Bank Competition, Cost of Credit and Economic Activity: Evidence from Brazil

Job Market Paper

Gustavo Joaquim *† — MIT

Bernardus van Doornik — Central Bank of Brazil

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Abstract

We use heterogeneous exposure to large bank mergers to estimate the effect of bank competition on both financial and real variables in local Brazilian markets. Using detailed administrative data on loans and firms, we employ a difference-in-differences empirical strategy to identify the causal effect of bank competition. Following M&A episodes, spreads increase and there is persistently less lending in exposed markets. We also find that bank competition has real effects: a 1% increase in spreads leads to a 0.2% decline in employment. We develop a tractable model of heterogeneous firms and concentration in the banking sector. In our model, the semi-elasticity of credit to lending rates is a sufficient statistic for the effect of concentration on credit and output. We estimate this elasticity and show that the observed effects in the data and predicted by the model are consistent. Among other counterfactuals, we show that if Brazilian spreads fall to world levels, output would increase by approximately 5%.

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†Corresponding author: gpgj@mit.edu

I. Introduction

The banking sector plays a central role in the functioning of the economy (e.g., Bernanke (1983)) and it is extremely concentrated: averaging across countries, the share of assets held by the 5 largest banks in each country is 78%,[1] and is recently increasing in various markets. In the U.S., for instance, the share of assets held by the 5-largest banks increased from 30% in the mid 1990's to more than 45% in 2016. In Brazil, this share grew from 50% to more than 85% in the same time span. Despite, the importance of banks and the potential consequences from decreased bank competition, there is still a limited understanding of its effects.

From a theoretical perspective, traditional industrial organization models predict that less competition will lead to higher interest rates and lower access to credit though movements along the demand curve. However, as shown in Petersen and Rajan (1995), theoretical banking specific models that take into account information problems and bank-firm relationships predict that less bank competition can increase credit access and decrease interest rates (or have a non-monotonic relationship).[2] We observe similar ambiguity in empirical work. Identifying the effect of bank competition is challenging due endogeneity, and for any source of identification (cross-industry analysis, geographic branching deregulation, etc.) there is evidence that supports the traditional IO view and, alternatively, evidence that the relationship lending/informational channel is such that competition can be detrimental to access to credit.[3]

In this paper, we use M&A episodes of large Brazilian banks as a source of exogenous variation of competition in local banking markets to identify the causal effect of bank competition. We focus on the Brazilian market for various reasons. First, bank lending represents

[1] For data sources, see Appendix C.

[2] Less competition increases a creditors ability to extend credit based on the intertemporal ability of the firm to generate cash, while a competitive market requires creditors to break even period by period. Therefore, in markets with risk and asymmetric information, competition among financial intermediaries reduces the space of contracts available and thus access to credit (and potentially increases the cost of finance. See Degryse and Ongena (2007) for a short summary of the literature.

[3] For the traditional IO view: See Cetorelli and Gambera (2001), Beck, Demirgüç-Kunt and Maksimovic (2004), Cetorelli and Strahan (2006), Black and Strahan (2002), Strahan et al. (2003), Rice and Strahan (2010) , Gao et al. (2019) and others. For relationship lending/informational channel and detrimental effect of competition: Petersen and Rajan (1994), Petersen and Rajan (1995), Shaffer (1998), Berger et al. (1998), Patti, Bonaccorsi and Dell'Ariccia (2004), Presbitero and Zazzaro (2011), Zarutskie (2006), Jiang, Levine and Lin (2019) , Fungáčová, Shamshur and Weill (2017) and others.

close to 52% of external finance in Brazil, close to the international average of 55%.[4] Second, Brazil is more representative of a set of developing countries where access to finance is a major constraint on firm growth. For instance, 45% firms in Brazil report that access to finance is a major constraint to growth and 43.7% of investments (and not only working capital) is funded by banks. Finally, the Brazilian Central Bank (BCB) credit registry has information on large M&A episodes and rich loan and firm level data.

We use a difference-in-differences (DiD) framework to estimate the effect of bank competition in financial and real outcomes. We say a market is 'treated' by a merger if it contained at least one branch from each of the banks involved in the merger. Although the decision of two large banks to merge is not exogenous,it is unlikely to be related to economic conditions between markets.[5] The identifying assumption is that absent an M&A episode in a local banking market, the outcomes in treated and non-treated municipalities would have followed parrallel trajectories. Consistent with the evidence in Nguyen (2019) and others that banking markets are local, we use in our benchmark setting the Brazilian municipalities as local banking markets, and we conduct our benchmark analysis at the municipality-month level.

Our set of empirical results is divided in four parts. First, we focus on the M&A effect on financial variables. We show that before M&A episodes, the level of competition, lending spreads and total loan volume (of new loans) follow parallel trends in treated and non-treated markets. After an M&A episodes, we find an average increase on local concentration (HHI) of .11, which is a little larger than the effect of going from 4 to 3 symmetric banks. Moreover, we find a positive and significant effect on market level spreads of approximately 5.88 percentage points (16% of sample average) and a reduction in new credit origination of 17.3% in loans from private banks to firms in treated markets. Unlike Garmaise and Moskowitz (2006) and others, we find that the effect on lending is persistent and that there is no subsequent entry after changes in competition. As predicted by standard competition models, we find that the effect of a merger is smaller with less concentrated markets at the moment of the episode, and can be close to zero for markets with more than 6 or 7 banks. Finally, our results are robust to which markets or loans we include in the analysis, and how we define banking

[4]The U.S. market is an outlier both in terms of share of bank lending in total external finance and concentration, at, respectively, second and sixth lowest across countries in Beck, Demirgüç-Kunt and Levine (1999).

