The Effects of Revealing Borrowers' Information on Credit Allocation, Defaults and Entrepreneurship

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Abstract

This paper examines how increased information for lenders affects credit access, default rates, and entrepreneurial activity. Using data from a major Brazilian credit bureau, we analyze a policy that expanded the information used to generate credit scores. Guided by a model of decision-making under imperfect information, we show that more informative credit scores shift credit allocation. Borrowers deemed more creditworthy received increased credit, and the improved precision of the credit scores widened credit distribution. The policy led to significantly lower default rates but also increased credit inequality, particularly widening racial disparities in access. While the policy did not affect firm creation, it improved the performance of firms started by those positively impacted, suggesting higher productivity among new entrepreneurs and reduced misallocation.

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1 Introduction

Imperfect information can lead to a misallocation of credit, compromising the overall efficiency of the economy (Stiglitz and Weiss, 1981; De Meza and Webb, 1987). In response, developed (and many developing) countries have credit reporting systems that consolidate information on borrowers' credit histories from multiple sources and provide summary "credit scores" to lenders. In principle these credit scores represent sufficient statistics on the creditworthiness of borrowers that allow suppliers of credit to make informed lending decisions. In reality, the depth of information available in such systems varies widely across countries, with potentially important consequences not only for credit market efficiency within different countries, but also for the global allocation of credit (Djankov et al., 2007).

Despite wide variation in the quantity and quality of information gathered by credit reporting systems, there is very little evidence on the potential efficiency gains in credit allocation that could arise from improved access to credit history information, or on the potential distributional effects of such changes. Studying these issues is hard because the complexity of the credit reporting system evolves endogenously as financial institutions mature and information gathering systems improve. Consequently, most of the previous literature relies on cross-country comparisons, e.g., Pagano and Jappelli (1993); Jappelli and Pagano (2002); Djankov et al. (2007). A few recent studies leverage local changes in credit systems, but cannot address potential market-wide credit re-allocations in response to improved information systems. [1]

In this paper, we study the effects of a large change in the information available to credit reporting agencies in Brazil. We explore the implementation of a 2019 law referred to as "Cadastro Positivo" that changed the status of information sharing about past credit uses that had not resulted in delinquency from explicit opt-in requirement to explicit opt-out, opening financial system access to this information for the vast majority of the population. Prior to the law change, credit bureaus only had access to information on delinquent loans (i.e., those with late payments or default). Within two years, this new information on non-delinquent credit was being shared with Brazilian credit bureaus and incorporated into a new set of credit scores.

Using detailed microdata from the largest credit bureau in the country, we conduct a simple difference in differences style analysis of the changes in outstanding loan balances (a

[1] Examples of these studies are Musto (2004); Liberman et al. (2018); Jansen et al. (2022); DeFusco et al. (2022); Herkenhoff et al. (2021); Dobbie et al. (2020) who explore bankruptcy flag removals to study the effects of information on credit access.

conventional measure of "credit"). [2] Specifically, we assign individuals their credit scores under the old system (which used only information on past credit with delinquencies) and the new system (which added "positive" information on past credit with no delinquency) in March 2021, a few weeks before the widespread adoption of the new system in May 2021. We then compare the evolution of loan balances one and a half years before and two and a half years after the implementation of the new system of credit scores for individuals in various ranges of scores under the old and new systems, and examine similar changes in the amounts of delinquent loans.

We characterize the winners and losers from the change in the information system, and evaluate the quality of the loan prospects that gained or lost access to credit under the new system, as revealed by their ex-post delinquency rates. Finally, linking credit records to unique micro data on business registrations in the largest Brazilian state (São Paulo), we study how changes in personal credit affect firm creation and the characteristics of new businesses, including their medium-term survival rates.

To frame our difference-in-differences analysis we begin by presenting a simplified model of lender decision making given a signal (the credit score) on the creditworthiness of potential borrowers. We assume that borrowers are characterized by a one-dimensional latent index of creditworthiness. Under plausible assumptions, a lender's credit decision can be represented as a monotonic function of the mean of the Bayesian posterior distribution for this latent index, given the observed signal. The impact of the change in the credit reporting system for a given individual then depends on the difference in the value of this function evaluated at the posterior means under the new system and the old.

This simple setup suggests two channels through which the change in the credit scoring system can affect specific borrowers. First, the new information derived from the Cadastro Positivo records can lead to a change in the posterior mean of creditworthiness – a "first-moment" channel. For example, the posterior mean for borrowers with an extensive record of credit use but no delinquencies will be raised. Second, the extra information incorporated in the new credit score will lead to an up-weighting of the information in the credit score itself relative to the prior mean – a "precision channel". Thus, a borrower whose posterior mean is the same under the old and new systems will receive more credit if her signal is above average (i.e., above the prior mean), but less credit if her signal is below average. We test the predictions from both these channels using the variation induced by the Brazilian policy.

[2] As explained below, our data include information on loans, credit card balances, and outstanding balances at retailers and other types of firms offering credit

We begin by testing if changes in the value of credit scores affect individuals' credit access (i.e., the first moment channel). Specifically, we estimate difference in differences models comparing the changes in loan balances for individuals with observed increases or decreases in their (standardized) credit scores relative to those whose credit scores were unchanged with the introduction of the new system.

We find that changes in the value of credit scores have significant effects on an individual's credit access. A one standard deviation increase in credit scores causes, on average, a 15 thousand Brazilian Reais (BRL) increase in credit 2 years after the policy, representing a 20% increase in their credit. Symmetrically, we find that a one standard deviation decrease in standardized credit scores is associated with a 20% decrease in credit relative to the comparison group.

We then assess if the increase in precision of the credit score arising from the incorporation of the Cadastro Positivo data affects credit. We focus on individuals whose standardized credit scores were (approximately) the same under the old and new systems, comparing those with scores above or below the overall mean score. We find that individuals with scores one standard deviation above the mean have a rise in average balances of about 5,000 BRL two years after the policy. In contrast, those with credit scores one standard deviation below the population mean have a decrease of around 12,000 BRL.

Given the evidence of both channels for the effect of the change in the scoring system, we then move to a combined analysis. We first divide the joint distribution of old and new (standardized) credit scores into a grid of bins of equal size. We then estimate a difference in differences model that gives us the change in observed credit for people in a given bin from before to after the policy change, relative to the group with no change in credit scores that had population-average scores. This is the group for whom our framework predicts no change in credit (i.e., neither first moment nor precision effects). We find patterns of changes across the bins that closely align with predictions from a simple two-channel model.

We then use our estimates to describe the winners and losers from the change in the information system. We find that the incorporation of additional information about borrowers' credit histories widens the inequality in credit access, with changes in credit that are positively correlated with the average level of credit just prior to the policy change. We estimate that the variance of credit distribution increases by 2.5% because of the policy. We also show that information revelation increases racial differences in credit access in our context. This is explained largely because of compositional effects, i.e., nonwhite individuals are disproportionately represented in parts of the distribution of credit scores that were negatively affected in terms of credit access.

Next, we turn to the question of whether the new system reallocated credit to more or less risky borrowers. Specifically, we ask how the default rates on the new loans attributable to expanded credit under the new system compare to default rates for the loans that were given under the old system but no longer available under the new system (i.e., the new loans that would be offered under a counterfactual policy of moving from the new system to the old system). Under the assumption that the policy change did not affect default rates of credit that would have been observed under either the old or new systems we show that the default rate of the marginal loans under the new policy can be estimated by the ratio of the change in financial delinquency to the change in credit for groups that had more credit under the new system. Conversely, under the same assumption, the default rate of the marginal loans under the old policy can be estimated by the ratio of the change in financial delinquency to the change in credit for groups that had less credit under the new system.

We find that, on average, credit given because of the policy had a default rate of around 3%. On the other hand, credit that would have been given under the old system, but not under the new system, had a default rate of around 15%. The comparison implies that the change in the system was highly effect in reallocating credit to more creditworthy borrows, potentially offsetting concerns about the distributional effects of the new system.

Having established how more informative credit scores affected credit access and default rates, we investigate if it had consequences beyond credit markets. We focus on entrepreneurial activity, exploring the longstanding link between credit constraints and the decision to become an entrepreneur (Evans and Jovanovic, 1989), which has been posited as an important barrier to economic development (Banerjee and Newman, 1993).

To understand how the policy affected entrepreneurial activity, we match the credit bureau data with firm records collected by us that comprise the universe of formal firms in the state of São Paulo, the most populous of the country.[3] We then pair this data with firm-level characteristics collected by our partner institution. This allows us to have a data set of individuals, with information on whether they own a firm or not and the characteristics of firms for those who own one.

We find no effect of changes in credit access on firm creation. To estimate this, we divide our sample into three groups according to their values of old and new credit scores. A group of positively exposed individuals is defined as those who are in parts of the joint distribution of credit scores that had gains in credit. The opposite holds for defining negatively exposed

[3]This data is different from RAIS, the matched employer-employee data that covers the universe of formal workers in Brazil. RAIS does not have information on ownership. It also differs from firm ownership data used in Colonnelli et al. (2022); Hsu Rocha and Dias (2021) as their data (i) is restricted to selected types of firms, (ii) do not track who creates new firms, just its existing ownership structure.

individuals. The third group consists of individuals in parts of the distribution for which we estimate small or no change in their credit access. With our treatment groups defined, we estimate hazard models with time-dependent covariates to investigate the probability that an individual opens their first firm. These work in the spirit of a difference in differences, where we compare the hazard rates of differently exposed groups before and after the policy. Our point estimates for both treatment groups are close to and not statistically different from zero. For positively (negatively) exposed individuals, we can reject effects as big as 3% (5%) in the probability of creating a new firm because of the policy.

However, the reallocation of credit affected new firms' outcomes. Comparing firms created by individuals in the three groups, we find that new firms created after the policy by positively (negatively) exposed individuals have a higher (lower) likelihood of surviving two years after their creation. We show that firms from positively exposed individuals are also, on average, in industries that are more productive, employ more, and have higher average wages.

Our findings indicate that the reallocation of credit caused by revealing information positively affected the average quality of new firms. Extrapolating average differences to the marginal firms' survival changes, our results show that new cohorts of firms are substituting less productive firms with more productive ones, indicating a positive effect on the average quality of new firms' cohorts.

This paper contributes to different threads of literature. First, we contribute to the literature on the effects of borrowers' information on credit access. Recent empirical work has extensively examined how adding/reducing information affects borrowers' access to credit (Musto, 2004; Bos and Nakamura, 2014; Dobbie et al., 2020; DeFusco et al., 2022; Jansen et al., 2022; Bos et al., 2018; Liberman et al., 2018; Herkenhoff et al., 2021; Gross et al., 2020). However, the focus of these papers have been in "first moment" changes in information[4]. Our paper adds to this literature by estimating not only first-moment changes in the creditworthiness assessment but also the effects of changes in the precision of the signal on credit access and default rates. We show that the overall effects of information combine both mechanisms. Furthermore, we estimate the effects on different parts of the credit score distribution, which is different from most papers in this literature, which are focused on local effects on the population of previously bankrupt individuals.

We also add to the literature that studies information-sharing institutions. Several em-

[4]That is, a shock occurs that changes the availability of information for a group (namely bankruptcy flags). This increases or decreases the creditworthiness assessment of that given group, and then credit outcomes are compared to a control group.

pirical studies using cross-country regressions show a positive relation between information-sharing institutions such as credit registries and private credit bureaus and financial and economic development (Pagano and Jappelli, 1993; Jappelli and Pagano, 2002; Djankov et al., 2007). In turn, within-country studies that leverage changes in these institutions are rarer and focused on the effects of information on firms, comparing firms' financing with more/less available information (Hertzberg et al., 2011; Behr and Sonnekalb, 2012). Our findings highlight both the equity and efficiency aspects of such policies. With relevant policy implications, we characterize who and by how much individuals are affected by information-sharing institutions and quantify how default rates change with such policy.

This paper also contributes to the literature that studies the role of credit constraints on entrepreneurial activity. This has long been studied in developed countries (Evans and Jovanovic, 1989; Black and Strahan, 2002; Hurst and Lusardi, 2004; Robb and Robinson, 2014; Schmalz et al., 2017; Herkenhoff et al., 2021; Cahn et al., 2021; Dobbie et al., 2020) but a debate still withstands with conflicting estimates[5]. In developing countries, there is substantial evidence that existing businesses are credit constrained (De Mel et al., 2008, 2009; McKenzie, 2017), but also evidence that credit access has no substantial effects in spurring new business (Karlan and Zinman, 2010, 2011; Banerjee et al., 2015), despite the influential early theories highlighting the importance of this link (Banerjee and Newman, 1993). Our setting allows us a rare empirical context where we can estimate both "extensive" (firm creation) and "intensive" (firm quality) margins of credit on entrepreneurship in developing countries. Our findings go in line with what has been found in the literature, suggesting no effects on the extensive margin but a positive relation between credit and firm outcomes for firms that would have existed anyway. Furthermore, we can characterize the effects of one of the most advocated policies in credit markets in developing countries (World Bank, 2012), going beyond specific credit products explored in other studies.

2 Institutional Background

Brazil is the largest country in Latin America and the 7th largest in the world, with 212 million inhabitants. Despite having the 8th largest GDP in the world, it is still considered a developing country, ranking 89th in HDI and around 80th in GDP per capita.

The country is characterized by a high level of banking penetration relative to their income level. Over 80% of the population had a bank account, and on average an individual

[5]For example, in two recent papers Dobbie et al. (2020) and Herkenhoff et al. (2021) study the effects of removing bankruptcy flags on self-employment and entrepreneurship and find conflicting estimates on the effects of credit on this type of occupational choice.

had accounts in 5.4 different banks.[6]. There are over 150 operating banks in the country and more than 600 other financial institutions, such as fintechs and credit cooperatives, that also provide loans.

Before *Cadastro Positivo*, credit bureaus were already relevant sources of assessment of creditworthiness. Their main source of information was a delinquency registry often referred to as *Cadastro Negativo de Devedores*. It tracked defaults, protests, bounced checks, overdue debts, and other financial irregularities. These complaints could be made by creditor companies such as banks, financial institutions, stores, service providers, among others. Since 1990[7], the inclusion of a consumer's name in defaulters' lists must follow the guidelines established by federal law, ensuring the right to information and the right to challenge by the consumer. On top of delinquency records, credit bureaus also used publicly available information to calculate credit scores. These included prosecution records, firm ownership, and bankruptcy records.

Cadastro Positivo **- increase in information available to Credit Bureaus**

Cadastro Positivo is the name given to the legislation that changed the information available for credit bureaus to create credit scores. With this change, on top of the previously available sources, credit bureaus gained access to *positive financial data*, including records of on-time payments, responsible credit usage, the opening of new credit accounts, successful loan repayments, and the length of an individual's credit history.

Lenders do not access the registry information directly. The data is shared only with authorized credit bureaus responsible for processing the information and generating credit scores for individuals and businesses. These credit scores are subsequently made available to potential lenders and creditors.[8]

Timeline: The Cadastro Positivo was initially established through the Law No. 12.414 on June 9, 2011. After the law, a long implementation period started where regulations were being established by government agencies. The first information originated from Cadastro Positivo became available to credit bureaus in the first semester of 2014.

Until 2019, individuals and businesses had to opt in to have their information included in

[6]We show average account ownership and share of individuals borrowing from financial institutions by country relative to their GDP per capita in Figure A1 We observe that Brazil has higher levels on both statistics relative to their prediction based on GDP per capita.

[7]Credit Bureaus in Brazil existed since at least the 1950s, but their operation was only regulated by Law 8.078/1990 also known as *Código de Defesa do Consumidor*

[8]Brazil has authorized four credit bureaus to access and employ Cadastro Positivo data for credit assessment purposes. These official credit bureaus include Serasa Experian, Boa Vista SCPC (Serviço Central de Proteção ao Crédito), Quod, and SPC Brasil (Serviço de Proteção ao Crédito).

the Cadastro Positivo. However, the opt-in system faced challenges in garnering a sufficient number of registrants as by 2019, less than 5% of the population had opted-in to Cadastro Positivo. BACEN (2021) documents that due to the low take-up of the policy, all major banks report not using credit scores built with Cadastro Positivo information in their lending decision during the opt-in phase.[9]

In April 2019, Congress approved Law 166/2019, which changed the default status in Cadastro Positivo to include all individuals and businesses with financial records in the Cadastro Positivo unless they expressly opted out. This change increased more than 15 times in the number of individuals with active information in the registry. This represented over 100 million individuals with active information in Cadastro Positivo. Even though individuals still could opt out of the system, by the end of 2020, less than 350 thousand people in the country had done so.

The execution of the new law took place over the following 2 years. Between April and December 2019 year, regulating agencies set up a new set of conditions for credit bureaus to gain access to the data. In December 2019, the change in the system took place, and individuals who were previously outside of Cadastro Positivo were now included in the system. During the first semester of 2020, information from institutions registered with the Central Bank started to be shared with credit bureaus. These data included consumers' payment records from the previous 2 years. Given the complexity of analyzing and adapting credit assessment to the new set of information available, credit scores under the new phase of Cadastro Positivo only began to be commercialized by May of 2021.

Aggregate Credit Patterns Around Policy Implementation: Before *Cadastro Positivo*, Brazil already had a robust and established credit market. Therefore large changes in aggregate credit patterns would not be expected to occur due to the policy. We show some aggregate statistics of household credit in Figure A2. We observe no clear break in the trends of total household credit, nor in their composition across different sources (Panels (a) and (b)). At the same time, there is a slight increase in the average cost of credit, mostly accompanying the spike in interest rates that happened around the time of the policy. Lastly, we also see that there are no major changes in the level of credit concentration in the market. The aggregate pattern suggests a continuation of an existing pattern of decrease in concentration, although the magnitude of these changes is relatively small.[10]

[9] On top of low take-up, BACEN (2021) documents that banks also reported large selection patterns in individuals registered in the new system. Lenders report that most registered individuals were formal credit delinquents who opted-in the system when they were renegotiating their debts.

[10] The normalized HHI decreases from around 0.125 to 0.10 between 2016 and 2023. This slight decrease in concentration is often attributed to the entry of digital credit products, which increased their participation

3 Conceptual Framework

In this section, we outline a conceptual framework to understand the effects of additional information in the construction of credit scores. We map how credit access should change for individuals with given pairs of signals in the less, and more informative states of the world. This provides us with testable implications of how credit access should change with a more informative signal.

We consider a lender's decision problem, in which they face borrowers who vary in their creditworthiness. The lender does not observe the true types of potential borrowers. Instead, they receive an unbiased signal and update their beliefs accordingly. Signals in our framework are not endogenously determined by borrowers in the spirit of Spence (1973). We think of them as information structures as in Green and Stokey (1978) or more recently Brooks et al. (2022a,b)

Our framework allows us to define two different effects of revealing information about potential borrowers on their credit access. The first one, which we refer to as the *effect of the signal's value*, is determined by changes in the assessment of creditworthiness given the new information. The second one, which we call *effect of signal's precision*, is driven by the change in the precision of the signal observed by the lender.

3.1 Setup

Borrowers: Potential borrowers differ in their (true) creditworthiness, denoted by $\theta_i \in \mathbb{R}$, with $\theta \sim \mathcal{G}(.)$, where $\mathcal{G}$ is a well-defined density function continuous on $\mathbb{R}$. One could conceptualize this parameter as one governing the profitability of loans and the probability of defaults.

Decision Problem: The lender faces a decision problem. They have an objective function $\pi(\theta_i, C_i)$ where they decide how much credit they supply to an individual (C_i), based on i's creditworthiness.

A lender does not directly observe the true creditworthiness of the borrower θ_i but receives a signal realization s_i.[11] The lender then forms expectations of the creditworthiness of the potential borrowers and offers them loans accordingly. The lender's solution to the decision problem is defined as:

in the Brazilian market at the end of the 2010 decade

[11]The information structure can be formally defined as follows. We define a signal S, ρ as a set of Signal realizations $S = \Theta$ and a joint distribution ρ over $\Theta \times S$. The marginal distribution of ρ over Θ is $\mathcal{G}(.)$, and that ρ marginal distribution over S is a governed by a distribution $\mathcal{F}(.)$. We assume that a signal realization $s_i \in S$ is an accurate estimate of the true creditworthiness θ_i. That is: $s_i = \mathbb{E}_\rho[\theta_i|s_i]$.

$$C^*(s_i) = \underset{C}{\operatorname{argmax}}\, \mathbb{E}[\pi(C, \theta)|s_i]$$

where the lender maximizes their expected value of the objective function given the signal they receive. To further understand how more informative signals affect credit allocation, we assume that the solution to the decision problem can be written as:

$$C^*(s_i) = g(\mathbb{E}[\theta|s_i])$$

In what follows, we focus on how $\mathbb{E}[\theta|s_i]$ behaves with more informative signals. We then return to our decision problem and discuss what are *rationalizable* changes in credit given a policy that increases the informativeness of signals.

3.2 Gaussian Parametrization

To gain intuition on the effects of the policy, it is useful to include a common parametrization of the signals as a function of the true value of individuals' types. We assume that an individual i's type θ_i is drawn from an underlying distribution $\theta_i \sim N(\mu, \sigma^2)$. Their signal is a noisy measure of their creditworthiness:

$$s_i = \theta_i + u_i, \quad u_i = \epsilon_i - \Delta_i$$

where Δ_i and ϵ_i are independent and distributed according to $N(0, \sigma^2_\Delta)$ and $N(0, \sigma^2_\epsilon)$ respectively. Thus $u_i \sim N(0, \sigma^2_\Delta + \sigma_\epsilon)$.[12] This implies that the lender's expected value about a given borrower's type is then given by:

$$\mathbb{E}[\theta|s_i] = \mu + \underbrace{\frac{\sigma^2}{\sigma^2 + \sigma^2_\Delta + \sigma^2_\epsilon}}_{\text{Precision Weight}} (s_i - \mu)$$

where the expected value is a linear combination of the prior of the lender, given by the population average, and the signal realization s_i. The *Precision Weight* establishes how much weight the decision maker puts on the signal relative to their prior. It captures how informative the signal is. If it was fully informative ($\sigma^2_\epsilon + \sigma^2_\Delta = 0$), the expected value of the individual's type would be equal to the signal. The only difference between the expression above and standard frameworks is that the variance of the *noise* is determined by $\sigma^2_u = \sigma^2_\epsilon + \sigma^2_\Delta$.

[12] This is an adaptation of the common formulation of signals in the statistical discrimination literature in labor markets (Phelps, 1972; Aigner and Cain, 1977).

3.3 Revealing Information

Policy: We now consider a policy that allows allows Δ_i to be observed. An interpretation of this policy is that signals are constructed with additional information that recovers Δ_i. Thus, lenders observe a new signal $s_i^{'} = s_i + \Delta_i$, which in turn reflects a new expected value of the borrowers' creditworthiness given the signal:

$$\mathbb{E}[\theta|s_i^{'}] = \left(\mu + \frac{\sigma^2}{\sigma^2 + \sigma_\epsilon^2}\left(s_i + \Delta_i - \mu\right)\right)$$

To understand the changes in credit given the new signal, we compare the two expected values of creditworthiness, $E[\theta_i|s_i^{'}]$ and $E[\theta|s_i]$. We can write the difference between them as:

$$\mathbb{E}[\theta|s_i^{'}] - \mathbb{E}[\theta|s_i] = \underbrace{\left(\mu + \frac{\sigma^2}{\sigma^2 + \sigma_\epsilon^2}\left(s_i + \Delta i - \mu\right)\right)}_{\mathbb{E}[\theta_i|s_i^{'}]} - \underbrace{\left(\mu + \frac{\sigma^2}{\sigma^2 + \sigma_\Delta^2 + \sigma_\epsilon^2}\left(s_i - \mu\right)\right)}_{\mathbb{E}[\theta_i|s_i]}$$

Define

$$r_1 = \frac{\sigma^2}{\sigma^2 + \sigma_\epsilon^2} \qquad r_2 = \frac{\sigma_\Delta^2}{\sigma^2 + \sigma_\epsilon^2 + \sigma_\Delta^2}$$

Then the change in the expected value of θ can be rewritten as:

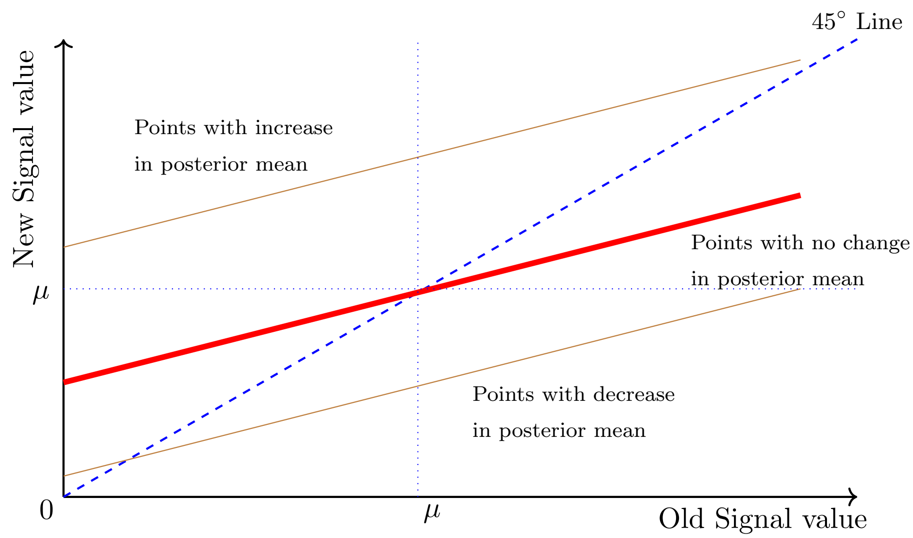
$$\mathbb{E}[\theta|s_i'] - \mathbb{E}[\theta|s_i] = \underbrace{r_1\Delta}_{\text{Signal Value Change}} + \underbrace{r_1 \cdot r_2(s_i - \mu)}_{\text{Change in Precision}}$$

The expression above provides us intuition on how an increase in the information changes the expected values of creditworthiness. If the information revealed indicates that the individual is more creditworthy, lenders will update their expectations accordingly. This is straightforward from the *signal value change* part in the expression above

Another implication of our framework is that the revealed information also increases the *precision* of our signal. This implies that expected values of creditworthiness should change even for individuals for whom the information revealed does not change their signal ($\Delta_i = 0$). The intuition behind this is that with a more precise signal, the lenders' precision weight increases; thus, they update the population prior closer to the signal.

