Microcredit Impacts: Evidence from a Large-scale Observational Study in Brazil[1]

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This version: July 2018

Abstract

This paper studies the impact of microcredit in Brazil. We use a propensity score matching approach on original primary data on business and personal outcomes to compare veteran clients of BNDES – Brazil's largest government-owned development bank – to a matched sample of new clients. By using administrative data as well as data from a survey of 2,107 clients, this large-scale study allows us to compare impacts in the poorer Northeast region of Brazil with those in the richer Southern region of the country. The findings show no significant impacts on income, employment generation, access to credit, and business formalization. We find that veteran clients in municipalities with a low Human Development Index have higher sales and profits than matched new clients in the same municipalities. This effect is, however, only significant in the Northeast of Brazil. We further find that female microentrepreneurs in the Northeast earn less, create less employment and have more restricted access to credit than their male counterparts. Our results confirm the findings of many existing impact evaluations that conclude that microcredit does not seem to transform poor peoples' lives and calls for specific public policies for the economic empowerment of women.

JEL Classification: O12, O16, G21
Keywords: BNDES, Brazil, impact study, microcredit, propensity score matching.

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[1] We thank Martin Valdivia, Lauro Gonzalez and Eduardo Ferreira for helpful comments and suggestions. We also thank Renata Carneiro for excellent research assistance. We further thank BNDES' Department of Evaluation and Effectiveness Promotion for discussions and for making the data available.

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1 Introduction

Existing studies of the impact of microcredit have mostly failed to show that microcredit has a transformative effect on poor peoples' lives, in particular with regards to income generation, consumption, and employment generation. Most of these studies have evaluated microcredit impacts by means of randomized controlled trials (RCTs), usually in cooperation with a local lender and for a relatively small region of the country that the lender is active in (e.g. Karlan and Zinman, 2011; Angelucci et al., 2013; Attanasio et al., 2015; Crépon et al., 2015; Banerjee et al., 2015; Augsburg et al., 2015; Tarozzi et al., 2015).[2] This paper reports microcredit impacts estimated by means of a large-scale observational study in Brazil, covering the relatively poorer Northeast of the country and the richer South.

Our study is the first evaluation of the impact of microcredit in Brazil using primary data.[3] Two features set the present study apart from others. First, we measure microcredit impacts for 16 different microfinance institutions (MFIs), while most studies typically focused solely on a single MFI. Furthermore, our study is able to gauge microcredit impacts for two vast macro-regions in the country, instead of a single small region of the country as in most microcredit impact evaluations. This should increase the external validity of our results.

Differently from recent impact evaluations of microcredit, we do not conduct an RCT. An RCT was not feasible because our study is national in scope and the microcredit program we examine is implemented through a large number of different independent MFIs. Instead, we compare old (or veteran) recipients of microcredit to new recipients. We are aware of the empirical drawbacks that our approach entails, and in particular the problems of self-selection and attrition that may hinder any causal inference (see Karlan, 2001, for a detailed description of the problems that may result from our approach). We try to circumvent these problems by matching veteran to new clients on several dimensions.[4] Veteran and new beneficiaries were matched exactly on municipality, MFI that extended the loan, economic sector (agriculture, industry and services), type of loan (individual or group contract), as well as age bracket, sex, marital status, and credit score. We combined the matching procedure with OLS regressions (following Ho et al., 2007) to measure the difference in the outcomes between veteran and new clients as the microcredit impacts. While this may not fully eliminate the problems of self-selection and attrition, it should reduce them significantly.

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[2] A notable exception is the study by Bruhn and Love (2014) who find positive effects of introducing a microfinance-like product on income and labor market outcomes.

[3] A study by Neri (2008) uses only administrative data.

[4] Floro and Swain (2012) and Cintina and Love (2017) also use propensity score matching to evaluate the impact of microfinance on the individual level.

The primary data collection for the impact evaluation occurred between September 2016 and January 2017, in cooperation with BNDES, Brazil's largest development bank. BNDES is owned fully by the Brazilian government and at the time the study was conducted had an active microcredit portfolio of around 230 million US dollars. BNDES does not grant loans directly to microentrepreneurs as it does not operate a branch network; it provides funds to MFIs which channel the funds to the clients according to their internal standards and operations.

We collected administrative data from 38 MFIs that received funds from BNDES – which amounts to 84% of all MFIs using BNDES funds in 2016 – and identified approximately 10,700 potential survey participants.[5] We then applied a one-time in-depth survey to 2,107 clients of the MFIs covering several important dimensions for impact evaluation: income generation, access to credit, employment generation, business formalization, subjective wellbeing, access to services, and access to consumption goods. Within each family of outcomes, we used several dependent variables to evaluate microcredit impacts in Brazil. We estimated and present microcredit impacts separately for the Northeast of Brazil and for the South. The reason is that while both areas are poor by Western standards, the Northeast of Brazil is much poorer than the South and is also substantively different in several other dimensions (e.g. culture, infrastructure), hence, mixing individuals from both regions in the impact evaluation would deliver a distorted picture of the effects.[6] We also tested for heterogeneous treatment effects within each macro-region comparing municipalities with different levels of human development. The final sample consisted of 1,601 survey participants, 1,070 from the Northeast and 531 from the South.

Our results are in line with most of the existing microcredit impact studies, as we did not find transformative effects of microcredit across a large family of impact dimensions and outcome variables. Individuals in the treatment group, on average, do not seem to have done better than individuals in the control group. The only treatment effect we find is among businesses located in areas with lower levels of human development in the Northeast of the country. In this subset of the participants, old beneficiaries have higher sales and less months in which the revenues were lower than costs compared to the control group. This should lead to higher profits for these businesses, but the coefficient in the profit regression, while being relatively large economically, it is not significant.

[5] The final sample includes clients from less than 38 MFIs because some of the MFIs only had very few clients that received loans disbursed from BNDES funds.
[6] We did not include individuals from other regions in Brazil because there are very few MFIs that use BNDES funds for their microcredit activities.

We do, however, find a number of interesting patterns in the data that have potentially important policy implications. The most robust of these patterns is that women have lower income than men, create less jobs (albeit this effect being economically small), and have less access to credit. These regularities hold, however, only in the Northeast and not in the South of Brazil, which might be an indication of gender discrimination in the economically less developed Northeast. Another interesting pattern we observe is that formalized businesses have higher revenues and income, but this may very well be a result of reverse causality. The data also show that businesses in municipalities with lower levels of development in the Northeast have higher credit scores than the businesses in municipalities with higher levels. This might be an indication of different client targeting practices by MFIs. In relatively richer municipalities, MFIs give loans also to riskier clients, while in the poorer municipalities they only target clients with a good credit track record. Overall, our results call for more or more efficient women empowerment, for instance through specific public policies targeted at women, in particular in the poorest areas of the country.

The remainder of the paper is organized as follows. Section 2 contains information about the institutional background of the impact evaluation and details about the study design. Section 3 presents results for the different impact families as well as heterogeneous treatment effects. Section 4 concludes.

2 Institutional background

Microcredit operations began in Brazil in 1973, in the northeastern part of the country, under the auspices of local lenders supported by international organizations. During the 1980s, nonprofit institutions expanded the supply of microcredit. In the late 1990s, the Brazilian government promoted the growth of microcredit mostly through the establishment of programs in state level development banks. *Crediamigo*, the current dominant player in the microcredit industry, came into existence in this period as an MFI related to the Bank of the Northeast of Brazil (BND, a public development bank).

The supply of microcredit was bolstered by a 2003 federal law that required banks to channel 2% of their cash deposits to microcredit operations in support of productive activities or consumption. If the banks did not use funds for these purpose, they would be retained at the Central Bank, which reduced the opportunity costs for banks to establish microcredit operations. Soon thereafter, in 2005, the Federal Government created the National Program of Oriented Productive Microcredit (PNMPO – *Programa Nacional de Microcrédito Produtivo Orientado*), which steered microcredit funds primarily towards productive activities as opposed to consumption, and sought to integrate microcredit into a broader set of development policies geared towards formal and informal microentrepreneurs.