[5]For instance, it can depend of national economic conditions.

markets. As our main dataset is proprietary, we also show that our results are consistent with results obtained using public data only in a case study of the largest merger in our sample.

Second, we focus on the role of bank-firm relationships and assess alternative explanations. In our sample, we do not find evidence that the firm-bank relationships emphasized in the theoretical (e.g., Petersen and Rajan (1995)) and empirical (e.g., Zarutskie (2006)) literatures are of first order importance in our setting. We do not observe a differential effect for small firms - which are more dependent of bank relationships - and negligible changes in the age of borrowers, loan maturity and the share of loans made between firms and banks with previous relationships. In terms of alternative explanations, we do not find similar effects for markets with only one of the banks involved in the M&A, indicating that our results are not driven by changes in the ownership structure of banks. Further, we find no evidence of branch closures (or openings), which Nguyen (2019) finds is the main driver in reducing credit following M&As in the U.S. due to the destruction of bank-firm relationships.

Third, we separate our sample into sectors and show the effects of competition on real variables. The effect on firms is a composition of the firm side (less and more expensive credit) and a lower demand for goods and services from households potentially facing higher spreads in consumer loans, mortgages etc. Firms in the tradable sector are less affected by the local demand for goods, while firms in the non-tradable sector are treated to both a lower demand and less credit (Mian, Sufi and Verner (2019)). We show that our results for financial variables are consistent across sectors, and that bank competition has a significant and negative effect on employment and wages. Importantly, we find no effect in the agricultural sector. Agricultural credit in Brazil is the target of several credit policies and only 25% of loans are obtained through competitive bank lending, such that it provides a placebo test in our setting. We estimate that the elasticity of payroll (non-agricultural) to lending spreads is -.2: for a 1% increase in spreads, there is a .2% bps decrease in total payroll.

Finally, we test for the presence of geographical spillovers across municipalities. We observe a significant effect on financial variables for municipalities close to those affected by a merger, and that this effect is decreasing at higher level of geographical aggregation. Quantitatively, however, the results are much smaller than our benchmark estimates. This is also reflected in the spillover effect for real variables, where we find a significantly smaller spillover effect on employment and wages when compared to municipalities directly exposed to the mergers.

Given our empirical results, we develop an analytically tractable model of bank competition that can be tested and used for counterfactuals that extend beyond our reduced form results. As in the data, our model consists of various independent markets, each with their own level of bank competition. Each market has heterogeneous firms that need external finance. With external finance, firms can increase the amount of capital and labor they use in production. Each bank has its own cost of providing loans, and banks compete à la Cournot (by choosing the quantity of credit in a given market). Under our functional form assumptions, each bank in the economy faces a downward sloping demand for bank credit with a constant semi-elasticity. Our model makes two quantitative predictions. First, individual bank optimization implies that the semi-elasticity is a sufficient statistic that relates local concentration to lending spreads. The idea is that beyond local concentration, the aggregate sensitivity of local credit demand to interest rates determines the equilibrium price in a oligopolistic setting. Second, we show that the same semi-elasticity that determines equilibrium rates in a market is also the sufficient statistic for the effect of spreads (and thus bank concentration) on output and total payroll. We show that the semi-elasticity multiplied by the share of capital that is competitively supplied by banks (i.e., not target of any credit policy or from other sources of external finance) is the effect of spreads on output, as output and credit in our economy our closely related.

We use a DiD instrumental variable framework to estimate our key parameter (the semi-elasticity of local demand for bank credit) using exposure to the merger as a supply shifter. We estimate this semi-elasticity to be -3.17: for a 1 percentage point change in spreads, local demand for credit falls 3.17%. Given the semi-elasticity estimate, the data are consistent with the two quantitative predictions of our model: the change in spreads implied by the change in concentration and that the effect of spreads on total payroll. We then use our model to investigate three counterfactuals: the introduction of a new bank in every market, active competition of public banks in markets they are already located in and, finally, a reduction of spreads in all markets in Brazil to the global average of 5.43 percentage points. In the last exercise, our results indicate that output would grow by almost 5%. As firms are constrained in Brazil, the share of production that the corporate sector can retain and invest is also important for output in the future, as highlighted in Itskhoki and Moll (2019).[6] In our counterfactual, we find that beyond the static effect on output, the corporate sector share of production profits would increase by 6.51 percentage points, increasing significantly the speed of capital accumulation and thus output in the future.

Finally, although we find limited evidence of efficiency gains between merging banks in our setting, we use our model to understand how big these potential efficiency gains would have to be to compensate for a higher local concentration. We find that in municipalities with both merging banks, where local concentration is and efficiency in the banking sector increase, the overall cost of the banking sector would have to fall by 30% to compensate for the loss of competition. Ultimately, our results indicate that the aggregate effect of M&As - or other policies that affect the cost structure and local concentration simultaneously - will depend on the share of municipalities exposed to the M&A and those exposed only in efficiency, but that competition plays a key quantitative role in the determination of spreads and real variables even in the presence of potential efficiency gains.