We can illustrate this in terms of *indifference curves.* Note that if: $r_1\Delta + r_1 \cdot r_2(s_i - \mu) = 0$ then the change in the posterior mean between the new and old signals is zero. This is the set of (s, s') for which $s' = r_2 \cdot \mu + (1 - r_2) \cdot s$. This set includes individuals with $(s_i, s_i') = (\mu, \mu)$,

Figure 1: Diagram of Indifference Curves in the Changes in Posterior Mean



that is, those with signal equal to the prior and no change with the new information. It also includes all points in the red line in Figure 1.

More generally, all points in a given line:

$$s' = k + (1 - r_2) \cdot s$$

have the same change in their posterior mean given by:

$$\mathbb{E}[\theta | s_i'] - \mathbb{E}[\theta | s_i] = r_1 \cdot (k - r_2 \cdot \mu)$$

A key feature of these *indifference curves* is that their slope r_2 will always be positive and smaller than 1. Lines in our diagram with slope equal to 1 (as the 45° Line) represent individuals with the same change in their signal value. As illustrated in Figure 1, our framework implies that conditional on the same signal value change, individuals with higher values of the signal are on higher *indifference curves*, thus implying a higher change in the posterior means.

Generalizing the Gaussian Parametrization:

The Gaussian parametrization of the distributions is useful for exposition purposes but not necessary for our rationalization of changes in credit with more informative signals. In what follows, we state general assumptions on the expected value of creditworthiness given the signal and how the less and more informative signals relate to each other. These add to the information structure described in the framework setup.

Assumption 1: *The expected value of creditworthiness given the signal can be written as a linear combination between the prior and the signal:*

$$\mathbb{E}[\theta|s_i] = \mathbb{E}[\theta] + \frac{1}{a}(s_i - E[\theta])$$

where a is increasing in the variance of the signal realization given the true type.

Assumption 2: *A pre-policy signal s_i is a mean-preserving spread of the new more informative signal s_i'. That implies for a given constant t:*

$$\begin{aligned} &\text{(i)} \quad \int_{-\infty}^{\infty} s f(s|\theta)\, ds = \int_{-\infty}^{\infty} s' f(s'|\theta)\, ds' \\ &\text{(ii)} \quad \int_{-\infty}^{t} F(s|\theta)\, ds > \int_{-\infty}^{t} F(s'|\theta)\, ds' \end{aligned}$$

Diaconis and Ylvisaker (1979) establishes sufficiency conditions for distributions to follow the assumption above. In particular, they show that with conjugate priors in the exponential family, it is possible to write the posterior expectation of a random variable with a known distribution given a signal as a convex combination of the expected value and the signal. Changes in the variance of the signal that generate mean-preserving spreads generate changes in the linear combination parameter that go according to the precision weight described above.[13]

3.4 Implications on Credit Allocation

We now describe how the policy reflects in the lender's solution to their decision problem. In what follows, we assume that the distribution of types and signal realizations follow assumptions 1 and 2.

Recall the solution of the decision problem under the old signal defined as:

$$C^*(s_i) = \underset{C}{\operatorname{argmax}}\, \mathbb{E}[\pi(C,\theta)|s_i] = g(\mathbb{E}[\theta|s_i])$$

With the more informative signal s' the lender decision problem becomes.

$$C^{**}(s_i') = \underset{C}{\operatorname{argmax}}\, \mathbb{E}[\pi(C,\theta)|s_i']$$

[13] Chambers and Healy (2012) provide more general conditions for what they define as *Update Towards the Signal* types of posteriors. In these cases we still observe a linear relation between the expected value of the random variable and a signal realization. However, they do not explicitly establish how the *precision weight a* relates to the variance of the signal realization.

where the expectation is taken over the posterior distribution of θ given the signal s'. We assume:

$$C^{**}(s') = g(\mathbb{E}[\theta|s'])$$

in other words, the credit allocation with the new signal depends only on the posterior mean of θ; with the same g(.) function as under the old signal.

Under the assumptions above, the changes in the solution of the decision problem for an individual who had an old signal equal to s_i and a new signal s_i' is:

$$h(s_i, s_i') = C^{**}(s_i') - C^{*}(s_i) = g(E[\theta|s_i]) - g(E[\theta|s_i])$$

We outline propositions that characterize how $h(s_i, s_i')$ credit should behave over the set comprising values of new and old signals $(s_i, s_i') \in \mathbb{R}^2$.

Proposition 1: *For any given pair of old and new signals, s_i, s_i', and a given positive constant c, if $g(.)$ is increasing, rationalizable changes in credit should follow*:

i. $h(s_i, s_i' + c) - h(s_i, s_i') > 0$

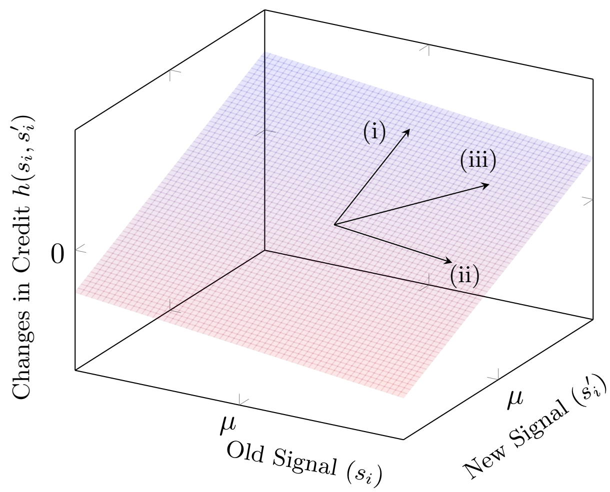
ii. $h(s_i + c, s_i') - h(s_i, s_i') < 0$

Statement (i) and (ii) are what we define as the *Effects of the signal's value*. Their proof is detailed in Appendix B. (i) states that changes in credit are increasing in the new signal's value. This comes from the fact that, for a fixed value of the old signal s_i, the function that defines changes in credit $h(s_i, s_i')$ is increasing in s_i' which in turn comes from the solution of the lenders problem being increasing in its argument, which is increasing in s_i'. The same argument is valid for statement (ii). It states that for a given value of the new signal, the changes in credit should be decreasing in the old signal.

The intuition for statement (i) is reasonably straightforward. A signal that implies that individual i is more creditworthy should imply bigger changes in credit. In turn, statement (ii) is slightly less intuitive. We could explain it as for a given value of the new signal, an old signal that implies i is more creditworthy would represent a bigger pre-policy value of credit, which would, in turn, represent a decrease in the credit change.

We can also think about how these statements would fail empirically. If the lender does not perceive the new information available in the new signal as useful, we would expect equality to hold in statement (i). At the same time, if lenders did not perceive information in the old signal as useful, we would expect equality in statement (ii).

Figure 2: Rationalizable Changes in Credit Under a Linear Relationship Between Expected Creditworthiness and Credit



Proposition 2: *If $h(s_i, s_i')$ is increasing in $\mathbb{E}[\theta|s_i'] - \mathbb{E}[\theta|s_i]$, for any given pair of old and new signals, s_i, s_i', and a given positive constant c, rationalizable changes in credit should follow:*

iii. $h(s_i + c, s_i' + c) - h(s_i, s_i') > 0$

Statement (iii) defines what we call the *Effects of the signal's Precision*. It states how, conditional on the same difference between signals, changes in credit should behave. If changes in credit are increasing in changes in the posterior of types given a signal, changes in credit should be increasing as we move towards larger values of the signal conditional on the same difference between new and old signal. It requires that we make assumptions on how function $h(s_i, s_i')$ varies with the differences in $\mathbb{E}[\theta|s_i'] - \mathbb{E}[\theta|s_i]$ instead of assumptions over $g(.)$. It holds because under our assumption over how posteriors are formed given a signal, $(\mathbb{E}[\theta|s_i' + c] - \mathbb{E}[\theta|s_i + c]) > (\mathbb{E}[\theta|s_i'] - \mathbb{E}[\theta|s_i])$. Intuitively, this occurs because, conditional on the same differences in the signal values, the precision weight multiplied by the signal realization is increasing in the value of the signal. The complete proof is written down in Appendix B.

The diagram illustrates how Propositions 1 and 2 allow us to rationalize changes in credit over the joint distribution of signals. It shows changes in credit in the *z-axis*, values of the old signal (s_i) in the *x-axis* and values of the new signal (s_i') in the *y-axis*. The arrows correspond to each of the statements from both propositions.

We observe that: (i) conditional on the old signal, changes in credit are increasing in the value of the new signal. (ii) conditional on the new signal, changes in credit are decreasing in the value of the new signal. (iii) conditional on the difference between signals, credit changes are increasing in the value of the signal.

Statement (iii) provides theoretical implications and important consequences of the changes in the credit distribution. As it posits that, conditional on the difference between signals, those with more credit scores should have higher changes, it implies an increase in the inequality in credit access and a widening in the credit distribution, conditional on the changes in credit scores.

When estimating the effects on credit access, we will return to statements (i), (ii), and (iii) to test if our framework can rationalize the changes in credit.

4 Data and Sample Construction

We use two main data sources. First, we use information from SERASA, the largest credit bureau in Brazil. We combine that with firm ownership records from São Paulo's trade board, *Junta Comercial do Estado de Sao Paulo* (JUCESP) an autarchy of the government that is responsible for organizing and keeping firm records.[14]

SERASA is the largest credit bureau in Brazil and one of the four authorized by the Central Bank to access the information from Cadastro Positivo. It collects information not only from institutions in the national financial system but also from retail, utility, and insurance companies.

We observe individuals' credit scores built with and without information from Cadastro Positivo, which allows us to build the counterfactual credit score in the absence of the policy for each individual. On top of that, we observe detailed information on credit access, including loans and purchases made on credit and loan payments made by those individuals. One caveat of our data is that we do not observe loan-specific information such as interest rates.

Our approach attempts to follow as closely as possible the guidelines for using credit agency data defined in Gibbs et al. (2024). Following their classification, we do not access *tradeline-level* data. Instead, our data agreement allowed us to access *tradeline-level* data aggregated to consumer-level. We detail the definition of key variables below when we discuss our summary statistics.

Firm Ownership Records (JUCESP): According to the Brazilian constitution, every

[14]We translate *Junta Comercial* to Trade Board. Still, it is possible to translate it to the commercial registry or business board.

formal business must register in its state's trade boards. In Brazil, firm ownership information is in theory public. All states' Trade Boards are obliged to make all information they collect public but can charge for it.[15] JUCESP is the only Trade Board that allows Brazilian Citizens to access information for free. To recover firm records, it is necessary to have a Brazilian Social Security Number and registration on JUCESP's website. Each citizen has access to 799 firm records per day.

We collect more than 4 million identified firm records, which cover all formal firms created in São Paulo between 2003 and 2023. These records contain all information from the firm ownership history. Importantly, they include the name and social security number of all founders, on top of firm identifiers. This allows us to match firm records from JUCESP with information from the Credit Bureau.

4.1 Sample Construction

Our main sample comprises around 200,000 randomly selected individuals aged between 20 and 60 years old in 2019 living in the state of São Paulo. This represents around 1% of the population of the State. We restrict our empirical focus to São Paulo due to data agreements. It is the most populous state in the Country with over 44 million inhabitants and concentrates around one-third of the country's economic activity.[16]

We begin with individual-level credit access information from SERASA for all individuals in the sample and match that with firm ownership information from the JUCESP firm records for those who eventually open a firm. For those who are firm owners, we then find information on their firm's outcomes using SERASA's information. In Appendix C we detail the sample construction procedures, indicating all the steps done by the company's researchers and de-identification processes that were required in our data agreement.

We show summary statistics of our sample in Table 1. Statistics are calculated in the last period before the implementation of the policy. Monetary values are presented in Dec. 2023 BRL. At the time, the corresponding exchange rate was 1 dollar to 5.03 BRL. On the left-hand side of the panel, we describe the demographic characteristics of our sample.[17] On the right side of the panel, we show credit characteristics. Given the novelty of these

[15] Almost all states in the country charge exorbitant price rates for firm records stating who are the owners or who created each firm.

[16] Compared to other Latin American countries, Sao Paulo has the same population as Argentina, and its economic activity would be on par with Mexico's GDP.

[17] Nonwhite individuals group those who declare themselves *Pretos, Pardos* and *Indigenas* (Black, Mixed and Indigenous). Less than High School groups all individuals without a High School diploma, whereas Some College groups individuals with at least some college education, thus including college dropouts and post-secondary technical degrees.

Table 1: Summary Statistics

Demographic Characteristics		Credit Characteristics		
	(1)		(2)	(3)
	Mean		Mean	Std. Dev.
Female	0.45	C. Sc. - Old System	467.15	310.59
Nonwhite	0.24	C. Sc. - New System	552.89	222.37
Less than H.S.	0.21	Total Credit	30325.63	100943.65
High School	0.56	Outstanding Loans	26870.59	97722.95
Some College	0.22	Credit Purchases	3455.04	9243.21
Age	40.19	Financial System Default	2435.93	95206.02
		Other Default	600.01	8291.51
N.Obs	194247			

This Table presents summary statistics of our sample. Statistics are calculated in the last period before the policy's implementation. Monetary values are presented in Dec. 2023 Brazilian Reais (1 USD = 5.03 BRL).

data, next, we discuss the characteristics and definitions of these key variables:

Credit Scores: we access two credit scores for each individual in our panel. The old system credit scores were constructed using only data sources available to the credit bureau before the policy. In contrast, the new system credit scores represent their updated creditworthiness assessment measure, using data made available by Cadastro Positivo and an updated prediction model.[18] Under the new system, the credit bureau defined values above 700 as *excellent* credit scores, those between 501 and 700 as *good*, 301 and 500 as *low*, and below 300 as *very low* credit scores.

We can see in Table 1 that new and old system credit scores differ in both average and variance. This happens because the new credit scores are subject to a different scale than the old ones. To make them comparable measures, we normalize them in our empirical analysis by subtracting the population's average and dividing by the standard error.

Total Credit: is our main outcome variable in the empirical analysis. When creating this variable, we attempted to mimic as closely as possible the definition of *Borrowing* in Gibbs et al. (2024), by summing outstanding balances in all active trade-lines. Total credit is defined at a given period as the sum of outstanding loans and Credit Purchases (mostly credit cards) from a given individual. This includes all types of credit, including credit for consumption and other personal use, and real estate related credit equivalent to mortgages in the US. [19]

[18]Serasa has multiple credit score measures with specific goals, which they commercialize for different purposes. The ones we use are their most standard measures.

[19]In Brazil, real estate related credit represents a much smaller share of the personal credit market than

Financial Delinquency: Financial institutions report the amount owed to credit bureaus when individuals fail to meet their financial obligations on time. Typically, the lender sends a notice indicating the debt and sets a deadline for regularization. If this deadline is not met, the overdue debt may be marked as delinquent and reported to the credit bureau. In our data, each individual's amount reported as delinquent from financial system obligations is observed at a given period.

Our credit bureau partner receives delinquency measures from other sources besides the financial system. Our data allows us to disentangle between these sources. In our empirical analysis of default rates, we focus on delinquency originating from the financial system. Table 1 shows that delinquency from other sources is, on average, smaller than financial system delinquency.

5 Effects on Individuals' Credit Access

In this section, we empirically analyze the effects of the increase in the information available to construct credit scores on individuals' credit access. We begin by characterizing the marginal distributions of credit scores constructed under the new and old systems and the joint distribution of both measures. We then estimate how changes in credit scores affected the credit access of individuals over the joint distribution of credit scores. We focus on the transition from the opt-in to the opt-out phase as a source of variation on lenders' information about borrowers, as the opt-in period had low take-up, and lenders report not using that information.

5.1 Credit Scores with and without Cadastro Positivo

We begin by characterizing credit scores constructed with and without the information from *Cadastro Positivo*. Let I^n_{it} be a vector that contains credit delinquency information from individual i at period t which was available to credit bureaus before the policy. I^p_{it} is a vector of additional information about individual i at period t that is made available to credit bureaus with Cadastro Positivo.

Credit bureaus use these data to assess the creditworthiness of individual i. Let $f^n_{it}(I^n_{it}) : \mathbb{R}^n \to \mathbb{R}$ be a function that transforms the information into a creditworthiness measure before Cadastro Positivo and $f^p_{it}(I^p_{it}, I^n_{it}) : \mathbb{R}^m \to \mathbb{R}$ be its counterpart under the new system. Credit scores under each system are defined by:

in the US (around 25% relative to around 70% in the US). Unfortunately, in our data we cannot decompose credit by their type, only by the type of institution. See Gonzalez et al. (2023) for a summary of how personal credit is divided across types in Brazil during our analysis period.

$$s_{it}^{'} = f_{i\tau}^{p}(I_{i\tau}^{p}, I_{i\tau}^{n}) \quad s_{it} = f_{i\tau}^{n}(I_{i\tau}^{n})$$

Marginal Distribution of Credit Scores: Despite only accessing delinquency records under the old system, credit scores varied substantially over the population. This is maintained under the new system. In Figure A4, we show the distribution of both credit scores. To make them comparable, we use the Z-score of both measures. $Z_i = \frac{s_i - \bar{s}}{sd(s)}$ where $\bar{s} = \sum_i \frac{s_i}{N}$. Next, we show how both measures correlate with observable characteristics of individuals.

In Panels A and B of Figure 3, we show the correlation between the Z-score of credit scores and the demographic characteristics of individuals. The plotted coefficients Γ are the OLS estimates of a linear model $Z_i = \Gamma X_i + \epsilon_i$. We include age, gender, race, and education in the vector of observable characteristics.

We observe in Panel A that women and nonwhite individuals have, on average, lower credit scores than men and white individuals. There are no clear differences in this estimate between the new and old systems of credit scores. When we look at education levels, we observe that both H.S. and college-educated individuals have a higher credit score than those with less than H.S. education. This difference is amplified when information from the positive credit score system is included. In Panel B, we show that credit scores are positively correlated with age in both the new and old systems.

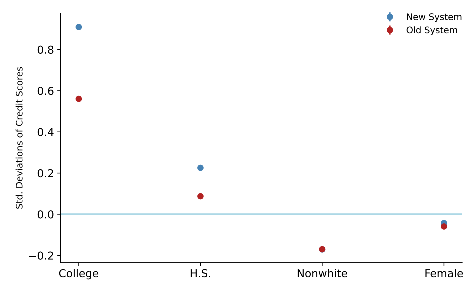
In Panels C and D of Figure 3, we show the correlation of the Z-score of credit scores with the amount of credit individuals take. We observe that credit is increasing in both old and new system credit scores. However, the correlation between both measures is substantially stronger in the new system. The opposite holds when we observe default measures in Panel C. Default is decreasing on credit scores, but the correlation is stronger in the old system measures.

Joint Distribution of Credit Scores:

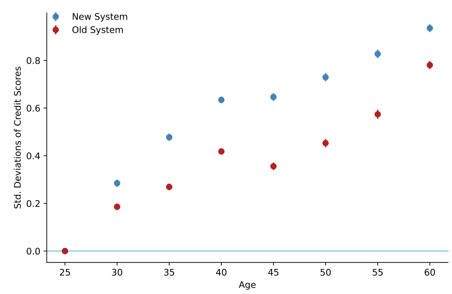
Next, we describe the joint distribution of credit scores in both systems. In Panel A of Figure 4, we show the histogram of our sample over both measures of credit scores. We restrict the sample to observations in the last period before the implementation of our policy and divide our sample into 11 equally sized bins.

The policy generates substantial variation in credit scores. For each individual, we measure the difference in their credit scores in the positive and negative systems. We refer to this measure below when describing our empirical strategies to estimate the effects of the signal's value and the effects of the signal's precision:

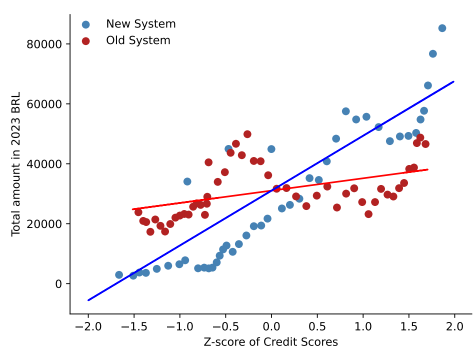
Figure 3: Credit Scores Correlation with Observable Characteristics



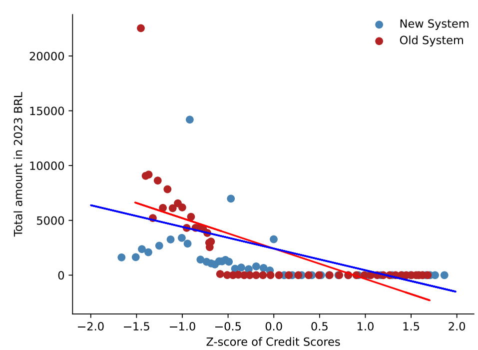
(a) Gender Race and Educ.



(b) Age Groups



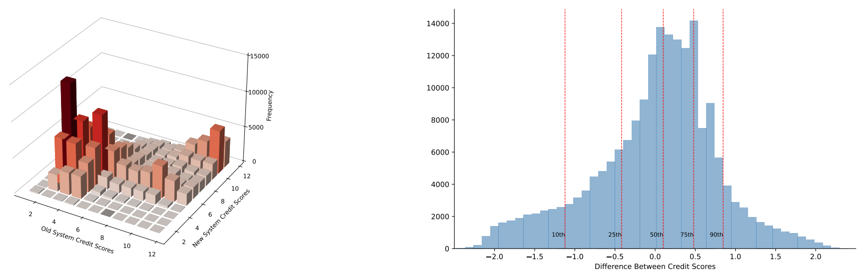
(c) Loans



(d) Financial Delinquency

Panels (a) and (b) plot the coefficients of a regression of $\frac{cs_i - \bar{cs}}{sd(cs_i)}$ on observable characteristics. The sample is restricted to the last period before the implementation of the policy. Coefficients in panels (a) and (b) are estimated in the same regression that includes dummies for gender, race, education groups, and age groups. We omit from the regression white men with less than high school education in the youngest age group. In panels (c) and (d), we show binscatters of Credit and Default with the Z-scores of credit scores in both positive and negative systems. The sample is restricted to the last period before the implementation of the policy.

Figure 4: Joint Distribution of Credit Scores in Positive and Negative System



(a) Joint Distribution

(b) Distribution of Difference in Credit Scores

In Panel (a), we show a histogram of the joint distribution of credit scores in the new and old systems. Individuals are divided in a grid of 11 equally sized bins of values in each system. In Panel (b), we show a histogram of the difference between the z-scores of credit scores in the new system and credit scores in the old systems. Vertical red lines represent the 10th, 25th, 50th, 75th and 90th percentile of the distribution.

$$\Delta_i = s_{i\tau}^{'} - s_{i\tau}$$

where we fix τ as the period before the policy implementation.

In Panel B of Figure 4, we plot the distribution of Δ_i. It has a shape similar to a normal distribution, with a median centered at 0. It has a slightly higher mass in the left tail than in the distribution's right tail. In Figure A5 we show the correlation of Δ_i with the demographic and credit characteristics of individuals. Δ_i has virtually no correlation with gender of race. It is positively correlated with education levels and age. Furthermore, Δ_i positively correlates with individuals' credit before the policy.

5.2 Effects on Credit Access

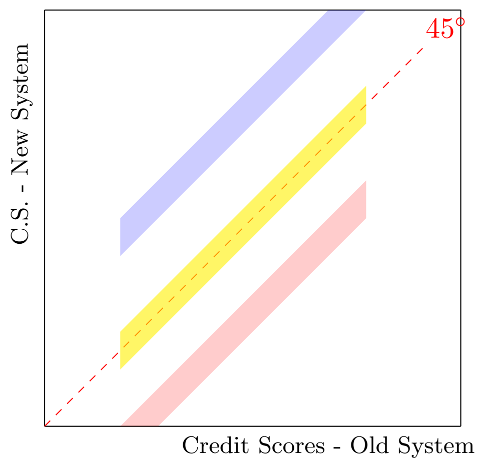
Next, we estimate how individuals' credit access changed according to the values of credit scores in both negative and positive systems. We start by testing both signal changes and precision mechanisms outlined above. We then estimate how credit access changes according to the joint distribution of credit scores in both systems.

5.2.A Effects of the Signal's Value

We begin testing whether changes in the signal value affect how much credit individuals access. To do so, we compare individuals whose credit scores increased or decreased with the additional information with those who did not have any change.

The diagram below provides a visual description of our empirical exercise. It represents points in the joint distribution of credit scores. Our exercise compares individuals at the joint distribution's blue, yellow, and red parts before and after the new credit scores were made available. Being in the blue part of the diagram implies that the additional information improved the creditworthiness assessment of that given individual. The opposite holds for those in the part of the distribution highlighted in red. Those in the yellow part have similar creditworthiness assessments with and without information made available by the policy.

Diagram of the Empirical Strategy to Estimate the Effects of Changes in Signals' Value



More formally, we define the three groups as:

$$D_i^+ = \mathcal{I}\Big[\Delta_i \in [0.75, 1.25]\Big] \qquad D_i^- = \mathcal{I}\Big[\Delta_i \in [-1.25, -0.75]\Big] \quad D_i^0 = \mathcal{I}\Big[\Delta_i \in [-0.25, 0.25]\Big]$$

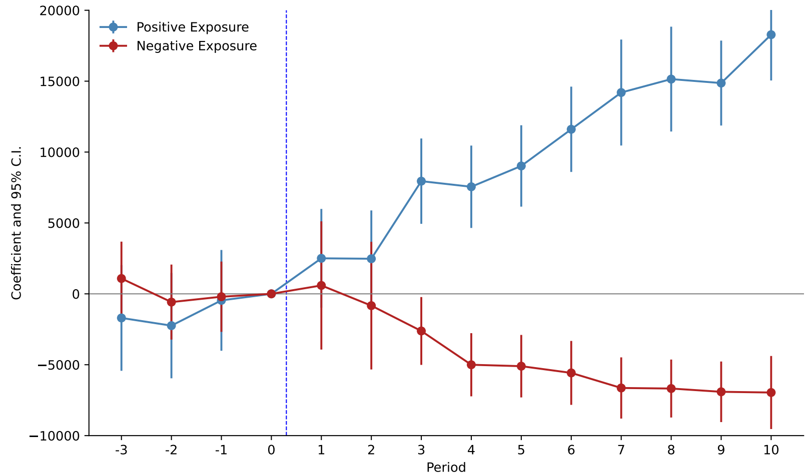
We restrict the sample for those individuals lying in one of the three groups (i.e., $D_i^+ = 1 | D_i^- = 1 | D_i^0 = 1$) and estimate the following difference in differences model.

$$Y_{it} = \alpha_i + \delta_t + \sum_{t \in T} \beta_t^+ \cdot D_i^+ \cdot \delta_t + \sum_{t \in T} \beta_t^- \cdot D_i^- \cdot \delta_t + \varepsilon_{it} \tag{1}$$

where α_i are individual fixed effects and δ_t are time dummies. Our identifying assumption is that, on average, individuals in different groups would follow parallel trends in the absence of the policy.