Another significant policy milestone was the launch of the Growth Program (*Programa Crescer)* in 2011, aimed at improving the interest rate conditions for microcredit loans through subsidies to seven federal public banks. Between 2011 and 2014, the *Programa Crescer* generated 10 million low interest loans of up to BRL 15,000 (USD 4,285[7]) to businesses with maximum annual incomes of BRL 120,000 (USD 34,285). It is important to point out that the distribution of microcredit loans is very imbalanced in Brazil. In 2014, for example, most microcredit operations occurred in the Northeast (55%), followed by the Southeast (20%), the South (18%), the Central-West (4%) and the North (3%) of Brazil. Most microcredit transactions were conducted by licensed banks (mostly development banks), which accounted for 92% of the total transaction volume. Although the federal government´s *Programa Crescer* was suspended because of the economic recession of 2015, the portfolio of microcredit operations reached 4.8 billion Reais (1.37 billion US dollars) in 2016.

The Brazilian Development Bank (henceforth BNDES – *Banco Nacional de Desenvolvimento Econômico e Social*) initially entered the microcredit market in 1996. At that time, the bank funded MFIs and in the following years established a technical cooperation agreement with the Interamerican Development Bank. BNDES's microcredit activities expanded in 2005 when a temporary program was created (*Programa de Microcrédito do BNDES*) and later transformed in 2014 into a permanent line of credit (*Produto BNDES Microcrédito*). This product does not directly fund microentrepreneurs, but instead grants loans to MFIs (such as development or commercial banks, credit cooperatives, local development agencies and non-governmental agencies). These MFIs offer microloans according to the PNMPO guidelines, which encompass loans for small entrepreneurs with annual income of up to BRL 120,000 (USD 34,285) channeled through MFIs that have loan officers in direct contact with the entrepreneurs.

BNDES started to operate with funds of 250 million Reais (71.4 million US dollars) and reached one billion Reais (286 million US dollars) in 2013. Among BNDES' active clients in 2016 there were 45 MFIs. Most of BNDES funded loans are disbursed in the Northeast of Brazil (77%) where *Crediamigo* is the main client (90% of Northeast contracts), followed by the South of Brazil (15%), where the loans are distributed in between credit cooperatives (26%), commercial banks (14%) and local development agencies (60%). MFIs in the Southeast and

[7] We convert Reais to US dollars using an exchange rate of 3.50/1.00 throughout the paper.

Central-West regions only account for a minor share of all the funds that BNDES distributes. This is the major reason why beneficiaries from those regions are not included in our sample.

3 Study design and data

Our goal is to assess the impact of BNDES targeted productive microcredit program *Produto BNDES Microcrédito* (henceforth PBM) on business and household indicators. PBM represents reasonably well the microcredit activity in Brazil in terms of its regional distribution as well as the types of MFIs in the market. Thus, our findings provide some valuable information in broader terms about microcredit in Brazil as opposed to the impact of microcredit in a particular region, as is usually the case in evaluations of microcredit impacts.

3.1.1 Design of the impact evaluation

While RCTs are the "gold standard" for impact evaluations, they are not feasible in every context. In addition to the usual obstacles that have been experienced in similar settings (Khandker et al., 2010), two particular features of our case made it impossible to randomize the treatment.

First, PBM was national in scope and implemented through a large number of different independent MFIs, most of them not used to impact evaluations and alien to the idea of randomization. This setup would have made it daunting to reach an agreement with all MFIs to randomize credit decisions and to adjust their routines accordingly.

Second, unlike some countries in which microcredit impact has been evaluated before, Brazil has a quite active microcredit market, with more than one MFI present in most localities. Even if randomization were feasible, clients assigned to the control group that would have the credit request denied by our partner MFI could seek and obtain a loan from another MFI in the short-run.

Non-randomized evaluations, such as ours, are not without merits. Non-experimental designs are often quicker and cheaper to carry out than RCTs (Shah et al., 2015). This facilitates the replication of policy evaluations in a larger number of countries allowing researchers to explore context-dependent heterogeneous treatment results (Pritchett and Sandefur, 2015; Vivalt, 2015). Furthermore, RCTs are typically conducted in small scale and their findings may not adequately represent the observed results when the program is implemented in larger scale due to general equilibrium effects (Breza and Kinnan, 2018). Additionally, the local organizations that participate in an early experiment are often different from the rest of the population (e.g. more organized) and this site-selection bias may affect the estimated impacts

(Banerjee et al., 2017). Finally, in many cases, as in ours, they are the only feasible option to implement any type of evaluation due to operational constraints.

Given these constraints, we implemented a comparison between early and late loans. The "treatment" in our study, therefore really means receiving a loan "earlier" as opposed to "later". In this paper, we refer interchangeably to new clients and late loans, which will compose the "control" group, and to early loans and old/veteran clients, who are the "treatment" group. This approach allowed us to observe microcredit effects in a program implemented in a large nation-wide scale and in a wide variety of MFIs, obtaining treatment estimates under general equilibrium and avoiding site-selection bias.

Two obvious threats to the validity of this comparison come from selection and attrition (Karlan, 2001). Attrition is particularly a problem in our design as it is harder to locate early beneficiaries than late beneficiaries. We dealt with this potential problem by making an effort to supplement the provider's information on beneficiaries using data purchased from credit bureaus. This way, even if a beneficiary interrupted her relationship with the microcredit provider, we were usually able to update contact information.

With respect to selection, all individuals in our study have eventually applied for and received a microfinance loan. By definition, this implies that any self-selection process that might be at work is necessarily weaker than what would be obtained in a comparison between other types of non-randomly defined groups of beneficiaries and non-beneficiaries of a microloan (Coleman, 2006). It is still possible that early beneficiaries are qualitatively different from late-beneficiaries due to some form of residual self-selection or even changes in the selection criteria employed by the microfinance agents. We sought to minimize this problem by matching early and late beneficiaries on many important pretreatment observed characteristics. As we discuss below, variants of matching were employed both at the sample design phase and at the analysis phase.

A third threat to inference comes from the fact that the operational difficulties in obtaining and standardizing the administrative data from all MFIs forced us to field the survey a few months after the recent beneficiaries had been awarded loans. This happened because not all MFIs complied with the data request, therefore delaying the survey. The consequence is that by the time in which we fielded the survey, on average, eight months had passed since the granting of the loans to late-beneficiaries. The difference between the control and treated groups, therefore, consists of having either received a first loan 20 months before, or eight months before the interview.

3.1.2. Sample Construction

We obtained the administrative data after an exploratory study of PBM, in which we interviewed local microfinance operators and requested the necessary beneficiary-level data that formed the population of individuals. We were authorized to request data about all microcredit loans that were funded totally or partially by PBM, since its roll out in 2014. For each such loan, we obtain loan specific information (amount, interest rate, number of installments, if first loan or not), client specific information (such as age, sex and contact information), as well as business-specific information (such as sector of activity and revenue, when available). We identified 45 MFIs participating in PBM, 38 of which complied with the data information request in time to be included in the study (encompassing more than 90% of PBM´s portfolio). We consolidated the data received from the providers to create a database of the population of beneficiaries of PBM.

The total of 123,977 first time microcredit loans granted by MFIs funded by BNDES was initially pruned by limiting eligible participants to those defined as veteran beneficiaries (who obtained their first loan from PBM in the first quarter of 2016) and new beneficiaries (those that obtained their first loan in the first quarter of 2015).[8] This left us with 32,494 loans. We then matched early beneficiaries to late beneficiaries to determine the subset of the population that was eligible to be included in the study. Old and new beneficiaries were matched exactly on municipality, MFI, economic sector (agriculture, industry and services), type (individual or group loan), as well as age bracket, sex, marital status and month in which the loan was obtained (January, February or March). These were very demanding matches, as they implied that early beneficiaries would only be compared to late beneficiaries within the same (typically small) municipality and only with other clients of the same MFI.

This procedure generated a matched subset of 24,906 loans. Due to operational concerns, we further limited this subset to municipalities in which there were at least 30 early and 30 late beneficiaries (in the Northeast) and 15 early and 15 late beneficiaries (in the South), which generated a sampling subset of 10,733 loans (or 43% of the matched subset). The sampling's subset geographic dispersion reflects that of the matched subset. Most loans in the Northeast are disbursed through *Crediamigo*, while in the South they are dispersed across a number of providers and tend to be of larger values.

