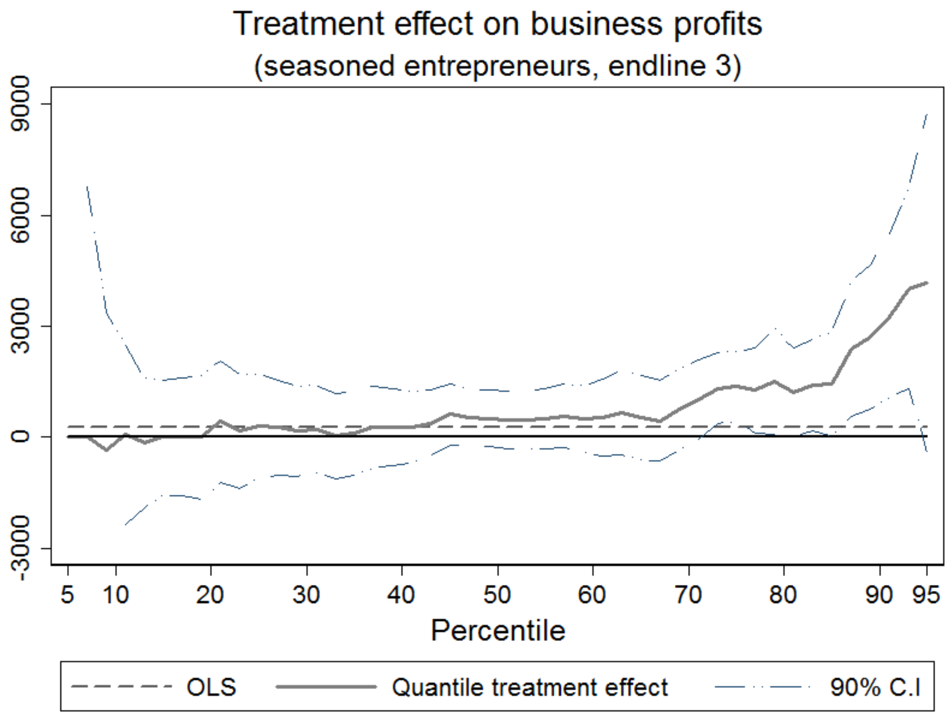


Figure 2. Timeline of Survey Activities and Microfinance Crisis

Panel A: Seasoned Entrepreneurs



Panel B: Novice Entrepreneurs

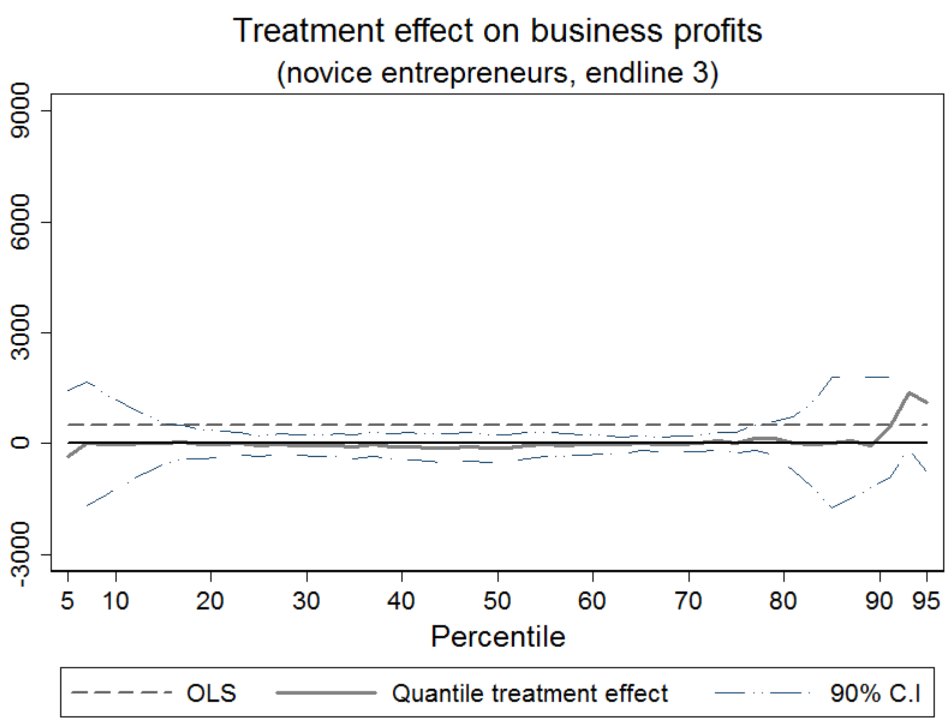
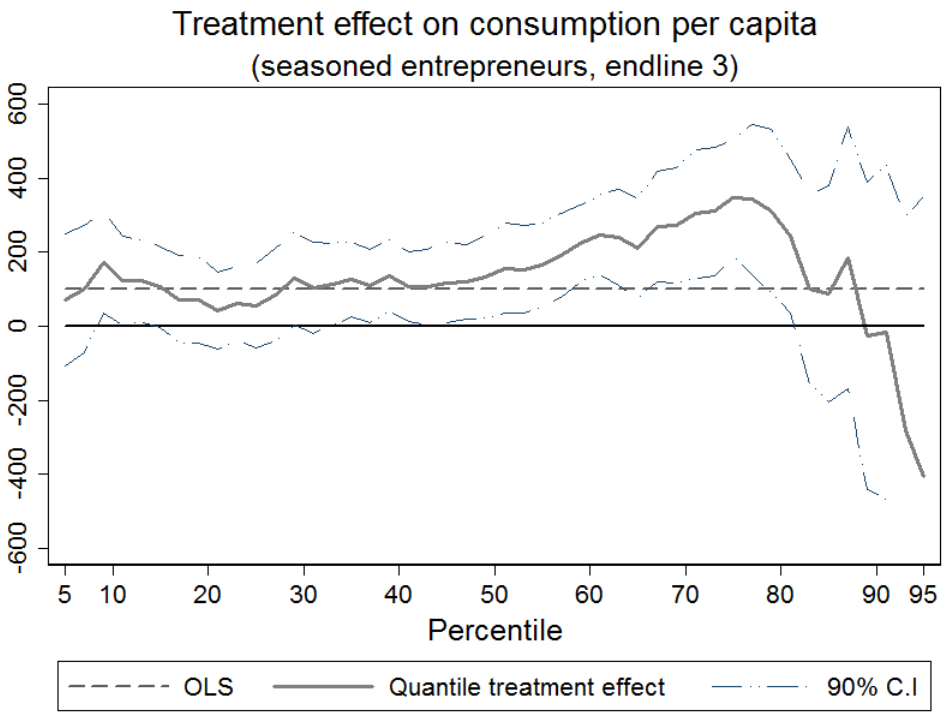


