Government Banks and Interventions in Credit Markets[*]

Gustavo Joaquim
Amazon

Felipe Netto[†]
Bank of England

José Renato Haas Ornelas[‡]
Banco Central do Brasil and FGV-EPPG

September 2025

Abstract

We study a large intervention in Brazil, where commercial government banks unexpectedly expanded lending to address excessive concentration in the banking sector. The policy led private banks to reduce loan interest rates, with stronger effects for smaller firms. Firms reliant on government banks experienced a large increase in leverage, which caused an increase in default rates for government bank loans. Nonetheless, the policy generated employment growth at a relatively low cost per job compared with other interventions. These results indicate that government banks can, at least in the short term, foster competition and expand SME credit access in emerging markets.

Keywords: Credit Market Interventions, Government Banks, Market Power, SME Lending

JEL Classification: E44, E65, G21, G28

[*]Felipe Netto thanks José Scheinkman, Martin Uribe and Olivier Darmouni for invaluable guidance and support. We thank Guy Aridor, Marco Bonomo, Andres Drenik, Matthieu Gomez, Emilien Gouin-Bonenfant, Rodrigo Gonzalez, Ralph De Haas, Neeltje van Horen, Martina Jasova, Artashes Karapetyan, Borja Larraín, Mauricio Larraín, Cameron LaPoint, Yueran Ma, Aakriti Mathur, Ioana Neamtu, Amine Ouazad, Joe Peek, Matthew Pritsker, Tommaso Porzio, Silvio Ravaioli, Bernardo Ricca, Dario Romero, Farzad Saidi, Arkodipta Sarkar, and seminar participants at FIRS 2023, EEA 2023, Eastern Finance Association 2023, WEFIDEV Webinar 2023, Luso-Brazilian Finance Meeting 2022, WEAI - IBEFA 2022, the Bonn-Boston-Cambridge-Columbia-UCL PhD Student Workshop, Insper, the Federal Reserve Board of Governors, the Federal Reserve Bank of Boston, PUC Chile, the Bank of England, University of Bonn, Columbia University, and the Central Bank of Brazil, for comments and suggestions. **Gustavo's contribution took place prior to his tenure at Amazon. The views expressed herein do not necessarily reflect those of the Bank of England or its committees. The views expressed herein do not necessarily reflect those of the Banco Central do Brasil.**

[†]Felipe.Netto@bankofengland.co.uk

[‡]jrenato.ornelas@bcb.gov.br

I. Introduction

Access to finance has been a long lasting challenge in developing countries, where firms obtain loans from highly concentrated banking sectors. Small and medium-sized enterprises (SMEs) are particularly vulnerable to market power in the banking sector, often facing higher borrowing costs in less competitive markets (Rice and Strahan, 2010). Consequently, governments often make use of interventions to reduce barriers to credit, including policies supported by the use of state-owned banks, which are widespread across the world.[1] The use of state-owned banks can be beneficial, such as to increase branch coverage (Fonseca and Matray, 2024) and to lend during economic downturns (Jiménez et al., 2019). However, government bank lending can also cause misallocation (Carvalho, 2014) and excessive growth in household debt (Garber et al., 2024). Despite these trade-offs, little is known about whether public banks can effectively foster bank competition and expand SME credit access.

In this paper, we assess the use of government banks to address excessive market power in the banking sector. We study an unexpected large-scale intervention in Brazil, whereby commercial state-owned banks expanded SME lending at low interest rates, aimed at pressuring private banks to lower their loan interest rates. The use of government banks to stimulate competition has received little attention in prior work, making our setting a natural experiment to study its effects. By examining this episode, we highlight how market failures shape lending rates, and assess the broader costs and benefits of deploying state-owned banks as a competitive instrument. Using credit registry data, we show that the policy increased access to credit along the intensive margin, driven by a reduction in private banks' interest rates, with larger effects for smaller firms. Moreover, we find an increase in firm leverage, leading to higher default rates for public banks. Nonetheless, the policy led to larger job creation in areas more exposed to government banks. The estimated cost per job created places the intervention favorably relative to other policies, illustrating the benefits of expanding SME credit access in emerging markets.

[1] We use the expressions *state-owned banks*, *government banks*, and *public banks* interchangeably, to refer to banks controlled by a local or federal government. We refer to other banks as *private banks*.

The episode provides a suitable setting to study how government banks can counter market power in credit markets. First, Brazil has many features which are common in credit markets of emerging economies: a large role for state-owned banks, limited SME access to credit, and high loan spreads. Between 2009 and 2013, state-owned banks accounted for 32% of assets in Brazil, compared to 24% across South America. Working capital loans financed by banks made up 21% of Brazilian firm funding, versus 16% regionally.[2] Second, SMEs, the focus of our analysis, are especially vulnerable to barriers to credit access, and play a key role for economic growth and employment.[3] Third, the intervention occurred outside of a crisis, and private banks were not differentially exposed to other systematic shocks, allowing us to better isolate the mechanisms through which the intervention affects credit markets.

The intervention used Brazil's two largest state-owned banks, *Banco do Brasil* (BB) and *Caixa Econômica Federal*, both among the country's five largest banks. In March 2012, the Brazilian government announced a large increase in lending by these two banks, citing concerns that high interest rates to households and firms reflected excessive market power by private banks. The program was massive, with public banks increasing their supply of working capital loans by more than three times. Additionally, public banks charged lower interest rates compared to private banks across the whole period. The relatively small market share of public banks prior to the policy, despite large differences in interest rates, is consistent with theoretical models in which banks operate with capacity constraints. We argue that the interaction between public and private banks, along with the underlying market failures, are essential to understand how the policy can lead to a drop in private interest rates. At the same time, using public banks to foster competition via lending expansion raises the question of whether the benefits of greater credit access outweigh the potential costs of higher defaults — a central policy trade-off that our analysis addresses.

Our empirical analysis starts by analyzing the response of private banks to the increase in lending by public banks, focusing on loan interest rates. The identification challenge is

[2] Data sources for descriptive statistics reported throughout the text are available in Appendix A.

[3] These firms account for 60-70 percent of employment worldwide (Ayyagari, Beck and Demirguc-Kunt, 2007) and for the majority of job creation in the United States (Neumark, Wall and Zhang, 2011).

that interest rates could vary in the cross-section *and* across time for several reasons, such as borrower, industry and region characteristics, on top of aggregate factors such as monetary policy changes. To isolate the effects of the policy on private banks' interest rates, we rely on the fact that the intervention was unexpected and that no other systematic events that could differentially affect the behavior of private and public banks took place at the time of the policy. Our identification hypothesis is that, absent the intervention, private and public loan interest rates for nearly identical borrowers would have changed similarly over time. This setup is akin to a difference-in-differences specification in which private bank loans are treated (to capture their response to the competition shock), and public bank interest rates remain mostly unchanged, as the policy did not entail changes in state-owned banks' pricing behavior.

We document a sharp drop in loan interest rates of private banks immediately after the increase in credit by public banks. This reduces the difference between the lending rates of public and private banks by about four to five percentage points, corresponding to 30-50 percent of the pre-intervention difference between private and public banks' lending rates. Moreover, micro firms, the smallest firm size in our sample, experience a drop in interest rates around 1 percentage point larger than the reduction experienced by small firms, and 4 percentage points larger than the drop for medium and large firms. The larger drop in private loan interest rates for smaller firms indicates that the policy is successful at improving credit access for the most constrained firms. Despite this reduction in private loan interest rates, however, we do not find an aggregate increase in credit supply by private banks during the same period.[4] Moreover, by the end of 2013, firms with a pre-intervention relationship with public banks experienced an increase in leverage more than twice as large as the increase in leverage of firms with a pre-intervention relationship with private banks.

As with other types of government interventions, the increase in lending by public banks could impact borrower default risk.[5] We explore these effects by looking at varia-

[4]The impact of the policy on private banks resembles the impact of US bank branch deregulation, with lower loan interest rates for incumbent banks, but no effect on loan amounts (Rice and Strahan, 2010).

[5]This could be caused by the impact of higher corporate leverage on default (Traczynski, 2017), if new borrowers who benefit from the intervention are riskier (Jiménez et al., 2019), or due to asymmetric information (Stiglitz and Weiss, 1981; Martinez-Miera and Repullo, 2010; Crawford, Pavanini and Schivardi,

tion in loan delinquency rates for public and private bank borrowers. Prior to the policy, government banks had *lower* delinquency compared to private banks. However, the intervention led to an increase in the delinquency rates of government bank loans relative to private bank loans. Our estimates at the firm level indicate that the probability that a public bank borrower became delinquent was 1 p.p. greater than the probability that a private bank borrower became delinquent after the policy. This corresponds to a 20 percent higher probability of delinquency for firms borrowing from public banks relative to firms borrowing from private banks. This relative increase in delinquency rates of public banks was entirely driven by loans to previously levered firms, and the difference between delinquency rates of public and private banks increases monotonically with the degree of firm leverage. In contrast, public banks continue to experience lower delinquency rates relative to private banks when lending to *unlevered* borrowers.

The second part of our analysis focuses on the effects of the policy at the local market level, which allows us to gauge the magnitude of the impact of additional public lending in a given market on credit and real outcomes. The main challenge is that, despite the quasi-experimental nature of our setup, changes in credit in a given location could correlate with time-varying demand in that specific location. We overcome this challenge by using the share of the outstanding volume of firm loans originated by public banks in a given market two years before the intervention, interacted with a post policy dummy, in an instrumented DiD approach. The core identifying assumption is that our instrument does not systematically correlate with changes in demand for credit around the time of the policy. After the policy, a 1 p.p. larger loan origination in a given quarter relative to 2011 leads to a drop of 29 basis points in private loan interest rates, which accounts for around half of the average reduction in private interest rates. The same 1 p.p. increase in loan origination within a quarter leads to an increase of 1.2 in the debt-over-payroll costs ratio of firms in the same market, around 60% of the weighted average leverage in 2011.

The broader availability of public loans *and* the lower cost of private loans should improve credit access. Since both effects are captured by our regional instrument, we employ a similar approach to analyze the effect of the policy on real outcomes. Using the

2018).

same instrumented DiD approach, we find that a 1 p.p. larger loan volume in a given year increase employment in a given municipality by 0.15% in the same year after the intervention. To further assess whether these effects are supply driven, we split our analysis between tradable and non-tradable employment, where the former are less dependent on potential local demand effects. We find that the effect of the policy is more than five times larger for local tradable employment compared to local non-tradable employment, further supporting the idea that the policy works mainly through supply side effects.

Finally, we assess the policy through a cost-benefit lens by calculating the cost per job created, a common metric for evaluating credit interventions such as loan guarantees (Brown and Earle, 2017; Bonfim, Custódio and Raposo, 2023; Barrot et al., 2024). We measure costs as the loan losses incurred by the increase in default from public banks, and link municipality-level changes in public credit to local employment to obtain job creation. This yields a cost per job of about R$ 1,987, or roughly three times the monthly minimum wage in Brazil. For comparability, we scale this figure by the minimum wage across countries. Our estimate is higher than the cost of France's loan guarantee program Barrot et al. (2024), but considerably lower than those for SBA loans in the US Brown and Earle (2017) and for Portugal's loan guarantees Bonfim, Custódio and Raposo (2023). While these estimate reflects only short-term effects and omit potential long-run costs from higher defaults, leverage, or misallocation, they suggest that state-owned banks can, under the right conditions, serve as relatively cost-effective instruments to stimulate competition and improve SME credit access in emerging markets.

Related Literature. Our paper contributes to the literature that studies government interventions in financial markets. Challenges associated with ensuring credit access for SMEs spurred a variety of policies aimed at addressing market failures that cause lack of credit access (de Haas and Gonzales-Uribe, 2024). One common example are loan guarantees, which have been shown to increase credit supply (Bachas, Kim and Yannelis, 2020), and drive employment growth (Brown and Earle, 2017; Bonfim, Custódio and Raposo, 2023; Barrot et al., 2024). However, loan guarantees could come at a cost of risk shifting by lenders (Stillerman, 2024). Furthermore, the pass through of loan guarantees can be affected by lender incentives (Martín, Mayordomo and Vanasco, 2024) and market power (Ornelas et al., 2024), with previous studies finding no effects of guarantees on loan interest rates (de Blasio et al., 2018). Another tool to enhance credit access are interest rate caps. Previous studies often find that rate caps lead to lower credit supply, especially to riskier borrowers (Cuesta and Sepulveda, 2018; Quirk, 2023; Cherry, 2024).[6] Our contribution is to show how an alternative tool, the use of government banks to foster competition, affects credit market equilibrium, quantifying the resulting tradeoffs.

We contribute to the literature assessing the use of state-owned banks to implement financial inclusion policies. Fonseca and Matray (2024), study an expansion in branch presence by government banks in Brazil, finding that better access to external funding increased economic activity, with unequal wage gains accrued by high skill workers. Additionally, Garber et al. (2024) shows how an increase in payroll lending by state-owned banks to improve household credit access caused an increase in debt, which was followed by a drop in household consumption during the 2014–2016 economic downturn. In contrast, we study an intervention whose impact on credit access occurs not only through the direct availability of public credit to SMEs, but also through the reduction in private bank interest rates arising from a competitive interaction. Moreover, higher SME leverage leads to an increase in default rates, which is less of a concern for payroll loans studied by Garber et al. (2024). In contrast, loan default corresponds to the main cost of the intervention we study.[7]

As such, this paper also contributes to the broad literature that studies the tradeoffs of the ownership of banks by the state. The costs are often caused by political capture and misallocation ((Porta, Lopez-De-Silanes and Shleifer, 2002; Sapienza, 2004; Dinç, 2005; Carvalho, 2014)), whereas benefits include state-owned banks lending during financial crises (Coleman and Feler (2015); Cortes, Silva and van Doornik (2019); Jiménez et al. (2019); Capeleti, Garcia and Sanches (2022)).[8] Our paper adds to this literature by ex-

[6] One exception is Kuroishi, LaPoint and Miyauchi (2025), who find an *increase* in corporate credit following the introduction of a rate cap on corporate loans in Bangladesh.

[7] In that sense, our study is connected to other papers exploring government interventions and the associated problems arising from higher firm leverage, such as Crouzet and Tourre (2021).

[8] See also Cole (2009), Assuncao, Mityakov and Townsend (2012), Lazzarini et al. (2015), Bertay, Demirgüç-Kunt and Huizinga (2015), Ru (2018), Cao et al. (2022) and Panizza (2024).

ploring how government banks can be used as instruments of competition policy in concentrated banking markets. We find costs associated with increased leverage and default, and benefits coming from enhanced credit access and job creation. In doing so, we also shed light on the nature of the competition between state-owned and private banks. Previous studies found that private banks are not sensitive to the presence of public banks (Coelho, Mello and Funchal, 2012), and that these two types of banks can generate positive complementarities if public and private banks offer different types of credit (Sanches, Junior and Srisuma, 2018). In contrast, we find that private and public loans are substitutes when both types of banks offer the same type of credit, with cheaper outside option for borrowers prompting private banks to reduce loan interest rates.

II. Data

Our main source of data is the credit registry from the Credit Information System (SCR) of the Central Bank of Brazil. Banks are required to disclose to the Brazilian central bank loan-level data for all outstanding loans with amounts above a certain threshold, allowing us to observe the near universe of loans to firms in Brazil.[9] The data includes detailed information about loan contracts, such as the type of credit, interest rate, amount, maturity, delinquency status, and firms' time-invariant taxpayer identifiers. Since development banks in Brazil fund a large amount of loans using on-lending to commercial banks (Lazzarini et al., 2015), which is often earmarked, we restrict the analysis to loans funded by banks' own resources. We focus on uncollateralized working capital loans, which are the primary source of funds for firms. Working capital loans account for roughly 50 percent of the loan volume in our sample before the intervention (March 2012) and 60 percent by the end of 2013.

We combine the credit registry data with employment data comes from the Annual Review of Social Information (RAIS) using each firm's time-invariant taxpayer identifier. All tax-registered firms in Brazil are required to complete a form in which they provide individual labor contract information for each of their employees. Given the severe penalties

[9]The threshold is R$ 5,000 ($\sim$ \$2,500) until December 2011 and R$ 1,000 ($\sim$ \$500.00) after that.

firms face for incomplete or late filings of the form, RAIS covers the universe of formal firms. We aggregate these data to obtain employment at the firm level and construct firm size. We classify firms' industry according to their 2-digit *CNAE* classification, and use employment headcount to construct firm-size categories. Both credit registry and employment data at further aggregated at the municipality level for our analysis of market level outcomes, which is complemented with other publicly available data with information on banks' balance sheets, branch level data, and municipality economic characteristics. Further details on our sample construction are available in Appendix A.

Table 1 shows summary statistics from our loan level data. Panel A displays a large spread between loan interest rates of private banks and public banks. Interest rates on public bank loans were more than 10 percentage points lower than interest rates on private bank loans, on average. Moreover, the standard deviation of loan interest rates of public banks is also smaller than the standard deviation of interest rates of private banks, suggesting less price discrimination across borrower characteristics. Public bank loans are also smaller and have longer maturities. Panel B provides displays firm characteristics based on their bank relationships. Firms that borrow exclusively from either of the two types of banks are similar across most characteristics, whereas firms with access to both types of banks are considerably larger.

Table 2 displays summary statistics of our municipality level sample. Municipalities where both types of banks operate have much larger local economies compared to municipalities in which only one of the two types of bank is present. Locations with only private bank branches or only state-owned bank branches are similar across most characteristics. The ratio of *branch* credit to GDP of public banks is substantially larger than the ratio of *branch* credit to GDP of private banks, where branch credit refers to the credit provided by branches in a given municipality. When looking at firm credit, based on *firm* location rather *branch* location, these ratios are more alike, suggesting that government banks played a larger role in lending to consumers rather than lending to firms prior to the intervention. Moreover, even in locations where all branches belong to a government bank, more than half of working capital loans is provided by private banks, on average.

III. Institutional Setting and Intervention Details

The inflation stabilization process started in 1994 led to successive waves of bank restructuring, mergers, and the privatization of regional state banks, leading to an oligopolistic banking sector in Brazil. By 2011–2013, the five largest commercial banks (Bradesco, Itaú, Santander, Banco do Brasil, and Caixa Econômica Federal) held nearly 80% of total bank assets. Two of them — Banco do Brasil (BB) and Caixa Econômica Federal (CEF) — are large state-owned *commercial* banks. Despite differences in their management and ownership structures, both are controlled by the federal government and are often used to implement credit policies. Unlike development banks, BB and CEF provide the same types of credit as private banks, and, similar to other commercial state-owned banks (Panizza, 2023), are profitable. Importantly, differences between public and private bank lending do not stem from regulatory treatment, since both are subject to the same regulatory framework.