From the sampling subset, we first randomly selected 64 municipalities, ensuring that the two macro-regions of the country that receive more than 90% of the funds from BNDES for

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[8] The number of beneficiaries in 2014 and previous years was too small to serve as the baseline.

microcredit and all levels of development, were represented. We stratified the municipalities in the sample by level of development (measured by the Human Development Index of each municipality – HDI-M) within each geographic region. Thus, we assured a balanced sample of the poorer and richer municipalities within the South and Northeast regions. We then acquired credit scores at the time of the loan take-up and six months after for all individuals in the subset, and further refined the matches of early to late beneficiaries using this information.[9]

The credit scores were related to the individual entrepreneur and not to the business as most of the financial transactions of these businesses are performed with the entrepreneur´s individual tax identification number (known as the CPF). Furthermore, this allowed us to match formal and informal entrepreneurs alike. We obtained the credit scores from one of Brazil´s largest credit bureaus so they were available regardless of their use by the MFIs in the credit decision process. More than 98% of the entrepreneurs had enough information in the bureau to have a credit score.[10] This was a fundamental measure for matching as it is objective, could be retrieved retrospectively (i.e. we could assess credit worthiness at the moment immediately before the loan was granted), and was also obtainable for the veteran clients. After the inclusion of the credit score we performed a Mahanalobis-distance matching and paired each veteran client to its nearest neighbor without replacement. We then sampled observations from these treatment-control pairs. A final sample of 3,223 loans was selected from this refined matched sampling subset. This number included possible replacements to achieve an expected number of 2,800 interviews.

We were able to complete 2,107 interviews because 854 businesses were not located, 243 refused to answer and 19 responses were invalidated by quality control concerns. The sample analyzed was further reduced to 1,602 after we excluded individuals who reported having previously received a microcredit loan from some other MFI, and those who reported never having had a business.[11] The final sample included loans from 16 different MFIs. In the appendix (Table A1) we provide balance statistics for the early and late sample groups. All differences were smaller than 0.2 standard deviations (Cochran, 1968), which is remarkable given that with this sample size we had more than 80% power to identify a difference of 0.25 standard deviations between treated and control groups.

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[9] It was prohibitively costly to obtain credit scores for the full population of cases.

[10] In Brazil, credit bureaus register data from a broad array of events such as payment of service bills (e.g. electricity) or installments of direct financing by shops, as well as any request for credit. Thus, even if the entrepreneur had no contact with financial institutions, she could have a credit score.

[11] It was not possible to eliminate these individuals before the survey, as we only possessed administrative data from the MFIs that work with BNDES.

3.1.3. Descriptive statistics

Table 1 displays descriptive statistics of the sample, separately for the Northeast and the South. The tables shows several notable differences between regions. Beneficiaries in the South are on average 5 years older than those from the Northeast. Furthermore, microcredit beneficiaries from the South are more often male, more often married, and somewhat better educated on average. They run older businesses, mainly in the service sector, while most microbusinesses in the Northeast are from the commerce sector. Beneficiaries from the South also have higher credit scores, which should translate into less risky loans. The most notable difference is the formalization status: 61% of all microbusinesses in the South were formalized before receiving the first microloan, most often in the form of an individual microentrepreneur (known as "MEI", for *Microempreendedor Individual*), while the number in the Northeast is a mere 7%, indicating that 93% of all beneficiaries in the Northeast owned informal businesses when they received their first microloan.

The table also displays notable differences between the regions for the outcome variables. Beneficiaries in the South are much richer than their counterparts in the Northeast and they also run larger businesses. This reflects the differences in the degree of development between the South and the Northeast and underlines why we present results separately for both regions. Finally, beneficiaries from the South also have a much better access to credit than those in the Northeast, which may be explained by their better education, the higher share of formalized businesses and their higher wealth.

3.1.4. Main specification

To eschew the potential effects of differential attrition between veteran and new clients[12], for the main analysis we conducted a second round of matching. Just as in the sample construction phase, we matched early loan survey respondents (treatment group) to late loan survey respondents (control group), requiring exact matches for municipality and microcredit provider. As the survey generated much more finely grained information on each interviewee, we were able to expand the set of variables to match on to include age, sex, educational level, marital status, sector of activity, credit score prior to the loan, age of business, month of loan – between January and March – and pre-loan formalization status.

However, due to the much smaller number of individuals available for matching, instead of exact matching, we performed nearest neighbor matching with replacement and up to three-

[12] In Table A2 we provide the results of logistic regressions for the Northeast and the South whereby we regress survey participation on important drivers of participation. The results suggest that attrition is not a major problem.

to-one control-to-treatment ratio on the other matching variables. As expected, given that the sample had already been drawn from a matched subset of all loans the propensity scores are distributed very similarly across treated and control loans (see Figure 1).

We estimated the effect of the treatment (defined as early vis-à-vis late loan) on each outcome of interest by estimating regressions of the sort:

$$Y_i = \beta_0 + \beta_1 Treat_i + \boldsymbol{\delta} X_i + \theta_i + \pi + \omega + \varepsilon_i$$

Where Y_i is the outcome of interest for individual i and Treat is an indicator that takes on the value of one if individuals were early beneficiaries and zero otherwise. The estimate of the treatment effect is the coefficient β_1, which captures the difference in means between the treatment and control groups, conditional on the other variables. X is a vector of individual level control variables common to all models. It includes an indicator for sex, individual's age and age of business (both measured at the time of the loan), marital status, the individual's credit score one month before the first loan, and a dummy indicator that takes on the value of one if the business was formalized before taking the loan. We include as covariates all the variables used in the matching to increase power. The specification also includes sector of activity (θ), municipality-MFI fixed effects (π), and interviewer fixed effects (ω). All models were estimated by OLS with the weights generated by the matching procedure, as recommended in Ho et al. (2007). In models with formalization as the dependent variable, those already formalized before the first microcredit loan were excluded from analysis (and so was the control variable related to pre-treatment formalization).

4 Results

We present and discuss separately the findings for the Northeast and for the South of Brazil. As both macro-regions are very different in terms of poverty levels, economic development, infrastructure, among many other aspects, we believe that presenting the results separately for both regions and comparing them will deliver a clearer picture of microcredit impacts in Brazil. We discuss the results for the four impact dimensions income, employment generation, access to credit and business formalization first. Then we present the results for all other impact dimensions and outcomes.

4.1. Results for income

The most important impact dimension is, arguably, income generation. The fundamental idea of microcredit was to give poor entrepreneurs small business loans so that they can make investments and grow their businesses. Eventually, this should result in higher incomes and improved living conditions. Yet, most impact evaluations did not find strong effects of microcredit on poor peoples' income or business profits (e.g. Karlan and Zinman, 2011; Angelucci et al., 2013; Crépon et al., 2015). One of the difficulties in impact evaluations of microcredit is income measurement as this relies on self-reporting of the microcredit beneficiaries (de Mel et al., 2009). For instance, in our case we computed business income in two ways. First, we asked the beneficiaries to self-report their monthly profit in the last month.[13] We then asked them about their revenues and their costs in the last month and computed the difference as profits. In the vast majority of cases, both profit figures were different.

We report the results using the computed profit figure in the regressions, but the findings hold considering the self-reported profit as well. Further to business profits, we use revenues in the last month, total household income, income from other sources than the business, and months in the last six months in which the revenues were below the costs as outcome variables. Finally, we also use a variable that combines all five outcomes into an index as sometimes individual outcomes are not significant because of low statistical power (Kling et al, 2007). The index was computed as the factor score from a one-dimensional factor analysis. All regressions are estimated using OLS and controls and fixed effects were included as indicated in the table. Results for the Northeast are displayed in Panel A of Table 2, and for the South in Panel B.

For the Northeast, the treatment dummy is not significant for any of the outcomes of interest and similar results are observed in the South. These results suggest that treated individuals did not do better than control individuals with regard to income generation.

One interesting result is that female business owners earn significantly less than male business owners, independent of the treatment in the Northeast. The effect is highly significant and economically large. For instance, the average male business owner has sales of BRL 2,174 (USD 621) and female owners of BRL 1,390 (USD 397). Even though the coefficient sign and size is similar in the South, it is not significant. One explanation for this result is that discrimination against women may be more prevalent in the poorer Northeast than in the South. Additionally, education seems to play a relevant role in the Northeast with positive and

[13] We only asked for business profits, expenses and sales in the last month because we believe it may be too difficult to remember business profits for months further in the past as poor business owners usually do not have any written records.

significant coefficients for the variables representing primary and high school (compared to the no formal degree reference category) in most observed outcomes. Surprisingly, there is no effect of education in the South, except for a significant effect of high school degree (vs. no formal education) on sales.

