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Government Banks and Interventions in Credit Markets

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Abstract:
We study a large-scale quasi-experiment in the Brazilian banking sector characterized by an unexpected and macroeconomically relevant increase in lending by commercial government banks. Using credit registry data, we find that this intervention led to a reduction in lending rates, but it did not lead to a change in private banks' credit supply. Firms reliant on government banks experienced a substantial increase in debt, and government banks faced a large increase in loan defaults driven by indebted firms. We find a small increase in employment at the firm level, suggesting limited direct benefits of the intervention. At the regional level, we find that branch presence cannot explain credit growth due to cross-market borrowing. Once we account for this channel, we find real effects at the regional level that are substantially larger than those at the firm level, emphasizing the general-equilibrium effects of large-scale interventions.

JEL Classifications: E44, E65, G21, G28
Keywords: credit market interventions, credit supply shocks, government banks

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1 Introduction

Government interventions to influence the equilibrium in credit markets are ubiquitous, and one of the tools used to intervene in credit markets are government banks.[1] The literature identifies benefits of interventions based on an increase in government lending, such as preventing a credit crunch during a financial crisis or counterbalancing the role of information asymmetries. On the other hand, government interventions can lead to misallocation, be subject to political capture, and sustain unproductive firms.[2] Nevertheless, there is no theoretical or empirical consensus on how increases in credit supply by government banks affect financial and real outcomes or on through which mechanisms these effects work, especially outside of crisis episodes.

We address these questions in a novel setting that exploits an unexpected and large-scale credit market intervention in Brazil. In March 2012, the Brazilian government announced, and shortly thereafter implemented, an increase in the credit supply of two of its largest commercial banks: Banco do Brasil (BB) and Caixa Economica Federal (CEF). Together, these two government banks were responsible for 38 percent of the outstanding credit in Brazil before the intervention. This intervention was targeted at small and medium-sized enterprises (SMEs), and its objective was to increase credit at low interest rates at the government banks, which, it was presumed, would lead to a reduction in interest rates and an increase in credit access at private banks. The program was massive; the volume of outstanding credit from these two government banks increased approximately 30 percent in 2012, while the volume of outstanding credit from the largest private banks increased only 11.5 percent.

Our setting is ideal for studying the mechanisms and effects of a large-scale intervention in credit markets implemented through government banks. First, Brazil is a great laboratory in which to study credit market interventions. At that time in Brazil, bank lending represented nearly 52 percent of external finance, close to the current international average of 55 percent. Second, SMEs, which were the focus of the intervention and are the focus of our analysis, play a key economic role in terms of aggregate growth and employment. These firms account for 60 to 70 percent of employment worldwide (Ayyagari, Beck and Demirguc-Kunt, 2007)

[1] Throughout this paper, we use the expressions *state-owned banks*, *government banks*, and *public banks* interchangeably. They refer to banks that have a local or federal government as their majority shareholder and can lend directly to households and firms. We refer to banks that are not government banks as *private banks*.

[2] Papers highlighting a beneficial view of government interventions in financial markets include Stiglitz (1994), Tirole (2012), Philippon and Skreta (2012), and Coleman and Feler (2015), among others. Papers that document a negative role for the state include Bertrand, Schoar and Thesmar (2007), Carvalho (2014), Acharya et al. (2019), Acharya et al. (2020), and Garber et al. (2021), among others.

and the majority of job creation in the United States (Neumark, Wall and Zhang, 2011). Third, we can isolate the specific mechanisms that affect credit allocation and firm outcomes, given that our experiment occurs outside of a crisis episode and thus in a setting where firms and banks are not subject to any other systematic shocks. Finally, we have access to rich administrative credit registry data that are matched with employer-employee data, enabling us to provide comprehensive evidence of the financial and real consequences of the intervention.

We begin our empirical analysis by focusing on the effects of the intervention on lending rates and loan originations by both government and private banks. Throughout the paper, we focus mainly on working capital loans, which are term loans that can be used for any purpose and represent one of the main sources of funding for firms in the Brazilian economy. Government banks had lower lending rates both before and after the intervention, and the intervention was not characterized by large reductions in the interest rates of working capital loans made by government banks. Instead, the main effect of the intervention was a sudden increase in the supply of credit by government banks. Working capital originations grew more than threefold after the intervention was announced.

Second, we turn to the response of private banks, relying on the fact that the intervention was unexpected and that no other systematic events that could cause changes in the behavior of private banks occurred during that period. Private banks responded to the increase in public banks' credit supply by reducing their lending rates. As a consequence, the difference between the lending rates of public and private banks fell about 3 percentage points. This drop in interest rates corresponded to 20 percent of the pre-intervention difference between private and public banks' lending rates. Although we do find a reduction in the lending rate, we do not find an aggregate increase in the credit supply of private banks during the same period. In fact, we find that firms that borrowed exclusively from public banks experienced a substantially larger growth of their outstanding debt after the intervention relative to firms that borrowed exclusively from private banks. For firms that borrowed from both private and public banks, we find evidence of a smaller increase in their debt-to-payroll ratio relative to those that borrowed exclusively from public banks, and a substantial reduction in their debt outstanding with private banks, indicating that there was some crowding out of private credit.

We then compare the delinquency rates on the government and private banks' loan portfolios, and their evolution reveals two noteworthy patterns. First, the intervention was associated with an increase in the delinquency of loans originated by government banks relative to those originated by private banks. After the intervention, the probability that a loan from a government bank would become delinquent was, on average, 100 basis points higher

than the delinquency probability of a loan originated by private banks, implying a 20 percent higher probability of delinquency for firms borrowing from public banks relative to firms borrowing from private banks. Second, we find that this relative increase in delinquency was entirely driven by levered firms. The first finding suggests that adverse selection is not a main driver of borrower riskiness in our context. Given the difference between the lending rates of public and private banks, government banks would have attracted safer borrowers in the presence of adverse selection. Our second finding suggests that government banks did not relax their credit standards for new borrowers as part of the intervention, since new borrowers from both public and private banks had similar delinquency rates. Our results suggest that public banks eased their lending standards for and favored previously levered firms. These firms eventually became delinquent, leading to a deterioration in the public banks' loan portfolio.

Next, we explore the real effects associated with the intervention at the firm level. The analysis of the real effects is complicated by the fact that borrowers from public banks faced increased credit availability, while borrowers from private banks faced a decrease in the cost of their new loans. At the firm level, we perform three comparisons. First, we compare borrowers that had exclusive relationships with public banks with those that had exclusive relationship with private banks throughout the sample. Second, we compare borrowers that had relationships with both types of banks with those that had exclusive relationship with private banks throughout the sample. In both of these comparisons, we find an increase in employment and payroll growth of 1.65 to 1.8 percent in 2013. Finally, we compare new borrowers from public banks with firms that held no debt throughout the sample period. For the former, we do not find any significant change in employment growth relative to latter. Overall, the employment effects we document at the firm level are roughly one-third of the estimates for these effects in the credit supply literature, and they are small given the size of the intervention.

Finally, we also explore the effects of the policy at the regional level. We first focus on a set of municipalities that allows for the cleanest identification of the intervention and its effects. This sample consists of local banking markets that had no new entries of previously absent banks and that are either public or private monopolies (that is, have branches from only one bank). Our identifying assumption is that branch presence in the baseline and exposure to the intervention were independent of changes in credit demand following the intervention. Since bank entry into most of these municipalities was the result of bank privatization processes that took place several years before the beginning of our analysis, it is unlikely they are correlated with changes in current economic conditions. We find that following the intervention, originations of working capital loans for firms in municipalities

with public bank branches grew substantially. We also find that credit outstanding from branches in those municipalities grew substantially. Consistent with the aggregate data, credit outstanding from branches was approximately 20 percentage points higher in public relative to private monopolies.

We do not find, however, that the volume of credit outstanding for firms in public monopolies grew more than for those in private monopolies. This is a direct consequence of the fact that firms in the latter borrowed from public banks. Not surprisingly, we do not find any effect on real outcomes of having a public bank branch when the intervention occurred. We address this cross-market borrowing channel by using an alternative measure of exposure to the intervention. This measure is the share of the outstanding volume of working capi-tal loans from public banks in a given municipality at the baseline. Using this alternative measure, we find that regions where all of the outstanding working capital loans were from public banks before the intervention experienced a 63 percent higher credit growth and a 4.65 percent higher GDP growth relative to regions where none of the outstanding working capital loans came from public banks before the intervention. We also find significant effects in terms of employment and payroll, emphasizing the general-equilibrium effects of large-scale interventions. Although these effects at the regional level are larger than those at the firm level, they are smaller than those estimated in the credit supply literature. Herreño (2020), for instance, finds a 0.2 elasticity of aggregate output to a credit supply shock.

Related Literature This project adds to the broad literature that studies government interventions in financial markets.[3] Closer to our paper, Jiménez et al. (2019) analyze a small credit facility of a Spanish state-owned bank during the 2008–2009 financial crisis. The authors find that although this facility attracted riskier borrowers, the social value of such intervention during the crisis was still positive. Our project complements their empirical evidence in two major ways. First, the intervention we analyze is not a response to a crisis episode, and there were no other large shocks affecting the decisions of banks and borrowers. Second, the intervention we study was macroeconomically large, such that we can account for the response of private banks, endogenous changes in the pool of borrowers, and aggregate real effects of the intervention. We show that large-scale government-induced credit expansions outside of economic downturns have limited direct and indirect real effects, and we cannot rule out the idea that the intervention led to credit misallocation.

[3]See, for instance, Bertrand, Schoar and Thesmar (2007), Veronesi and Zingales (2010), Acharya et al. (2020), and Acharya et al. (2021). For government banks in particular, see La Porta, Lopez-De-Silanes and Shleifer (2002), Sapienza (2004), Dinç (2005), Iannotta, Nocera and Sironi (2013), Assuncao, Mityakov and Townsend (2012) Bertay, Demirgüç-Kunt and Huizinga (2015), Ru (2018), Cao et al. (2022), and others. For government banks in Brazil, see Coelho, De Mello and Rezende (2013), Carvalho (2014), Lazzarini et al. (2015), Sanches, Silva Junior and Srisuma (2018), and others.

Our paper also contributes to the literature on credit supply shocks. The empirical literature focuses primarily on negative credit supply shocks (for example, Khwaja and Mian (2008), Chodorow-Reich (2014), and many others), especially following the Great Financial Crisis. Understanding the role of positive credit supply shocks is important, as there are theoretical reasons why their effects would be different from those of credit crunches, and booms are the best predictors of financial crisis (for example, Freixas, Laeven and Peydró (2015)). One notable exception is Jiménez et al. (2020), who study a credit boom in Spain and find no increase in credit supply at the firm level and, as a consequence, no real effects as well. Contrary to Jiménez et al. (2020), we provide novel evidence of the strength of the real effects of credit booms in a quasi-experimental setting where credit, in fact, grows significantly at the firm level.

Our paper also contributes to the regional banking literature. The evidence on credit supply shocks at levels of aggregation above the firm is still mixed (for example, Mian, Sufi and Verner (2019) and Nguyen (2019) in the United States and Huber (2018) in Germany). As argued in Ashcraft (2005), Huber (2018), and others, one reason for this finding is that there is no heterogeneity in regional exposure to large, systemic shocks. Our evidence suggests that this is indeed the case in our setting when exposure is measured by physical branch presence, but that once we also account for firms' locations, we can estimate the effects of this particular large-scale intervention. More broadly, our evidence suggests that even if small-business lending is mostly local (for instance, see Granja, Leuz and Rajan (2018)), when analyzing large shocks, it is fundamental to account for the location of both the bank and the borrower, as well as the potential for the expansion (or contraction) of credit markets as a function of the shock itself.

Our paper is also related to the literature on the Brazilian banking sector. Several papers study the use of these government banking institutions during the Great Recession to prevent a credit crunch, including Coleman and Feler (2015) and Cortes, Silva and Van Doornik (2019). Capeleti, Garcia and Sanches (2022) study the asymmetric effects of procyclical and countercyclical expansions of public banks' credit on economic growth. Garber et al. (2021) show that the same expansion in credit that we study led to an increase in household debt that ultimately led to reduced consumption during the 2014–2016 economic downturn. Our paper complements the evidence in Garber et al. (2021) and Capeleti, Garcia and Sanches (2022) by focusing on the mechanisms and inner workings during the credit expansion and on loans made to firms. We take into account the response of private banks and connect the increase in credit supply to changes in delinquency and real effects of the intervention at the firm and regional levels before the economic downturn.

2 Data Sources

Our main source of data is the confidential credit registry data from the Brazilian central bank matched with employment and payroll data from the Annual Review of Social Information (RAIS). We complement these matched data with publicly available data from various sources as outlined below.

Credit registry data are 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 specific threshold (at the time of origination),[4] allowing us to observe the near universe of loans to firms in Brazil. The database includes detailed information about loan contracts, such as the type of credit, interest rate, amount, maturity, and collateral, as well as some basic information at the firm level (such as firms' time-invariant taxpayer identifiers). We restrict the analysis to loans funded by banks' own resources.[5,6] These data also allow us to track delinquency and firms' credit history. Given that loan identifiers are not constant across time, we track delinquency information at the firm-month of origination-loan type-bank dimension, up to one year after origination.[7] Following Jiménez et al. (2014), we define a loan as delinquent if it is more than 90 days past due.

The firm-level employment and payroll data that we use in our analysis are from the Annual Review of Social Information (RAIS). All tax-registered firms in Brazil are required to complete a survey in which they provide individual labor contract information for each of their employees. Because of the severe penalties firms face for incomplete or late filings of the survey, the RAIS covers the universe of all tax-registered firms. We aggregate these data at the firm-employee level to obtain employment and total payroll at the firm level. We merge the SCR and RAIS data based on the firm's time-invariant taxpayer identifier. Each data set contains firms without a correspondent in the other data set. Not all firms have access to credit and/or decide to borrow in a given year (are in the RAIS but not the SCR), and non-employer firms that do borrow are in the SCR but not in the RAIS. The latter firms

[4] R$ 5,000 (around $2,500) until December 2011; BRL 1,000 (around $500) from January 2012 onward.

[5] We construct a time series of loan originations by looking at all loans originated in a given month that have a positive amount outstanding at the end of that month. While we exclude very short-term loans in the process, the majority of the corporate loans have maturities of more than one month.

[6] We can separate loans funded by banks' internal resources (deposits and capital) and external resources. This distinction is important since development banks in Brazil fund a significant number of loans using commercial banks as intermediaries (see, for instance, Lazzarini et al. (2015)).

[7] Our approach to measuring delinquency is comprehensive, despite the constraints, for two reasons. First, most of the firms in our sample are small and thus have a unique month of origination-loan type-bank loan in the sample. Second, our definition of borrower quality reflects lenders' information for a given firm for a given type of credit, which is the economically relevant dimension.

correspond 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. We use employment headcount to construct a variable of firm-size categories.[8]

Throughout the paper, we also use other publicly available data with information on banks' balance sheets, branch locations, and regional variables at the municipal level. Banks' 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 containing detailed information about assets and liabilities at the branch level are available at a monthly frequency from the Monthly Bank Statistics by Municipality (ESTBAN). ESTBAN data also include the municipality of each branch and thus allow us to identify entries and exits of banks in each municipality. Finally, we use population and output data from the Brazilian Institute of Geography and Statistics (IBGE).

There are five types of corporate loans that most commercial banks provide using their own funding sources: working capital, discounted receivables (loans where firms anticipate the receipt of cash flows from sales and other accounts receivables), auto loans, credit cards, and overdraft accounts. Our paper focuses primarily on working capital loans for three reasons. First, they were, together with discounted receivables, the focus of the intervention. Second, working capital loans are the primary source of funds for firms, accounting for roughly 50 percent of the loan volume in our sample before the intervention (March 2012) and 60 percent by the end of 2013. Third, they have longer maturities than discounted receivables, which allows us to track borrower delinquency over time more accurately. Within the set of working capital loans, we focus primarily on the uncollateralized ones. These are the majority of working capital loans in our sample, and we do not observe accurate collateral information to properly account for the riskiness and cost of the collateralized working capital loans.