Following the Global Financial Crisis, Brazil experienced fast economic recovery, with GDP growth of 7.5% in 2010 and 4.3% in 2011 (Appendix B, Figure B.1, Panel (a)). Dilma Rousseff assumed the presidency in January 2011 and expanded policies aimed at sustaining economic momentum. Monetary policy turned expansionary, with the interbank interest rate falling from about 12.5% in July 2011 to 7.5% in October 2012 (Appendix B, Figure B.1, Panel (b)). Nonetheless, loan interest rates remained high during this period (Table 1), even by emerging-market standards.[10] Despite the expansionary monetary policy, loan interest rates for small businesses loans remained high throughout 2011, prompting the government to pursue further intervention in the banking sector.

Initially, these interventions included capital injections in public banks and regulatory changes in households loans (Garber et al., 2024). In early 2012, the government deployed BB and CEF more directly to address the perceived lack of competition in the banking sector. In March 2012, it announced that both banks would expand consumer and firm lending at lower interest rates. The aim was to exert competitive pressure on private

[10] The lending spread in 2011 was 32.9 percentage points in Brazil, compared with 3.4 p.p. in Argentina and 3.7 p.p. in Mexico, for example.

banks and induce them to reduce their own loan rates. Both BB and CEF had the balance sheet capacity to expand credit, following capital injections in 2011. Although the policy coincided with a slowdown in the Brazilian economy, its timing does not appear linked to political or broader macroeconomic concerns.[11] Consistent with this, Appendix C shows no evidence that political concerns influenced the regional allocation of public credit.

Figure 1, Panel (a), shows a sharp rise in total credit from BB and CEF relative to March 2012, with no comparable expansion among the largest private banks. Figure 1, Panel (b), shows that this increase was not offset by a contraction of other assets. Both public and private banks experienced an initial decline in profitability, as reflected in lower ROA in the quarters following the intervention (Figure B.4). By mid-2013, however, macroeconomic conditions changed, and banks anticipated monetary policy tightening. Government officials indicated public banks could no longer sustain the credit expansion due to balance sheet constraints and rising default risk.[12] In 2013, public and private banks started increasing lending rates, and public banks slowed their credit expansion.

Figure 2, Panel (a), shows a surge in the origination of working capital loans at the onset of the intervention. Public bank loan origination rose from an average of R$520 million per month before the policy to R$2.8 billion afterwards — an increase of roughly R$46 billion between April 2012 and December 2013 relative to the pre-intervention average. Despite this sudden and large increase in government banks' lending, there was no drop in credit supply by private banks. Figure 2, Panel (b) shows the average interest rate of working capital loans of public and private banks. Government banks consistently charged lower rates, both before and after the intervention. However, the beginning of the policy coincided with a sharp decline in private bank interest rates. Despite this reduction, the spread between public and private banks remained substantial, averaging 12.8 percentage points before and 7.4 percentage points after the intervention.[13]

[11] Dilma Rouseff's net approval was high when the policy was announced (Appendix B, Figure B.2). Figure B.1 shows that the policy does not coincide with a severe drop in stock markets (Panel (c)), or with excessive volatility in exchange rates (Panel (d)). Figure B.3 shows that the policy announcement does not coincide with a drop in GDP growth forecasts (Panel (a)), or an higher in inflation expectations (Panel (b)).

[12] For instance, see https://www.valor.com.br/financas/3017518/governo-ve-limite-para-bb-e-caixa.

[13] Appendix B Figure B.5 shows a sharp drop in the share of public bank loans for unlevered borrowers after the policy. Since most unlevered borrowers are new borrowers, this indicates that financial inclusion gains from the policy occurred along the *intensive*, rather than *extensive*, margin.

III.1. Theoretical Background

The ability of government banks to influence private loan interest rates and the overall effects of the intervention depends on how public and private banks interact. A priori, it is unclear if the intervention would have achieved the government's objective of reducing loan interest rates and increasing credit access. Previous studies that focused on the interaction between public and private banks in Brazil did not find evidence that these banks compete. For example, Coelho, Mello and Funchal (2012) finds evidence of competition between private banks in a given market, but no evidence that the presence of a public bank affects lending by private banks. Moreover, Sanches, Junior and Srisuma (2018) finds that public banks generate positive complementarities for private banks. However, in our context, private and public banks provide similar loans to SMEs, and so competition between these two bank types is more likely, instead of complementarities, which could occur if government banks focus on other credit types (Sanches, Junior and Srisuma, 2018).

More generally, the effectiveness of credit interventions is directly related to the underlying market failures affecting credit allocation, which can make creditworthy borrowers rationed and affect default risk more broadly. High interest rates might reflect excessively large market power by private banks. At the same time, higher loan interest rates might also reflect private banks' expectations of higher default risk. Furthermore, adverse selection exacerbate the negative implications of borrower risk for interest rates and credit supply, but could be mitigated by market power (Crawford, Pavanini and Schivardi, 2018).

What does the aggregate data suggest about how public and private banks interact for working capital loans? First, Figure 2, Panel (b), shows that, prior to the intervention, private and public banks charge substantially different average interest rates for the same loan product. Despite such differences in prices, public banks have a much smaller market share prior to the policy, as shown in Figure 2, Panel (a). Such equilibrium is consistent with a theoretical setup where public banks have smaller capacity constraints than private banks, as in the duopoly case explored by Marquez (2002), which results in lower prices for the more constrained competitor. From that perspective, the intervention can be seen as an increase in the capacity constraints faced by public banks, the competitor with the smallest market share. This increase in capacity constraints could be the result, for example, of the capital injection that took place in late 2011 Garber et al. (2024). Such increase would affect the pool of applicants faced by private banks, which would bid more aggressively for borrowers, leading to a reduction in loan interest rates.

Another possibility, which finds empirical support for interventions using state-owned banks during crises (Jiménez et al., 2019), is that the policy results in public banks attracting riskier borrowers. From that perspective, the policy can affect the lending decisions of private banks if the increase in lending by government banks improves the perceived creditworthiness of the residual pool of borrowers of private banks. This improvement in borrower quality could lead to a reduction in loan interest rates if private banks price the lower probability of default accordingly. Such effect would be similar to the mechanism underlying interventions during financial crisis, when the government acquires worse quality assets, creating incentives for private lenders to resume lending, as in Tirole (2012) and Philippon and Skreta (2012).

Both scenarios would result in lower interest rates for private banks, as observed after the intervention is implemented in Figure 2, Panel (b), but would have different implications for loan default risk of new and existing borrowers. Hence, we will revisit the mechanism through which the policy is intended to lead to a reduction in private banks' interest rates when exploring ex-post loan default.

IV. Loan Interest Rates, Firm Debt and Default

The intervention was motivated by the idea that private banks would respond to additional competition caused by public banks sudden and large increase in lending, and cause a reduction in private loan interest rates. The policy entailed a large increase in the amount of working capital loans, but no changes in *public bank* loan interest rates, which remained low at comparable levels before and after the intervention. The evidence in Figure 2, Panel (b), suggests a drop in private loan interest rates in response to the policy, which did close the gap between public and private banks' interest rates. However, interest rates can reflect borrower or loan characteristics which could be different for private and government banks. We account for these borrower characteristics using a broad range of fixed effects, and compare loans issued by private and government banks before and after the intervention. Since we are interested in the response of private bank *interest rates*, our setup resembles a difference-in-differences specification in which private banks are treated. We rely on the fact that there were no changes to public banks' pricing strategy, and use public bank loans as control.

Although it can be challenging to identify the effects of the intervention, our experiment does not coincide with other systematic shocks that could cause meaningful changes in the difference between private and public interest rates. In particular, there were no large mergers, bank failures, or other macro-prudential policies that would affect different banks differently. Furthermore, the absence of a concurrent financial crisis eliminates concerns about differential behavior of private and government banks — or their borrowers — during such episodes. Hence, our identification assumption is that, given the absence of any systematic shocks that hit private and government banks differently, sharp changes in the spread between private and public banks' interest rates were caused by the intervention. The underlying hypothesis is that the spread between interest rates of public and private banks would have remained roughly constant other than for changes induced by the policy. We test this hypothesis estimating Equation (1) at the loan level:

$$i_{jtmbfs} = \alpha_{tmsf(size)} + \alpha_b + \sum_{\tau \neq 0} \delta_\tau \; Private_b^\tau + \varepsilon_{jtmbfs} \tag{1}$$

where i_{jtmbfs} denotes the interest rate of a loan j issued in month t, in municipality m, by bank b ,to firm f, which belongs to in industry s. $Private_b^\tau$ is a dummy equal to one in month τ if bank b is a private bank. Furthermore, we include time-municipality-industry-size fixed effects, $\alpha_{tmsf(size)}$, to account for differential demand industry or region specific demand. We weight the regressions by loan volume. The coefficients of interest are δ_τ, the differential change in interest rates charged by private banks relative to public banks in period τ, relative to March 2012, the reference month. The use of a broad range of fixed effects ensures that we are comparing loans in the same region and month and for firms in the same industry that have the same size. Finally, we estimate two separate specifications, one with bank (α_b) fixed effects, and another with bank-firm fixed effects α_{bf}, to account for time-invariant differences across banks and bank-firm pairs. We chose to do so to understand how the inclusion of firm fixed effects — which drops all firms that only borrow once in our sample — affects our point estimates.

The results are shown in Figure 3. The baseline specification in Panel (a) displays a large drop in private banks' loan interest rates relative to government banks' loan interest rates after the intervention. It also shows pre-trends in the spread between public and private interest rates which are similar to what can be seen in average rates shown on Figure 2, Panel (b). Such pre-trends can be largely explained by loans to firms that borrow only once. Figure 3, Panel (b), shows our specification with bank-firm fixed effects, in which violations to pre-trends are much less noticeable. To confirm that this the previous trend is largely explained by firms that only borrow once within our time window, in panel (c) we re-estimate Equation 1 including only Bank fixed effects, but with the same sample of loans as from Figure 3, panel (b). Both plots show the same large drop in the private-public interest rate spread, with a larger drop for our less saturated specification with only bank fixed effects.

Nonetheless, since all plots show evidence of a pre-trend, we implement the tests proposed by Rambachan and Roth (2023) for the parallel trends assumption in the presence of a pre-existing linear trend. Appendix B Figure B.6 shows the resulting relative magnitude tests for the specification with bank (Panel (a)) and bank-firm (panel (b)) fixed effects. The negative response of private banks interest rates in May 2012 is robust to the existence of a previous linear downward trend of private interest rates relative to public interest rates in both cases.

To obtain an estimate of the average reduction in private bank interest rates in response to the intervention, we estimate the following specification:

$$i_{jtmbfs} = \alpha_{tmsf(size)} + \beta_0 Private_b + \beta_1 Post_t \times Private_b + \varepsilon_{tmbfs}, \tag{2}$$

Where $Post_t$ is a dummy which equals 1 after March 2012, $Private_b$ is a dummy which equals 1 for loans issued by private banks, and the other terms are defined as before. Our coefficient of interest, β, captures the average change in the difference between interest rates of private and public banks after the intervention. We estimate four variations of equation 2 with different sets of fixed effects to gauge the magnitude of the adjustment in interest rates when accounting for different characteristics. The results are shown in Table 3. The first column shows that the weighted spread between public and private interest rates for similar firms before the policy is around 7.6 percentage points, but falls by around 6 percentage points after the policy. Column two indicates a drop of 5.5 p.p. when including bank fixed effects, suggesting a limited role for heterogeneity across banks in explaining the drop in interest rates. The last column indicates a drop of around 3.3 percentage points in the private-public spread once we include α_{bf}. To understand the extent to which the smaller number of observations in column (3) explains the smaller drop in the spread between private and public interest rates, we re-estimate the baseline and bank fixed effect specifications, restricting the sample to be equal to the sample used in column (3). The resulting point estimates are very close to the results across the whole sample. This suggests that specific firm-bank relationships matter for the pass-through of the policy.

Taken together, the point estimates suggest the policy led to a drop ranging between 40-80% in the spread between public and private rates. This large and sudden decrease suggests private banks did respond to the competitive pressure arising from the increase in state-owned banks' credit by reducing their loan interest rates.

IV.1. Heterogeneous Effects on Private Banks' Interest Rates

The literature has emphasized how information frictions prevent firms from switching banks, and how these frictions are particularly relevant for SMEs (Beck, Demirgüç-Kunt and Maksimovic, 2004). These information frictions give incumbent banks monopoly power over their captive borrowers, allowing lenders to charge higher interest rates, making smaller firms particularly vulnerable to excessive market power. Hence, knowing if the effects of the policy on private banks' interest rates vary for firms of different size is informative about the effects of an increase in lending by state-owned banks on credit availability for SMEs.

To understand to what extent the effect of the policy is heterogeneous across firm size, we estimate a dynamic triple DiD specification, focusing on how the drop in the private-public interest spread interacts with firm size. This dynamic specification is also useful to assess if average interest rates for firms of different size were falling at a different pace before the intervention. Specifically, we estimate:

$$i_{jtmbfs} = \alpha_{tmsf(size)} + \alpha_b + \sum_{\tau \neq 0} \delta_\tau \; Private_b^\tau + \sum_{\tau \neq 0} \beta_\tau \; Private_b^\tau \times I_f^{Size} + \varepsilon_{jtmbfs} \tag{3}$$

We estimate the regression above three times, each time comparing micro firms (baseline group) with small/medium/large firms, one group at a time. I_f^{Size} equals 1 for small/medium/large firms in each corresponding specification. The first set of coefficients of interest, $\{\delta_\tau\}_{\tau \neq 0}$, capture the change in the difference in interest rates of private and public banks for the baseline group (micro firms) in period τ relative to March 2012. The second set of coefficients of interest, $\{\beta_\tau\}_{\tau \neq 0}$, capture the differential change in the difference between interest rates of public and private banks for small/medium/large firms relative to micro firms in a given period τ. We include the same set of fixed effects as in our baseline specification from equation 1.

The results in Figure 4 indicate that the drop in interest rates of private banks *decreases with firm size*, with very little variation in interest rates of medium and large firms after the intervention. This indicates larger firms benefit less from the competition shock caused by the policy. It is worth noting that the difference between private and public bank interest rates is falling faster for micro firms relative to other groups in the first half of 2011. However, that trajectory stabilizes from August 2011 onwards, which reduces concerns that violations of parallel are biasing our results.

We gauge the magnitude of this heterogeneous effect of the policy on private bank loan interest rates across firms of different sizes during the policy by estimating the following triple DiD specification:

$$
\begin{aligned}
i_{jtmbfs} = \alpha_{tmsf(size)} + \alpha_b + \beta_1 \times Post_t \times Private_b \\
+ \sum_{Size=Sm,Md,Lg} \beta_2^{type} \times Post_t \times D_f^{Size} \times Private_b + \varepsilon_{jtmbfs}
\end{aligned}
\tag{4}
$$

Where D_f^{Size} is a categorical variable for each firm size among Micro, Small, Medium and Large (with Micro firms being the reference size). $Post_t$ is a dummy which equals 1 if t is a month after March 2012. $Private_b$ is a dummy which equals 1 if bank b is a private bank. The coefficients of interest reflect the change in the difference between interest rates of public and private banks for the baseline group (micro firms), and the differential changes experienced by small/medium/large firms. The results for the coefficients in the post period are shown in Table 4. Micro firms benefit the most from the reduction in interest rates. Across all specifications, the reduction in private interest rates for smaller firms is roughly two thirds of the reduction experienced by micro firms. For medium and large firms, the drop in loan interest rates is about one third of the drop experienced by micro firms.

It is worth considering the implications of these results for the determinants of the spread between public and private banks in Brazil. While both market power and risk can explain larger interest rates for smaller firms, a larger reduction in interest rates for riskier firms suggests market power accounts for an important part of the spread charged by private banks when lending to SMEs. This also indicates that implementing anti-trust policy with the use of government banks can be successful at improving lending conditions to firms that face stricter barriers to credit access.

IV.2. Firm Debt

The large increase in lending by public banks was not followed by severe crowding out of private credit, and most loans issued by public banks went to borrowers that already had access to credit. Thus, the intervention should increase leverage if firms do not use newly acquired loans to pay off legacy debt. In this part of the analysis, we shift our focus to public bank borrowers, which experienced a larger increase in loan availability. Since we do not have balance sheet information for firms, we use total payroll costs as a measure of firm size. We define debt-to-payroll costs as a firm's outstanding debt divided by its payroll costs in 2011, and use it as our proxy for firm leverage.[14] We include all types of debt outstanding to capture substitution between working capital loans and other types of legacy debt. We then estimate a difference-in-differences specification to understand how the debt-to-payroll ratio of borrowers from public banks changes relative to that of borrowers from private banks:

$$\frac{\text{Debt}_{tf}}{\text{Payroll}_{2011,f}} = \alpha_{tmsf(size)} + \alpha_f + \sum_{\tau \neq 0} \gamma_\tau \cdot Public_f^\tau + \varepsilon_{tf}, \tag{5}$$

where the dependent variable is the outstanding debt of firm f in month t relative to its total payroll in 2011, our leverage proxy. $\alpha_{tmsf(size)}$ are time-municipality-industry-industry fixed effects, $Public_f^\tau$ is a dummy which equals 1 in month τ if firm f borrows exclusively from public banks prior to the intervention, and γ_τ are the coefficients of interest. We estimate Equation (5) for the subset of firms with exclusive relationships with private and public banks in the pre-intervention period.

The results are shown in Figure 5. The increase in funding availability caused by the intervention has a remarkable effect on the debt of firms with an exclusive relationship with public banks, relative to firms with exclusive relationships with private banks. From June 2012 onward, there is a sharp increase in leverage of public bank borrowers relative to private bank borrowers. The point estimate in December 2013 indicates that the debt over payroll ratio of firms borrowing from public banks increased 0.55 their relative to firms who borrowed exclusively from private banks. Average debt-over-initial payroll costs for public borrowers was around 1.44 in March 2012, so this relative increase is close to 40% of public borrowers' leverage at the onset of the policy. Moreover, between March 2012 and December 2013, leverage of public bank borrowers increased by 76%, compared with a leverage growth of 27% for private bank borrowers. Hence, the percentage growth in public borrower leverage was more than two times the percentage growth

[14] A similar scaling of debt at the individual level in the context of household debt is used by Garber et al. (2024).

in private borrower leverage. For reference, before the intervention, both public and private bank borrowers experience an average monthly growth of around 1% (in nominal terms) in their leverage. After the intervention, private bank borrowers experience a nearly identical increase of 1.16% per month on average, whereas public bank borrowers experience a monthly leverage growth of 2.73% on average. Such large increase in public bank borrowers' leverage can have an effect on firms' ability to repay their loans, which we turn to next.

IV.3. Borrower Risk and Default

Changes in loan interest rates and leverage brought about by the policy can affect loan default risk. First, the large increase in leverage could lead to higher borrower default for public banks, as higher leverage is associated with higher default rates (Traczynski, 2017). Second, public banks might attract riskier borrowers, which would lead to a deterioration in the quality of government banks' loan portfolio (Tirole, 2012; Jiménez et al., 2019). Third, a direct link between interest rates and borrower risk, such as in adverse selection and moral hazard models (Stiglitz and Weiss, 1981; Martinez-Miera and Repullo, 2010; Crawford, Pavanini and Schivardi, 2018), can also lead to changes in borrower default relative to the beginning of the policy, following the large drop in private banks' loan interest rates.