Finally, owners of formalized businesses have higher sales and bigger business profits, both in the Northeast and the South. This result has to be interpreted with caution, though, as it could be a consequence of reverse causality whereby more profitable business owners were more likely to formalize their businesses.

4.2. Results for employment generation

The second impact category we investigate is employment generation. While we did not find any direct impact of microcredit on poor peoples' income, it might be that they create opportunities for others by creating new jobs. It could also be that by creating jobs, there might be a reduction in the number of family members working in the business which, for instance, might increase schooling. To exploit this possibility, we measure impact across four outcomes: workers from the own household (i.e. family members), workers outside the own household, the number of businesses without workers from the own family, and the number of businesses with non-family member workers. As an aggregated index we used the total number of employees adding those living in and out of the household. The results for the Northeast are displayed in Panel A of Table 3 and for the South in Panel B.

We do not find any significant results in either macro-region for any of the outcomes. Coefficients are not significant and economically very small. The results in Panel A show that female business owners in the Northeast seem to generate less employment as they employ less non-family member workers and run more businesses without any workers, regardless of whether they are family or non-family members. We do not see the same effect for female business owners from the South, in which female-owned businesses in fact have more employees in the household. On the other hand, business owners from the South that have a formal business seem to have more workers both from within and outside the own family. As in the case of income generation, we are cautious in interpreting this result as it could be subject to reverse causality. Overall, treated individuals did not fare any different from control individuals. Educational level of the business owner is positively related to having more employees out of the household, but not in households in the Northeast. In the South formalization is the variable more strongly correlated to having employees in and out of the household.

4.3 Results for access to credit

To measure the impact of microcredit on access to credit, we created three different outcomes as well as an overall index based on nine indicators of access to credit by credit source (e.g. bank, credit cooperative). Besides asking the survey participants about the number of sources of credit they think they have access to (Table 4, column 1), we also asked them whether they had any loan application rejected in the last six months (column 2) and whether they think they have no access to any source of credit at all (column 3). Finally, in column (4), we report the results for the index that combines the answer to three questions.

As before, we do not find any treatment effects for individuals from the Northeast and for the South. Some baseline patterns, however, are quite interesting. In the Northeast (Panel A) female microentrepreneurs seem to have access to fewer sources of finance, and the number of sources of credit increases with education. In both cases, results are significant for the number of sources of credit, the absence of any source of credit, and for the combined index. The results for gender are in line with the effects we documented before: female business owners in the Northeast earn less, they create less jobs and they have more difficult access to credit. These may be the result of severe gender discrimination in the Northeast and calls for specific public policies targeted at women empowerment, in particular in the very poor regions of the country. The fact that education has no effect in the South might reflect a higher education premium linked to lower overall levels of education in the Northeast.

Formalization (observed ex-ante) is another baseline pattern that seems to have a consistent effect, and in this case the effect is present not only in the Northeast but also and particularly in the South. It is probably the case that formal businesses are also better structured and business owners more prepared to successfully seek out credit, but in this case we suppose that formalization serves more as signal of creditworthiness than an actual cause of it. Given this result and the fact that the Federal Government has sponsored since 2009 the "Individual Microentrepreneur Program", which is a major policy initiative to formalize microentrepreneurs, more research is needed on the direct effects of formalization on business performance and access to credit (Lenz and Valdivia, 2017).

4.4. Results for business formalization

The fourth family of outcomes we investigate is whether treated beneficiaries formalized their businesses as a consequence of taking out a microloan. Differently than in the previous analysis, here we examine only individuals that were informal before the loan to determine whether they were more likely than others to formalize. One striking result of the survey was that around

90% of survey participants in the Northeast were running informal businesses while this figure is reduced to 24% in the South. Formalizing a business (in Brazil and elsewhere) can have positive effects for the business owners. For instance, they can hire workers that are looking for a formal job, it makes it easier to apply for a loan from the formal financial sector, and the business owner is granted the right to get access to the public pension system. Of course, there are also negative aspects such as formal workers are more expensive, formal businesses have to pay taxes and are subject to more bureaucracy than informal businesses.[14] It could very well be that these disadvantages outweigh the positive aspects of formalizing a business. In fact, many of the owners of informal businesses replied that they would not know why they should formalize their business when asked during the survey.

While the majority of the surveyed businesses are informal businesses, our interest was whether there were significantly more formal businesses among the treated business owners. We used three outcomes to analyze this question.[15] Column (1) of Table 5 shows whether treated individuals have more often formalized businesses, column (2) shows whether there are more businesses formalized as individual microbusinesses (MEI) among treated business owners (this information was obtained from secondary sources, alongside formalization date), and column (3) shows the intention to formalize the business.[16] The results are displayed again separately for the Northeast in Panel A and for the South in Panel B. As before, none of the treatment effects are significant, but some baseline patterns in the Northeast (Panel A) are worth noting. Female microentrepreneurs, once again, underperform relative to their male counterparts, with fewer formalizing and even, though not significantly, intending to formalize. This result reinforces the previous interpretation of gender discrimination in the region. Education seems to have a strong effect on intention to formalize, but its effects with respect to actual formalization are only observable for the highest education group.

Summing up our findings so far, we did not find any evidence that treated individuals, i.e. owners of microbusinesses that took out a microloan about one year before than those in the control group, did any better across the four important impact dimensions income generation, employment generation, access to credit, and business formalization and across altogether 18 outcomes. This stands in sharp contrast to the idea of microcredit and to what

[14] The simplest way to formalize a business in Brazil is to register it as a MEI, which stands for individual microentrepreneur. The annual revenues of these businesses must not exceed BRL 60,000 (USD 17,143) and they have to pay a monthly flat tax of BRL 55 (USD 16). While the monthly tax payment is a trivial amount by Western standards, it may still be prohibitively high for many microbusinesses.

[15] In these regressions we do not control for whether a business is formalized or not unlike in previous regressions.

[16] We do not use an aggregated index for this outcome category because it is difficult to interpret such an index economically.

most practitioners (and many academics) believe. While we cannot rule out that what we measure is not a causal relationship due to the biases that may be associated with our empirical approach, it is rather unlikely that any resulting bias from our approach should bias the treatment effects downwards. Hence, within the limits of our empirical approach we have to state that microcredit in Brazil does not deliver the desired outcomes.

4.5. Other results

Besides the four main impact dimensions described and discussed above, we measured treatment effects across the three additional impact dimensions: access to consumption goods (internet access besides cellphone internet access, ownership of a washing machine, ownership of a car, ownership of a computer, index across all four), access to services (whether beneficiary saved any money in the last six months, whether she had a health insurance in the last six months, whether she paid for any cleaning services in the last six months, index across the three), and subjective wellbeing, based on questions that inquired whether the beneficiary felt happy, calm, worried, sad one day before the interview (following Kahneman and Deaton, 2010) and an aggregate index of the four questions.

We found null results for all items in the first two categories (results omitted),[17] but we found a negative effect of the treatment on subjective emotional wellbeing (Table 6). This result showed up as a statistically significant reduction in the share of respondents in the Northeast that reported feeling happy. We also found noticeable effects for the three other emotions, none of which was statistically significant, but all of which were in the "negative" direction. Together, these results imply a significant reduction in the overall subjective wellbeing index. Results in the South were very similar, but noisier. Although somewhat surprising, this result is compatible with the null findings on the business performance variables. If subjects' shared our expectations that access to microcredit would improve their business' performance, and if these expectations were frustrated, it would make sense that this would show up in a negative emotional wellbeing.

In addition to this treatment effect, a few additional baseline results can also been seen in the Northeast with respect to participants' subjective wellbeing. Education, for instance, is associated with higher wellbeing, particularly for respondents in the highest education bracket. More conspicuously, the negative baseline result for gender in the Northeast is present once again. Given the previous results we reported, this is not really surprising.

[17] They are available from the authors on request.

