Open Finance Adoption and Financial Inclusion: Experimental Evidence from Microentrepreneurs*

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Abstract

Does improving knowledge about open finance increase its adoption and lead to greater financial inclusion? We implement a randomized experiment that provides Brazilian microentrepreneurs with targeted information about open finance. The intervention increases open finance adoption by more than 7 percentage points, a 40% increase relative to a control-group mean of 17%. While the intervention increases adoption broadly across the study population, the impacts are largest among firms with greater baseline digital payment (Pix) activity, consistent with lower adoption barriers for digitally engaged firms. Analysis of firm-level administrative data shows that the intervention increases the probability of having an available credit card limit by 4–5 percentage points, or approximately 5–7% relative to the control-group mean. Consistent with this effect, the intervention increases the probability of credit-card spending by 3–4 percentage points, equivalent to approximately 4–6% of the control-group mean. Over a six-month horizon, the intervention also increases access to credit through business bank accounts to a greater extent than through personal bank accounts; raises reliance on working capital loans by 5 percentage points, equivalent to 7% of the control-group mean; and increases the number of Pix transactions in business accounts by 15%.

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1 Introduction

Open banking initiatives are increasingly being implemented by regulators around the world (He et al., 2023; Nam, 2023; Babina et al., 2025). Despite their potential to improve access to financial services for consumers and micro, small, and medium-sized enterprises (MSMEs), their take-up remains far from universal. In the United Kingdom, one of the earliest adopters of open banking, only one in five consumers and small businesses actively uses it (Open Banking Limited, 2025). Brazil is a global leader in absolute terms, with more than 60 million active data-sharing consents. Nevertheless, administrative data suggest that adoption remains limited, reaching at most 20% of individuals and less than 2% of firms (Open Finance Brasil Association, 2025). The limited take-up is striking given the rapid diffusion of digital payments and the entry of fintech firms (Puri et al., 2017; Berg et al., 2020; Higgins, 2024; Mariani et al., 2025). These developments increase the potential benefits of sharing transaction data across financial institutions, including more tailored financial services and better credit terms (Alok et al., 2025; Babina et al., 2025).

Realizing these benefits requires customers to authorize the sharing of their financial data. Yet, even customers who routinely use digital financial services may have limited knowledge of how open banking works or what they can gain from it. In this paper, we address two questions central to the effectiveness of open banking policies: whether limited knowledge and understanding constrain adoption among microentrepreneurs, and whether an informational intervention that increases adoption also expands access to credit and changes how entrepreneurs use personal and business accounts. We exploit a randomized intervention that provided Brazilian microentrepreneurs with concise information on how Open Finance operates, its potential benefits, and its data-sharing protocols. We find that our short informational video describing the operation and benefits of Open Finance increases reported adoption in the treatment group by more than 7 percentage points, equivalent to approximately 40% of the control-group mean at follow-up.

Our experiment involved 1,151 microentrepreneurs running small storefront businesses in the retail and service sectors in the Brazilian state of Espírito Santo. Between April and July 2025, we administered a short in-person baseline survey to each individual. At the end of the survey, we showed a video to both the treatment and control groups covering digital payments ("Pix") usage, financial organization, and the benefits of having separate business bank ac-

counts. Microentrepreneurs assigned to the treatment group saw a video that contained an additional segment describing the Open Finance scheme and its potential benefits. After an average of 16 days, we administered a short follow-up survey. Since the control group received general information about financial inclusion, the treatment effects we identify reflect the information specific to Open Finance.

To assess the impacts of our intervention, we assemble a novel dataset that combines original baseline and follow-up surveys with administrative records from the Central Bank of Brazil (BCB). These data are distinctive in two respects. First, our surveys provide granular, microentrepreneur-level measures of reported Open Finance adoption at baseline and follow-up, alongside detailed owner- and firm-level characteristics. These measures allow us to assess changes in adoption and examine heterogeneity in the response to the intervention. Second, the linked administrative records allow us to observe credit outcomes and Pix payment activity under both owners' personal and firms' business tax identifiers. This feature is particularly valuable because the financial boundary between small businesses and their owners is often thin: entrepreneurs use personal credit cards and balance sheets to finance their firms, pledge household assets as collateral, and mix personal and business assets (Chatterji and Seamans, 2012; Robb and Robinson, 2014; Schmalz et al., 2017; Ersahin et al., 2021). Observing both identifiers therefore allows us to capture financing and payment activity that would be missed in firm-level records alone and to trace the intervention's effects across personal and business accounts. This distinction is especially relevant for microentrepreneurs in developing countries, where household and business finances are often closely intertwined (Fafchamps et al., 2014; Riley, 2024).

The intervention increases reported Open Finance adoption by more than 7 percentage points, approximately 40% relative to the control-group mean of 17% at follow-up. We exploit our rich survey and administrative data, which contain detailed information on owner demographics, firm characteristics, financial and management practices, and prior credit and digital payment activity, to examine how this response varies with entrepreneurs' baseline characteristics. We adopt an iterative approach based on causal forests, progressively refining the set of baseline characteristics to identify those most informative about variation in treatment effects. We detect heterogeneity along one margin: adoption effects are larger among firms with greater baseline Pix transaction activity, a proxy for firm size and prior engagement with digital payments. This pattern is consistent with digitally engaged firms facing lower barriers to adopting new financial technologies (Breza et al., 2020). We find no statistically significant heterogeneity along the rich set of other demographic, financial, and managerial dimensions captured by

our baseline data. Taken together, these findings suggest that the intervention promotes Open Finance adoption across a broad range of microentrepreneurs.

The linked administrative records of the BCB further show that the intervention improves credit access and changes how microentrepreneurs conduct their financial activities. The treatment increases the probability of having an available credit card limit by 3.6 and 5.1 percentage points, and the probability of credit card spending by 2.7 and 4.1 percentage points across three- and six-month windows, respectively. These effects are primarily driven by the financial activities carried out either under the owner's tax identifier, or under a mix of personal and business accounts. At the same time, we find no evidence of an increase in costly revolving credit-card financing – suggesting that while the treatment group makes larger use of cards, this is not driven by a greater likelihood of rolling over outstanding balances. We also find significant treatment effects on credit issued on business accounts in the six-month window, with the probability of obtaining new business credit increasing by 6.4 percentage points, or by approximately 34% relative to the control-group mean, which is stronger than for personal accounts. Finally, we observe an increase in reliance on working capital in the six-month window by 5 percentage points, equivalent to 50% of the control-group mean.

The positive and statistically significant treatment effects on the availability of credit-card limits point to an expansion in credit supply (Chava et al., 2023; Yin, 2026) through a form of financing particularly important for MSMEs (Akcigit et al., 2025). These gains are not accompanied by a significant increase in revolving credit-card financing, suggesting that improved access to credit need not entail greater reliance on expensive short-term borrowing. Over the six-month window, we also find that the intervention increases the probability of obtaining new credit through business accounts and holding working-capital loans. Together, these findings suggest that the intervention expands credit access and may also help entrepreneurs obtain products better suited to their firms' liquidity and working-capital needs.

Finally, turning to firm activity, the treatment increases the number of digital payments received through business accounts—which we use as a proxy for firm sales activity—by 16.5%. Although we find no statistically significant effect on transaction values, the estimated effects on both transaction counts and values are positive for business accounts and negative for owners' personal accounts. Together with the increase in credit issued directly to businesses, this pattern provides suggestive evidence that the intervention shifted financial activity toward formal business channels.

Our paper contributes to three strands of literature. First, we add to the emerging literature on open banking and fintech adoption (He et al., 2023; Babina et al., 2025; Nam, 2023).

Consistent with information-based models of technology adoption (Foster and Rosenzweig, 1995; Conley and Udry, 2010), we provide the first experimental evidence that reducing information barriers may substantially increase the adoption of open bank initiatives and deepen their effects. Beyond adoption, we complement the growing literature on the effects of open banking on credit access (Babina et al., 2025; Alok et al., 2025): While existing studies generally examine consumer credit or business credit in isolation, our linked administrative data allow us to observe both channels. Thanks to these novel data, our intervention therefore connects the demand-side barriers to Open Finance adoption with the financial-inclusion downstream effects of a short, readily scalable intervention that increases the take-up of this initiative.

Second, we contribute to the literature on small-business financing by highlighting the blurred financial boundary between entrepreneurs and their firms (Fafchamps et al., 2014; Riley, 2024). Previous research shows that entrepreneurs frequently finance their businesses through personal credit cards and balance sheets, pledge household assets as collateral, and combine personal and business assets (Chatterji and Seamans, 2012; Robb and Robinson, 2014; Schmalz et al., 2017; Ersahin et al., 2021; Berger et al., 2024; Akcigit et al., 2025; Fonseca and Wang, 2022). A related literature demonstrates that transaction-account records, digital footprints, and payment histories can reduce information frictions by improving borrower screening and monitoring (Mester et al., 2007; Berg et al., 2020; Parlour et al., 2022; He et al., 2023; Babina et al., 2025; Ghosh et al., 2026; Mariani et al., 2025). We connect these strands by showing that the same informational intervention that increases Open Finance adoption also expands entrepreneurs' personal credit card limits and use and, over a longer horizon, increases the probability of new credit being issued directly to businesses.

Finally, we contribute to the literature on financial inclusion and digital formalization in developing economies (Burgess and Pande, 2005; Banerjee et al., 2013; Barboni et al., 2021). Brazil provides an especially relevant setting because recent financial-inclusion policies and the rapid diffusion of Pix have transformed access to digital financial services (Fonseca and Matray, 2024; Burga et al., 2024; Mariani et al., 2025), paralleling developments in India (Dubey and Purnanandam, 2023; Alok et al., 2025). Our results show that the intervention increases both the probability of obtaining new credit through business accounts and the number of Pix payments received through these accounts. These findings provide suggestive evidence that improved access to credit was accompanied by a shift in financial activity toward formal business channels.

2 Context and Experimental Design

Open banking comprises a series of data-sharing initiatives, typically regulated by Central Banks, that allow individuals and firms to securely share their financial information, such as transaction history, investments, and credit profiles, with other authorized financial institutions, always with explicit customer consent. As of 2024, 95 jurisdictions had adopted open banking initiatives to some extent (Cambridge Centre for Alternative Finance, 2024).

Since 2010, BCB has had financial inclusion as a key strategic objective (Banco Central do Brasil, 2010; Alliance for Financial Inclusion, 2018). The BCB's current work agenda (*Agenda BC#*) focuses on inclusion, competitiveness, transparency, education, sustainability, and excellence. Within this framework, Brazil's financial landscape has undergone a profound transformation over the past five years, driven by the entry of fintech firms, the widespread adoption of digital payments, particularly Pix, and the roll-out of Open Finance.

2.1 Open Finance in Brazil

In May 2020, the Central Bank of Brazil introduced Open Finance to encourage innovation, promote competition, increase the efficiency of the financial and payment systems, and promote financial inclusion.[1] Its implementation started in 2021, with a continuous roll-out of new features.[2] As with other open banking initiatives, Open Finance's core principle is to give customers greater control over their financial data, allowing them to share it securely with other financial institutions for better access to services such as loans and advice, and more tailored credit terms.[3]

To initiate sharing, the customer starts in the app/website of the institution that will receive the data (the "data recipient"). They then select the data types they would like to share, and from which source institution (the "data holder"), after which they are redirected to the holder to authenticate and approve access. Customers must provide consent for each account individually.[4] Once consent is in place, the chosen recipient may use the shared data to deliver services such as credit proposals, financial management tools, or investment recommendations.

[1] Joint Resolution No. 1 of May 4, 2020.

[2] Open Finance is a broad initiative not only targeted at individuals and businesses but also designed to extend data interoperability to other domains, such as investments and supplemental pension schemes.

[3] Open Finance–enabled data sharing constitutes an additional layer built upon existing initiatives, including the BCB's Public Credit Bureau (SCR) and private credit bureaus (notably Cadastro Positivo). In contrast to these mechanisms, Open Finance permits clients to authorize the disclosure of more granular information and to exercise fine-grained control over which specific data elements are shared and the temporal scope of such sharing.