FIGURE 3. Quantile treatment effects for business profits

Panel A: Seasoned Entrepreneurs



Panel B: Novice Entrepreneurs

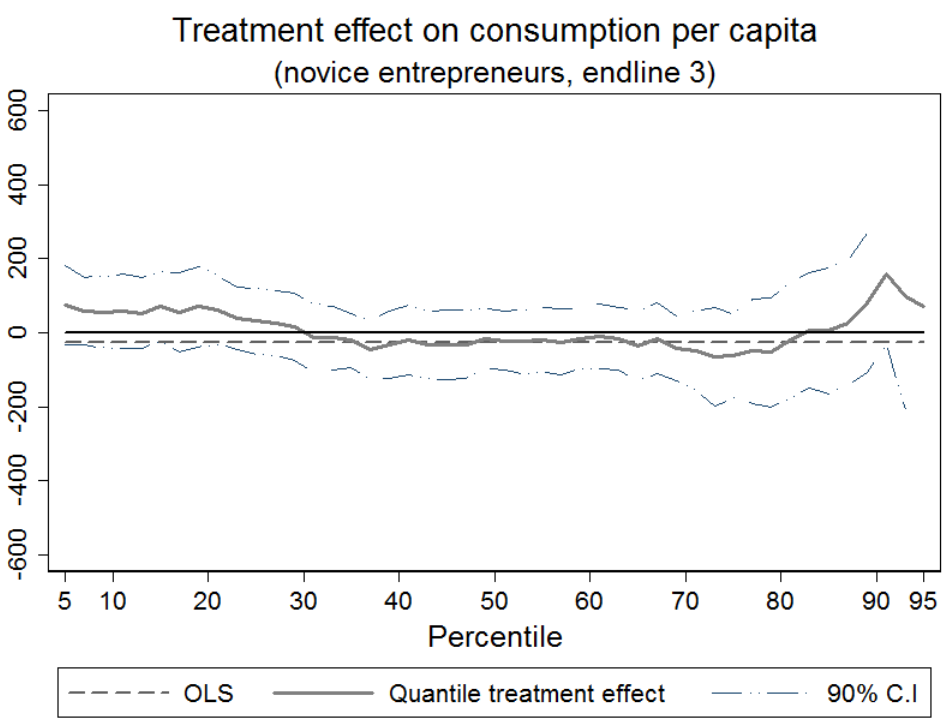


FIGURE 4. Quantile treatment effects for monthly consumption per adult equivalent