4.6. Heterogeneous treatment effects by municipality Human Development Index

We analyze, here, whether the wealth of the municipality the entrepreneurs live in influences the impacts of microcredit. The underlying idea is that microcredit may impact poor business owners differently if they are surrounded by predominantly poor individuals or by relatively richer individuals. For instance, it could be the case that in poorer municipalities cheaper labor is available and that it is easier to hire workers and grow the business. On the other hand, in relatively less poor areas, it may be that potential consumers have more purchasing power and it is easier for poor business owners to stimulate demand for their products after making use of the microloans by investing in the business.[18]

We measure wealth of the municipality through the HDI-M in which higher values indicate more developed (and richer) municipalities. To explore whether the wealth of the business surroundings influence the impact of microcredit, we interact the treatment dummy with an indicator for whether the municipality belongs to the lower half of the distribution of HDI-M in the sample of the region. The results for income are displayed in Table 7. Panel A contains the results for the Northeast and Panel B the results for the South of Brazil.

For two outcomes of interest, we found a significant treatment effect in the poorer half of the municipalities. Whereas the treated individuals in high development areas reported lower sales in comparison to the control group, those in the less developed areas reported higher sales. The difference in the treatment effects between the two groups of municipalities was almost BRL 600 (USD 171). This is a large economic effect, given that the average sales for beneficiaries in the control condition was approximately BRL 1,800 (USD 514) in less developed municipalities.

Moreover, the negative and significant coefficient of the interaction term in column (4) of Panel A suggests that treated individuals from the relatively poorer municipalities experienced ¼ fewer months (in the last six) in which their revenues were below the costs. Together with the higher sales, this should result in higher profits. However, the interaction term in column (3) in Panel A, while being economically relevant, is not statistically significant. We do not see a similar pattern for beneficiaries from the South in Panel B.

Further descriptive analysis reveals that both treated beneficiaries from municipalities with HDIs above the median have on average much higher credit scores (around 25% higher) than treated individuals from municipalities with HDIs below the median. We interpret this as

[18] One may imagine a beauty salon that after making an investment becomes more attractive for customers because of an improvement of how the beauty salon looks to customers. While this may matter less for poor consumers, relatively richer consumers may find it more attractive to demand the salon's services.

different client targeting by MFIs. It seems that MFIs in relatively richer municipalities also give loans to individuals with higher credit risk (maybe because the low-risk customers have access to other sources of financing), while in the relatively poorer municipalities the MFIs predominantly target low-risk business owners. We cannot tell from our data and analyses whether this is a strategic choice of the MFIs or whether this is due to different degrees of market saturation in municipalities with high and low HDIs. However, we view this to be an interesting result that warrants further analyses as it may inform us about under which circumstances and how microcredit creates positive effects.

5 Conclusion

This paper presents results from a large-scale, observational microcredit impact evaluation in Brazil. The impact evaluation was conducted in cooperation with BNDES, Brazil's largest fully government-owned development bank. We interviewed 2,107 beneficiaries of microcredit who received their first microloan in the first quarter of 2016 or in the first quarter of 2017. We then compare differences in outcomes for 27 outcome variables across seven outcome categories between old, treated and new, control clients. We use a PSM approach to achieve comparability between treated and control beneficiaries and interpret any resulting differences between both groups as the impact of microcredit. We estimate and present treatment effects separately for the richer South and for the Northeast of Brazil.

We do not find any significant microcredit impacts for income, employment generation, access to credit, business formalization, access to goods and services and subjective wellbeing in the Northeast or in the South. As our sample resembles the overall distribution of microcredit activity in Brazil, our findings suggest that there are no general transformative impacts of microcredit in Brazil. This result is in line with most of the existing impact evaluations of Microcredit.

Further analysis shows that beneficiaries in the Northeast who live in less developed municipalities have higher sales and less months in which costs are higher than revenues, suggesting a positive impact of microcredit on business success in these regions. The data also suggest that MFIs target riskier clients in municipalities with HDIs below the sample median and that this correlates with the impact of microcredit on business success.

One consistent finding for beneficiaries from the Northeast is that female owners of microbusinesses fare much worse in several dimensions than their male counterparts. They have less income, run smaller microbusinesses and face more credit constraints. Furthermore, they more often run informal businesses and show a lower subjective wellbeing than male owners of microbusinesses. These findings may be a consequence of gender discrimination in the poorer Northeast and call for public policies that are specifically targeted at the economic empowerment of women.

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Table 1: Descriptive statistics

	Northeast		South	
	Mean	**SD**	**Mean**	**SD**
<u>Demographics</u>				
Number of people in household	3.84	1.60	3.36	1.31
Beneficiary age (years)	33.60	11.43	38.74	11.74
Female	0.68	0.47	0.57	0.50
Marital Status				
Single	0.33	0.47	0.20	0.40
Married	0.61	0.49	0.72	0.45
Widow, divorced or separated	0.06	0.24	0.08	0.26
Education				
No formal degree	0.08	0.27	0.05	0.23
Primary school	0.43	0.50	0.41	0.49
High school or more	0.49	0.50	0.54	0.50
Business age (years)	4.49	3.77	5.77	4.30
Business Sector				
Industry	0.02	0.14	0.06	0.24
Commerce	0.76	0.43	0.35	0.48
Service	0.14	0.35	0.53	0.50
Farming	0.05	0.22	0.01	0.11
Mixed	0.03	0.16	0.04	0.20
Credit score before first loan	448	205	494	215
Formalized before first loan (according to MFI)	0.07	0.26	0.61	0.49
<u>Outcomes</u>				
Income				
Total household income (in Reais)	2,059	2,045	4,614	4,334
Sales last month (in Reais)	1,636	2,769	4,120	5,115
Profit last month (in Reais)	620	1,344	1,688	2,933
Months with income smaller than expenses in last six	1.77	1.15	1.80	1.41
Employment generation				
Number of employees in household	0.32	0.64	0.27	0.56
Number of employees out of household	0.19	0.60	0.44	1.07
No employees in household	0.74	0.44	0.77	0.42
No employees out of household	0.88	0.32	0.79	0.41
Access to credit				
Number of accessible sources of credit	1.57	1.87	3.78	2.65
Had credit request refused in last 6 months	0.02	0.13	0.05	0.22
No access to credit	0.38	0.49	0.16	0.37
Formalization				
Formalized	0.10	0.30	0.77	0.42
Formalized as MEI	0.09	0.28	0.60	0.49
Intent to formalize	0.37	0.48	0.42	0.50
<u>Data collection</u>				
Time from first loan to interview for control group	8.66	32.18	8.19	36.24
Time from first loan to interview for treated group	20.88	34.02	20.64	36.68

This table shows descriptive sample statistics. SD indicates the standard deviation.