[4] Consents can be revoked at any time through either the data recipient's or the data holder's channel, and they do not grant blanket access to other institutions—only the specific recipient named in the consent can access the approved data.

The ecosystem operates through standardized Application Programming Interfaces (APIs), and participation from major financial institutions is mandatory.[5] As of December 2024, financial institutions had received 62 million active data-sharing consents from individuals and businesses (Open Finance Brasil Association, 2025). In the same year, 102 billion API calls were executed by financial institutions. Although this means that Brazil leads open banking in terms of absolute numbers, overall adoption is low. Less than 20% of Brazilians shared their data via Open Finance, and adoption rates for firms are even lower, with a participation rate of less than 2% (Open Finance Brasil Association, 2025).

2.2 Pix

In the same year Open Finance was launched, BCB also introduced Pix, a real-time payment system. Pix is instant, alias-based, available 24/7, and free for individuals and for many businesses, requiring only a bank or payment institution account and internet access.[6] Pix has become an essential tool for both individuals and businesses, processing over 63 billion transactions in 2024 and becoming nearly universal.

One of the key benefits of systems like Pix is their ability to generate "hard information" about users' financial behavior, which is valuable to the financial institutions that can access it. Unlike informal cash transactions, digital payments create a traceable record of financial activities that banks and other financial service providers can use to assess creditworthiness and offer tailored financial products more effectively.

2.3 Experimental Design

We use a cluster-randomized controlled trial to evaluate whether a short, video-based information intervention could increase adoption of Open Finance—and other downstream financial outcomes—among microentrepreneurs. We used a simple random assignment: Firms assigned to the control group watched a video that simply focused on the benefits of financial inclusion, while those assigned to the treatment group watched a longer version of the same video that also discussed the benefits of adopting Open Finance and data sharing across financial institutions more broadly (see Figure A2 for screenshots from both videos).

To evaluate impact, we conducted an in-person baseline survey followed by a phone-based follow-up survey. We also paired primary data with administrative data provided by BCB. Compensation was provided to all firms for participation in both surveys: a voucher worth

[5] API calls are a useful proxy for backend adoption in Open Finance. Each call represents a data-sharing request made between participating institutions. High API volumes would indicate strong institutional and technical uptake even if individual or firm-level adoption is still developing.

[6] Some businesses may need to pay small fees depending on the business size and bank.

approximately 70 BRL, or 13 USD for the baseline and a voucher worth approximately 35 BRL, or 6.50 USD, for the follow-up.

Sampling. We used a two-step sampling strategy. First, we geolocated a set of microenterprises from the Brazilian Federal Revenue Service (*Receita Federal*) and visited each one. However, only a subset of firms could be reached, as many had moved or gone out of business.[7]

To ensure we met the target sample size, we then moved on to conducting on-street canvassing within predefined cluster boundaries and invited all newly observed firms to participate using the same eligibility criteria.[8] Due to the potential impact of peer effects through shared business networks or word-of-mouth, we clustered firms spatially before randomization. Clustering proceeded in three steps: (1) Geo-coding: All candidate firms were geo-referenced using address data from *Receita Federal*. (2) Spatial algorithm: A constrained k-means algorithm grouped firms into compact, non-overlapping clusters that respected administrative-boundary contiguity and a maximum radius. (3) Balancing checks: Cluster solutions were iteratively refined to minimize variance in baseline characteristics (sector mix, firm age, owner gender) across clusters.

The final design comprises 56 clusters with a mean size of approximately 21 firms, for a total sample size of 1,170, as shown in Figure 1. Excluding firms located outside the predefined clusters reduces the sample to 1,151 firms.

3 Conceptual Framework

Our intervention provides microentrepreneurs with information about Open Finance. The experiment was motivated by widespread evidence that individuals have limited knowledge of open finance.[9] For example, a 2024 survey of 1,452 people across Brazil found that 74% of respondents reported knowing nothing or almost nothing about Open Finance (Zetta Institute, 2024). This study also revealed that this lack of knowledge about Open Finance is more pronounced among low-income individuals, a group that typically has been excluded from formal financial markets. Moreover, the Zetta survey also found that only 12% of individuals had shared or tried to share data; this figure was also confirmed in our study sample, where we

[7] 1,118 interviews (96.96%) were completed through on-street replacement canvassing while 35 interviewed microenterprises (3.04%) came from the *Receita Federal*-based sample. Replacement firms were searched within the same geographic clusters defined using *Receita Federal* addresses. Eligibility for replacement firms required that the firm operate in one of the sectors included in the original sample and that the respondent had more than two bank accounts of the same type (business or personal) at the time of interview. For replacement firms, enumerators used the observed business location within the pre-defined *Receita Federal*-based cluster boundaries.

[8] Enrollment and canvassing activities were completed before the treatment assignment was finalized.

[9] Cambridge Centre for Alternative Finance (2026) describes several government initiatives to raise consumer awareness of open finance in developing economies, where limited consumer awareness is even more pronounced.

observe that only 10% of respondents know what Open Finance is at baseline.[10]

Building on a large and well-established literature showing that providing information significantly boosts technology adoption (e.g., Conley and Udry, 2010; Foster and Rosenzweig, 1995), we hypothesize that receiving information on the benefits of Open Finance should increase its adoption. This leads us to formulate the following hypothesis:

Hypothesis 1: Open Finance Adoption. *Compared to the control group, microentrepreneurs in the treatment group should be more likely to adopt Open Finance when provided specific information about it.*

Because other adoption frictions remain, the effect of information depends on which barriers are most important. For example, the consent process may be complex and time-consuming, particularly for less digitally experienced users and firms with multiple owners, for which obtaining consent may require authorization from multiple representatives. In addition, limited trust in the financial system may constrain adoption even when information about Open Finance is readily available.

By design, participation in open banking initiatives expands the information available to financial intermediaries, potentially reducing information asymmetries and improving access to financial services and products (Pagano and Jappelli, 1993; World Bank Group, 2024).[11] This may be particularly relevant for microbusinesses, which are typically more opaque and lack real assets that can be pledged as collateral, motivating the following hypothesis:

Hypothesis 2: Effects on Financial Inclusion. *Compared to the control group, microentrepreneurs in the treatment group should experience higher rates of financial inclusion.*

Several features of Open Finance, however, make this prediction nontrivial in our institutional setting. First, users choose both which information to share and with which institutions, so the information available to a recipient may remain incomplete or selectively chosen. Second, data sharing is directional: sharing information from bank A with bank B does not require bank B to share its information with bank A. Users may therefore share information with institutions that already have more information about them than with those that have less, limiting the extent to which Open Finance levels the informational playing field across institutions. Third, newly available information need not immediately affect credit decisions. When Open Finance is relatively new, shared data may provide only a limited history, while incorporating these data into

[10] We asked the following question at baseline (translated from Portuguese): "There is an initiative by the Central Bank of Brazil called Open Finance. What do you think this initiative is about?" Answers were unprompted.

[11] Beyond its effects on credit volumes, prices, and product composition (e.g., through greater availability of unsecured loans), Open Finance may benefit entrepreneurs by shortening loan turnaround times. Greater availability of verifiable information may also facilitate automated underwriting and lower loan-origination costs.

credit-risk models may require standardization, validation, and investment in model development. If aggregate take-up remains low, financial institutions may have limited incentives to incur these fixed costs.

Open finance may also affect financial inclusion through channels beyond information sharing and credit-risk assessment. Greater integration across financial accounts may reduce administrative costs, facilitate accounting and cash-flow management, and improve firms' ability to manage their finances.[12] On the other hand, implementing and maintaining open finance may entail significant and heterogeneous infrastructure costs across financial institutions. If these costs are passed on to consumers, they may offset some of the efficiencies generated by information sharing.

4 Data

To assess the effects of our intervention on Open Finance adoption as well as downstream impacts on financial access, including and credit outcomes and digital payments via Pix, we leverage the following sources of data.

Primary data. We collected two rounds of primary data with firms based in the state of Espírito Santo: an in-person baseline survey, and a phone-based follow-up survey, both administered by OppenSocial, a Brazil-based survey firm. The baseline took place from April to July 2025 and was completed by 1,151 microentrepreneurs. The baseline survey captured firms' knowledge and adoption of Open Finance, as well as their use of major payment methods, including Pix. We also gathered characteristics of the employer or owner responsible for payment decisions (gender, education, age) and detailed managerial practices (frequency of financial accounting, how supply orders are placed, and typical timing of supply purchases). Finally, the survey measured microentrepreneurs' trust in government and institutions, privacy concerns, and risk aversion. Table 1 presents the mean for the main characteristics in the control and treatment groups: the two groups were on average balanced along the main characteristics, suggesting that the randomization worked well. In particular, Table 1 shows that roughly half of the business owners we interviewed are women; they are fairly educated, with 80% of them having completed at least middle school. The businesses we included in the study are quite small, having on average 1.4 employees in addition to the business owner. Interestingly, at baseline approximately 10% of respondents knew what Open Finance was, and about 8% had adopted it, this share being balanced across treatment and control.

[12] Some financial institutions have developed tools that use Open Finance data to notify customers when an account becomes overdrawn, illustrating how data sharing can facilitate financial management.

The follow-up survey was conducted via phone on average sixteen days after the baseline. This survey was substantially shorter than the baseline survey and contained questions to assess whether respondents had adopted Open Finance, when the adoption occurred, and which banks were used for adopting Open Finance (e.g., whether they were digital or nondigital banks,[13] and whether they were the respondent's main bank). Although the follow-up survey was administered relatively soon after baseline, the pick-up rate was considerably low: only 35% of the original sample (401 subjects) completed the follow-up.[14] Table A1 shows that attrition is balanced across treatment and control. While most baseline characteristics are similar between the two groups, attritors were slightly more educated, were more likely to have obtained a loan in the past, and reported higher trust towards the financial system (Table A2). Although the attrition-rate is balanced across the treatment and control groups as shown in Table A1, to rule out selective attrition we conduct several robustness checks which we discuss in the next Section.

Finally, both at baseline and at follow-up, both the owner's personal tax identifier (*Cadastro de Pessoas Físicas*, CPF) and the firm's business tax identifier (*Cadastro Nacional da Pessoa Jurídica*, CNPJ) were collected directly from respondents to link our survey data with administrative information from the BCB.

Administrative Data. We use two administrative datasets maintained by the BCB. The first consists of transaction-level records from Pix, the instant-payment system operated by the BCB as a 24/7 real-time gross settlement system. For each Pix transaction, the records report its date and value, whether the payer and payee are individuals or businesses, and their payment service providers. These records originate from the BCB-operated settlement infrastructure. We use these data to construct monthly measures of the number and value of Pix payments received by the microentrepreneurs in our sample. The second dataset is the BCB's monthly credit registry, which covers the universe of bank credit relationships exceeding BRL 200 and identifies the borrower, lender, and type of credit operation. From these records, we construct measures of credit-card limits, credit-card expenditures, revolving credit-card financing, and new credit operations. For the credit analysis, we restrict attention to credit extended by banks that were registered as recipients of Open Finance data at the beginning of our intervention, as these institutions could plausibly incorporate information shared through Open Finance into their lending decisions. Across both datasets, we observe financial activity recorded under owners' personal tax identifiers (CPF) and, for the 801 formally registered businesses in our

[13] Following Mariani et al. (2025), we define digital banks as financial institutions without branches or, more generally, those that rely exclusively on digital customer channels.

[14] Our survey completion rate is in line with studies with WhatsApp-based follow-up interviews, see for example Barboni et al. (2024) in the context of Venezuelan migrants in Colombia.

sample, firms' tax identifiers (CNPJ). Because microentrepreneurs frequently conduct business through personal accounts, combining these records allows us to capture financial activity that would be missed by examining CNPJ records alone and to distinguish between owner-level and formal business-level responses.[15]

Figure 2 shows our final sample of 1,151 firms and the number of firms we observe through different datasets and across survey rounds. Our analysis is performed on a sample of 401 firms when relying on primary data at follow up, while analyses based on credit and Pix data are performed on a sample of 1,128 and 1,131 firms, respectively.