Table 1 shows borrower summary statistics from our data. Panel A illustrates how large the differences are between the interest rates charged by private banks and those charged by public banks. In particular, interest rates for working capital loans issued by public banks are more than 10 percentage points lower than interest rates charged by private banks. Loans issued by public banks are also smaller in size and have longer maturities. Panel B provides

[8] In particular, we follow the classification by Brazilian Support Service for Small and Medium Enterprises (SEBRAE). Micro firms: firms in the service/commerce sectors with fewer than 10 employees, or in the industry sectors with fewer than 20 employees. Small firms: firms in the service/commerce sectors with more than 10 and fewer than 50 employees, or in the industry sectors with more than 20 and fewer than 100 employees. Medium-sized firms: firms in the service/commerce sectors with more than 50 and fewer than 100 employees, or in the industry sectors with more than 100 and fewer than 500 employees.

a breakdown of firm characteristics based on their relationships over the whole period. Firms that borrow from private banks only are larger than firms borrowing exclusively from public banks, on average, and have more debt outstanding. Firms with access to both types of banks are larger, consistent with the notion that such firms benefit less from exclusive relationships with banks.

3 Institutional Setting and Intervention Details

After the Great Financial Crisis of 2008 and 2009, the Brazilian economy experienced a rapid economic recovery, with the country's GDP growing 7.5 percent in 2010 and 4.3 percent in 2011 (Figure A.1, Panel (a)). Dilma Rousseff assumed the presidency in January of 2011 and increased the number of policies geared toward avoiding an economic slowdown. Monetary policy became expansionary, with the policy rate going from 12.50 percent in July 2011 to 7.25 percent in October 2012 (Figure A.1, Panel (b)). As evidenced by the summary statistics of our data (Table 1), lending rates in Brazil were high in our sample period. These rates (and the implied spread over the deposit rate) were high even when compared with those of other developing countries. The lending spread in 2011 was 32.9 percentage points in Brazil, compared with 3.4 percentage points in Argentina and 3.7 percentage points in Mexico, for example.[9] Even following the reduction in policy rates after July 2011 and other regulatory changes, rates for consumers and small businesses stayed at high levels, which led the government to further intervene in the Brazilian banking sector.

In March 2012, the government announced that it would use two state-owned banks—Banco do Brasil (BB) and Caixa Economica Federal (CEF)—to promote credit supply increases for several types of loans, both to consumers and firms, at subsidized interest rates.[10] These actions were taken through various separate government programs. Two prominent examples are "Bom pra Todos," which was implemented by BB, and "Caixa Melhor Credito," implemented by CEF. As these programs were large and broadly unexpected, in the two weeks following their announcement, BB stock prices fell 7.62 percent.[11] These programs were focused on consumer products and working capital and other short-term loans for micro/small firms. Initially, both BB and CEF had balance sheet capacity to increase their credit supply and originate these loans at lower lending rates, as the reduction in margins

[9]Source: IMF International Financial Statistics

[10]State-owned banks were, and still are, large players in the Brazilian financial sector. Although there are specific differences in how these banks are managed and in their ownership structures (for instance, BB has publicly traded shares, whereas CEF does not), both institutions are under the control of the Brazilian government and can be actively used as a means to implement credit policies.

[11]Caixa Economica Federal is not publicly traded.

was being compensated for by increases in volume. By the end of 2012, outstanding credit from public banks had grown approximately 30 percent, compared with an increase of 11.5 percent in outstanding credit from private banks.

The reasoning behind the intervention was that by increasing the amount of credit provided by government banks and charging lower interest rates, the government would successfully increase the competitive pressure on private banks. Achieving lower interest rates was a fundamental goal for Brazil's economic policymakers, who held the belief that lower interest rates were necessary for sustainable economic growth and would prevent a slowdown of economic activity.[12] Importantly, we do not find that the intervention was motivated by political concerns (see, for instance, the net approval rating in Figure A.1, Panel (c)), high stock prices or large exchange movements (Figure A.1, Panels (e) and (d)), inflation expectations (Figure A.1, Panel (f)), or other macroeconomic factors. In Appendix B, we discuss the heterogeneity in credit growth across municipalities. We show that funds were not disproportionately allocated to municipalities with mayors from the party of the president, even though a mayoral election would be taking place in October 2012. We do find a higher than expected allocation in municipalities where the previous mayoral elections were close, but this effect is smaller relative to the overall credit growth in the intervention and present for less than 2 percent of the municipalities in our sample. Moreover, we show that credit growth differences between municipalities with higher industrial or agricultural shares, more or less private bank credit concentration, or with higher GDP or credit per capita were also small relative to the overall increase in credit from the intervention.

By mid-2013, however, macroeconomic conditions changed, and banks believed that a significant tightening of financial conditions was on the horizon. Government officials indicated that the public banks were no longer able to keep the same pace of credit expansion due to lack of balance sheet capacity and risk of default.[13] Although the intervention does not have an official end date, we focus our analysis on the period from 2011 to 2013. By the end of 2013, public and private banks had started increasing lending rates, and public banks had significantly reduced the pace of their credit supply expansion (Figure A.4).

The effects of the intervention, and its undoing, can be directly observed in banks' balance

[12]Former President Roussef was particularly unhappy with the high interest rates. For instance, in her 2012 Labor Day speech, which took place after the intervention, she said, "The Brazilian economy will only be completely competitive when our interest rates (...) match the interest rates employed in international markets (...) It is unacceptable that Brazil, which has one of the most stable and profitable financial sectors in the world, continues to have one of the highest interest rates (...) government banks proved that it is possible to reduce interest rates in loan operations, credit cards and even payroll loans. It is important that private banks follow suit." Source: https://g1.globo.com/economia/noticia/2012/04/dilma-critica-altas-taxas-de-juros-e-diz-que-bancos-tem-logica-perversa.html

[13]For instance, see https://www.valor.com.br/financas/3017518/governo-ve-limite-para-bb-e-caixa and https://www.valor.com.br/financas/3023666/bancos-federais-chegam-ao-limite-da-baixa-de-juro.

sheet data. In Figure 1, Panel (a), we show the change in the volume of outstanding credit (relative to March 2012) issued by the two public banks in the program (BB and CEF) and the five largest private banks in Brazil.[14] We observe a significant credit expansion from public banks. We do not find a similar increase in credit from private banks. In Figure 1, Panel (b), we show the change in the other assets in banks' balance sheets. We do not observe that the intervention-driven credit increase was associated with a contraction of other parts of public banks' balance sheets or a differential trajectory of other assets between public and private banks.

We find that this increase in credit was funded by various sources. In Figure A.5, we show that there was a larger increase in deposits and no differential increase in equity at public banks relative to private banks, suggesting that the intervention was partly funded through deposits. However, we show in Figure A.6 that the share of deposits relative to total liabilities fell for both types of banks, and the asset increase was mainly funded through a mix of onlending (mostly of government funds) and security issuance. Although BB and CEF are controlled by the government, both banks were profitable (from an accounting perspective) before the intervention, and their return on assets was in line with those of banks in comparable economies.[15] In Figure A.7, we see that a few quarters after the intervention, there was a large increase in public banks' ROA due to their increased lending activity. This trend was reversed when the economy slowed and loan delinquencies increased.

The effects of the intervention can also be observed in new originations in the credit registry data. In Figure 2, Panel (a), we see a large jump in originations of working capital loans right after the beginning of the intervention. Despite a sudden and large increase in government banks' lending, we do not observe an immediate large reduction in the volume of loans originated by private banks. Figure 2, Panel (b) shows the average interest rates of working capital loans from public and private banks. The notion that government banks were able to provide loans at lower interest rates is evident in Figure 2, Panel (b). We can see that government banks provided substantially cheaper credit relative to private banks, both before and after the intervention. Moreover, we observe a large decrease in the interest rates of private banks right after the beginning of the intervention. Despite the decline, the difference in interest rates between private and government banks remained large after the intervention, with private loans being, on average, 12.8 percentage points more expensive

[14] We use only the five largest private banks so that we have a comparable group of financial institutions. Together, these seven financial institutions are responsible for more than 80 percent of the volume of credit outstanding in the baseline in our data.

[15] For instance, the average ROA of banks in Chile was around 1.5 percent in 2012, while the joint ROA of BB and CEF was 1.2 percent. The average across all OECD countries during the period was around 0.5 percent. Source: IMF's Global Financial Development database

before the intervention and 7.4 percentage points more expensive after the intervention.[16]

As a consequence of the increase in new originations, we also observe in the credit registry data an increase in the volume outstanding of working capital loans from public banks (Figure 3, Panel (a)). Consistent with our narrative description of the intervention, the volume outstanding of working capital loans grows at a rapid pace until the end of 2012 and then slows in 2013. In Figure 3, Panel (b), we show that there was also an increase in the volume outstanding of discounted receivables, but the scale of the increase was 2.5 percent of the increase in the volume outstanding of working capital loans. The aggregate evidence suggests private banks responded by reducing interest rates but not increasing (and, if anything, decreasing) their credit supply. In the next section, we confirm these results, exploiting the richness of the credit registry data.

4 Lending Rates, Firm Debt, and Delinquency

In this section, we estimate the effects of the intervention at the loan and firm levels. We focus on the effects on lending rates, firm debt, and delinquency.

4.1 Interest Rates and Debt

At the core of the intervention was the government belief that private banks would respond to the additional competition from their public counterparts by reducing interest rates on their loans. The aggregate evidence in Figure 2 indicates that there was a reduction, but it was not enough to bring the difference between public and private banks' interest rates to zero. These aggregate differences can reflect borrower or loan characteristics of private and government banks that differ or change in response to the intervention. To account for this possibility, we focus on individual loan issuance and compare loans issued by private and government banks before and after the intervention while controlling for firm and contract-specific features and a broad range of fixed effects.

Our setup resembles a difference-in-differences specification, but one in which both types of banks were affected by the intervention. Although this can pose an additional hurdle for identification of the effects of the intervention, the context of our analysis allows us to confidently state that there were no 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 macroprudential policies that would affect different banks

[16] We also find evidence that public banks price-discriminate based on firm size less than private banks do, as can be seen in Figure A.9 and Table A.4. This dispersion in financing spreads can also have implications for financial development (see Cavalcanti et al. (2021)).

differently. Furthermore, the absence of a financial crisis means we do not have to worry about the different behavior of private and government banks—or their borrowers—during such episodes. Therefore, our identification assumption is that, given the absence of any systematic shocks that hit private and government banks differently, changes in the difference between private and government banks' interest rates were caused by the intervention. Formally, we estimate Equation (1) at the loan level:

$$i_{jtmbfs} = \alpha_{tms} + \alpha_{bf} + \alpha_{t,j(maturity)} + \alpha_{t,f(size)} + \sum_{\tau \neq 0} \delta_\tau \ Private_b + \varepsilon_{jtmbfs} \tag{1}$$

where i_{jtmbfs} denotes the interest rate of a loan j issued in month t in municipality m by bank b to firm f in industry s. $Private_b$ is a dummy equal to one if bank b is a private bank, α_{tms} are time-municipality-industry fixed effects, α_b are bank fixed effects, $\alpha_{t,j(maturity)}$ are time-maturity fixed effects, and $\alpha_{t,f(size)}$ are time-firm-size fixed effects. For loan maturity and firm size, we bin the underlying continuous variables in several different categories. For firm size, we use the micro, small, medium, and large definitions from Section 2. For loan maturity, the bins are one to three months, three to six months, six to nine months, nine to twelve months. For maturities of more than a year, we create six-month bins until 42 months and one final bin for working capital loans with maturities longer than 42 months. 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. The use of a broad range of fixed effects guarantees that we are comparing loans in the same region and month and for firms in the same industry that have the same size, and that firm-bank specific characteristics are also accounted for. Additionally, time-maturity fixed effects guarantee that we are comparing loans with the same maturity. In summary, our specification guarantees our analysis is not capturing changes in the composition of banks' loan portfolios in response to the intervention. We also estimate a version of Equation (1) with firm-time fixed effects, as in Khwaja and Mian (2008). This alternative specification accounts for firms' credit demand by comparing the lending rates from different bank types for the same firm, but it severely limits the sample in our analysis since it requires that the SMEs in our sample originate working capital loans from more than one type of bank in a given quarter.

The results are shown in Figure 4. The results in Figure 4, Panel (a) indicate that private banks' lending rates fell sharply relative to those of government banks after the intervention. The spread between private and public banks' interest rates fell about 2.7 percentage points on average in the post-intervention period.[17] This is a reduction of about 20 percent of the

[17]We also find suggestive evidence that the interest rate reduction caused by the intervention was larger for micro firms relative to small, medium-sized, and large firms, as seen in Table A.4).

pre-intervention difference between private and public interest rates. We find a quantitatively similar result if we estimate Equation (1) with firm-time fixed effects (Figure 4, Panel (b)), indicating that our benchmark set of fixed effects can account for credit demand.

Next, we focus on the effects of the intervention on firm debt. We use all types of debt outstanding to capture potential substitution between working capital loans and other types of credit. Since we do not have balance sheet information, we use payroll as a measure of firm size. Specifically, we define the debt-to-payroll ratio as a firm's outstanding debt divided by its payroll costs in 2011. 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, as in Equation (2):[18]

$$\frac{\text{Debt}_{tf}}{\text{Payroll}_{2011,f}} = \alpha_t + \alpha_f + \sum_{\tau \neq 0} \gamma_\tau \cdot Public_f + \varepsilon_{tf}, \tag{2}$$

where the dependent variable is the outstanding debt of firm f in month t relative to its total payroll in 2011, α_t and α_f are time and firm fixed effects, $Public_f$ is an indicator that is one if firm f is a borrower from a public bank, and γ_τ are the coefficients of interest. We estimate Equation (2) for two different samples. First, we consider firms with exclusive relationships with private and public banks throughout the sample period. Second, we compare firms with non-exclusive relationships with private banks with those that have exclusive relationships with private banks. For the latter sample of firms, we also estimate Equation (2) with debt originated by private banks relative to payroll as a dependent variable. This allows us to test if borrowers with non-exclusive relationships reduce their reliance on private debt after the intervention, relative to borrowers with exclusive private relationships. For both samples, we restrict our analysis to a balanced panel of firms to avoid picking up changes in the composition of the pool of borrowers.

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 (Panel a). The debt-to-payroll ratio of firms that borrowed only from government banks increases substantially relative to firms that borrowed only from private banks. The coefficient estimate of 1.14 in December 2013 indicates that firms borrowing from public banks experienced an average increase of 1.14 times their annual payroll relative to those that borrowed exclusively from private banks. For reference, firms that borrowed exclusively from public banks had a baseline average debt-to-payroll ratio of 4.14 in March

[18] We opt to estimate this regression relative to a firm-size measure rather than simply in the logs for two reasons. First, we are not as interested in the growth of credit as we are in the size of this growth relative to the firms' operations. Second, since there is a large increase in credit in a previously small segment from public banks, the log-growth nonlinearity can bias our results.

2012, so the increase corresponds to roughly 27 percent of their baseline level.

Figure 5, Panel (b), shows a qualitatively similar but quantitatively smaller effect when comparing firms that borrowed from both types of banks with firms that borrowed exclusively from private banks. The coefficient estimate of 0.72 in December 2013 implies a 12 percent increase in their debt-to-payroll ratio relative to their baseline level of 5.75 in March 2012. This smaller effect is explained by the reduction in debt obtained from private banks, as shown in Figure 5, Panel (c). In particular, firms that borrowed from both types of banks reduced their volume of debt outstanding from private banks by 0.63 of their annual payroll relative to firms that borrowed from private banks exclusively, which indicates within-firm crowding out of private debt. Non-exclusive borrowers had a private debt-to-payroll ratio of 3.58, on average, in March 2012. Thus, our estimates translate to a reduction of 17 percent in non-exclusive firms' private debt-to-payroll ratio relative to exclusive private borrowers. Since interest rates for these loans from private banks fell after the intervention, one could expect private debt to have also increased. However, the intervention can be seen as a relaxation of a constraint in the supply of credit provided by the government, which in level was still cheaper than loans offered by private banks. We find that when they faced an increase in the availability of cheaper funds, firms with non-exclusive relationships chose to increase their share of loans from public banks.

Although throughout this section we keep the denominator fixed as firms' payroll in 2011, one important question is whether firms that were borrowing more were also hiring more workers and increasing their payroll. We come back to this issue when we analyze the real effects of the intervention. Moreover, the substantial increase in the debt-to-payroll ratio we document can have an effect on firms' ability to comply with their financial contracts, which can lead to higher delinquency rates for government banks, an issue we turn to next.

4.2 Borrower Risk and Delinquency

To understand how the intervention affected delinquency rates, we compare the trajectories for public and private banks before and following the intervention. We say 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 nine months after origination.[19] For example, if a firm obtained a loan in May 2012, we track that firm's delinquency until May 2013. If the firm failed to pay its loan installments for at least 90 days in this one-year window, we define the firm as delinquent on loans it contracted in May 2012.

[19]The choice of a 90-day cutoff follows other papers in the literature, such as Jiménez et al. (2014) and Jiménez et al. (2019).