We show our estimates of $\{\beta_t^+, \beta_t^-\}$ in Figure 5. Vertical bars represent 95% confident intervals, centered around our coefficient estimates.

Figure 5: Effects of Signal Value Change on Credit Access



This Figure shows how credit changes for individuals with increases and decreases in their credit scores due to the policy. The connected dots plot the $\{\beta_t^+, \beta_t^-\}$ estimates from equation 1 with their respective 95% confidence intervals. Positively exposed individuals are those with $\Delta_i \in [0.75, 1.25]$ and negatively exposed are those with $\Delta_i \in [-1.25, -0.75]$.

We find significant increases in credit access for those who had a positive change in their credit scores, shown in the blue connected dots. Two years after the policy, this group observed an increase of 15 thousand BRL in their credit access. On the other hand, we estimate a decrease in credit access for those with negative changes in their credit scores. We estimate a reduction of 8 thousand reais 2 years after the policy. This represents a change of 20% in credit access for both groups relative to their counterfactual credit access without the policy.[20]

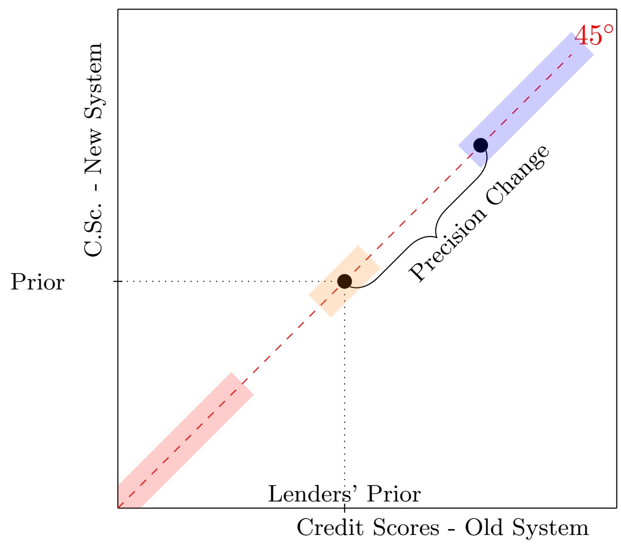
5.2.B Effects of Signal's Precision

We next test if the increase in the precision of the signal had effects on individuals' credit access. With a reduction in the noise of the signal, our conceptual framework predicts that lenders expected values of the individuals' true creditworthiness should be closer to the received signal. This generates changes in credit access even for those without changes in their credit scores.

[20] We arrive at percentage effects by dividing our coefficient estimates by the baseline level of the respective group in period=0 summed with the time fixed effects (for positive exposure $\frac{\hat{\beta_t^+}}{E[Y_{it}|t=0,D_i^+]+\hat{\delta_t}}$, and for negative exposure $\frac{\hat{\beta_t^-}}{E[Y_{it}|t=0,D_i^-]+\hat{\delta_t}}$) . In Figure A6, we show our estimates normalized.

To test this hypothesis, we make comparisons among individuals who did not have changes in their credit scores between positive and negative systems. We illustrate this in the diagram below. Individuals in the blue area have high credit scores, and the lender's posterior about their true creditworthiness should increase with a more precise measure. The opposite holds for those in the red area. Those in the orange area should not observe changes in their creditworthiness assessment.

Diagram of the Empirical Strategy to Estimate the Effects of Changes in Signals' Precision



We formally describe our exercise. First, we restrict our sample to individuals with $\Delta_i \in (-0.25, 0.25)$. We then define the following three groups

$$D_i^+ = \mathcal{I}\Big[s_i > 1\Big] \qquad D_i^- = \mathcal{I}\Big[s_i < -1\Big] \quad D_i^0 = \mathcal{I}\Big[s_i \in [-0.25, 0.25]\Big]$$

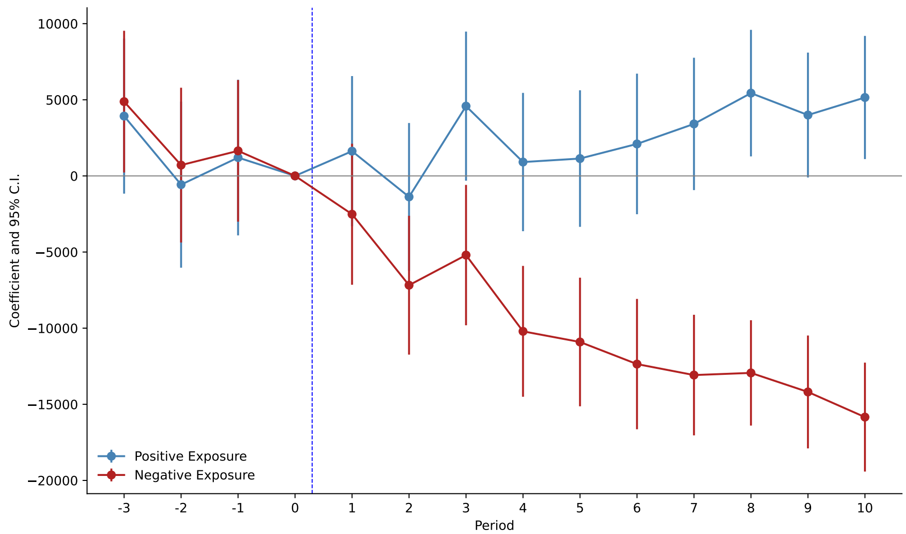
We restrict the sample for those individuals lying in one of the three groups (i.e., $D_i^+ = 1 | D_i^- = 1 | D_i^0 = 1$) and estimate the following difference in differences model.

$$Y_{it} = \alpha_i + \delta_t + \sum_{t \in T} \beta_t^+ \cdot D_i^+ \cdot \delta_t + \sum_{t \in T} \beta_t^- \cdot D_i^- \cdot \delta_t + \varepsilon_{it} \tag{2}$$

where again α_i are individual fixed effects and δ_t are time dummies.

Our estimates are of $\{\beta_t^+, \beta_t^-\}$ are shown in Figure 6.

Figure 6: Effects of Signal Value Change on Credit Access



This Figure shows how credit changes for individuals with small changes in credit scores, but who were far from the population average. The connected dots plot the $\{\beta_t^+, \beta_t^-\}$ estimates from equation 2 and their respective 95% confidence intervals. Sample is restricted to individuals with $\Delta_i \in [-0.25, 0.25]$. Positive exposure are individuals with $s_i > 1$ and negative exposure corresponds to individuals with $s_i < -1$. Standard errors are clustered at the individual level.

We observe that individuals with no substantial change in their credit scores are also affected by the policy. Those who, before and after the information revelation had credit scores above the population average, present an increase of around 5 thousand BRL two years after the policy. Conversely, individuals who, before and after the policy had credit scores below the population average, had a decrease of around 12 thousand BRL in their credit two years after the policy.

These findings are consistent with our framework of lenders' decision-making under imperfect information. The lender should positively update their creditworthiness assessment for those with above-average credit scores as the signal becomes more credible. The opposite holds for those with below-average credit scores.

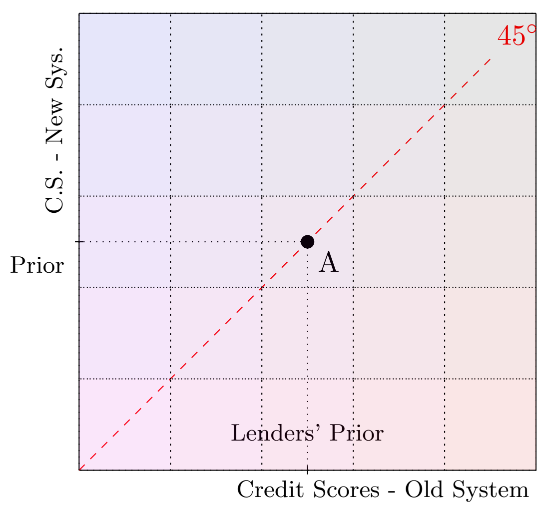
5.2.C Effects over the Joint Distribution of Credit Scores

The previous exercises are helpful as they isolate the two channels through which additional information affects individuals' credit access. However, they are restricted to specific parts of the joint distribution of credit scores. In what follows, we show how credit changes over the whole joint distribution of credit scores by estimating the function $h(s_i, s_i')$ over the full space spanned by $s_i \times s_i'$. This allows us to test our conceptual framework's predictions and estimate and compare the effects of increasing precision and changes in signal value across the joint distribution.

We start with a semi-parametric approach. We divide the joint distribution of credit scores into a grid of equally sized bins, defined by their credit score values in the old and new systems. We then estimate the average credit access change for each group.

The diagram below provides intuition for our bins. Vertical dashed lines represent the division between groups in the old system, and horizontal dashed lines the division into groups of the new credit score system. Each square represents one of the groups used in the estimation.

Diagram of the Empirical Strategy to Estimate the Effects over the Joint Distribution of New and Old Credit Scores



Formally, the sample is divided into five groups of equal size based on the z-score of credit scores in the old system, with groups defined by $k \in \mathcal{K} = \{1, 2, 3, 4, 5\}$. $D_i^k = 1$ if individual i belongs to the group k. Similarly, the sample is divided into equally sized groups based on the z-score of credit scores in the positive system, with groups defined by $j \in \mathcal{J} = \{1, 2, 3, 4, 5\}$ and $D_i^j = 1$ if individual i belongs to the group j. Each group in $\mathcal{K}$ ranges over 0.6 standard deviations of the old system credit score (i.e., $(\max s_i \in k) - (\min s_i \in k) = 0.6$) and Group k=3 is centered around zero. Groups in $\mathcal{J}$ range over 0.8 standard deviations of the new system credit score, and j=3 is also centered around zero. Ranges are slightly different between both systems because the distribution of credit scores is more spread in the new system than in the old one, as we show in Figure A4.[21]

[21] Although rather arbitrary, our choices in the definition of groups are relatively innocuous in terms of our findings, as shown below by our linear and non-parametric assumptions defined below.

The interaction $D_i^k \cdot D_i^j = 1$ defines 25 groups across the joint distribution of credit scores. This is equivalent to approximating the function that determines changes in credit as follows:

$$h(s_i, s_i') \approx h_{sp}(s_i, s_i') = \sum_{k \in \mathcal{K}} \sum_{j \in \mathcal{J}} \beta^{kj} \cdot D^j(s_i') \cdot D^k(s_i)$$

We estimate $\{\beta^{kj}\}$ with OLS through the following equation:

$$Y_{it} = \alpha_i + \delta_t + \sum_{k \in \mathcal{K}} \sum_{j \in \mathcal{J}} \beta^{kj} \cdot D_i^j \cdot D_i^k \cdot Post_t + \varepsilon_{it} \tag{3}$$

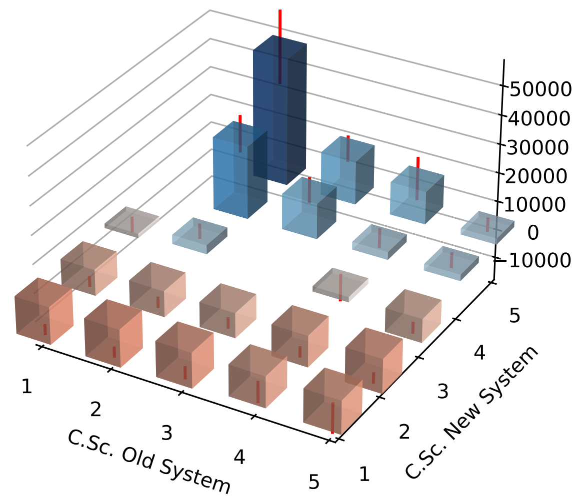
where once more α_i are individual fixed effects, δ_t are time dummies and $Post_t$ is an indicator function that takes value one in periods after the policy ($t > 0$).

In our estimation, we omit $D^3 \cdot D^3$, the group for which both the effects of the signal's value and the effects of the signal's precision are 0 according to our framework. Therefore, coefficients $\{\beta^{kj}\}$ correspond to difference in differences estimates of the change in credit access caused by the policy for individuals in the joint group defined by $D^k \cdot D^j$. Our identifying assumption is that credit in these groups would follow trends parallel to the one of the group $D^3 \cdot D^3$ in the absence of the policy.[22]

We present our estimates in Figure 7. The 3-D visualization is helpful for the interpretation of our findings. Each bar corresponds to a given coefficient, with 95% confidence intervals plotted in the red lines. Bars are organized such that the x-axis (labeled C. Sc. old system) indexes coefficients for a given group k, and the y-axis (labeled C.Sc. new system) indexes coefficients for a given group j. Thus, for example, the bar plotted in C. Sc. old system k= 2, and C. Sc. new system j= 5, corresponds to $\beta^{2,5}$, the difference in differences estimate of the change in credit for individuals who were in group 2 of credit scores in the old system, and 5 with credit scores under the new system. Figure A7 shows the same plot from different angles. Figure A8 shows a 2-dimensional visualization of the estimates in a heatmap.

[22]This implies as well that we restrict the function $h_{sp}(s_i, s_i')$, imposing $h_{sp}(s_i, s_{i'}) = 0$ if $s_i \in 3$ and $s_i' \in 3$

Figure 7: Estimates of Change in Credit over the Joint Distribution of Credit Scores



This Figure shows our estimates of changes in credit over the joint distribution of credit scores, i.e., the estimates of coefficients $\{\beta^{kj}\}$ from equation 3. Each bar corresponds to a given coefficient, with 95% confidence intervals plotted in the red lines. Standard Errors are clustered at the individual level. Bars are organized such that the x-axis (labeled C. Sc. old system) indexes coefficients for a given group k, and the y-axis (labeled C.Sc. new system) indexes coefficients for a given group j. Positive estimates of β^{kj} are shown in blue, whereas negative estimates are shown in red. $\beta^{14}, \beta 15$ are not defined because there is no individual in the sample in those groups of the joint distribution of credit scores.

Our estimates show that credit changes over the joint distribution are consistent with our conceptual framework. First, we can compare estimates *vertically*. This implies comparing individuals with similar levels of the old system credit scores and different values of new system credit scores. We observe that changes in credit are increasing as the groups of new system credit scores increase. We can also compare our estimates *horizontally*. These comparisons are across individuals with similar levels of new system credit scores but different levels in the old system. Our estimates suggest that changes in credit decrease horizontally. This suggests a *catch-up* of groups towards their new creditworthiness assessment.

Lastly, we can compare estimates *diagonally*. By doing so, we are comparing changes in credit across groups that had similar changes in the value of the credit score. Our estimates increase as individuals go from lower to higher credit scores, consistent with our proposition outlined in the conceptual framework. To further visualize these comparisons, Figure A9 plots linear fits between coefficients over *vertical*, *horizontal*, and *diagonal* comparisons, and Table A2 shows the estimates of linear fits between coefficients.

To pin down values for the *vertical*, *horizontal*, and *diagonal* comparisons, we make parametric assumptions. In particular, we assume write changes in credit as a linear function of these signals: $h_l(s_i, s'_i) = a \cdot s_i + b \cdot s'_i$. We estimate $h_l(s_i, s'_i)$ with OLS through the following equation:

$$Y_{it} = \alpha_i + \delta_t + \beta_0 \cdot \text{C. Sc. Old sys.}_{\cdot i} \cdot \text{Post}_t + \beta_1 \cdot \text{C. Sc. New sys.}_{\cdot i} \cdot \text{Post}_t + \varepsilon_{it} \quad (4)$$

where C. Sc. Old sys.$_{\cdot i}$, C. Sc. New sys.$_{\cdot i}$ are the Z-score of credit scores under the old and new systems calculated in the last period before the implementation of the policy.

Our findings are presented in Figure 8. On the left-hand side, the table shows the estimated coefficient and standard deviation of β_0, β_1. On the right-hand-side, the figure shows the predicted changes in credit which consists of $\hat{y}_i = \hat{\beta}_0 \cdot \text{C. Sc. Old sys.}_{\cdot i} \cdot \text{Post}_t + \hat{\beta}_1 \cdot \text{C. Sc. New sys.}_{\cdot i}$.

The linear model reveals that the effects of signal value represent a 10.95 thousand Reais increase in credit for each standard deviation increase in positive system credit scores, which represents a 20% increase relative to the average individual in the sample. In terms of precision effects, there is a 5.78 thousand Reais increase in credit with a one standard deviation increase in both positive and negative system credit scores, equating to a 10% increase relative to the average individual in the sample.

In Appendix D, we relax the linearity assumption and estimate the function $h(s_i, s'_i)$ non-parametrically with sieve-estimators. This allows us to further test the implications of our conceptual framework and evaluate the quality of the linear fit. We show that the non-parametric estimates propose a qualitatively similar function of changes in credit over the joint distribution of credit scores. Furthermore, average partial derivatives of the non-parametric estimates also suggest the *vertical*, *horizontal*, and *diagonal* patterns described above when comparing changes in credit across groups. Lastly, when calculating partial derivatives at each point of the joint distribution of credit scores, we show that in the majority of the space spanned by $s_i \times s'_i$, the direction of partial derivatives correspond to the patterns predicted by our conceptual framework.

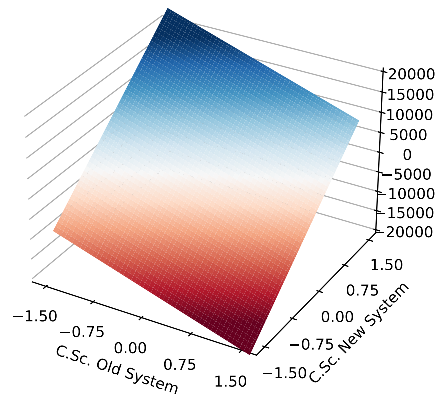
5.3 Who gets more affected and distributional consequences

We next investigate the impacts of revealing information about borrowers on credit inequality and calculate differences in the effects of the policy across demographic groups.

Let C_{i0}, C_{i1} define credit access without and with the policy. Our goal is to compare

Figure 8: Estimates of Changes in Credit over the Joint Distribution with Linear $h(s_i, s_i')$

	(1)
	Credit
C. Sc. Old Sys.	- 5,172.3
	(273.66)
C. Sc. New Sys.	10,951.58
	(370.70)
Observations	2875942



This Figure shows the estimates of changes in credit with linear restriction. In the Table on the left-hand side, we show our estimates for the coefficient and the standard deviation of β_0, β_1 from equation 4. On the right-hand-side, the figure shows the predicted changes in credit which consists of $\hat{y}_i = \hat{\beta}_0 \cdot$ C. Sc. Old sys.$_{\cdot i} \cdot$ Post$_t$ + $\hat{\beta}_1 \cdot$ C. Sc. New sys.$_{\cdot i}$.

$\mathcal{G}_0(C_0)$ and $\mathcal{G}_1(C_1)$, which represent the counterfactual distribution of credit access in the absence of the policy and the distribution of credit under the actual policy. Using the estimates of the effects of the policy on credit access described above, we first compute for each individual their credit access with and without the policy at any given period as:

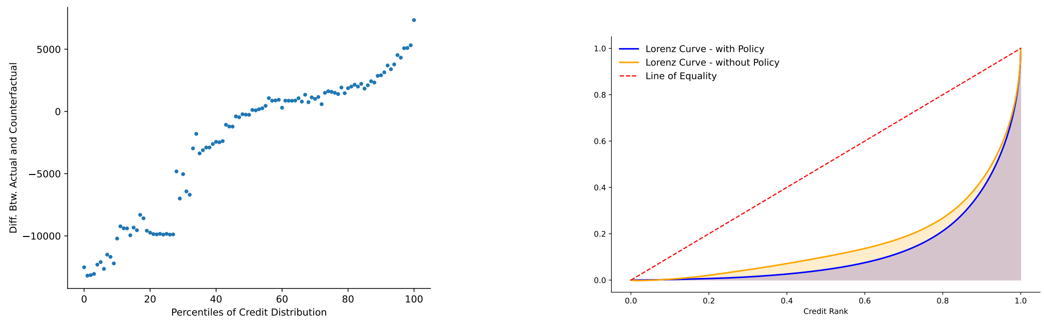
$$C_{it0} = \hat{\alpha}_i + \hat{\hat{\delta}}_t$$
$$C_{it1} = \hat{\alpha}_i + \hat{\hat{\delta}}_t + \hat{\beta}^{kj} \cdot D_i^k \cdot D_i^j \cdot Post_t$$

For periods before the policy implementation, we see that $C_{it0} = C_{it1}$. Thus, we focus only on periods after the policy implementation and use the average value of C_{it0}, C_{it1} across periods for each individual as their credit access with and without the policy (C_{i0}, C_{i1}).

Different statistics allow us to characterize the differences between both distributions. First, we calculate the variance of both distributions. We find that under the policy, variance of the distribution of credit is 2.5% higher than without the policy.[23]. We then conduct two different exercises with results shown in Figure 9 In Panel (a), we plot the average difference $C_{i1} - C_{i0}$ across percentiles of credit distribution. In Panel (b), we show Lorenz curves of the actual and counterfactual distributions of credit.

[23]
$$\frac{\mathrm{Var}(G_1(C_1)) - \mathrm{Var}(G_0(C_0))}{\mathrm{Var}(G_0(C_0))} = 0.025$$

Figure 9: Comparisons between Distributions of Credit



(a) Difference in Credit across Distribution

(b) Lorenz Curve of Credit Access

This Figure shows comparisons between distributions of credit with and without the policy $\mathcal{G}_0(C_0)$ and $\mathcal{G}_1(C_1)$. In Panel (a), we plot the average difference $C_{i1} - C_{i0}$ across percentiles of credit distribution. In Panel (b), we show Lorenz curves of the actual and counterfactual credit distributions.

We observe that changes in credit are almost monotonically increasing on the percentiles of credit distribution. The Lorenz curve shows that the distribution of credit without the policy is closer to the line of equality, indicating a more unequal credit distribution with the policy.

Comparisons by Demographic Characteristics: We can also characterize how the policy affected individuals differently according to their demographic characteristics. Our analysis is based on two dimensions in an Oaxaca-Blinder spirit. First, estimated treatment effects can be different according to each group. Second, the distribution of groups can vary across the joint distribution of credit scores. Thus, even if effects are homogeneous across different groups, the policy can have different aggregate effects for different groups due to a composition component.

Empirical Strategy: Let $G_i \in \{0, 1\}$ represent a binary variable that defines a specific demographic group, such as male vs. female or white vs. nonwhite. Additionally, C_{i0} and C_{i1} still represent individual credit access before and after the implementation of the policy, respectively.

We are interested in comparing the effect of the policy on credit access across different demographic groups. Specifically, we analyze the difference in the expected change in credit access between two groups. Formally, we make comparisons of the following type:

$$\mathbb{E}[C_{i1} - C_{i0} | G_i = 1] - \mathbb{E}[C_{i1} - C_{i0} | G_i = 0].$$

To estimate the differences in credit access before and after the policy for each group, we rely on our semi-parametric approach, exploring the 5 groups of the old credit score $\mathcal{K} \in \{1, 2, 3, 4, 5\}$ and the 5 groups of the new system credit scores $\mathcal{J} \in \{1, 2, 3, 4, 5\}$. This generates the following estimating equation:

$$Y_{it} = \alpha_i + \delta_t + \sum_{k\in\mathcal{K}} \sum_{j\in\mathcal{J}} \beta_{g0}^{kj} \cdot D_i^k \cdot D_i^j \cdot \text{Post}_t + \sum_{k\in\mathcal{K}} \sum_{j\in\mathcal{J}} \beta_{g1}^{kj} \cdot D_i^k \cdot D_i^j \cdot \text{Post}_t \cdot G_i + \varepsilon_{it}. \quad (5)$$

where α_i represents individual-specific effects, δ_t represents time-specific effects, D_i^k and D_i^j represent credit score categories, and Post_t is a dummy variable indicating the post-policy period. The coefficients β_{g0}^{kj} capture the impact of the policy for the baseline group, while the interaction terms $\beta_{g1}^{kj} \cdot G_i$ capture the differential effect for group $G_i = 1$.

Given these estimates, we define the expected change in credit access for individuals according to the part of the grid of the joint distribution of credit to which they belong. This can be written as:

$$\begin{aligned}
\mathbb{E}[C_{i1} - C_{i0} | G_i = 0, D_i^k = 1, D_i^j = 1] &= \hat{\beta}_{g0}^{kj} \\
\mathbb{E}[C_{i1} - C_{i0} | G_i = 1, D_i^k = 1, D_i^j = 1] &= \hat{\beta}_{g0}^{kj} + \hat{\beta}_{g1}^{kj}
\end{aligned}$$

We then express the expected change in credit access for each demographic group as follows:

$$\begin{aligned}
\mathbb{E}[C_{i1} - C_{i0} | G_i = 0] &= \mathbb{E}[\hat{\beta}_{g0}^{kj}] \\
\mathbb{E}[C_{i1} - C_{i0} | G_i = 1] &= \mathbb{E}[\hat{\beta}_{g0}^{kj} + \hat{\beta}_{g1}^{kj}]
\end{aligned}$$

To calculate the value of expected changes by demographic group, we weigh the coefficient change by the number of people in each of the bins of our grid that divides the joint distribution of credit scores. This is equivalent to:

$$\begin{aligned}
\mathbb{E}[C_{i1} - C_{i0} | G_i = 0] = \mathbb{E}[\hat{\beta}_{g0}^{kj}] &= \frac{\sum_i (1 - G_i) \cdot D_i^k \cdot D_i^j \hat{\beta}_{g0}^{kj}}{\sum_i (1 - G_i)} \\
\mathbb{E}[C_{i1} - C_{i0} | G_i = 1] = \mathbb{E}[\hat{\beta}_{g0}^{kj} + \hat{\beta}_{g1}^{kj}] &= \frac{\sum_i G_i \cdot D_i^k \cdot D_i^j (\hat{\beta}_{g0}^{kj} + \hat{\beta}_{g1}^{kj})}{\sum_i G_i}
\end{aligned}$$

Thus, we can write the difference in policy effects between the two groups as:

$$\mathbb{E}[C_{i1} - C_{i0}|G_i = 1] - \mathbb{E}[C_{i1} - C_{i0}|G_i = 0] = \frac{\sum_i G_i \cdot D_i^k \cdot D_i^j (\hat{\beta}_{g0}^{kj} + \hat{\beta}_{g1}^{kj})}{\sum_i G_i} - \frac{\sum_i (1 - G_i) \cdot D_i^k \cdot D_i^j \hat{\beta}_{g0}^{kj}}{\sum_i (1 - G_i)} \tag{6}$$

The expression suggests that to understand which group is more affected by the policy, we must consider two components. First, the groups are potentially differently distributed across the joint distribution of credit scores. Second, the heterogeneous treatment effects, captured by the values of $\hat{\beta}_{g1}^{kj}, \hat{\beta}_{g0}^{kj}$.