5 Results

5.1 Impact on Open Finance Adoption

We start by analyzing whether the informational intervention increased Open Finance adoption. To do so, we estimate the following specification:

$$Y_i = \alpha + \beta T_i + \rho Y_{i,0} + \theta' X_i + \lambda_{c(i)} + \lambda_{s(i)} + \lambda_{\tau(i)} + \epsilon_i, \tag{1}$$

where Y_i is an indicator equal to one if respondent i reports having adopted Open Finance at follow-up and zero otherwise, T_i is an indicator for assignment to the treatment group, and $Y_{i,0}$ is the baseline value of the outcome. The vector X_i contains respondent and firm characteristics measured at baseline, while $\lambda_{c(i)}$, $\lambda_{s(i)}$, and $\lambda_{\tau(i)}$ denote city, economic-sector, and survey-time fixed effects, respectively. Standard errors are clustered at the geographic-cluster level, corresponding to the unit of randomization (Abadie et al., 2023). The coefficient of interest is β, which captures the intention-to-treat effect of providing specific information about Open Finance on reported adoption. Whenever a baseline measure of the outcome is available, we estimate Equation 1 as an ANCOVA specification by controlling for $Y_{i,0}$. In additional specifications, we select the baseline controls in X_i using Post-Double Selection Lasso (Belloni et al., 2014; Cilliers et al., 2024). We use the same framework in subsequent analyses that redefine Y_i to distinguish adoption with the respondent's main bank from adoption with other banks and adoption with digital banks from adoption with traditional banks.

Table 2 reports the results. At follow-up, 17% of respondents in the control group report having adopted Open Finance. The intervention increases reported adoption by more than 7 percentage points, corresponding to an increase of approximately 40% relative to the control-

[15] Brazil has a legal category called "*Microempreendedor Individual* (MEI)," or individual microentrepreneur, intended for self-employed workers and very small businesses with at most one employee. The financial activity of these businesses frequently passes through their owners' personal bank accounts.

group mean.[16] The estimates remain statistically significant and similar in magnitude when we control for baseline adoption in column 2, include the controls selected by Post-Double Selection Lasso in column 3, and add city, sector, and survey-time fixed effects in the remaining specifications. At the bottom of each column, we also report randomization-inference p-values, which are very similar to the cluster-robust p-values reported above.[17]

To assess whether attrition could bias the results shown in Table 2, we compute Lee (Lee, 2009) bounds on the treatment effect on Open Finance adoption. The approach trims the sample to construct worst- and best-case bounds on the treatment effect under the assumption of monotone selection — that is, that treatment affects selection into the follow-up sample in only one direction — treated firms are either weakly more or weakly less likely to respond than control firms, but not both. The results are shown in Table A3 in the Appendix. The lower bound (0.039, 95% CI: $[-0.051, 0.129]$) corresponds to the most pessimistic scenario, where all excess treated respondents are those who would have adopted Open Finance regardless. The upper bound (0.187, 95% CI: $[0.012, 0.362]$) corresponds to the most optimistic scenario. Both point estimates are positive. We further assess robustness to adverse attrition by calculating the tipping point at which selective nonresponse would overturn our main result. Because the intervention was an informational encouragement, we expect it to have a positive or null effect on adoption. To drive the full-sample average treatment effect to zero, the unobserved treatment effect among the 750 nonrespondents would have to be -4.4 percentage points. Given the baseline characteristics of the attritors—who have lower institutional trust and less prior credit-seeking than respondents—it is highly likely that the informational intervention simply had a null effect on them. Assuming it had a strictly negative effect of this magnitude on disengaged firms is highly implausible.[18]

We further assess robustness to attrition using an adverse-imputation exercise in which we impute effects on Open Finance adoption we observe for the follow-up survey respondents for all non-respondents and apply an increasing penalty δ to treated non-respondents, ranging

[16] One should notice that our main outcome is self-reported adoption at follow-up. Because the follow-up questionnaire does not provide respondents with a definition of Open Finance, reporting adoption requires that respondents both hold a data-sharing consent and recognize that consent as Open Finance. Our informational video may therefore increase reported adoption of Open Finance through two related channels: (i) by inducing respondents to establish new data-sharing consents and (ii) by improving their recognition of existing consents. We thus interpret our estimates as treatment effects on *recognized* adoption. If the video also changes how people recall or report their status, some reporting error may remain; absent systematic differences in misreporting beyond improved recognition, any non-differential misclassification would likely bias estimated effects toward zero.

[17] Through randomization inference, we assess how frequently treatment effects are at least as large as the estimated effect arising under 1,000 alternative assignments consistent with the experimental design (Gerber and Green, 2012; Heß, 2017).

[18] We compute the treatment effect among non-respondents that would be required to drive the overall intention-to-treat effect to zero. Using our preferred estimate of 8.2 percentage points (column 5, Table 2) and the formula $\hat{\tau}_{attr} = -(N_{comp}/N_{attr}) \times \hat{\tau}_{comp}$, we find that treatment would need to have reduced Open Finance adoption by 4.4 percentage points among the 750 non-respondents.

from 0 to 100 percentage points. Results are presented in Figure A1.[19] The treatment effect remains positive and statistically significant across all penalty values, including at $\delta = 100$ pp — a scenario that is logically impossible given that the control group % adoption rate is only 17% — confirming that our main result is highly robust to any plausible pattern of selective attrition. Taken together, these results provide reassurance that our main finding of a positive treatment effect on Open Finance adoption is robust to selective attrition.

Additionally, one natural question is whether the increase in Open Finance adoption shown in Table 2 may be driven by increased demand for financial services and products triggered by the first part of the video, rather than by the specific segment on Open Finance. To the extent that both the treatment and the control group were shown the first part of the video, this does not represent a threat to our identification. Nevertheless, to rule out this possibility, in the follow-up survey we elicited respondents' willingness to obtain additional financial products, including bank loans, business credit cards, personal credit cards, and microcredit. Table A4 shows no statistically significant treatment-control differences in demand for these products. These results suggest that the increase in reported adoption is unlikely to reflect a general increase in demand for financial products and is instead specific to the information provided about Open Finance.[20]

Finally, it should be noticed that reported Open Finance adoption also increased substantially during the study period among firms in the control group. Aggregate data from Open Finance Brasil indicate that individual and business consents, which are commonly used as proxies for adoption, grew by 29% and 27%, respectively, between March and July 2025. This implies a growth rate of approximately 3% over the 16 days separating our baseline and follow-up surveys. The within-control increase therefore cannot be attributed primarily to the expansion of Open Finance over the study period and may instead reflect some combination of the common information provided to both groups, priming induced by the baseline survey, and measurement error in self-reported adoption.

Individual-level Heterogeneity: We then investigate whether the effect of the informational intervention varies across microentrepreneurs with different baseline characteristics. Rather

[19] We first estimate our preferred specification (shown in column 2 of Table 2 on the observed follow-up sample and recover the original treatment effect, $\hat{\beta}$. For each penalty $p \in \{0, 10, \ldots, 100\}$, missing control outcomes are imputed as the observed follow-up mean, while missing treated outcomes are imputed as the observed follow-up mean plus $(1 - p/100)\hat{\beta}$. Thus, $p = 0$ assigns attrited treated units the full original treatment effect, while $p = 100$ assigns them no treatment effect relative to the observed mean. We then re-estimate the main specification on the completed sample. Confidence intervals are based on 1,000 bootstrap replications clustered at the geographical-cluster level.

[20] Yet, it should also be noted that the test shown in Table A4 cannot rule all the alternative explanations underlying our main results – for example, that the intervention changed how respondents recognize and report Open Finance adoption.

than specifying a separate interaction for each characteristic, we use a causal forest (Chernozhukov et al., 2018; Semenova and Chernozhukov, 2021) to flexibly estimate conditional average treatment effects (CATE).

$$\tau(x) = \mathbb{E}\left[Y_i(1) - Y_i(0) \mid X_i = x\right], \tag{2}$$

where $Y_i(1)$ and $Y_i(0)$ denote potential Open Finance adoption under treatment and control, respectively, and X_i contains the baseline characteristics of microentrepreneur i. The causal forest identifies which characteristics are most informative about variation in treatment effects without imposing a particular functional form. We use out-of-bag predictions to reduce overfitting and assess variable importance by examining how the forest's prediction loss changes when each covariate is excluded.[21]

Figure 3 shows that baseline Pix receipts are the most important predictors of treatment-effect heterogeneity. Table 3 formally tests the heterogeneity in treatment effects for the sample of microentrepreneurs we were able to re-contact at follow-up. Panel A confirms the presence of genuine treatment effect heterogeneity: the differential forest prediction (1.029, p=0.033) is statistically significant and well above zero, indicating that the causal forest recovers real variation in CATEs rather than noise. Panel B projects the CATE on ten baseline covariates using doubly-robust scores, with Benjamini-Hochberg adjusted p-values to account for multiple testing. Across all covariates, # of Pix payments received (expressed in deciles) is the only statistically significant predictor of treatment effect heterogeneity, with a coefficient of approximately 0.036 in both the joint (p=0.052) and marginal (p=0.063) specifications. This suggests that firms more deeply integrated in digital payment flows at baseline are significantly more likely to adopt Open Finance. All other covariates — including owner age and race, business account intensity, Open Finance adoption at baseline, internet access, account separation, formal firm status, long-term arrears, and microentrepreneur status — show no significant association with the CATE after multiple testing correction.[22]

Figure 4 examines treatment effects across the distribution of baseline Pix receipts. The estimated effect increases from approximately 2–4 percentage points in the lowest deciles to

[21] Following Davis and Heller (2017), we use a causal forest to flexibly characterize treatment-effect heterogeneity. We begin with 25 baseline covariates and apply an iterative screening procedure based on the drop-and-relearn measure of variable importance proposed by Bénard and Josse (2025). At each iteration, we refit the forest, recompute each covariate's importance using the change in out-of-bag R-loss when that covariate is excluded, and retain the highest-ranked variables. We successively reduce the set from 25 to 20, from 20 to 15, and finally from 15 to 10 covariates. We restrict the final best linear projection to ten covariates to maintain parsimony relative to our sample of 372 firms and 54 treatment clusters.

[22] We note that the selection of the final ten moderator variables was guided by variable importance scores from the causal forest, rather than purely theoretical grounds. Accordingly, the finding that Pix activity is the most robust moderator should be treated as exploratory and interpreted as hypothesis-generating rather than confirmatory.

approximately 17–19 percentage points in the highest deciles. Because baseline Pix receipts provide a proxy for firm size and prior engagement with digital payments, this pattern indicates that larger and more digitally active firms respond more strongly to the intervention, potentially because they face lower barriers to adopting new digital financial services. The overall picture is one of relatively homogeneous treatment effects, with digital payment intensity as the single robust predictor of heterogeneity. This result provides reassuring evidence that the average treatment effect reflects a broad response to the intervention.[23]

Although the effects are larger for firms with greater baseline Pix activity, the intervention also significantly increases Open Finance adoption among older entrepreneurs and smaller, less digitally active firms. Overall, the results indicate that the informational intervention increases Open Finance adoption broadly across the study population, while the magnitude of the response varies with firm size and prior use of digital payments. For the remaining baseline characteristics, adoption also increases significantly, but the size of the treatment effect does not vary systematically across groups.

5.2 Administrative Data

The analysis thus far has focused on Open Finance adoption and its determinants using primary survey data collected at baseline and follow-up. We now ask whether the treatment produced changes in financial activity that can be observed independently in administrative records.