We first analyze the average delinquency over time for private and government banks separately. Formally, we estimate the following specification at the firm-bank level:

$$D_{tmbfs} = \alpha_{ms} + \alpha_b + \alpha_{f(size)} + \sum_{\tau \neq -1} \gamma_\tau + \varepsilon_{tmbfs}, \tag{3}$$

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, α_{ms} are municipality-industry fixed effects, α_b are bank fixed effects, $\alpha_{f(size)}$ are firm-size fixed effects, and γ_τ are time dummies. Each γ_τ indicates the average change in delinquency probability at a given month relative to March 2012.

The results are shown in Figure 6. Prior to the intervention, public and private banks had very similar delinquency rates relative to their respective baseline levels, despite large differences in the average interest rate of their borrowers. However, after the intervention, government banks experienced a deterioration of their loan portfolio, while the delinquency rate on private banks' loans initially improved and eventually went back to its pre-intervention level. From a quantitative perspective, the effects we find are large. Before the intervention, 5 percent of borrowers were delinquent. Although we do not find any increase in delinquency for loans originated right after March 2012, it does rise for loans originated later on. For loans originated after October 2012, we find a 2 percentage point increase in the likelihood of default. For loans originated in December 2013, we find an increase of more than 3 percentage points. If we aggregate all of these coefficients, weighting by the volume of loans originated in each period, we arrive at a 1 percentage point increase, on average, in the likelihood of default for loans extended by public banks relative to private banks by the end of 2013. We confirm these findings in a difference-in-differences specification where we estimate Equation (3) for both types of banks jointly with time-municipality-industry fixed effects. The use of time-municipality-industry fixed effects guarantees that the differences in delinquency are not explained by shocks that affected firms in the same month that were in the same location and operating in similar industries (for instance, credit demand shocks).[20] The results of this difference-in-differences specification are shown in Figure A.10.

To understand the source of the relative increase in delinquency of loans originated by public banks, we redo our analysis of public and private banks' delinquency over time for levered and unlevered firms separately. A levered firm is a firm that has any outstanding debt when the new working capital loan is originated. Figure 6, Panel (b) shows that levered borrowers from government banks became delinquent more often than firms that

[20] One implication of the use of these controls is that our results are not explained, for example, by the fact that government banks entered many new locations during the intervention.

borrowed from private banks during the intervention. By contrast, Panel (c) shows that new borrowers of public and private banks had comparable risk, both before and during the intervention. This indicates that the increase in default for government banks can be explained by the increase in default from previously levered firms. By facilitating credit access to already levered firms, government banks exposed themselves to greater risk of borrower delinquency. This view incorporates the notion that more levered firms are more likely to become delinquent, which is true in our sample (Figure A.11).

Our results cast doubt on the idea that an asymmetric information mechanism that generated a negative relationship between interest rates and borrower quality was at play. Despite lower interest rates, public banks attracted borrowers whose risk was similar (higher) to that of borrowers from private banks before (after) the intervention.[21] Given the differences between the interest rates of public and private banks, one would expect that government banks would have attracted borrowers that were safer than those that private banks attracted, both before and after the intervention, which is not what we observe in the data.

Furthermore, our results allow us to rule out the hypothesis that government banks relaxed their credit standards for new borrowers as part of the intervention, since new borrowers from both public and private banks had similar risk.[22] Our results are consistent with the idea that by increasing credit supply during the intervention, public banks favored already levered firms. These firms eventually became delinquent, leading to a deterioration in public banks' loan portfolio. Loans to unlevered borrowers accounted for 14 percent of public banks' working capital originations before the intervention and only 7 percent afterward (Figure A.12). We do not find a similar change in working capital loans made by private banks. This result is consistent with public banks easing their lending standards for previously levered firms. More specifically, given that delinquency differences are insignificant for loans originated right after the policy was implemented and increased for loans originated later on, our results suggest that public banks progressively moved down the borrower-riskiness ladder as they increased their credit supply. This last result highlights one of the key differences between small and systematically large interventions: The latter

[21] Another possibility is that lower interest rates attracted riskier borrowers, such as in the advantageous selection models (De Meza and Webb (1987), Mahoney and Weyl (2017)). Similarly, lower interest rates can have a causal effect on borrower risk, as in moral hazard models, which would imply a lower risk level for government-bank borrowers (Boyd and De Nicoló (2005), Martinez-Miera and Repullo (2010)). Our empirical evidence rules out these alternative mechanisms as well.

[22] We also perform one additional test to rule out the possibility that adverse selection is driving our results. We compare firms that had a relationship with a bank at some point during the two years preceding the intervention with first-time borrowers and find that, conditional on having no outstanding debt, firms with previous relationships and new borrowers had similar risk, contradicting the hypothesis that new borrowers, which would be attracted by lower interest rates, were safer (riskier), as in adverse (advantageous) selection models.

must mechanically include a larger pool of borrowers and therefore will likely ease credit standards.

5 Real Effects of the Intervention

In this section, we study the effects of the intervention on real outcomes, both at the firm and regional levels. A priori, firm- and regional-level analyses do not have to deliver the same results and are both important economically. The distinction comes from the fact that our regional analysis encompasses local general equilibrium effects, credit reallocation across firms, and changes in borrower composition, all of which are controlled away in our firm-level analysis. At the firm level, we estimate the employment effects based on firms' exclusive and non-exclusive relationships with public and private banks. At the regional level, we focus on two questions. First, we study the extent to which credit grew more in municipalities more exposed to the intervention. Second, we study whether there were real effects on local output, employment, and payroll as a consequence of the regional change in credit conditions and how these effects compare with those at the firm level.

5.1 Firm-Level Employment Effects

To understand the real effects of the intervention, we perform three comparisons. First, we compare public and private borrowers that had exclusive relationships with either type of bank throughout the sample period. Firms with exclusive relationships with public banks benefited relatively more from increased credit availability (Figure 5, Panel a), which can lead to higher employment. On the other hand, firms with exclusive relationships with private banks faced lower interest rates following the intervention, which can also lead to higher employment (Figure 4, Panel a). Second, we compare firms that had non-exclusive relationships with public banks throughout the sample period with those that had exclusive relationships with private banks. This set of firms benefited less from the increase in credit availability (Figure 5, Panel b) relative to those with exclusive relationships, but they were also exposed to interest rate decreases following the intervention (Figure 4, Panel b). Finally, we compare new borrowers from public banks with firms that did not have any debt outstanding throughout our sample period. The idea of this final comparison is to understand whether increased credit access at the extensive margin, captured by new loans to firms without prior relationships, had any effect on employment. In all cases, we constrain our analysis to firms that borrowed at some point in 2012 to allow for at least one year of post treatment. For each of these comparisons, we estimate the following regression at the

firm-year level:

$$y_{tmsf} = \alpha_{tmsf(size)} + \alpha_f + \beta_{2012} Public_f \cdot I_{2012} + \beta_{2013} Public_f \cdot I_{2013} + \epsilon_{tmsf}, \quad (4)$$

where the dependent variables are log-growth of employment and log-growth of payroll in year t of a firm f that operates in sector s and is located in city m; $\alpha_{tmsf(size)}$ are time-municipality-industry-firm size fixed effects; α_f are firm fixed effects; $Public_f$ is an indicator equal to one if the firm borrowed exclusively from public banks (Panel A), from both types of banks (Panel B), or if this firm did not have any prior outstanding debt but borrowed from a public bank following the intervention (Panel C); I_{2012} and I_{2013} are indicator variables for years 2012 and 2013. The coefficients of interest, β_{2012} and β_{2013}, capture the relative change in employment and payroll growth in years 2012 and 2013 relative to employment and payroll growth in 2011.

The results are shown in Table 2. For firms with exclusive relationships with either public or private banks (Panel A), there was a significant increase in employment and payroll growth of around 1.8 percent. The effect was stronger for small firms relative to micro firms. This result can come from the fact that smaller firms were more productive at the margin or due to the fact that the reduction in interest rates in the control group was larger for micro firms relative to small firms (Figure A.9 and Table A.4). In Table 2, Panel B, we perform the same exercise, but comparing firms that borrowed from both types of banks with firms that borrowed exclusively from private banks. Coefficients for 2012 are modestly negative, and coefficients for 2013 are smaller than those in Panel A. This suggests the credit-amount effect was smaller for non-exclusive firms, which is in line with the evidence that the effect of the intervention on non-exclusive firms' debt-to-payroll ratio was smaller than the effect on the ratio for exclusive public borrowers.

Our final test represents an attempt to understand if the increase in credit access at the extensive margin allowed new borrowers to increase employment. We compare firms that had no outstanding debt and borrowed from a public bank after the intervention with firms that did not borrow during our sample period. Although the decision to borrow is endogenous, the coefficients of interest are likely to be biased upward due to credit demand and the screening process firms have to go through before a loan is originated. The results are shown in Table 2, Panel C. For firms that did not borrow before the policy and borrowed from a public bank after its implementation, there was no significant change in employment growth relative to those that did not borrow at all during our sample period. While there are limitations to the exercise, the lack of effects is concerning from the point of view of the policy and suggests other selection mechanisms were potentially at play.

The employment effects we document in this section are small relative to other estimates in the credit supply literature focusing on credit crunches. For instance, Huber (2018), Chodorow-Reich (2014), and Bentolila, Jansen and Jiménez (2018) find that firms that depended on distressed banks reduced employment growth by 4 to 6 percent. Huber (2018), for instance, finds this decrease in employment for a 16 to 20 percent decrease in credit. Studying a credit boom in Spain, Jiménez et al. (2020) find no increase in credit supply at the firm level and, as a consequence, no real effects as well.

5.2 Regional Level

We now explore the effects of the intervention at the regional level. We consider a municipality in Brazil to be our benchmark definition of a local banking market.[23] To identify the effects of the intervention on credit growth and real outcomes, we use municipalities' heterogenous exposure to public banks before the intervention. Our first measure of heterogeneous exposure is an indicator variable based on the presence of branches from public banks in a given municipality. Our identifying assumption is that branch presence in the baseline is independent of changes in credit demand following the intervention.

Our benchmark samples include municipalities where there were branches from only one bank (either private or public) and no branch openings during our sample period of banks previously absent from those municipalities. These municipalities provide the cleanest experiment for measuring the effects of the intervention on credit and real outcomes at the regional level. Bank entry into most of these municipalities was the result of M&As and privatization processes at the state and national levels that took place several years before the beginning of our sample period. In other words, it is unlikely that branch location was correlated with changes in the economic conditions at the time. We also find that local monopolies from private banks were comparable to those from public banks in various dimensions, such as agricultural and industry shares, and GDP per capita, reinforcing our identification assumption. This sample includes 894 of the 3,398 municipalities with at least one bank branch in the baseline.[24]

To measure the credit effects of the intervention at the regional level, we estimate the following regression at the municipality m, quarter t level

$$y_{mt} = \alpha_m + \gamma_{t,s} + \sum_{\tau \neq -1} \beta_\tau Public_m + \varepsilon_{mt}, \tag{5}$$

[23] This is the same definition as in Sanches, Silva Junior and Srisuma (2018) and Coelho, De Mello and Rezende (2013) and various other papers that study the Brazilian banking sector.

[24] Results are also shown for a more representative sample of municipalities later on.

where y_{tmr} is a measure of credit (or credit growth) originated in municipality m and quarter t, α_m are municipality fixed effects, $\gamma_{t,s}$ are state-time fixed effects, and $Public_m$ is the municipalities' exposure to the intervention. We estimate Equation (5) with various measures of credit and credit growth. For working capital originations, we compute the cumulative mean of originations relative to the origination average before the intervention.[25] We also use as dependent variables the logarithm of the outstanding volume of working capital loans to firms in a given municipality and the volume of credit outstanding from branches in a given municipality. Standard errors are clustered at the state level.

Figure 7 shows the estimates of differences in within-municipality credit growth from Equation (5). We find large relative increases in working capital originations (Panel a) and outstanding amounts of credit from branches (Panel b) in municipalities that had branches from public banks. The magnitude of the change in total credit outstanding from these branches is consistent with the overall change in credit following the intervention. Although we do observe a growth in originations of working capital loans and credit outstanding from branches in municipalities with branches of public banks, we do not observe a similar growth in the outstanding volume of working capital loans for firms in those municipalities (Panel c). This apparent puzzle can be explained by cross-municipality borrowing. To see that, we estimate the following regression at the municipality m, quarter t level:

$$\Delta \ln \left(\text{WK Outstanding}\right)_{b,m,t} = \alpha + \sum_{\tau \neq -1} \delta_t + \varepsilon_{b,m,t}, \tag{7}$$

where $\ln \left(\text{WK Outstanding}\right)_{b,m,t}$ is the log of the outstanding volume of working capital loans for firms in municipality m from bank type b (public or private) at quarter t. We estimate Equation (7) separately for the municipalities with branches of private and public banks and present the results in Figure 9. We see that the outstanding volume of working capital loans from public banks grew significantly in municipalities with or without branches from public banks, while the outstanding volume of working capital loans from private banks grew only in municipalities with branches from private banks. In other words, firms in municipalities with only private banks significantly increased their borrowing from both public and private banks, while firms in municipalities with only public banks did not increase their borrowing

[25] Mathematically, we compute:

$$\%\Delta Orig_{t,m} = \frac{\frac{1}{t+T}\sum_{\tau=-T}^{t} Orig_{m,\tau}}{\frac{1}{T}\sum_{\tau=-T}^{0} Orig_{m,\tau}}, \tag{6}$$

where $t = -T$ denotes the beginning of our sample (2011Q1). For this exercise, we use only loans originated in the final month of each quarter, but our results are the same if we conduct our analysis at the monthly level.

from private banks. The end result is that the volume outstanding of working capital loans grew more in locations without a public bank branch.

To account for this cross-municipality borrowing in our analysis, we construct a measure of exposure to the intervention based on the baseline share of the volume outstanding of working capital loans that were originated by public banks to firms located in a given municipality. While our previous exposure measure is based on the locations of banks' branches, this second measure uses the locations of the banks' borrowers. Importantly, the share of the volume outstanding of working capital loans from public banks was relatively stable before the intervention, such that it is unlikely that it was correlated with changes in economic conditions. The within-municipality standard deviation in this share is 0.08 relative to an average of 0.63. We replicate the results of Figure 7 and re-estimate Equation (5) with this alternative measure of exposure. The results are shown in Figure 8. We find that credit grew significantly more in municipalities with a higher share of the volume outstanding of working capital loans from public banks at the baseline. Consistent with the cross-municipality borrowing channel, we find a smaller increase in credit outstanding from branches in municipalities using this alternative measure of exposure to the policy (Panel c).

To match our analysis of real outcomes, we also estimate Equation (5) at the annual level using data from December of each year, that is

$$y_{mt} = \alpha_m + \alpha_{t,s} + \beta_{2012} Public_m \times I_{2012} + \beta_{2013} Public_m \times I_{2013} + \varepsilon_{mt}, \tag{8}$$

where I_{2012} and I_{2013} are year indicator variables, and the rest of the terms are the same as in Equation (5). The results are shown in Table 3. We confirm our previous findings that used quarterly data. We find that municipalities with branches from public banks experienced a larger growth in credit outstanding from those branches but also a relative reduction in the volume outstanding of working capital loans made to firms in those municipalities. Once we use this alternative exposure measure, we find that credit outstanding for firms in municipalities with a higher exposure grew significantly and much more so than credit originated from branches in the same municipalities. Quantitatively, we find that the volume outstanding of working capital loans grew 63 percent more in municipalities where all of the volume outstanding of working capital loans was from public banks before the intervention relative to those where none was. Alternatively, we find that a one standard deviation change in exposure to the intervention (standard deviation of 0.3) was associated with a 16 percent higher growth in credit outstanding for firms in that municipality.

To understand the real effects of the intervention, we estimate Equation (8) with the log of GDP, employment, and total payroll as the dependent variables. The results are shown

in columns 1 through 3 of Table 4. We find no differential real effects in municipalities with branches of private banks (Panel A). This is not surprising given that we do not find an increase in credit to firms in those municipalities. We find that GDP growth was 4.65 percent higher in municipalities where all of the working capital loans were from public banks before the intervention relative to those where none was. Alternatively, we find that a one standard deviation change in the share of the volume outstanding of working capital loans from public banks before the intervention was associated with a 1.4 percent higher GDP growth. We find quantitatively similar (although not statistically significant) effects on employment and payroll in these municipalities.