Literature Review. Despite extensive research, the effects of bank competition on financial and real outcomes has not yielded definitive results - either theoretically or empirically. Branching deregulation episodes are the most credible identification technique used in the literature, but these episodes does not shed much light on bank competition. Following branching deregulation episodes in the U.S., the HHI index remained the same as smaller local banks were acquired by larger and more efficient banks (Black and Strahan (2002)). As shown in Jayaratne and Strahan (1996) and others, the real effects of the deregulation come mostly from quality of loans (increased efficiency of banks) instead of volume. Further, branching deregulation changes the ability of banks to geographically diversify risks (Goetz, Laeven and Levine (2016)), can reduce their funding costs (Levine, Lin and Xie (2019)), and introduces incumbent-entrant information asymmetries that can be more relevant than competition *per se* (Gao et al. (2019)). Two recent papers on bank competition with identification efforts that do not rely on branching deregulation are Liebersohn (2018) and Carlson, Correia and Luck (2019). Liebersohn (2018) uses a discontinuity in the DOJ criteria to approve

[6] Itskhoki and Moll (2019) show that policies or interventions that facilitate capital accumulation in a financially constrained corporate sector are welfare increasing compared to laissez-faire. The intuition is that the returns to capital in the corporate sector are higher the cost of capital in the economy due to the financial friction, and thus increasing the overall ability of the corporate sector to save and invest can reduce this wedge over time.

mergers, while Carlson, Correia and Luck (2019) uses a discontinuity in entry costs created by regulation in the national banking era.

Our data and empirical setting has two key advantages over the bank competition literature. First, M&A of large banks is not subject to the criticisms of deregulation exercises, and we can deal with bank efficiency concerns by looking at control with only one of the treated banks. Second, we use monthly data from the Brazilian credit registry, local bank balance sheets and labor outcomes in Brazil that comprise essentially the universe of all loans and tax registered firms, not only loans to small business (as most of the branching deregulation) or commercial real estate (as Liebersohn (2018)). In particular, we can test for the relationship channel directly, since we know which firms had relationship with each bank before an M&A episode. We also contribute to this literature by providing a tractable model of bank competition that relates local concentration to lending spreads and lending spreads to real outcomes, and show that the predictions of the model are consistent with the data. Our model provides a bridge between the empirical work on bank competition and its aggregate effects from a macro perspective.

We also contribute to the literature on credit supply shocks. The evidence on credit supply shocks at levels of aggregation above the firm is still mixed. Recent evidence suggests that credit supply shocks affect real outcomes (e.g., Mian, Sufi and Verner (2019) in the U.S. and Huber (2018) in Germany). In particular, Fonseca and Van Doornik (2019) uses the same credit registry as our paper and shows that a creditor rights reform in Brazil in 2005 led to an expansion of credit and employment at constrained firms. On the other hand, Greenstone, Mas and Nguyen (2019) and others find negligible. As argued in Huber (2018), a reason for this inconsistency is that heterogeneity in regional exposure to shocks is small in some studies. We contribute to this literature by studying a large credit supply shock through changes in the banking market structure, and show that for a bank-dependent emerging market like Brazil, a competitive change in the banking sector can have larger effects than those found in Huber (2018) for a banking crisis. Moreover, by exploring the effects separately on tradable and non-tradable industries, we show that the well-identified finding in Mian, Sufi and Verner (2019) that credit supply shocks work through local demand in various settings is not present in our sample, and that firm financing is important for explaining real outcomes in other contexts.

This paper is broadly related to the effect of market power in the financial sector in the transmission of monetary policy (see, e.g., Drechsler, Savov and Schnabl (2017) for the deposit market). We credibly estimate sensitivity of bank credit to changes in bank interest rates, which is a key statistic in how monetary policy will be transmitted to the real economy in Wang et al. (2018), where firms have a logit demand system for bank credit. Finally, this paper also contributes to the macro-development literature that studies the static and dynamic effect of financial frictions(e.g. Buera and Shin (2013) and Moll (2014)). We show that beyond of contracting frictions, as usually highlighted in the literature, lack of bank competition can cause static inefficiencies in credit markets consistent with the data. Further, we show that a large cost of finance can have detrimental effects for savings and capital accumulation in the corporate sector, which Itskhoki and Moll (2019) claim is the key mechanism development policies should target.

Section II describes the data and shows characteristics for banking markets in Brazil. Section III discusses our empirical framework. Section IV presents the reduced form results on both financial and real variables, as well as the case study of the largest episode in our sample. Section V presents a theoretical model that is consistent with the evidence of section IV and presents the counterfactuals.

II. Data and Banking Markets in Brazil

Our empirical strategy uses M&A activity of large banks as a source of exogenous variation in competition in local market. In this section, we present the data, our definition of a banking market and characteristics of a market in our sample.

II.1. Data Sources

Our analysis combines four different data sources: (i) credit registry from the Brazilian Central Bank (BCB), (ii) physical location, balance sheets and branches of each bank by municipality, (iii) employer and employee data from the Brazilian Ministry of Labor and Employment, and (iv) real outcomes from the Brazilian Statistics and Geography Institute. In this section we discuss the main characteristics of each dataset and how we can merge them. For details on dataset construction and additional considerations, see Appendix C.