To disentangle between both effects, we can also make an Oaxaca-Blinder type decomposition, replacing the coefficients above by $\hat{\beta}^{kj}$ from equation 3. This would give us just the effects of the differential composition of individuals by group. [24]

Results: We focus our analysis on comparisons across four key demographic dimensions: gender, race, education, and age. Table 2 summarizes our findings. In Column (1), we show the difference with homogeneous treatment effects, while in column (2), we show the differences allowing for heterogeneous treatment effects by group (i.e., equation 6)

We observe no substantial difference in credit access by gender. When looking only at the composition side, women have a slightly higher credit change than men. This implies that women are slightly overrepresented among groups with positive changes in credit access. However, this flips when observing the Total Effects. This is because the estimates of credit increases are slightly lower for positively exposed women than men. This can be seen in Panel (b) of Figure A10, where we show our estimates of β_{g1}^{kj}.

When comparing effects by race, we observe that the policy enhanced race inequality in credit. This is due both to compositional effects, i.e. nonwhite individuals are overrepresented in negatively exposed parts of the joint distribution of credit scores, and to heterogeneous treatment effects. In panel (c) of Figure A11, we illustrate the compositional aspect by showing the share of nonwhite individuals in each of the 23 groups of the joint distribution. We observe that they are overrepresented at groups with lower values of credit scores. At the same time, by looking at panel (b), we see that increases in credit are substantially lower for nonwhite individuals than for white ones. Thus, through both

[24] This essentially means calculating the following formula

$$\frac{\sum_i G_i \cdot D_i^k \cdot D_i^j (\hat{\beta}^{kj})}{\sum_i G_i \cdot D_i^k \cdot D_i^j} - \frac{\sum_i (1 - G_i) \cdot D_i^k \cdot D_i^j \hat{\beta}^{kj}}{\sum_i (1 - G_i) \cdot D_i^k \cdot D_i^j}$$

where $\hat{\beta}^{kj}$ are the coefficients estimated from equation 3 and presented in Figure 7

Table 2: Effects on Credit Differences Across Groups

	(1) Composition	(2) Total
Gender		
Women - Men	317.88	-745.18
Race		
Nonwhite - White	-2034.85	-4629.11
Education		
High School - Less than H.S.	1167.26	2659.22
Some College - Less than H.S.	6089.81	17462.14
Age		
$<$ 40 y.o. - $\geq$ 40 y.o.	-3002.64	-1888.03

This Table shows our estimates of how difference in credit access between groups change due to the policy. Values correspond to our estimates of equation 6 with different comparisons in gender, race, education, and age. When looking at education, we add a second set of coefficients β^{kj}_{g2} and estimate equation 5 using the full sample. Column (1) shows the compositional role on the total effect by fixing treatment effects for the average population estimate (β^{kj} from equation 3.

compositional and treatment effects, our estimates suggest that the policy increased the racial inequality in credit access.[25]

These findings are a counterpoint to Blattner and Nelson (2021), who argue that reducing the gap in credit score noise between white and black individuals in the U.S. could substantially reduce the racial inequality in mortgage markets. Our *policy* differs from theirs as it is theoretically race-neutral (borrowers' information increases for both white and nonwhite individuals). In our case, the revelation of information amplifies overall credit inequality. This disproportionately affects nonwhite and less educated individuals as they are disproportionately represented in the less favored parts of the joint distribution of credit scores. Thus, race-neutral increases in information can increase racial inequality in credit access.

[25]The same pattern occurs when looking at education levels. Individuals with less than high school education have a decrease in credit relative to more educated groups. Both results in the table and in Figure A12 show that composition and treatment effects play a role in this. When looking at age, we observe that older individuals benefit more from the policy, despite treatment effects being more favorable to younger individuals, as we can see in Figure A14.For visualization purposes in case the 3D graphs are not straightforward, Figure A15 shows heatmaps of the distribution of demographic characteristics across the joint distributions of credit scores.

6 Effects on Default Rates

In this section, we investigate the implications of the revelation of information about borrowers on the quality of loans. So far, we have demonstrated that credit is reallocated along the joint distribution of credit scores. However, we have not yet addressed the outcomes resulting from these changes. Next, we will explore whether the policy affected the quality of credit supplied by examining the key question: did revealing information reallocate credit to more or less risky loans? To investigate this, we will delve into defaults, commonly used in the literature as an approximation for the costs of providing loans to lenders (Liberman et al., 2018; DeFusco et al., 2022).

The ideal empirical setting to analyze default rates would involve observing loan-specific defaults to estimate the effects on the defaults of new loans. In our approach, we approximate default rates by calculating the ratio between an individual's total *financial delinquency* and their total amount of credit. We start by estimating the effects of the policy on overall financial delinquency. Next, we assess how default rates vary across the joint distribution of credit. We then calculate the default rates for credit reallocated by the policy. Finally, we compare the default rates of credit increases to those of credit decreases.

6.1 Effects on Financial Delinquency

We begin our analysis by estimating the effects on total financial delinquency, which refers to the amount reported by financial institutions to credit bureaus when individuals fail to meet their financial obligations on time. Typically, the lender issues a notice regarding the debt and sets a deadline for regularization. If the deadline is not met, the overdue debt may be marked as delinquent and reported to the credit bureau.

We proceed with the same research design as used in the previous section, estimating the effects of the signal's value (equation 1), the effects of the signal's precision (equation 2), and the effects over the joint distribution of credit scores (equation 3). Now, instead of total credit at a given period as the outcome, we use the total amount of financial delinquency at a given period. This includes 0s whenever individual i at a given period t does not have any financial delinquency reported. Figure 10 shows our findings for the three different empirical strategies[26].

Panel (a) shows difference in differences estimates comparing over time, financial delinquency of individuals with credit score increases and decreases with those that did not have

[26] Figure A16 shows a 2-dimensional visualization in a heatmap of the estimates from Panel (c) of Figure 10.

substantial changes in their scores with the policy. We observe that prior to the policy, there are no differences in the trend of financial delinquency across groups, which we take as evidence that our identifying assumption holds. After the policy, we observe that those who were positively affected, increase their total financial delinquency by almost 3 thousand BRL 2 years after the policy. On the other hand, negatively exposed individuals decrease financial delinquency by around 1 thousand BRL.

We find similar results when looking at individuals who did not change their credit scores but were distant from the population average. Those estimates are plotted in Panel (b). We see that positively exposed individuals had on average 1800 BRL more in financial delinquency two years after the policy. We observe no significant change for those negatively affected.

When looking at the effects of the policy on financial delinquency over the joint distribution of credit scores, we observe that most of the changes in financial delinquency are concentrated at the lower part of the distribution of the old system credit scores. Although the patterns are less clear than in the credit results, our estimates also suggest the *vertical*, *horizontal* and *diagonal* patterns. Financial delinquency increases when comparing changes across groups *vertically*, except for those in the first group of old system credit scores. Estimates on average also increase *diagonally*, which implies that for groups with similar differences between credit scores in both systems, delinquency changes increase along the distribution of credit scores. When comparing estimates *horizontally*, we observe slightly different results. For levels below the average of new system credit scores, estimates are increasing horizontally.[27]

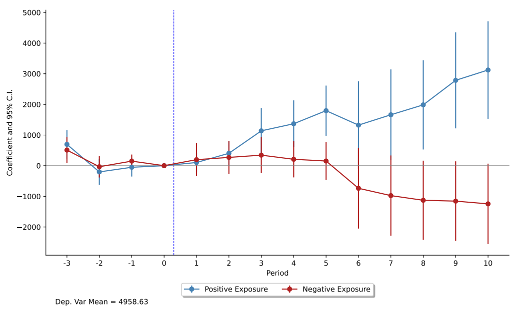
6.2 Default Rates

Financial delinquency tends to rise among groups that experience an increase in credit access. However, as the availability of credit rises, the amount of money over which an individual can be delinquent increases as well. A more policy-relevant question in this context is to determine the proportion of the credit extended that ultimately results in financial delinquency. To understand this, we first calculate average default rates over the joint distribution of credit scores. We define default rates as the ratio between the financial delinquency of an individual at a given period and their total credit at the same period:

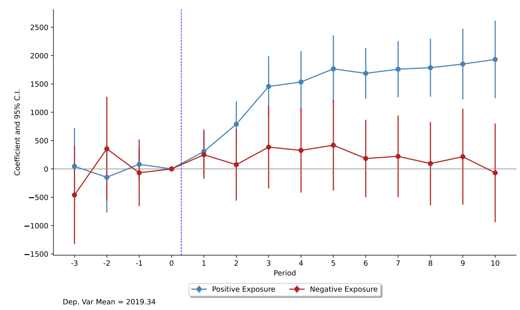
$$\text{Default Rate} = \frac{\text{Financial Delinquency}}{\text{Credit}}$$

[27]Similarly to our analysis on credit, Figure A17 plots linear fits between coefficients over *vertical*, *horizontal*, and *diagonal* comparisons, and Table A3 shows the estimates of linear fits between coefficients.

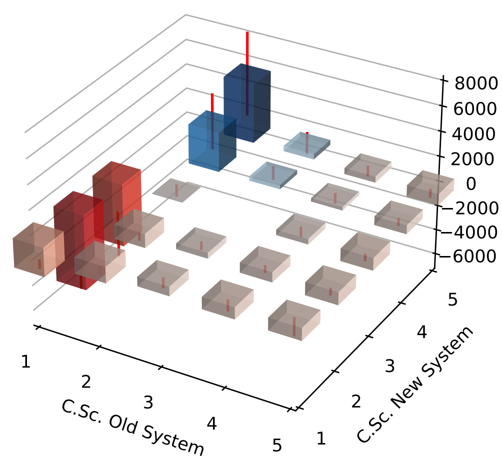
Figure 10: Effects on Financial Delinquency



(a) Signal Value Effects



(b) Precision Effects



(c) Effects over the Joint Distribution

This Figure shows estimates of the effect of the policy on Total Financial Delinquency. Individuals with no financial delinquency at a given period are included and have $Y_{it} = 0$. Panel (a) estimates the effects of the signal value, comparing individuals with $\Delta_i \in (-0.25, 0.25)$ with those positively affected $\Delta_i \in (1, 1.5)$ and negatively affected $\Delta_i \in (-1.5, -1)$. The sample is restricted to those three groups. Panel (b) estimates the effects of the signals' precision, comparing individuals with $s_i \in (-0.25, 0.25)$ with those positively affected $s_i > 1$ and negatively affected $s_i < -1$. The sample is restricted to individuals in those three groups and $\Delta_i \in (-0.25, 0.25)$. Panel (c) shows estimates of equation 3, using the full sample of individuals.

Since both financial delinquency and credit endogenously change with the information revealed by the policy, we need to be careful in estimating the effects on changes in Default Rates. In particular, since the denominator can be zero for a given individual, that generates cases where default rates are not defined.

We overcome this using our estimates of financial delinquency and credit access separately to construct Default Rates in the presence and in the absence of the policy. It is useful to write down both objects under a Potential Outcomes notation as follows:

$$DR^0_{it} = \text{Default Rate}_{it}|\text{no policy} \approx \frac{E[\text{Financial Delinquency}_{it}|\text{no policy}]}{E[\text{Credit}_{it}|\text{no policy}]}$$
$$DR^1_{it} = \text{Default Rate}_{it}|\text{with policy} \approx \frac{E[\text{Financial Delinquency}_{it}|\text{with policy}]}{E[\text{Credit}_{it}|\text{with policy}]}$$

We use our estimates of the policy effects on financial delinquency and credit from our semi-parametric approach over the full joint distribution of credit scores to construct the expected values of both measures with and without the policy.[28]

For each outcome, the corresponding expectations with and without the policy are calculated as follows:

$$\begin{aligned} E[Y_{it}|\text{no policy}] &= \hat{\alpha}_i + \hat{\delta}_t \\ E[Y_{it}|\text{with policy}] &= \hat{\alpha}_i + \hat{\delta}_t + \textstyle\sum_{k\in\mathcal{K}}\sum_{j\in\mathcal{J}} \hat{\beta^{kj}} \cdot D^k_i \cdot D^j_i \cdot Post_t \end{aligned}$$

To visualize how default rates change with the policy, we aggregate them to the groups defined over the joint distribution of credit $D^k(s_i)D^j(s'_i)$. To do so, we take the average of $E[Y_{it}|\text{no policy}]$ and $E[Y_{it}|\text{with policy}]$ in each group across periods after the implementation of the policy, and calculate the ratio between expected values for financial delinquency and credit.[29]

We show our findings for default rates in Figure 11. In panel (a), we plot the default rates of each group in the presence of the policy, whereas panel (b) shows counterfactual default rates in its absence.[30] We can look at the patterns similarly to our analysis on credit. In both Panels (a) and (b), default rates are increasing *vertically* and decreasing *horizontally*. *Vertically*, we see that they are substantially higher for groups with lower credit scores in the

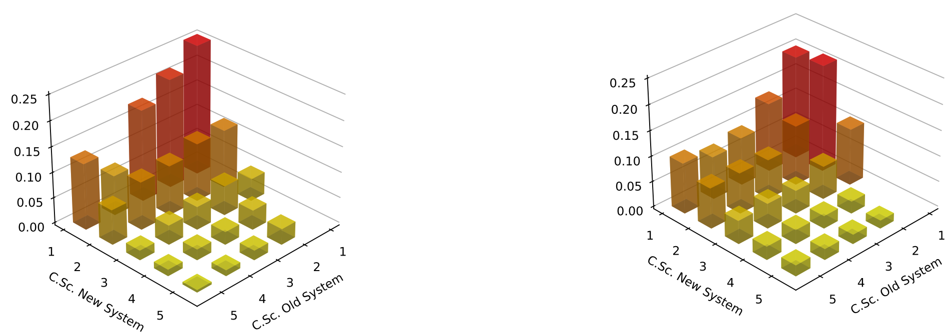
[28] That is, we estimate for both outcomes equation 3, which corresponds to $Y_{it} = \alpha_i + \delta_t + \sum_{k\in\mathcal{K}}\sum_{j\in\mathcal{J}} \beta_{kj} \cdot D^k_i \cdot D^j_i \cdot \text{Post}_t + \varepsilon_{it}$

[29] In periods before the policy $E[Y_{it}|\text{with policy}] = E[Y_{it}|\text{no policy}]$ since $Post_t = 0$

[30] We change the angle from the previous Figures for better visualization, but Figure A20 shows the same estimates in the previous angle.

new system (group 1), and decrease as the new system credit scores increase for all initial levels of old system credit scores. At the same time, *horizontally* we observe that for a fixed group in the new system credit score, default rates are decreasing as we move from worse to better groups of the old system credit scores. In turn, Default Rates decrease as we move across groups *diagonally*, suggesting that, for the same difference in credit scores across the new and old systems, individuals with higher credit scores individuals have lower default rates. When looking at the differences in default rates between actual and counterfactual estimates, we do not observe a clear pattern across the distribution. These results are shown in Figure A19.

Figure 11: Default Rates Across the Joint Distribution of Credit Scores



(a) Default Rates Post Policy

(b) Counterfactual Default Rates in the Absence of the Policy

This Figure shows average Default Rates in the periods after the policy. Panel (a) plots our estimates of default rates in the presence of the policy, whereas Panel (b) plots our estimates of Default rates in the absence of the Policy.

6.3 Default Rates of Reallocated Credit

Having established our definition of default rates, and shown how they vary over the joint distribution of credit scores, we can estimate default rates of the reallocated credit. Under the assumption that the Cadastro Positivo did not affect default rates of credit that would have been observed in the absence of the policy, we can write the default rate of the marginal credit as the ratio between the change in financial delinquency and changes in credit(Angrist and Imbens, 1995).[31] To show this, it is useful to introduce more potential outcomes

[31] This is the same assumption as in Card and Hyslop (2005) and subsequent papers that evaluate outcomes for marginally treated observations (compliers) in the absence of an empirical setting that allows more precise identification of outcome tests.

notation. C_i^0, C_i^1 are still representing the credit access for individual i in the absence of the policy in its presence. DR_i^1, DR_i^0 are the default rates in the presence and absence of the policy, respectively.

We can write $C_i^1 \cdot DR_i^1 = (C_i^1 + C_i^0 - C_i^0) \cdot DR_i^1$, by adding and subtracting C_i^0 in the right hand side. We define the object $\Delta C_i = C_i^1 - C_i^0$, the change in credit caused by the policy. Thus $C_i^1 \cdot DR_i^1 = (C_i^0 + \Delta C_i) \cdot DR_i^1$. Our assumption implies that among the credit given with the policy, the C_i^0 part would have default rate DR_i^0, whereas the marginal credit (ΔC_i) would have their own default rate DR^{Re}. Thus, we can rewrite $C_i^1 \cdot DR_i^1 = C_i^0 \cdot DR_i^0 + \Delta C_i DR^{Re}$. From this, it is straightforward to see that

$$DR^{Re} = \frac{\mathbb{E}[C_i^1 \cdot DR_i^1 - C_i^0 \cdot DR_i^0]}{\mathbb{E}[\Delta C_i]} = \frac{\mathbb{E}[\Delta Delinquency_i]}{\mathbb{E}[\Delta C_i]}$$

Thus, having consistent estimates of the effects of the policy on financial delinquency and credit allows us to calculate the default rate of the marginal credit. We use our estimated coefficients from our semi-parametric analysis on delinquency and credit as approximations of the effects for each individual. We can write this as:

$$\Delta \text{Delinquency}_i = \mathbb{E}[\Delta \text{Delinquency}_i | D_i^k = 1, D_i^j = 1] = \hat{\beta}_{Del}^{kj}$$
$$\Delta \text{C}_i = \mathbb{E}[\Delta \text{Credit}_i | D_i^k = 1, D_i^j = 1] = \hat{\beta}_{C}^{kj}$$

where $\hat{\beta}_{Del}^{kj}, \hat{\beta}_{C}^{kj}$ are the estimated coefficients of equation 3 using financial delinquency and credit as outcomes respectively, and D_i^k, D_i^j are dummies indicating that individual i belongs to groups k, j of the new and old system credit scores.

To compare default rates over credit that was given because of the policy, counterfactual credit that would have been given in its absence, but was not given because of the policy, we divide our sample into those with positive change in credit and those with negative change in credit. We then compute the average changes in financial delinquency and credit weighted by the number of observations in each bin of joint distribution of credit scores. This can be written as:

$$\begin{aligned} Positive &= \frac{\sum_i D_i^k \cdot D_i^j \cdot \hat{\beta_{Del}^{kj}}}{\sum_i D_i^k \cdot D_i^j \cdot \hat{\beta_{C}^{kj}}} \quad \text{for k, j with} \quad \mathbb{E}[\Delta \text{Credit} | D_i^k = D_i^j = 1] > 0 \\ Negative &= \frac{\sum_i D_i^k \cdot D_i^j \cdot \hat{\beta_{Del}^{kj}}}{\sum_i D_i^k \cdot D_i^j \cdot \hat{\beta_{C}^{kj}}} \quad \text{for k, j with} \quad \mathbb{E}[\Delta \text{Credit} | D_i^k = D_i^j = 1] < 0 \end{aligned} \tag{7}$$

where *Positive* shows our estimates of the default rates of credit that was observed because of the policy, and *Negative* shows our estimates of the default rate of credit that would have been observed in the absence of the policy, but was not observed because of the policy.

We summarize our findings about default rates in Table 3. First, we see in column (1) that the default rate of credit that was positively reallocated was around 12p.p. lower compared to the credit that would have been given in the absence of the policy.

Table 3: Default Rates of Credit Reallocated with the Policy

	(1) Default Rate Reallocated Credit
Default Rate of Credit that was allocated because of the Policy	0.03
Default Rate of Credit that was not allocated because of the policy	0.15

This Table shows our estimates of the default rate of credit that was reallocated positively and negatively because of the policy. These estimates were calculated using our findings for changes in credit and financial delinquency over the joint distribution of credit scores using our expression in Equation 7.

Discussion on the Type of Selection in this Market

Classical discussions about the consequences of imperfect information in credit markets focus on the type of selection present in the market. Stiglitz and Weiss (1981) demonstrate that asymmetric information leads to adverse selection, where the marginal borrower is less risky than supra-marginal ones, resulting in credit rationing. In contrast, De Meza and Webb (1987) argue the opposite, showing conditions under which asymmetric information leads to advantageous selection, where the marginal borrower is less profitable than supra-marginal ones, resulting in overinvestment.

Recent papers have utilized default rates to interpret the type of selection in credit markets. Liberman et al. (2018); DeFusco et al. (2022); Jansen et al. (2022), borrowing insights from the insurance markets literature of Einav et al. (2010), examine how changes in default rates following credit expansion can serve as a measure of the quality of marginal borrowers, often finding evidence of adverse selection. If we interpret our results through their frameworks, our findings would imply advantageous selection in credit markets. This occurs because, on average, marginal credit is riskier than the credit that would have been extended without the policy. Liberman et al. (2018) specifically interpret absolute delinquency as their measure of default, and if we adopt their interpretation, our findings would suggest adverse selection in the credit market.

7 Effects on Entrepreneurial Activity

In this section, we examine if changes in credit allocation had consequences beyond credit market outcomes, focusing on Entrepreneurial activity. We begin by describing entrepreneurship in our context, with statistics on firms and entrepreneurs' characteristics for the universe of new formal firms in São Paulo. We then use our sample from the credit bureau matched with firm records to understand if the reallocation of credit across the population had consequences in firm creation (extensive margin of entrepreneurship) and firm quality (intensive margin). Lastly, we assess whether the reallocation of personal credit led to an improved allocation of resources to entrepreneurial activity.

Additional Sample: To gain precision in the analysis, we increase the sample of individuals by around 560 thousand randomly selected individuals from the same pool of adults in the state of São Paulo used in our credit market analysis. We only observe credit scores in the new and old systems for these additional individuals. We could not obtain individual-level credit and default information for them (this is why this sample is not included in previous exercises). As expected, given that the sample is randomly selected, the characteristics of both samples are extremely similar. In Table A4, we show summary statistics of both samples. We then match credit scores data with the universe of formal firm ownership in the State of São Paulo. Therefore, we observe if and when each of the individuals created their first firm. Using the firm identification number, we then match them to firm-level data provided by our partner institution. Appendix C fully details the sample construction procedures.

Before we start our empirical analysis, describing the characteristics of firms and entrepreneurs we observe in our data is useful. Our firm records are restricted to the formal sector, implying that informal firms are excluded from our analysis. In Appendix F, we detail the differences between informal and formal entrepreneurs using household survey data that encompasses both formal and informal sectors. We show that more than 90% of individuals that self-declare as *employers* have a formal business registration.[32] This share

[32]These Figures are strikingly different than those in the well-known Ulyssea (2018) paper, whose analysis point to 70% of employing firms being informal. This is due to three reasons. First, we are looking specifically at the state of São Paulo, the country's richest state, which has substantially lower informality rates than the rest of the country. Second, we look at data 20 years after his analysis. Brazil had a large informality reduction during this period (Haanwinckel and Soares, 2021). Lastly, we use PNAD instead of ECINF. PNAD is supposed to be representative of the full workforce, whereas ECINF is a survey restricted to firms with up to five employees. As shown by Ulyssea (2018), there is a positive gradient between formalization and firm size.

reduces to around 40% when we look at *self-employed* workers.[33] But high informality levels in the latter group are particularly driven by the construction and transportation sectors, with almost 80% of informality. In contrast, retail has, for example, less than 50% of informal self-employment.

Despite not being likely to be *disruptive* entrepreneurs, these firms are still responsible for the majority of job creation. In Appendix E, we combine our entrepreneurship data with matched employer-employee data that covers the entirety of the formal labor market[34] and replicate exercises from Haltiwanger et al. (2013) and Decker et al. (2014) that show the importance of entrepreneurs in job creation in the U.S. We find that firms in their three first years are responsible for around 20% of new jobs and over 80% of *net job creation* highlighting the economic relevance of these new firms.

7.1 Effects on Firm Creation

Next, we investigate if the policy changed the probability that an individual creates a new business.

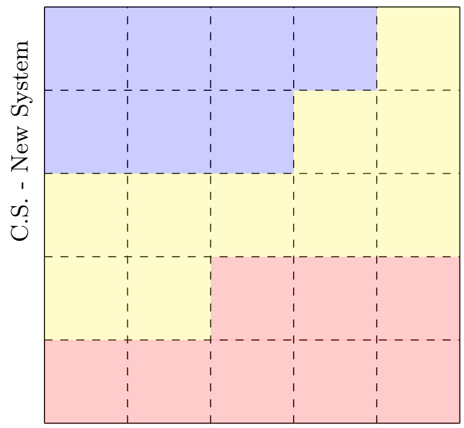
To empirically assess the effects of the policy on business creation, we slightly change our empirical strategy from the previous sections in two ways. First, we group individuals into three groups instead of using the 25 groups defined by the joint distribution of credit scores. We define the three groups from our estimates of the effects in credit access given the policy. The diagram of the joint distribution of credit scores illustrates our comparison groups. Those who lie in the blue area had an increase in their credit access, while those in the red area had a decrease in their credit access. Meanwhile, individuals in the yellow area are considered our control group, as our results show that they did not have substantial changes in credit[35].