In all cases one would expect default rates of public bank borrowers to become relatively larger than default rates of private bank borrowers following the intervention. However, these channels have different implications for default rates of levered and unlevered borrowers.[15] First, if the leverage buildup caused by the intervention leads to higher default rates, risk differences between public and private banks will be concentrated among *levered* firms. On the other hand, if public banks systematically attract worse quality borrowers, or if the reduction in private loan interest rates lower default probability of private bank borrowers relative to public bank borrowers, differences in default occur equally for existing and new, unlevered borrowers. Hence, analyzing lev-

[15]In our sample the vast majority of unlevered borrowers are new borrowers, which did not borrow before the policy.

ered and unlevered firms separately is informative about the overall mechanism affecting default rates. We define levered firms as those that have positive debt in the month prior to loan origination.

We start by comparing the average delinquency rate over time for private and government banks separately. We say that a firm that borrowed in a given month from a certain bank was *delinquent* if any of the loans from that bank to that firm in that month became delinquent for more than 90 days within a year after origination. If the firm failed to pay its loan installments for at least 90 days within this one year window, we define the firm as delinquent on loans it contracted in May 2012. Using this delinquency measure, we estimate the following specification at the firm-bank level:

$$D_{tmbfs} = \alpha_{msf(size)} + \alpha_b + \sum_{\tau \neq -1} \gamma_\tau + \varepsilon_{tmbfs}, \tag{6}$$

where D_{tmbfs} is an indicator equal to one if a loan originated in month t in municipality m from bank b to firm f in industry s becomes delinquent within one year after origination, $\alpha_{msf(size)}$ are municipality-industry fixed effects, α_b are bank fixed effects, and γ_τ are unconditional time dummies that equal 1 for each month τ. Each γ_τ indicates the average change in delinquency probability in a given month relative to March 2012. The inclusion of a broad range of fixed effects allows us to compare similar borrowers, so that differences in observed delinquency rates are not due to market segmentation or differential regional or al exposure. However, we *do not* include firm fixed effects, which would drop firms that show up only once in our sample. This is because firms that default on a loan are less likely to borrow from any banks that have access to that information. Hence, if a new borrower defaults on a loan in the beginning of our sample, it is unlikely that such a borrower would show up again in the later part of the sample. In contrast, *all new borrowers* who show up only once in the latter part of the sample would be excluded, irrespective of their loan performance. This would introduce a dynamic bias, since a smaller share of loans in the earlier part of the sample would be marked as delinquent.

Since we estimate Equation 6 separately for government banks and for private banks, we obtain two sets of $\{\gamma_\tau\}_{\tau \neq -1}$ for each subset of firms, which we report in Figure 6.

Both for levered and unlevered borrowers, public and private banks have very similar delinquency trajectories relative to March 2012 before the intervention. However, after the intervention, government banks experienced a deterioration of their loan portfolio. In contrast, the delinquency rate on private banks' loans initially improves and eventually goes back to its pre-intervention level. Such large differences in default stem from loans to levered firms. Figure 6, Panel (b) shows that levered borrowers from government banks became delinquent more often than firms that borrowed from private banks during the intervention. In contrast, Figure 6, Panel (c), shows that new borrowers of public and private banks had comparable risk, both before and during the intervention.

This trajectory can also be observed in a dynamic difference-in-differences specification where we estimate Equation (6) for both types of banks jointly with time-municipality-industry-size fixed effects. The results of this dynamic difference-in-differences specification are shown in Figure B.7, where we plot coefficients for the $Public_b$ dummy variable, measuring differences in delinquency for public banks. Public delinquency starts to detach from private delinquency around August 2012, more or less the same time as when public borrowers experience a large increase in leverage, as shown in Figure 5. This indicates that the leverage buildup experienced by public bank borrowers after the intervention is connected to the increase in default rates experienced by these banks.

To quantify the differential effects on delinquency rates of public banks identified in the dynamic specifications, we estimate a standard difference in differences regression:

$$D_{tmbfs} = \alpha_{tmsf(size)} + \alpha_b + \beta Post_t \times Public_b + \varepsilon_{tmbfs}, \tag{7}$$

Where fixed effects and other variables are defined as above. We also augment Equation 7 with a triple differences specification capturing the differential effect between levered and unlevered firms. The results are shown in Table 5. Columns (1) and (2) indicate that firms that borrow from public banks have a one p.p. larger probability of default than firms that borrow from private banks, on average, after the intervention. The coefficient on $Public$ in column (1) shows that public banks experience lower default rates before the intervention for otherwise comparable borrowers. Moreover, column (3) confirms that accounting for debt is fundamental when exploring differences in ex-post riskiness of public and private bank borrowers. Public loans to levered firms are nearly 2 p.p. more likely to result in default when compared to private loans to levered firms after the intervention. In contrast, public loans to unlevered firms, most of which are new borrowers, have comparatively *lower* default rates.

To better understand the extent to which *higher* leverage explains the observed differences in default between public and private banks, we zoom in on the subset of levered firms, divide such firms into population quintiles based on their lagged leverage, and estimate the following regression:

$$D_{tmbfs} = \alpha_{mts} + \alpha_b + \alpha_{t,f(size)} + \sum_{l>1} \beta_l \times Q_f^l + \varepsilon_{tmbfs}, \tag{8}$$

where Q_f^l are indicator variables equal to one if firm f belongs to the $l-th$ quintile. The coefficients β_l in Equation 8 capture the average delinquency difference between firms in the bottom quintile and those in the upper quintiles of the debt-to-payroll-ratio distribution. We restrict this analysis to firms with positive leverage so that our results are not contaminated by differences in riskiness of levered and unlevered firms, and focus on the post intervention period. The results are shown in Table 6. Delinquency increases monotonically in leverage quintiles, and this effect is larger for government banks. Public bank borrowers in the top leverage quintile are around 8 p.p. more likely to default compared to firms at the bottom leverage quintile. This is 2.5 p.p. higher than the difference between default rates of firms in the top and bottom leverage quintile borrowing from private banks. Hence, differences in default between public and private banks are sensitive to the level of firms' debt-to-initial payroll ratio.

IV.4. Economic Mechanism

The joint analysis of interest rate decisions and default allows us to shed light on the underlying mechanism driving the response of private banks to the policy-driven increase in credit by public banks. We find little evidence that public banks "clearing" the market of its worse assets explains why private banks provide cheaper lending. Even though public loans to unlevered firms experience similar, if not better, quality, compared with private loans to levered firms. Appendix B Table B.1 shows that the interest rates of private bank loans to unlevered borrowers also falls after the policy. Thus, the response of private banks comes from a competitive effect, caused by broader availability public credit after the policy. Moreover, differences in default between public and private banks are tightly connected to firm leverage. The results from the analysis of delinquency rates suggests commercial state-owned banks are not worse at screening compared with private banks. To the extent that the costs of direct government lending during crisis are larger when the government cannot distinguish good project from bad projects (Bebchuk and Goldstein, 2011), our findings provide further support in favor of the use of government banks to increase lending during economic downturns.

Our results also shed light about the conditions under are these mechanisms are more likely to generalize to other contexts. First, the intervention imposed few requirements for firms interested in applying for government loans. Broader availability of credit from commercial public banks to private bank borrowers increases the competitive pressure private banks would face. At the same time, lack of restrictions on loan size relative to firm size can increase the cost of these interventions, as leverage increases the likelihood of default (Traczynski, 2017). Taken together, these findings suggest that the dominant mechanism behind the reduction in private bank interest rates is competitive pressure from expanded public bank lending, rather than shifts in borrower risk. However, the increase in defaults among more levered borrowers highlights that the policy's benefits may occur in the short run, while the costs materialize later through rising credit risk.

V. Regional Competition and the Effects of the Intervention

We next examine the policy's effects at the regional level, focusing on how exposure to the expansion of public lending impacted credit and real outcomes. We consider a municipality to be our benchmark local banking market. We restrict the sample to those municipalities with at least one bank branch before the policy. These include markets with only private banks, only public banks, or both. The final sample includes around 2,800 municipalities, accounting for over 93% of Brazil's economic activity and employment. We define public credit origination in a given market as $Credit_{tm}^{Orig,Pub} \equiv \frac{Working\ Capital_{tm}^{Pub}}{Total\ Cred_m^{Pub,2011} + Total\ Cred_m^{Pr,2011}}$, where $Working\ Capital_{tm}^{Pub}$ denotes the amount of working capital loans originated in municipality m in period t, and $Total\ Cred_m^{Pub,2011}$ and $Total\ Cred_m^{Pr,2011}$ denote the average amount of SME credit *outstanding* across 2011, our baseline year. Thus defined, $Credit_{tm}^{Orig,Pub}$ captures the magnitude of the increase in public credit in a given period, relative to the size of the firm credit market in a given municipality in the year prior to the intervention.

Our objective is to understand how changes in $Credit_{tm}^{Orig,Pub}$ led to changes in credit outcomes such as private interest rates and firm leverage, and real outcomes such as local output and employment. However, directly estimating how changes in credit affect the outcomes of interest via OLS would be plagued with endogeneity. For example, demand for bank credit could be larger in areas with more productive investment opportunities, generating a positive correlation between state-owned bank credit growth and local employment and lead to a positive bias in the estimated impact of public bank lending and real outcomes.

Since the policy made use of the largest state-owned commercial banks to increase lending, we conjecture that markets where these banks have a larger footprint experience larger credit growth during the intervention. Government banks would have a larger operational capacity relative to market size in these areas, allowing them to increase credit supply at a faster pace. Thus, we measure exposure to the policy by calculating state-owned banks' market share in firm credit in a given municipality in 2010, two years prior to the intervention. Specifically, we use the share of total firm credit outstanding at monthly frequency and average across all months in 2010 to obtain the representative share. Using this measure we estimate the following DiD instrumental variable (DiD IV) approach:

$$y_{tm} = \alpha_{tr} + \alpha_m + \beta Credit_{tm}^{Orig,Pub} + \Gamma X_{tm} + \varepsilon_{tm} \tag{9}$$

$$Credit_{tm}^{Orig,Pub} = \alpha_{tr} + \alpha_m + \delta Post_t \times Share_m^{Pub,2010} + \Gamma X_{tm}, \tag{10}$$

Where y_{tm} denotes the dependent variable of interest, $Share_m^{Pub,2010}$ is the market share of public banks in firm lending in municipality m in 2010, $Post_t$ equals 1 after the intervention, α_{tr} and α_m are time-region and municipality fixed effects, respectively. X_{tm} is a set of time-varying municipality level controls. β can be interpreted as an average causal response (ACR), which measures a weighted average of causal responses of credit and real outcomes to a one unit increase in $Credit_{tm}^{Orig,Pub}$, our measure of public credit origination (Hudson, Hull and Liebersohn, 2017). We use exposure to public banks' market share $Share_m^{Pub,2010}$ interacted with our post-policy dummy $Post_t$ as an instrument for credit origination $Credit_{tm}^{Orig,Pub}$. We cluster our standard errors at the municipality level and weight the regression by population size in 2011, the baseline year, to estimate aggregate effects on local output and employment.[16]

The core identification assumption is that public banks' market share in firm credit in 2010 is not systematically correlated with local demand shocks that would occur at the same time as the policy. We assess this assumption in several ways. First, the use of market shares two years prior to the implementation of the policy alleviates concerns that the exposure measure is correlated with other shocks taking place in 2011. Second, we estimate a dynamic DiD using $Share_m^{Pub,2010}$ as our measure of treatment intensity, to assess how credit and real outcomes vary over time in areas with higher exposure to public banks, as captured by $Share_m^{Pub,2010}$:

$$y_{tm} = \alpha_{tr} + \alpha_m + \sum_{\tau \neq 0} \gamma_\tau \times Share_m^{Pub,2010} + \Gamma X_{tm} + \varepsilon_{tm} \tag{11}$$

Where all variables are defined as before. We include lagged employment growth, and lagged growth in the individual components of local economic activity, including the value added of industry, services, agriculture, and public administration as controls X_{tm}. The inclusion of region by time fixed effects in all our specifications accounts for any time-varying local economic shocks that would affect municipalities in the same region and simultaneously affect credit and real outcomes. Finally, the covariate balance in Figure 7 shows that the correlation between normalized values of our regional instrument and

[16] We express both $Credit_{tm}^{Orig,Pub}$ and $Share_m^{Pub,2010}$ in decimal units rather than as percentage points to facilitate visualization of point estimates.

several municipality characteristics is very small once we compare municipalities within the same region. The conditional correlations are smaller than 0.2, minimizing concerns that our instrument suffers from endogeneity issues.

V.1. Credit Outcomes

The first set of tests concerns the validity of our instrument in instrumenting for changes in credit growth, and potential endogeneity issues that would manifest in violations of parallel trends for different dependent variables. We perform the regional analysis of credit variables at quarterly frequency to avoid excess variability for municipalities with infrequent loan origination.

We first set $y = Credit_{tm}^{Orig,Pub}$ to establish the relevance of our instrument, and estimate Equation 11. Figure 8 shows a sharp increase in credit supply by public banks after the policy is announced at the end of March 2012, which persists throughout 2012 and 2013. The point estimates suggest that one percentage point larger government banks' market share in 2010 leads to around 13% larger quarter credit growth in a given municipality, on average, after the intervention. This implies that a municipality at the top quartile of the market share distribution ($Share_m^{Pub,2010} \sim 65\%$) experiences working capital lending growth roughly five times as large as a municipality at the bottom quartile of the market share distribution ($Share_m^{Pub,2010} \sim 30\%$). This strong effect confirms our conjecture that relative exposure to public banks prior to the intervention is a strong predictor of which areas are more affected by the intervention.[17] Moreover, the lack of previous trends in public credit origination reduces concerns that the instrument is correlated with unobserved time-varying demand for credit.

We then turn to quantifying the impact of the increase in public lending in a given market on other credit outcomes. We estimate equation 11 with the dependent variable y_{tm} equal to our credit variables of interest, namely, private banks' interest rates and firm leverage. Figure 9 shows that private banks lower interest rates relatively more in municipalities more exposed to public banks after the intervention, in line with the idea that

[17] Appendix B Figure B.8 shows that the increase in credit caused by the policy monotonically increase with public bank market shares.

added competition from public banks caused a drop in private banks' loan interest rates. Similarly, firms located in markets more exposed to the policy experience a larger increase in leverage compared to firms in markets with less exposure to state-owned banks.

Next, we measure by how much additional public credit origination in a given market leads to a drop an private interest rates and an increase in firm leverage by estimating Equation 9. The results in Table 7 indicate that an increase public credit origination relative to total firm credit in 2011 in a given quarter by 0.01 leads to an increase in debt over payroll of around 1.2, which is 60% the the weighted average leverage across municipalities in 2011. The same increase of 1 p.p. in quarterly public credit origination relative to total firm credit in 2011 leads to a drop in private banks' interest rates of 28 basis points.[18] For reference, the weighted average origination growth in our sample is roughly 1% before and 6% percent after the intervention. This 5 p.p. increase in quarterly credit growth would translate into an average 1.5 p.p. drop in interest rates, between one fourth and half of the aggregate drop in private interest rates documented in Table 3.

V.2. Real Effects

Financial frictions that motivate the use of the credit policies have implications for economic development and growth. Hence, to assess the overall effectiveness of the use of state-owned commercial banks to address credit barriers, we now turn to the potential real effects of the policy, focusing on local output and employment.

In the context of government bank lending, there are two channels through which the policy can have real effects by facilitating access to credit. First, the policy entail a sudden increases in public credit supply at low interest rates, allowing public borrowers to increase their leverage. Moreover, this increase in public lending causes a reduction in private banks' loan interest rates, reducing the cost of loans for private bank borrowers. Hence, both channels through which the policy can ease financial constraints of SMEs are captured by the regional instrument.

To assess whether or not the increase in the supply of state-owned banks SME loans led

[18]The number of observations varies across different dependent variables since private bank interest rates are often not available in most public monopolies, as private banks do not lend in these markets.

to an increase in economic activity, we first assess how higher exposure to public banks is associated with output and employment over time. We estimate Equation 11 using local output and employment as dependent variables y_t. Since local employment and output are observed at yearly frequency in our data, we aggregate credit origination for each year, so that $Credit_{tm}^{Orig,Pub}$ represents the annual increase in credit origination relative to the baseline year of 2011. Moreover, we further define *tradable* and *non-tradable* and estimate equations 11 and 9 to understand the relative importance of local demand to explain our employment effects, as in Mian and Sufi (2014) and Fonseca and Matray (2024). We include observations for 2010 when estimating equation 11 to assess the existence of pre-trends linking public banks' market share with differential economic growth in 2010.

The results in Table 8 show β estimates capturing differential output and employment growth relative to 2011. There is no evidence of violation of parallel trends assumption, with all variables exhibiting similar trajectory regardless of treatment exposure relative to the baseline year before the intervention takes place. While there is no noticeable effect on local output, the credit supply shock has a positive impact on employment, including employment in tradable and non-tradable s, which seems to intensify in 2013.

To obtain point estimates for the elasticity of employment with respect to local public lending, we estimate the 2SLS specification from Equation 9 with the same set of dependent variables, instrumenting for changes in public credit growth with the use of public banks' local SME lending market share in 2010, $Share_m^{Pub,2010}$ interacted with the post-treatment dummy $Post_t$. The results are shown in Table 9. A one p.p. increase in total credit at the market level in a given year leads to an increase of 15 basis points in total employment. Moreover, this effect is stronger for tradable employment, with a 1 percentage point increase in total credit at the market level leading to a 0.3% increase in tradables employment and 0.06% for non-tradables employment. This larger effect of public credit growth on employment in tradable relative to non-tradable s suggests our estimates capture supply side effects, rather than solely local demand effects. Thus, our results do not merely reflect other economic policies that bolstered demand, such as the increase in household credit by public banks around the same time of the policy studied by Garber et al. (2024).

Robustness and Discussion of the Results Our approach to controlling for regional demand effects consists of comparing municipalities which are located close to each other. There are different types of region with varying degrees of granularity which can be used to impose comparisons among firms exposed to similar local shocks. Appendix B Figure B.9 shows that the dynamic results for our results for credit outcomes are robust to the use of state or micro region as our definition of region.[19] Moreover, Appendix B Table B.2 shows that using both state (Panel A) or micro-region (Panel B) as the relevant regional control delivers estimates which are very close to the baseline effects showed on 9. This indicates that the choice of regional granularity we impose when comparing outcomes across municipalities has little effect on our results.