Credit Registry. The BCB collects and maintains data on loans made to firms in Brazil in SCR (*Sistema de Informações de Crédito*). The unit of observation is a loan. The dataset has loan-level information (interest rate, volume, collateral requirement etc.), together with firm zip code and firm and bank level identifiers. The information is reported monthly by banks to the BCB and must match bank's reported accounting figures. Initially, all loans above BRL 5000 (≈ US$ 1400) would be included in SCR. This limit decreased over time and currently all loans made above BRL 200 (≈ US$ 55) are included in SCR. Our sample uses monthly data from 2005-2015. We drop periods before 2005 due to data quality issues and after 2015 in the SCR dataset due to a large merge in June/2016 due and that we would not be able to analyze the post period to data limitations. [7] [8]

We exclude from our sample loans earmarked and and real state loans. First, earmarked loans account from roughly 50% of all loans in Brazil and 86% of them are subject to regulations such as interest rate caps and sector targets (Santos, 2016). Therefore, as most of the terms of these loans are not decided by the banks themselves, we exclude them from our sample. Second, the market for real state loans in Brazil was dominated by one public bank (*Caixa Economica Federal*) during the years of our study. Historically, *Caixa* held approximately 70 % of the market share in real-state lending. Therefore, we also exclude real-state lending from our sample. Finally, we exclude loans that are in default or renegotiation (based on loan rating) or that have missing information in rates, size, collateral requirement, maturity or firm zip code. We end with approximately 550 million loans across 2005-2015. Our sample of non-subsidized, non-real state and no-missing values loans includes on average (across years) 45% of loans made to firms when compared to the national accounts value of all loans to firms.[9]

To compute local concentration or number of banks, we use the definition that a bank is a *banking conglomerate*. We recover conglomerate structure (which banks belong to each conglomerate) and bank ownership (public and private) from the Unicad dataset. Unicad is a dataset maintained by the BCB with bank identifiers and which bank belongs to each

[7] The total number of loans recorded before May/2004 are less than 1% of loans recorded immediately afterwards.

[8] In Jun/2016, *Bradesco* acquired *HSBC*, see https://www.reuters.com/article/us-hsbc-bank-brasil-m-a-banco-bradesco-a-idUSKCN0YU2JV for more details.

[9] As close to 50% of loans in the national accounts correspond to earmarked in real state loans, our sample corresponds to nearly the universe of loans made to firms in Brazil.

banking conglomerate.

Due to the nature and level of detail of the loan level data, access to the SCR can only be made by BCB employees and other authorized parties. However, as we discuss below, all other datasets are publicly available.

Bank Branches and Local Balance Sheets. Beyond the credit registry, BCB maintains a publicly available dataset on banks at the municipality-month level denoted ESTBAN (*Estatística Bancária Mensal*). ESTBAN includes the number of branches and a balance sheet from each bank at the municipality-month level. From the balance sheet, we can recover credit (stock) for both firms and households, and then compute market share of each bank and concentration for each market in our sample (which we also recover from SCR). The credit variable we recover from ESTBAN does exclude real state, but not earmarked or subsidized loans.

Employer/Employee Data. We use the employer and employee data from RAIS (*Relação Anual de Informações*) collected by the Brazilian Ministry of Labor and Employment, which contains labor market data for the universe of formal firms and workers. [10] Since we are only interested in municipality level outcomes, we use the publicly available version of RAIS. [11] RAIS is publicly available in two forms: employee level and firm level (not identified over time). From the employee data, we use the month the employee was hired/fired, average monthly wages, establishment size and sector for this employee to construct monthly wage and employment series for each municipality. From firm level data, we compute the number of firms and employees per firm over time.

Real Outcomes. The Brazilian institute of Labor and Geography (IBGE) compiles municipality level output (measured through value added) and divided by agricultural, services and industry and construction sectors. Additionally, they compile population data for each

[10] Ulyssea (2018) shows that 40 percent of GDP, and 35 percent of employees are informal. This informality is either (i) firms not registered with tax authorities (extensive margin) or that have workers off the books (intensive margin). Firms that are not registered with tax authorities do not appear in either the credit registry or employer/employee data. Firms that potentially have workers off the books appear in both (if they borrow), but with unreported workers/salaries. For the purposes of this paper, the estimated results on employment and wages will be a combination of the direct effect on these variables and changes in firm/work formalization. In Section IV.4, we show that the competition effect on payroll is quantitatively close to the effect on Value Added (which includes formal and informal firms), which indicates that changes in formalization are not significant for this project.

[11] Linked employer-employee panel data exists and used in other studies, e.g. Fonseca and Van Doornik (2019)). In the public dataset, one cannot match employee-employer or follow them over time (they are unidentified), which is not a loss for the purposes of this project. By using the public version of RAIS, a significant part of our main results are reproducible by other researchers.

municipality. The average municipality in Brazil has 74,122 people and a GDP per capita of approximatelly US$ 6700. Contrary to the other datasets, the data on real variables are only available annually (not monthly).