[33] The informality rate of self-employed workers reduced substantially since 2010 because of a new tax system referred to as MEI, which was designed specifically for the formalization of these workers. See Hsu Rocha and de Farias (2021) for a complete description of the effects of these new tax systems on firm creation and informality.

[34] We use the well-known RAIS data combined with our entrepreneurship records to do this exercise. Important to highlight that this was done outside of our partner institution environment, as we did not share RAIS information with them.

[35] Using our definitions of the 25 groups and the same notation as before, we can define the three groups as $D^+ = \{D^{14}, D^{15}, D^{24}, D^{25}, D^{34}, D^{35}, D^{45}\}$ and $D_i^- = \{D^{11}, D^{21}, D^{22}, D^{31}, D^{32}, D^{41}, D^{42}, D^{51}, D^{52}\}$, where the first superscript refers to the group in the old system and the second the group in the new system of credit scores.

Diagram of Groups to be Compared in our Empirical Analysis



The second difference from the previous sections is that we move away from the OLS estimation of the difference in differences to a survival model, using hazard models with time-dependent covariates to estimate the probability of opening a business. We do so because individuals who establish a firm at time t are unlikely to do so again at $t + 1$. Our previous approach, using an indicator variable if the individual i opened a firm at period t as Y_{it} would incorrectly measure those at risk of creating a firm.

We focus on the probability of individual i creating their first firm at period t. We define as our *risk set*, those individuals who had never owned a firm up to 3 years before the policy. Excluding individuals who had already created firms before the 3 year window of our analysis implies our final analysis sample comprises around 650 thousand individuals. In Table A5, we summarize the demographic characteristics of our analysis sample in comparison with those who were excluded. The remaining sample is similar in age and gender composition but slightly less educated and more likely to be nonwhite.

Using our analysis sample, we construct an unbalanced panel to estimate the hazard model with time-dependent covariates. In the data, each observation corresponds to an individual x quarter. It covers six years, between the first quarter of 2018 to the last quarter of 2023. The panel is unbalanced because if an individual creates a firm, they are excluded from subsequent periods.

Our estimating equation can be written as follows:

$$\lambda(t|X(t)) = \lambda_0(t) \exp\left(\beta_0^- \cdot D_i^+ + \beta_1^+ \cdot D_i^+ \cdot Post_t + \beta_0^- \cdot D_i^- + \beta_1^- \cdot D_i^- \cdot Post_t + \Gamma X_i\right) \quad (8)$$

where D_i^+ is an indicator that individual i is in the blue area in the diagram above, D_i^-

indicates that they were in the red area, $Post_t$ is an indicator that the respective period is after the implementation of the policy. We include a vector of controls X_i that consists of fixed effects by cells, which are defined by interacting education, gender, race and age.

The coefficients β_1^+, β_1^- capture how the probability of creating a firm changes after the policy for individuals in the positively and negatively exposed groups, relative to those in the control group who on average did not experience substantial changes in their credit access.

We show our estimates in Table 4. Both coefficients β_1^+, β_1^- are extremely close to zero and not statistically significant. Looking at hazard ratios in column (2), we show that our estimates can reject changes in the probability of opening a business as big as 5%.

Table 4: Effects on the Probability of Creating a Firm

	(1)	(2)
	Created a Firm	
	Coefficient	Hazard Ratio
Positive Exposure x Post	0.004104	1.004
	(0.0169)	[0.971,1.038]
Negative Exposure x Post	-0.0179	0.982
	(0.0119)	[0.959,1.006]
Number of Individuals	645040	
Observations	13170363	

This table shows coefficients and hazard ratio estimates from equation 8. In column (1), we show coefficient estimates and standard errors in parenthesis; in column (2), we show the hazard ratio and 95% confidence interval in brackets. The sample comprises all individuals we observe who were never a firm owner until three years before the policy implementation. A row in the data corresponds to sub-spells of a quarter for each individual. If the individual creates a firm, they are not observed in subsequent periods.

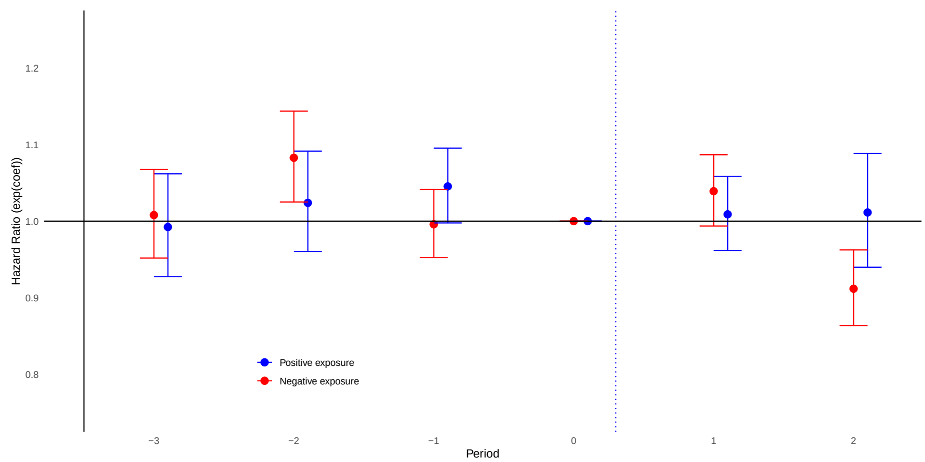
In addition to the pre-post analysis, we can also estimate coefficients for each year relative to the policy. This can be summarized in equation 9 below:

$$\begin{aligned}\lambda(t|X(t)) = \lambda_0(t)\exp\Big(&\beta_0^+ \cdot D_i^+ + \beta_2 \cdot D_i^- + \sum_{year\in\{-3,2\}} \beta_{year}^+ \cdot D_i^+ \cdot \delta_{year(t)} \\ &+ \sum_{year\in\{-3,2\}} \beta_{year(t)}^- \cdot D_i^- \cdot \delta_{year(t)} + \Gamma X_i\Big)\end{aligned} \tag{9}$$

where $\delta_{year(t)}$ is an indicator function that the corresponding sub-spell indexed by t is in the respective year relative to the policy implementation. We then recover coefficients $\beta^+_{year}, \beta^-_{year}$ for each year before and after the policy in our analysis.

We show our estimates of β^+_t, β^-_t in Figure 12. For the positively exposed group, our estimates indicate a precise zero in the change in the probability of opening a business. At the same time, for those negatively exposed, we find that estimates vary more across years. Despite having a negative effect 2 years after the policy (in line with what a credit constraints story would suggest), we show that one year after the policy, estimates actually suggest a positive effect of the policy for negatively exposed individuals. Pooling those estimates gets us the zero effects described above.

Figure 12: Effects on the Probability of Creating a Firm by Year



This Figure shows how the policy affected firm survival. In Panel (a), we show the estimates of coefficients $\{\beta^+_t, \beta^-_t\}$ from equation 9. In panel (b), we show the survival rates of these new firms with and without the policy. The sample comprises all individuals we observe who were never a firm owner until three years before the policy implementation. A row in the data corresponds to sub-spells of a quarter for each individual. If the individual creates a firm, they are not observed in subsequent periods.

7.2 Effects on Firm's Outcomes

We next explore if the policy affected the outcomes of firms created. Since we find no effects on firm creation, we return to the difference in differences analysis using linear models. We restrict our sample only to eventual entrepreneurs. We then compare the outcomes of firms created by positively and negatively exposed individuals, before and after the policy, with those created by individuals in our control who had no changes in credit access.

Using our sample of entrepreneurs, we estimate the following equation:

$$Y_i = \delta_{t(i)} + \beta^+ \cdot D_i^+ \cdot Post_{t(i)} + \beta^- \cdot D_i^- \cdot Post_{t(i)} + \Gamma X_i + \varepsilon_i \qquad (10)$$

where $\delta_{t(i)}$ are fixed effects of the quarter in which individual i created their firm. D_i^+, D_i^- are indicator variables that take value one if individual i belongs to groups positively or negatively affected. $Post_{t(i)}$ is an indicator variable that takes value one if the firm was created after the policy. We include a set of control variables X_i, which includes gender, race, age, education, and fixed effects of the group of the joint distribution of credit individual i belong to.

Therefore, our coefficients of interest $\{\beta^+, \beta^-\}$ capture the difference in outcomes of firms created by positively (negatively) exposed individuals before and after the policy, under the assumption that in the absence of the policy, their difference would behave similarly to the difference for control individuals.

Firm Survival: We look at firm survival one and two years after firm creation. We define a firm surviving as having an active registry in *Receita Federal*, the Brazilian equivalent of the IRS. Using public data, we can observe firm survival until August 2024 for all formal firms.[36] To make it a consistent outcome, we restrict our sample to firms created until August 2022, around six quarters after the policy implementation.

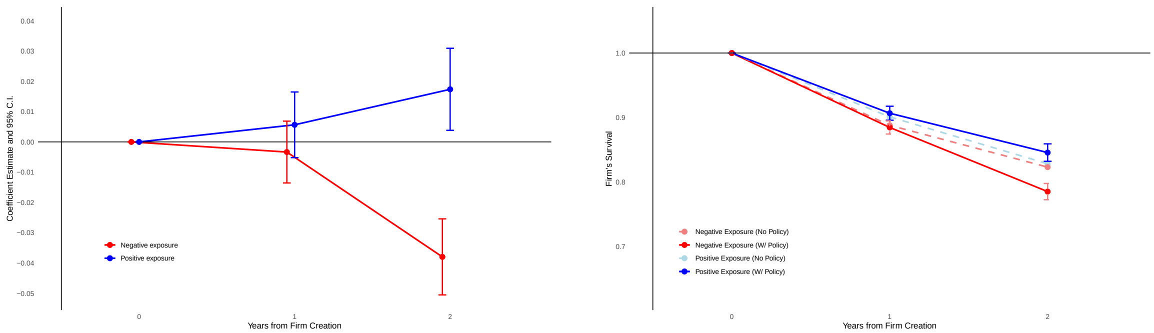
We find that more (less) credit access increases (decreases) the likelihood of firm survival two years after their creation. We show these results in Figure 13. In Panel (a) we plot our estimates and 95% confidence intervals of the coefficients β^+, β^- from equation 10. We estimate two different regressions using as outcome firm survival one and two years after firm creation. Our estimates indicate that firms created by positively exposed individuals increase by 1.8 p.p. their likelihood of survival two years after their creation because of the policy. In contrast, those created by negatively exposed individuals have a 3.8p.p. smaller likelihood of survival because of the policy.

This represents a 2.1% increase in the likelihood of firm survival and a 4.6% decrease for firms created by positively and negatively exposed individuals, respectively. We can see this in Panel (b) of Figure 13. We plot the average survival of firms created by positively and negatively exposed individuals with and without the policy. Survival without the policy is calculated as the average likelihood of firms created before the policy by the respective group. Survival with the policy is defined as that value plus the coefficient estimate from

[36] We use CNPJ public data to construct firm survival information. This data is a snapshot of all active and inactive registered firms in the country, identified by their registry number (often referred to as CNPJ number). A firm is defined as inactive if it has *situação cadastral* different from *Ativa* (active). This includes firms defined as *nula, suspensa, inapta or baixada*. In the data, we also see the date the firm became inactive, which allows us to construct our outcome variables.

Panel (a). The percent increase is calculated as the ratio between the firm survival with the policy by the firm survival without the policy.

Figure 13: Effects on Firm Survival



(a) Effects on Firm Survival **(b)** Survival Rates

This Figure presents our estimates of the effects of the policy on the survival of firms created by individuals exposed to changes in credit access. The sample comprises all individuals who created their first firm three years before and two years after the implementation of the policy. In Panel (a), we show the estimates and 95% confidence intervals of coefficients $\{\beta^+, \beta^-\}$ from equation 10. In Panel (b), we show firm survival rates with and without the policy for each group. Firm survival rates without the policy are calculated as the average survival of firms created by individuals in each group before the policy. Coefficients for one-year survival and two years survival are estimated in separate regressions. Controls included are cells of education, gender, race, and year of birth, and dummies for the group of the joint distribution of credit scores the individual belonged to.

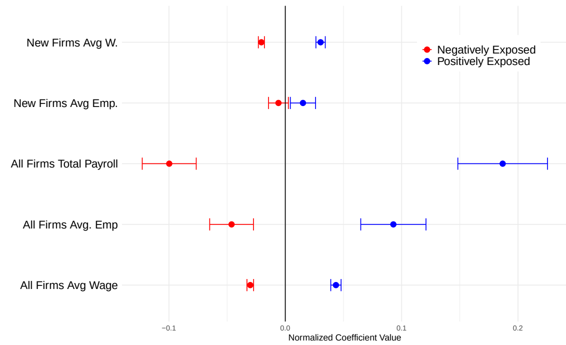
7.3 Consequences on Quality of Entrepreneurial Activity

Next, we discuss the consequences of the policy on the aggregate quality of entrepreneurial activity. We observe that changes in personal credit access cause firms' probability of survival to change. If firms that are surviving more because of the policy are of better *quality* than the ones that are not surviving because of it, then the policy increases the average quality of firms in the new cohorts of entrepreneurs.

In what follows, we compare *quality* measures of firms created by individuals across the groups of positive and negative exposure to the policy. A challenge we face is the availability of detailed firm-level data on their outcomes. Unfortunately, we do not observe revenues or other balance sheet outcomes in our firm records. We overcome this challenge in two ways. First, we use the characteristics of these firms' industries to characterize them. Second, we use firm-level data from the credit bureau. The latter strategy suffers from a data availability limitation as the credit bureau does not have detailed information on all firms in our sample.

Industry Level Analysis: Firms created by positively exposed individuals are in better

Figure 14: Industry Characteristics



This Figure shows characteristics of the industries in which firms created by individuals in different treatment groups operate. They show point estimates and 95% confidence intervals of coefficients β^{+}, β^{-} from equation 11 normalized by the average outcome value of the control group. New firm characteristics are constructed using matched employer-employee data three years after creation. "All firms" characteristics are constructed using the average value of outcomes for all firms in the given 5-digit CNAE industry classification. The sample comprises all individuals who created their first firm three years before and two years after the implementation of the policy.

industries. To show this, we calculate industry characteristics at the 5-digit CNAE level.[37] We then estimate linear models of the following type:

$$Y_{ind(i)} = \delta_{t(i)} + \beta^{+} \cdot D_i^{+} + \beta^{-} \cdot D_i^{-} + \Gamma X_i + \varepsilon_i \tag{11}$$

where $Y_{ind(i)}$ represent characteristics of industry *ind* in which i's firm is operating. In the vector X_i, we include controls for the observable characteristics of these individuals, namely fixed effects for cells of education, gender, race, and year of birth. We also add fixed effects of the period of firm creation $\delta_{t(i)}$.

Exposure to the policy is positively correlated with the employment characteristics of firms' industries. We show this in Figure 14, where we plot the coefficients β^{+}, β^{-} from equation 11 normalized by the average value of the control group.[38] Firms created by individuals in the positively exposed groups are in industries that, on average, employ more and pay higher wages. This is true not only for the average firms in these industries but also for new firms.

We also show that exposure positively correlates to the industries' productivity characteristics. We calculate value added per firm at the industry level[39] and firm fixed effects

[37]CNAE is the Brazilian equivalent of NAICS industry definition. In the 5-digit level, it consists of around 700 different industries. All formal firms must report their CNAE classification.

[38]Table A6 shows coefficient estimates not normalized.

[39]Value Added measures are calculated using the Brazilian Institute of Statistics (IBGE) surveys of the

Table 5: Industry Value Added and Avg. Firm F.E.

	(1)	(2)	(3)	(4)
	Value Added		Firm Wage F.E.	
	Avg.	I(Above Median)	Men	Women
Positive Exposure	2872.113	0.010*	0.009***	0.005***
	(5605.677)	(0.005)	(0.001)	(0.001)
Negative Exposure	-3053.021	-0.026***	-0.004***	-0.001**
	(4477.762)	(0.004)	(0.000)	(0.000)
Control Avg.	145811.131	0.464	-0.039	-0.054
N. of observations	83860	83860	90532	90532

from two-way fixed effects wage regressions.[40] We observe in Table 5 that exposure to the policy correlates positively with the productivity measures. Firms created by positively exposed individuals are 1 p.p. more likely to be in industries above the median in average value added. In contrast, those created by negatively exposed individuals are 2.5 p.p. less likely to be operating in such industries than the control group.

Our findings indicate that firms created by positively exposed groups are, on average, of higher quality than those created by individuals negatively exposed to the policy. Extrapolating the average difference to marginal firms whose likelihood of surviving changes due to the policy, our results indicate that the average quality of new cohorts of firms increases due to the policy. That is because firms who close due to the policy are of lower quality than those who survive because of it.

8 Conclusion

This paper studies the effects of revealing borrowers' information to lenders through credit scores. We explore a unique policy that took place in Brazil, which changed the information that financial institutions must share with credit bureaus from a financial delinquency registry to a complete registry of borrowers' credit history.

Our empirical analysis shows that the effects of revealing information about borrowers on credit can be rationalized by a simple conceptual framework that takes into account

services, manufacturing, and construction (PIA, PAS, PAC) in 2019. These are aggregate measures in around 100 industries categories. They are less granular than the 5-digit CNAE-Level and therefore, have less variation.

[40]These consists of estimating the Abowd et al. (1999) (AKM) model where for worker i in firm j we estimate $y_{it} = \alpha_i + \phi_{j(i)} + \Gamma X_{it} + \varepsilon_{it}$. We decompose firm fixed effects for men and women as in Card et al. (2016) and aggregate them at the industry level. Bender et al. (2018) shows that firm fixed effects from wage regressions with individual and firm fixed effects are positively correlated with firm-level productivity.

changes in both the value of the signal and its precision.

We show that revealing information generates an equity-efficiency trade-off. The policy increases overall inequality in credit access and enhances the racial gap in credit between white and nonwhite individuals. At the same time, we show that the policy increases efficiency in the market as credit gets reallocated from more to less risky loans.

We then show that changes in credit had effects beyond credit markets by investigating the effects of the policy on entrepreneurial activity. We identify that despite not increasing the likelihood of an individual creating a firm, an increase in credit access increases the revenue and investments of firms.

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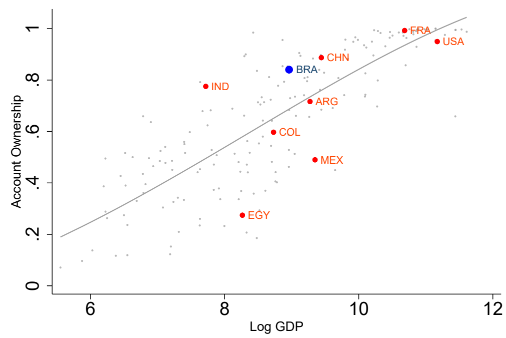
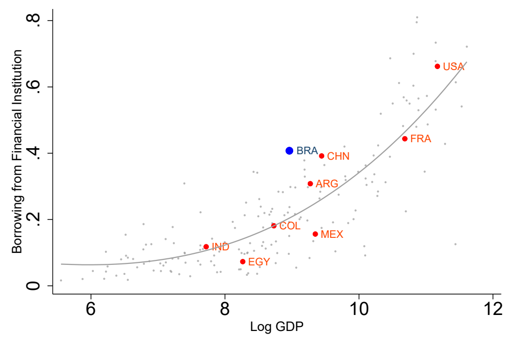
A Additional Figures and Tables

Figure A1: Credit Characteristics by Country and Income



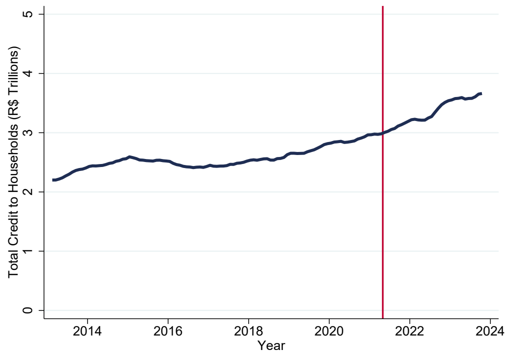
(a) Has Bank Account



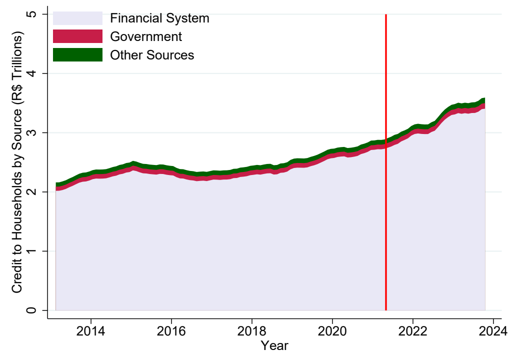
(b) Loan From Financial Institutions

This Figure shows the credit characteristics of countries relative to the Log of their Per capita GDP. Figures were constructed using 2021 data from the World Bank Global Financial Development Database.

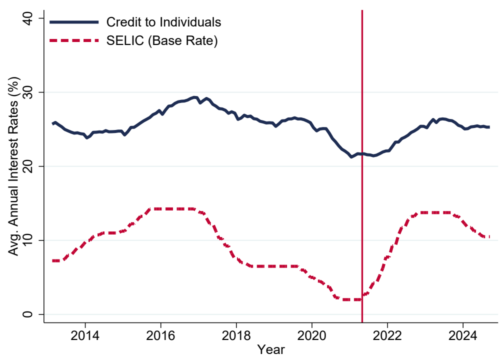
Figure A2: Aggregate Credit Statistics Before and After Cadastro Positivo



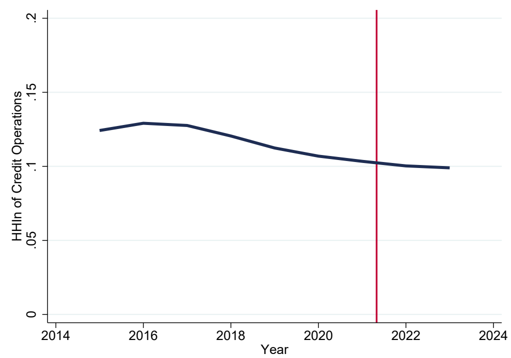
(a) Total Household Credit



(b) Household Credit by Source



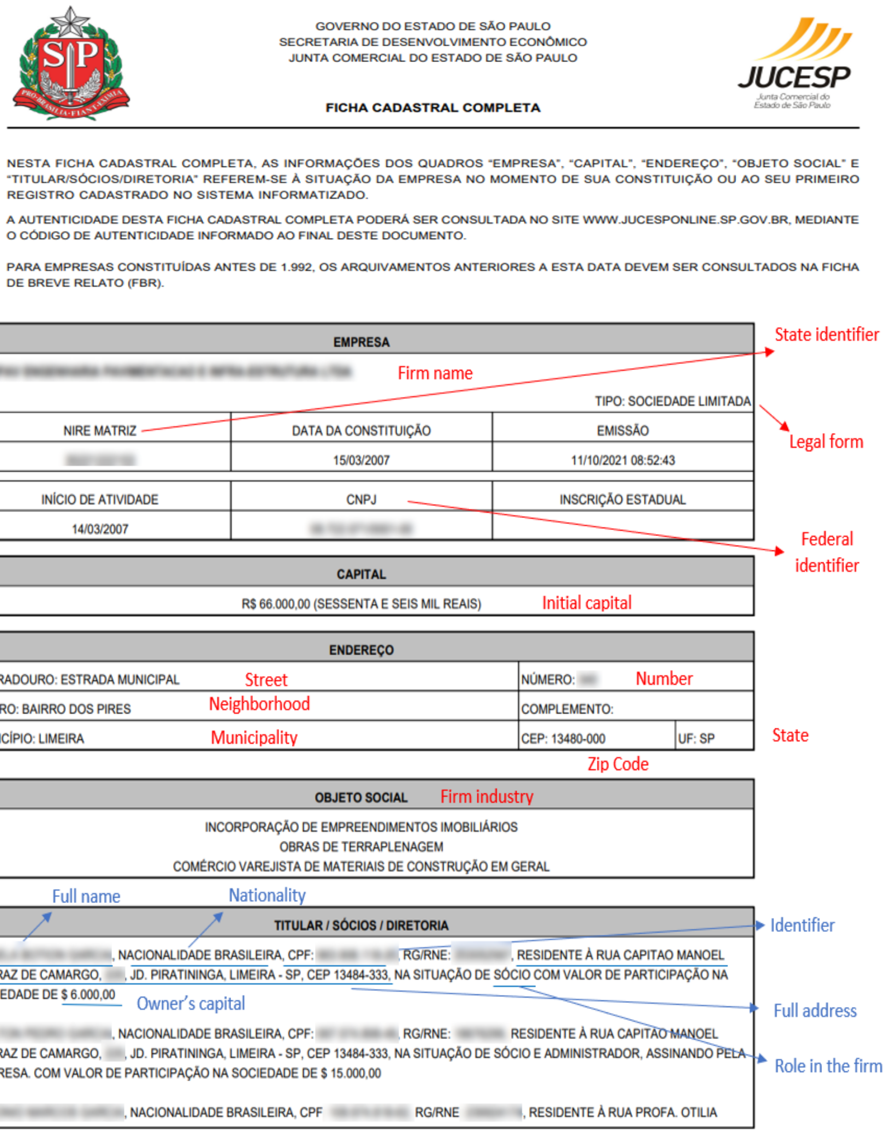
(c) Average Annual Interest Rates



(d) Concentration (HHIn)

This Figure shows aggregate credit statistics in the periods before and after *Cadastro Positivo* implementation, indicated by the red vertical lines. In Panel (a), we show Total Household Credit in the country. Panel (b) shows the same measure decomposed by the source of that given credit. Other Sources included credit cooperatives and institutions non directly tied to the financial system. Panel (c) plots the average interest rate of credit given to households. Panel (d) plots normalized Herfindahl-Hirschman Index. The index is obtained by summing the square of the market share (in decimal form) of each financial institution in the considered market: $\text{HHIn} = (\text{IF1})^2 + (\text{IF2})^2 + ... + (\text{IFj})^2$, resulting in a number between 0 and 1. Normalization choice was made by the Brazilian Central Bank. Panels (a), (b), and (c) use data available in the Brazilian Central Bank's Time Series Management System. Panel (d) uses data collected from the Brazilian Central Bank's report of banking activity.