To do so, we link the survey sample to administrative data from BCB. For the 801 formally registered businesses, we track activity under both the firm's tax identifier (CNPJ) and the owner's personal identifier (CPF). This allows us to capture financial activity associated both with the registered business and with the owner's personal accounts – this is an important distinction for microentrepreneurs who may conduct business transactions through either channel. For informal firms, we rely solely on the owner's CPF. We successfully match 98% of the firms in the original sample, corresponding to 1,128 firms.[24]

Table 4 examines baseline balance using the BCB outcomes measured in the six months preceding the intervention. These outcomes capture debit- and credit-card use, several dimensions of formal borrowing, and Pix activity. We find no evidence of systematic pre-intervention

[23] To assess whether differential follow-up could contribute to this pattern, we regress survey completion on treatment assignment, baseline Pix-activity quartiles, and their interactions. A joint test of the treatment-by-quartile interactions does not reject equality to zero ($p = 0.558$), providing no detectable evidence that treatment changed response rates differently across Pix quartiles. Still, because adoption is observed only for follow-up respondents, we interpret the Pix heterogeneity as exploratory and conditional on survey response.

[24] Thirteen firms in the original sample report no revenue from non-cash transactions in the administrative records and are therefore excluded from the BCB analysis. In addition, only 801 firms in our sample have a business tax identification number (CNPJ); consequently, business-level outcomes are available only for this subsample.

differences between the treatment and control groups in the administrative data – if anything, we observe that microentrepreneurs in the control group have higher credit card spending at the extensive margin.[25]

All in all, results from Table 4 corroborate the balance results based on the survey outcomes reported in Table 1. Having established the quality and comparability of the linked sample, we use the administrative data to estimate treatment effects on credit outcomes and Pix payments.

Treatment Effects on Credit Outcomes and Pix Transactions: We use administrative data from the BCB to examine whether the treatment affected microentrepreneurs' credit outcomes and potential downstream effects on Pix revenues. Rather than restricting the analysis to a common post-intervention period, we align the data around each entrepreneur's intervention date and construct pre- and post-intervention outcomes using three- and six-month windows. Consistent with the repeated-measurement approach used in previous randomized studies (Bertrand et al., 2004; McKenzie, 2012), aggregating monthly observations reduces sensitivity to transitory fluctuations and can improve precision for noisy firm outcomes. The three-month window captures responses closest to the intervention, whereas the six-month window assesses their persistence and allows slower-moving credit effects to materialize. For each window specification, we estimate

$$Y_{ip} = \alpha_i + \beta T_i \times \text{Post}_p + \sum_m \lambda_m \mathbf{1}\{M_i = m\} \times \text{Post}_p, \tag{3}$$

where i indexes owners and $p \in \{\text{pre}, \text{post}\}$ indexes the period relative to the intervention. T_i is an indicator for treatment assignment, Post_p is an indicator equal to one for the post-intervention window, and M_i denotes the owner's treatment month. The owner fixed effect α_i is defined at the tax-identification-number (CPF) level. The treatment-month-by-post terms, $\mathbf{1}\{M_i = m\} \times \text{Post}_p$, allow the pre-to-post change to vary across treatment-month cohorts. Thus, β captures the difference-in-differences effect of the intervention. Standard errors are clustered at the geographic-cluster level.

For the extensive-margin credit outcomes, Y_{ip} is an indicator equal to one if a positive credit-card limit, new credit, or positive credit-card expenses are recorded at any point during the corresponding window. The Pix outcomes are the mean number and mean value of transactions over the window. Because these outcomes are nonnegative, continuous, and include zeros, we estimate them by Poisson pseudo-maximum likelihood (PPML). Because we randomize ex-

[25] We perform a similar analysis with outcomes measured in the three months preceding the intervention. Results are shown in Table A6 in the Appendix and are similar, both in significance and magnitude, to the ones shown in Table 4.

posure to the video rather than open finance adoption itself, all downstream administrative outcomes are intention-to-treat estimates of the information campaign.

Tables 5 and 6 report the extensive-margin results for credit-card limits and expenses, respectively. A consistent pattern is that the strongest effects appear when we aggregate owner- and business-level outcomes, and when we consider the owner-level outcomes alone. By contrast, the business-level effects are less systematic and are concentrated primarily in the six-month window. This timing pattern is consistent with effects on business credit taking longer to materialize.

We begin by examining credit-card limits, which provide a proxy for the credit-supply response to the treatment in a form of financing that is particularly important for SMEs. In fact, unlike realized borrowing or expenditures, which reflect both credit demand and supply, credit limits are determined by lenders and measure the borrowing capacity made available before borrowers decide whether to use it (Chava et al., 2023; Yin, 2026). Table 5 shows that the treatment increases the probability that a positive credit-card limit is available. In the three-month window, this probability rises by 4.3 percentage points for the combined measure and by 5.1 percentage points for the owner-level measure; both estimates are significant at the 1% level. The corresponding six-month estimates are 3.6 and 4.1 percentage points and are significant at the 10% and 5% level, respectively. Relative to control-group means, these coefficients represent increases of approximately 5–7.5%. Although the six-month estimates have weaker levels of statistical significance, their magnitudes are close to the corresponding three-month estimates. This similarity – together with the larger standard errors we observe in the six-month specification – suggests that the weaker significance may reflect lower statistical power rather than a meaningful attenuation of the effects. Moreover, the stability and magnitudes of the point estimates also suggests that the results is not driven by a particular choice of time window. The concentration of the effects in the combined and owner-level measures indicates that the expansion in credit availability occurred primarily through credit cards associated with the owner's personal tax identification number. By contrast, the business-level estimate is close to zero in the three-month window. It rises to 2.9 percentage points in the six-month window but is significant only at the 10% level. This delayed and less precise response is consistent with credit supply attached directly to the business taking longer to adjust.

We next ask whether the increase in available credit-card limits was accompanied by more widespread card use. The credit-card-expense outcomes in Table 6 display a closely related pattern. In the three-month window, the probability of recording positive credit-card expenses increases by 2.7 percentage points for the combined measure and by 3.6 percentage points for the

owner-level measure; these estimates are significant at the 10% and 5% levels, respectively. In the six-month window, the effects increase modestly to 3.8 and 4.1 percentage points, and both are significant at the 5% level. In relative terms, these estimates represent increases of roughly 4–6%. Their persistence across time windows, together with the corresponding increase in credit-card limits, suggests that the additional credit availability was not entirely dormant but was accompanied by a greater likelihood of active card use. The business-level expense estimates are small and statistically insignificant in both windows. Thus, even though there is some evidence of a delayed increase in business-level credit-card limits in the six-month window, there is no corresponding detectable increase in business-card use over the same horizon.

Overall, the utilization response appears to be concentrated in cards associated with the owner rather than directly with the business. Importantly, the increase in card use does not appear to have been accompanied by greater reliance on costly revolving credit-card financing: Table 7 shows that none of the estimated effects on the probability of using revolving financing is statistically distinguishable from zero, regardless of the account type or window considered. The increases in available credit-card limits and card spending indicate that treated entrepreneurs gained access to credit cards and used them more often.

We then turn to credit operations beyond credit cards expenses, in Table 8, focusing on whether the treatment increased the probability that owners or businesses obtained new credit. In the three-month window, the probability of obtaining new credit increases by 4.8 percentage points for the combined measure and by 4.1 percentage points for the owner-level measure, with both estimates significant at the 10% level. In relative terms, these effects represent increases of approximately 9% and 8%, respectively. Neither estimate remains statistically significant in the six-month specification. Conversely, the business-level coefficient is positive but insignificant in the three-month window and rises to 6.4 percentage points in the six-month window, where it is significant at the 5% level. In relative terms, the latter estimate represents an increase of approximately 34%. The contrast between the null results for revolving credit and the increase in new credit operations suggests that the treatment did not simply encourage entrepreneurs to roll over credit-card balances at high cost. Instead, the findings are more consistent with an expansion in newly originated credit, initially through the owner and subsequently through credit issued directly to the business. The delayed business-level response may therefore reflect access to more formal financing intended for medium-term or working-capital needs, although the available data do not allow us to identify the specific purpose for which the new credit was used.

We add to the results on credit by looking at whether our intervention affected the likelihood

of microentrepreneurs to hold other types of credit products, namely personal credit, vehicle financing, and working capital loans. Results are shown in Table 9. Similar to the previous analysis, each outcome is captured as a binary indicator equal to one if the microentrepreneur holds the respective credit product within a 3-month (Panel A) or a 6-month window (Panel B). The results in Panel A (3-month window) show no statistically significant effects across any of the three credit categories. The point estimates are small and precisely estimated. This suggests that, in the short run, Open-Finance adoption does not lead microentrepreneurs to take up personal credit, vehicle financing, or working capital. Panel B (6-month window) reveals a similar picture for personal credit and vehicle financing, with coefficients remaining small and non-significant. However, we find a positive and statistically significant treatment effect for working capital credit: treated microentrepreneurs are 4.8 percentage points more likely to hold a working capital loan in the 6-month window ($p < 0.01$), relative to a control mean of 9.6%. This finding suggests that the informational benefits of Open Finance adoption may extend beyond the firm's primary credit relationship to facilitate access to working capital financing — a product particularly relevant for managing short-term liquidity needs – although these effects may take some time to materialize.

As a final step in the analysis of administrative data, we look at engagement with Pix. Pix activity provides a proxy for firm sales, particularly when transactions flow through business accounts. Table 10 provides evidence that the intervention affects the transaction-frequency margin: the treatment increases the mean number of Pix transactions through business accounts by 16.5%, an effect significant at the 1% level. More broadly, both business-account ITT coefficients—for transaction count and value—are positive, whereas both business owner-account coefficients are negative. Although only the business-account Pix count estimate is statistically significant, the consistency of this sign pattern suggests a shift in financial activity from the owner's personal account toward the firm's registered business account, rather than an aggregate expansion across all account types. This result complements the results on credit outcomes, particularly the delayed increase in credit issued directly to the business, and provides suggestive evidence that the treatment initiated financial formalization.

Overall, the results suggest that the treatment affected both access to finance and the channel through which microentrepreneurs conduct their financial activity. In the shorter run, the clearest effects are concentrated in the owner's personal accounts, with increases in credit-card limits, credit-card use, and new credit. Over the six-month window, positive effects also emerge for credit limits and new credit issued directly to the business, consistent with formal business credit taking longer to materialize. The Pix results reinforce this interpretation: the coefficients

on business-account transaction counts and values are positive, whereas the corresponding owner-account coefficients are negative, and the number of business-account transactions increases significantly. Taken together, these patterns are consistent with the treatment both easing financial constraints and encouraging a gradual shift from personal toward formal business finance and payment channels.

6 Conclusion

We study a randomized informational intervention involving 1,151 Brazilian microentrepreneurs. Subjects in the treatment group were exposed to a short video explaining how Open Finance operates, its potential benefits, and the protocols governing data sharing. Leveraging a novel dataset that combines primary data with administrative records, we find the intervention to increase reported adoption by more than 7 percentage points, or approximately 40% relative to the control-group mean. The effects are positive and statistically significant across all the principal subpopulations examined, indicating that informational frictions constrain adoption broadly rather than only among a narrow group of entrepreneurs. Nevertheless, the response is larger among firms with greater baseline Pix activity, pointing to the synergies between digital payments infrastructures and open banking initiatives.

Administrative data further reveal that the intervention increases the probability of holding a positive credit-card limit by approximately 3.6–5.1 percentage points and the probability of using a credit card by approximately 2.7–4.1 percentage points. The initial effects are concentrated in credit associated with the owner, while the six-month results also show an increase in new credit issued directly to the business as well as business' reliance on working capital. The intervention additionally raises the number of Pix transactions received through business accounts by 16.5%, although we find no statistically significant effect on transaction values.

Our paper shows that a simple informational campaign can increase Open Banking adoption and in turn significantly affect financial outcomes. Moreover, granular data on personal and business accounts allow us to further observe that the intervention first affects personal finances and then gradually reaches credit formally issued to the firm.