M&A Episodes. Our measure from M&A episodes comes from the association of banks to banking conglomerates. From the Unicad dataset, which contains information of banking conglomerates for each registered financial institution in Brazil, we define an M&A episode as a situation where

1. A bank has changed conglomerates and has more than US$ 4.2 bn (10 bn Brazilian Reais in 2010) in assets

2. The original conglomerate of this bank exits the dataset.

We recover from this criteria a total of 12 episodes from 2002-2018, 9 of those in 2005-2015 (the sample we use of the credit registry), as shown in Table H.1 . The date of the episode is the date the bank changes conglomerates. We suppose that the bank conglomerate that changed its code is the target, while the one that kept their code is the acquirer. Our events are in fact large: the mean bank targeted (acquired) in an M&A episode in our sample has US$ 16 bn (US$ 84 bn) in credit at the time of the episode. A market is exposed to the episode if it has at least one branch of both banks when the episode happens.

The date a M&A episode appears in our sample, that is, where bank conglomerates identifiers change, is not necessarily the date the M&A had received approvals from all of the governing bodies (but for some episodes we it is). We consider the date of an M&A episode as the date after all approvals.[12] Take the largest merger in our sample as an example. In Oct/2008, Itaú and Unibanco announced their merger. At the time, Itaú and Unibanco were respectively the 3rd and 6th largest banks in Brazil, and together had over US$ 100 bn in assets. At the time, the new bank was among the top 20 largest banks in the world. In Unicad, their merge date appears as Oct/2008, even if the merger was only authorized by the BCB in Feb/2009 and by CADE in Aug/2010. We use Aug/2010 in this case. In Appendix C

[12] Within our sample period, the competition framework to analyze banks in Brazil was a legal grey area between the BCB and CADE (*Conselho Administrativo de Defesa Econômica*) - a government department responsible in evaluating competition aspects from all sectors. Currently, both the BCB and CADE must approve mergers, but the BCB can overrule any decision if it considers there is a threat of systemic risk to the banking system.

we also discuss an alternative measure of exposure to M&A, consistent with the one we use throughout the paper.

II.2. Banking Markets

We consider a *municipality* in Brazil to be our benchmark definition of a local banking market, as in Sanches, Silva Junior and Srisuma (2018), Coelho, De Mello and Rezende (2013). This definition is more precise than most of the literature in banking in the U.S., and thus requires explanation. In the banking literature in the U.S. (e.g., Black and Strahan (2002)), the standard definition of a banking market is a Metropolitan Statistical Area (MSA) or non-MSA county. However, as pointed out by Garmaise and Moskowitz (2006), and confirmed by Nguyen (2019), there is significant evidence that banking markets are highly localized for small and medium sized business (Garmaise and Moskowitz (2006) uses a $24km$ radius as the definition of a market). Empirically, Granja, Leuz and Rajan (2018) show that the the median distance between small firms and banks in the U.S. is close to 10km in 2016. Part of the rationale for the standard definition in the U.S. of a local banking market is data-availability, but our data allows us to compute market level outcomes at a much narrower sense. We show in Section IV that our results are robust to IBGE's microregions as our market definition, as used in Adão (2015) for labor markets.

As we want to focus on local competition, we want markets that have some exposure to private credit, but not those that are outliers in terms of bank competition (as the largest cities in the country). Our benchmark sample includes only municipalities with at least one and, for a few of our results, not more than 20 private banks in Dec/2005. This corresponds to approximately 40% of 5507 Brazilian municipalities. We show later on that our results are robust to this choice. The exclusion of municipalities with more than 20 private banking conglomerates excludes only 7 municipalities in Brazil.

For each month in each municipality, we compute the total loan volume on new loans and loan characteristics weighted by volume (such as maturity, spreads, collateral requirement etc.). Table 1 has the descriptive statistics in our dataset for the sample of municipalities with at least one private bank in Dec/2005. For SCR variables, we show the results weighted by population (given that this is how we will use them in our regressions), while market level characteristics (such as HHI) we presented unweighted results. The lending spread (lending rate minus national level deposit rate) in our sample are on average 36.5 p.p.. This is not surprising, given that Brazil has the world second highest spread at 32 p.p. in the World Bank's WDI dataset.

Table 1 shows that level of concentration of loans (and private loans only) in both SCR and ESTBAN. SCR uses firm location and new loans only, while ESTBAN uses bank location and balance sheet of banks. The measures of concentration are consistent across the two datasets and indicate that banking markets in Brazil are (i) very concentrated (HHI < .25) and (ii) heterogeneous in their degree of concentration (given the large standard deviation). Figure H.1 shows the histogram of HHI and HHI of private credit across municipalities for Dec/2010 in ESTBAN. A large share of markets with at least one private bank has exactly only one bank, while a minority is below the very concentrated threshold. Figure H.2 has the figure for data from SCR. To compute our concentration measures, ESTBAN uses bank location, while SCR uses firm location. Even with ESTBAN and SCR using different measures of lending (stocks versus flows), a different set of loans and ESTBAN uses bank location while SCR uses firm location, we have that the concentration measures from the two different datasets has approximately a .6 correlation. The same conclusion that local markets are concentrated is reached when analyzing the number of banks. The average number of banks for a municipality in Brazil is 3.84, while only 2.2 are private. [13]

III. Empirical Framework

This paper aims to estimate the effect of bank competition in market level financial and real outcomes. The key identification challenge is that bank competition is not exogenous to these outcomes. For instance, suppose a market receives a positive productivity shock. It is expected that this shock will increase total demand for lending and make the market more attractive to potential entrants, which changes the behavior of incumbents and competition. We overcome this identification challenge by using M&A activity of large banks as an exogenous source of variation in competition in local markets. For each M&A episode, which hap-

[13] We show in Figure H.3 the number of banks by municipality. Brazil's population is mostly distributed through the coasts (see Figure H.4), but even in densely populated areas the number of banking conglomerates is small. The average number of private banking conglomerates per 100,000 inhabitants is 5.49.

pens in different moments in time, markets will be exposed or not to the episode, allowing us to use both cross-sectional and time variation to identify the effect of bank competition.