Figure A3: Example of Firm Record from Sao Paulo's Trade Board



GOVERNO DO ESTADO DE SÃO PAULO
SECRETARIA DE DESENVOLVIMENTO ECONÔMICO
JUNTA COMERCIAL DO ESTADO DE SÃO PAULO

FICHA CADASTRAL COMPLETA

JUCESP Junta Comercial do Estado de São Paulo

NESTA FICHA CADASTRAL COMPLETA, AS INFORMAÇÕES DOS QUADROS "EMPRESA", "CAPITAL", "ENDEREÇO", "OBJETO SOCIAL" E "TITULAR/SÓCIOS/DIRETORIA" REFEREM-SE À SITUAÇÃO DA EMPRESA NO MOMENTO DE SUA CONSTITUIÇÃO OU AO SEU PRIMEIRO REGISTRO CADASTRADO NO SISTEMA INFORMATIZADO.

A AUTENTICIDADE DESTA FICHA CADASTRAL COMPLETA PODERÁ SER CONSULTADA NO SITE WWW.JUCESPONLINE.SP.GOV.BR, MEDIANTE O CÓDIGO DE AUTENTICIDADE INFORMADO AO FINAL DESTE DOCUMENTO.

PARA EMPRESAS CONSTITUÍDAS ANTES DE 1.992, OS ARQUIVAMENTOS ANTERIORES A ESTA DATA DEVEM SER CONSULTADOS NA FICHA DE BREVE RELATO (FBR).

EMPRESA		
		TIPO: SOCIEDADE LIMITADA
NIRE MATRIZ	DATA DA CONSTITUIÇÃO	EMISSÃO
	15/03/2007	11/10/2021 08:52:43
INÍCIO DE ATIVIDADE	CNPJ	INSCRIÇÃO ESTADUAL
14/03/2007		

CAPITAL
R$ 66.000,00 (SESSENTA E SEIS MIL REAIS)

ENDEREÇO		
LOGRADOURO: ESTRADA MUNICIPAL	NÚMERO:	
BAIRRO: BAIRRO DOS PIRES	COMPLEMENTO:	
MUNICÍPIO: LIMEIRA	CEP: 13480-000	UF: SP

OBJETO SOCIAL
INCORPORAÇÃO DE EMPREENDIMENTOS IMOBILIÁRIOS OBRAS DE TERRAPLENAGEM COMÉRCIO VAREJISTA DE MATERIAIS DE CONSTRUÇÃO EM GERAL

TITULAR / SÓCIOS / DIRETORIA

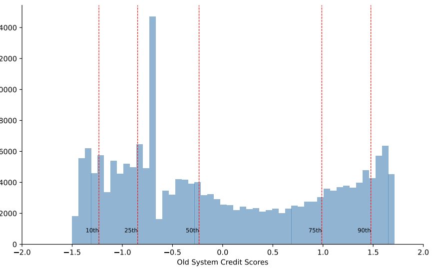
, NACIONALIDADE BRASILEIRA, CPF: , RG/RNE: , RESIDENTE À RUA CAPITAO MANOEL FERRAZ DE CAMARGO, , JD. PIRATININGA, LIMEIRA - SP, CEP 13484-333, NA SITUAÇÃO DE SÓCIO COM VALOR DE PARTICIPAÇÃO NA SOCIEDADE DE $ 6.000,00

, NACIONALIDADE BRASILEIRA, CPF: , RG/RNE: RESIDENTE À RUA CAPITAO MANOEL FERRAZ DE CAMARGO, , JD. PIRATININGA, LIMEIRA - SP, CEP 13484-333, NA SITUAÇÃO DE SÓCIO E ADMINISTRADOR, ASSINANDO PELA EMPRESA. COM VALOR DE PARTICIPAÇÃO NA SOCIEDADE DE $ 15.000,00

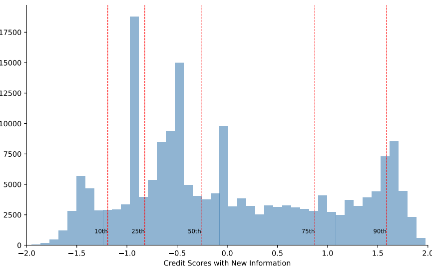
, NACIONALIDADE BRASILEIRA, CPF RG/RNE , RESIDENTE À RUA PROFA. OTILIA

This Figure shows an example of a firm record from São Paulo's trade board. Despite being publicly available records, we blurred identifiable information of individuals and firms to protect their confidentiality. All records are available in *Serviços Online* under the option *Pesquisar Empresas*. It is necessary to have a Brazilian social security number (CPF) to log in the website. Additional information such as ownership and capital change are also available in these records despite not being apparent in this example.

Figure A4: Histogram of Credit Scores under both Systems



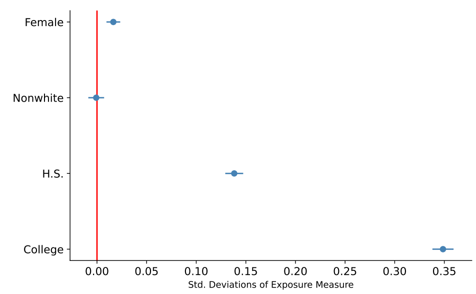
(a) Negative Registry Information Only



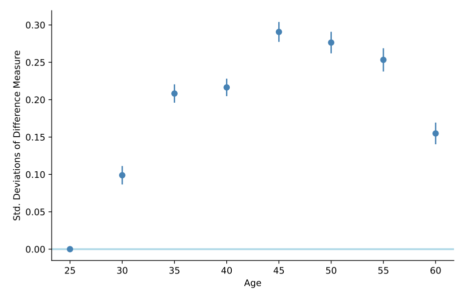
(b) Positive and Negative Registry Information

This Figure shows the histogram of credit scores calculated with information from both positive and negative registries, and credit scores calculated with information from the negative registry. The sample is restricted to the last period before the implementation of the policy.

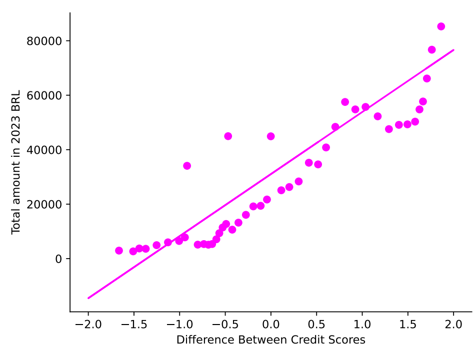
Figure A5: Correlation of the Difference between Credit Scores with Observable Characteristics



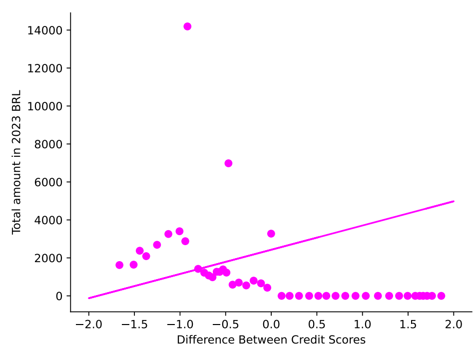
(a) Gender Race and Educ.



(b) Age Groups



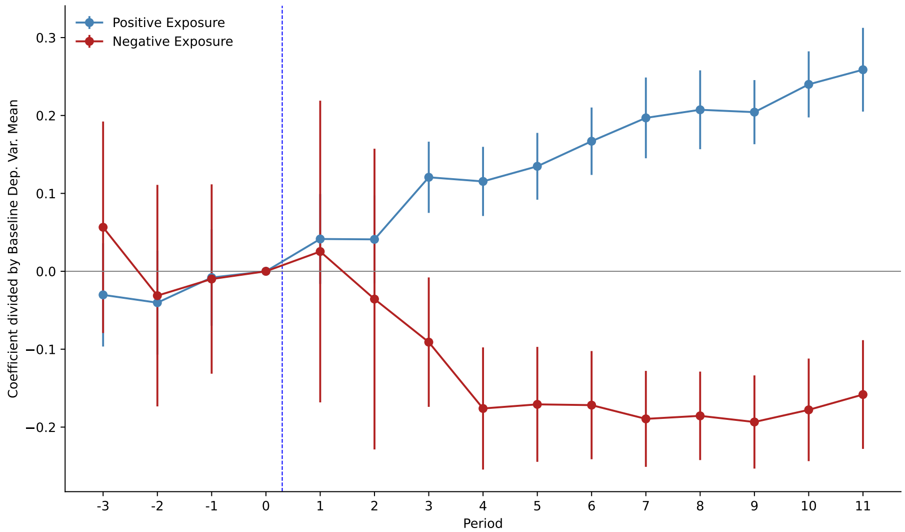
(c) Total Credit



(d) Payments not made

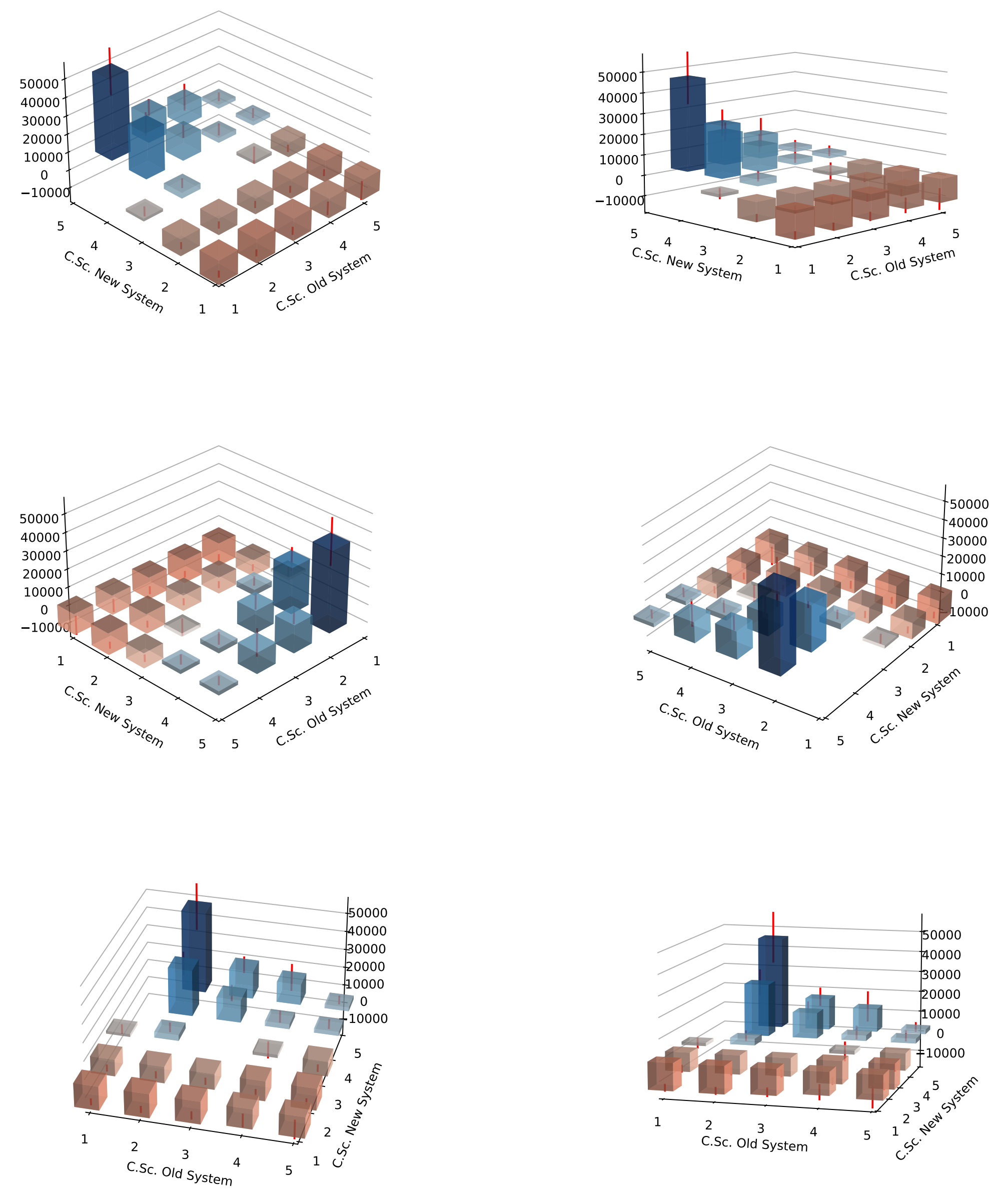
Panels (a) and (b) plot the coefficients of a regression of Δ_i on observable characteristics. The sample is restricted to the last period before the implementation of the policy. Coefficients in panels (a) and (b) are estimated in the same regression that includes dummies for gender, race, education groups, and age groups. We omit from the regression white men with less than high school education in the youngest age group. In panels (c) and (d), we show binscatters of Credit and Default with respect to Δ_i. The sample is restricted to the last period before the implementation of the policy.

Figure A6: Changes in Credit Divided by Predicted Credit w/o Policy



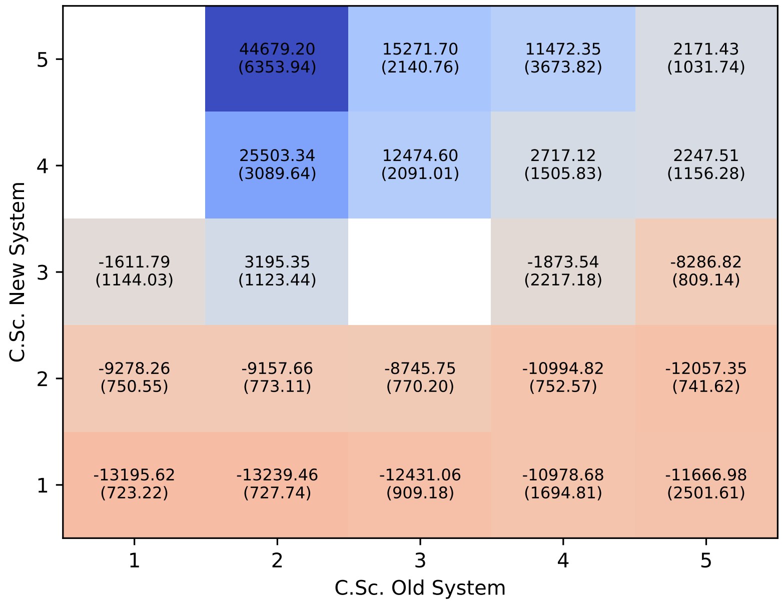
This figure plots the $\{\beta_t^+, \beta_t^-\}$ estimates from equation 1 normalized. We divide the coefficient estimates by the baseline level of the respective group in period=0 summed with the time fixed effects (for positive exposure $\frac{\hat{\beta_t^+}}{E[Y_{it}|t=0,D_i^+]+\hat{\delta_t}}$, and for negative exposure $\frac{\hat{\beta_t^-}}{E[Y_{it}|t=0,D_i^-]+\hat{\delta_t}}$). Positive exposure is defined as individuals with $\Delta_i \in [0.75, 1.25]$ and negative exposure as $\Delta_i \in [-1.25, -0.75]$.

Figure A7: Effects on Credit over the Joint Distribution of Credit Scores - Rotated Bars



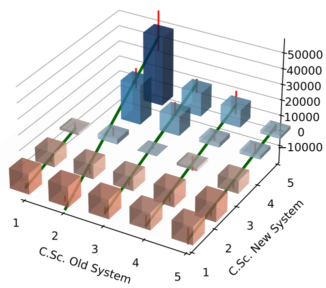
This Figure shows the estimates of coefficients $\{\beta^{kj}\}$ from equation 3. It shows the same results as in Figure 7 but over different angles. Each bar corresponds to a given coefficient, with 95% confidence intervals plotted in the red lines. Standard Errors are clustered at the individual level. Bars are organized such that the x-axis (labeled C. Sc. old system) indexes coefficients for a given group k, and the y-axis (labeled C.Sc. new system) indexes coefficients for a given group j. Positive estimates of β^{kj} are shown in blue, whereas negative estimates are shown in red. $\beta^{14}, \beta 15$ are not defined because there is no individual in the sample in those groups of the joint distribution of credit scores.

Figure A8: Effects of the Policy on Credit over the Joint Distribution of Credit Scores

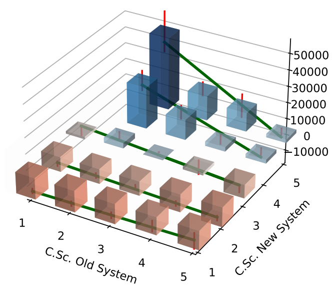


Notes: This Figure shows estimates of coefficients $\{\beta^{kj}\}$ from equation 3. Standard errors in parenthesis are clustered at the individual level. Coefficients are organized such that the x-axis (labeled C. Sc. old system) indexes coefficients for a given group k, and the y-axis (labeled C.Sc. new system) indexes coefficients for a given group j.

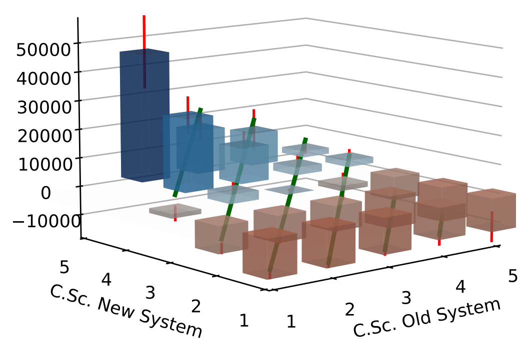
Figure A9: Comparisons Between Changes in Credit over the Joint Distribution of Credit Scores



(a) *Vertical Comparisons*



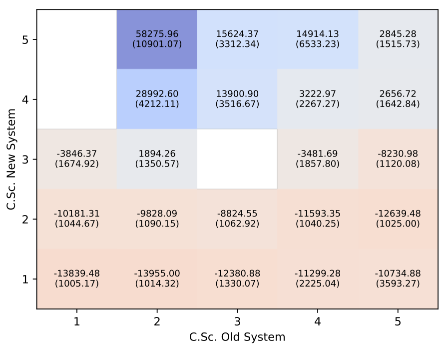
(b) *Horizontal Comparisons*



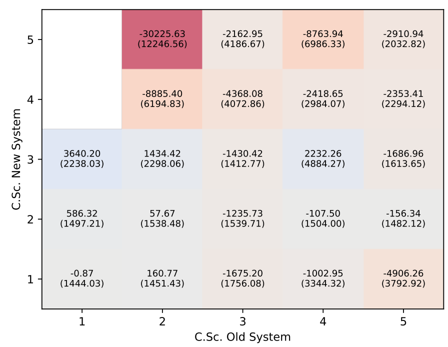
(c) *Diagonal Comparisons*

This Figure shows the estimates of coefficients $\{\beta^{kj}\}$ from equation 3 with the same specifications as Figure 7. The difference is the green lines that correspond to the linear fit between coefficients. A linear fit is computed by regressing values of estimates along each coordinate of the corresponding comparison without weighting. Panel (c) shows the same estimates but rotated to a different angle to better visualize the diagonal comparisons. Table A2 shows the estimates of the linear fits across coordinates.

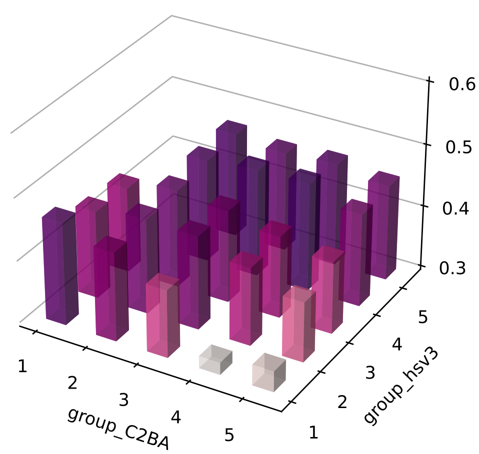
Figure A10: Gender by Group of the Joint Distribution of Credit Scores



(a) Male β_{g0}^{kj}



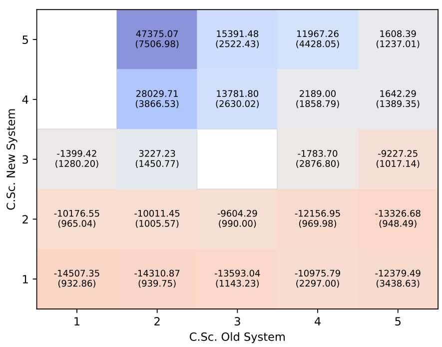
(b) β_{g1}^{kj} (Female - Male)



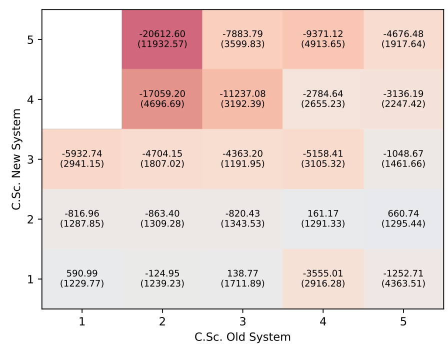
(c) Share of Women across the Joint Distribution of Credit Scores

In Panels (a) and (b), we plot our coefficient estimates from Equation 5. $G_i = 1$ indicates that indiviual i is a woman. These are the values used to calculate our expression in equation 6. Standard errors are shown in parenthesis. In Panel (c) we show the share of women across the joint distribution of credit scores.

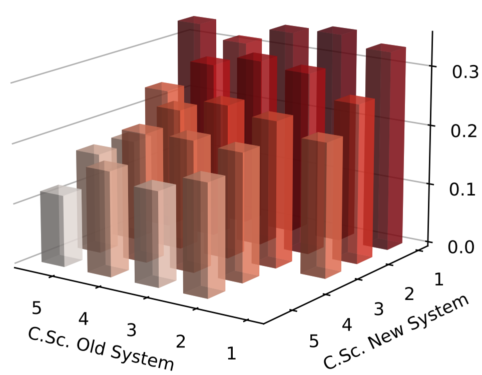
Figure A11: Changes in Credit by Race by Group of the Joint Distribution of Credit Scores



(a) White β_{g0}^{kj}



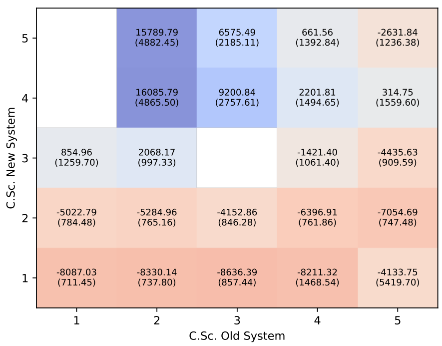
(b) β_{g1}^{kj} (Nonwhite - White)



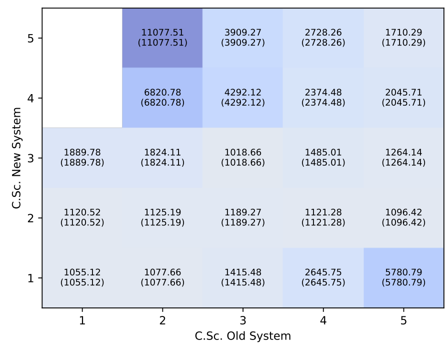
(c) Share of Nonwhite by Group

In Panels (a) and (b), we plot our coefficient estimates from Equation 5. $G_i = 1$ indicates that individual i is a nonwhite individual. These are the values used to calculate our expression in equation 6. Standard errors are shown in parentheses. In Panel (c), we show the share of nonwhite individuals across the joint distribution of credit scores.

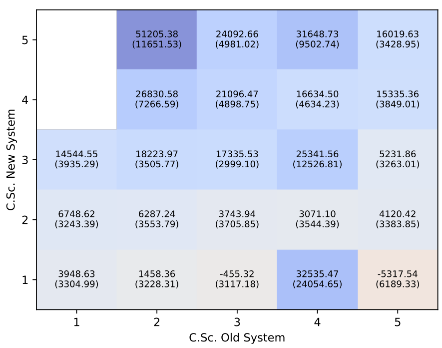
Figure A12: Education by Group of the Joint Distribution of Credit Scores



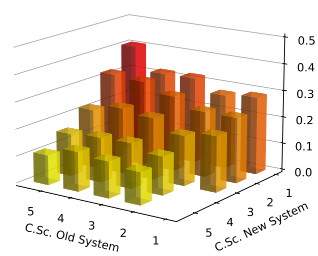
(a) Less than H.S. β_{g0}^{kj}



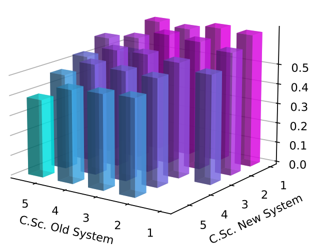
(b) β_{g1}^{kj} (H.S. - < H.S.)



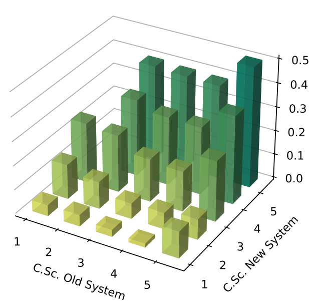
(c) β_{g2}^{kj} (Coll. - < H.S.)



(d) Share with Less than H.S.



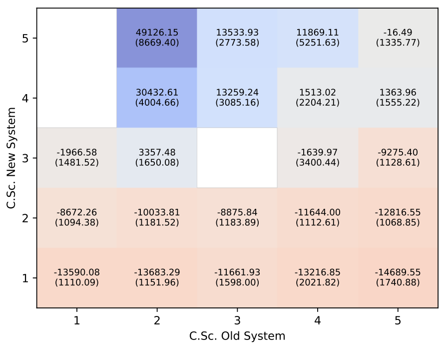
(e) Share with High School



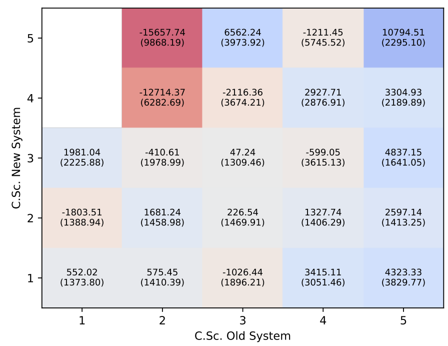
(f) Share with Some College

Figure A13: In Panels (a) and (b), we plot our coefficient estimates from Equation 5. We add a second heterogeneity group, leaving at base individuals with less than high school. These are the values used to calculate our expression in equation 6. Standard errors are shown in parentheses. In Panel (c), we show the share of individuals with each educational attainment across the joint distribution of credit scores.