We find similar results in terms of credit growth and real outcomes in a broader sample of municipalities. For this alternative sample, we consider municipalities where there were no branch openings of previously absent banks during our sample period. Contrary to our benchmark sample, municipalities in this sample had any combination of public- and private-bank branches. Although in this sample the effects of the intervention are potentially not as well identified as in the case of local monopolies, this sample is much more representative and includes 2,785 municipalities. We find the same results in terms of credit growth (Table A.1) and real outcomes (Columns 4-6 of Table 4). We also replicate our analysis using pre-intervention population as weights in the credit-growth and real-outcomes regressions and find similar results (Tables A.2 and A.3). Overall, we find larger real effects at the regional level relative to our firm-level evidence. This is qualitatively consistent with the result in Huber (2018), which finds the direct effect of a credit crunch episode accounted for 25 percent of the regional effects. The fact that our firm-level evidence points to similar effects on employment and payroll and our regional level evidence points to smaller effects on employment relative to payroll indicates that the intervention had a large effect on local wages through local general-equilibrium effects.

Our results also have far-reaching implications for the banking literature that studies regional exposure based on branch location. Although there is evidence that credit markets for small businesses are typically local (see, for example, Granja, Leuz and Rajan (2022) and Li and Strahan (2021)), we show that when analyzing macroeconomically large shocks, one must account for the location of both the bank and the borrower, as well as the potential for the expansion of credit markets as a function of the shock itself. As argued in Ashcraft (2005), Huber (2018), and others, a reason why some papers do not find large effects of credit supply shocks at the level of aggregation beyond the firm is that there is no heterogeneity in regional exposure to large, systemic shocks. Our evidence suggests that this problem of heterogeneity in exposure is particularly acute if exposure is measured by physical branch presence, but that once we account for firms' locations, the real effects of positive credit

supply shocks can be identified and, in our setting, are significant at the regional level.

6 Conclusion

In this paper, we study a credit market intervention implemented by the Brazilian government using public banks. The intervention was characterized by a large and unexpected increase in the supply of credit to firms at subsidized interest rates and implemented during a period when the economy was growing and neither banks nor borrowers were in distress. The combination of this unique quasi-experiment and the availability of detailed data allow us to jointly analyze the implications of the intervention for lending rates, loan originations, debt outstanding, default, and real effects at the firm and regional levels.

We document that the intervention was associated with a large increase in loan originations and debt from public banks and a reduction in the lending rates of private banks. Firms that obtained loans issued by government banks during the intervention were more likely to default on those loans than comparable firms that borrowed from private banks. This deterioration in the quality of government banks' loan portfolios is connected to loans issued to levered firms, which were favored in the allocation of loans in the program. We rule out alternative explanations of why the intervention was characterized by a worsening of credit quality, such as selection or poor screening by government banks. We find that despite a large relative increase in credit, the intervention had only a small real effect at the firm level.

At the regional level, we find that bank branch presence cannot account for the increase in credit for firms in a given municipality. We provide evidence of cross-municipality borrowing in response to the intervention. We show that once we account for borrowers' locations, we observe large increases in credit at the regional level based on pre-intervention exposure to the intervention. We find real effects at the regional level that are substantially larger than the within-region firm-level effects, pointing to significant general equilibrium and spillover effects. However, these regional effects are still smaller than those in the credit supply literature, pointing to the ineffectiveness of government interventions in credit markets outside of crisis episodes. Beyond the estimated effectiveness of the intervention, our results suggest that the empirical banking literature that estimates the effects of large shocks must account for the location of both borrowers and bank branches and the potential for large cross-market borrowing in general and as a response to the shocks themselves.

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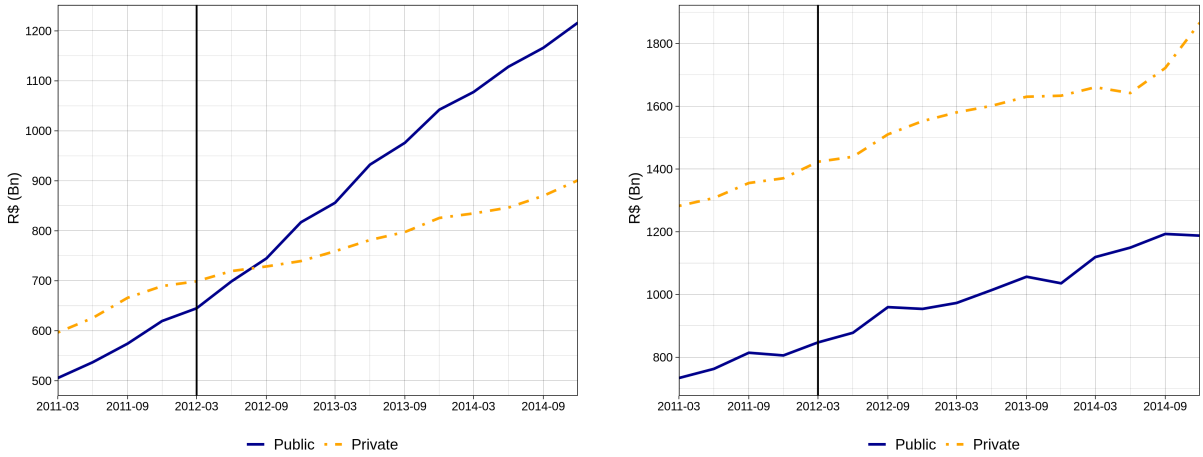
Table 1: Summary Statistics

This table reports summary statistics for the main variables in our data set. There are N_{obs} =2.6M observations and N_{firms} = 793,121 firms in the matched sample. Our sample period is from 2011 to 2013. Sources: Credit Information System (SCR), RAIS, and authors' calculations.

Variable	Mean	Median	SD
Panel A: Loans			
Panel A.1 - Public Banks Loans			
Amount (R$)	62422	36268	92095
Maturity (months)	23.17	24	10
Interest Rate (APR)	25	23.63	10.69
Panel A.2 - Private Banks Loans			
Amount (R$)	84868	33847	143916
Maturity (months)	16.09	15	10.60
Interest Rate (APR)	37.75	34.48	16.62
Panel B: Firms			
Panel B.1 - Firms that borrow exclusively from Public Banks			
Num. of Employees	10.17	4	40.13
Payroll Costs (R$ per Month)	11,666	3,738	57,023
Total Outstanding Debt	97,833	27,218	1,041,000
Debt-to-Payroll Ratio	1.88	0.443	8.12
Panel B.2 - Firms that borrow exclusively from Private Banks			
Num. of Employees	11.42	3.01	66,37
Payroll Costs (R$ per Month)	14,659	3,074	98,539
Total Outstanding Debt	168,655	11,398	2,132,000
Debt-to-Payroll Ratio	1.94	0.230	80.2
Panel B.3 - Firms that borrow from both types of Banks			
Num. of Employees	18.66	6.5	76.9
Payroll Costs (R$ per Month)	23,084	6,545	97,106
Total Outstanding Debt	392,461	111,134	1,829,000
Debt-to-Payroll Ratio	3.51	1.32	12.5

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

This figure shows the volume of credit outstanding and other assets by bank type by quarter. *Public* (government-owned) banks are *Banco do Brasil* (BB) and *Caixa Economica Federal* (CEF). Private banks are: Bradesco, HSBC, Itau Unibanco, and Santander. Total volume 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.

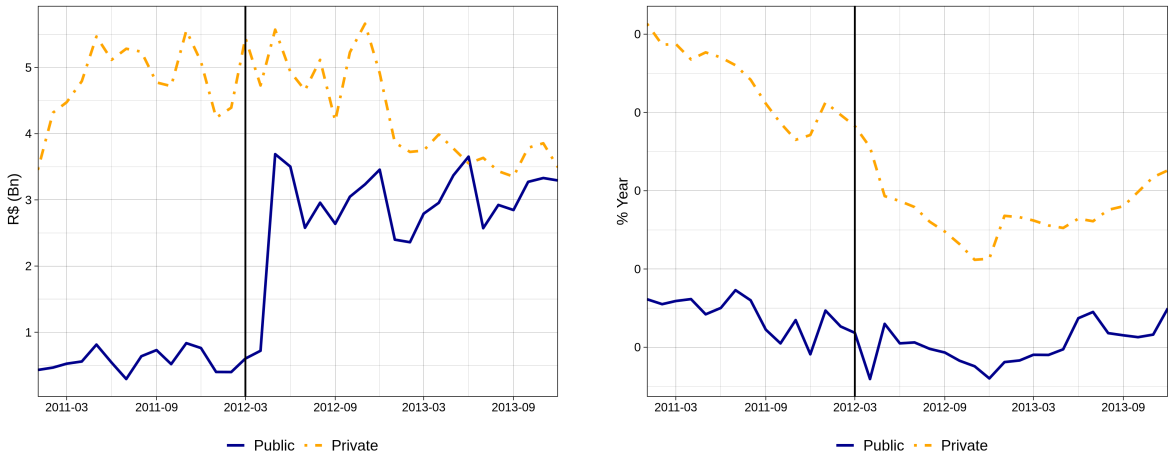


(a) Credit

(b) Other Assets

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

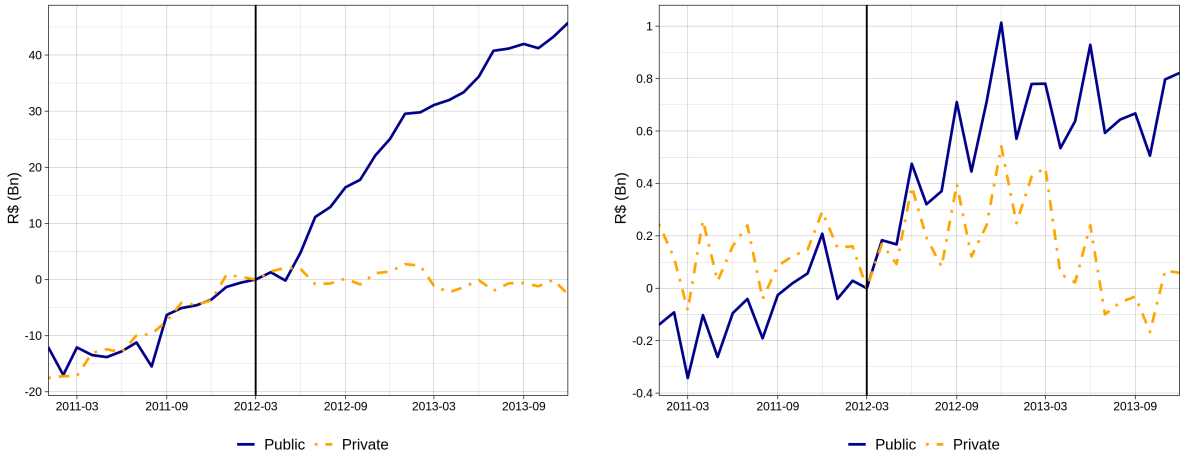
This figure shows the volume and interest rates of monthly origination of uncollateralized working capital loans to firms by type of bank. *Public* (government-owned) banks are Banco do Brasil (BB) and Caixa Economica Federal (CEF). Private banks are all other banks that are not controlled by the government. Interest rate is shown as annual percentage rate (APR). The vertical line indicates the start of the intervention (March 2012). Sources: Credit Information System (SCR), and authors' calculations.



(a) Volume (b) Interest Rates

Figure 3: Change in Total Credit Outstanding: Working Capital and Discounted Receivables

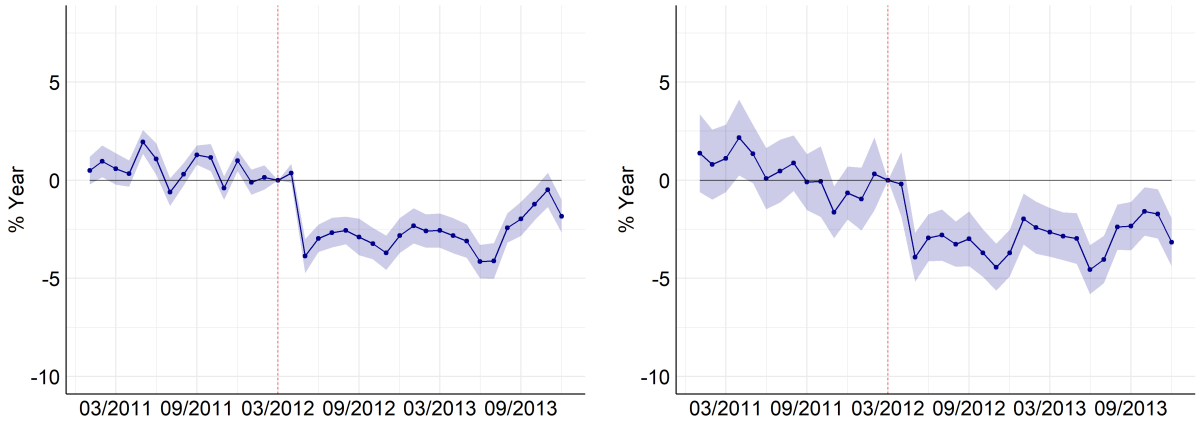
This figure shows the quarterly volume of loans outstanding to firms by type of bank. *Public* (government-owned) banks are Banco do Brasil (BB) and Caixa Economica Federal (CEF). Private banks are all other banks that are not controlled by the government. Panel (a) shows the change in the volume of uncollateralized working capital loans, and Panel (b) shows the change in the volume of discounted receivables relative to baseline (March 2012). The vertical line indicates the start of the intervention. Sources: Credit Information System (SCR), and authors' calculations.



(a) Working Capital (b) Discounted Receivables

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

This figure shows the estimates of δ_τ from Equation (1) at the loan level, with March 2012 as the reference month (vertical line), weighted by loan amount. For **Panel (a)**, we run: $i_{jtmbfs} = \alpha_{tms} + \alpha_{fb} + \alpha_{t,j(maturity)} + \alpha_{t,f(size)} \sum_{\tau \neq -1} \delta_\tau \; Private_b + \varepsilon_{jtmbfs}$, where i_{jtmbfs} denotes the interest rate of a loan j issued in month t municipality m by bank b to firm f in industry s, α_{tms} are time-municipality-industry fixed effects, α_b are bank fixed effects, $\alpha_{t,j(maturity)}$ are time-maturity fixed effects, $\alpha_{t,f(size)}$ are time-firm size fixed effects, and $Private_b$ is an indicator equal to one if bank b is a private bank. For **Panel (b)**, we replace the firm-bank fixed effects by bank and firm-time fixed effects Standard errors are clustered at bank-municipality level. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), and authors' calculations.