Taken together, our results reveal how the effects of financial-data portability interact with the organization of small-business finance. From a policy perspective, our findings suggest that the functioning and the effects of open banking depend not only on regulation and lenders' ability to process alternative data, but also on firms' capacity to activate their data-sharing rights. This demand-side margin is particularly relevant in developing credit markets, where informational frictions are pronounced, and the boundary between household and business finance is

often blurred.

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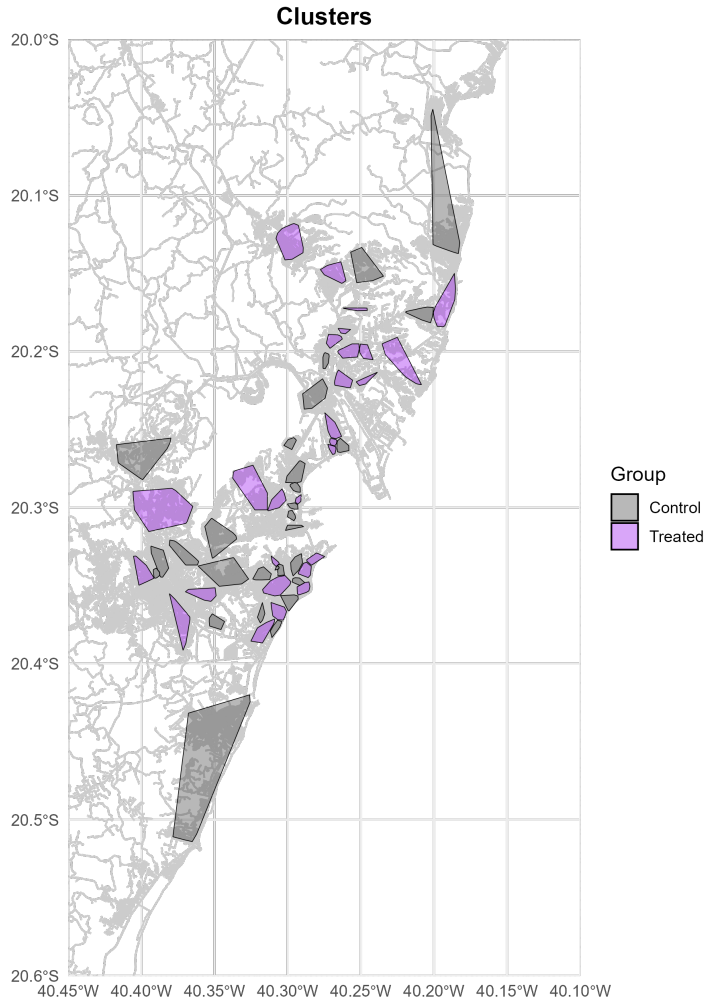
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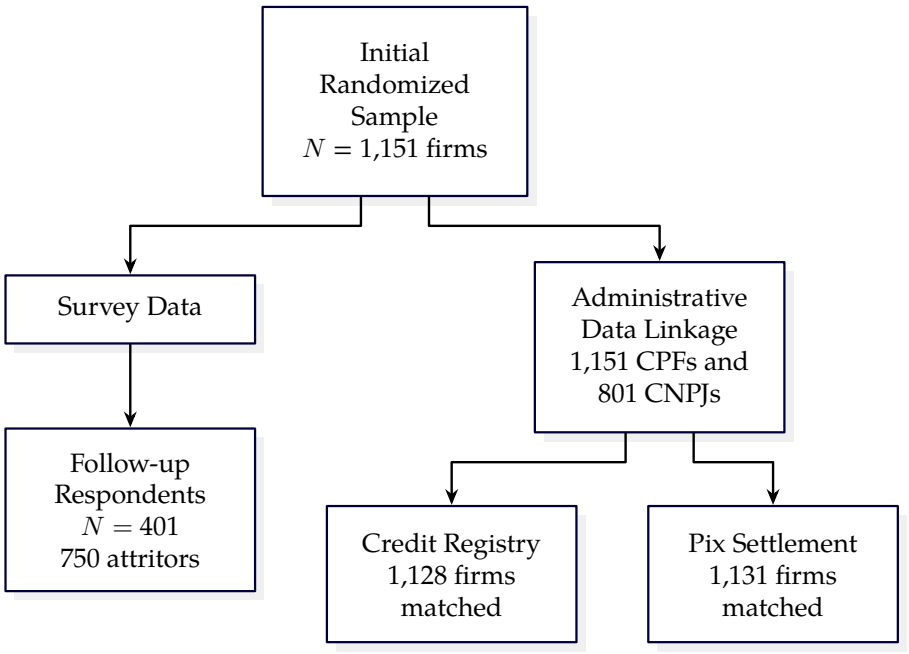
Figures and Tables

Figure 1: Cluster Map



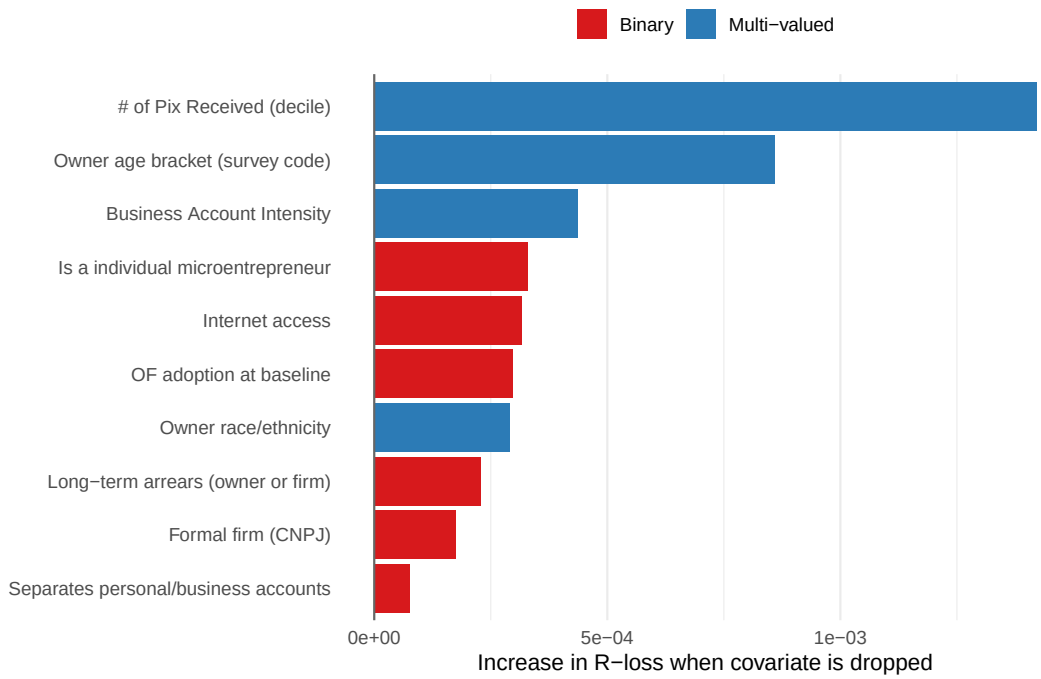
Notes. The figure depicts the geographical distribution of the 56 clusters, categorized into treated (purple) and control (gray) groups. Each polygon represents a distinct cluster area within the study region.

Figure 2: Sampling Flow



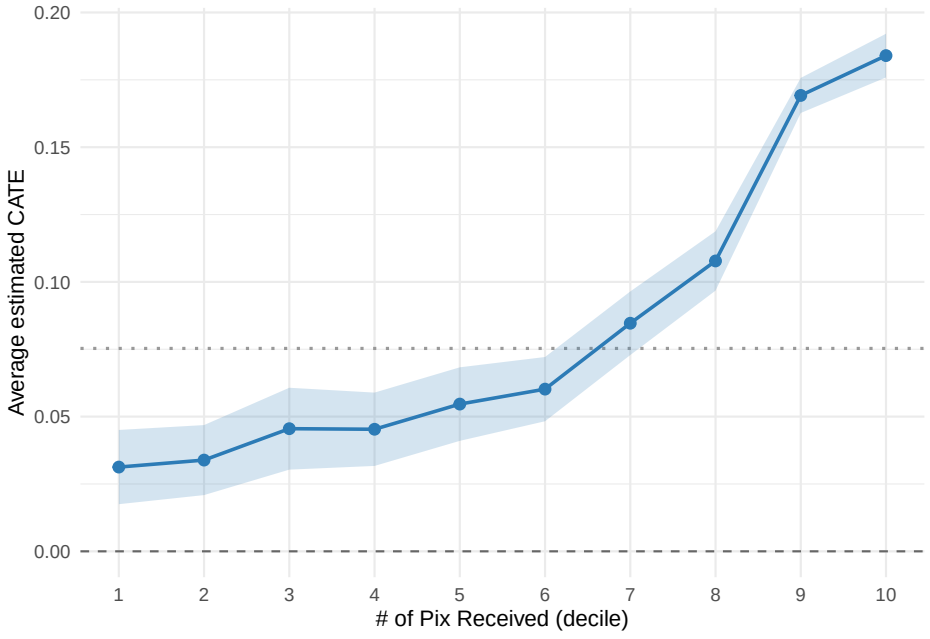
Notes. The figure reports the number of observations in our sample across different data sources (primary and administrative). CPF (*Cadastro de Pessoas Física*) is the owner's personal tax identifier, and CNPJ (*Cadastro Nacional da Pessoa Jurídica*) is the firm's business tax identifier.

Figure 3: Predictors of Treatment-Effect Heterogeneity



Notes: The figure reports ablation-based variable importance from the causal forest. Each bar shows the increase in R-loss when the forest is refit without the indicated baseline covariate. Larger positive values indicate that the covariate contributes more to predicting treatment-effect heterogeneity. Colors distinguish binary from multivalued covariates. Values close to zero are within refit noise.

Figure 4: Treatment-Effect Heterogeneity by Baseline Pix Receipts



Notes: The figure reports average out-of-bag conditional average treatment effects by decile of the baseline number of Pix transactions received. Vertical bars represent 95% confidence intervals. The horizontal dashed line indicates the average treatment effect for the full sample.

Table 1: Primary Data Balance Checks

	(1) Control	(2) Treatment	(3) (1) vs. (2)	(4) p-value (difference)
Owner age under 35	0.359	0.353	0.006	0.843
	(0.019)	(0.025)	(0.032)	
Owner age 50 or older	0.232	0.215	0.017	0.613
	(0.019)	(0.028)	(0.033)	
Female owner, yes	0.507	0.474	0.034	0.377
	(0.024)	(0.030)	(0.038)	
Completed secondary education or above	0.815	0.794	0.020	0.411
	(0.015)	(0.020)	(0.025)	
Completed higher education	0.193	0.189	0.005	0.897
	(0.023)	(0.027)	(0.035)	
Mixed/brown race	0.536	0.517	0.019	0.553
	(0.019)	(0.026)	(0.032)	
White race	0.251	0.245	0.006	0.843
	(0.021)	(0.022)	(0.030)	
Number of workers	1.359	1.447	-0.088	0.619
	(0.119)	(0.132)	(0.176)	
Revenue last 30 days (BRL)	22050.536	23637.618	-1587.082	0.553
	(1893.426)	(1895.741)	(2656.509)	
Number of bank accounts linked to CNPJ	1.373	1.306	0.066	0.418
	(0.054)	(0.062)	(0.081)	
Number of bank accounts linked to CPF	2.755	2.766	-0.011	0.920
	(0.069)	(0.084)	(0.107)	
Number of total bank accounts	4.129	4.072	0.057	0.653
	(0.080)	(0.099)	(0.127)	
Adhered to Open Finance, yes	0.076	0.068	0.008	0.597
	(0.010)	(0.011)	(0.015)	
Balance sheet updated at least monthly	0.707	0.757	-0.050	0.108
	(0.023)	(0.020)	(0.030)	
Has internet access, yes	0.937	0.932	0.005	0.784
	(0.012)	(0.014)	(0.019)	
Separates personal/business accounts, yes	0.673	0.677	-0.004	0.913
	(0.030)	(0.025)	(0.039)	
Uses Pix frequently (daily or weekly)	0.707	0.689	0.018	0.572
	(0.020)	(0.025)	(0.032)	
Asked for a loan, yes	0.143	0.151	-0.008	0.711
	(0.014)	(0.015)	(0.020)	
Was given a loan, yes	0.124	0.130	-0.006	0.751
	(0.014)	(0.013)	(0.019)	
Formal firm, yes	0.712	0.689	0.023	0.492
	(0.022)	(0.025)	(0.033)	
Low trust in banking system	0.415	0.438	-0.022	0.404
	(0.017)	(0.021)	(0.026)	
High trust in banking system	0.585	0.562	0.022	0.404
	(0.017)	(0.021)	(0.026)	
Low risk tolerance	0.285	0.287	-0.002	0.941
	(0.015)	(0.019)	(0.024)	
High risk tolerance	0.295	0.300	-0.005	0.838
	(0.016)	(0.020)	(0.026)	
N	621	530	1151	