Table 2: Income results

	(1)	(2)	(3)	(4)	(5)
	Total household income	Sales last month	Profit last month	Months with income smaller than expenses	Business performance index
Panel A: Northeast					
Treated	-128.34	9.93	-33.96	-0.10	-0.04
	[132.12]	[174.71]	[90.63]	[0.07]	[0.07]
Beneficiary age	15.30**	7.14	3.47	-0.00	0.01*
	[7.34]	[9.72]	[5.04]	[0.00]	[0.00]
Female	-392.93**	-799.40***	-370.29***	-0.04	-0.27***
	[158.42]	[211.20]	[109.56]	[0.09]	[0.08]
Credit Score	0.39	0.53	0.33	-0.00**	0.00
	[0.36]	[0.48]	[0.25]	[0.00]	[0.00]
Business age	54.28***	62.41**	13.69	-0.00	0.03***
	[19.10]	[25.32]	[13.07]	[0.01]	[0.01]
Formal business	631.44**	2031.90***	911.95***	-0.22	0.51***
	[297.68]	[397.85]	[204.60]	[0.15]	[0.15]
Primary school	1392.23***	907.26**	529.26**	-0.06	0.65***
	[307.42]	[401.75]	[206.98]	[0.17]	[0.15]
High school	2298.92***	1500.97***	825.77***	-0.00	1.05***
	[316.83]	[413.96]	[213.20]	[0.17]	[0.16]
Single	-315.19**	-293.94	-175.47*	-144.07	-0.15*
	[154.64]	[204.86]	[106.33]	[116.07]	[0.08]
Divorced/widow	-307.58	16.44	128.46	-371.20*	0.05
	[293.43]	[387.92]	[199.51]	[220.25]	[0.16]
Observations	991	947	929	795	929
Adjusted R^2	0.222	0.109	0.110	0.216	0.185
Panel B: South					
Treated	189.77	-121.27	243.07	-0.06	-0.02
	[342.20]	[459.00]	[264.95]	[0.13]	[0.08]
Beneficiary age	37.80**	-18.49	14.14	-0.00	0.00
	[19.14]	[25.72]	[14.79]	[0.01]	[0.00]
Female	-26.65	-696.60	-486.09	-0.14	-0.15
	[408.64]	[542.96]	[312.46]	[0.16]	[0.10]
Credit score	1.56*	-0.24	-0.53	-0.00	0.00
	[0.86]	[1.16]	[0.66]	[0.00]	[0.00]
Business age	38.41	92.53	-5.37	-0.00	0.02
	[45.54]	[60.46]	[34.99]	[0.02]	[0.01]
Formal business	794.80*	2812.11***	1203.45***	-0.12	0.36***
	[474.25]	[632.23]	[362.62]	[0.18]	[0.11]
Primary school	757.99	-26.63	-163.49	-0.11	0.08
	[809.26]	[1048.24]	[597.93]	[0.33]	[0.18]
High school	1858.21**	-297.57	130.58	-0.06	0.27
	[826.37]	[1071.58]	[610.90]	[0.34]	[0.19]
Single	206.13	-1182.13*	-525.31	641.26	0.30
	[500.37]	[673.88]	[400.30]	[408.59]	[0.19]
Divorced/widow	-1561.12**	69.18	-57.84	-1509.69***	-0.08
	[670.69]	[922.49]	[546.02]	[547.67]	[0.26]

Observations	462	436	428	364	425
Adjusted R^2	0.180	0.113	0.051	0.399	0.159

This table shows results of OLS regressions for five outcomes of the income outcome category. All regressions include fixed effects for the sector of activity of the beneficiaries, municipality-MFI fixed effects, and interviewer fixed effects. Standard errors are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 3: Employment generation results

	(1)	(2)	(3)	(4)	(5)
	Number of employees in household	Number of employees out of household	Business without employees in household	Business without employees out of household	Total number of employees
Panel A: Northeast					
Treated	-0.01	-0.02	0.03	0.01	-0.03
	[0.04]	[0.04]	[0.03]	[0.02]	[0.05]
Beneficiary age	-0.00	0.01**	-0.00	-0.00*	0.01*
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Female	-0.02	-0.12***	0.03	0.06***	-0.13**
	[0.05]	[0.05]	[0.03]	[0.02]	[0.06]
Credit Score	-0.00	0.00	-0.00	-0.00	0.00
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Business age	0.01**	-0.00	-0.01**	0.00	0.01
	[0.01]	[0.01]	[0.00]	[0.00]	[0.01]
Formal business	0.07	0.05	-0.02	-0.07	0.11
	[0.09]	[0.08]	[0.06]	[0.04]	[0.12]
Primary school	0.05	0.19**	-0.09	-0.09**	0.25*
	[0.09]	[0.09]	[0.06]	[0.05]	[0.13]
High school	0.01	0.28***	-0.06	-0.13***	0.29**
	[0.09]	[0.09]	[0.06]	[0.05]	[0.13]
Single	-0.04	0.03	-0.01	0.07**	-0.03
	[0.04]	[0.04]	[0.06]	[0.03]	[0.02]
Divorced/widow	-0.19**	0.02	-0.17	0.16***	-0.06
	[0.08]	[0.08]	[0.12]	[0.06]	[0.04]
Observations	991	991	991	991	991
Adjusted R2	0.088	0.126	0.083	0.106	0.140
Panel B: South					
Treated	-0.01	-0.03	-0.01	0.03	-0.03
	[0.05]	[0.11]	[0.04]	[0.04]	[0.09]
Beneficiary age	0.00	-0.01	-0.00	0.00	-0.01
	[0.00]	[0.01]	[0.00]	[0.00]	[0.01]
Female	0.12**	0.10	-0.07	0.05	-0.02
	[0.06]	[0.13]	[0.05]	[0.04]	[0.11]
Credit score	-0.00	0.00	0.00	0.00	0.00
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Business age	-0.01	0.02	0.00	-0.01	0.03**
	[0.01]	[0.01]	[0.01]	[0.00]	[0.01]
Formal business	0.28***	0.69***	-0.21***	-0.05	0.42***
	[0.07]	[0.15]	[0.05]	[0.05]	[0.13]
Primary school	0.23**	0.19	-0.16*	0.08	-0.05
	[0.12]	[0.25]	[0.09]	[0.09]	[0.22]
High school	0.08	0.11	-0.08	-0.02	0.02
	[0.12]	[0.26]	[0.09]	[0.09]	[0.22]
Single	-0.03	-0.12	-0.09	0.04	-0.02
	[0.07]	[0.15]	[0.13]	[0.06]	[0.05]
Divorced/widow	-0.12	0.19	0.32*	0.08	-0.11

	[0.09]	[0.21]	[0.18]	[0.07]	[0.07]
Observations	462	462	462	462	462
Adjusted R2	0.144	0.320	0.153	0.142	0.292

This table shows results of OLS regressions for five outcomes of the employment generation outcome category. All regressions include fixed effects for the sector of activity of the beneficiaries, municipality-MFI fixed effects, and interviewer fixed effects. Standard errors are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 4: Access to credit results

	(1)	(2)	(3)	(4)
	Number of sources of accessible sources of credit	Had credit request refused in last 6 months	No access to credit	Credit access index
Panel A: Northeast				
Treated	-0.14	-0.00	0.04	-0.08
	[0.10]	[0.01]	[0.03]	[0.06]
Beneficiary age	0.01**	0.00	-0.00	0.01**
	[0.01]	[0.00]	[0.00]	[0.00]
Female	-0.52***	0.01	0.08**	-0.28***
	[0.12]	[0.01]	[0.03]	[0.07]
Credit Score	0.00	-0.00	-0.00**	0.00
	[0.00]	[0.00]	[0.00]	[0.00]
Business age	-0.00	0.00**	-0.00	0.00
	[0.01]	[0.00]	[0.00]	[0.01]
Formal business	0.40*	0.02	-0.10	0.24*
	[0.23]	[0.02]	[0.06]	[0.13]
Primary school	0.85***	-0.02	-0.17**	0.49***
	[0.24]	[0.02]	[0.07]	[0.13]
High school	1.30***	-0.02	-0.29***	0.74***
	[0.25]	[0.02]	[0.07]	[0.13]
Single	-0.06	0.00	0.03	-0.04
	[0.12]	[0.01]	[0.03]	[0.06]
Divorced/widow	0.31	0.04**	-0.09	0.16
	[0.23]	[0.02]	[0.06]	[0.12]
Observations	991	991	991	991
Adjusted R2	0.303	0.029	0.202	0.294
Panel B: South				
Treated	-0.20	0.03	0.02	-0.08
	[0.20]	[0.02]	[0.03]	[0.08]
Beneficiary age	0.00	0.00	0.00	0.00
	[0.01]	[0.00]	[0.00]	[0.00]
Female	-0.11	-0.03	0.03	-0.05
	[0.23]	[0.03]	[0.04]	[0.09]
Credit score	0.00	-0.00**	-0.00	0.00
	[0.00]	[0.00]	[0.00]	[0.00]
Business age	0.03	-0.00	-0.00	0.01
	[0.03]	[0.00]	[0.00]	[0.01]
Formal business	0.82***	0.02	-0.09*	0.30***
	[0.27]	[0.03]	[0.05]	[0.11]
Primary school	0.54	0.01	-0.02	0.16
	[0.46]	[0.05]	[0.08]	[0.18]
High school	0.77	0.01	-0.03	0.26
	[0.47]	[0.05]	[0.08]	[0.18]
Single	0.23	0.03	0.00	0.12
	[0.29]	[0.03]	[0.05]	[0.11]
Divorced/widow	-0.17	0.05	0.00	-0.05
	[0.39]	[0.04]	[0.07]	[0.15]

Observations	459	459	459	459
Adjusted R2	0.416	0.095	0.197	0.394

This table shows results of OLS regressions for four outcomes of the access to credit outcome category. All regressions include fixed effects for the sector of activity of the beneficiaries, municipality-MFI fixed effects, and interviewer fixed effects. Standard errors are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 5: Formalization results