The literature studying differences-in-differences estimators suggests caution in contexts where heterogeneity of treatment effects might be present (de Chaisemartin and D'haultfoeuille, 2020). In our context this could arise as a result of heterogeneity in firm population across municipalities, with variation in the degree of financial constraints faced by firms across different markets. We investigate the possibility of null treatment effects in our real outcomes regressions by estimating the first stage of our instrumental variables approach, which consists of regressing $Credit_{tm}^{Orig,Pub}$ on the interaction $Share_m^{Pub,2010} \times Post_t$. We then use the predicted values of $Credit_{tm}^{Orig,Pub}$ to estimate the weights associated with our DiD specification, following de Chaisemartin and D'haultfoeuille (2020). Around 46% of the weights are negative, and the negative weights sum up to about 0.7. Moreover, the standard deviation of weights that would result in zero ATE equals 0.045.

To understand the magnitude of this estimate, consider the circumstances under which an increase in public credit would lead to *negative* employment. Such negative employment effects might be caused by crowding out of credit from private borrowers to public borrowers. For example, Ru (2018) finds that firms crowded out by loans from a development state-owned bank to target firms in the same industry experience a 1.7% drop in employment for an increase of 100% increase in credit by the government bank. To place

[19] In our sample, the median meso-region (our preferred regional variable) has 16 municipalities. The median state has around 144 municipalities, and the median micro-region, the most granular subregion, has 5.

this in our context, let us assume that municipality level treatment effects are normally distributed around zero. A standard deviation of 0.045 implies that the mean treatment effect, conditional on negative values, equals 0.038, or a 3.8% decrease in employment for a 100% increase in $Credit_{tm}^{Orig,Pub}$. Hence, one would need a negative crowding out driven effect which is *more than* twice as large as documented by the literature. Furthermore, to obtain negative effects *at the market level*, one would also need to account for the positive effect of additional credit for public bank borrowers in the same market. All in all, this provides support to our hypothesis that the average treatment effect of the policy on local employment is positive.

A more general concern is that our instrument correlates with unobserved degree of financial frictions experienced by firms. Indeed, Figure 7 displays a negative correlation between the market share of public banks and the average firm size in each municipality (as measured by the number of employees). To the extent smaller firms experience tighter financial constraints, treatment intensity would correlate positively with underlying treatment effects, and we could be *overestimating* the real effects of the policy.

We gauge the sensitivity of our results to the negative correlation between our instrument and average firm size by re-estimating equation 9, adding a control for the share of firms in a given municipality in 2011 which are micro firms ($Share_m^{Micro,2011}$) interacted with the post dummy ($Post_t$). Table B.3 in Appendix B shows the results. The point estimates are marginally smaller and broadly aligned with our baseline specification. The coefficient on total employment indicates that a 1 percentage point increase in total credit at the municipality level in a given year leads to an increase of 14 basis points in total employment in that municipality, very close to our baseline effect of 15 basis points increase in employment. This further supports to our conjecture that heterogeneity in average firm size is likely not a major source of bias to our estimates.

VI. Policy Implications

To put the impact of the policy into a cost-benefit perspective, we calculate the cost per job created. This allows us to compare our the policy we study with interventions such as loan guarantees, which also aim to improve SME credit access.

Public banks faced increased loan default risk caused by the increase in leverage resulting from credit expansion, which represents the main cost of the policy. We assume that in the counterfactual where the policy is not implemented, the difference between delinquency rates of public and private banks would have remained at pre-intervention levels. Hence, we measure policy costs by multiplying the identified increase in public bank delinquency rates, relative to private banks, by the total supply of working capital loans by public banks, accounting also for loss of interest rate revenue:

$$Cost = \hat{\beta}^{D} \sum_{t \in T_{Post}} \sum_{m} Working\ Capital_{tm}^{Pub} \times (1 + \overline{Interest\ Rate^{Post}})^{2} \tag{12}$$

Where $\hat{\beta}^{D}$ is the estimate of the interaction term $Public \times Post$ in Column 1 of Table 5, $Working\ Capital_{tm}^{Pub}$ denotes the total amount of working capital loans originated in municipality m during month t. $\overline{Interest\ Rate^{Post}}$ denotes the average interest rate on public bank working capital loans between April 2012 and December 2013. We compound this annual rate for two years since average loan maturity for public banks is 24 months. This leads to a total cost of around R$ 900 million for the policy.

To calculate the number of jobs created, we multiply our estimated elasticity of local employment with respect to public credit by the average municipality level employment at the end of 2011, times the increase in credit origination after the policy:

$$\Delta Emp = \hat{\beta}^{Emp} \sum_{m} Emp_{m}^{2011} \times \left(\overline{Credit}_{m}^{Orig,Post} - \overline{Credit}_{m}^{Orig,Pre} \right) \tag{13}$$

Emp_{m}^{2011} denotes total employment in municipality m at the end of 2011, the baseline year. $\overline{Credit}_{m}^{Orig,Pre}$ and $\overline{Credit}_{m}^{Orig,Post}$ denote the average yearly origination credit growth in 2012 and 2013, in percentage points. $\hat{\beta}^{Emp}$ corresponds to estimated effect of one p.p. additional credit origination in a given year on total employment (column 4 in Table 9). This delivers a total of 452,000 jobs created, which, combined with our cost estimates, corresponds to an average cost per job created of around R$ 1,987.00.

To make such estimates comparable with policies implemented in other countries, we normalize our cost per job estimates using the prevailing monthly minimum wage in each country at the time policies were implemented. Such normalization helps us understand how cost per job estimates compare to the *actual* cost of creating a job in any given country, while also accounting for exchange rate differences. Table 10 places our estimated cost per job relative to three credit guarantee programs implemented worldwide.

In 2012 and 2013 the minimum wage per month was R$ 622-678 in Brazil, leading to a cost per job of around 3 times the monthly minimum wage. This normalized cost per job is smaller to the cost per job of €5,784 in recovery years estimated by Bonfim, Custódio and Raposo (2023), which corresponds to 11 times the monthly minimum wage in Portugal. They also estimate a larger cost per job of €11,788 during crisis years, which is 26 times the monthly minimum wage in crisis years. All of these estimates are considerably larger than the cost per job ranging between €425-€1,400 found by Barrot et al. (2024) for a loan guarantee program in France, which corresponds roughly to around 0.4-1.4 times the French monthly minimum wage at the time. Nonetheless, our estimate is considerably smaller than the cost per job of $21,580 to $25,450 found by Brown and Earle (2017) for SBA loans. This translates into a normalized cost per job between 19 to 22.4 times the monthly minimum wage in the US.[20] These numbers suggest the benefits of the policy were achieved at a relatively low cost when compared with other policies.

Importantly, our cost-per-job estimate reflects only short-term effects. It does not account for potential misallocation, which prior work links to state-owned bank lending and which could carry long-run efficiency costs (Carvalho, 2014). Higher default rates also raise concerns about the sustainability of public credit expansion, especially during Brazil's subsequent 2015–2016 fiscal crisis. Moreover, increased firm leverage could amplify downturns, much like the adverse effects of household leverage documented in Garber et al. (2024). These considerations are not captured in our estimates and caution against interpreting low short-term costs as evidence that interventions using public banks as a competition tool are unambiguously beneficial.

[20] We calculate a representative monthly minimum wage in the United States, defined as the average minimum wage across states, weighted by total employment in each state.

External Validity Our findings are most relevant for highly concentrated markets with large loan spreads. In more competitive banking sectors, private banks have less room to reduce rates without incurring losses, which would dampen the impact of public lending. The intervention also occurred outside of a crisis, and the competitive benefits and the magnitude of the costs may differ during crises. Another key factor is that both BB and CEF are large banks, able to expand credit substantially. Their prior experience in SME lending suggests suitable screening and monitoring abilities, enabling them to attract relatively safe unlevered borrowers. By contrast, using public banks as a competition tool is less feasible where such banks are smaller or specialize in other loan types.

VII. Conclusion

In this paper we study a credit market intervention in Brazil that used commercial state-owned banks to address excessive market power. The policy was characterized by a large and unexpected increase in the supply of credit to firms at low interest rates, and implemented in a period where no other large shocks took place. The combination of this unique quasi-experiment and the availability of detailed data allow us to jointly analyze the effectiveness of the intervention at affecting private bank loan interest rates, along with its effects in firm debt and default risk. Moreover, we quantify the effects of public credit expansion in different markets on credit and real outcomes.

We show that the intervention was successful in prompting a reduction in private loan interest rates, with the largest effects for smaller firms. On the cost side, the intervention led to an increase in firm leverage and weakened the quality of public banks' loan portfolio. Nonetheless, the increase in public credit generated job creation, with the estimated cost per job comparing favorably to other interventions. The broader implication is that state-owned banks can be deployed as tools of competition policy in the short term. Policymakers should weigh the benefits of lower SME borrowing costs and employment creation against the risks of excessive leverage and potential misallocation. Our results provide benchmark estimates of this trade-off to inform similar interventions in emerging markets.

References

Assuncao, Juliano, Sergey Mityakov, and Robert Townsend. 2012. "Ownership Matters: the Geographical Dynamics of BAAC and Commercial Banks in Thailand."

Ayyagari, Meghana, Thorsten Beck, and Asli Demirguc-Kunt. 2007. "Small and Medium Enterprises Across the Globe." *Small Business Economics*, 29: 415–434.

Bachas, Natalie, Olivia S Kim, and Constantine Yannelis. 2020. "Loan Guarantees and Credit Supply." *Journal of Financial Economics*, 139: 872–894.

Barrot, Jean-Noel, Thorsten Martin, Julien Sauvagnat, and Boris Vallee. 2024. "The Labor Market Effects of Loan Guarantee Programs." *Review of Financial Studies*, 37: 2315–2354.

Bebchuk, Lucian A., and Itay Goldstein. 2011. "Self-fulfilling Credit Market Freezes." *Review of Financial Studies*, 24: 3519–3555.

Beck, Thorsten, Asli Demirgüç-Kunt, and Vojislav Maksimovic. 2004. "Bank Competition and Access to Finance: International Evidence." *Journal of Money, Credit and Banking*, 36: 627–648.

Bertay, Ata Can, Asli Demirgüç-Kunt, and Harry Huizinga. 2015. "Bank Ownership and Credit Over the Business cycle: Is Lending by State Banks Less Procyclical?" *Journal of Banking and Finance*, 50: 326–339.

Bonfim, Diana, Cláudia Custódio, and Clara Raposo. 2023. "Supporting Small Firms Through Recessions and Recoveries." *Journal of Financial Economics*, 147: 658–688.

Brown, J. David, and John S. Earle. 2017. "Finance and Growth at the Firm Level: Evidence from SBA Loans." *Journal of Finance*, 72: 1039–1080.

Cao, Yiming, Raymond Fisman, Hui Lin, and Yongxiang Wang. 2022. "SOEs and Soft Incentive Constraints in State Bank Lending." *American Economic Journal: Economic Policy*, 15: 174–195.

Capeleti, Paulo, Marcio Garcia, and Fabio M. Sanches. 2022. "Countercyclical Credit Policies and Banking Concentration: Evidence from Brazil." *Journal of Banking and Finance*, 143: 106589.

Carvalho, Daniel. 2014. "The Real Effects of Government-Owned Banks: Evidence from an Emerging Market." *Journal of Finance*, 69: 577–609.

Cherry, Susan. 2024. "Regulating Credit: Effects on Market Structure, Lender Technologies, and Credit Access."

Coelho, Christiano A., João M. P. De Mello, and Bruno Funchal. 2012. "The Brazilian Payroll Lending Experiment." *Review of Economics and Statistics*, 94: 925–934.

Coleman, Nicholas, and Leo Feler. 2015. "Bank Ownership, Lending, and Local Economic Performance During the 2008–2009 Financial Crisis." *Journal of Monetary Economics*, 71: 50–66.

Cole, Shawn. 2009. "Fixing Market Failures or Fixing Elections? Agricultural Credit in India." *American Economic Journal: Applied Economics*, 1: 219–250.

Cortes, Gustavo S., Christiano Silva, and Bernardus van Doornik. 2019. "Credit Shock Propagation in Firm Networks: Evidence From Government Bank Credit Expansions."

Crawford, Gregory S., Nicola Pavanini, and Fabiano Schivardi. 2018. "Asymmetric Information and Imperfect Competition in Lending Markets." *American Economic Review*, 108: 1659–1701.

Crouzet, Nicolas, and Fabrice Tourre. 2021. "Can the Cure Kill the Patient? Corporate Credit Interventions and Debt Overhang."

Cuesta, Jose Ignacio, and Alberto Sepulveda. 2018. "Price Regulation in Credit Markets: A Trade-off between Consumer Protection and Credit Access."

de Blasio, Guido, Stefania De Mitri, Alessio D'Ignazio, Paolo F Russo, and Lavinia Stoppani. 2018. "Public Guarantees to SME Borrowing. A RDD Evaluation." *Journal of Banking and Finance*, 96: 73–86.

de Chaisemartin, Clément, and Xavier D'haultfoeuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review*, 110: 2964–96.

de Haas, Ralph, and Juanita Gonzales-Uribe. 2024. "Public Policies for Private Finance."

Dinç, I. Serdar. 2005. "Politicians and Banks: Political Influences on Government-Owned Banks in Emerging Markets." *Journal of Financial Economics*, 77: 453–479.

Fonseca, Julia, and Adrien Matray. 2024. "Financial Inclusion, Economic Development, and Inequality: Evidence from Brazil." *Journal of Financial Economics*, 156: 103854.

Garber, Gabriel, Atif Mian, Jacopo Ponticelli, and Amir Sufi. 2024. "Consumption Smoothing or Consumption Binging? The Effects of Government-led Consumer Credit Expansion in Brazil." *Journal of Financial Economics*, 156: 103834.

Hudson, Sally, Peter Hull, and Jack Liebersohn. 2017. "Interpreting Instrumented Difference-in-Differences." *MIT Mimeo*.

Jiménez, Gabriel, José-Luis Peydró, Rafael Repullo, and Jesús Saurina. 2019. "Burning Money? Government Lending in a Credit Crunch."

Joaquim, Gustavo, Bernardus van Doornik, and José Renato Ornelas. 2023. "Bank Competition, Cost of Credit and Economic Activity: Evidence from Brazil."

Kumar, Nitish. 2020. "Political Interference and Crowding Out in Bank Lending." *Journal of Financial Intermediation*, 43: 100815.

Kuroishi, Yusuke, Cameron LaPoint, and Yuhei Miyauchi. 2025. "Interest Rate Caps, Corporate Lending, and Bank Market Power: Evidence from Bangladesh."

Lal, Apoorva, MacKenzie Lockhart, Yiqing Xu, and Ziwen Zu. 2024. "How Much Should We Trust Instrumental Variable Estimates in Political Science? Practical Advice Based on 67 Replicated Studies." *Political Analysis*, 32: 521–540.

Lazzarini, Sergio G, Aldo Musacchio, Rodrigo Bandeira de Mello, and Rosilene Marcon. 2015. "What do State-owned Development Banks do? Evidence From BNDES, 2002–09." *World Development*, 66: 237–253.

Marquez, Robert. 2002. "Competition, Adverse Selection, and Information Dispersion in the Banking Industry." *Review of Financial Studies*, 15: 901–926.

Martinez-Miera, David, and Rafael Repullo. 2010. "Does Competition Reduce the Risk of Bank Failure?" *Review of Financial Studies*, 23: 3638–3664.

Martín, Alberto, Sergio Mayordomo, and Victoria Vanasco. 2024. "Banks vs. Firms: Who Benefits from Credit Guarantees?"

Mian, Atif, and Amir Sufi. 2014. "What Explains the 2007–2009 Drop in Employment?" *Econometrica*, 82: 2197–2223.

Montiel Olea, José Luis, and Carolin Pflueger. 2013. "A Robust Test for Weak Instruments." *Journal of Business and Economic Statistics*, 31: 358–369.

Neumark, David, Brandon Wall, and Junfu Zhang. 2011. "Do Small Businesses Create More Jobs? New Evidence for the United States from the National Establishment Time Series." *Review of Economics and Statistics*, 93: 16–29.

Ornelas, Jose Renato Haas, Alvaro Pedraza, Claudia Ruiz-Ortega, and Thiago Christiano Silva. 2024. "Market Power and the Transmission of Loan Subsidies." *The Review of Corporate Finance Studies*, 13: 931–965.

Panizza, Ugo. 2023. "State-Owned Commercial Banks." *Journal of Economic Policy Reform*, 26: 44–66.

Panizza, Ugo. 2024. "Bank Ownership Around the World." *Journal of Banking and Finance*, 166: 107255.

Philippon, Thomas, and Vasiliki Skreta. 2012. "Optimal Interventions in Markets with Adverse Selection." *American Economic Review*, 102: 1–28.

Porta, Rafael La, Florencio Lopez-De-Silanes, and Andrei Shleifer. 2002. "Government ownership of banks." *Journal of Finance*, 57: 265–301.

Quirk, Thomas. 2023. "Interest Rate Caps, Segmented Markets, and Microfinance: A Cautionary Tale from Zambia."

Rambachan, Ashesh, and Jonathan Roth. 2023. "A More Credible Approach to Parallel Trends." *Review of Economic Studies*, 90.

Reyes-Housholder, Catherine. 2019. "A Theory of Gender's Role on Presidential Approval Ratings in Corrupt Times." *Political Research Quarterly*, 73: 540–555.

Rice, Tara, and Philip E Strahan. 2010. "Does Credit Competition Affect Small-Firm Finance?" *Journal of Finance*, 65: 861–889.

Ru, Hong. 2018. "Government Credit, a Double-Edged Sword: Evidence from the China Development Bank." *Journal of Finance*, 73: 275–316.

Sanches, Fabio, Daniel Silva Junior, and Sorawoot Srisuma. 2018. "Banking Privatization and Market Structure in Brazil: a Dynamic Structural Analysis." *RAND Journal of Economics*, 49: 936–963.

Sapienza, Paola. 2004. "The effects of government ownership on bank lending." *Journal of Financial Economics*, 72: 357–384.

Stiglitz, Joseph E., and Andrew Weiss. 1981. "Credit Rationing in Markets with Rationing Credit Information Imperfect." *American Economic Review*, 71: 393–410.

Stillerman, David. 2024. "Loan Guarantees and Incentives for Information Acquisition."

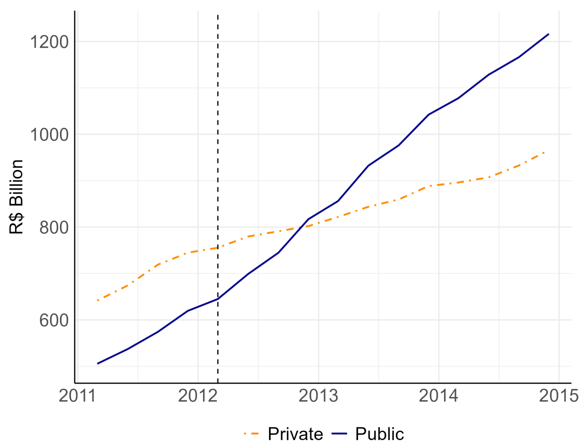
Tirole, Jean. 2012. "Overcoming Adverse Selection: How Public Intervention Can Restore Market Functioning." *American Economic Review*, 102: 29–59.