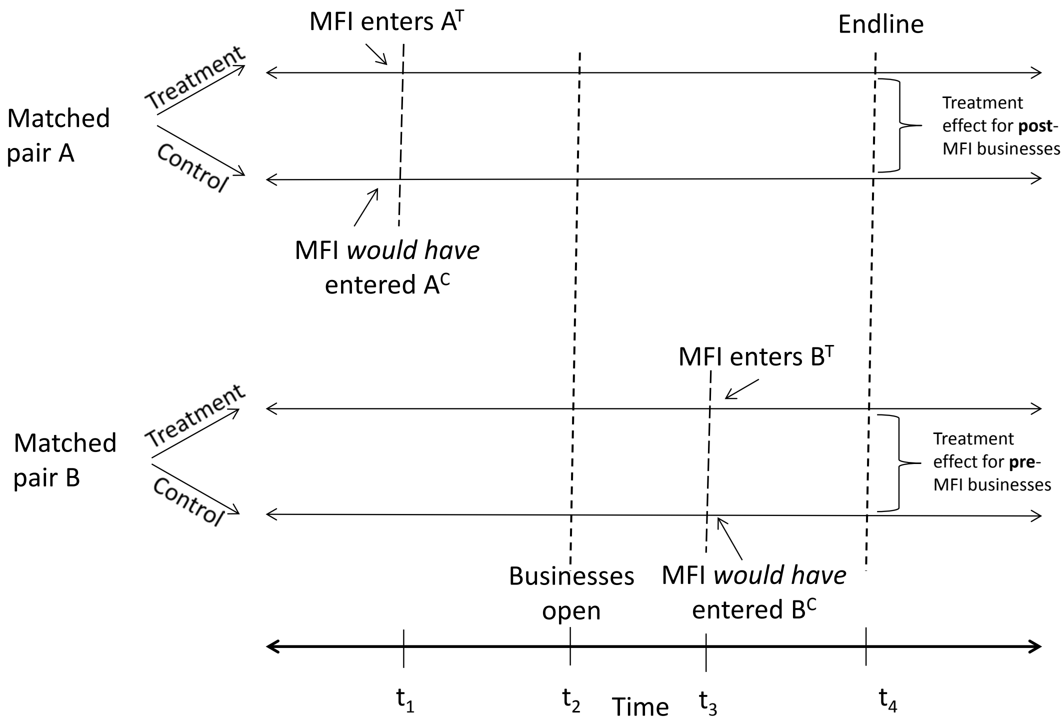


FIGURE 5. Overlapping sample identification

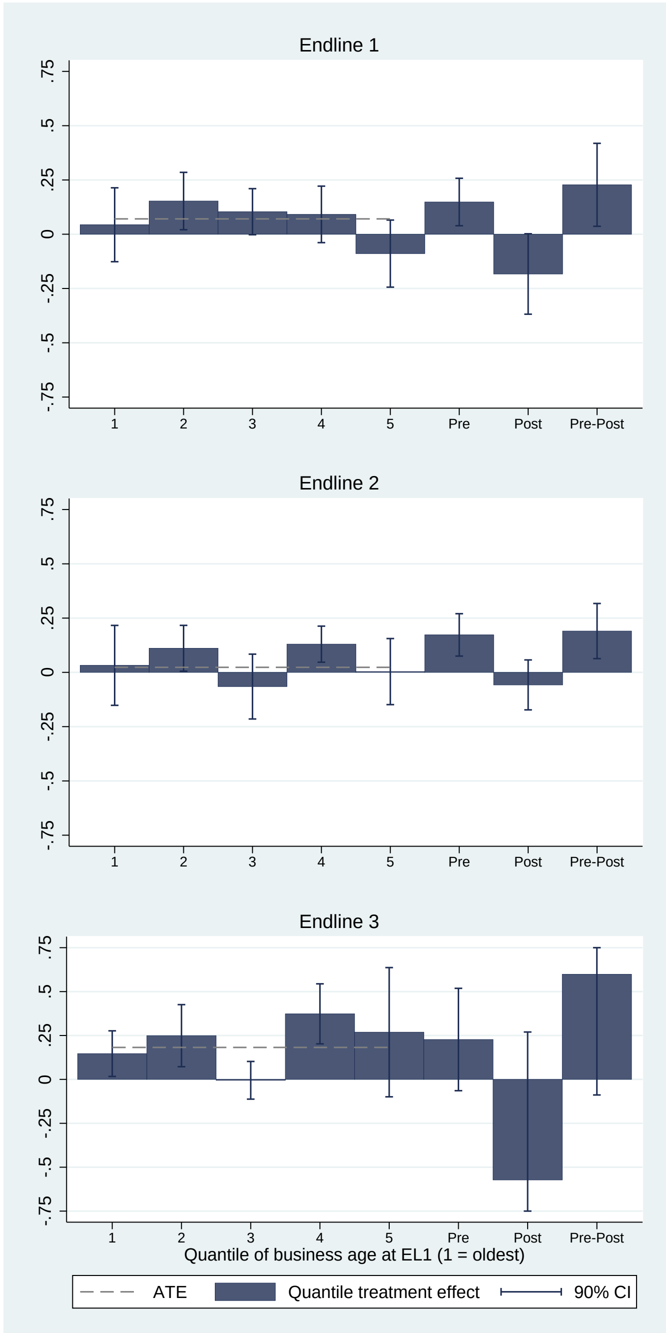


FIGURE 6. Experience vs. Selection: Treatment effects on index of business outcomes

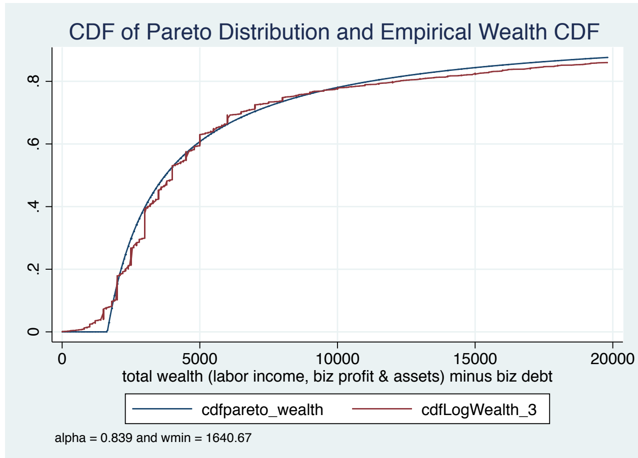
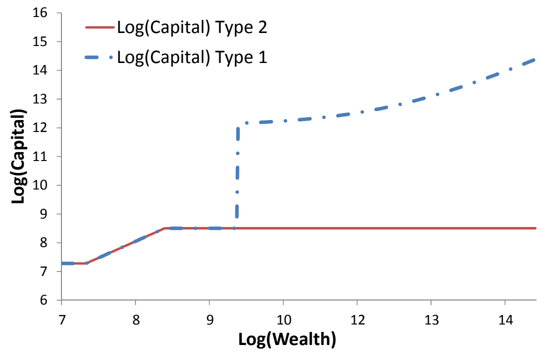