Figure 2 illustrates the heterogeneous exposure across municipalities using the direct measure of mergers in our sample for the Itaú-Unibanco merger (the largest in our sample). As different regions of the country can have different characteristics and exposition to aggregate shocks, the variation we will in our estimates is a within region variation. The idea is in the Panel B of Figure 2. We show in the figure one particular region in Brazil, the northwest of the state of Parana, and the municipalities that are control and treatment in this sample. We compare the outcomes over time within region for the municipalities affected or not by the change in competition coming from the merger episode. For now, we consider that municipalities are isolated markets - we focus on the role of geographic spillovers in Section IV.

More specifically, we estimate the effect of bank competition on market outcomes using M&A episodes of large banks employing a DiD research design. We compare outcomes of treatment (markets exposed to the episode) markets with those in the control group (not exposed), before and after each merge. We say that a market is treated if it has at least one branch of both banks involved in the M&A episode at the moment of the episode, and a control market otherwise. The role of the control municipalities is to provide a counterfactual to what would happen in treated markets if the M&A episode had not occurred. The identifying assumption for internal validity of our estimate is that of parallel trends, that is: absent M&A's, treatment and control would have parallel outcomes (conditional on controls) over time. Although this assumption is not directly testable, we provide evidence of its validity by examining the outcomes of treatment and control markets before mergers.

Our identification assumption would be violated if buyer or target banks decision to merge is driven by factors that are specific to the markets where their activities intersect (or do not intersect). For instance, a national bank can acquire a local bank due to weak local economic conditions (where the local bank could be in the risk of failure).[14]To avoid this issue, we focus on merger of large banks only in our sample and control for time-region characteristics.

As competition varies at the market level and our objective is to understand the effects of

[14] Alternatively, the decision can come due to strong local economic conditions (i.e., the bank wants to reduce consolidate locally and extract rents from borrowers).

competition in aggregate outcomes, we focus our analysis on loan and firm data aggregated at the market level. Our baseline specification consists of a DiD of the form in Eq.(1),

$$y_{m,r,t} = \gamma_m + \gamma_{r,t} + X_{m,r}\beta_t + \sum_{\tau} \delta_\tau \mathcal{M}_{m,r,t-\tau} + \varepsilon_{m,r,t} \tag{1}$$

where $y_{m,t}$ is an output of interest of municipality m, in region r, at month t, γ_m and $\gamma_{r,t}$ are, respectively, municipality and time-region fixed effects, $X_{m,r}$ is a vector of control variables that is allowed to have a varying effect over time β_t, and $\mathcal{M}_{m,r,t-\tau}$ is a dummy variable that is equal to 1 if the municipality m is exposed to an M&A episode month $t-\tau$. In this project, we use τ ranging from -18 to $12, 24, 36$ and 48, that is, a year and a half before each M&A episode up to 4 years after, which covers more than half of the total sample in terms of time for each episode. Our main financial outcomes are loan spreads, total credit volume and average loan size, while our main real outcomes are employment and wages. We control for GDP and credit per capita in 2005, number of banks in 2005, and exposure to business cycles interacted with year dummies. [15] As we want to focus on the aggregate effects of bank competition, we weight our regressions of credit and real variables by population in 2005 (as Huber (2018)) and those of spreads by total stock of credit from public banks (as not to have an influence of spread levels on weights) in 2005 (from ESTBAN). Our results are robust to this choice, [16] and we also present the results of unweighted regressions.

To better understand the magnitude of the effects we measure, we also estimate a more restrictive version of Eq.(1) where we aggregate all of the effect of the M&A episodes in δ_{POST} in Eq. (2)

$$y_{m,r,t} = \gamma_m + \gamma_{r,t} + X_{m,r,t}\beta + \delta_0 T_{m,r,t} + \delta_{POST} T_{m,r,t} \times P_{m,r,t} + \varepsilon_{m,r,t} \tag{2}$$

where $T_{m,r,t} \equiv \sum_\tau \mathcal{M}_{m,r,t-\tau}$ represents if a municipality is a treatment or control at time t, and $T_{m,r,t} \times P_{m,r,t} \equiv \sum_{\tau>0} \mathcal{M}_{m,r,t-\tau}$ is the interaction of treatment with the post M&A period ($\tau > 0$). Moreover, there are several dimensions of heterogeneity that may be important in our application. For instance, one would expect that the effect of the merger would be larger less competition in the baseline, or if the merged banks had a higher market share. In our results

[15]The exposition to business cycles is computed as in Fonseca and Van Doornik (2019). It is given by the slope in a regression of local GDP growth as a function of a constant and national GDP growth from 2002-2018.

[16]Results weighted by number of firms, total credit, employment are available upon request.

section, we also extend Eq.(2) to include a triple interaction with municipality characteristics in the baseline.