Table 2: Treatment Effects on Open Finance Adoption

Dependent Variable: Open Finance Adoption					
	(1)	(2)	(3)	(4)	(5)
Treated	0.064*	0.071**	0.071**	0.072**	0.082**
	(0.036)	(0.033)	(0.033)	(0.034)	(0.033)
Mean of OF Adoption (control group)	0.170	0.170	0.170	0.170	0.170
SD of OF Adoption (control group)	0.376	0.376	0.376	0.376	0.376
Baseline OF		✓	✓	✓	✓
PDS LASSO Controls			✓	✓	✓
City FE				✓	✓
Sector FE				✓	✓
Month FE (FUP & BL)					✓
RI p-value (Treat)	0.071	0.033	0.033	0.040	0.025
Observations	401	401	401	401	401
R^2	0.006	0.295	0.295	0.299	0.317

Notes. The table reports coefficients obtained regressing a dummy for Open Finance adoption at follow up (which equals one if at follow-up the respondent reports having adopted Open Finance) over the dummy for treatment assignment. We report the p-value from the Randomization Inference (with 1,000 cluster-preserving permutations). Standard errors are clustered at the geographic-cluster level.

Specifications: (1) No covariates; (2) Baseline OF; (3) Baseline OF + PDS LASSO; (4) Baseline OF + PDS LASSO + City & Sector fixed effects; (5) Baseline OF + PDS LASSO + follow-up and baseline month fixed effects. PDS LASSO indicates that high-dimensional controls were selected via `pdslasso`. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Heterogeneity Analysis

Panel A: Omnibus test for any heterogeneity

	Estimate	Std. Error	t-stat	p-value
Differential forest prediction	1.029**	0.557	1.85	0.033

Panel B: Best linear projection of the CATE on baseline covariates

	Joint (partial)			Marginal (one at a time)		
	Estimate	S.E.	p^{BH}	Estimate	S.E.	p^{BH}
Owner age bracket (survey code)	−0.027	0.016	0.463	−0.026	0.015	0.391
Owner race/ethnicity	0.022	0.043	0.878	0.001	0.042	0.994
Business Account Intensity	0.054	0.071	0.878	0.074	0.068	0.652
OF adoption at baseline	−0.071	0.125	0.878	−0.001	0.118	0.994
Internet access	−0.014	0.182	0.940	0.049	0.171	0.994
Separates personal/business accounts	0.063	0.097	0.878	0.074	0.086	0.652
Formal firm (CNPJ)	−0.032	0.094	0.916	0.069	0.072	0.652
Long-term arrears (owner or firm)	0.011	0.123	0.940	0.013	0.114	0.994
Is an individual microentrepreneur	0.119	0.088	0.592	0.091	0.078	0.652
# of Pix received (decile)	0.036*	0.013	0.052	0.036*	0.013	0.063

Note: $N = 372$ firms in 54 clusters; standard errors are clustered at the geographical cluster unit. Panel A reports the calibration test of Chernozhukov, Demirer, Duflo and Fernández-Val (2018) as implemented in `grf`: a differential forest prediction coefficient of 1 indicates the forest recovers real variation in treatment effects, while a coefficient at or below 0 indicates no detectable heterogeneity. The p-value for the omnibus test in Panel A is derived from a one-sided (directional) test, as the null hypothesis is that the differential forest prediction is less than or equal to zero. Panel B should be read conditional on Panel A. Panel B reports the best linear projection of the conditional average treatment effect on baseline covariates (Semenova and Chernozhukov 2021), estimated from doubly-robust scores. The joint columns include all covariates simultaneously and are therefore partial associations, holding the remaining covariates fixed; the marginal columns project on one covariate at a time and are unconditional associations. The two differ when covariates are correlated. Reported p-values are Benjamini–Hochberg adjusted across the 10 covariates within each column to account for multiple testing; under the null roughly 1 covariate per column would cross $p < 0.10$ unadjusted. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: Administrative Data Balance Checks, 6-Month Pre-Intervention Window

	Extensive margin (any use)				Intensive margin (BRL, monthly mean)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Control	Treatment	(2)−(1)	p-value	Control	Treatment	(6)−(5)	p-value	N
Credit card limit available									
Owner and business account	0.747	0.716	-0.031	0.222	6,779	7,837	1,058	0.514	1,131
	(0.013)	(0.021)	(0.025)		(835)	(1,380)	(1,612)		
Owner account	0.726	0.695	-0.030	0.268	5,731	6,704	972	0.474	1,131
	(0.014)	(0.023)	(0.027)		(734)	(1,130)	(1,347)		
Business account	0.268	0.241	-0.027	0.361	1,450	1,639	189	0.711	801
	(0.017)	(0.024)	(0.030)		(264)	(433)	(507)		
Credit card expenses									
Owner and business account	0.654	0.590	-0.063	0.024	6,570	7,783	1,213	0.563	1,131
	(0.014)	(0.023)	(0.027)		(957)	(1,849)	(2,082)		
Owner account	0.639	0.577	-0.062	0.028	5,798	7,306	1,508	0.444	1,131
	(0.015)	(0.023)	(0.027)		(858)	(1,760)	(1,958)		
Business account	0.161	0.136	-0.026	0.263	1,068	688	-380	0.181	801
	(0.015)	(0.017)	(0.023)		(230)	(160)	(280)		
Revolving credit card financing									
Owner and business account	0.491	0.437	-0.054	0.051	776	1,288	512	0.051	1,131
	(0.019)	(0.020)	(0.027)		(133)	(219)	(257)		
Owner account	0.468	0.423	-0.045	0.132	639	1,066	427	0.054	1,131
	(0.021)	(0.020)	(0.029)		(89)	(198)	(217)		
Business account	0.093	0.091	-0.002	0.922	190	320	131	0.467	801
	(0.013)	(0.012)	(0.018)		(91)	(154)	(179)		
New credit									
Owner and business account	0.606	0.600	-0.006	0.829	1,960	2,030	70	0.898	1,131
	(0.021)	(0.020)	(0.029)		(386)	(380)	(541)		
Owner account	0.573	0.573	0.000	0.993	1,153	1,247	94	0.733	1,131
	(0.022)	(0.021)	(0.031)		(207)	(179)	(273)		
Business account	0.186	0.147	-0.040	0.210	1,117	1,132	15	0.978	801
	(0.020)	(0.024)	(0.031)		(305)	(449)	(543)		
Pix received, value									
Owner and business account					6,086	6,695	609	0.454	1,131
					(410)	(696)	(808)		
Owner account					3,799	4,311	513	0.418	1,131
					(266)	(569)	(628)		
Business account					3,165	3,446	281	0.731	801
					(461)	(669)	(812)		
Pix received, number (monthly mean, count)									
Owner and business account					90.5	96.7	6.2	0.628	1,131
					(9.0)	(9.0)	(12.7)		
Owner account					49.0	58.7	9.7	0.199	1,131
					(4.2)	(6.1)	(7.4)		
Business account					57.4	54.9	-2.5	0.863	801
					(10.2)	(9.9)	(14.2)		
Joint test, all firms				0.088				0.273	

Notes. Variables are measured over the 6 months before each firm's baseline month, with months without records counted as zero. Extensive-margin outcomes (columns 1–4) are indicators equal to one if the variable is positive in any month of the window; for Pix, the number of transfers received is reported as a monthly mean. Intensive-margin outcomes (columns 5–8) are monthly means of BRL values over the window. For intensive-margin outcomes, the combined row is the sum of the owner and business accounts; for extensive-margin outcomes, the combined row is an indicator equal to one if there is positive activity in either account. Owner-and-business and owner rows cover all firms (N = 1,131); business-account rows cover formal firms only (N = 801). Columns (1)–(2) and (5)–(6) report group means, columns (3) and (7) the treatment–control difference, and columns (4) and (8) its p-value; standard errors in parentheses are clustered at the geographic-cluster level. The joint tests regress treatment on all listed variables and report the p-value of the clustered F-test.

Table 5: Treatment Effects on Credit Card Limits - Extensive Margin

Panel A: 3-month window

	Cred. Card Limits (All) Flag	Cred. Card Limits (Owner) Flag	Cred. Card Limits (Business) Flag
	(1)	(2)	(3)
Treatment × Post	0.043***	0.051***	0.004
	(0.015)	(0.014)	(0.012)
Observations	2256	2256	1600
Firms	1128	1128	800
R Squared	0.919	0.915	0.952
Control mean	0.707	0.685	0.248
Owner FE	✓	✓	✓
Treatment Month X Post FE	✓	✓	✓

Panel B: 6-month window

	Cred. Card Limits (All) Flag	Cred. Card Limits (Owner) Flag	Cred. Card Limits (Business) Flag
	(1)	(2)	(3)
Treatment × Post	0.036*	0.041**	0.029*
	(0.021)	(0.020)	(0.016)
Observations	2256	2256	1600
Firms	1128	1128	800
R Squared	0.888	0.889	0.920
Control mean	0.750	0.728	0.269
Owner FE	✓	✓	✓
Treatment Month X Post FE	✓	✓	✓

Notes: The specifications use 3- and 6-month windows centered around the treatment date. The outcome variables are indicator variables equal to one if a positive credit-card limit was recorded at any point during the corresponding window, and zero otherwise. Owner FE denotes fixed effects for the owner's tax identification number (CPF). Treatment Month × Post FE denotes fixed effects for each treatment month interacted with a Post indicator equal to one after the intervention and zero before it. Standard errors are clustered at the geographic-cluster level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Treatment Effects on Credit Card Expenses - Extensive Margin

Panel A: 3-month window

	Cred. Card Expenses (All) Flag	Cred. Card Expenses (Owner) Flag	Cred. Card Expenses (Business) Flag
	(1)	(2)	(3)
Treatment × Post	0.027*	0.036**	-0.001
	(0.015)	(0.016)	(0.014)
Observations	2256	2256	1600
Firms	1128	1128	800
R Squared	0.917	0.916	0.928
Control mean	0.623	0.608	0.157
Owner FE	✓	✓	✓
Treatment Month X Post FE	✓	✓	✓

Panel B: 6-month window

	Cred. Card Expenses (All) Flag	Cred. Card Expenses (Owner) Flag	Cred. Card Expenses (Business) Flag
	(1)	(2)	(3)
Treatment × Post	0.038**	0.041**	0.011
	(0.018)	(0.018)	(0.017)
Observations	2256	2256	1600
Firms	1128	1128	800
R Squared	0.886	0.887	0.899
Control mean	0.656	0.641	0.162
Owner FE	✓	✓	✓
Treatment Month X Post FE	✓	✓	✓

Notes: The specifications use 3- and 6-month windows centered around the treatment date. The outcome variables are indicator variables equal to one if positive credit-card expenses were recorded at any point during the corresponding window, and zero otherwise. Owner FE denotes fixed effects for the owner's tax identification number (CPF). Treatment Month × Post FE denotes fixed effects for each treatment month interacted with a Post indicator equal to one after the intervention and zero before it. Standard errors are clustered at the geographic-cluster level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Treatment Effects on Revolving Credit Card Financing - Extensive Margin