Figure 7. Distribution of baseline wealth

Panel A: Control



Panel B: Treatment

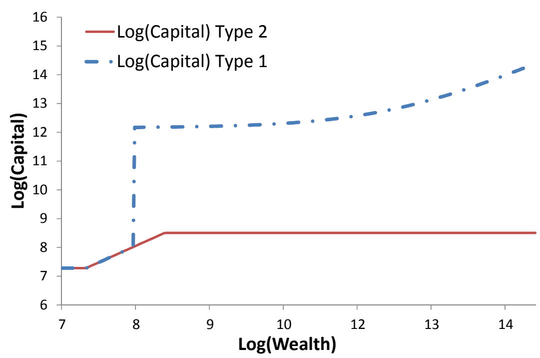


FIGURE 8. Investment Policy Functions

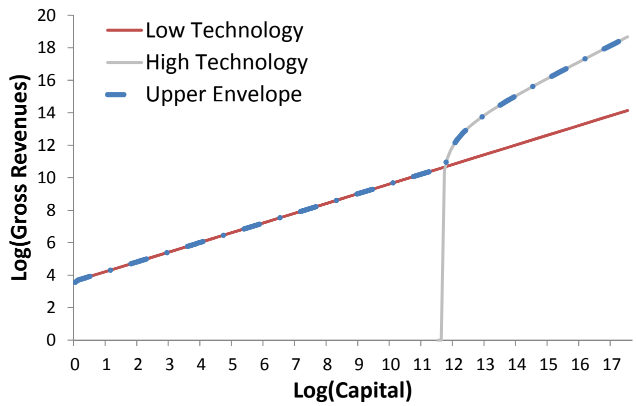


FIGURE 9. Gross revenues

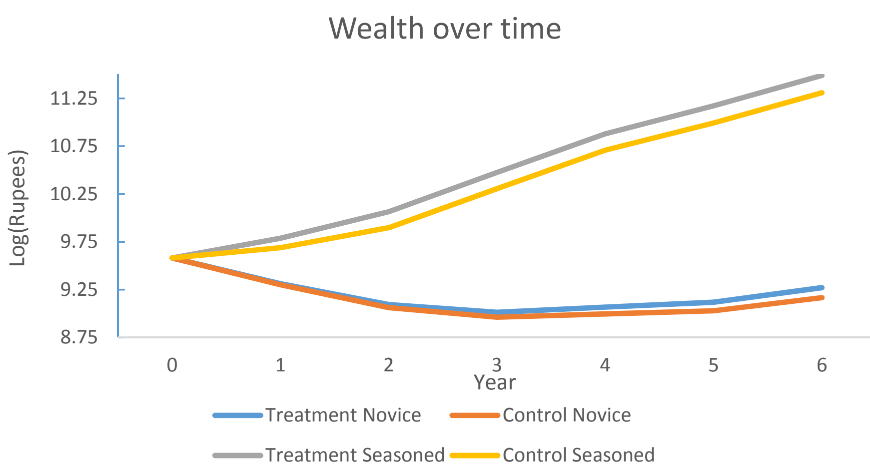


FIGURE 10. Wealth paths

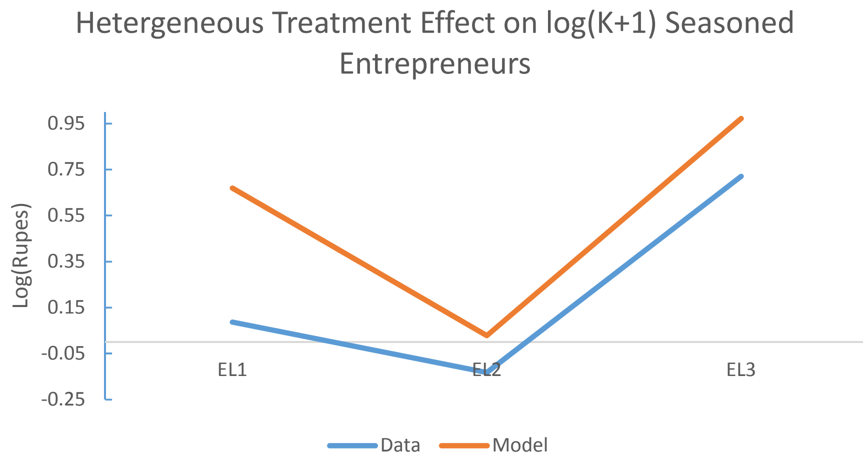


FIGURE 11. Differential treatment effects for seasoned entrepreneurs, data vs model