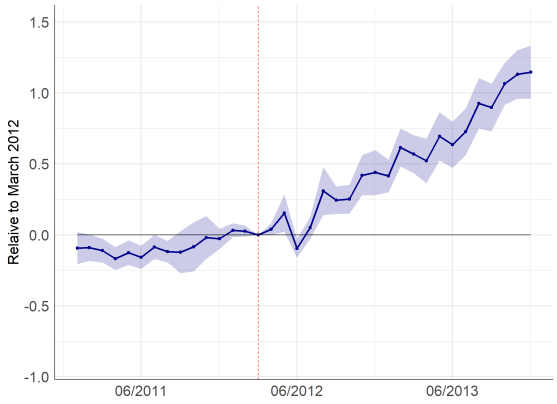


(a) Firm-Bank Fixed Effects

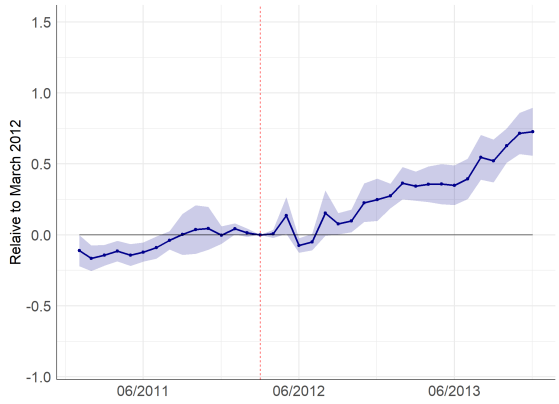
(b) Firm-Time Fixed Effects

Figure 5: Debt-to-Payroll Ratio: Difference-in-Differences Specification

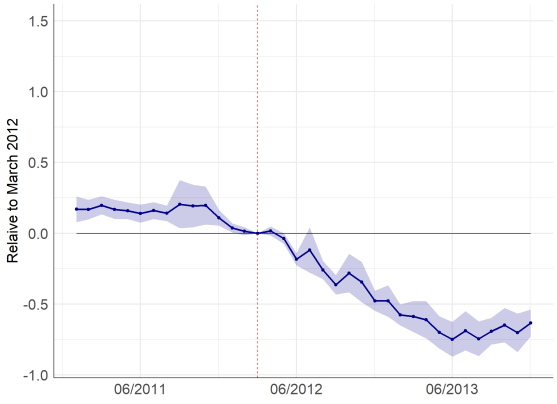
This figure shows the estimates of δ_τ from Equation (2) at the firm level, with March 2012 as the reference month (vertical line). **Panel A**: the sample consists of firms with exclusive relationships with types of banks. **Panels B and C**: the sample consists of firms that borrow from both types of banks with firms that have exclusive relationships with private banks. More specifically, for each sample, we run: $Debt\ to\ Payroll_{tf} = \alpha_t + \alpha_f + \sum_{\tau \neq 0} \gamma_\tau \cdot Public_f + \varepsilon_{tf}$, where $Debt\ to\ Payroll_{tf}$ denotes the debt-to-payroll ratio of a firm f in month t, α_t, and α_f are time and firm fixed effects, and $Public_f$ is an indicator if the firm has a relationship with a public bank. **Panels A and B**: the dependent variable is total outstanding debt. **Panel C:** the dependent variable is debt outstanding from private banks only. Standard errors are clustered at the bank-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) Exclusive Relationships



(b) Non-Exclusive Relationships

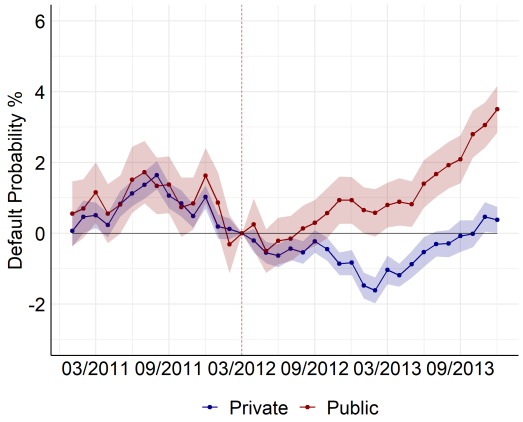


(c) Non-Exclusive Relationships, Private Debt

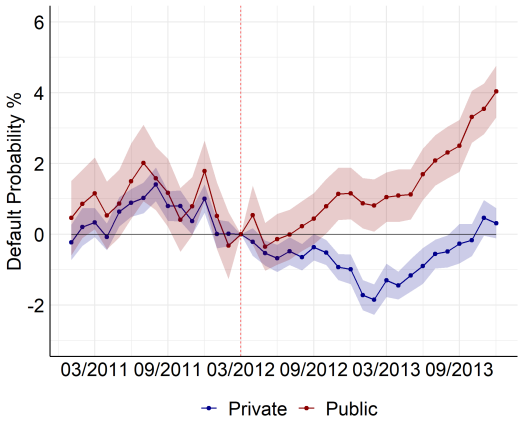
Figure 6: Delinquency Likelihood for Public and Private Banks

This figure shows the estimates of δ_τ from the estimation of Equation 3 at the firm-bank level for public and private banks separately for three different samples of borrowers. **Panel A:** all borrowers. **Panel B:** levered borrowers (at the moment of origination). **Panel C:** unlevered borrowers (at the moment of origination). More specifically, we run: $I^D_{tmbfs} = \alpha_{ms} + \alpha_b + \alpha_{f(size)} + \sum_{\tau \neq -1} \gamma_\tau + \varepsilon_{tmbfs}$, where I^D_{tmbf} is an indicator equal to one if a loan originated by firm f located in municipality m in month t from bank b becomes delinquent within one year after origination. Standard errors are clustered at the municipality level. Shaded areas are the 95 percent confidence intervals. Sources: Credit Information System (SCR), and authors' calculations.

(a) All Borrowers



(b) Levered Borrowers



(c) Unlevered Borrowers

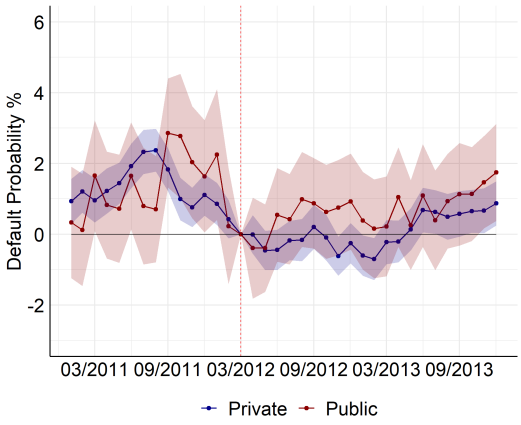


Table 2: Firm-Level Employment and Payroll Growth

This table shows the estimates of the β's on Equation (8) with two different dependent variables: log of employment (Columns 1 to 3 5), and log of total payroll (Columns 4 and 6). Each panel consists of a different sample of firms. $Public_m$ is an indicator that is one if a firm borrows from a public bank in the sample. **Panel A:** firms with exclusive relationships with public or private banks throughout the sample. **Panel B:** firms with relationships with public and private or only private banks throughout the sample. **Panel C:** firms that either do not borrow throughout the sample or that do not have outstanding debt at the time they borrow. Firm size definitions are as follows. Micro firms: firms in the service/commerce sectors with fewer than 10 employees, or in the industry sectors with fewer than 20 employees. Small firms: firms in the service/commerce sectors with more than 10 and fewer than 50 employees, or in the industry sectors with more than 20 and fewer than 100 employees. Standard errors are clustered at the municipality level. All of the specification include time-industry-municipality-firm size fixed effects. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

Panel A. Exclusive Public Bank Borrowers

	Log Employment			Log Total Payroll		
	All	Micro	Small	All	Micro	Small
$Public \times I_{2012}$	0.0116*	0.0067	0.0313**	0.0126**	0.0112	0.0283*
	(0.0063)	(0.0074)	(0.0132)	(0.0188)	(0.0078)	(0.0148)
$Public \times I_{2013}$	0.0188***	0.0139*	0.0248*	0.0188**	0.0149*	0.0269*
	(0.0064)	(0.0073)	(0.0135)	(0.0074)	(0.0084)	(0.0157)
Observations	162766	121539	33233	163503	121050	33222

Panel B. Non-Exclusive Public Bank Borrowers

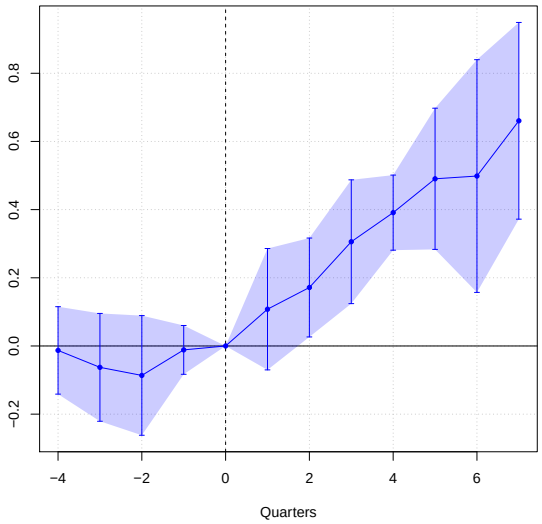
	Log Employment			Log Total Payroll		
	All	Micro	Small	All	Micro	Small
$Public \times I_{2012}$	−0.0059*	−0.0124***	0.0068	−0.0073**	−0.0129***	0.0052
	(0.0031)	(0.0032)	(0.0055)	(0.0032)	(0.0036)	(0.0063)
$Public \times I_{2013}$	0.0165***	0.0128***	0.0249***	0.0170***	0.0131***	0.0243***
	(0.0030)	(0.0031)	(0.0054)	(0.0037)	(0.0040)	(0.0059)
Observations	526859	393533	104051	528760	391050	104040

Panel C. Unlevered Firms

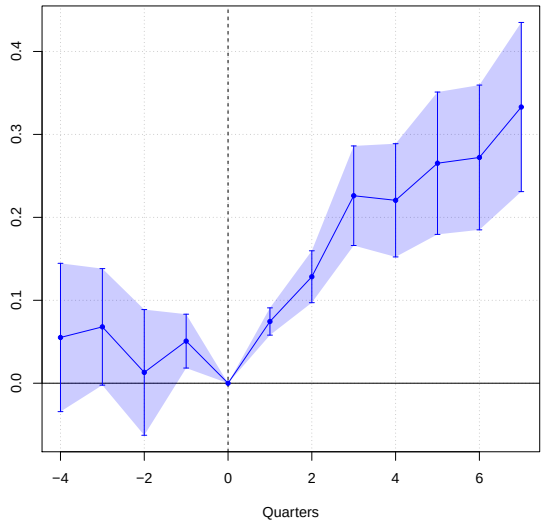
	Log Employment			Log Total Payroll		
	All	Micro	Small	All	Micro	Small
$Public \times I_{2012}$	0.0051	0.0001	0.0064	0.0026	-0.0026	-0.0009
	(0.0060)	(0.0067)	(0.0133)	(0.0066)	(0.0077)	(0.0129)
$Public \times I_{2013}$	-0.0053	-0.0096	0.0113	-0.0073	-0.0135	0.0051
	(0.0077)	(0.0077)	(0.0207)	(0.0076)	(0.0083)	(0.0198)
Observations	101867	79320	18659	102189	78998	18653

Figure 7: Branch Presence and Differential Credit Growth

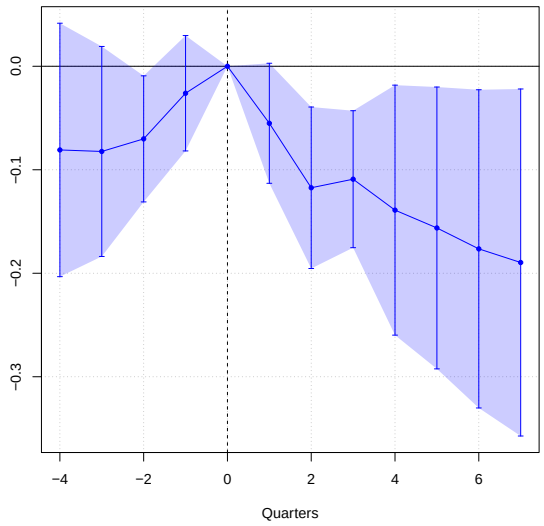
This figure shows the estimates of the β's on Equation (5) with $Public_m$ as an indicator function that is one if municipality m has a branch form a public bank. These coefficients capture the differences in within-municipality credit evolution for municipalities with and without branches from public banks. The municipalities in this sample are those that had no bank entry after January of 2011 and that are local monopolies of either a public or private bank. Panel (a) shows the evolution of the growth in originations of working capital loans (computed as in Equation (6)). Panel (b) shows the evolution of the log of the total amount outstanding at branches in a given municipality. Panel (c) shows the evolution of the log of the total amount outstanding for working capital loans for firms in a given municipality. Standard errors are clustered at the state level. Shaded areas are the 95 confidence intervals. Sources: Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.



(a) WK Originations



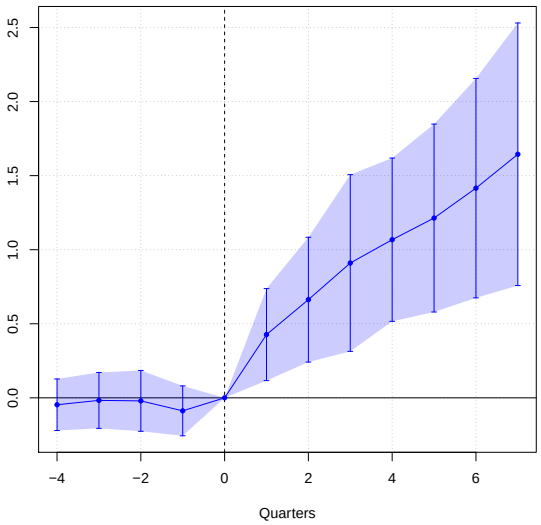
(b) Outstanding (Branches)



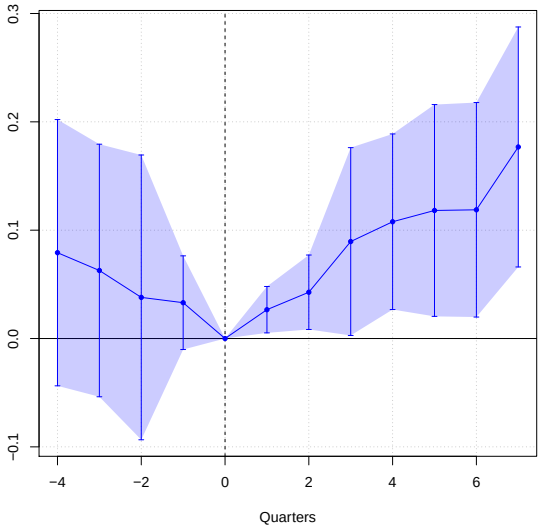
(c) Outstanding (Firms)

Figure 8: Pre-Intervention Share of Outstanding Working Capital Loans and Differential Credit Growth

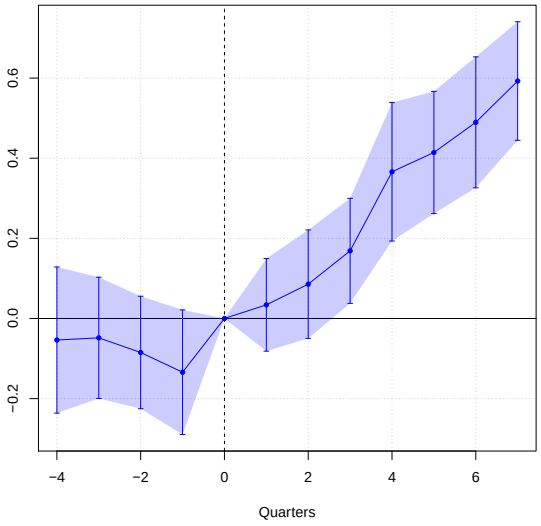
This figure is equivalent to Figure 7, except for the municipality measure of exposure to the intervention. While in Figure 7 $Public_m$ is an indicator function that is one if municipality m has a branch form a public bank, here it is the share of the total amount outstanding of working capital loans that is from public banks for firms in municipality m. Standard errors are clustered at the state level. Shaded areas are the 95 confidence intervals. Sources: Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.



(a) WK Originations



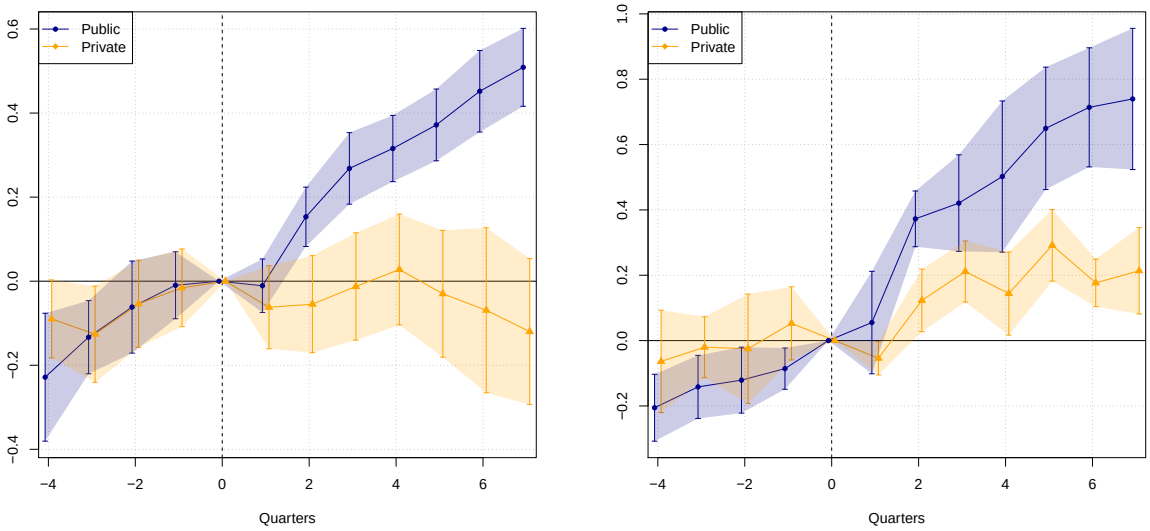
(b) Outstanding (Branches)



(c) Outstanding (Firms)

Figure 9: Branch Presence: Working Capital Outstanding

This figure shows the estimates of the δ's on Equation (7), that is, the credit evolution for municipalities with and without branches from public banks (relative to baseline). The municipalities in this sample are those that had no previously absent bank entry after January of 2011 and that are local monopolies of either a public or private bank. Panel (a) shows the evolution of the log of the total amount outstanding of working capital for firms in municipalities with *public* branches only by public and private banks. Panel (b) shows the evolution of the log of the total amount outstanding of working capital loans for firms in municipalities with *private* branches only by public and private banks. Standard errors are clustered at the state level. Shaded areas are the 95 confidence intervals. Sources: Credit Information System (SCR), and authors' calculations.