Traczynski, Jeffrey. 2017. "Firm Default Prediction: A Bayesian Model-Averaging Approach." *Journal of Financial and Quantitative Analysis*, 52: 1211–1245.

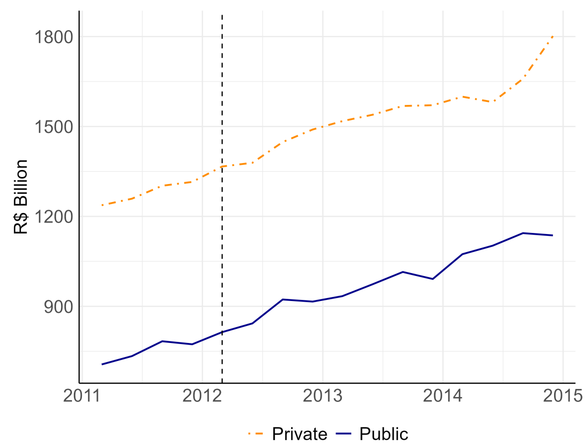
FIGURES

Figure 1: Outstanding Credit and Other Assets: Public and Large Private Banks

Notes: This figure shows the total amount of outstanding loans (Panel (a)) and other assets (Panel (b)) in banks' balance sheet, separated by bank ownership type, for each quarter, for the largest 6 banks in Brazil. The sample period is January 2011 to December 2013. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are: Bradesco, HSBC, Itau Unibanco, and Santander. Total amount outstanding includes all outstanding credit to firms and households. The vertical line indicates the start of the intervention (2012Q1). Sources: IF.data and authors' calculations.



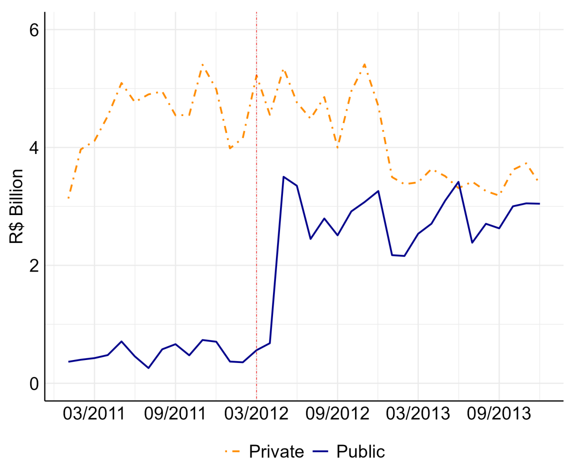
(a) Outstanding Loans



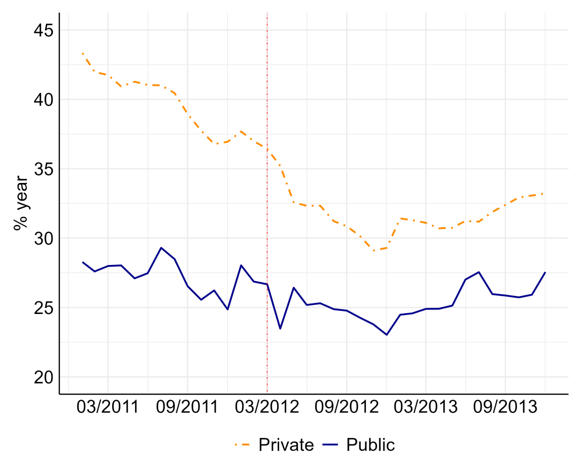
(b) Other Assets

Figure 2: Working Capital Origination and Interest Rates: Public and Private Banks

Notes: This figure shows the total volume and the interest rates of newly originated uncollateralized working capital loans to firms at monthly frequency. The sample period is January 2011 to December 2013. The sample is split between public and private banks. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF), and other state-owned banks under federal control. Private banks are all other banks that are not controlled by the government. Interest rate is shown as Annual Percentage Rate (APR). The vertical line indicates the announcement date of the intervention (March 2012). Sources: Credit Information System (SCR) and authors' calculations.



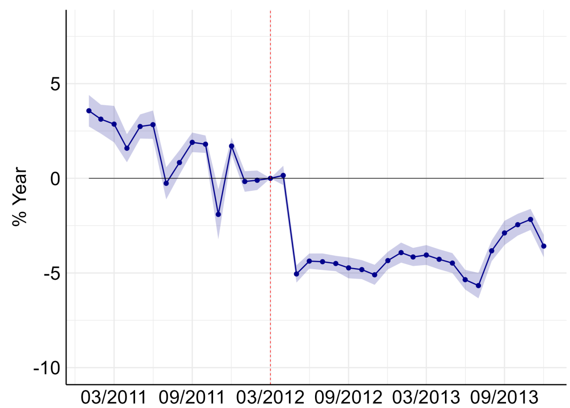
(a) Volume



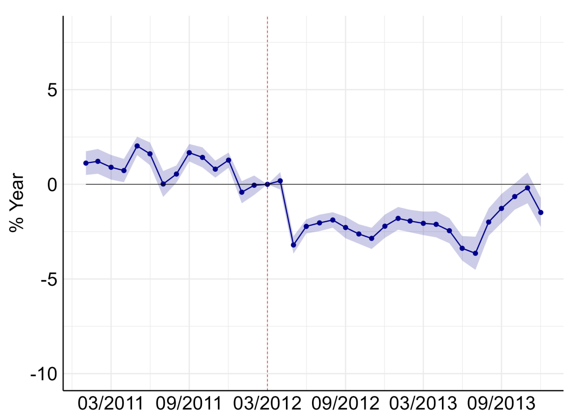
(b) Interest Rates

Figure 3: Differential Interest Rates: Public and Private Banks

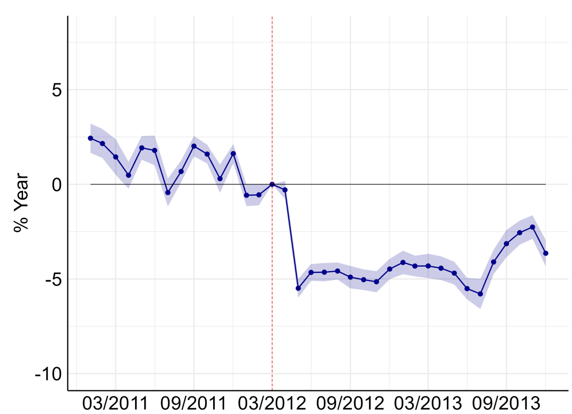
Notes: This figure shows the estimated δ_τ from Equation (1) with March 2012 as the reference month (vertical line). The unit of observation is loan-origination month. The sample period is January 2011 to December 2013. The dependent variable i_{jtmbfs} is the interest rate of a loan j issued in month t, municipality m, by bank b, to firm f, which operates in industry s. For **Panel (a)**, we estimate Equation (1) including bank fixed effects α_b. For **Panel (b)** we estimate the same specification, but substitute bank fixed effects by bank-firm fixed effects α_{fb}. For **Panel (c)** we re-estimate the specification of Panel (a) with bank rather than bank-firm fixed effects, but restrict the sample to the be same sample used in Panel (b). Standard errors are clustered at bank-municipality level. Regressions are weighted by loan amount. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.



(a) Bank Fixed Effects



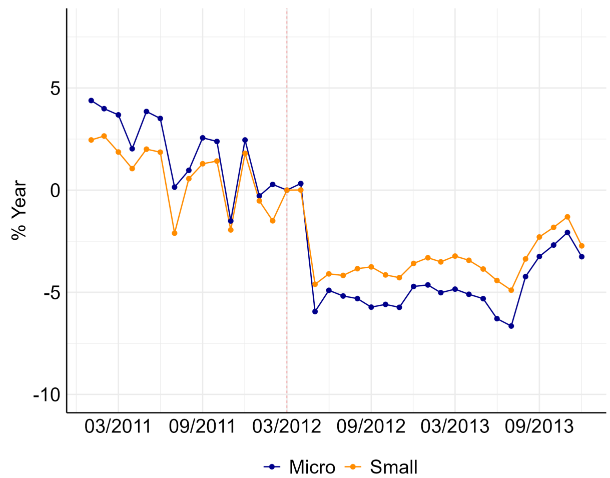
(b) Bank-firm Fixed Effects



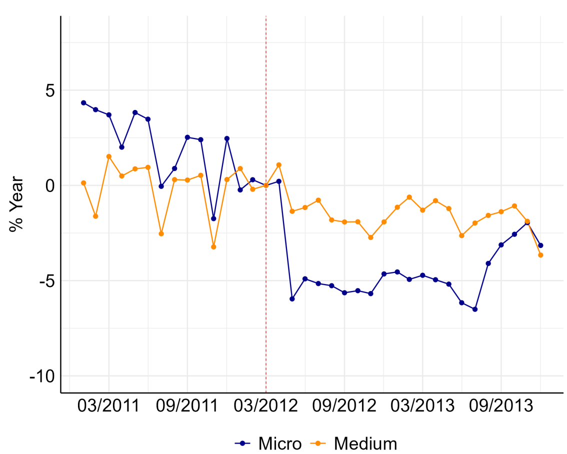
(c) Bank Fixed Effects (smaller sample)

Figure 4: Differential Interest Rate Changes: Public and Private Banks

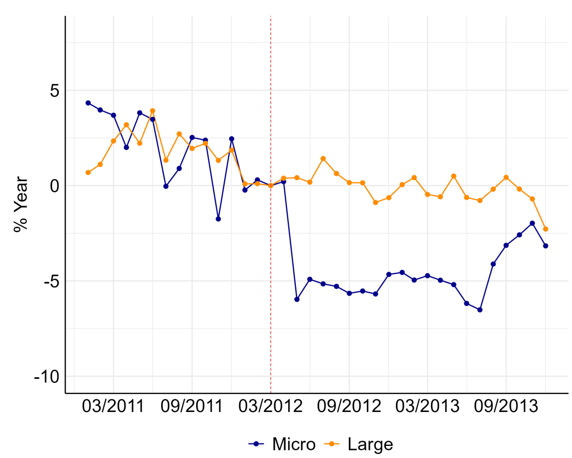
Notes: This figure shows the point estimates of δ_τ from Equation (3) with March 2012 as the reference month (vertical line). The unit of observation is loan-origination month. The sample period is January 2011 to December 2013. The dependent variable i_{jtmbfs} is the interest rate of a loan j issued in month t, municipality m, by bank b, to firm f, which operates in industry s. For **Panel (a)**, we estimate the regression with micro and small firms only. For **Panel (b)**, we estimate the regression with micro and medium size firms only. For **Panel (c)**, we estimate the regression for micro and large firms only. We plot separately the point estimates for coefficients δ_τ for micro firms (baseline group) and the sum of $\delta_\tau + \beta_\tau$ for each other firm size category in a given month τ. Regressions are weighted by loan amount. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.



(a) Micro versus Small Firms



(b) Micro versus Medium Firms



(c) Micro versus Large Firms

Figure 5: Debt-to-Payroll Ratio: Public and Private bank borrowers

Notes: This figure shows the estimates of δ_τ from Equation (5) with March 2012 as the reference month (vertical line). The unit of observation is firm-month. The sample period is January 2011 to December 2013. The dependent variable $\frac{\text{Debt}_{tf}}{\text{Payroll}_{2011,f}}$ is the total outstanding debt of firm f in month t, normalized by the total payroll costs of firm f in 2011. The γ_τ estimates from specification (5) show the difference between debt-over-payroll costs of firms with exclusive public bank relationships compared to firms with exclusive private bank relationships, relative to March 2012. The sample consists of firms with exclusive relationships with types of banks, where relationships are defined based on loan issuance between 2011 and March 2012. Standard errors are clustered at the municipality level. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

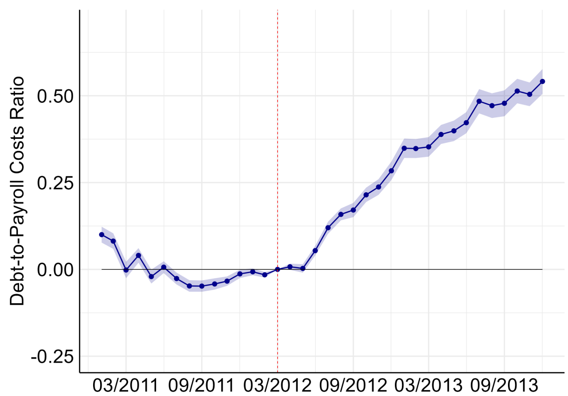
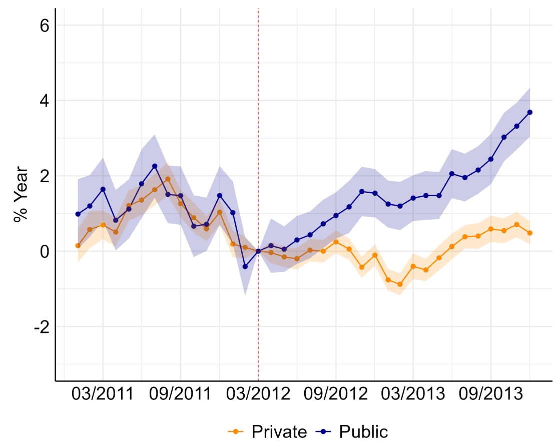
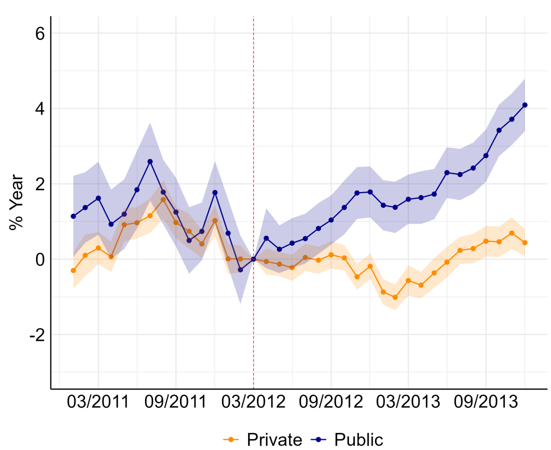


Figure 6: Delinquency Likelihood For Public and Private Banks

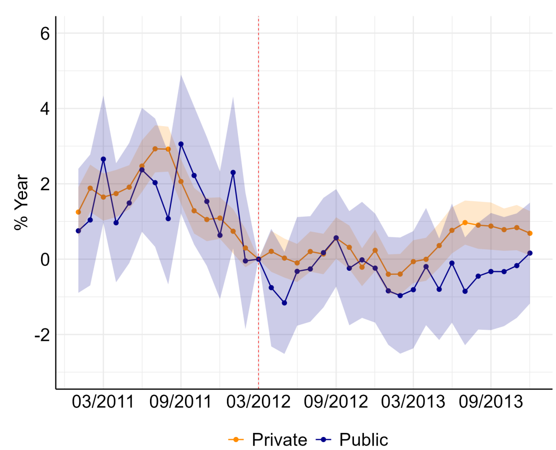
Notes: This figure shows the estimates of δ_τ from the estimation of Equation (6) with March 2012 as the reference month (vertical line). The unit of observation is firm-bank-origination month. The sample period is January 2011 to December 2013. The dependent variable I^D_{tmbf} is an indicator variable which is equal to one if a loan originated by bank b to firm f located in municipality m in month t becomes delinquent for at least 90 days within one year after origination. We estimate (6) for three different subsets of firms. **Panel (a)** includes all firms in our sample. **Panel (b)** includes only *levered* borrowers, which are defined as firms with positive debt outstanding in the month before the origination month. **Panel (c)** includes only *unlevered* borrowers, which are firms without debt outstanding in the month before the origination month. Standard errors are clustered at the municipality level. Regressions are weighted by loan amount. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.



(a) All Borrowers



(b) Levered Borrowers



(c) Unlevered Borrowers

Figure 7: Covariate Balance - Public Banks SME Lending Market Share

Notes: This figures shows the covariate balance for our regional instrument. The regional instrument $Share_m^{Pub,2010}$ denotes the monthly market share of public banks in firm credit outstanding in municipality m, averaged over 2010. Each row corresponds to the correlation between the standardized value of the corresponding variable and the standardized value of $Share_m^{Pub,2010}$. All variables are calculated in the baseline year of 2011. *Unconditional* refers to unconditional correlation between the instrument and each standardized variable. *Region FE* corresponds to the coefficients of a regression of the standardized values of each variable on the instrument, including meso-region fixed effects. Error bars around point estimates represent 95% confidence intervals. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.

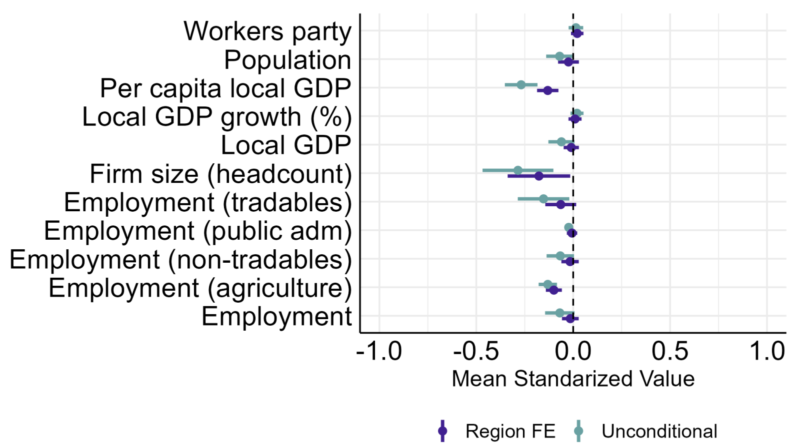


Figure 8: First stage: credit origination

Notes: This figure shows the estimates of γ_τ from Equation 11 with 2012Q1 as the reference quarter. The unit of observation is municipality-quarter level. The sample period is 2011Q1 to 2013Q4. The dependent variable $Credit_{tm}^{Orig,Pub}$ is the growth of public credit origination in quarter t, municipality m, relative to average total firm debt outstanding in municipality m in 2011, in decimal points. The sample includes only municipalities that had bank branches at the beginning of 2011. Standard errors are clustered at the municipality level. Regressions are weighted by the municipality population in 2011. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.