Tables

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

	Obs	Mean	St. Dev.
Household composition			
# 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)
Access to credit (endline 2)			
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)
Amount borrowed at endline 2 from (Rs.):			
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)
Businesses			
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)
Consumption (per household per month)			
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.
All links			
Degree (hypothetical)	2677	5.948	(3.722)
Financial links (hypothetical)	2677	4.372	(2.603)
Non-financial links (hypothetical)	2677	2.926	(2.569)
Supported links			
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)
Non-supported links			
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)
Links from microfinance groups (MFI borrowers only)			
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	-0.012	0.039	0.004
	(0.019)	(0.024)	(0.005)
Seasoned entrepreneur	0.035**	0.002	0.004
	(0.016)	(0.029)	(0.006)
Treatment X Seasoned entrepreneur	0.009	-0.016	0.004
	(0.025)	(0.037)	(0.010)
Control mean	0.202	0.386	0.027
	(0.402)	(0.297)	(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***	0.032	0.008	0.044*	
	(0.022)	(0.022)	(0.020)	(0.024)	
Control mean	0.256	0.420	0.332	0.498	
	(0.436)	(0.494)	(0.471)	(0.500)	
N	6804	6128	6143	5467	
Panel B: Cumulative exposure to microcredit by entrepreneurial status					
Seasoned entrepreneur	0.163***	0.112***	0.093***	0.110***	
	(0.023)	(0.022)	(0.020)	(0.022)	
Treatment	0.109***	0.029	0.003	0.036	
	(0.021)	(0.023)	(0.021)	(0.026)	
Treatment X Seasoned entrepreneur	-0.002	0.005	0.013	0.020	
	(0.030)	(0.030)	(0.031)	(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	0.385	0.302	0.463	
	(0.404)	(0.487)	(0.459)	(0.499)	
Control mean (seasoned entrepreneurs only)	0.372	0.503	0.401	0.580	
	(0.483)	(0.500)	(0.490)	(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	0.026	946.417**	0.061***	1132.643***
	(0.020)	(0.038)	(474.365)	(0.014)	(257.510)
Control mean	0.332	0.530	6670.434	0.112	1897.522
	(0.471)	(0.937)	(13627.432)	(0.316)	(6768.526)
N	6143	6143	6143	6143	6143
Panel D: Microcredit exposure as of endline 2 by entrepreneurial status					
Seasoned entrepreneur	0.093***	0.173***	2557.957***	0.052***	798.113**
	(0.020)	(0.049)	(671.712)	(0.018)	(388.901)
Treatment	0.003	0.000	677.234	0.050***	800.099***
	(0.021)	(0.038)	(508.180)	(0.014)	(267.354)
Treatment X Seasoned entrepreneur	0.013	0.075	754.962	0.034	1036.985**
	(0.031)	(0.073)	(929.289)	(0.024)	(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	0.472	5812.723	0.096	1629.648
	(0.459)	(0.878)	(12661.459)	(0.294)	(6782.720)
Control mean (seasoned entrepreneurs only)	0.302	0.472	5812.723	0.096	1629.648
	(0.459)	(0.878)	(12661.459)	(0.294)	(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) Has business	(2) Number of businesses	(3) Started a business in last 12 months	(4) Closed a business in last 12 months	(5) More than 1 worker in any business	(6) More than 2 workers in any business	(7) Workers in largest business	(8) Total wages paid	(9) Assets (stock)	(10) Log expenses	(11) Log revenue
Panel A: Treatment effects											
Treatment	0.038*	0.056*	0.006	0.008**	0.030**	0.016**	0.208**	373.747***	2012.651***	0.272*	0.311*
	(0.020)	(0.031)	(0.005)	(0.004)	(0.015)	(0.006)	(0.087)	(133.018)	(515.398)	(0.140)	(0.157)
Control mean	0.307	0.371	0.032	0.027	0.102	0.035	0.507	348.367	6811.262	2.296	2.640
	(0.461)	(0.613)	(0.176)	(0.161)	(0.303)	(0.184)	(1.292)	(4700.427)	(22049.316)	(3.786)	(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***	0.525***	0.025***	0.019**	0.174***	0.055***	0.765***	488.639*	8751.106***	3.366***	3.900***
	(0.020)	(0.026)	(0.008)	(0.008)	(0.018)	(0.009)	(0.071)	(266.816)	(1044.605)	(0.176)	(0.181)
Treatment	0.024	0.031	0.009	0.006	0.017	0.009	0.174**	275.264**	827.149	0.101	0.127
	(0.018)	(0.024)	(0.006)	(0.004)	(0.012)	(0.006)	(0.076)	(118.604)	(615.730)	(0.115)	(0.128)
Treatment X Seasoned entrepreneur	0.040	0.076**	-0.011	0.006	0.040	0.023*	0.102	311.864	3744.398**	0.502**	0.588**
	(0.028)	(0.035)	(0.013)	(0.012)	(0.024)	(0.013)	(0.143)	(368.366)	(1851.031)	(0.222)	(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	0.208	0.025	0.021	0.049	0.019	0.279	197.888	4139.482	1.259	1.452
	(0.382)	(0.483)	(0.155)	(0.144)	(0.215)	(0.135)	(0.865)	(2496.403)	(19953.887)	(2.989)	(3.320)
Control mean (seasoned entrepreneurs only)	0.604	0.742	0.048	0.039	0.226	0.073	1.031	694.898	12934.882	4.687	5.416
	(0.489)	(0.712)	(0.215)	(0.194)	(0.419)	(0.261)	(1.842)	(7648.542)	(25192.959)	(4.313)	(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) Total weekly labor hours	(2) Total weekly hours in self-employment	(3) Total weekly hours in wage labor	(4) Total weekly hours in casual labor
Panel A: Treatment effects				
Treatment	2.170	2.752**	0.351	-0.937
	(1.661)	(1.159)	(2.037)	(1.166)
Control mean	87.490	15.400	56.918	15.120
	(56.528)	(30.304)	(53.373)	(30.015)
N	5744	5744	5744	5744
Panel B: Treatment effects by entrepreneurial status				
Seasoned entrepreneur	4.798**	23.537***	-14.248***	-4.480***
	(2.107)	(1.587)	(2.257)	(1.347)
Treatment	0.150	1.259	-0.067	-1.011
	(2.021)	(0.859)	(2.226)	(1.403)
Treatment X Seasoned entrepreneur	6.501*	4.569**	1.527	0.293
	(3.321)	(1.962)	(3.279)	(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	8.175	61.501	16.376
	(55.490)	(22.456)	(53.817)	(30.954)
Control mean (seasoned entrepreneurs only)	90.652	31.957	46.415	12.239
	(58.751)	(38.404)	(50.835)	(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) Per capita consumption (monthly)	(2) Temptation goods (monthly)	(3) All durables	(4) Non-business durables	(5) Business durables	(6) Festivals (annual)	(7) Education (monthly)	(8) Health (monthly)
Panel A: Treatment effects								
Treatment	-11.583	-19.609	559.362*	351.696	23.900***	388.891	86.911	-45.121
	(58.735)	(32.860)	(283.691)	(239.737)	(8.242)	(304.751)	(88.338)	(99.606)
Control mean	2791.712	619.036	9264.343	8482.853	88.575	8932.731	1724.557	1775.509
	(1964.732)	(971.999)	(15748.713)	(14264.700)	(351.011)	(10950.345)	(3105.393)	(4107.905)
N	5738	5739	5744	5744	5744	5737	5738	5738
Panel B: Treatment effects by entrepreneurial status								
Seasoned entrepreneur	106.282	11.260	20.574	-513.234	92.895***	512.767	307.695**	91.821
	(77.229)	(29.245)	(658.347)	(563.800)	(16.248)	(551.631)	(153.525)	(177.110)
Treatment	-48.970	-21.350	-54.105	-175.322	6.847	351.483	111.138	-77.592
	(61.965)	(37.511)	(385.449)	(323.643)	(9.196)	(372.316)	(108.004)	(122.781)
Treatment X Seasoned entrepreneur	120.166	5.530	1991.273**	1716.980**	54.254**	114.824	-82.426	104.366
	(107.356)	(48.037)	(850.072)	(725.416)	(26.063)	(663.376)	(211.525)	(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	614.207	9266.641	8635.084	60.866	8781.591	1631.834	1736.854
	(1886.292)	(986.241)	(15716.467)	(14350.628)	(293.414)	(9151.870)	(2663.565)	(4262.700)
Control mean (seasoned entrepreneurs only)	2866.309	630.105	9259.076	8133.946	152.080	9280.014	1937.360	1864.224
	(2133.184)	(939.016)	(15831.694)	(14067.978)	(450.203)	(14248.019)	(3929.819)	(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) Overall worries index	(2) Financial worries index	(3) Happiness scale	(4) Financial security scale	(5) Beaten in last month
Panel A: Treatment effects					
Treatment	-0.052*	-0.037	-0.082***	0.020	-0.002
	(0.030)	(0.031)	(0.027)	(0.053)	(0.004)
Control mean	-0.000	-0.000	0.000	3.763	0.043
	(0.588)	(0.658)	(1.000)	(1.270)	(0.204)
N	5717	5717	5716	5721	5702
Panel B: Treatment effects by entrepreneurial status					
Seasoned entrepreneur	0.039	0.045*	0.015	0.076	-0.016*
	(0.025)	(0.024)	(0.034)	(0.060)	(0.009)
Treatment	-0.061*	-0.049	-0.088***	0.009	-0.000
	(0.032)	(0.033)	(0.030)	(0.055)	(0.006)
Treatment X Seasoned entrepreneur	0.029	0.041	0.018	0.033	-0.005
	(0.036)	(0.035)	(0.055)	(0.077)	(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	-0.016	-0.005	3.745	0.049
	(0.587)	(0.654)	(1.010)	(1.285)	(0.217)
Control mean (seasoned entrepreneurs only)	0.033	0.037	0.012	3.805	0.030
	(0.591)	(0.665)	(0.977)	(1.235)	(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) Has informal loan	(2) Informal loan amount	(3) Has bank loan	(4) Bank loan amount	(5) Has loan from SHG or savings group	(6) SHG or savings group loan amount
Panel A: Treatment effects						
Treatment	-0.020	2668.157	-0.008	-2994.125	-0.003	-259.847
	(0.015)	(3545.218)	(0.006)	(2322.857)	(0.014)	(434.327)
Control mean	0.627	57151.686	0.056	11021.185	0.032	787.648
	(0.484)	(113288.950)	(0.231)	(89825.165)	(0.176)	(5497.976)
N	5744	5744	5744	5744	5744	5744
Panel B: Treatment effects by entrepreneurial status						
Old business	-0.030	3647.067	0.014	3773.799	-0.002	93.033
	(0.020)	(5833.084)	(0.010)	(3814.638)	(0.005)	(200.672)
Treatment	-0.044**	-1683.957	-0.006	-1954.600	-0.003	-285.879
	(0.018)	(4226.917)	(0.007)	(2893.642)	(0.015)	(397.298)
Treatment X Seasoned entrepreneur	0.078***	14085.007*	-0.009	-3419.550	0.002	83.402
	(0.030)	(7387.176)	(0.014)	(4740.845)	(0.008)	(262.552)
Treatment + (Treatment X Seasoned entrepreneur)	0.035	12401.050	-0.014	-5374.151	-0.001	-202.478
P(Treatment + (Treatment X Seasoned entrepreneur)!=0)	0.146	0.046	0.225	0.159	0.952	0.716
Control mean (novice entrepreneurs only)	0.635	55097.667	0.052	9923.156	0.034	789.324
	(0.482)	(115425.793)	(0.221)	(89924.523)	(0.181)	(5074.296)
Control mean (seasoned entrepreneurs only)	0.610	61859.421	0.067	13537.825	0.027	783.806
	(0.488)	(108151.800)	(0.251)	(89599.334)	(0.163)	(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) Degree (hypothetical)	(2) Financial links (hypothetical)	(3) Non-financial links (hypothetical)	(4) Degree (supported links only)	(5) Financial links (supported links only)	(6) Non-financial links (supported links only)	(7) Proportion of links that are supported	(8) Degree (non-supported links only)	(9) Financial links (non-supported links only)	(10) Non-financial links (non-supported links only)
Panel A: Treatment effects										
Treatment	-0.369***	-0.307***	-0.125	-0.323**	-0.299***	-0.125*	-0.037***	-0.045	-0.007	-0.001
	(0.134)	(0.090)	(0.091)	(0.125)	(0.101)	(0.066)	(0.014)	(0.093)	(0.067)	(0.059)
Control mean	5.948	4.372	2.926	2.755	2.286	1.360	0.402	3.191	2.084	1.565
	(3.722)	(2.603)	(2.569)	(3.127)	(2.537)	(1.944)	(0.357)	(2.699)	(1.893)	(1.751)
N	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492
Panel B: Treatment effects by entrepreneurial status										
Seasoned entrepreneur	0.195	0.145	-0.085	0.098	0.081	-0.083	-0.019	0.099	0.065	-0.002
	(0.163)	(0.106)	(0.110)	(0.128)	(0.100)	(0.081)	(0.014)	(0.111)	(0.085)	(0.065)
Treatment	-0.492***	-0.382***	-0.226**	-0.432***	-0.383***	-0.211***	-0.053***	-0.056	0.004	-0.014
	(0.136)	(0.094)	(0.094)	(0.125)	(0.101)	(0.071)	(0.015)	(0.103)	(0.076)	(0.067)
Treatment X Seasoned entrepreneur	0.394*	0.238	0.328**	0.351*	0.269	0.281**	0.052**	0.036	-0.035	0.043
	(0.228)	(0.144)	(0.165)	(0.205)	(0.164)	(0.130)	(0.024)	(0.172)	(0.133)	(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	4.328	2.971	2.727	2.256	1.392	0.407	3.174	2.070	1.578
	(3.655)	(2.522)	(2.587)	(3.030)	(2.453)	(1.948)	(0.357)	(2.741)	(1.902)	(1.778)
Control mean (seasoned entrepreneurs only)	6.051	4.473	2.821	2.820	2.354	1.286	0.391	3.229	2.118	1.535
	(3.872)	(2.778)	(2.526)	(3.342)	(2.722)	(1.936)	(0.356)	(2.599)	(1.873)	(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) Listed any MFI links	(2) Percent of links from MFI group	(3) Total MFI links	(4) Total MFI links (known before MFI group)	(5) Total MFI links (from financial links)	(6) Total MFI links (from non-financial links)
Panel A: Treatment effects						
Treatment	0.002	-0.002	-0.007	-0.008	-0.010	0.005
	(0.018)	(0.010)	(0.034)	(0.033)	(0.029)	(0.015)
Control mean	0.188	0.091	0.308	0.305	0.261	0.118
	(0.391)	(0.218)	(0.731)	(0.726)	(0.626)	(0.419)
N	5185	5185	5185	5185	5185	5185
Panel B: Treatment effects by entrepreneurial status						
Seasoned entrepreneur	0.021	0.004	0.021	0.020	0.008	0.011
	(0.016)	(0.009)	(0.031)	(0.030)	(0.027)	(0.017)
Treatment	-0.007	-0.008	-0.033	-0.036	-0.030	-0.012
	(0.019)	(0.011)	(0.037)	(0.037)	(0.031)	(0.018)
Treatment X Seasoned entrepreneur	0.028	0.017	0.084*	0.090*	0.064*	0.055**
	(0.024)	(0.013)	(0.047)	(0.047)	(0.038)	(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	0.090	0.302	0.299	0.258	0.115
	(0.385)	(0.219)	(0.732)	(0.727)	(0.632)	(0.412)
Control mean (seasoned entrepreneurs only)	0.206	0.095	0.323	0.319	0.269	0.124
	(0.404)	(0.215)	(0.730)	(0.725)	(0.613)	(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.