(a) Public Branches Only

(b) Private Branches Only

Table 3: Credit Growth at the Regional Level: Branch vs. Borrower Location

This table shows the estimates of the β's on Equation (8). For Panel A, $Public_m$ is an indicator function that is one if municipality m has a branch form a public bank For Panel B, it is the share of the total amount outstanding of working capital loans that is from public banks for firms in municipality m in December of 2011. The municipalities in this sample are those that had no bank entry after January of 2011 and that are local monopolies of either a public or private bank. We run Equation (8) with four different dependent variables. Column (1): growth in originations of working capital loans (computed as in Equation (6)). Column (2): log of the amount outstanding of working capital loans for firms in a given municipality. Column (3): log of the amount outstanding of loans for firms and households in branches in a given municipality. Column (2): log of the amount outstanding of all loans for firms in a given municipality. Standard errors are clustered at the state level. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.

	Panel A. Branch Presence			
	Working Capital		Branches	Firms
	Originations	Outstanding	Outstanding	Outstanding
Public Branch × 2012	0.4722***	-0.0594**	0.1723***	-0.0179
	(0.1373)	(0.0280)	(0.0413)	(0.0520)
Public Branch × 2013	0.7824***	-0.1952*	0.2634***	-0.0925
	(0.1442)	(0.1048)	(0.0670)	(0.0930)
Mun FE	Yes	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes	Yes
Observations	2,682	2,682	2,682	2,682
R^2	0.60105	0.88319	0.98093	0.92491

	Panel A. Share of Working Capital Outstanding			
	Working Capital		Branches	Firms
	Originations	Outstanding	Outstanding	Outstanding
Public Share (2011) × 2012	1.085***	0.3806**	0.0358	0.2415*
	(0.3214)	(0.1593)	(0.0468)	(0.1334)
Public Share (2011) × 2013	1.759***	0.7951***	0.1094*	0.6332***
	(0.4923)	(0.1473)	(0.0544)	(0.1353)
Mun FE	Yes	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes	Yes
Observations	2,682	2,682	2,682	2,682
R^2	0.60425	0.88776	0.97943	0.92760

Table 4: Real Effects at the Regional Level: Branch vs. Borrower Location

This figure shows the estimates of the β's on Equation (8). The municipalities in the sample of Columns 1-3 are those that had no previously absent bank entry after January of 2011 and that are local monopolies of either a public or private bank. The municipalities in the sample of Columns 4-5 are those that had no previously absent bank entry after January of 2011. For Panel A, $Public_m$ is an indicator function that is one if municipality m has a branch form a public bank For Panel B, it is the share of the total amount outstanding of working capital loans that is from public banks for firms in municipality m in December of 2011. We run Equation (8) with three different dependent variables: log of GDP (Columns 1 and 4), log of employment (Columns 2 and 5), and log of total payroll (Columns 3 and 6). Standard errors are clustered at the state level. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Brazilian Institute of Geography and Statistics (IBGE), Annual Review of Social Information (RAIS), and authors' calculations.

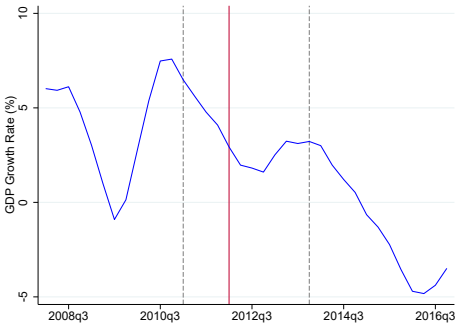
	Panel A. Branch Presence					
	Local Monopolies			**No Entry**		
	GDP	Emp.	Payroll	GDP	Emp.	Payroll
Public Branch × 2012	0.0038	-0.0039	-0.0079	0.0118	0.0050	-0.0010
	(0.0062)	(0.0138)	(0.0119)	(0.0075)	(0.0144)	(0.0132)
Public Branch × 2013	0.0003	0.0040	0.0007	0.0087	-0.0003	-0.0060
	(0.0099)	(0.0192)	(0.0175)	(0.0102)	(0.0096)	(0.0100)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,682	2,682	2,682	8,355	8,355	8,355
R^2	0.98076	0.95300	0.9518	0.99707	0.99579	0.99535
	Panel B. Share of Working Capital Outstanding					
	Local Monopolies			**No Entry**		
	GDP	Emp.	Payroll	GDP	Emp.	Payroll
Public Share (2011) × 2012	0.0296**	-0.0109	-0.0003	0.00007	-0.0043	0.0167
	(0.0142)	(0.0273)	(0.0267)	(0.0150)	(0.0144)	(0.0135)
Public Share (2011) × 2013	0.0465**	0.0382	0.0450	0.0324***	0.0257	0.0435**
	(0.0181)	(0.0360)	(0.0318)	(0.0093)	(0.0183)	(0.0178)
Mun FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,682	2,682	2,682	8,355	8,355	8,355
R^2	0.98084	0.95309	0.95190			

Appendix

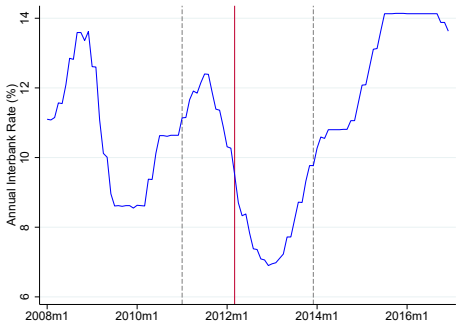
A Additional Figures and Tables

Figure A.1: Evolution of Macroeconomic Variables

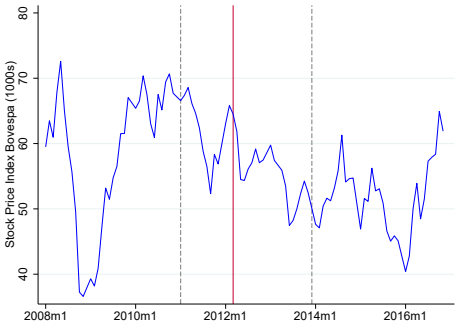
This figure shows the evolution of key macroeconomic variables during our sample. The vertical solid line denotes March 2012 or 2021Q1 for quarterly data. The dotted gray lines indicate our sample period (2011 to 2013). Panel (a) displays the Real GDP growth (seasonally adjusted). Panel (b) displays 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/Haver Analytics, OECD/Haver Analytics, B3/Haver Analytics, and authors' calculations.



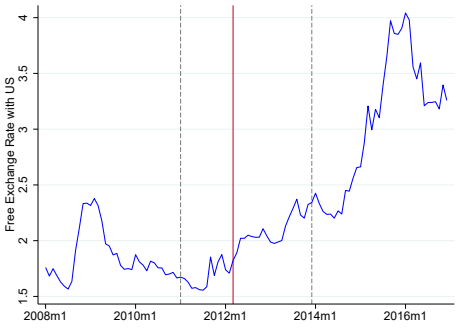
(a) Real GDP Growth



(b) Interbank Rates



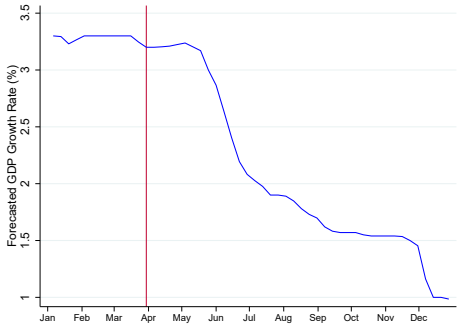
(c) Stock Index



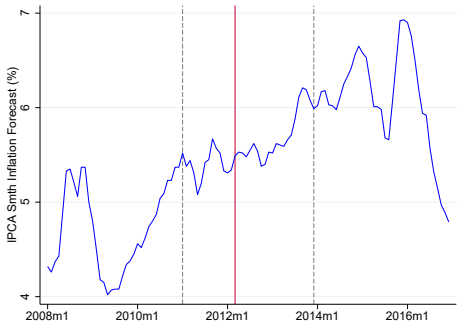
(d) Exchange Rate (R$ per US$)

Figure A.2: Forecasts of Macroeconomic Variables

This figure shows the evolution of macroeconomic forecasts during our sample period. Panel(a): GDP forecast for 2012 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) 2012 Real GDP Growth



(b) One Year Ahead Inflation

Figure A.3: President's Net Approval Rating

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 percentage of positive ratings minus the percentage of negative ratings. The vertical solid line denotes March 2012. The dotted gray lines indicate our sample period (2011 to 2013). Sources: Reyes-Housholder (2020), and authors' calculations

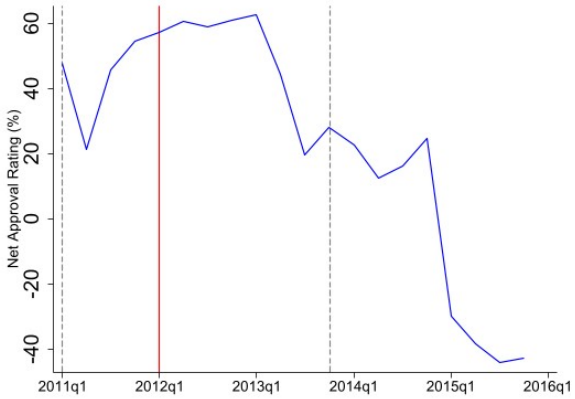


Figure A.4: Outstanding Credit by Public Banks Relative to Linear Trend

This figure shows the total amount of outstanding credit and other assets by Banco do Brasil (BB) and Caixa Economica Federal (CEF) by quarter relative to the pre intervention linear trend. That is, for each t, we plot $x_t = x - \left[\frac{t}{4} \cdot (x_0 - x_{-4}) + x_0\right]$ where x_t is the total amount outstanding includes all outstanding credit to firms and households in quarter t. Quarter $t = 0$ is the start of the intervention (2012Q1) and $t = -4$ is the start of our sample. The vertical solid line indicates the start of the intervention and the vertical dashed line the end of our sample. Sources: IF.data, and authors' calculations.

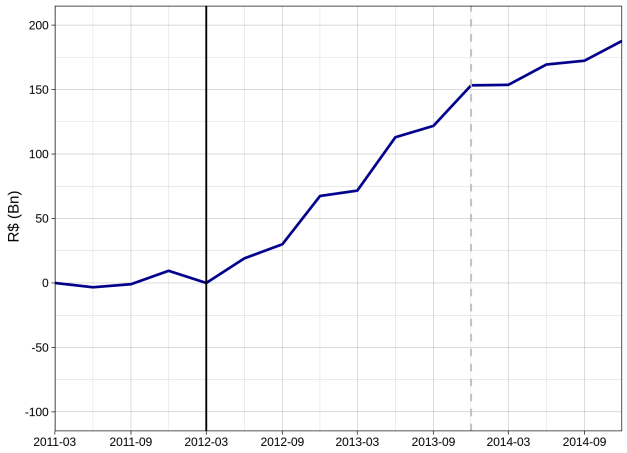
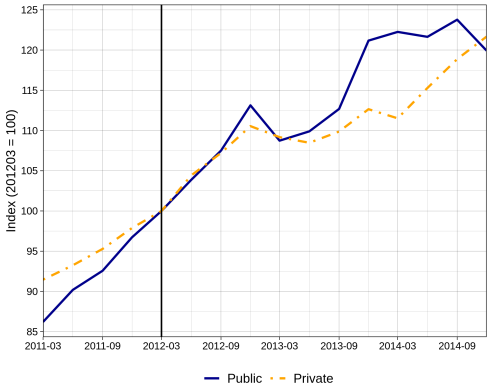
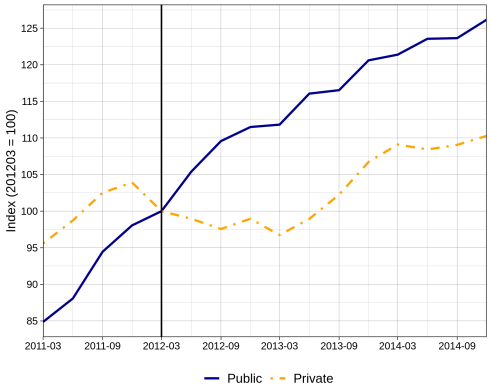


Figure A.5: Banks' Equity and Deposit Growth: Public and Large Private Banks

This figure shows the ratio in deposits and equity by type of bank and quarter relative to baseline (2012Q1). *Public* (government-owned) banks are Banco do Brasil (BB) and Caixa Economica Federal (CEF). Private banks are Bradesco, HSBC, Itau Unibanco, and Santander. Panel (a) shows the evolution of equity and Panel (b) of deposits. The vertical line indicates the start of the intervention. Sources: IF.data, and authors' calculations.



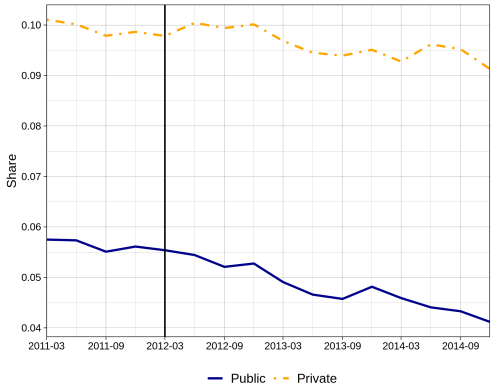
(a) Equity



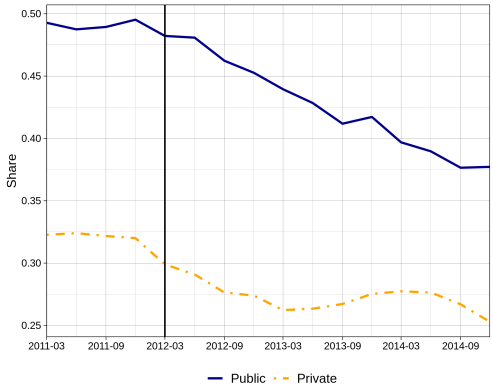
(b) Deposits

Figure A.6: Banks' Liability Decomposition: Public and Large Private banks

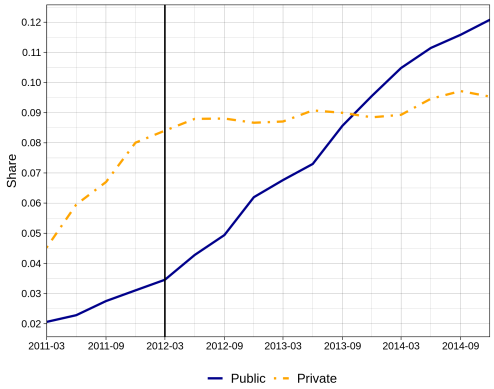
This figure decomposes the liability by bank type and quarter. *Public* (government-owned) banks are Banco do Brasil (BB) and Caixa Economica Federal (CEF). Private banks are Bradesco, HSBC, Itau Unibanco, and Santander. Each variable is shown as a share of total liabilities. The variables are equity (panel a), deposits (panel b), real estate, mortgage and similar notes and debentures (panel c), onlending (mostly from government funds, panel d), repurchase agreements (repos, panel e), and other liabilities (panel f). For each bank type, we compute the shares as if each type of bank is an institution, that is, the within-bank type sum of a given liability over the within-bank type sum of total liabilities.The vertical line indicates the start of the intervention. Sources: IF.data, and authors' calculations.



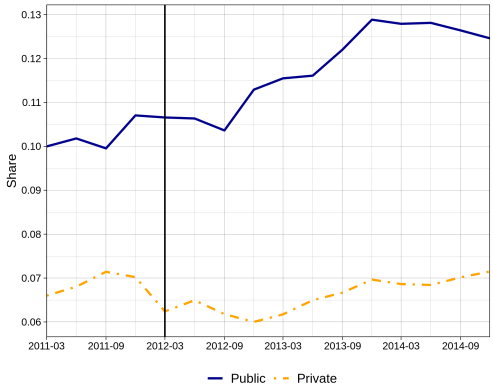
(a) Equity



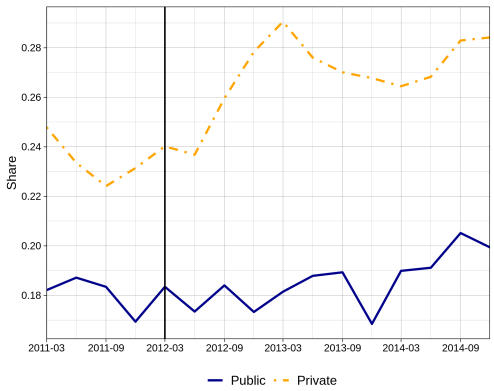
(b) Deposits



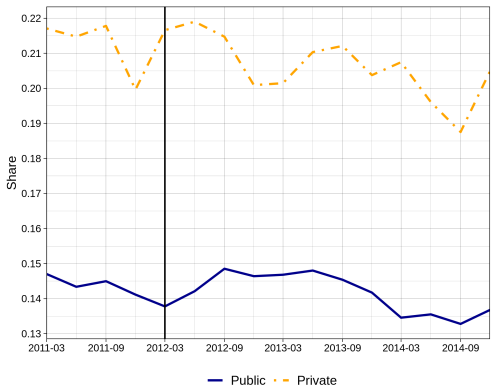
(c) Securities



(d) Onlending



(e) Repos



(f) Other

Figure A.7: Banks' Return on Equity (ROA): Public and Large Private banks

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.