	(1)	(2)	(3)
	Formalized	Formalized as MEI	Intent to formalize
Panel A: Northeast			
Treated	0.00	0.01	-0.03
	[0.02]	[0.01]	[0.03]
Beneficiary age	0.00	0.00	-0.00
	[0.00]	[0.00]	[0.00]
Female	-0.08***	-0.03**	-0.06
	[0.02]	[0.01]	[0.04]
Credit Score	-0.00	0.00	-0.00**
	[0.00]	[0.00]	[0.00]
Business age	0.01***	-0.00	-0.01
	[0.00]	[0.00]	[0.00]
Primary school	0.03	0.01	0.14*
	[0.04]	[0.02]	[0.08]
High school	0.06	0.04*	0.20**
	[0.04]	[0.02]	[0.08]
Single	-0.04*	0.00	0.10**
	[0.02]	[0.01]	[0.04]
Divorced/widow	-0.04	-0.01	-0.05
	[0.04]	[0.02]	[0.08]
Observations	732	925	697
Adjusted R2	0.081	-0.003	0.201
Panel B: South			
Treated	-0.00	0.02	-0.11
	[0.09]	[0.05]	[0.19]
Beneficiary age	0.00	0.00	-0.01
	[0.00]	[0.00]	[0.01]
Female	-0.22**	-0.07	0.05
	[0.11]	[0.05]	[0.19]
Credit score	-0.00	-0.00	0.00
	[0.00]	[0.00]	[0.00]
Business age	-0.01	-0.01	0.03
	[0.01]	[0.01]	[0.02]
Primary school	0.24	0.06	-0.40
	[0.18]	[0.08]	[0.34]
High school	0.31	0.13	-0.55
	[0.18]	[0.09]	[0.35]
Single	-0.11	0.01	-0.07
	[0.16]	[0.07]	[0.30]
Divorced/widow	-0.24	-0.06	-0.02
	[0.19]	[0.08]	[0.33]
Observations	112	183	79
Adjusted R2	0.413	0.022	0.006

This table shows results of OLS regressions for three outcomes of the business formalization outcome category. All regressions include fixed effects for the sector of activity of the beneficiaries, municipality-MFI fixed effects, and interviewer fixed effects. Standard errors are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 6: Subjective wellbeing

	(1)	(2)	(3)	(4)	(5)
	Feeling happy	Feeling peaceful	Feeling worried	Feeling sad	Subjective wellbeing index
Panel A: Northeast					
Treated	-0.04*	-0.03	0.03	0.02	-0.12**
	[0.02]	[0.02]	[0.03]	[0.02]	[0.06]
Beneficiary age	0.00	0.00	-0.00	-0.00	0.00
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Female	-0.06**	-0.06***	0.06*	0.03	-0.20***
	[0.02]	[0.02]	[0.03]	[0.02]	[0.07]
Credit Score	0.00*	0.00	-0.00	-0.00*	0.00
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Business age	-0.00	0.00	0.01*	0.00	-0.01
	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]
Formal business	0.09*	-0.04	0.05	-0.06	0.09
	[0.05]	[0.04]	[0.06]	[0.04]	[0.13]
Primary school	0.07	0.05	-0.13**	-0.06	0.29**
	[0.05]	[0.05]	[0.06]	[0.04]	[0.14]
High school	0.10**	0.08*	-0.13*	-0.09**	0.39***
	[0.05]	[0.05]	[0.06]	[0.04]	[0.14]
Single	-0.03	-0.00	-0.08**	0.01	0.01
	[0.02]	[0.02]	[0.03]	[0.02]	[0.07]
Divorced/widow	-0.09*	-0.07	-0.00	0.03	-0.19
	[0.04]	[0.04]	[0.06]	[0.04]	[0.13]
Observations	991	991	991	991	991
Adjusted R2	0.151	0.191	0.105	0.037	0.113
Panel B: South					
Treated	-0.02	-0.05	0.05	0.03	-0.14
	[0.04]	[0.04]	[0.04]	[0.03]	[0.09]
Beneficiary age	0.00	0.00	-0.00	0.00	0.00
	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]
Female	-0.05	-0.10*	0.13**	0.04	-0.29**
	[0.04]	[0.05]	[0.05]	[0.03]	[0.11]
Credit score	0.00***	0.00	-0.00**	-0.00	0.00**
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Business age	0.01**	-0.00	-0.01	-0.00	0.02
	[0.00]	[0.01]	[0.01]	[0.00]	[0.01]
Formal business	0.12**	0.05	0.00	0.00	0.13
	[0.05]	[0.06]	[0.06]	[0.04]	[0.13]
Primary school	-0.04	-0.04	-0.06	-0.06	0.06
	[0.08]	[0.10]	[0.10]	[0.06]	[0.21]
High school	-0.02	-0.08	-0.09	-0.04	0.05
	[0.08]	[0.10]	[0.11]	[0.06]	[0.22]
Single	0.06	-0.01	-0.13**	-0.03	0.20
	[0.05]	[0.06]	[0.07]	[0.04]	[0.14]
Divorced/widow	-0.11	0.04	0.01	0.09*	-0.18
	[0.07]	[0.08]	[0.09]	[0.05]	[0.18]
Observations	459	459	459	459	459

Adjusted R2	0.331	0.137	0.159	0.054	0.126

This table shows results of OLS regressions for five outcomes of the subjective wellbeing outcome category. All regressions include fixed effects for the sector of activity of the beneficiaries, municipality-MFI fixed effects, and interviewer fixed effects. Standard errors are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

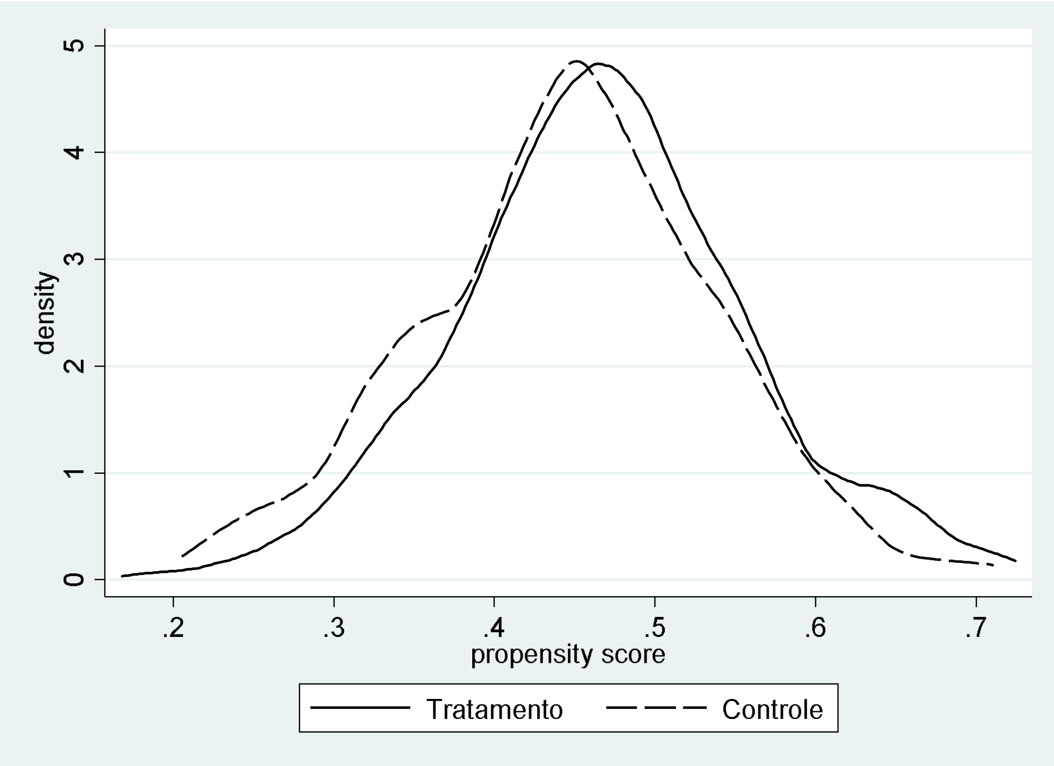
Table 7: Heterogeneous treatment effects (HDI)