TABLE 12. Overlapping sample results

	(1) Workers in largest business	(2) Assets (stock)	(3) Log expenses	(4) Log revenue	(5) Profit	(6) Index of business variables
Panel A: Entered entrepreneurship pre-2006						
Treatment	0.024	391.360	0.316	0.434	2220.869**	0.071*
	(0.097)	(406.039)	(0.194)	(0.280)	(946.406)	(0.037)
Control mean	0.415	2571.425	7.287	7.660	2831.201	0.002
	(1.751)	(4803.255)	(2.848)	(3.115)	(14427.058)	(0.551)
N	1305	1184	1273	1232	1232	1305
Panel B: Entered entrepreneurship post-2006, pre-Spandana						
Treatment	0.212*	900.734	0.488	1.008*	2801.011**	0.148**
	(0.123)	(829.002)	(0.456)	(0.567)	(1293.561)	(0.066)
Control mean	0.415	2571.425	7.287	7.660	2831.201	0.002
	(1.751)	(4803.255)	(2.848)	(3.115)	(14427.058)	(0.551)
N	133	119	130	128	128	133
Panel C: Entered entrepreneurship post-2006, post-Spandana						
Treatment	-0.265	-1500.608	-0.566	-1.007	-1400.672	-0.183
	(0.264)	(1159.788)	(0.648)	(0.823)	(1286.628)	(0.112)
Control mean	0.227	2539.005	6.377	6.719	1785.719	-0.021
	(1.107)	(4850.283)	(3.402)	(3.591)	(6797.191)	(0.490)
N	164	145	158	154	154	164

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 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 -reclink- command, which conducts fuzzy matches between datasets when identifiers (in our case, links' names) may not exactly match. -Reclink- 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 -reclink-, setting a minimum matching score of 0.985 for possible matches.[41]

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 -reclink- (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.