Panel A: 3-month window

	Revolving C. Card (All) Flag	Revolving C. Card (Owner) Flag	Revolving C. Card (Business) Flag
	(1)	(2)	(3)
Treatment × Post	0.020	0.021	-0.008
	(0.021)	(0.020)	(0.012)
Observations	2256	2256	1600
Firms	1128	1128	800
R Squared	0.866	0.866	0.883
Control mean	0.435	0.418	0.075
Owner FE	✓	✓	✓
Treatment Month X Post FE	✓	✓	✓

Panel B: 6-month window

	Revolving C. Card (All) Flag	Revolving C. Card (Owner) Flag	Revolving C. Card. (Business) Flag
	(1)	(2)	(3)
Treatment × Post	0.032	0.027	0.008
	(0.026)	(0.024)	(0.020)
Observations	2256	2256	1600
Firms	1128	1128	800
R Squared	0.849	0.853	0.840
Control mean	0.493	0.470	0.093
Owner FE	✓	✓	✓
Treatment Month X Post FE	✓	✓	✓

Notes: The specifications use 3- and 6-month windows centered around the treatment date. The outcome variables are indicator variables equal to one if positive revolving credit-card financing was recorded at any point during the corresponding window, and zero otherwise. Owner FE denotes fixed effects for the owner's tax identification number (CPF). Treatment Month × Post FE denotes fixed effects for each treatment month interacted with a Post indicator equal to one after the intervention and zero before it. Standard errors are clustered at the geographic-cluster level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Treatment Effects on New Credit - Extensive Margin

Panel A: 3-month window

	New credit (All) Flag	New credit (Owner) Flag	New credit (Business) Flag
	(1)	(2)	(3)
Treatment × Post	0.048*	0.041*	0.020
	(0.026)	(0.024)	(0.023)
Observations	2256	2256	1600
Firms	1128	1128	800
R Squared	0.809	0.815	0.808
Control mean	0.530	0.501	0.146
Owner FE	✓	✓	✓
Treatment Month X Post FE	✓	✓	✓

Panel B: 6-month window

	New credit (All) Flag	New credit (Owner) Flag	New credit (Business) Flag
	(1)	(2)	(3)
Treatment × Post	0.029	0.020	0.064**
	(0.026)	(0.023)	(0.026)
Observations	2256	2256	1600
Firms	1128	1128	800
R Squared	0.803	0.802	0.796
Control mean	0.608	0.575	0.187
Owner FE	✓	✓	✓
Treatment Month X Post FE	✓	✓	✓

Notes: The specifications use 3- and 6-month windows centered around the treatment date. The outcome variables are indicator variables equal to one if new credit was recorded at any point during the corresponding window, and zero otherwise. Owner FE denotes fixed effects for the owner's tax identification number (CPF). Treatment Month × Post FE denotes fixed effects for each treatment month interacted with a Post indicator equal to one after the intervention and zero before it. Standard errors are clustered at the geographic-cluster level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Treatment Effects on Other Types of Credit - Extensive Margin

Panel A: 3-month window

	Personal Cred Flag	Vehicles Owner Flag	Working Capital Flag
	(1)	(2)	(3)
Treatment × Post	0.013	-0.005	0.026
	(0.027)	(0.010)	(0.017)
Observations	2256	2256	1600
Firms	1128	1128	800
R Squared	0.775	0.496	0.660
Control mean	0.224	0.016	0.059

Panel B: 6-month window

	Personal Cred Flag	Vehicles Owner Flag	Working Capital Flag
	(1)	(2)	(3)
Treatment × Post	0.017	0.001	0.048***
	(0.025)	(0.012)	(0.017)
Observations	2256	2256	1600
Firms	1128	1128	800
R Squared	0.758	0.509	0.701
Control mean	0.298	0.028	0.096

Notes: The specifications use 3- and 6-month windows centered around the treatment date. The outcome variables are all a binary indicator equal to one if the owner holds the respective credit product. Owner FE denotes fixed effects for the owner's tax identification number (CPF). Treatment Month × Post FE denotes fixed effects for each treatment month interacted with a Post indicator equal to one after the intervention and zero before it. Standard errors are clustered at the geographic-cluster level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

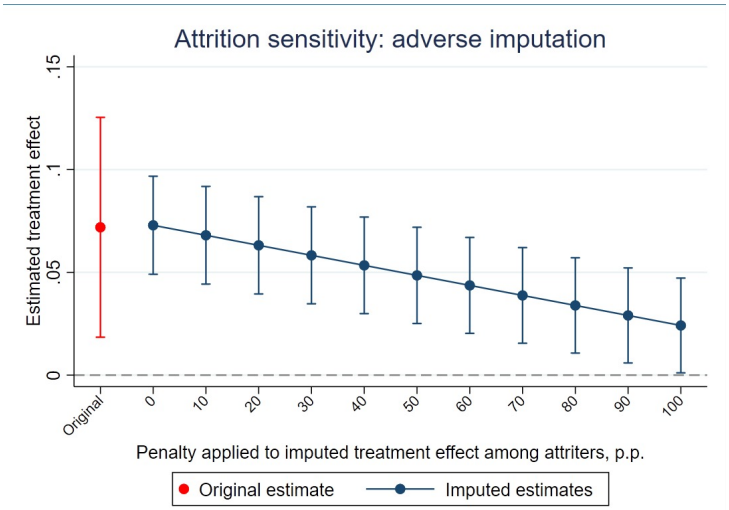
Table 10: Treatment Effects on Pix Transactions - Count and Value

	Pix (All)		Pix (Owner)		Pix (Business)	
	Count	Value	Count	Value	Count	Value
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	0.029	-0.011	-0.056	-0.049	0.153***	0.054
	(0.041)	(0.029)	(0.067)	(0.040)	(0.059)	(0.048)
Observations	2262	2262	2236	2236	1292	1292
Firms	1131	1131	1118	1118	646	646
Control mean, pre	824.9	57942	403.6	32591	754.3	45580
Owner FE	✓	✓	✓	✓	✓	✓
Treatment Month × Post FE	✓	✓	✓	✓	✓	✓

Notes: The outcome variables are the number and value of Pix transactions measured over a 6-month pre-treatment window and a separate 6-month post-treatment window. Control pre-treatment means represent 6-month totals. The specifications are estimated using Poisson pseudo-maximum likelihood (PPML). The PPML estimator mechanically drops firms with zero transactions across all periods, accounting for the varying sample sizes across columns. Owner FE denotes fixed effects for the owner's tax identification number (CPF). Treatment Month × Post FE denotes fixed effects for each treatment month interacted with a Post indicator equal to one after the intervention and zero before it. Standard errors are clustered at the geographic-cluster level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

A Online Appendix: Figures and Tables

Figure A1: Attrition sensitivity: Adverse Imputation



Notes. The figure shows an attrition sensitivity exercise. Missing control outcomes are imputed as the observed follow-up mean. Missing treated outcomes are imputed as the observed follow-up mean plus $(1 - p/100)\hat{\beta}$, where $\hat{\beta}$ is the original observed-sample treatment effect and p is the penalty shown on the horizontal axis. Thus, $p = 0$ assigns attrited treated units the full original treatment effect, while $p = 100$ assigns them no treatment effect. Vertical bars are 95 percent confidence intervals from 1,000 bootstrap replications clustered by geographical cluster.

Table A1: Attrition Analysis

VARIABLES	(1) Model 1: Treatment only	(2) Model 2: Treat + Covariates
Treatment assignment	0.0477.	0.0445
	(0.0304)	(0.0311)
Owner age under 35		-0.0158
		(0.0348)
Owner age 50 or older		-0.0077
		(0.0376)
Female owner, yes		-0.0174
		(0.0291)
Completed secondary education or above		-0.0683
		(0.0396)
Completed higher education		-0.0268
		(0.0335)
Mixed/brown race		0.0233
		(0.0349)
White race		0.0157
		(0.0402)
Number of workers		0.0093
		(0.0076)
Number of bank accounts linked to CPF		0.0142
		(0.0156)
Number of total bank accounts		-0.0138
		(0.0141)
Adhered to Open Finance, yes		-0.0441
		(0.0581)
Balance sheet updated at least monthly		-0.0264
		(0.0408)
Has internet access, yes		0.0470
		(0.0549)
Separates personal/business accounts, yes		0.0265
		(0.0332)
Uses Pix frequently (daily or weekly)		0.0120
		(0.0341)
Asked for a loan, yes		-0.1420
		(0.0888)
Was given a loan, yes		0.0582
		(0.1027)
Formal firm, yes		0.0814
		(0.0326)
Low trust in banking system		0.0639
		(0.0248)
Low risk tolerance		0.0026
		(0.0329)
High risk tolerance		0.0299
		(0.0345)
Observations	1151	1149

Note: Robust standard errors clustered at the geographical cluster unit in parentheses. The dependent variable is an indicator equal to 1 if the firm was surveyed at baseline but did not complete the follow-up survey. Column (1) presents unadjusted p-values. Column (2) presents on Benjamini-Hochberg adjusted p-values across 21 covariates. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A2: Attrition Check Balance (firms only surveyed at baseline vs. firms surveyed both at baseline and follow-up)

	(1) Only Baseline	(2) Baseline and Follow-up	(3) (1) vs. (2)	(4) p-value (difference)
Owner age under 35	0.345	0.377	-0.031.	0.326
	(0.020)	(0.024)	(0.032)	
Owner age 50 or older	0.231	0.212	0.019.	0.476
	(0.021)	(0.019)	(0.026)	
Female owner, yes	0.483	0.509	-0.026.	0.384
	(0.022)	(0.026)	(0.030)	
Completed secondary education or above	0.787	0.840	-0.054**	0.025
	(0.015)	(0.018)	(0.023)	
Completed higher education	0.181	0.209	-0.028.	0.212
	(0.018)	(0.024)	(0.022)	
Mixed/brown race	0.535	0.514	0.021.	0.523
	(0.018)	(0.028)	(0.033)	
White race	0.247	0.252	-0.005.	0.856
	(0.016)	(0.027)	(0.028)	
Number of workers	1.423	1.357	0.066.	0.545
	(0.089)	(0.122)	(0.108)	
Revenue last 30 days (BRL)	22778.201	22764.699	13.501.	0.995
	(1695.919)	(1560.305)	(1977.115)	
Number of bank accounts linked to CNPJ	1.322	1.380	-0.058.	0.285
	(0.044)	(0.055)	(0.054)	
Number of bank accounts linked to CPF	2.752	2.776	-0.024.	0.797
	(0.055)	(0.089)	(0.091)	
Number of total bank accounts	4.073	4.157	-0.084.	0.447
	(0.066)	(0.105)	(0.110)	
Adhered to Open Finance, yes	0.064	0.087	-0.023.	0.178
	(0.009)	(0.014)	(0.017)	
Balance sheet updated at least monthly	0.716	0.756	-0.040.	0.266
	(0.023)	(0.024)	(0.035)	
Has internet access, yes	0.928	0.948	-0.020.	0.158
	(0.011)	(0.012)	(0.014)	
Separates personal/business accounts, yes	0.663	0.698	-0.036.	0.207
	(0.021)	(0.028)	(0.028)	
Uses Pix frequently (daily or weekly)	0.700	0.696	0.004.	0.892
	(0.020)	(0.025)	(0.031)	
Asked for a loan, yes	0.131	0.177	-0.046**	0.043
	(0.012)	(0.018)	(0.022)	
Was given a loan, yes	0.115	0.150	-0.035.	0.117
	(0.012)	(0.017)	(0.022)	
Formal firm, yes	0.715	0.676	0.039.	0.150
	(0.019)	(0.024)	(0.027)	
Low trust in banking system	0.452	0.377	0.075***	0.008
	(0.015)	(0.023)	(0.027)	
High trust in banking system	0.548	0.623	-0.075***	0.008
	(0.015)	(0.023)	(0.027)	
Low risk tolerance	0.291	0.277	0.014.	0.606
	(0.016)	(0.019)	(0.027)	
High risk tolerance	0.301	0.289	0.012.	0.691
	(0.018)	(0.021)	(0.030)	
N	750	401		

Table A3: Lee (2009) Bounds for Selective Attrition

	Lower Bound	Upper Bound
Estimate	0.039	0.187
95% CI lower	-0.051	0.012
95% CI upper	0.129	0.362
N	1151	

Note: Lee (2009) bounds on the treatment effect on Open Finance adoption. Lower (upper) bound assumes worst (best) case selective attrition.