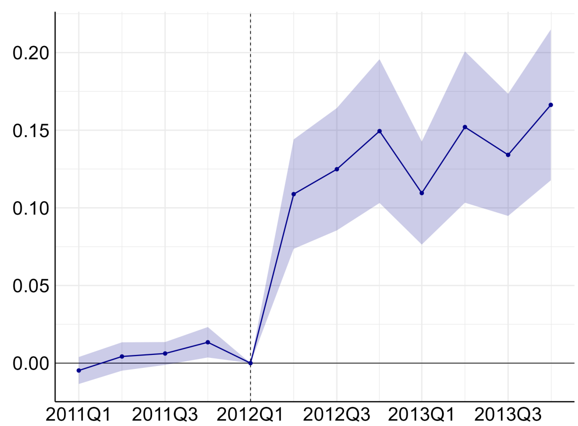
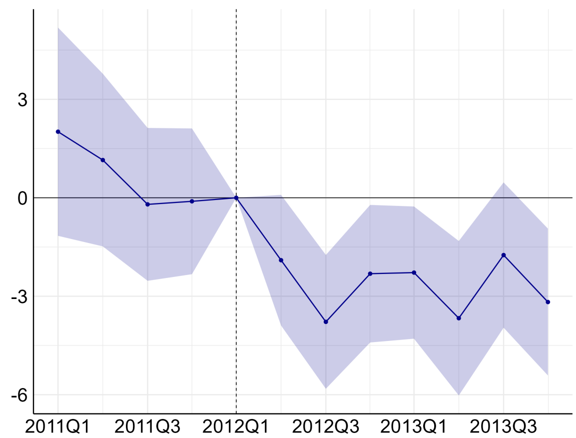
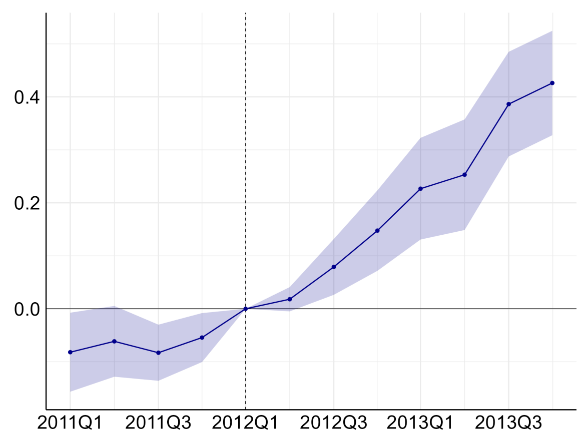


Figure 9: Second stage: credit outcomes

Notes: This figure shows the estimates of γ_τ from Equation 11 with 2012Q1 as the reference quarter. The unit of observation is municipality-quarter level. The sample period is 2011Q1 to 2013Q4. In **Panel (a)** the dependent variable is i^{Pr}_{tm}, the weighted average private interest rates in quarter t, municipality m. In **Panel (b)** the dependent variable is $debt\ over\ payroll_{tm}$, the weighted average debt over payroll costs in quarter t, municipality m. The sample includes only municipalities that had bank branches at the beginning of 2011. Standard errors are clustered at the municipality level. Regressions are weighted by the municipality population in 2011. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.



(a) Private Interest Rates



(b) Debt over payroll costs

TABLES

Table 1: Summary Statistics

Notes: This table reports summary statistics for the main variables in our data set, across all years in our sample (2011 to 2013). There are N_{obs} =2.6M observations and N_{firms} = 738,652 firms in the matched sample. **Panel A** shows the summary statistics of the loans in our sample. **Panel A** shows the summary statistics of the firms in our sample. The sample period is from 2011 to 2013. Appendix A contains a description of our sample construction process. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

	Mean	Median	SD
Panel A: Loans			
Panel A.1 - Public Banks Loans			
Amount (R$)	64,104	37,603	100,082
Maturity (months)	23.27	24	10.78
Interest Rate (APR)	25.58	23.64	8.477
Panel A.2 - Private Banks Loans			
Amount (R$)	92,801	35,995	166,824
Maturity (months)	16.82	17	10.99
Interest Rate (APR)	35.21	33.54	14.25

Panel B: Firms			
Panel B.1 - Firms that borrow exclusively from Public Banks			
Num. of Employees	11.04	4	45.52
Payroll Costs (R$ per Month)	12,837	3,847	61,846
Total Outstanding Debt	121,329	28,435	1,037,000
Debt-to-Payroll Ratio	2.96	0.59	15.20
Panel B.2 - Firms that borrow exclusively from Private Banks			
Num. of Employees	11.65	3	66.11
Payroll Costs (R$ per Month)	15,190	3,122	102,505
Total Outstanding Debt	184,094	13,406	2,166,000
Debt-to-Payroll Ratio	2.52	0.38	21.10
Panel B.3 - Firms that borrow from both types of Banks			
Num. of Employees	22.99	7.33	100.1
Payroll Costs (R$ per Month)	29,699	7,689	121,494
Total Outstanding Debt	551,396	173,240	2,118,000
Debt-to-Payroll Ratio	5.94	2.06	25.26

Table 2: Municipality Characteristics

Notes: This table reports summary statistics for the main variables in our municipality level data set for the baseline year (2011). **Panel A** shows the summary statistics for municipalities in which all branches belong to public banks. **Panel B** shows the summary statistics for municipalities in which all branches belong to private banks. **Panel C** shows the summary statistics for municipalities in which both bank types are present. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.

	Mean	Median	SD
Panel A: Public Banks Only			
Share Loans Public	0.41	0.34	0.29
Interest Private (% year)	42.28	40.13	10.72
Interest Public (% year)	28.2	27.68	3.94
Private Credit (branches) over GDP	0.0	0.0	0.0
Public Credit (branches) over GDP	0.16	0.12	0.15
Private Credit (firms) over GDP	0.02	0.01	0.04
Public Credit (firms) over GDP	0.01	0.01	0.01
GDP R$	115,241.0	95,541.0	84,613.12
Population	10,835.83	8,305.0	7,600.9
Total Employment	593.28	377.0	674.38
Private HHI	0.36	0.32	0.15
Panel B: Private Banks Only			
Share Loans Public	0.16	0.11	0.17
Interest Private (% year)	43.04	42.07	8.71
Interest Public (% year)	28.46	27.28	4.49
Private Credit (branches) over GDP	0.04	0.02	0.05
Public Credit (branches) over GDP	0.0	0.0	0.0
Private Credit (firms) over GDP	0.02	0.01	0.03
Public Credit (firms) over GDP	0.01	0.0	0.01
GDP R$	92,908.76	68,017.5	89,800.12
Population	7,818.83	6,582.0	5,375.78
Total Employment	544.3	303.0	633.6
Private HHI	0.41	0.35	0.2
Panel C: Both Bank Types			
Share Loans Public	0.26	0.19	0.2
Interest Private (% year)	41.45	40.48	7.31
Interest Public (% year)	27.76	27.53	2.84
Private Credit (branches) over GDP	0.03	0.02	0.07
Public Credit (branches) over GDP	0.14	0.12	0.1
Private Credit (firms) over GDP	0.04	0.02	0.06
Public Credit (firms) over GDP	0.02	0.01	0.02
GDP R$	2,002,846.7	391,189.0	13,544,807.5
Population	79,271.73	28,963.0	334,513.2
Total Employment	14,436.07	2,743.0	92,401.44
Private HHI	0.27	0.24	0.13

Table 3: Interest Rate Differences Between Private and Public Banks

Notes: This table shows the estimates of the β's on Equation 2. The unit of observation is loan-origination month. The sample period is January 2011 to December 2013. The dependent variable i_{jtmbfs} is the interest rate of a loan j issued in month t, municipality m, by bank b, to firm f, which operates in industry s. $Private_b$ is a dummy variable which equals 1 for loans issued by private banks. $Post_t$ is a dummy variable which equals 1 for loans issued after March 2012. Industry denotes the two-digit CNAE code of the corresponding borrower. Firm size and maturity are categorical variables which are described in Appendix A. Standard errors are clustered at the municipality level. Regressions are weighted by loan amount. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.

	(1)	(2)	(3)	(4)	(5)
$Private_b$	7.629***			7.135***	
	(0.383)			(0.472)	
$Private_b \times Post_t$	-6.102***	-5.521***	-3.131***	-5.688***	-5.470***
	(0.201)	(0.171)	(0.185)	(0.246)	(0.197)
Time × Municipality × Industry × Size FE	✓	✓	✓	✓	✓
Bank FE		✓			✓
Bank × Firm FE			✓		
R^2	0.455	0.482	0.844	0.454	0.487
Observations	2193712	2193712	1683213	1683213	1683213

Table 4: Interest Rate Differences Between Private and Public Banks - Firm Size

Notes: This table shows the estimates of the β_1 and β_2^{type} coefficients from Equation 4. The unit of observation is loan-origination month. The sample period is January 2011 to December 2013. The dependent variable i_{jtmbfs} is the interest rate of a loan j issued in month t, municipality m, by bank b, to firm f, which operates in industry s. $Private_b$ is a dummy variable which equals 1 for loans issued by private banks. $Post_t$ is a dummy variable which equals 1 for loans issued after March 2012. Industry denotes the two-digit CNAE sector code of the corresponding borrower. Firm size and maturity are categorical variables which are described in Appendix A. Standard errors are clustered at the municipality level. Regressions are weighted by loan amount. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

	(1)	(2)	(3)
$Private_b$	10.07***		
	(0.396)		
$Private_b \times D_f^{Small\ Firm}$	-5.147***	-4.577***	-0.798***
	(0.146)	(0.135)	(0.180)
$Private_b \times D_f^{Medium\ Firm}$	-7.145***	-6.329***	-1.794***
	(0.424)	(0.440)	(0.321)
$Private_b \times D_f^{Large\ Firm}$	-6.055***	-5.630***	-3.081***
	(0.361)	(0.397)	(0.532)
$Private_b \times Post_t$	-7.371***	-6.722***	-4.097***
	(0.191)	(0.261)	(0.250)
$Private_b \times Post_t \times D_f^{Small\ Firm}$	2.521***	2.198***	1.139***
	(0.153)	(0.155)	(0.167)
$Private_b \times Post_t \times D_f^{Medium\ Firm}$	5.347***	4.621***	2.454***
	(0.354)	(0.381)	(0.328)
$Private_b \times Post_t \times D_f^{Large\ Firm}$	4.903***	4.623***	3.820***
	(0.282)	(0.300)	(0.380)
Time × Municipality × Industry × Size FE	✓	✓	✓
Bank FE		✓	
Bank × Firm FE			✓
R^2	0.458	0.504	0.849
Observations	2193712	2193712	1683213

Table 5: Delinquency Differences Between Private and Public Banks

Notes: This table shows the estimates of the β's on Equation 7. The unit of observation is firm-bank-origination month. The sample period is January 2011 to December 2013. The dependent variable I^{D}_{tmbf} is an indicator variable which is equal to one if a loan originated by bank b to firm f located in municipality m in month t becomes delinquent for at least 90 days within one year after origination. $Public_b$ is a dummy variable which equals 1 for loans issued by public banks. $Post_t$ is a dummy variable which equals 1 for loans issued after March 2012. $Debt_{tf}$ is a dummy variable which equals 1 if borrower f had positive debt outstanding in the month before t. Industry denotes the two-digit CNAE sector code of the corresponding borrower. Firm size is a categorical variable which is described in Appendix A. Standard errors are clustered at the municipality level. Regressions are weighted by loan amount. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

	(1)	(2)	(3)
$Public_b$	-0.00305		
	(0.00189)		
$Public_b \times Post_t$	0.0109***	0.0104***	-0.00878**
	(0.00174)	(0.00181)	0.00268)
$Debt_{tf}$			0.0226***
			(0.00165)
$Post_t \times Debt_{tf}$			-0.00429**
			(0.00152)
$Public_b \times Debt_{tf}$			0.00354
			(0.00308)
$Post_t \times Public_b \times Debt_{tf}$			0.0179***
			(0.00321)
Time× Municipality × Industry × Size FE	✓	✓	✓
Bank FE		✓	✓
R^2	0.175	0.176	0.177
Observations	1812219	1812219	1812219

Table 6: Leverage and Default Risk by Bank Type

Notes: This figure shows the estimates of β_l in Equation (8). The sample period is the post-intervention period, of April 2012 to December 2013. The unit of observation is firm-bank-origination month. The dependent variable I^{D}_{tmbf} is an indicator variable which is equal to one if a loan originated by bank b to firm f located in municipality m in month t becomes delinquent for at least 90 days within one year after origination. Each Q^{j}_{f} denotes the corresponding leverage quintile to which a firm belongs based on its debt-over-payroll costs ratio. $Public$ is a dummy which equals 1 for loans issued by public banks. Columns 1 and 2 show the results for regressions including only private banks. Columns 3 and 4 show the results for regressions including only public banks. Columns 5 and 6 include all observations, and the interaction terms between leverage quintiles and the $Public$ dummy. Industry denotes the two-digit CNAE sector code of the corresponding borrower. Firm size is a categorical variable which is described in Appendix A. Standard errors are clustered at the municipality level. Regressions are weighted by loan amount. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

	Private Banks		Public Banks		Both	
	(1)	(2)	(3)	(4)	(5)	(6)
Q^2_f	0.00560***	0.00549***	0.00886***	0.00895***	0.00406***	0.00393***
	(0.000918)	(0.000937)	(0.00145)	(0.00144)	(0.000892)	(0.000900)
Q^3_f	0.0180***	0.0178***	0.0251***	0.0252***	0.0166***	0.0164***
	(0.00124)	(0.00123)	(0.00198)	(0.00198)	(0.00115)	(0.00113)
Q^4_f	0.0340***	0.0337***	0.0485***	0.0485***	0.0321***	0.0319***
	(0.00137)	(0.00135)	(0.00211)	(0.00211)	(0.00125)	(0.00122)
Q^5_f	0.0615***	0.0609***	0.0859***	0.0859***	0.0605***	0.0602***
	(0.00182)	(0.00179)	(0.00266)	(0.00267)	(0.00163)	(0.00162)
$Q^2_f \times Public_b$					0.00809***	0.00826***
					(0.00141)	(0.00141)
$Q^3_f \times Public_b$					0.0113***	0.0114***
					(0.00188)	(0.00185)
$Q^4_f \times Public_b$					0.0182***	0.0184***
					(0.00208)	(0.00205)
$Q^5_f \times Public_b$					0.0257***	0.0259***
					(0.00262)	(0.00260)
Time × Municipality × Industry × Size FE	✓	✓	✓	✓	✓	✓
Bank FE		✓		✓		✓
R^2	0.208	0.211	0.235	0.235	0.201	0.202
Observations	479004	479000	395504	395504	988954	988953

Table 7: Exposure to the policy and credit outcomes

Notes: This table shows the estimated β's from equation 9. The unit of observation is municipality-quarter. The sample period is from 2011Q1 to 2013Q4. The dependent variables are: $Interest\ Rates^{\%,Pr}_{tm}$, the average private bank loan interest rate for working capital loans in quarter t (in % points), municipality m, and $Debt\ Over\ Payroll_{tm}$, the average debt over payroll costs of firms in quarter t at municipality m. $Credit^{Orig,Pub}_{tm}$ denotes the ratio of total public credit origination in quarter t at municipality m, divided by average total firm credit outstanding in 2011, expressed in decimal points. Even numbered columns show the estimates using the specification that includes municipality level controls. The controls are: lagged employment in the tradable and non-tradable sectors, and lagged industry, services and agriculture local output growth. Standard errors are clustered at the municipality level. Regressions are weighted by municipality population in 2011. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. We follow Montiel Olea and Pflueger (2013) and report the Effective F-statistic, which is calculated with the package 'ivDiag' for R, developed by Lal et al. (2024). Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.

	$Interest\ Rates^{\%,Pr}_{tm}$		$Debt\ Over\ Payroll_{tm}$	
	(1)	(2)	(3)	(4)
$Credit^{Orig,Pub}_{tm}$	-27.77***	-27.72***	1.231***	1.218***
	(5.302)	(5.313)	(0.3212)	(0.3219)
Controls		✓		✓
Region-Time FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
R^2	0.69215	0.69240	0.85677	0.85706
Observations	30,320	30,320	33,839	33,839
Effective F-Statistic	39.49	40.35	49.38	50.74

Table 8: Public Banks Market Share and Local Outcomes

Notes: This table shows the estimated β's from equation 11. The unit of observation is municipality-year. The sample period is 2010 to 2013. The dependent variables are: $Log\ GDP_{tm}$, the log of gross local product in year t at municipality m, $Log\ Emp_{tm}$ total employment excluding finance, utility, agricultural and public sector jobs in year t at municipality m, $Log\ Emp^{T}_{tm}$ and $Log\ Emp^{NT}_{tm}$ employment in tradable and non-tradable sectors in year t at municipality m, respectively. $Share^{Pub,2010}_{m}$ is the monthly market share of private banks in firm credit at municipality m, averaged across 2010, expressed in decimal points. Standard errors are clustered at the municipality level. Regressions are weighted by municipality population in 2011. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.

	$Log\ GDP_{tm}$ (1)	$Log\ Emp_{tm}$ (2)	$Log\ Emp^{T}_{tm}$ (3)	$Log\ Emp^{NT}_{tm}$ (4)
$Share^{Pub,2010}_{m}$ × Year = 2010	-0.0004	0.0056	0.0513	-0.0025
	(0.0199)	(0.0160)	(0.0411)	(0.0164)
$Share^{Pub,2010}_{m}$ × Year = 2012	0.0130	0.0519***	0.0863**	0.0242*
	(0.0187)	(0.0182)	(0.0427)	(0.0128)
$Share^{Pub,2010}_{m}$ × Year = 2013	0.0032	0.0736***	0.1565***	0.0263
	(0.0219)	(0.0237)	(0.0512)	(0.0173)
Controls	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
R^2	0.99934	0.99919	0.99372	0.99950
Observations	11,128	11,128	11,128	11,128

Table 9: Real Effects of Public Credit Growth

Notes: This table shows the estimated β's from equation 9. The unit of observation is municipality-year. The sample period is 2011 to 2013, as we do not collect credit origination data for 2010. The dependent variables are: $Log\ GDP_{tm}$, the log of gross local product in year t at municipality m, $Log\ Emp_{tm}$ total employment excluding finance, utility, agricultural and public sector jobs in year t at municipality m, $Log\ Emp^{T}_{tm}$ and $Log\ Emp^{NT}_{tm}$ employment in tradable and non-tradable sectors in year t at municipality m, respectively. $Credit^{Orig,Pub}_{tm}$ denotes the ratio of total public credit origination in year t at municipality m, divided by average total firm credit outstanding in 2011, expressed in decimal units. We follow Montiel Olea and Pflueger (2013) and report the Effective F-statistic, which is calculated with the package 'ivDiag' for R, developed by Lal et al. (2024). Standard errors are clustered at the municipality level. Regressions are weighted by municipality population in 2011. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.