[41]Appendix Table 15 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.

Panel A: Attrition in treatment vs. control (relative to census)				
Found in endline 3, in treatment			0.5995	
Found in endline 3, in control			0.6366	
p-value of difference			*0.229*	
Panel B: Attrition in treatment vs. control (relative to endline 1)				
Found in endline 3, in treatment			0.8231	
Found in endline 3, in control			0.8522	
p-value of difference			*0.143*	
	(1)	(2)	(3)	(4)
Panel C: Attrition, by household characteristics (measured in census)				
Treatment	0.0416	0.0348		
	(0.0302)	(0.0323)		
Spandana borrower		-0.0597***		
		(0.0224)		
Pucca house		0.0270*		
		(0.0138)		
Months in slum		-0.000796		
		(0.000675)		
Woman's occupation: business		-0.0351		
		(0.0243)		
Woman's occupation: salary		0.00777		
		(0.0205)		
Husband's occupation: business		0.0241		
		(0.0188)		
Husband's occupation: salary		0.00868		
		(0.0153)		
First Spandana loan date			-8.02e-05	
			(8.66e-05)	
10th pct. Spandana loan date				-0.000323
				(0.000231)
Constant	0.357***	0.363***	1.760	5.912
	(0.0183)	(0.0278)	(1.481)	(3.936)
Observations	7,341	7,291	3,831	3,431

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 13. Attrition

	(1) Degree	(2) Has informal loan	(3) Informal loan amt	(4) Has business	(5) Number of businesses	(6) Workers in largest business	(7) Total wages paid	(8) Assets (stock)	(9) Log expenses	(10) Log revenue	(11) Profit	(12) Index of business variables
Panel A: All Households												
Treatment effect estimate	-0.369***	-0.020	2668.157	0.038*	0.056*	0.208**	408.535***	1712.677***	0.272*	0.311*	267.161	0.069***
	(0.134)	(0.015)	(3545.218)	(0.020)	(0.031)	(0.087)	(112.457)	(454.773)	(0.140)	(0.157)	(293.866)	(0.019)
Lower Lee bound	-0.531	-0.044	-905.134	0.009	0.026	0.132	290.887	948.636	0.109	0.121	-73.082	0.043
	(0.202)	(0.026)	(4356.199)	(0.022)	(0.031)	(0.107)	(169.587)	(763.593)	(0.175)	(0.185)	(338.151)	(0.026)
Upper Lee bound	0.105	0.001	18323.169	0.061	0.110	0.339	683.682	4443.053	0.552	0.611	1582.681	0.153
	(0.272)	(0.018)	(9575.464)	(0.029)	(0.048)	(0.099)	(148.388)	(1621.154)	(0.278)	(0.304)	(570.027)	(0.046)
N	5487	5745	5745	5745	5745	5739	5737	5745	5725	5591	5582	5745
Panel B: Seasoned Entrepreneurs Only												
Treatment effect estimate	-0.242	0.042**	13645.036**	0.069***	0.101***	0.268**	716.530**	4179.711***	0.677***	0.763***	586.440	0.105***
	(0.214)	(0.020)	(5420.598)	(0.021)	(0.036)	(0.103)	(288.607)	(1227.022)	(0.199)	(0.207)	(688.068)	(0.031)
Lower Lee bound	-0.209	0.018	8876.946	0.032	0.067	0.208	552.234	2576.493	0.359	0.413	168.785	0.074
	(0.275)	(0.029)	(6986.417)	(0.030)	(0.046)	(0.165)	(375.814)	(1572.648)	(0.249)	(0.276)	(896.019)	(0.033)
Upper Lee bound	0.233	0.047	22264.317	0.066	0.142	0.415	1126.038	5754.794	0.697	0.821	2709.655	0.168
	(0.278)	(0.025)	(10170.045)	(0.029)	(0.048)	(0.155)	(343.467)	(2159.168)	(0.260)	(0.284)	(1107.880)	(0.046)
N	1702	1781	1781	1781	1781	1778	1776	1781	1771	1704	1700	1781
Panel C: Novice Entrepreneurs Only												
Treatment effect estimate	-0.432***	-0.046***	-2506.439	0.019	0.025	0.163***	293.908***	696.090	0.073	0.106	246.484	0.064***
	(0.128)	(0.017)	(4007.956)	(0.017)	(0.022)	(0.061)	(84.586)	(461.133)	(0.109)	(0.122)	(181.736)	(0.022)
Lower Lee bound	-0.675	-0.071	-5261.031	-0.001	0.004	0.150	173.489	164.488	-0.028	-0.028	-193.931	0.022
	(0.194)	(0.030)	(5056.501)	(0.019)	(0.029)	(0.105)	(153.477)	(735.447)	(0.145)	(0.161)	(296.839)	(0.031)
Upper Lee bound	0.029	-0.023	16166.697	0.056	0.089	0.281	450.740	3510.967	0.439	0.488	896.519	0.153
	(0.306)	(0.024)	(10096.237)	(0.037)	(0.048)	(0.097)	(135.146)	(1316.052)	(0.318)	(0.334)	(388.669)	(0.047)
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. Cluster bootstrapped SEs for Lee bounds in parentheses. Regressions include stratum fixed effects.