Table A4: Desired Sources of Financing

	(1) BankLoan	(2) BusinessCard	(3) PersonalCard	(4) SupplierCred	(5) CreditUnion	(6) Microcredit	(7) NoInterest
Treatment	0.041	0.059	0.029	0.059	-0.022	0.046	-0.004
	(0.055)	(0.055)	(0.055)	(0.042)	(0.035)	(0.039)	(0.041)
Baseline (ANCOVA)	0.274***	0.396***	0.337***	0.253**	0.500***	0.391***	0.319***
	(0.067)	(0.052)	(0.068)	(0.113)	(0.104)	(0.080)	(0.053)
Mean (Control, FUP)	0.213	0.291	0.278	0.204	0.122	0.196	0.235
SD (Control, FUP)	0.410	0.455	0.449	0.404	0.328	0.398	0.425
RI p-value	0.462	0.260	0.600	0.208	0.546	0.326	0.915
Observations	401	401	401	401	391	401	401

Notes. Each column reports the coefficient on *Treatment* from the same specification: ANCOVA controls, PDS-selected controls, City & Sector fixed effects, and Week fixed effects in both follow-up and baseline. Standard errors are clustered at the geographic cluster level. ANCOVA includes the baseline value of the outcome and a dummy for missing baseline; missing baselines are set to zero prior to including the missing indicator. High-dimensional controls were selected via `pdslasso`. RI p-value is from randomization inference with 1,000 cluster-preserving permutations. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: BCB Attrition Check-1,3,and 6 Months Before Baseline Survey

	(1)	(2)	(3)
	1 month before the baseline survey	3 months before the baseline survey	6 months before the baseline survey
Treatment	0.00216	0.00323	0.00539
	(0.00488)	(0.00675)	(0.00959)
Observations	1135	1135	1135
Attrited Firms	10	15	25

Clustered standard errors at the geographical unit level. footnotesize * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Administrative Data Balance Checks, 3-Month Pre-Intervention Window

	Extensive margin (any use)				Intensive margin (BRL, monthly mean)				
	(1) Control	(2) Treatment	(3) (2)−(1)	(4) p-value	(5) Control	(6) Treatment	(7) (6)−(5)	(8) p-value	(9) N
Credit card limit available									
Owner and business account	0.704	0.678	-0.026	0.350	6,888	8,140	1,252	0.461	1,131
	(0.017)	(0.022)	(0.028)		(810)	(1,477)	(1,685)		
Owner account	0.683	0.653	-0.030	0.321	5,792	7,019	1,227	0.396	1,131
	(0.018)	(0.024)	(0.030)		(709)	(1,245)	(1,433)		
Business account	0.248	0.219	-0.029	0.291	1,517	1,620	103	0.829	801
	(0.016)	(0.022)	(0.027)		(283)	(384)	(477)		
Credit card expenses									
Owner and business account	0.621	0.565	-0.056	0.042	6,801	7,783	982	0.649	1,131
	(0.015)	(0.022)	(0.027)		(1,016)	(1,887)	(2,143)		
Owner account	0.606	0.548	-0.058	0.030	5,964	7,262	1,298	0.521	1,131
	(0.015)	(0.021)	(0.026)		(905)	(1,796)	(2,011)		
Business account	0.157	0.133	-0.024	0.302	1,159	753	-405	0.182	801
	(0.015)	(0.017)	(0.023)		(257)	(155)	(300)		
Revolving credit card financing									
Owner and business account	0.433	0.389	-0.045	0.102	801	1,382	581	0.072	1,131
	(0.020)	(0.018)	(0.027)		(127)	(291)	(317)		
Owner account	0.417	0.379	-0.038	0.182	658	1,180	522	0.080	1,131
	(0.020)	(0.019)	(0.028)		(93)	(277)	(293)		
Business account	0.075	0.075	0.000	0.991	198	293	94	0.576	801
	(0.014)	(0.011)	(0.017)		(80)	(147)	(168)		
New credit									
Owner and business account	0.529	0.519	-0.010	0.710	2,024	2,147	123	0.846	1,131
	(0.018)	(0.018)	(0.026)		(391)	(492)	(629)		
Owner account	0.499	0.489	-0.011	0.701	1,227	1,293	66	0.819	1,131
	(0.021)	(0.018)	(0.028)		(225)	(180)	(288)		
Business account	0.145	0.127	-0.018	0.502	1,103	1,235	132	0.854	801
	(0.016)	(0.022)	(0.027)		(305)	(643)	(712)		
Pix received, value									
Owner and business account					5,832	6,705	873	0.313	1,131
					(411)	(752)	(858)		
Owner account					3,574	4,271	697	0.286	1,131
					(235)	(602)	(647)		
Business account					3,125	3,520	395	0.634	801
					(457)	(685)	(824)		
Pix received, number (monthly mean, count)									
Owner and business account					88.2	97.2	9.0	0.501	1,131
					(9.7)	(9.1)	(13.3)		
Owner account					45.9	57.0	11.2	0.116	1,131
					(4.0)	(5.8)	(7.0)		
Business account					58.6	58.1	-0.5	0.973	801
					(10.8)	(10.5)	(15.1)		
Joint test, all firms (combined and owner rows)				0.261				0.214	
Joint test, formal firms (all rows)				0.379				0.028	

Notes. Variables are measured over the 3 months before each firm's baseline month, with months without records counted as zero. Extensive-margin outcomes (columns 1–4) are indicators equal to one if the variable is positive in any month of the window; for Pix, the number of transfers received is reported as a monthly mean. Intensive-margin outcomes (columns 5–8) are monthly means of BRL values over the window. For intensive-margin outcomes, the combined row is the sum of the owner and business accounts; for extensive-margin outcomes, the combined row is an indicator equal to one if there is positive activity in either account. Owner-and-business and owner rows cover all firms (N = 1,131); business-account rows cover formal firms only (N = 801). Columns (1)–(2) and (5)–(6) report group means, columns (3) and (7) the treatment–control difference, and columns (4) and (8) its p-value; standard errors in parentheses are clustered at the geographic-cluster level. The joint tests regress treatment on all listed variables and report the p-value of the clustered F-test.

B Online Appendix: Intervention Details

B.1 Script of the video (English Translation)

Common Section (Treated and Control)

Scene 1: Opening

ANIMATION:

1. Map of Brazil with emojis spread across the territory, representing microentrepreneur businesses (nails, clothes, food, money) .
2. Lines connecting regions.
3. Text on screen: "Less Complicated"

NARRATOR: *"Did you know that access to credit and financial technologies can transform your business? Today, with initiatives from the Central Bank, doing business in Brazil is becoming less complicated"*

Scene 2: Recent Developments

ANIMATION:

1. Cell phone showing a completed Pix transaction.
2. Green bar chart in ascending order showing increasing revenue.
3. Animation of bank balance rising with numbers changing in an increasing manner.
4. Chart showing payment history: entries into the account as positive numbers and exits as negative numbers - at the end a horizontal line goes from left to right and below it appears "+$$$$" in blue.

NARRATOR:*"Tools like Pix are an excellent start. With it, you can receive instant payments, even outside of business hours, directly into your account, without the need for a card machine or additional costs. In addition, it is possible to better organize cash flow, since the amounts are deposited into your account immediately, facilitating the day-to-day financial control of your business."*

Scene 3: Financial Technologies and Access to Credit

ANIMATION:

1.

NARRATOR: *"In addition to Pix, there are several technologies that can make life easier for microentrepreneurs. Tools like [Placeholder: tool 1] help organize your finances in a simple and efficient way, allowing greater control over inflows and outflows. [Placeholder: tool 2] offers access to personalized credit options, adapted to your specific needs. Other solutions, like [Placeholder: tool 3], connect you to partnership and funding opportunities, opening doors for your business to grow even further."*

Scene 4: Closing - Control

ANIMATION:

1. Different microentrepreneurs smiling in their businesses.
2. Arrows grow vertically around them.
3. On-screen text: "Explore what technology can do for the future of your business!"

NARRATOR: *"Take advantage of these opportunities and explore what technology can do for you and the future of your business!"*

Part Two: Open Finance (Treated Only)

Scene 5: Introduction to Open Finance

ANIMATION:

1. Padlock opening.
2. Lines connecting financial institutions to microentrepreneurs.
3. Text on screen: "Open Finance: The new way to access credit and financial products."

NARRATOR: *"But it doesn't stop there. Now, the Central Bank is bringing a new initiative to make it even easier for you to access credit and financial products: Open Finance. But what is that?"*

Scene 6: Explanation about Open Finance and Security

ANIMATION:

1. Cell phone showing generic step-by-step instructions for activating Open Finance.
2. Seal with text on screen: "Regulated by the Central Bank."

NARRATOR: *"Open Finance is a way for you to share financial information, such as credit history and bank transactions, with institutions you trust, in a safe and transparent way. And don't worry: Open Finance is regulated by the Central Bank, ensuring that your data is protected."*

Scene 7: Benefits of Open Finance

ANIMATION:

1. Chart showing red lines falling to symbolize interest rate reduction.
2. Screen with messages like "Credit approved" and "Reduced rate available."
3. Written on the screen: "You're in control: share whenever you want and with the institutions you trust."

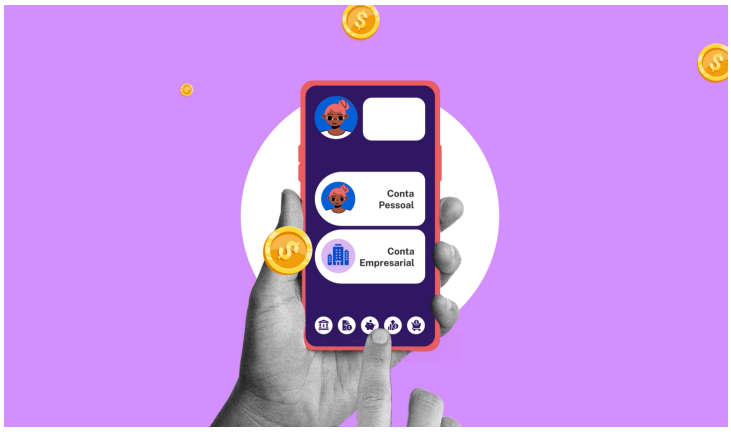
NARRATOR: *"By sharing your information responsibly, you can access better credit terms, personalized financial products and services that truly meet your needs. Now, you have complete control over your data: you decide what to share and with whom."*

Scene 8: Closing - Treated

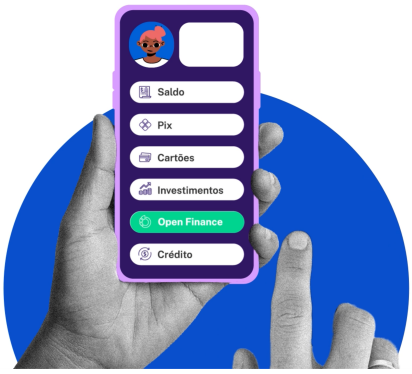
ANIMATION:

1. Microentrepreneur in a larger and more organized store.
2. Graphs and ascending arrows in the background.
3. Text on screen: "Adopt Open Finance and transform the future of your business!"

NARRATOR: *"The sooner you adopt Open Finance, the sooner you can enjoy all these benefits and boost your business. Adopt Open Finance and transform the future of your business!"*



(**a**) Control Video



(**b**) Treatment Video



(**c**) Treatment Video

Figure A2: Screenshots from Intervention Video