Finally, we estimate a DiD instrumental variable (DiD-IV) specification to estimate the effect of competition on outcomes directly, as in in Eq.(3).

$$y_{m,r,t} = \gamma_m + \gamma_{r,t} + X_{m,r,t}\beta + X_{z,t}\beta + \delta^{IV}_{POST} Comp_{zrt} + \varepsilon_{zrt} \tag{3}$$
$$Comp_{m,r,t} = \gamma_m + \gamma_{r,t} + X_{m,r,t}\beta + \beta_0 T_{m,r,t} + \beta_{POST} T_{m,r,t} \times P_{m,r,t} + \omega_{m,r,t}$$

where we use exposure M&A episode as an instrument to changes in competition. We can interpret δ^{IV}_{POST} as an average causal response (ACR), which captures a proper weighted average of causal responses to a one unit change in competition (see Hudson, Hull and Liebersohn (2017)).

It is worth noting that our empirical setting, we can estimate the reduced form effect of bank competition, but not the effect of each individual channel. For instance, we can estimate the effect on interest rates and maturity, but not the effect of the changed maturity of contracts on interest rates. Identifying the causal effect of each channel would require one instrument per channel (Chodorow-Reich (2013)), and controlling for contract characteristics post merger in Eq.(1) would bias the coefficients.

IV. The Effects of Bank Competition

This section presents our reduced form evidence on the effect of bank competition using the data described in Section II and the methodology of Section III. First, we focus on financial variables. We show that a reduction in local competition increases lending spreads and reduces credit, consistent with the traditional IO view of competition. We provide evidence that this result is robust in several dimensions, such as market definition, types of loans included, municipalities included in the sample etc. Second, we show that our results are unlikely to be explained by alternative explanations. We do not see economically significant changes in the branches, firm age, loan maturity and loan and firm specific variables. We show that for municipalities with only one of the exposed banks, we do not observe any effect on quantities and a small, but significant, reduction in spreads. Third, we show that our results are consistent across sectors and that there is a significant effect on employment and output in most sectors, except for agriculture (that relies mostly on subsidized credit). Finally, we show that there are geographic spillovers between municipalities, but that the effects is small when compared to our benchmark estimates.

IV.1. Financial Outcomes

This section presents evidence of the effect of bank competition on financial variables. Figure 3 shows the concentration in a given market pre and post merger. It plots the δ_τ estimated from Eq.(1) with HHI of credit stock from private banks in municipality m, in region r at month t as the dependent variable. The bars show the 99 percent confidence intervals. Following an M&A episode, the concentration in a given market mechanically increases. Figure 3 is important, however, to show that (i) exposed municipalities were not consistently different that those non-exposed before the M&A episode and (ii) the M&A events we study are indeed large. The concentration measured with the HHI increases approximately .11 following a merge. Moreover, as shown in Table H.2, the number of banks after an M&A episode is 1.3 smaller in the treatment versus control municipalities. This indicates that we might be capturing consecutive mergers in a concentration wave that happened in Brazil from 2008-2011. To understand if this effect has any bearings on our main results, we will also use a subsample of municipalities exposed to at most one M&A episode in our sample for robustness. For our research design to yield causal estimates, we must have that the parallel trend assumption holds, that is, the path for credit between exposed and non-exposed municipalities would be parallel absent the change in competition. Before diving in our main results, we show the trajectory of the stock private credit in local markets using the stock of credit from ESTBAN data around the main merger in our sample (Itau-Unibanco). Figure 4 shows the results of estimating Eq.(4)

$$\ln\left(Credit_{m,t}^{Pr}\right) = \gamma_m + \gamma_t + \varepsilon_{m,t} \tag{4}$$

for subsamples of treatment and control municipalities in the Itau-Unibanco merger specifically. Figure 4 shows that credit follows an almost identical trajectory in exposed and non-exposed municipalities before the merger, that is, we find no evidence of pre-existing trends.

We see an initial effect on credit after the BCB approval and a larger one after the CADE (and final) approval. In our benchmark estimation, we use the second vertical line, i.e., the date after all approvals as the date of the M&A episode.

Figure 3 shows the mechanic effect of M&A on concentration, while Figure 4 is indicative that the parallel trends assumption is not violated in the main merger using local bank balance sheet data. Do we see the effect of local banking markets becoming more concentrated and less competitive in prices (lending spreads) and quantities (credit volume) in the exposed markets in our main data and all mergers? Figure 5 shows the reduced form relationship before and after an M&A episode for lending spreads and log of new credit (from private banks) using the SCR dataset. To not bias our results with changes of sample composition when estimating each individual δ_τ from Eq.(1), we use in Figure 5 the subsample of municipalities that we observe fully in an M&A window, that is: those that are not exposed to other episodes 36 months before or after the one analyzed and all of the window is within 2005-2015.

The left panel on Figure 5 shows a large and significant increase in lending spreads after the episode. Moreover, the right panel shows a large and persistent decline in credit volume from private banks in exposed municipalities relative to controls. Together, these results suggest that the main effect of a change in local competition in banking is as in the traditional IO view: less competition leads to higher prices and lower quantities. We show in Figures H.5 and H.6 the same for 24 and 48 month windows, respectively. For completeness, we also show the results using the date of changes in identifiers in bank ownership data (rather than the data of final approval) for a 36 month window in Figure H.7. We observe a quantitatively similar effect, but that happens only many months after the change in identifiers in bank ownership data (as approvals can take several months).