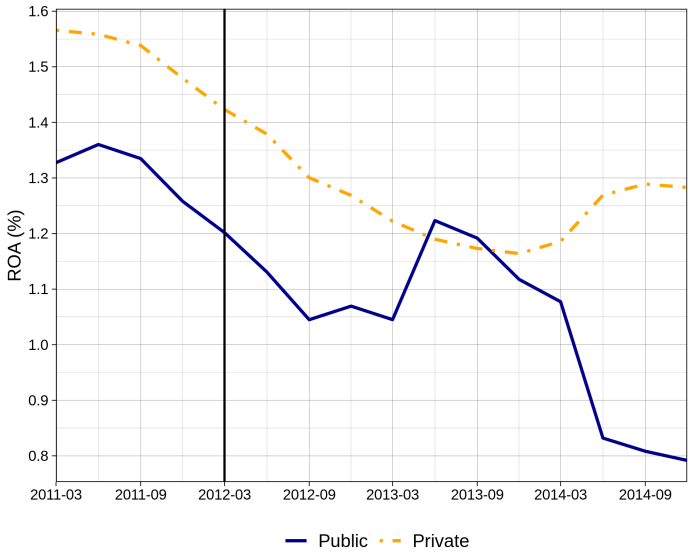
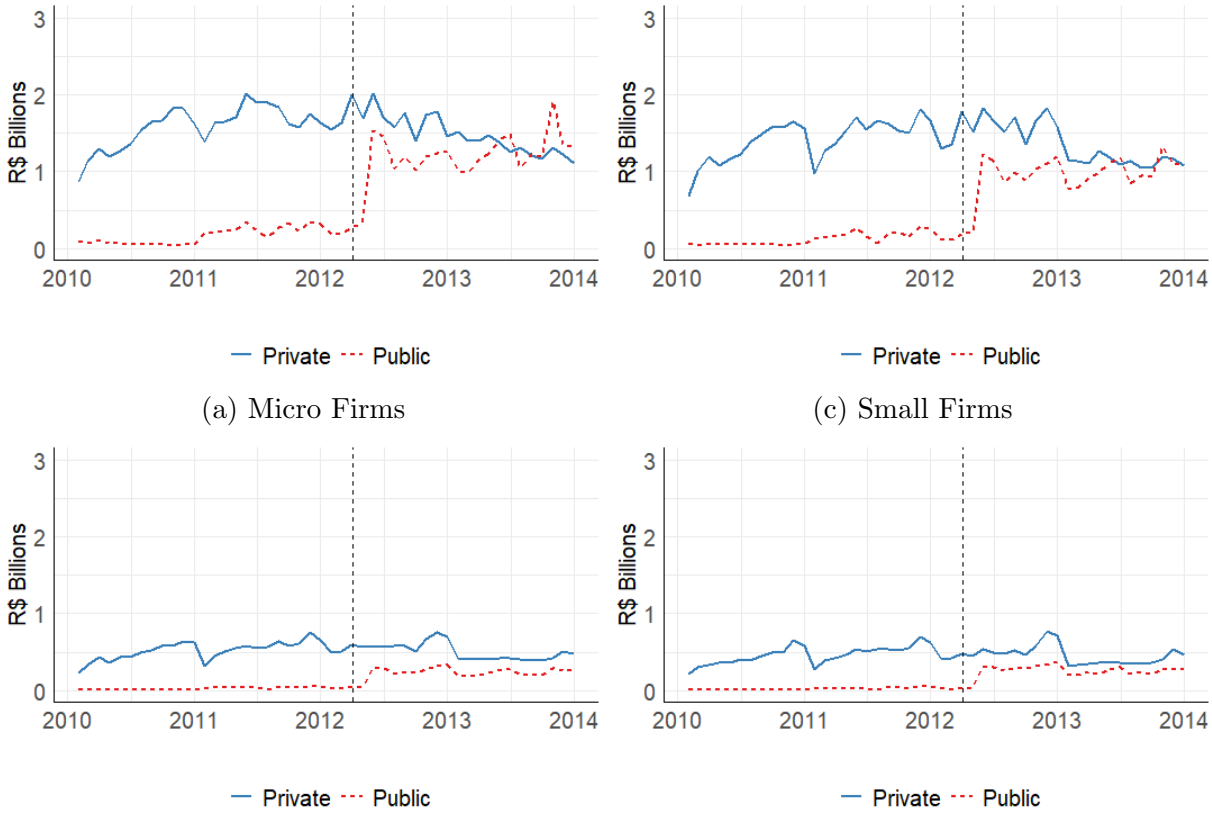


Figure A.8: Working Capital Origination by Firm Size

This figure shows the monthly origination of uncollateralized working capital loans to firms by type of bank and firm size. *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. Firm-size is defined as follows. Micro firms: firms in the service/commerce sectors with fewer than 10 employees, or in the industry sectors with fewer than 20 employees. Small firms: firms in the service/commerce sectors with more than 10 and fewer than 50 employees, or in the industry sectors with more than 20 and fewer than 100 employees. Medium firms: firms in the service/commerce sectors with more than 50 and fewer than 100 employees, or in the industry sectors with more than 100 and fewer than 500 employees. The vertical line indicates the start of the intervention (March, 2012). Sources: Credit Information System (SCR), and authors' calculations.



(a) Micro Firms

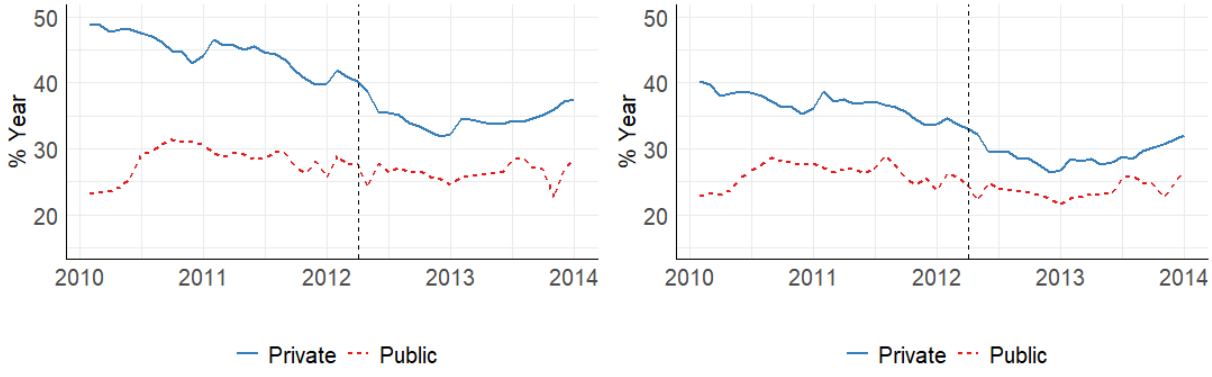
(c) Small Firms

(b) Medium Firms

(d) Large Firms

Figure A.9: Working Capital APRs by Firm Size

This figure shows the APR of newly originated uncollateralized working capital loans to firms by type of bank and firm size. *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. Firm size is defined as follows. Micro firms: firms in the service/commerce sectors with fewer than 10 employees, or in the industry sectors with fewer than 20 employees. Small firms: firms in the service/commerce sectors with more than 10 and fewer than 50 employees, or in the industry sectors with more than 20 and fewer than 100 employees. Medium firms: firms in the service/commerce sectors with more than 50 and fewer than 100 employees, or in the industry sectors with more than 100 and fewer than 500 employees. The vertical line indicates the start of the intervention (March 2012). Sources: Credit Information System (SCR), and authors' calculations.



(a) Micro Firms

(b) Small Firms

(c) Medium Firms

(d) Large Firms

Figure A.10: Borrower Delinquency: Public and Private Banks

This figures shows the estimates of γ_τ from: $I^D_{tmbfs} = \alpha_{tms} + \alpha_b + \alpha_{f(size)} + \sum_{\tau \neq -1} \gamma_\tau \cdot Public_b + \varepsilon_{tmbfs}$, 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.

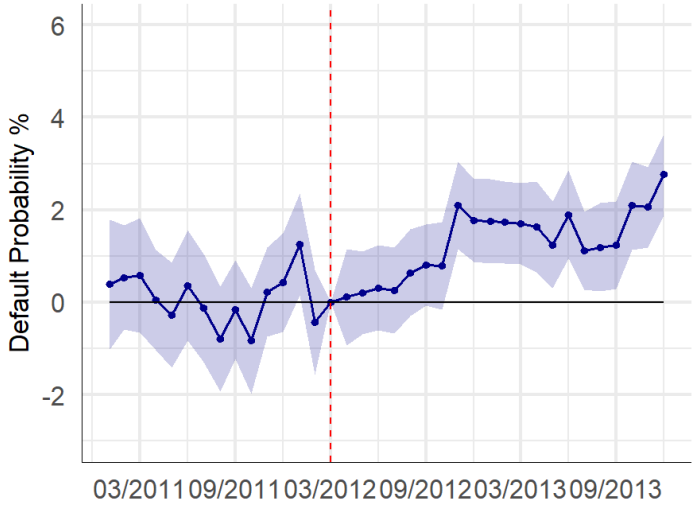


Figure A.11: Default Risk Differences: Leverage Heterogeneity by Bank Type

This figure shows the estimates of β_l in: $I^D_{tmbfs} = \alpha_{mts} + \alpha_b + \alpha_{t,f(size)} + \sum_l \beta_l \times Ind^l_f + \varepsilon_{tmbfs}$, 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 Ind^l_f are indicator variables equal to one if firm f belongs to the l-th leverage quintile. We estimate this regression at the firm-bank level for public and private banks separately. Leverage is calculated as the debt-to-payroll ratio. The first quintile is our reference category (omitted). Shaded areas are the 95 confidence intervals. Standard errors are clustered at the bank-municipality level. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

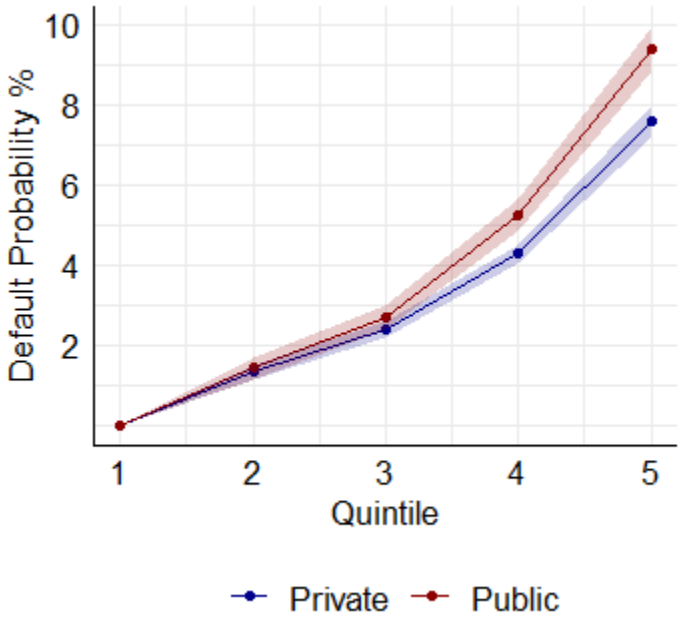


Figure A.12: Share of Originations to Unlevered Firms

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.

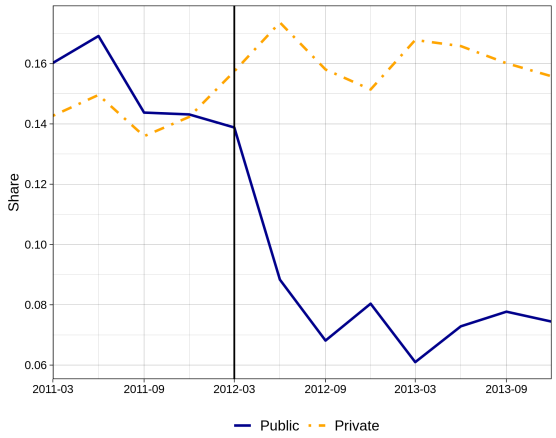


Table A.1: Credit Growth at the Regional Level: Branch vs. Borrower Location (No Entry Sample)

This table shows the estimates of the β's on Equation (8). This table is equivalent to Table 3 but for a different set of municipalities. The municipalities in this sample are those that had no previously absent bank entry after January of 2011. **Panel A**: $Public_m$ is an indicator function that is one if municipality m has a branch form a public bank. **Panel B**: $Public_m$ it is the share of the total amount outstanding of working capital loans that is from public banks for firms in municipality m in December of 2011. We run Equation (8) with four different dependent variables. Column (1): growth in originations of working capital loans (computed as in Equation (6)). Column (2): log of the amount outstanding of working capital loans for firms in a given municipality. Column (3): log of the amount outstanding of loans for firms and households in branches in a given municipality. Column (2): log of the amount outstanding of all loans for firms in a given municipality. Standard errors are clustered at the state level. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.

	Panel A. Branch Presence			
	Working Capital		Branches	Firms
	Originations	Outstanding	Outstanding	Outstanding
Public Branch × 2012	0.0023	-0.0670**	0.1423**	-0.0186
	(0.0884)	(0.0267)	(0.0551)	(0.0320)
Public Branch × 2013	-0.1017	-0.1460***	0.2597***	-0.0705
	(0.0781)	(0.0423)	(0.0720)	(0.0471)
Mun FE	Yes	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes	Yes
Observations	8,355	8,355	8,355	8,355
R^2	0.61586	0.97528	0.99600	0.98172

	Panel A. Share of Working Capital Outstanding			
	Working Capital		Branches	Firms
	Originations	Outstanding	Outstanding	Outstanding
Public Share (2011) × 2012	1.120***	0.4305***	0.0170	0.2326***
	(0.1732)	(0.0865)	(0.0110)	(0.0561)
Public Share (2011) × 2013	1.931***	0.7704***	0.0408**	0.5479***
	(0.2533)	(0.0853)	(0.0148)	(0.0633)
Mun FE	Yes	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes	Yes
Observations	8,355	8,355	8,355	8,355
R^2	0.62428	0.97665	0.99565	0.98238

Table A.2: Credit Growth at the Regional Level: Branch vs. Borrower Location (No Entry Sample), Weighted by Population

This table shows the estimates of the β's on Equation (8). This table is equivalent to Table A.1 but using baseline population as weights in the estimation. **Panel A**: $Public_m$ is an indicator function that is one if municipality m has a branch form a public bank. **Panel B**: $Public_m$ it is the share of the total amount outstanding of working capital loans that is in public banks for firms in municipality m in December of 2011. We run Equation (8) with four different dependent variables. Column (1): growth in originations of working capital loans (computed as in Equation (6)). Column (2): log of the amount outstanding of working capital loans for firms in a given municipality. Column (3): log of the amount outstanding of loans for firms and households in branches in a given municipality. Column (2): log of the amount outstanding of all loans for firms in a given municipality. Standard errors are clustered at the state level. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.