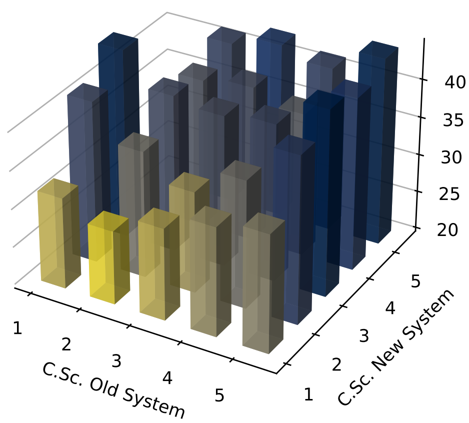
Figure A14: Changes in Credit by Age Group by Group of the Joint Distribution of Credit Scores



(a) 40 y.o. or more β_{g0}^{kj}



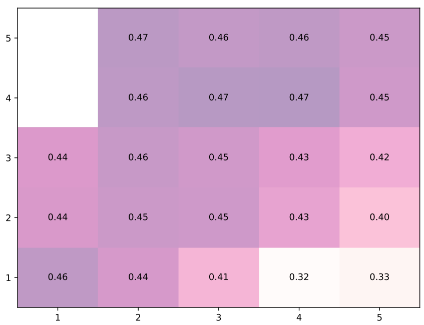
(b) β_{g1}^{kj} (Below 40 - 40 y.o. or more)



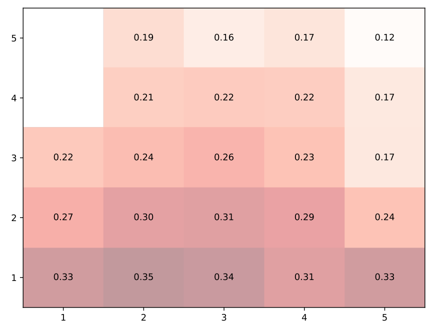
(c) Avg. Age by Group

In Panels (a) and (b), we plot our coefficient estimates from Equation 5. $G_i = 1$ indicates that individual i is less than 40 years old. These are the values used to calculate our expression in equation 6. Standard errors are shown in parenthesis. In Panel (c) we show the average age of individuals across the joint distribution of credit scores.

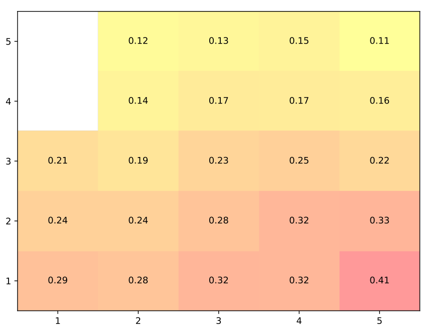
Figure A15: Education by Group of the Joint Distribution of Credit Scores



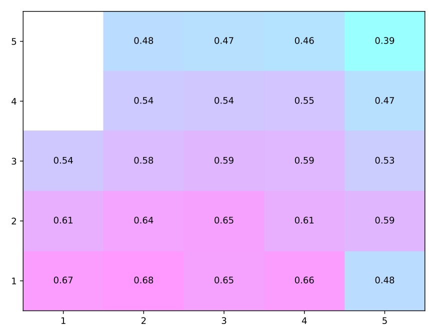
(a) Share Women



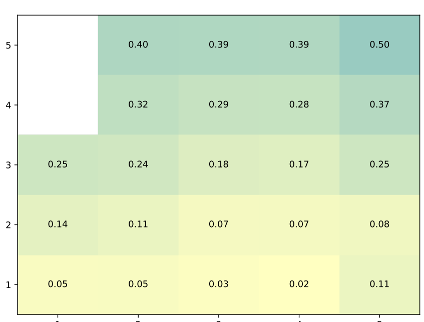
(b) Share Nonwhite



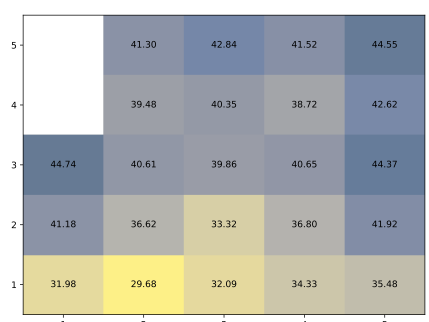
(c) Share with Less than H.S.



(d) Share with High School



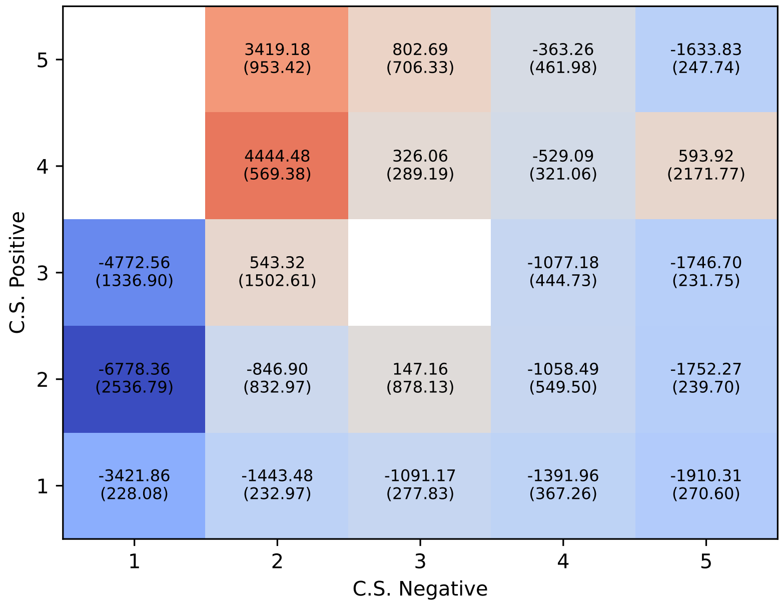
(e) Share with Some College



(f) Avg. Age

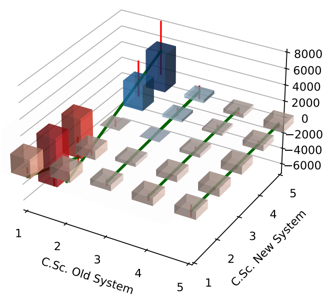
This Figure shows heatmap plots of how demographic characteristics vary across the joint distribution of credit scores. These are the same results from Panel (c) in Figures A10,A11,A14 and Panels (d), (e), (f) of Figure A12.

Figure A16: Effects of the Policy on Financial Delinquency over the Joint Distribution of Credit Scores

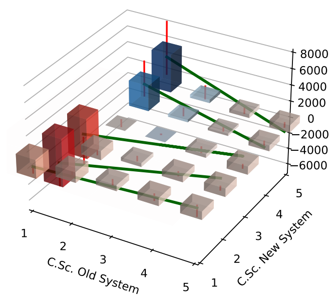


Notes: This Figure shows estimates of coefficients $\{\beta^{kj}\}$ from equation 3 with total financial delinquency from individual i at period t as the outcome. Standard errors in parenthesis are clustered at the individual level. Coefficients are organized such that the x-axis (labeled C. Sc. old system) indexes coefficients for a given group k, and the y-axis (labeled C.Sc. new system) indexes coefficients for a given group j.

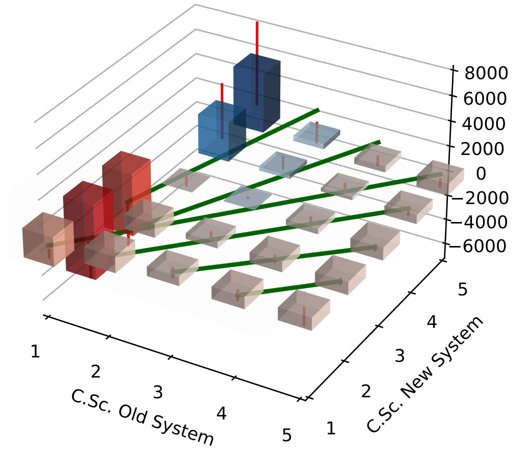
Figure A17: Comparisons Between Changes in Financial Delinquency over the Joint Distribution of Credit Scores



(a) *Vertical Comparisons*



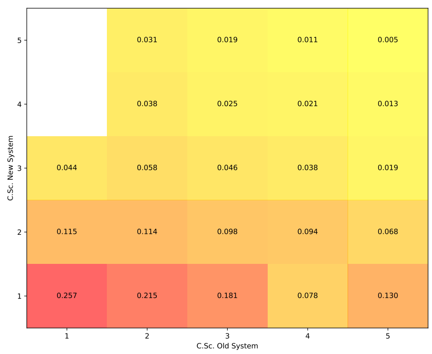
(b) *Horizontal Comparisons*



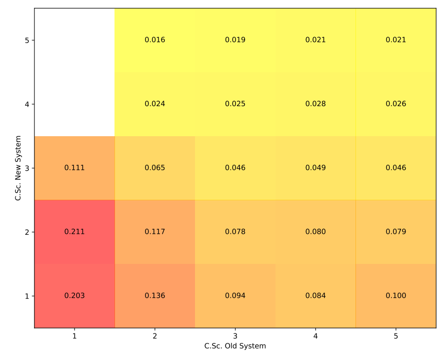
(c) *Diagonal Comparisons*

This Figure shows the estimates of coefficients $\{\beta^{kj}\}$ from equation 3 with the same specifications as Panel (c) in Figure 10. The difference is the green lines that correspond to the linear fit between coefficients. A linear fit is computed by regressing values of estimates along each coordinate of the corresponding comparison without weighting. Panel (c) shows the same estimates but rotated to a different angle to better visualize the diagonal comparisons. Table A2 shows the estimates of the linear fits across coordinates.

Figure A18: Estimates of Default Rate



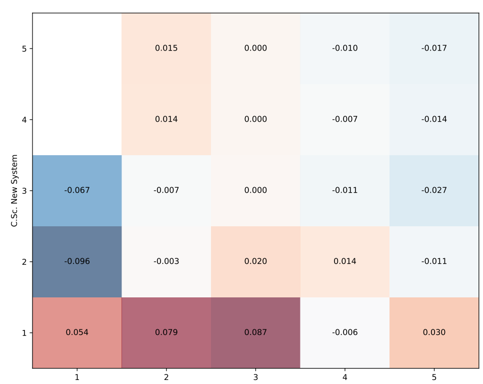
(a) Default Rates - with Policy



(b) Default Rates - no Policy

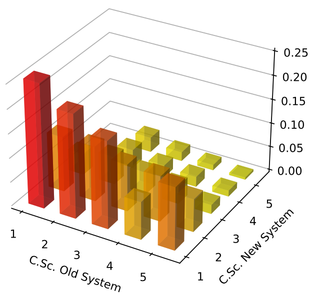
This Figure shows average Default Rates in the periods after the policy. These are the same results from Panels (a) and (b) of Figure 11 in a 2Dimensional perspective. Panel (a) plots our estimates of default rates in the presence of the policy, whereas Panel (b) plots our estimates of Default rates in the absence of the Policy.

Figure A19: Changes in Overall Default Rate

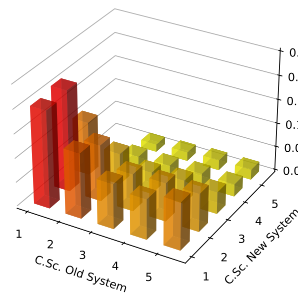


This Figure shows changes in overall default rate for each group. This consists of the difference between average default rate for each group with policy DR^1_{it} in the periods after the policy and the counterfactual average default rate without the policy in the periods after the policy implementation.

Figure A20: Estimates of Default Rate



(a) Default Rates - with Policy



(b) Default Rates - no Policy

This Figure shows average Default Rates in the periods after the policy. Panel (a) and (b) are the same results from Figure 11 from a different angle. Panel (a) plots our estimates of default rates in the presence of the policy, whereas Panel (b) plots our estimates of Default rates in the absence of the Policy.

Table A1: Correlation between Credit Scores and Demographic Characteristics

	Negative System	Positive System	Difference in C.Scs.
	(1)	(2)	(3)
Female	-0.059	-0.043	0.016
	(0.004)	(0.004)	(0.004)
Nonwhite	-0.170	-0.171	-0.001
	(0.005)	(0.005)	(0.004)
High School	0.087	0.226	0.138
	(0.005)	(0.005)	(0.005)
College	0.561	0.909	0.349
	(0.007)	(0.006)	(0.005)
30	0.186	0.285	0.099
	(0.007)	(0.008)	(0.006)
35	0.269	0.477	0.208
	(0.007)	(0.008)	(0.006)
40	0.418	0.634	0.216
	(0.007)	(0.007)	(0.006)
45	0.356	0.646	0.291
	(0.008)	(0.008)	(0.007)
50	0.453	0.729	0.276
	(0.009)	(0.009)	(0.007)
55	0.574	0.827	0.253
	(0.010)	(0.009)	(0.008)
60	0.780	0.935	0.155
	(0.009)	(0.008)	(0.007)
Intercept	-0.475	-0.824	-0.349
	(0.007)	(0.007)	(0.006)
Observations	195179	195179	195179
R^2	0.103	0.196	0.036

This Table presents the correlation between Credit Scores and demographic characteristics of individuals. Coefficients are estimates of a linear regression of $\frac{cs_i - \bar{cs}}{sd(cs_i)}$ on observable characteristics. The sample is restricted to the last period before the implementation of the policy. Column (1) shows results using credit scores from the negative system, and Column (2) from the positive system. The outcome in Column (3) is Positive System credit scores minus Negative System credit scores. Robust Standard errors in parenthesis.

Table A2: Linear Fit between Estimates of β^{kj} from Equation 3 with Credit as Outcome

Vertical		Horizontal		Diagonal	
	(1)		(2)		(3)
Fit Between Coordinates		Coordinates		Coordinates	
(1,1) - (1,3)	5791.9119	(1,1) - (5,1)	531.8042	(1,1) - (5,5)	4260.8871
	(1082.2779)		(184.4172)		(957.9481)
(2,1) - (2,5)	15049.8305	(1,2) - (5,2)	-739.5318	(4,1) - (5,2)	-1078.6651
	(2091.4539)		(291.3136)		(.)
(3,1) - (3,5)	7662.5857	(1,3) - (5,3)	-1841.8956	(3,1) - (5,3)	2072.1172
	(895.8384)		(1100.7821)		(367.1220)
(4,1) - (4,5)	5861.3999	(2,4) - (5,4)	-7952.4951	(2,1) - (5,4)	5333.3116
	(850.7122)		(2030.8364)		(367.4760)
(5,1) - (5,5)	4198.1683	(5,4) - (5,5)	-13132.2642	(1,3) - (3,5)	8441.7446
	(1023.4545)		(3865.9973)		(10781.0828)
				(1,2) - (2,5)	7153.1092
					(2188.8593)

This Table shows coefficients and standard errors in parenthesis of linear fit between coefficients β^{kj} from equation 3. Superscript k corresponds to groups of old system of credit scores, and superscript j corresponds to groups of the new system of credit scores. The coordinates $(x_1, y_1) - (x_2, y_2)$ in parenthesis represent the corresponding start and end points of each linear fit. For example, (1,1) - (1,3) corresponds to the linear fit between our estimates of coefficients $\{\beta^{11}, \beta^{12}, \beta^{13}\}$. In turn, (1,1) - (5,1) corresponds to the line between $\{\beta^{11}, \beta^{21}, \beta^{31}, \beta^{41}, \beta^{51}\}$

Table A3: Linear Fit between Estimates of β^{kj} from Equation 3 with Financial Delinquency as outcome

Vertical		**Horizontal**		**Diagonal**	
	(1)		(2)		(3)
Fit Between Coordinates		Coordinates		Coordinates	
(1,1) - (1,3)	-1373,5292	(1,1) - (5,1)	299.6956	(1,1) - (5,5)	367.4869
	(1372.4108)		(150.9276)		(254.7191)
(2,1) - (2,5)	1633.5986	(1,2) - (5,2)	1045.7595	(4,1) - (5,2)	-61.7526
	(302.1776)		(611.7093)		(.)
(3,1) - (3,5)	311.7982	(1,3) - (5,3)	776..0316	(3,1) - (5,3)	-129.2677
	(36.3450)		(642.3734)		(40.3109)
(4,1) - (4,5)	142.0436	(2,4) - (5,4)	-1224.3008	(2,1) - (5,4)	174.0391
	(57.9121)		(403.3562)		(177.7955)
(5,1) - (5,5)	46.5992	(5,4) - (5,5)	-1788.8731	(1,3) - (3,5)	2748.9782
	(37.5222)		(650.1486)		(3170.6027)
				(1,2) - (2,5)	1712.5215
					(1133.2098)

This Table shows coefficients and standard errors in parenthesis of linear fit between coefficients β^{kj} from equation 3 with financial delinquency as the outcome. Superscript k corresponds to groups of old system of credit scores, and superscript j corresponds to groups of the new system of credit scores. The coordinates $(x_1, y_1) - (x_2, y_2)$ in parenthesis represent the corresponding start and end points of each linear fit. For example, (1,1) - (1,3) corresponds to the linear fit between our estimates of coefficients $\{\beta^{11}, \beta^{12}, \beta^{13}\}$. In turn, (1,1) - (5,1) corresponds to the line between $\{\beta^{11}, \beta^{21}, \beta^{31}, \beta^{41}, \beta^{51}\}$

Table A4: Comparisons with Additional Sample

	(1) Credit Sample	(2) Additional Sample	(3) Difference
Female	0.446701	0.458609	0.011908
Nonwhite	0.235102	0.236513	0.001411
Less than H.S.	0.214555	0.201845	-0.012709
High School	0.561083	0.566405	0.005321
Some College	0.224362	0.231750	0.007388
Avg. Age	40.193400	39.146562	-1.046838
Old Sys. C. Sc.	467.149813	473.890127	6.740313
New Sys. C. Sc.	552.889273	548.229299	-4.659974
N. Observations	194235	560172	

This Table shows summary statistics of the sample used for credit analysis and the additional sample included for the entrepreneurship analysis. Credit Score values are from the last period before the implementation of the policy.

Table A5: Comparisons Entrepreneurship Analysis Sample (Risk Set)

	(1) Excluded	(2) Analysis Sample	(3) Difference
Female	0.455	0.455	0.0005
Nonwhite	0.165	0.248	0.0831
Less than H.S.	0.142	0.215	0.0731
High School	0.558	0.566	0.0080
Some College	0.299	0.218	-0.0812
Age	40.86	39.17	-1.696
Old Sys. C. Sc.	459.61	474.28	14.6
New Sys. C. Sc.	565.50	546.70	-18.79
N. Observations	109367	645040	

This Table shows summary statistics of the sample used in entrepreneurship analysis and the excluded sample. Excluded individuals are those who had already created a firm at any moment before three years of the implementation of the policy. Analysis sample includes the remaining individuals. Credit Score values are from the last period before the implementation of the policy.

Table A6: Firm Quality by Industry

	(1)	(2)	(3)	(4)	(5)
	New Firms		All Firms		
	Num. Emp.	Avg. Wages	Num. Emp.	Total Payroll	Avg. Wages
Positively Exposed	0.055**	27.952***	0.836***	3817.944***	46.903***
	(0.020)	(1.891)	(0.129)	(401.742)	(2.441)
Negatively Exposed	-0.021	-18.928***	-0.416***	-2038.311***	-32.438***
	(0.016)	(1.227)	(0.086)	(241.912)	(1.571)
Omitted group average	3.611	925.095	9.044	20635.467	1083.043
N. Observations	90531	90531	90532	90532	90532

This table shows the characteristics of the industries in which firms created by individuals in different treatment groups operate. It presents point estimates and standard deviations of coefficients β^{+}, β^{-} from equation 11. New firm characteristics are constructed using matched employer-employee data three years after creation. "All firms" characteristics are constructed using the average value of outcomes for all firms in the given 5-digit CNAE industry classification. The sample comprises all individuals who created their first firm three years before and two years after the implementation of the policy.

B Proof of Propositions

Proposition: *For any given pair of old and new signals, s_i, s_i', and a given constant c, rationalizable changes in credit should follow*:

i. $h(s_i, s_i' + c) - h(s_i, s_i') > 0$

ii. $h(s_i + c, s_i') - h(s_i, s_i') < 0$

(i)

$$
\begin{aligned}
h(s_i, s_i' + c) - h(s_i, s_i') &= \Bigg(g(\mathbb{E}[\theta|s_i' + c]) - g(\mathbb{E}[\theta|s_i])\Bigg) - \Bigg(g(\mathbb{E}[\theta|s_i']) - g(\mathbb{E}[\theta|s_i])\Bigg) \\
&= g(\mathbb{E}[\theta|s_i' + c]) - g(\mathbb{E}[\theta|s_i']) \\
&> 0
\end{aligned}
$$

since $\mathbb{E}[\theta|s_i' + c] > \mathbb{E}[\theta|s_i']$ and $g(.)$ is increasing

(ii)

$$
\begin{aligned}
h(s_i + c, s_i') - h(s_i, s_i') &= \Bigg(g(E[\theta|s_i']) - g(\mathbb{E}[\theta|s_i + c])\Bigg) - \Bigg(g(\mathbb{E}[\theta|s_i']) - g(\mathbb{E}[\theta|s_i])\Bigg) \\
&= g(\mathbb{E}[\theta|s_i]) - g(\mathbb{E}[\theta|s_i + c]) \\
&< 0
\end{aligned}
$$

since $\mathbb{E}[\theta|s_i] < \mathbb{E}[\theta|s_i + c]$ and $g(.)$ is increasing.

Proposition 2: *If $h(s_i, s_i')$ is increasing in $\mathbb{E}[\theta|s_i'] - \mathbb{E}[\theta|s_i]$, for any given pair of old and new signals, s_i, s_i', and a given positive constant c, rationalizable changes in credit should follow:*

iii. $h(s_i + c, s_i' + c) - h(s_i, s_i') > 0$

Given the increasing differences assumption, prooving statement iii is equivalent to prooving that

$$\mathbb{E}[\theta|s_i' + c] - \mathbb{E}[\theta|s_i + c] > \mathbb{E}[\theta|s_i'] - \mathbb{E}[\theta|s_i]$$

We can rewrite this as:

$$\mathbb{E}[\theta|s_i' + c] - \mathbb{E}[\theta|s_i'] > \mathbb{E}[\theta|s_i + c] - \mathbb{E}[\theta|s_i]$$

We can simplify this expression given Assumption 1, which states that $E[\theta|s] = \mathbb{E}[\theta] + \frac{1}{a(s)}(s_i - E[\theta])$. Here, we slightly change the notation, explaining stating that the term $a(.)$ varies with the signal.

We can rewrite the left and right hand sides of the expression as:

$$\mathbb{E}[\theta] + \frac{1}{a(s')}(s_i' + c - \mathbb{E}[\theta]) - \mathbb{E}[\theta] + \frac{1}{a(s')}(s_i' - \mathbb{E}[\theta])\frac{1}{a(s')}c$$

$$\mathbb{E}[\theta] + \frac{1}{a(s)}(s_i + c - \mathbb{E}[\theta]) - \mathbb{E}[\theta] + \frac{1}{a(s)}(s_i - \mathbb{E}[\theta])\frac{1}{a(s)}c$$

From Assumption 2, we know that Variance of s is bigger than the variance of s', thus from the second part of Assumption 1, we have that $\frac{1}{a(s')} > \frac{1}{a(s)}$.

Therefore:

$$\frac{1}{a(s')}c > \frac{1}{a(s)}c$$

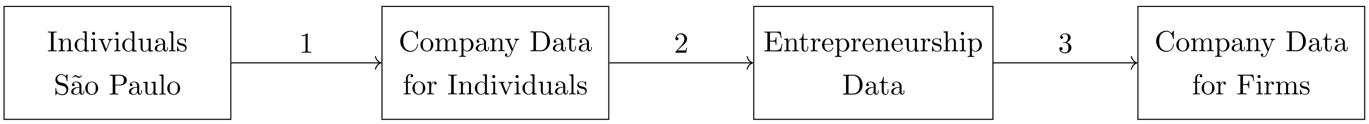
which implies

$$\mathbb{E}[\theta|s_i' + c] - \mathbb{E}[\theta|s_i'] > \mathbb{E}[\theta|s_i + c] - \mathbb{E}[\theta|s_i]$$

C Sample Construction Details

In this appendix, we describe our sample construction procedure. We use Figure A21 to guide us in the process.

Figure A21: Data Construction Diagram



We begin with a random sample of 200,000 individuals who live in Sao Paulo. This initial pool of individuals is selected from the universe of formal employees in Sao Paulo between 2015 and 2021, summed with the universe of entrepreneurs (including microentrepreneurs registered as MEI) in the state between 2015 and January 2024. I.e. if an individual had at least one formal job contract in the state of Sao Paulo in the period described above, they are in the pool of potential individuals in the sample. The end product of this step is a list of social security identifiers. In Brazil, the *Cadastro de Pessoa Fisica* (CPF) is the equivalent of Social Security Numbers.

We merge this data with a panel of credit information provided by SERASA. The end product of this step is a panel of individuals with their respective credit profiles.

In the next step, we merge this panel with Entrepreneurship data constructed by scrapping firm records from JUCESP and performing text analysis on them. This data has both the CPF of the entrepreneurs as well as the firm identification number (CNPJ) of the firm they own. We also add gender, age (in bins of five years), education, and race of entrepreneurs, as well as an indicator variable if the firm had multiple owners if the given individual was a founder or joined the company later, and an indicator if the firm is in the MEI tax system.

Due to data construction limitations, we make the entrepreneurship data set uniquely identified at the individual level. This implies that if an individual who owns multiple firms cannot have their CPF-CNPJ pair for all of them. We proceed with the following order of priorities to choose the firm we keep: First, we prioritize non-MEI firms, second we prioritize firms created after 2016, third, we prioritize firms employing someone, then we prioritize if the individual was a founder of that firm, lastly we take the oldest firm among the remaining ones.

In the last step, we merge firm-level information provided by SERASA about the firms created by the entrepreneurs in our sample. As a firm can be owned by multiple individuals, this procedure comprises a ”many-1” merge, as potentially a group of individuals in our sample owns the same firm.