	$Log\ GDP_{tm}$		$Log\ Emp_{tm}$		$Log\ Emp^{T}_{tm}$		$Log\ Emp^{NT}_{tm}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Credit^{Orig,Pub}_{mt}$	0.0202	0.0169	0.1569***	0.1550***	0.3035***	0.3081***	0.0631**	0.0656**
	(0.0490)	(0.0475)	(0.0462)	(0.0457)	(0.1159)	(0.1144)	(0.0307)	(0.0306)
Controls		✓		✓		✓		✓
Region-Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.99955	0.99955	0.99940	0.99941	0.99507	0.99509	0.99964	0.99964
Observations	8,346	8,346	8,346	8,346	8,346	8,346	8,346	8,346
Effective F-Statistic	46.83	48.50	46.83	48.50	46.83	48.50	46.83	48.50

Table 10: Cost per job across countries

Notes: This table summarizes the estimated cost per job estimates of four credit market interventions, which were implemented in different countries. Brazil estimates reflect the calculation outlined in Section V. For Portugal, the estimates are based on Bonfim, Custódio and Raposo (2023). For France, data comes from Barrot et al. (2024). For the United States, Brown and Earle (2017). Monthly minimum wage information assumes a 35 hour per week journey for all countries other than Brazil, where the information is already provided at monthly frequency.

	Brazil	**Portugal**	**France**	**United States**
Cost per job	R$ 1,987	€5,784–11,788	€425-1,400	$21,580–25,450
Monthly minimum wage	R$ 622–678	€425–580	€1,005–1,060	$1,133
Cost per job over minimum wage	3	11–26	0.4–1.4	19–22.4
Intervention years	2012–2013	2008–2018	2009–2010	1998–2009

Internet Appendix

A. Data Appendix

A.1. Data sources for descriptive statistics in the text

We report cross-country data from a variety of sources throughout the text:

- Data compiled by Panizza (2024), including the share of total bank assets held by commercial state-owned banks.
- The World Bank Global Financial Development Database, which contains financial deepening indicators for several countries, including Brazil.
- IMF's Global Financial Development database and the IMF International Financial Statistics, for information on loan spreads and return over assets across countries.

A.2. Loan level and firm level data

Credit Registry : The starting point in the construction of our main dataset is to collect loan origination information for working capital loans on a monthly basis. We obtain this information from the SCR, the credit registry maintained by the Brazilian Central Bank. The basic variables are:

- *Loan Interest Rate*: We focus on loans with fixed interest rates which are financed by banks' own capital. We use the loan base rate, which corresponds to loan interest rates for fixed rate loans. We exclude loans with negative or larger than 1000 % (annual) interest rates. On top of collecting loan rate information at the month of origination. We collect loans level information for posterior months in instances in which the information at the origination month is incorrect. In those cases we use the loan interest rates from posterior months as the reference rate. We also calculate annual interest rates for one institution which reported monthly loan rates for the earlier months of the sample.

- *Loan Amount*: Denotes the amount outstanding for a given loan at the month of origination. We drop any loans w/ no amount outstanding information.

- *Default*: The credit registry collect information on the amount of loans past due for different periods of time. We consider a loan in default if it is more than 90 days late. Since loan identifiers are not constant across time, we track delinquency information at the firm-month of origination-loan type-bank dimension, rather than at the loan level. We track delinquency for up to one year after origination.

After constructing the basic data we perform the following exclusions:

- Drop firms from utilities and public industries (CNAE 2 digit industry codes 33-39 and 84). [21];

- Drop loans with annual interest rates smaller than 5%, which are likely miss classified as fixed interest loans;

- Drop loans with credit scoring worse than D, which include only renegotiations;

- Include only limited liability, corporations and sole proprietors firms;

We winsorize loan amounts, maturity and interest rates at the 1% in each tail. We then merge this dataset with a monthly employment dataset constructed based on RAIS Annual files. We only include firm observations for firms with a RAIS registry, which corresponds to more than two thirds of our data.

We use firm employment to construct our firm size categories, following the SEBRAE classification:

- *Micro Firms*: Firms with less than 10 employees in the service/commerce sectors, or less than 20 employees in industry sectors.

- *Small Firms*: Firms with more than 10 and less than 50 employees in the service/commerce sectors, or more than 20 and less than 100 employees in industry sectors.

[21] For details on industry classification, see https://cnae.ibge.gov.br/

- *Medium Firms*: Firms with more than 50 and less than 100 employees in the service/commerce sectors, or more than 100 and less than 500 employees in industry sectors.
- *Large Firms*: Firms with more than 100 employees in the service/commerce sectors, or more than 500 employees in industry sectors.

We also construct a loan maturity categorical variable by defining the following categories, which are centered around 6 month intervals:

- *Up to 3 Months*: Loans with maturity smaller than 3 months.
- *6 Months*: Loans with maturity between 3 and 9 months.
- *1 Year*: Loans with maturity between 9 and 15 months.
- *1.5 Years*: Loans with maturity between 15 and 21 months.
- *2 Years*: Loans with maturity between 21 and 27 months.
- *2.5 Years*: Loans with maturity between 27 and 33 months.
- *3 Years*: Loans with maturity between 33 and 39 months.
- *3.5 Years*: Loans with maturity between 39 and 45 months.
- *More than 3.5 Years*: Loans with maturity of 45 months or more.

The monthly employment panel is constructed using hiring and termination dates for each employee in the RAIS dataset. We aggregate such information at the firm-month level. Each dataset contain firms without a correspondent in the other dataset. Not all firms have access to credit and/or decide to borrow in a given year (are in RAIS but not SCR), and non-employer firms that do borrow are in SCR but not in RAIS. The latter corresponds to less than 15 percent of the total amount originated by government banks as part of the intervention, and are not included in our sample.

A.3. Municipality level data

To construct the municipality-time dataset, we aggregate the loan level information from SCR to obtain working capital loan origination, weighted average interest rates, and total outstanding debt for firms in each location, separately for public and private banks. We obtain local employment by aggregating firm level employment for each municipality and each year in our sample. We apply the same sector and firm ownership exclusions used in the credit registry file. When calculating employment at the municipality level, we also exclude jobs in the public sector and jobs in agriculture.

We perform the following exclusions to our municipality-month dataset:

- Keep only municipalities which have at least one bank branch before the intervention
- Keep only municipalities that have employment both in the tradable and non-tradable sectors
- Exclude municipalities in which either industry, agriculture, services or public sector value added equals zero
- Keep only municipalities in which both $Credit_{tm}^{Orig,Pub}$ and $Share_{m}^{Pub,2010}$ are not missing
- Exclude municipalities which have yearly $Credit_{tm}^{Orig,Pub}$ in the top and bottom 0.5% of the distribution
- Exclude municipalities which experience employment growth greater than 100% in a given year

Financial institutions' balance sheets, income statements, and regulatory capital information for all financial institutions in the country are available at a quarterly frequency at the Central Bank of Brazil's *IF.data* website. Branch balance sheet data is available at a monthly frequency from the Monthly Bank Statistics by Municipality (ESTBAN). The data allow us to identify entry and exit of banks in each municipality, and is used to define branch presence. We also use gross local product and population data from the Brazilian Institute of Geography and Statistics (IBGE). Appendix C also uses local elections data from the Superior Electoral Court (TSE), for mayoral elections taking place in 2008 and 2012.

Finally, we follow Joaquim, van Doornik and Ornelas (2023) and define *tradable* and *non-tradable* sectors using the following mapping of CNAE industry codes:

Table A.1: Sector Classification from the First Two Digits of CNAE Code

Sector	**CNAE (first 2 digits)**
Tradable	5–8, 10–13, 15, 22–31
Non-Tradable	45–47, 55–56

A.4. Minimum wage data

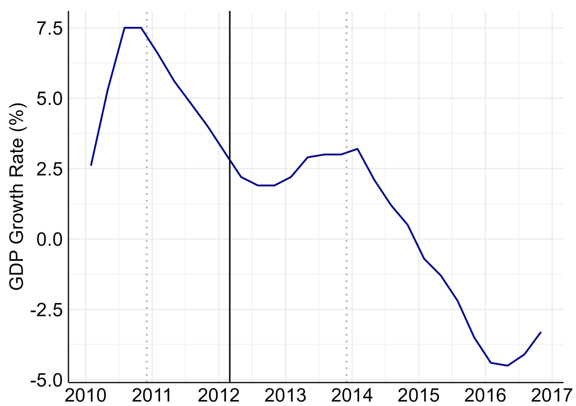
The analysis in Section VI makes use of minimum wage data for Brazil, France, Portugal and the United States. Brazilian minimum wage information was obtained from the Ministry of Labor's website. France minimum wage information was obtained from the National Institute of Statistics and Economic Studies (Insee). Portugal minimum wage information was obtained from the Directorate General for Administration and Public Employment (DGAEP).

For the US, we calculate a representative national minimum wage by averaging state-level minimum wages by the total employment in each state per year. Historical minimum wage data across states is obtained from the Department of Justice (DOJ), available at https://www.dol.gov/agencies/whd/state/minimum-wage/history. Historical state-level employment data is obtained from the Bureau of Economic Analysis (BEA), available at https://www.bea.gov/data/employment/employment-by-state.

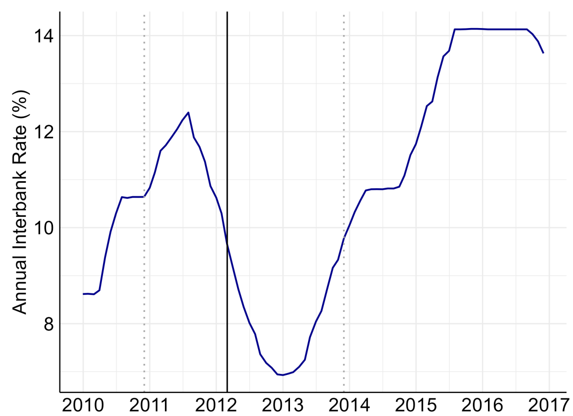
B. Additional Figures and Tables

Figure B.1: Evolution of Macroeconomic Variables

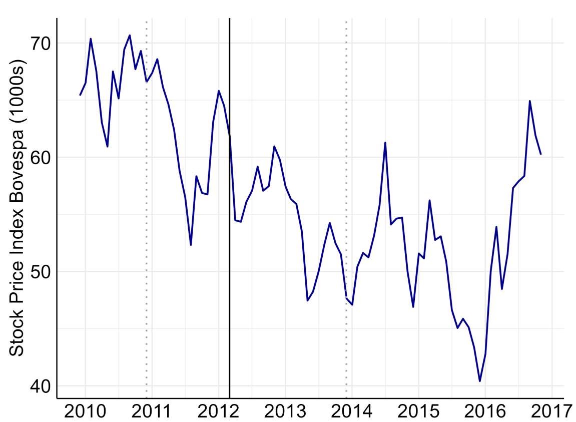
Notes: This figure shows the evolution of key macroeconomic variables during our sample. The vertical solid line denotes March 2012. The dotted gray lines indicate our sample period (2011 to 2013). Panel (a) displays the Real GDP growth (seasonally adjusted). Panel (b) displays the annualized overnight interbank rates. Panel (c) displays the Bovespa Stock Price Index. Panel (d) displays the R$ per US$ exchange rate. Sources: Central Bank of Brazil, B3, IBGE, and authors' calculations.



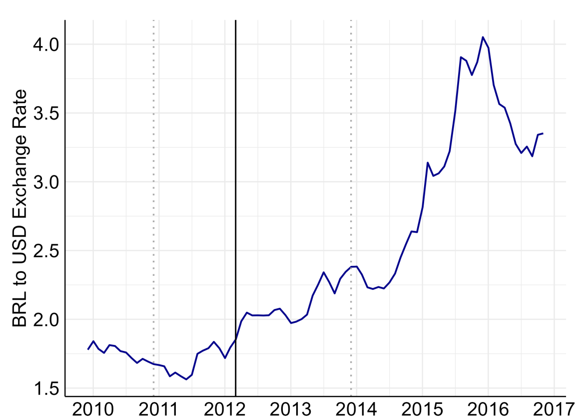
(a) Real GDP Growth



(b) Interbank Rates



(c) Stock Index



(d) Exchange Rate (R$ per US$)

Figure B.2: President's Net Approval Rating

Notes: This figure shows the evolution of net approval rating of Dilma Rouseff (President) from the time the took office until her impeachment. Net approval rating is defined as the percent of positive ratings minus the percent of negative ratings. The vertical solid line denotes March 2012. The dotted gray lines indicate our sample period (2011 to 2013). Sources: Reyes-Housholder (2019), and authors' calculations

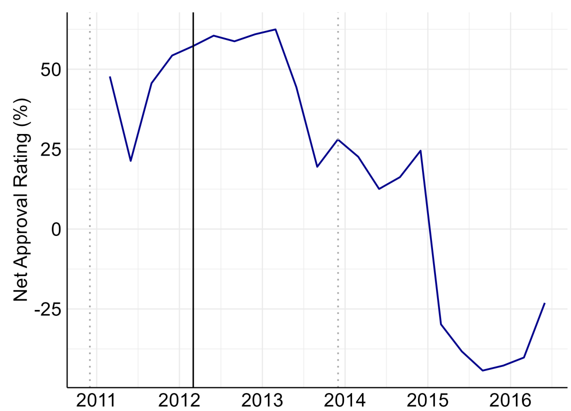
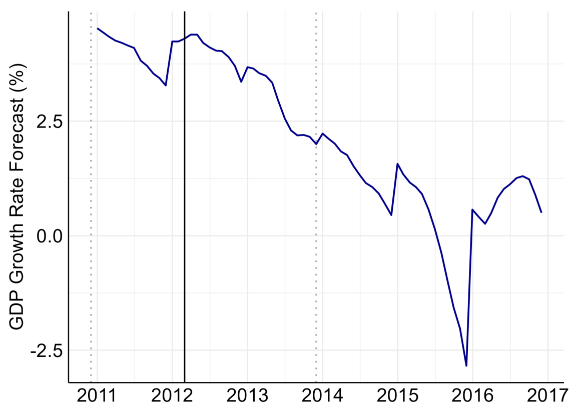
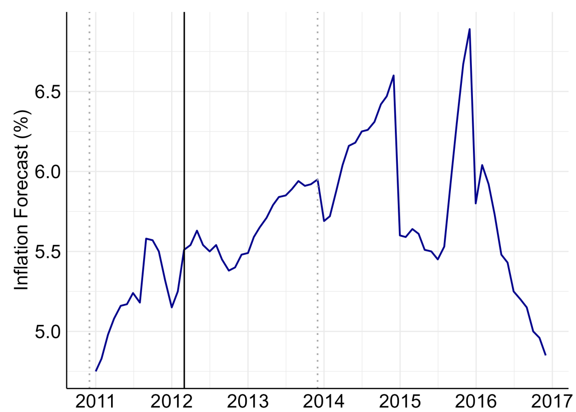


Figure B.3: Forecasts of Macroeconomic Variables

Notes: This figure shows the evolution of macroeconomic forecasts during our sample period. Panel(a): GDP forecast by month from FOCUS survey (average). Panel (b): The vertical solid line denotes March 2012. The dotted gray lines indicate our sample period (2011 to 2013). The plotted variable is 12 months ahead expected IPCA form the FOCUS survey (average). Sources: FOCUS Survey/Haver Analytics, and authors' calculations.



(a) Real GDP Growth



(b) One Year Ahead Inflation

Figure B.4: Banks' Return on Equity (ROA): Public and Large Private banks

Notes: This figure shows the Return over Assets (ROA) by bank type and quarter. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are: Bradesco, HSBC, Itau Unibanco, and Santander. The returns are computed as the last four quarters net income. For each bank type, we compute the ROA as if each type of bank is an institution, that is, the within-bank type sum of net income over the within-bank type sum of assets. The vertical line indicates the start of the intervention. Sources: IF.data, and authors' calculations.

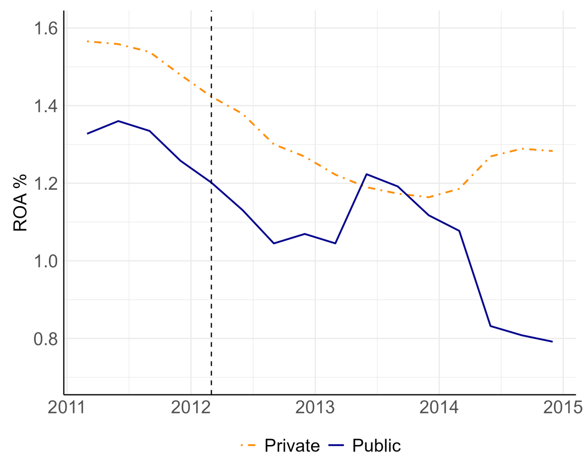


Figure B.5: Share of Loan Origination to Unlevered Firms

Notes: This figure shows the quarterly share of originations for levered vs unlevered firms (at the time of the origination) of working capital loans to firms by type of bank. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are all other banks that are not controlled by the government. The vertical line indicates the start of the intervention. Sources: Credit Information System (SCR), and authors' calculations.

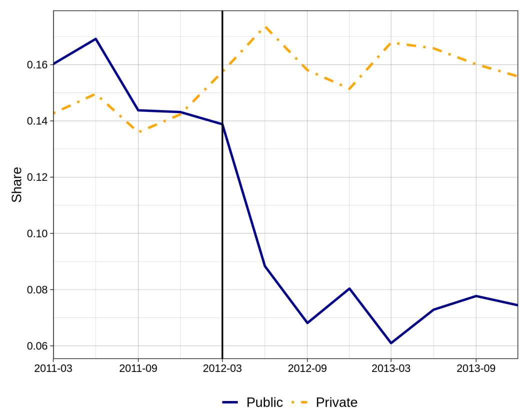
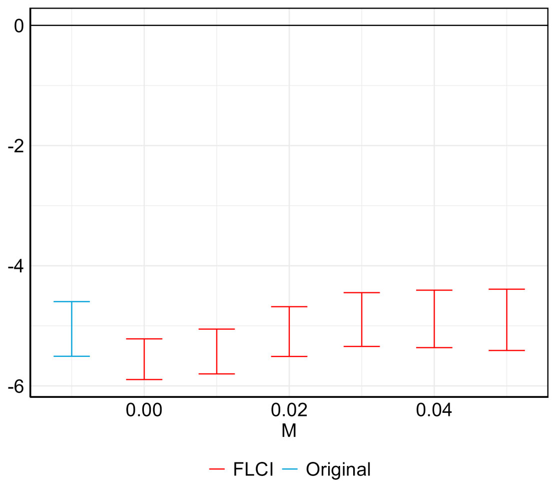
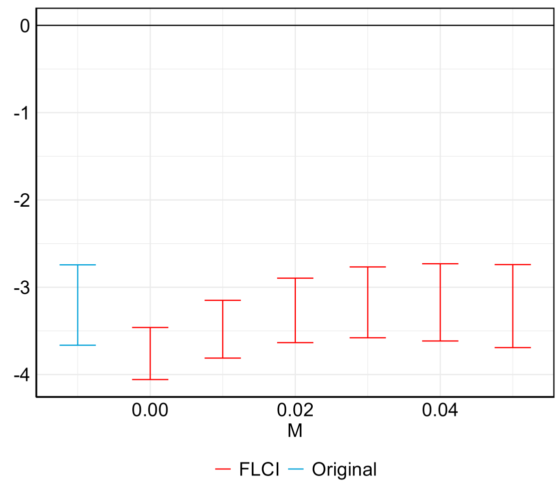


Figure B.6: Private Bank Interest Rates - Parallel Trends

Notes: The figure plots the 95% confidence intervals obtained from the robust pre-trends tests proposed by Rambachan and Roth (2023). We use the *Second Differences* (SD) test, used to assess the robustness of dynamic DiD post-policy effects in the presence of a pre-existing trend. We consider the point estimate of May 2012, the initial drop after the intervention, for these exercises. The light blue confidence interval corresponds to the original point estimate for May 2012 from Figure 3. $M = 0$ considers the point estimates relative to a linear trend. Increasing values of M allow for larger deviations from linearity. **Panel (a)** employs the SD test using the results from the specification that includes bank fixed effects. **Panel (b)** employs the SD test using the results from the specification that includes bank-firm fixed effects.