	Panel A. Branch Presence			
	Working Capital		Branches	Firms
	Originations	Outstanding	Outstanding	Outstanding
Public Branch × 2012	-0.2945***	-0.1346***	0.1522***	-0.0844***
	(0.0515)	(0.0220)	(0.0481)	(0.0230)
Public Branch × 2013	-0.5982***	-0.2056**	0.2488***	-0.0985
	(0.0913)	(0.0989)	(0.0711)	(0.0906)
Mun FE	Yes	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes	Yes
Observations	8,355	8,355	8,355	8,355
R^2	0.61910	0.99557	0.99942	0.99600

	Panel A. Share of Working Capital Outstanding			
	Working Capital		Branches	Firms
	Originations	Outstanding	Outstanding	Outstanding
Public Share (2011) × 2012	0.9048***	0.3822***	0.0629***	0.2396***
	(0.1226)	(0.0489)	(0.0212)	(0.0382)
Public Share (2011) × 2013	1.629***	0.7282***	0.1633***	0.5280***
	(0.2075)	(0.0779)	(0.0304)	(0.0724)
Mun FE	Yes	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes	Yes
Observations	8,355	8,355	8,355	8,355
R^2	0.63884	0.99592	0.99942	0.99620

Table A.3: Real Effects at the Regional Level: Branch vs. Borrower Location Weighted by Population

This table shows the estimates of the β's on Equation (8). This table is equivalent to Columns 4-6 of Table 4, but using population weights in the estimation. The municipalities in the sample are those that had no previously absent bank entry after January of 2011. **Panel A**: $Public_m$ is an indicator function that is one if municipality m has a branch form a public bank. **Panel B**: $Public_m$ it is the share of the total amount outstanding of working capital loans that is from public banks for firms in municipality m in December of 2011. We run Equation (8) with three different dependent variables: log of GDP (Column 1), log of employment (Column 2), and log of total payroll (Column 3). Standard errors are clustered at the state level. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Brazilian Institute of Geography and Statistics (IBGE), Annual Review of Social Information (RAIS), and authors' calculations.

	Panel A. Branch Presence		
	GDP	Emp.	Payroll
Public Branch × 2012	0.0135*	0.0137	-0.0039
	(0.0072)	(0.0359)	(0.0332)
Public Branch × 2013	0.0005	-0.0139	-0.0337
	(0.0081)	(0.0241)	(0.0205)
Mun FE	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes
Observations	8,355	8,355	8,355
R^2	0.99949	0.99940	0.99936
	Panel B. Share of WK Outstanding		
	GDP	Employment	Payroll
Public Share (2011) × 2012	0.0031	-0.0043	0.0456***
	(0.0136)	(0.0155)	(0.0137)
Public Share (2011) × 2013	0.0444**	0.0375*	0.0828***
	(0.0209)	(0.0184)	(0.0246)
Mun FE	Yes	Yes	Yes
Year-State FE	Yes	Yes	Yes
Observations	8,355	8,355	8,355
R^2	0.99949	0.99940	0.99936

Table A.4: Differences in Interest Rates by Firm Size: Public and Private Banks

This table shows the average differences in interest rates for firms of different size before and after the intervention. We estimate at the loan level the following specification: $i_{jtmbfs} = \alpha_{tms} + \alpha_b + \sum_{\tau \in \{2,3,4\}} \delta_\tau + \sum_{\tau \in \{2,3,4\}} \gamma_\tau \cdot Post_t + \varepsilon_{jtmbfs}$, where i_{jtmbfs} denotes the interest rate of a loan j issued in month t municipality m by bank b to firm f in industry s, α_{tms} are time-municipality-size fixed effects, α_b are bank fixed effects, $\tau \in \{2, 3, 4\}$ correspond to the size bins for small, medium and large firms, and $Post_t$ is an indicator if month t is after March 2012. Firm-size is defined as follows. Micro firms: firms in the service/commerce sectors with fewer than 10 employees, or in the industry sectors with fewer than 20 employees. Small firms: firms in the service/commerce sectors with more than 10 and fewer than 50 employees, or in the industry sectors with more than 20 and fewer than 100 employees. Medium firms: firms in the service/commerce sectors with more than 50 and fewer than 100 employees, or in the industry sectors with more than 100 and fewer than 500 employees. Coefficients δ_τ and γ_τ estimate the difference in interest rates paid by firms of size τ relative to micro firms in the baseline and post intervention periods. Standard errors clustered at the bank-municipality level. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Credit Information System (SCR), Annual Review of Social Information (RAIS), and authors' calculations.

	Public Banks	Private Banks
Small Firms	-2.1506^{***}	-7.5576^{***}
	(0.0864)	(0.1099)
Medium Firms	-4.2603^{***}	-12.4970^{***}
	(0.1988)	(0.1695)
Large Firms	-5.7714^{***}	-15.5565^{***}
	(0.2496)	(0.2897)
Post × Small	-0.5971^{***}	1.3918^{***}
	(0.0855)	(0.1434)
Post × Medium	-0.5728^{***}	2.4843^{***}
	(0.1918)	(0.2053)
Post × Large	-0.5213^{***}	2.9530^{***}
	(0.2522)	(0.2735)
Time × Ind × Mun FE	Yes	Yes
Bank FE	Yes	Yes
R^2	0.332	0.355
Observations	845279	1402587

B Regional Credit Allocation

In this section, we explore the regional credit allocation following the intervention. First, we show that there were no systematic differences across municipalities in terms of their credit growth based on political affiliations. Second, we show that other municipalities' characteristics also cannot explain the observed growth in credit.

Political Capture. 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)). To test if there is a political influence in the allocation of loans in our experiment, we run the following regression at the municipality m level:

$$\text{Credit Growth}_m = \alpha_s + \beta \cdot \textit{Same Party}_m + \varepsilon_m, \tag{9}$$

where *Credit Growth* is a measure of credit growth, α_s are state fixed effects, and $Ally_m$ is an indicator variable if the mayor of municipality m is in the same party as the president. These data on local elections are publicly available and are provided by Superior Electoral Court (TSE). We focus the political capture analysis at the municipality level (and not at the state level, for instance) since mayoral elections took place in October 2012, while gubernatorial and presidential elections did not take place until 2014.

To account for municipality pre-intervention exposure to public banks, we use the following within-share growth measure:

$$\%\Delta^{within} Orig_m = \frac{1}{2} \cdot \frac{\text{Share Post} - \text{Share Pre}}{\text{Share Post} + \text{Share Pre}}, \tag{10}$$

where shares are computed from originations in the pre- and post-intervention periods within bank types. 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, this implies that $\%\Delta^{within} Orig_{pub,m} = 0$ everywhere—and thus the allocation is not systematically geared to borrowers or branches in municipalities controlled by political allies. We compute a similar within-measure of growth for the outstanding volume of working capital loans for borrowers in a given municipality and the total amount of credit outstanding from branches in a given municipality. Since these last two are stock variables, instead of using all periods before and after the intervention, we use simply the period before and the last period in our sample.

The results are shown in Table B.1. We show our results both unweighted and weighted by population. We do find a systematic larger increase in working capital origination or amount outstanding for borrowers in a given municipality (Panel A). We do find an increase

in credit from branches located in municipalities where the mayor is a political ally, but the effect is economically small. For reference, the standard deviation across municipalities of the growth of credit outstanding in branches was 0.097. In Panel B, we extend Equation (9) to include an indicator (and its interaction with $Same\ Party_m$) if the previous election (in 2008) was contested. We define a contested election as one where there was either a second round and in the second round a candidate won with less than 55 percent of all votes, or if there was no second round, but the winner in the first round won with less than 50 percent of the votes.[26] The idea behind this exercise is that the political incentives to increase credit are larger in municipalities where elections are more competitive. We find a statistically significant increase in the outstanding volume of working capital loans, but this effect is also economically small. For reference, the standard deviation across municipalities of the growth in the volume outstanding of working capital loans was 0.19 (unweighted) and 0.11 (weighted). Note also that the share of municipalities where elections were contested and the mayors were from the same party as the president is approximately 2 percent.[27] Therefore, this heterogeneous allocation result is not driving any of the results in the main text.

Importantly, the period we analyze is marked by public bank entry following the intervention (Figure B.1). We define bank entry as a previously absent bank opening a branch in a new municipality. When we repeat our analysis on a sample of municipalities with no bank entry, we do not find any systematically different allocation of credit. This suggests that the political capture channel was working partly through openings of new branches, rather than relative credit growth for existing branches. Our results are different from those in the Lazzarini et al. (2015), Carvalho (2014) and others in the government-banks literature. This distinction comes from two sources. First, unlike the firms evaluated in these papers, the firms in our sample are small and thus unlikely to have political connections. Second, the intervention we analyze takes place close to a mayoral election, but we do find significant spillovers across municipalities in terms of credit allocation.

Other Characteristics. We conduct a similar analysis with other municipality characteristics (before the intervention) to understand if there were other systematic differences in the allocation of credit. To do so, we replace the right-hand variable in Equation (9). The results are shown in Table B.3. We focus on the within-growth in the volume outstanding of working capital loans for firms in a given municipality for this result. The overall patterns are robust if we use our difference measures of local credit growth. We do not find any economically significant difference in credit allocation across our samples. For reference, the standard deviation of the HHI of private credit, credit per capita, and industrial share (weighted) are,

[26] Municipalities with fewer than 200,000 residents do not have second rounds for mayoral elections.

[27] We arrive at a similar figure on the share of the population in these municipalities.

respectively, 0.42, 2.94, and 0.14. Given that the standard deviation in our measure of credit growth is 0.19 (unweighted) and 0.11 (weighted), a one standard deviation change in any of these statistically significant coefficients does not represent a significant increase in credit growth. Moreover, these effects are not robust across samples and weighting schemes. Note that this does not imply that the allocation was not heterogeneous across municipalities, but rather that it wasn't systematically heterogeneous *beyond* baseline exposure. We document the dependence of the allocation on baseline exposure in Section 5.2 and use it as a source of variation to estimate the regional effects of the intervention.

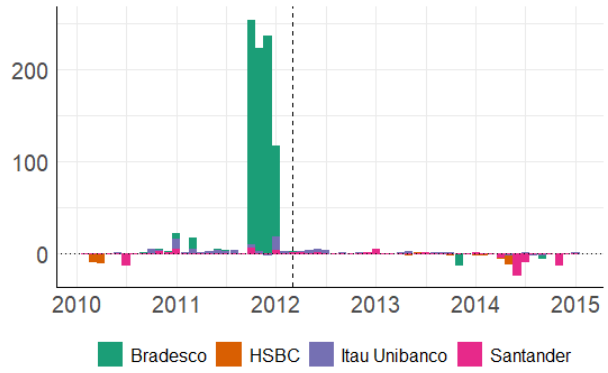
Table B.1: Political Capture and Credit Growth

This table shows the estimates of the β's from estimating regression in Equation (9). In all cases, we run this regression with state fixed effects, α_s. We run this regression with six different dependent variables. These are based on working capital originations (*WK Originations*) for firms in that municipality, working capital outstanding (*WK Outstanding*) for firms in that municipality, and total credit outstanding in branches from that municipality (*Outstanding (Branches)*). We compute these for public banks only and for both types of banks. For the *WK Originations*, we use the total amount originated in pre and post periods as described in Equation (10). For the outstanding measures, we use the baseline and end of the sample amount of credit. The *Same Party*$_m$ variable is an indicator if the mayor of municipality m is from the same party as the president. The *Contested* variable is an indicator if the 2008 election was contested (see text for definition). The weights for panels C and D are population (baseline). Standard errors are clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Superior Electoral Court (TSE), Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN) and authors' calculations.

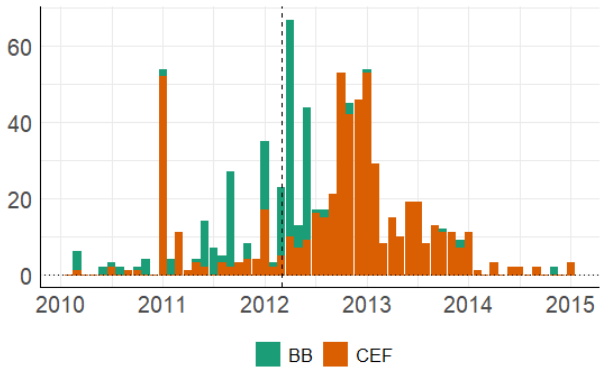
	WK Originations		WK Outstanding		Outstanding (Branches)	
	Public	All	Public	All	Public	All
	Panel A: Level, Unweighted					
Same Party	0.0055	0.0040	-0.0006	-0.0127*	0.0074**	0.0059
	(0.0129)	(0.0094)	(0.0095)	(0.0073)	(0.0031)	(0.0039)
Obs	4,618	4,618	4,618	4,618	4,618	4,618
	Panel B: Interaction, Unweighted					
Same Party × Contested	-0.0050	0.0246	0.0440*	0.0420**	0.0012	0.0026
	(0.0390)	(0.0196)	(0.0217)	(0.0195)	(0.0142)	(0.0072)
Obs	4,618	4,618	4,618	4,618	4,618	4,618
	Panel C: Level, Weighted					
Same Party	-0.0029	-0.0183**	0.0157	0.0025	0.0063	-0.0037
	(0.0082)	(0.0081)	(0.0105)	(0.0065)	(0.0060)	(0.0049)
Obs	4,618	4,618	4,618	4,618	4,618	4,618
	Panel D: Interaction, Weighted					
Same Party × Contested	0.0042	0.0255	0.0315*	0.0121	-0.0006	0.0083*
	(0.0269)	(0.0172)	(0.0183)	(0.0140)	(0.0093)	(0.0045)
Obs	4,618	4,618	4,618	4,618	4,618	4,618

Figure B.1: Branch Openings in New Municipalities

This figure shows branch openings in new municipalities by the five largest private banks and the two public banks that are the focus of our study. Entry is defined by a bank opening a branch in location where it had no previous presence. Sources: Monthly Bank Statistics by Municipality (ESTBAN), and authors' calculations.



(a) Private Banks



(b) Public Banks

Table B.2: Political Capture and Credit Growth: No Entry Sample

This table is equivalent to Table B.1. The only difference is that we include only municipalities that had no branch openings in our sample. Standard errors are clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Superior Electoral Court (TSE), Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN) and authors' calculations.

	WK Originations		WK Outstanding		Outstanding (Branches)	
	Public	All	Public	All	Public	All
	Panel A: Level, Unweighted					
Same Party	0.0053	0.0013	-0.0047	-0.0209**	0.0040*	0.0023
	(0.0133)	(0.0091)	(0.0083)	(0.0077)	(0.0022)	(0.0021)
Obs	3,881	3,881	3,881	3,881	3,881	3,881
	Panel B: Interaction, Unweighted					
Same Party × Contested	-0.0107	0.0213	0.0348*	0.0381*	-0.0066**	-0.0016
	(0.0351)	(0.0258)	(0.0200)	(0.0198)	(0.0026)	(0.0037)
Obs	3,881	3,881	3,881	3,881	3,881	3,881
	Panel C: Level, Weighted					
Same Party	-0.0063	-0.0192**	0.0134	0.0011	0.0069	-0.0041
	(0.0077)	(0.0070)	(0.0105)	(0.0069)	(0.0062)	(0.0038)
Obs	3,881	3,881	3,881	3,881	3,881	3,881
	Panel D: Interaction, Weighted					
Same Party × Contested	0.0015	0.0228	0.0267	0.0055	-0.0058	0.0038
	(0.0228)	(0.0189)	(0.0169)	(0.0142)	(0.0073)	(0.0056)
Obs	3,881	3,881	3,881	3,881	3,881	3,881

Table B.3: Baseline Regional Characteristics and Credit Growth

This table shows the estimates of the β's from estimating regression in Equation (9) but with different dependent variables. We run this regression with state fixed effects, α_s. The dependent variable is the within-share growth of working capital outstanding (*WK Outstanding*) for firms in that municipality as described in Equation (10) using the baseline and end of the sample amount of credit. The right hand side variables in this case are municipality characteristics measured at the baseline. These are: GDP per Capita (R$ 1,000), the HHI of Private Credit (measured from ESTBAN), the share of output from the agricultural and industrial sectors and Total Credit Per Capita (R$ 1,000, measured from ESTBAN). The weights for the last two columns are population (baseline). The independent Standard errors are clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: Superior Electoral Court (TSE), Credit Information System (SCR), Monthly Bank Statistics by Municipality (ESTBAN) and authors' calculations.

	Unweighted		Weighted	
	All Municipalities	No Entry	All Municipalities	No Entry
GDP per Capita (R$ 1,000)	-4.9×10^{-5}	-9.49×10^{-5}	-9.63×10^{-5}	1.16×10^{-6}
	(0.0001)	(0.0001)	(0.0002)	(0.0007)
HHI Private Credit	0.0205***	0.0272***	-0.0294	0.0613
	(0.0055)	(0.0097)	(0.0350)	(0.0371)
Agricultural Share	-0.0401	-0.0311	-0.0338	-0.0191
	(0.0277)	(0.0290)	(0.0463)	(0.0828)
Industrial Share	0.0140	0.0103	0.0688*	-0.0025
	(0.0225)	(0.0220)	(0.0364)	(0.0335)
Credit per Capita (R$ 1,000)	-0.0017*	-0.0016*	-0.0005	-0.0010
	(0.0010)	(0.0010)	(0.0026)	(0.0020)