Table 14. Main Results with Lee Bounds

	Cutoff = 0.970				Cutoff = 0.975				Cutoff = 0.980				Cutoff = 0.985			
	Degree	Financial links	Non-financial links	Proportion of links supported	Degree	Financial links	Non-financial links	Proportion of links supported	Degree	Financial links	Non-financial links	Proportion of links supported	Degree	Financial links	Non-financial links	Proportion of links supported
Panel A: Treatment effects																
Treatment	-0.373***	-0.303***	-0.135	-0.038***	-0.377***	-0.308***	-0.133	-0.037***	-0.374***	-0.309***	-0.129	-0.038***	-0.369***	-0.307***	-0.125	-0.037***
	(0.133)	(0.089)	(0.091)	(0.014)	(0.134)	(0.090)	(0.091)	(0.014)	(0.134)	(0.090)	(0.091)	(0.014)	(0.134)	(0.090)	(0.091)	(0.014)
Control mean	5.917	4.361	2.930	0.404	5.934	4.370	2.932	0.403	5.950	4.378	2.934	0.403	5.948	4.372	2.926	0.402
	(3.674)	(2.589)	(2.563)	(0.357)	(3.698)	(2.595)	(2.567)	(0.357)	(3.719)	(2.603)	(2.571)	(0.357)	(3.722)	(2.603)	(2.569)	(0.357)
N	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	0.149	-0.084	-0.019	0.194	0.155	-0.086	-0.019	0.191	0.150	-0.087	-0.019	0.195	0.145	-0.085	-0.019
	(0.160)	(0.104)	(0.109)	(0.014)	(0.160)	(0.104)	(0.109)	(0.014)	(0.162)	(0.105)	(0.110)	(0.014)	(0.163)	(0.106)	(0.110)	(0.014)
Treatment	-0.497***	-0.377***	-0.238**	-0.054***	-0.495***	-0.380***	-0.235**	-0.054***	-0.497***	-0.381***	-0.232**	-0.054***	-0.492***	-0.382***	-0.226**	-0.053***
	(0.135)	(0.093)	(0.094)	(0.015)	(0.136)	(0.093)	(0.094)	(0.015)	(0.137)	(0.094)	(0.095)	(0.015)	(0.136)	(0.094)	(0.094)	(0.015)
Treatment X Seasoned entrepreneur	0.398*	0.238*	0.332**	0.053**	0.378*	0.227	0.330**	0.053**	0.396*	0.231	0.334**	0.052**	0.394*	0.238	0.328**	0.052**
	(0.223)	(0.143)	(0.163)	(0.024)	(0.224)	(0.143)	(0.163)	(0.024)	(0.227)	(0.144)	(0.165)	(0.024)	(0.228)	(0.144)	(0.165)	(0.024)
Treatment + (Treatment X Seasoned entrepreneur)	-0.099	-0.139	0.094	-0.001	-0.117	-0.153	0.095	-0.001	-0.102	-0.150	0.102	-0.002	-0.098	-0.144	0.101	-0.001
P(Treatment + (Treatment X Seasoned entrepreneur)!=0)	0.660	0.330	0.556	0.957	0.605	0.285	0.554	0.980	0.658	0.296	0.529	0.941	0.671	0.316	0.531	0.966
Control mean (novice entrepreneurs only)	5.875	4.316	2.975	0.409	5.890	4.323	2.977	0.408	5.907	4.333	2.980	0.408	5.903	4.328	2.971	0.407
	(3.597)	(2.502)	(2.577)	(0.357)	(3.625)	(2.506)	(2.583)	(0.357)	(3.647)	(2.517)	(2.587)	(0.357)	(3.655)	(2.522)	(2.587)	(0.357)
Control mean (seasoned entrepreneurs only)	6.012	4.465	2.826	0.393	6.036	4.478	2.826	0.392	6.049	4.482	2.827	0.392	6.051	4.473	2.821	0.391
	(3.846)	(2.777)	(2.530)	(0.357)	(3.861)	(2.786)	(2.530)	(0.356)	(3.879)	(2.790)	(2.534)	(0.356)	(3.872)	(2.778)	(2.526)	(0.356)
N	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492

	Cutoff = 0.990				Cutoff = 0.995				Cutoff = 1.000			
	Degree	Financial links	Non-financial links	Proportion of links supported	Degree	Financial links	Non-financial links	Proportion of links supported	Degree	Financial links	Non-financial links	Proportion of links supported
Panel A: Treatment effects												
Treatment	-0.380***	-0.316***	-0.130	-0.037***	-0.387***	-0.324***	-0.127	-0.037***	-0.383***	-0.322***	-0.126	-0.037***
	(0.135)	(0.090)	(0.092)	(0.013)	(0.135)	(0.090)	(0.091)	(0.013)	(0.136)	(0.090)	(0.092)	(0.014)
Control mean	5.991	4.400	2.941	0.402	6.039	4.427	2.947	0.401	6.071	4.445	2.952	0.400
	(3.756)	(2.618)	(2.579)	(0.356)	(3.788)	(2.639)	(2.588)	(0.355)	(3.819)	(2.657)	(2.597)	(0.354)
N	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492
Panel B: Treatment effects by entrepreneurial status												
Old business	0.189	0.145	-0.083	-0.018	0.165	0.135	-0.085	-0.017	0.159	0.135	-0.087	-0.016
	(0.163)	(0.106)	(0.110)	(0.014)	(0.164)	(0.106)	(0.111)	(0.014)	(0.164)	(0.107)	(0.111)	(0.014)
Treatment	-0.502***	-0.389***	-0.230**	-0.053***	-0.517***	-0.399***	-0.230**	-0.053***	-0.520***	-0.400***	-0.229**	-0.052***
	(0.138)	(0.094)	(0.095)	(0.015)	(0.137)	(0.093)	(0.095)	(0.015)	(0.137)	(0.094)	(0.095)	(0.015)
Treatment X Seasoned entrepreneur	0.391*	0.231	0.327*	0.052**	0.416*	0.240	0.333**	0.051**	0.439*	0.249*	0.335**	0.049**
	(0.229)	(0.144)	(0.165)	(0.024)	(0.231)	(0.146)	(0.166)	(0.024)	(0.230)	(0.146)	(0.166)	(0.024)
Treatment + (Treatment X Seasoned entrepreneur)	-0.111	-0.158	0.096	-0.002	-0.101	-0.159	0.103	-0.002	-0.081	-0.152	0.106	-0.003
P(Treatment + (Treatment X Seasoned entrepreneur)!=0)	0.631	0.274	0.554	0.936	0.665	0.273	0.526	0.923	0.728	0.297	0.517	0.896
Control mean (novice entrepreneurs only)	5.949	4.356	2.986	0.406	6.004	4.386	2.992	0.405	6.038	4.405	2.998	0.404
	(3.686)	(2.532)	(2.592)	(0.356)	(3.722)	(2.550)	(2.601)	(0.355)	(3.753)	(2.566)	(2.610)	(0.354)
Control mean (seasoned entrepreneurs only)	6.089	4.501	2.837	0.391	6.121	4.522	2.842	0.391	6.148	4.539	2.846	0.390
	(3.911)	(2.805)	(2.547)	(0.356)	(3.937)	(2.832)	(2.557)	(0.356)	(3.966)	(2.857)	(2.564)	(0.355)
N	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492	5492

TABLE 15. Social Networks: Alternative Cutoffs for Matching