(a) Bank Fixed Effects



(b) Bank-firm Fixed Effects

Figure B.7: Borrower Delinquency: Public and Private Banks

Notes: This figures shows the estimates of γ_τ from: $I^D_{tmbfs} = \alpha_{tmsf(size)} + \alpha_b + \sum_{\tau \neq -1} \gamma_\tau \cdot Public_b + \varepsilon_{tmbfs}$, where I^D_{tmbfs} is an indicator equal to one if a loan originated in month t in municipality m from bank b by firm f in industry s becomes delinquent within one year after origination, α_{tms} are time-municipality-industry fixed effects, α_b are bank fixed effects, $\alpha_{f(size)}$ are firm-size fixed effects, γ_τ are time dummies, and $Public_b$ is a indicator that is one if b is a public bank. Shaded areas are the 95 confidence intervals. Standard errors are clustered at the bank-municipality level. Sources: Credit Information System (SCR), and authors' calculations.

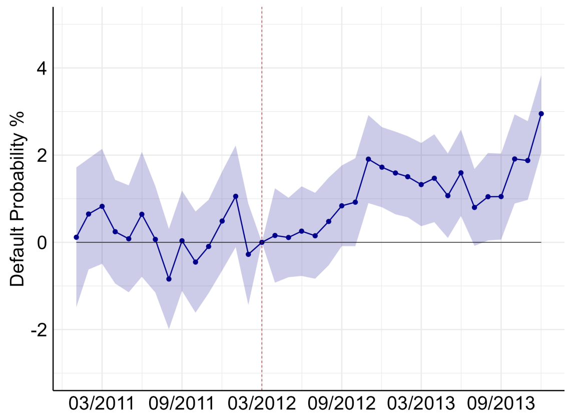


Figure B.8: Public Banks Market Shares and Credit Growth

Notes: This figure visually displays the relationship between public credit growth and our market shares instrument. The y-axis shows the average change in public credit caused by the intervention for each given decile of our market shares instrument. The x-axis shows each distribution deciles of the $Share_m^{Pub,2010}$ instrument. Sources: Credit Information System (SCR), and authors' calculations.

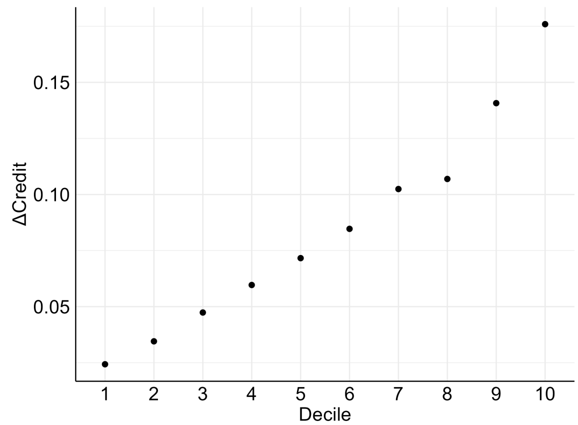
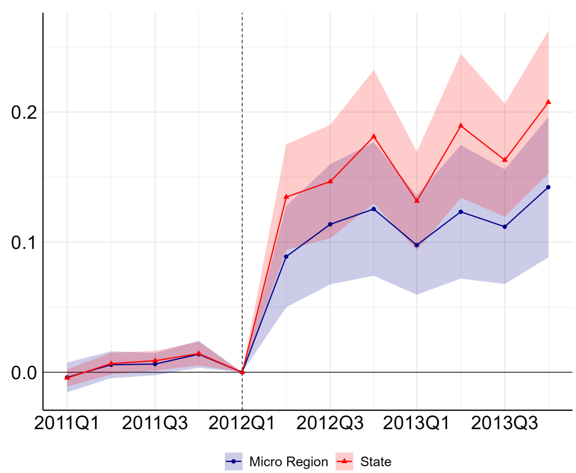
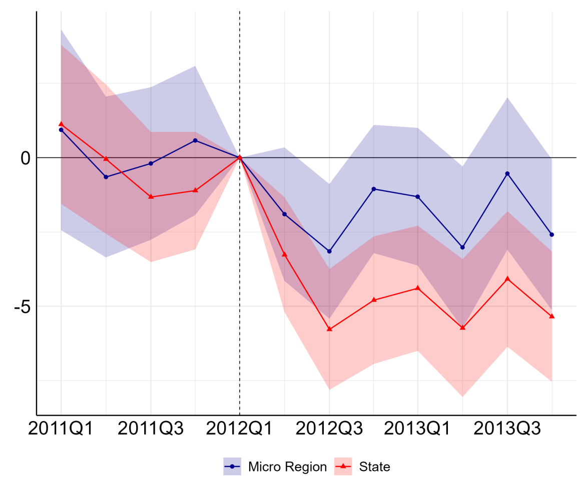


Figure B.9: Alternative Time-Region Fixed Effects

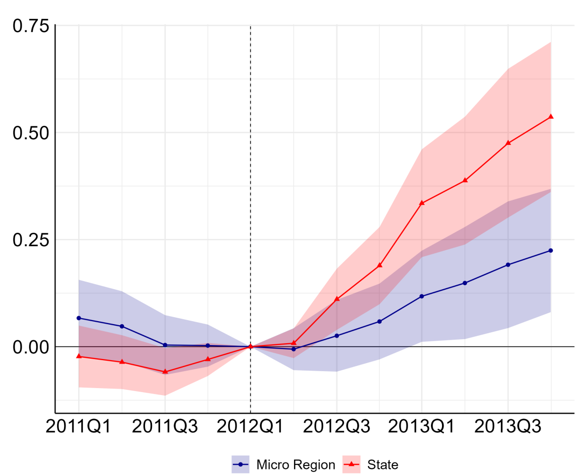
Notes: This figure shows the estimates of γ_τ from Equation 11 at the municipality-quarter level, with 2012Q1 as the reference quarter. The unit of observation is municipality-quarter level. The sample period is 2011Q1 to 2013Q4. **Panel (b)**: The dependent variable is $Credit_{tm}^{Orig,Pub}$, the ratio of total public credit origination in quarter t at municipality m, divided by average total firm credit outstanding in 2011, expressed in decimal units.**Panel (b)**: The dependent variable is $Interest\ Rate_{tm}^{\%,Pr}$, the weighted average private interest rates in quarter t, municipality m, in %. **Panel (c)**: The dependent variable is $debt\ over\ payroll_{tm}$, the weighted average debt over payroll costs in quarter t, municipality m. The sample includes only municipalities that had bank branches at the beginning of 2011. Standard errors are clustered at the municipality level. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.



(a) Credit Supply



(b) Private Interest Rates



(c) Debt over payroll costs

Table B.1: Interest Rate Differences Between Private and Public Banks - Debt

	(1)	(2)	(3)
$Private_b$	9.238***		
	(0.365)		
$Private_b \times Post_t$	-6.958***	-6.321***	-3.202***
	(0.226)	(0.262)	(0.289)
$Debt_{tf}$	1.411***	0.826***	-1.156***
	(0.131)	(0.129)	(0.153)
$Private_b \times Debt_{tf}$	-1.834***	-1.455***	1.438***
	(0.179)	(0.178)	(0.189)
Post $\times$ $Debt_{tf}$	-0.365*	-0.179	1.641***
	(0.156)	(0.142)	(0.174)
$Private_b \times Post_t \times Debt_{tf}$	1.059***	0.836***	-0.141
	(0.194)	(0.177)	(0.253)
Time × Municipality × Industry × Size FE	✓	✓	✓
Bank FE		✓	
Bank × Firm FE			✓
R^2	0.455	0.502	0.849
Observations	2193712	2193712	1683213

Table B.2: Real Effects of Public Credit Growth - Alternative Region Definitions

Notes: This table shows the estimated β's from equation 9, but with alternative definitions for the region considered in the fixed effects. The unit of observation is municipality-year. The sample period is 2011 to 2013, as we do not collect credit origination data for 2010. The dependent variables are: $Log\ GDP_{tm}$, the log of gross local product in year t at municipality m, $Log\ Emp_{tm}$ total employment excluding finance, utility, agricultural and public sector jobs in year t at municipality m, $Log\ Emp^{T}_{tm}$ and $Log\ Emp^{NT}_{tm}$ employment in tradable and non-tradable sectors in year t at municipality m, respectively. $Credit^{Orig,Pub}_{tm}$ denotes the ratio of total public credit origination in year t at municipality m, divided by average total firm credit outstanding in 2011, expressed in decimal units. We follow Montiel Olea and Pflueger (2013) and report the Effective F-statistic, which is calculated with the package 'ivDiag' for R, developed by Lal et al. (2024). Standard errors are clustered at the municipality level. Regressions are weighted by municipality population in 2011. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.

	$Log\ GDP_{tm}$		$Log\ Emp_{tm}$		$Log\ Emp^{T}_{tm}$		$Log\ Emp^{NT}_{tm}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: State FE								
$Credit^{Orig,Pub}_{mt}$	0.0583*	0.0559*	0.1498***	0.1502***	0.2135***	0.2222***	0.1114***	0.1125***
	(0.0318)	(0.0314)	(0.0309)	(0.0310)	(0.0806)	(0.0808)	(0.0225)	(0.0228)
Controls		✓		✓		✓		✓
State-Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.99949	0.99949	0.99938	0.99938	0.99484	0.99485	0.99961	0.99962
Observations	8,346	8,346	8,346	8,346	8,346	8,346	8,346	8,346
Effective F-Statistic	50.30	51.33	50.30	51.33	50.30	51.33	50.30	51.33
Panel B: Micro-region FE								
$Credit^{Orig,Pub}_{mt}$	0.0184	0.0149	0.1970***	0.1934***	0.4003**	0.3942**	0.0224	0.0260
	(0.0830)	(0.0804)	(0.0723)	(0.0711)	(0.1723)	(0.1681)	(0.0506)	(0.0498)
Controls		✓		✓		✓		✓
Micro-region-Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.99965	0.99965	0.99952	0.99953	0.99600	0.99602	0.99970	0.99970
R^2	0.99965	0.99965	0.99952	0.99953	0.99600	0.99602	0.99970	0.99970
Effective F-Statistic	31.86	33.61	31.86	33.61	31.86	33.61	31.86	33.61

Table B.3: Real Effects of Public Credit Growth - Alternative Specification

Notes: This table shows the estimated β's from equation 9. The unit of observation is municipality-year. The sample period is 2011 to 2013, as we do not collect credit origination data for 2010. The dependent variables are: $Log\ GDP_{tm}$, the log of gross local product in year t at municipality m, $Log\ Emp_{tm}$ total employment excluding finance, utility, agricultural and public sector jobs in year t at municipality m, $Log\ Emp^{T}_{tm}$ and $Log\ Emp^{NT}_{tm}$ employment in tradable and non-tradable sectors in year t at municipality m, respectively. $Credit^{Orig,Pub}_{tm}$ denotes the ratio of total public credit origination in year t at municipality m, divided by average total firm credit outstanding in 2011, expressed in decimal units. $Share^{Micro,2011}_{m}$ denotes the share of firms in 2011 at municipality m which are micro firms. $Post_t$ is an indicator variable which equals 1 after 2011. We follow Montiel Olea and Pflueger (2013) and report the Effective F-statistic, which is calculated with the package 'ivDiag' for R, developed by Lal et al. (2024). Standard errors are clustered at the municipality level. Regressions are weighted by municipality population in 2011. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.

	$Log\ GDP_{tm}$		$Log\ Emp_{tm}$		$Log\ Emp^{T}_{tm}$		$Log\ Emp^{NT}_{tm}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Credit^{Orig,Pub}_{mt}$	0.0059	0.0020	0.1450**	0.1426**	0.3033**	0.3057**	0.0485	0.0506
	(0.0676)	(0.0657)	(0.0590)	(0.0582)	(0.1491)	(0.1468)	(0.0372)	(0.0370)
$Share^{Micro,2011}_{m} \times Post_t$	0.0842	0.0879	0.0700	0.0737	0.0012	0.0140	0.0862	0.0889
	(0.1281)	(0.1283)	(0.1106)	(0.1099)	(0.2967)	(0.2946)	(0.0703)	(0.0703)
Controls		✓		✓		✓		✓
Region-Time FE	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.99955	0.99955	0.99941	0.99941	0.99507	0.99509	0.99964	0.99964
Observations	8,346	8,346	8,346	8,346	8,346	8,346	8,346	8,346
Effective F-Statistic	46.60	48.89	46.60	48.89	46.60	48.89	46.60	48.89

C. Regional Allocation of Public Credit

There is empirical evidence that politicians use lending by government banks to influence credit allocation and the real behavior of firms in Brazil (for example, Carvalho (2014), Lazzarini et al. (2015)), a factor that can also be at play during the intervention. At the same time, the outspoken objective of the government with the policy was to address excessive market power of private banks. We explore the allocation of public credit across municipalities to assess these two hypothesis.

First, we measure public credit allocation across municipalities by using the following within-share growth measure:

$$\%\Delta^{within} Orig_m = \frac{1}{2} \cdot \frac{\text{Share Post} - \text{Share Pre}}{\text{Share Post} + \text{Share Pre}}, \tag{14}$$

where shares are computed from public loan origination in the pre- and post-intervention periods. Therefore, what this measure tells us is the change in credit *beyond* what would be expected if credit had a uniform expansion. For instance, if public banks had increased credit by the same percentage in all markets after the intervention, then $\%\Delta^{within} Orig_{pub,m} = 0$ everywhere — and thus the allocation is not systematically geared to borrowers in municipalities controlled by political allies or municipalities where private banks hold more market power. Using this measure, we explore the determinants of the allocation of public lending by estimating the following regression at the municipality level:

$$\Delta^{within} Orig_m = \alpha_r + \beta_1 \cdot HHI_m + \beta_2 \cdot Political\ Alignment_m \tag{15}$$

$$+\beta_3 \cdot Political\ Alignment_m \times Contest_m + \beta_X \cdot \mathbf{X}_m + \varepsilon_m \tag{16}$$

Where $\Delta^{within} Orig_m$ is the within-share growth measure for public credit, $Political\ Alignment_m$ is an indicator variable for municipalities whose elected mayor is politically aligned with the federal government, $Contest_m$ is an indicator variable which equals 1 for contested elections, and α_r are meso-region fixed effects and $\mathbf{X}_m$ is a vector of controls. We follow Kumar (2020) and set $Contest_m$ equal to 1 if the margin of victory of the elected mayor was smaller than 5%.[22] This interaction term captures the idea that political incentives to increase credit are larger in municipalities where elections are more competitive. Finally, we also include the HHI of private firm credit in municipality m in 2011 to account for the possibility that public banks target municipalities based on the perceived market power private banks held in these markets.

We use two different definitions political alignment: First, we use an indicator variable $Political\ Alignment_m$ which equals 1 if the elected mayor of municipality m is in workers' party, the same party as the president. Second, we augment the measure of political alignment by defining an indicator variable $Coallition_m$ which equals 1 if the mayor belongs either to the workers' party, or to PMDB, the party of the vice-president. By doing so we account for the possibility that broader alignment with the executive power might be driving the allocation of public credit. We estimate equation 15 for mayoral elections of 2008 and 2012. The vector of controls $\mathbf{X}_m$ contains municipality characteristics that can also affect allocation, such as lagged GDP and employment growth, and the percent share of industry and services over total value added, all as of the baseline year of 2011.

The results are shown in Table C.1. Throughout all specifications private HHI in 2011 has a strong influence over public banks' credit allocation during the intervention. In comparison, mayoral political alignment does not seem to influence the allocation of public credit. These results are consistent across both elections, and hold even when accounting for contested elections. Therefore, we find no evidence that political capture is driving any of the results in the main text. Instead, the allocation appears to be geared towards areas where private banks hold more market power.

[22]Municipalities with fewer than 200,000 residents do not have second rounds for mayoral elections. Hence, simple voting majority in elections at these municipalities is sufficient for a candidate to be declared mayor.

Table C.1: The Allocation of Public Credit

Notes: This table shows estimates of equation 15. $\Delta Credit_{mt}^{Pub}$ denotes the within credit variable defined in Equation 14. HHI_m denotes the HHI of private credit, calculated using firm credit information obtained from SCR. $Same\ Party_m$ and $Coallition_m$ are an indicator variable which equal 1 for municipalities whose mayor is aligned with the president's party or with either the president or vice-president's party, respectively. $Contest_m$ is an indicator variable which equals 1 if the margin of victory of the elected mayor was smaller than 5%. Columns 1 through 4 define the political capture variables based on outcomes of the 2008 municipal election. Columns 5 through 8 define the political capture variables based on outcomes of the 2012 municipal election. Standard errors are clustered at the municipality level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), IBGE, and authors' calculations.

	$\Delta Credit_{mt}^{Pub}$							
Election Year	2008 Elections				2012 Elections			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HHI_m	0.0389*	0.0390*	0.0409**	0.0411**	0.0384*	0.0382*	0.0374*	0.0369*
	(0.0200)	(0.0201)	(0.0200)	(0.0201)	(0.0201)	(0.0201)	(0.0202)	(0.0202)
$Same\ Party_m$	-0.0537		-0.0457		0.0143		-0.0284	
	(0.0397)		(0.0463)		(0.0340)		(0.0424)	
$Coallition_m$		0.0114		0.0087		0.0142		0.0410
		(0.0274)		(0.0322)		(0.0280)		(0.0339)
$Contest_m$			0.0518*	0.0506			-0.0378	0.0084
			(0.0275)	(0.0330)			(0.0307)	(0.0281)
$Same\ Party_m \times Contest_m$			-0.0046				0.0978	
			(0.0779)				(0.0691)	
$Coallition_m \times Contest_m$				0.0115				-0.0648
				(0.0581)				(0.0622)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Region fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.19333	0.19207	0.19521	0.19431	0.19209	0.19214	0.19350	0.19305
Observations	2,796	2,796	2,796	2,796	2,797	2,797	2,797	2,797