	(1) Total household income	(2) Sales last month	(3) Profit last month	(4) Months with income smaller than expenses	(5) Business perform. Index
Panel A: Northeast					
Treated	-13.56	-248.36	-61.85	0.01	-0.03
	[177.17]	[233.05]	[121.39]	[0.10]	[0.09]
Low HDI-M	81.40	-317.11	69.08	0.30	0.01
	[681.85]	[883.17]	[454.85]	[0.37]	[0.34]
Low HDI-M*Treated	-260.09	591.85*	63.41	-0.24*	-0.02
	[267.47]	[353.87]	[183.60]	[0.15]	[0.14]
Baseline	2266.00	1789.93	630.01	1.73	0.07
Observations	991	947	929	795	929
Adjusted R^2	0.222	0.111	0.110	0.218	0.183
Panel B: South					
Treated	-231.98	-409.89	112.71	-0.02	-0.10
	[462.85]	[617.95]	[370.20]	[0.17]	[0.11]
Low HDI-M	-2449.85	-3746.27	-2972.50	1.56	-0.85
	[4082.06]	[5343.87]	[3134.41]	[1.64]	[0.97]
Low HDI-M*Treated	694.69	552.47	97.20	-0.04	0.17
	[686.79]	[927.18]	[549.19]	[0.26]	[0.17]
Baseline	5126.64	4239.83	1459.38	1.99	1.45
Observations	459	433	425	362	425
Adjusted R^2	0.180	0.119	0.053	0.408	0.157

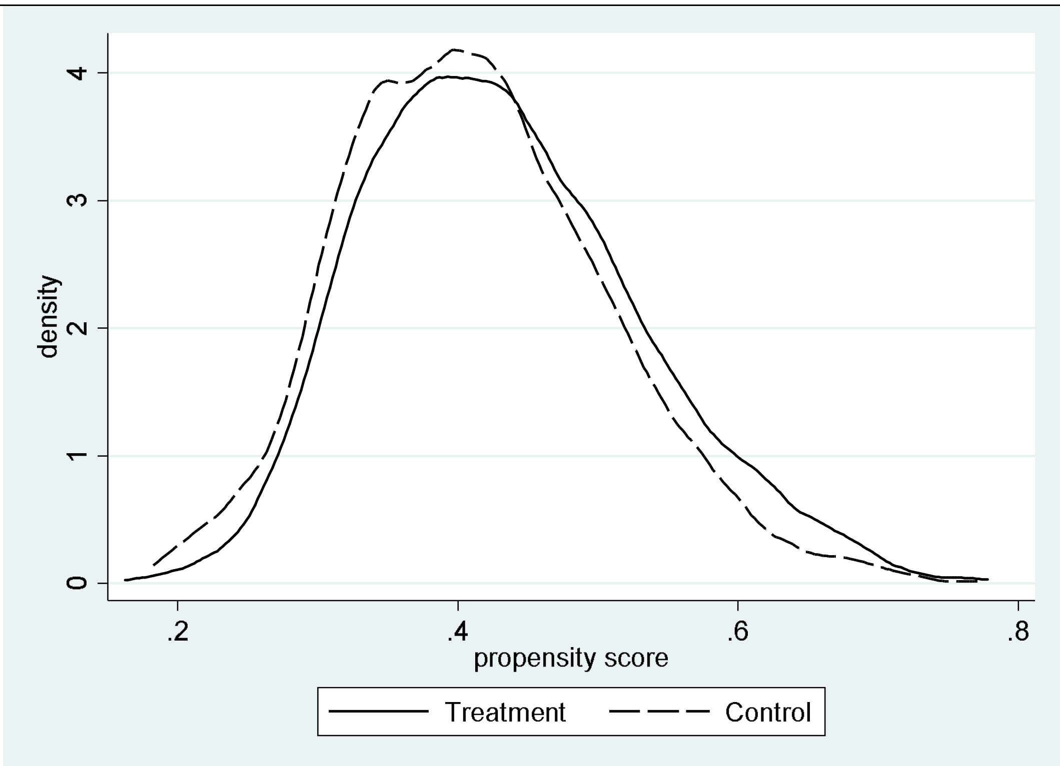
This table shows results of OLS regressions for five outcomes of the income outcome category. All regressions include fixed effects for the sector of activity of the beneficiaries, municipality-MFI fixed effects, and interviewer fixed effects. The regressions also include all control variables used in the other regressions. Their coefficients are omitted to save space. Baseline indicates the average value for new clients in low HDI-M regions. Standard errors are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Figure 1 - Distributions of propensity scores by treatment condition and region

Panel A: Northeast



Panel B: South



This figure presents the distribution of propensity scores by region and control condition. The scores do not take into account the exact matching in Municipality and MFI.

Appendix

Table A1: Balance statistics by Region

	Northeast					South				
	Control		Treatment		Std. diff.	Control		Treatment		Std. diff.
Variables	Mean	SD	Mean	SD		Mean	SD	Mean	SD	
Beneficiary age (years)	33.88	11.07	34.88	11.68	0.09	40.66	12.11	39.79	39.79	0.07
Female	0.68	0.47	0.69	0.46	0.02	0.59	0.49	0.51	0.50	0.16
Education										
No degree	0.06	0.24	0.07	0.25	0.04	0.05	0.22	0.07	0.25	0.09
Primary school	0.44	0.50	0.43	0.50	0.02	0.46	0.50	0.40	0.49	0.12
High school or higher	0.50	0.50	0.50	0.50	0.00	0.48	0.50	0.53	0.50	0.10
Marital Status										
Single	0.33	0.47	0.32	0.47	0.02	0.19	0.40	0.23	0.42	0.10
Married	0.61	0.49	0.61	0.49	0.00	0.74	0.44	0.67	0.47	0.16
Widow, divorced or separated	0.06	0.24	0.06	0.24	0.00	0.07	0.25	0.11	0.31	0.16
Business Sector										
Industry	0.03	0.16	0.02	0.14	0.07	0.05	0.23	0.04	0.20	0.04
Commerce	0.77	0.42	0.76	0.43	0.02	0.46	0.50	0.41	0.49	0.10
Service	0.14	0.35	0.15	0.36	0.03	0.47	0.50	0.51	0.50	0.08
Farming	0.05	0.21	0.04	0.21	0.05	0.01	0.09	0.01	0.10	0.00
Mixed	0.02	0.14	0.02	0.15	0.00	0.02	0.13	0.02	0.15	0.00
Credit score before first loan	440.98	206.26	453.15	205.23	0.06	515.98	218.65	495.59	216.46	0.09
Formalized before first loan (according to MFI)	0.06	0.23	0.07	0.25	0.04	0.62	0.49	0.61	0.49	0.02
Business age (years)	4.63	4.12	4.68	3.54	0.01	6.16	4.86	5.85	3.92	0.06
Month of first loan with MFI										
January	0.21	0.41	0.22	0.41	0.02	0.23	0.42	0.26	0.44	0.07
February	0.16	0.36	0.15	0.36	0.03	0.39	0.49	0.35	0.48	0.08
March	0.63	0.48	0.63	0.48	0.00	0.38	0.49	0.39	0.49	0.02

This table contains the mean and standard deviation (SD) for treatment and control group separately for the Northeast and the South of Brazil. Std. diff. indicates the standardized differences, or the mean difference divided by the standard deviation of the whole sample (treated and controls).

Table A2: Attrition model

Dependent variable	Survey participation	
	Northeast	South
Treated	0.1993*	0.5740***
	[0.1065]	[0.1285]
Credit score before loan	0.0002	-0.0001
	[0.0004]	[0.0004]
Credit score on June 1st	0.0005	0.0005
	[0.0003]	[0.0004]
Formal business (before loan)	0.3452	-0.1171
	[0.2146]	[0.1277]
Beneficiary age	0.0164***	0.0124**
	[0.0048]	[0.0053]
Female	0.0976	0.2839**
	[0.1129]	[0.1259]
Observations	1,648	1,259
Pseudo- R^2	0.012	0.0262

This table shows results of logistic regressions using survey participation as dependent variable, separately for the Northeast and the South of Brazil. Survey participation is a dummy variable that takes on the value of one if a client participated in the survey, and zero otherwise. Standard errors are shown in brackets. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.