After these steps, the CPF and CNPJ identifiers are masked, implying that all information provided by the company was not observed by non-company-affiliated researchers with their identifiers. It is important to highlight that the data provided by the researchers not affiliated with SERASA is not uniquely identified in any of the columns in our sample. That ensures that the researchers not affiliated with the company had no way to identify any of the individuals in our sample following the procedures of Brazilian law.

In Table A7 we summarize how the different pieces of data were merged.

Table A7: Merge Descriptions

	Master Base ("Left df")	Using Base ("Right df")	Key Variable Merge (key)	Match Type	"How"
→ 1	Sample of Individuals SP	Credit Panel of Individuals	CPF	1-many	Keep only the ones that match with how=='inner' in Python
→ 2	Individuals' Panel with credit information	Data on Entrepreneurship	CPF	many-1	Keep all from the master base , drop the ones from using that did not merge with how=='left' in Python
→ 3	Individuals in SP + Enriched + Entrepreneur.	Credit and Balance Panel Firms	CNPJ	many-1	Keep all from the master base , drop the ones from using that did not merge with how=='left' in Python

Notes: This table provides a summary of how the data was constructed combining information from the company with datasets provided by the non-company affiliated researchers.

D Non Parametric Estimation of $h(s_i, s_i')$

In addition to the linear projection of changes in credit into the s_i, s_i' presented in Figure 8, we can also estimate $h(s_i, s_i')$ non-parametrically using sieve-estimators. This is useful as it gives us an additional test of our framework's empirical propositions and also assesses the quality of the linear fit.

To estimate $h(s_i, s_i')$ non parametrically we write assume

$$h(s_i, s_i') \approx h_{np}(s_i, s_i') = \phi(s_i) + \phi(s_i')$$

where $\phi()$ are flexible functions of their arguments. We then estimate the following equation using our full sample:

$$Y_{it} = \alpha_i + \delta_t + \phi(\text{C. Sc. Old System}_i) \cdot Post_t + \phi(\text{C. Sc. New System}_i) \cdot Post_t + \varepsilon_{it} \quad \text{(D.12)}$$

where α_i, δ_t are individual and time fixed effects, and $Post_t$ is a dummy that takes value one for observations after the implementation of the policy. We approximate $\phi()$ using fifth-order polynomials. As in our previous analysis, credit Scores in both old and new systems are normalized measures with mean 0 and standard deviation 1 (Z-scores).

Our coefficient estimates for both linear and non-parametric models are presented in Table A9. But the visualization in Figure A22 is more intuitive. In Panels (a) and (b), we observe the fit of both the nonparametric and linear estimates over the space spanned by s_i, s_i'. In Panel (c), we show how the fit differs between both estimates over the same space.

Visually, the restricted model under the linear assumption is similar to the non-parametric estimation. The latter also predicts that changes in credit are increasing *vertically* and *diagonally*, and decreasing *horizontally* (we further show this below). By looking at Panel (c), we observe that the linear model does a particularly good job in predicting changes around the (0,0) point and a poorer job in the extremities. This is somewhat by construction as $h(0, 0) = 0$ in both models. As discussed, this assumption comes from our conceptual framework. Despite the visual comparison between both estimates suggesting that they are similar, as somewhat expected given the size of our sample, tests of the linear restriction reject that the linear model is sufficient.

On top of the estimated changes in credit, we can also test the implications of our conceptual framework by looking at the derivatives from the estimated shape of the function. Given our estimated coefficients of function $\phi()$ we can recover numerical values for $\frac{\partial h_{np}(s_i,s_i')}{\partial s_i}$ and $\frac{\partial h_{np}(s_i,s_i')}{\partial s_i'}$.

We show the values of partial derivatives over the space spanned by (s_i, s_i') in Figure A23. In Panel (a), we show $\frac{\partial h_{np}(s_i,s_i')}{\partial s_i'}$, which we label as *vertical* changes consistent with our terms in the paper. Points in which the surface is colored blue indicate positive estimates for the partial derivative, whereas negative values are colored in red. We can observe that partial derivatives are positive in almost every point of the distribution of credit scores, consistent with the Proposition in our conceptual framework.

Our framework also predicts that changes in credit should decrease *horizontally.* We show our estimates for $\frac{\partial h_{np}(s_i,s_i')}{\partial s_i}$ in Panel (b). We can see that red values dominate in the figure, indicating that in the majority of the distribution, credit changes decrease as we increase the value of the old system credit score.

Figure A23 also allows us to evaluate the last proposition of our framework by observing how changes in credit scores vary *diagonally.* Panel (c) plots values of $\frac{\partial \hat{h_{np}}(s_i,s_i')}{\partial s_i'} + \frac{\partial \hat{h_{np}}(s_i,s_i')}{\partial s_i}$, therefore representing a marginal change in the 45-degree lines summarized by $s_i = s_i' + c$. Our estimates suggest that in the majority of the joint distribution of credit scores, $h(s_i, s_i')$ increases in diagonal comparisons.

Lastly, we can compute the expected value of the derivatives. We do it in two different ways, each with its own interpretation. First, we compute the average of over a grid of equally 1000 points in a space of $s_i \in (-1.5, 1.5) \times s_i' \in (-1.5, 1.5)$. We can define a distribution $\mathcal{F}(s_i, s_i')$ for this grid with density function $f(s_i, s_i') = 1/1000$ for points s_i, s_i' in the grid and 0 otherwise. Our average derivatives will then be:

Vertical:

$$\mathbb{E}\left[\frac{\partial \hat{h_{np}}(s_i, s_i')}{\partial s_i'}\right] = \int \frac{\partial h_{np}(s_i, s_i')}{\partial s_i'} d\mathcal{F}(s_i, s_i')$$

Horizontal:

$$\mathbb{E}\left[\frac{\partial \hat{h_{np}}(s_i, s_i')}{\partial s_i'}\right] = \int \frac{\partial h_{np}(s_i, s_i')}{\partial s_i} d\mathcal{F}(s_i, s_i')$$

Diagonal:

$$\mathbb{E}\left[\frac{\partial \hat{h_{np}}(s_i, s_i')}{\partial s_i'}\right] = \int \frac{\partial h_{np}(s_i, s_i')}{\partial s_i'} d\mathcal{F}(s_i, s_i') + \int \frac{\partial h_{np}(s_i, s_i')}{\partial s_i'} d\mathcal{F}(s_i, s_i)$$

With this method, we input equal weights for all points in the space spanned by s_i, s_i'. Thus, the moment we estimate could be considered an *unweighted* average of partial derivatives.

In another way, we use our sample of individuals with their s_i, s_i' and compute the average derivatives for each individual using the estimated partial derivatives. We could think of this as a *weighted* average over the joint distribution of signals, with the weights being the empirical distribution of signals. We compute the moments as follows:

Vertical:

$$\mathbb{E}\left[\frac{\partial \hat{h_{np}}(s_i, s'_i)}{\partial s'_i}\right] = \frac{1}{N}\sum_i \frac{\partial h_{np}(s_i, s'_i)}{\partial s'_i}$$

Horizontal:

$$\mathbb{E}\left[\frac{\partial \hat{h_{np}}(s_i, s'_i)}{\partial s_i}\right] = \frac{1}{N}\sum_i \frac{\partial h_{np}(s_i, s'_i)}{\partial s_i}$$

Diagonal:

$$\mathbb{E}\left[\frac{\partial \hat{h_{np}}(s_i, s'_i)}{\partial s_i} + \frac{\partial \hat{h_{np}}(s_i, s'_i)}{\partial s'_i}\right] = \frac{1}{N}\sum_i \frac{\partial h_{np}(s_i, s'_i)}{\partial s_i} + \frac{1}{N}\sum_i \frac{\partial h_{np}(s_i, s'_i)}{\partial s'_i}$$

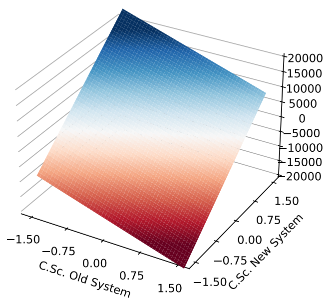
Our estimates for both methods suggest that on the three *directions*, the non-parametric model goes in line with our conceptual framework predictions. Changes in credit increase with respect to the value of the new signal and decrease with respect to the old signal. Furthermore, *vertical* increases are larger than *horizontal* ones, thus indicating that changes in credit also increase *diagonally*.

Table A8: Expected Values of Partial Derivatives in the Non-Parametric Estimation

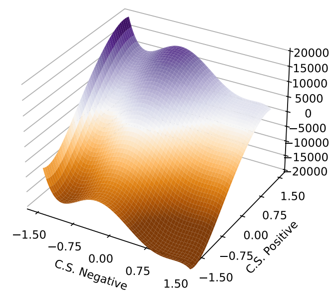
	(1) Over the Grid		(2) Over the Sample	
	Partial Derivative	Value	Partial Derivative	Value
Vertical	$\int \frac{\partial \hat{h}_{np}(s_i,s'_i)}{\partial s'_i} d\mathcal{F}(s_i, s'_i)$	9137.39	$\frac{1}{N}\sum_i \frac{\partial \hat{h}_{np}(s_i,s'_i)}{\partial s'_i}$	6731.50
Horizontal	$\int \frac{\partial \hat{h}_{np}(s_i,s'_i)}{\partial s_i} d\mathcal{F}(s_i, s'_i)$	-6104.38	$\frac{1}{N}\sum_i \frac{\partial \hat{h}_{np}(s_i,s'_i)}{\partial s_i}$	-6278.01
Diagonal	$\int \frac{\partial \hat{h}_{np}(s_i,s'_i)}{\partial s'_i} d\mathcal{F}(s_i, s'_i) + \int \frac{\partial \hat{h}_{np}(s_i,s'_i)}{\partial s_i} d\mathcal{F}(s_i, s'_i)$	3033.01	$\frac{1}{N}\sum_i \frac{\partial \hat{h}_{np}(s_i,s'_i)}{\partial s'_i} + \frac{1}{N}\sum_i \frac{\partial \hat{h}_{np}(s_i,s'_i)}{\partial s_i}$	453.49

In this Table we present the estimated values of partial derivatives using non parametric estimation. *Over the grid* estimates assume a 1000 point grid over $s_i \in (-1.5, 1.5) \times s'_i \in (-1.5, 1.5)$ with equal weights to each point. In turn, *Over the Sample* calculates partial derivatives to every individual in our sample and weights each individual equally.

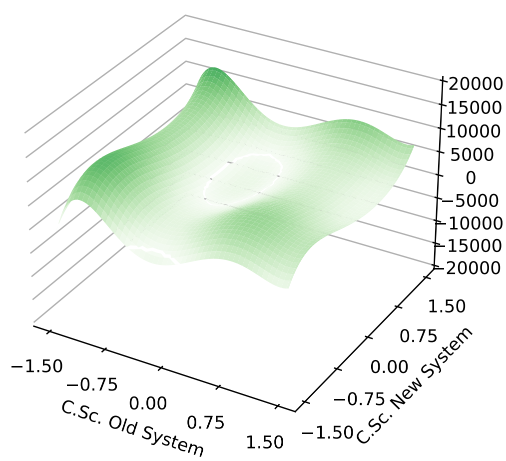
Figure A22: Comparisons Between Linear and Non-Parametric Estimates of Credit Change



(a) Linear Function



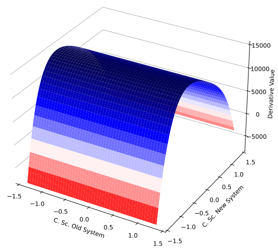
(b) Non-Parametric Function



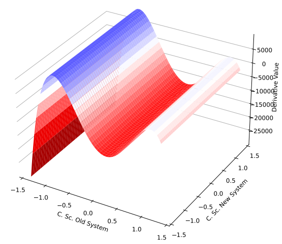
(c) Difference (a) - (b)

Panel (a) and (b) show predicted values of changes in credit over the space spanned by old and new system credit scores using our linear restricted model (equation 4) and the non-parametric model (equation D.12), respectively. Panel (c) shows the differences between estimates from (a) and (b).

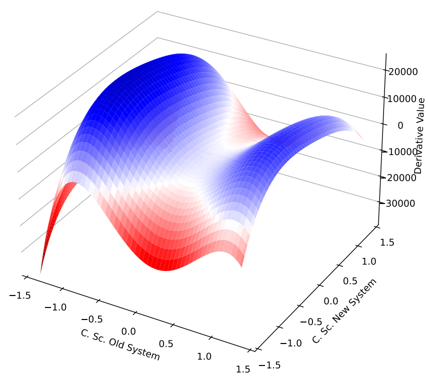
Figure A23: Comparisons Between Linear and Non-Parametric Estimates of Credit Change



(a) $\frac{\partial \hat{h}_{np}(s_i, s_i')}{\partial s_i'}$



(b) $\frac{\partial \hat{h}_{np}(s_i, s_i')}{\partial s_i}$



(c) $\frac{\partial \hat{h}_{np}(s_i, s_i')}{\partial s_i'} + \frac{\partial \hat{h}_{np}(s_i, s_i')}{\partial s_i}$

This Figure plots partial derivatives calculated over the joint distribution of credit scores in the old and new systems. We use our estimates from equation D.12 and calculate partial derivatives over a grid of 100 equally distributed points over the space spanned by s_i, s_i'

Table A9: Coefficients of Linear and Nonlinear Estimates

	(1)	(2)
	Dependent variable: Credit	
	Linear	Polynomials
C. Sc. Old System	-5172.3***	-11026.4486***
	(273.66)	(1471.2169)
C. Sc. Old System^2		-9494.6893***
		(1087.5093)
C. Sc. Old System^3		11295.7751***
		(2009.2653)
C. Sc. Old System^4		3954.4166***
		(507.6831)
C. Sc. Old System^5		-4041.9399***
		(651.5566)
C. Sc. New System	10951.58***	14178.6652***
	(370.70)	(721.0755)
C. Sc. New System^2		-2217.2853***
		(732.6252)
C. Sc. New System^3		-1089.4464**
		(532.9137)
C. Sc. New System^4		636.4236***
		(226.3324)
C. Sc. New System^5		-508.6011***
		(184.7629)
Observations	2875942	2875942

This Table presents coefficient estimates of equations 4 and D.12. Credit Scores in both old and new systems are normalized measures with a mean of 0 and a standard deviation of 1 (Z-scores). Standard errors in parenthesis are clustered at the individual level.

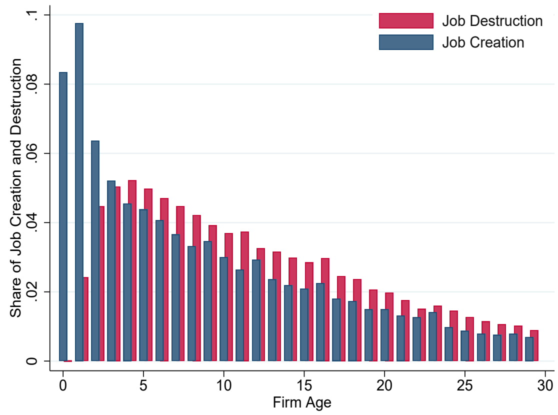
E Appendix - Entrepreneurs and Job Creation in Sao Paulo

Startups in São Paulo mimic behavior that has been documented in the United States. They are responsible for a large share of job creation in the economy. Nevertheless, most startups start small and remain small through time, conditional on survival. In this section, we replicate patterns documented for the U.S. in Decker et al. (2014) combining our entrepreneurship records from JUCESP with matched employer-employee data that covers the universe of formal labor markets (RAIS).

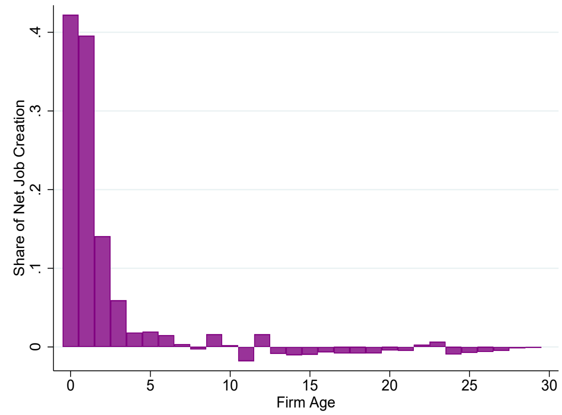
These patterns are illustrated in Figure A24, which are constructed using firm-level data from 2003-2015. We use an older period for this analysis as by the time this paper is being written, more recent RAIS data that allows us to follow firms' for at least 5 years after their creation is not yet available.

Panel A and B reflect job creation and destruction. A job is considered to be created if, for a given firm, one employee was added between year t and year t+1. The opposite holds for job destruction. Panel A shows that firms in their first 5 years of existence are responsible for around 35% of all jobs created in the period. In Panel B, we see that net job creation, calculated as job creation minus job destruction, is especially high in the first two years of a firm's history. This is found mechanically, as firms that have just been opened have no job destruction. In Panel C, we see that the median growth in the number of employees is 0 throughout all the first 15 years of firms' existence. Furthermore, we see that in the firms' first years, there is a higher variance in firm growth, which stabilizes around year 6. Lastly, in Panel D, we observe that firms are usually small in number of employees. The median startup in Sao Paulo starts with 3 employees, and by year 10, among active firms, the median firm still only employs 5 people.

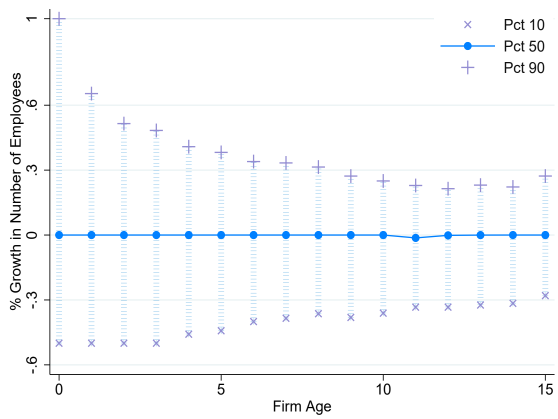
Figure A24: Startups Job Creation and Growth



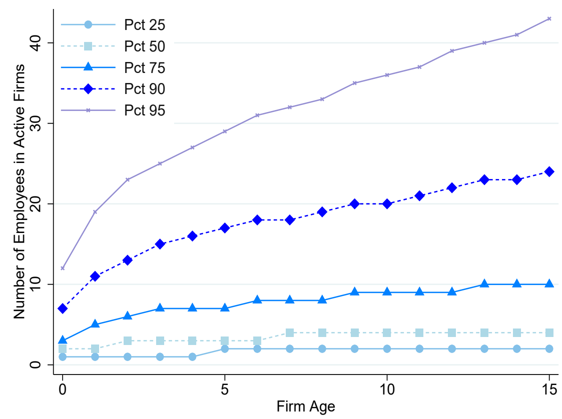
A. Job Creation and Destruction by Firm's Age



B. Net Job Creation by Firm's Age



C. Distribution of Growth by Firms' Age



D. Distribution of Firm Size by Firms' Age

Notes: Write

F Informality

In our main entrepreneurship analysis, our focus lies on formal firms. This is due to the fact that both our firm ownership records and the credit bureau data comprise only information from firms with a registration number in the *Cadastro Nacional de Pessoas Jurídicas* (CNPJ). In this Appendix, we discuss the role of informality in entrepreneurship, providing some descriptive evidence using the National Household Survey (PNAD-C) that encompasses both the formal and informal sectors.

First, we describe our informality definition. Brazil is a setting where this is very clear. For employees, all formal labor relations must have a "signed" *carteira de trabalho*. [41] Although this dichotomous distinction masks informal labor ties between formal workers (see Feinmann et al. (2022) for a full discussion about this issue), it is still useful as it is observable in the household survey. Informal employees represent 15% of all employees in the state of São Paulo and 13% of all non-domestic employees.

In the case of firms, we use as an indicator of formality the registration in the *Cadastro Nacional de Pessoas Jurídicas.* is the Brazilian National Registry of Legal Entities. It is a unique identifier issued by the Brazilian Federal Revenue Service (Receita Federal) to businesses and other legal entities operating in Brazil. The CNPJ number is similar to a tax identification number in other countries and is required for businesses to conduct legal and financial activities within the country. In PNAD-C, self-employed individuals and employers are asked if their business is registered in CNPJ.

Given the definition of formal businesses, we can describe the differences between formal and informal entrepreneurs in this setting. Given the PNAD-C questionnaire, we have to use a broad definition of entrepreneurs, which encompasses employers and self-employed individuals without employees. In our sample, 25% of entrepreneurs are employers.

We summarize the informality levels of entrepreneurs in Table A10. On average, 90.8% of employers' businesses are registered. In turn, 38.5% of self-employed individuals have a registration in CNPJ. When summing employers and self-employed, 51.4% of entrepreneurs have their businesses registered.

We further describe the informality rates of self-employed by economic sectors in Table A11. In Columns (1) and (2) we show the share of self-employed and employers in each sector. In Columns (3) and (4) we show the share of formal businesses in each sector. We observe that construction sector has the lowest formalization rates both among employers and self-employed individuals.

Lastly, we characterize the earnings distributions of formal and informal entrerpeneurs

[41] In the past, this consisted of an actual booklet for each employee, in which employers had to sign and register contract wages and hours. Nowadays, the process can be done fully electronically. Nonetheless, Brazilians still refer to forma job ties as those with "signed" *carteira de trabalho.*

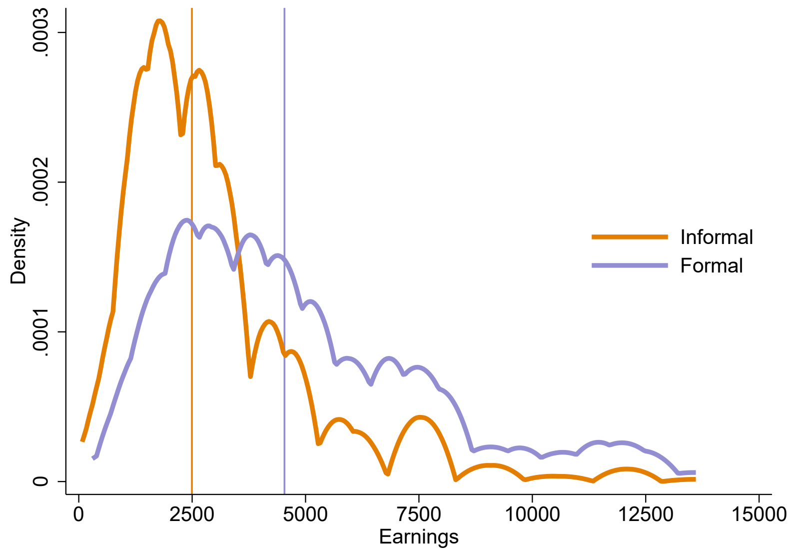
Table A10: Informality among Entrepreneurs

	(1) Informal	(2) Formal
Self-Employed	61.51	38.49
Employers	9.16	90.84
Entrepreneurs	48.63	51.37

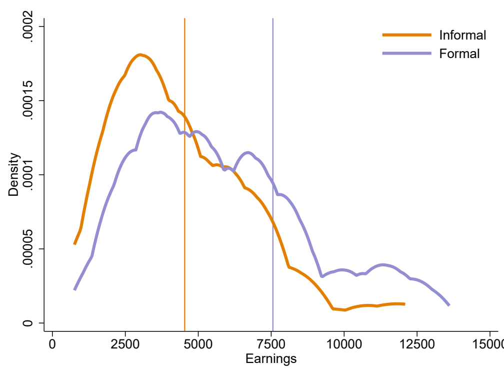
The sample is restricted to those between 18 and 65 years old in the State of São Paulo working at least 30 hours a week as entrepreneurs in their main job. Employers are those who self-report as *empregadores*, and self-employed are those who report as *conta-própria*. Entrepreneurs encompass both groups. Formal is defined as if their business had a registration in the CNPJ.

in Figure A25. We observe that the earnings of informal entrepreneurs are substantially lower than those of formal ones. This is valid for both employers and employees. The median earnings of formal entrepreneurs are almost double that of informal ones.

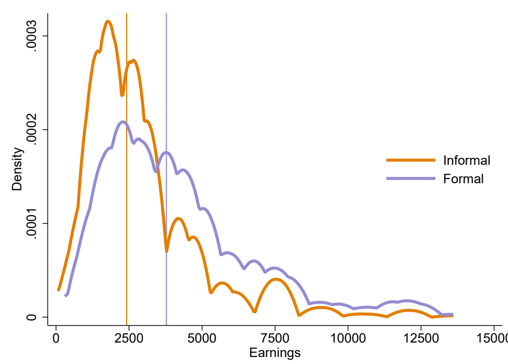
Figure A25: Entrepreneurs Earnings in the Formal and Informal Sector



(a) Entrepreneurs



(b) Employers



(c) Self-Employed

This Figure shows the distribution of labor earnings of entrepreneurs in the State of São Paulo in the last quarter of 2019 built with PNAD-C. The sample is restricted to those between 18 and 65 years old working at least 30 hours a week as entrepreneurs in their main job. Values are adjusted for December 2023 BRL. Employers are those who self-report as *empregadores*, and self-employed are those who report as *conta-própria*. Entrepreneurs encompass both groups. Formal is defined as if their business had a registration in the CNPJ. Vertical lines correspond to median earnings of each group.

Table A11: Share of Business Registration by Economic Sectors

	(1)	(2)	(3)	(4)
	Share in each Sector		Share of Formal Businesses	
	Self-Employed	Employers	Self-Employed	Employers
Agriculture	3.56	4.32	57.32	85.21
Manufacturing	8.61	9.45	32.41	92.39
Construction	17.61	6.59	20.40	71.47
Retail	20.19	31.39	51.62	97.38
Transport & Food	19.88	17.06	28.96	88.10
Professional Services	17.64	25.23	51.84	90.90
Domestic Services	12.51	5.97	38.11	87.05

The sample is restricted to those between 18 and 65 years old in the State of São Paulo working at least 30 hours a week as entrepreneurs in their main job. Employers are those who self-report as *empregadores*, and self-employed are those who report as *conta-própria*. Entrepreneurs encompass both groups. Formal is defined as if their business had a registration in the CNPJ. Sectors are constructed using the first 2-digit of cnae as follows values 1-9 (Agriculture), 2 for 10-39 (Manufacturing), 3 for 41-43 (Construction), 4 for 45-48 (Retail), 5 for 49-56 (Transport & Food), 6 for 58-88 (Professional Services), and 7 for 90-99 (Domestic Services).