Table 2 shows the results of estimating Eq.(2) on spreads and credit from both private banks only and overall. Each coefficient from Table 2 comes from estimating a different version of Eq.(2) with the variables in the rows as the dependent variable. For each variable, we use data from 18 months before each M&A episode until the 12, 24, 36 and 48 months post the M&A episode for each column of Table 2. In Column 3 of Table 2, we observe that spreads from private loans have increased 5.88 p.p., while new loan generation has decreased 17.3% in exposed municipalities 3 years after the M&A episode. Given the baseline level of spreads of 35 p.p., this amounts to a 16.8 % increase. The results that include loans from private and public banks are qualitatively similar, but quantitatively smaller. This is consistent with the evidence in Coelho, De Mello and Rezende (2013) and Sanches, Silva Junior and Srisuma (2018) that public banks in Brazil are not directly competing with private ones.

In the U.S. literature, entry of new banks and increased loan supply by new banks leads to a new competitive equilibrium about 3 years after an M&A episode, such that the effect of the episode is short lived (see Berger et al. (1998), Garmaise and Moskowitz (2006) and Nguyen (2019)). Our results suggest that this is not the case in Brazil. We do not observe entry following an M&A episode of either public or private banks, and our effects on spreads and amount of credit are persistent over time. Brazil is an economy with a more concentrated, nationalized banking system, where entry costs in local markets tend to be higher, which is more representative of the banking sector in other countries.

We decompose the effect on total volume in number of loans and size of loans, which is an important distinction to understand the effects of bank competition. Table 3 shows the decomposition. Almost all of the effect we find comes from the extensive margin, that is, from changes in the number of loans. This is consistent with a banking model where banks and firms first choose the optimal level of credit based on contractual and information constraints and then split the surplus of the intermediation relation according to the bank market power, as in the limmited commitment model of Karaivanov and Townsend (2014). The result that loan competition affects the number of loans rather than the size of loans is consistent with the evidence Liebersohn (2018) for commercial real estate lending in the U.S. market.

As described in Section III, our benchmark results use weighted regressions to not give excessive weight to small markets. We now model this heterogeneity in terms of concentration and number of banks at the time of the M&A episode with a triple interaction, as in Eq.(5)

$$\begin{aligned} y_{m,r,t} = {} & \gamma_m + \gamma_{r,t} + \gamma_t \times Comp_{m,r,t_0} + X_{m,r}\beta_t + \delta_0 T_{m,r,t} + \delta_1 T_{m,r,t} \times Comp_{m,r,t_0} \\ & + \delta_{POST} T_{m,r,t} \times P_{m,r,t} + \delta_C T_{m,r,t} \times P_{m,r,t} \times Comp_{m,r,t_0} + \varepsilon_{m,r,t} \end{aligned} \tag{5}$$

where $Comp_{m,r,t_0}$ is the competition variable at the time of the M&A episode t_0. Compared to Eq.(2), Eq.(5) includes an interaction of the month-year fixed effects with the level of competition in the baseline, $\gamma_t \times Comp_{m,r,t_0}$, the interaction of the treatment status with competition, $T_{m,r,t} \times Comp_{m,r,t_0}$, and the triple interaction term. We are interested in both δ_{POST} and δ_C, i.e., the level of the effect and the heterogeneity with respect to bank competition in the baseline. We use three competition variables: number of private banking conglomerates N_B^{Pr} and concentration of private credit (stock) HHI^{Pr}. If the effect we are capturing the effect of bank competition on local markets, we should observe a smaller (in absolute value) effect for markets with more banks.

The results are in Table 4 for 36 months after the M&A episode. Column 1 of panels and A and B show the results without the interaction term running Eq.(1) without any weights. As expected, the results are much larger than in our benchmark estimates in Table 2, since now smaller markets, where the potential effect of an M&A episode is larger, have the same weights as larger ones. Column 2 has the M&A effect using the number of private banking conglomerates as a measure of competition. Our results show that the credit reduction is 7 p.p. smaller (Panel A), while spreads increase by approximately 2 p.p. less (panel B) for each extra bank in the baseline. The results in Table 4 are consistent with changes in competition in a textbook Cournot model, where each additional bank has a diminishing effect on equilibrium prices and quantities as more banks are present in a given market. Moreover, we expand Eq.(5) to include also the market share of merging banks in the baseline (and their initial levels and interactions). The idea is to test if a large or lower market share (a potential channel of relationship lending) has any differential effect in our results. As shown in columns 4 and 5, we do not find a significant result. [17]

Two-way fixed effects interpretation There is a recent literature on the estimation and interpretation of two-way fixed effects models (e.g., Goodman-Bacon (2018), Callaway and Sant'Anna (2019)) when a DiD estimation relies on differential treatment time across units. Goodman-Bacon (2018) shows that the coefficient δ_{POST} in Eq.(2) is a combination of all of the two by two differences (between treatment and control and pre and post periods), and that some of these uses early treated units as a control for later treated units. With dynamic treatment effects (as we observe in Figure 5), our estimates for δ_{POST} could be smaller (in absolute value) than the true treatment effect.

[17] Note that market shares will be smaller for larger markets automatically. The question in this setting is if market shares have any effect beyond those capture by the number of banks/local concentration, and that is why we only include baseline market share when controlling for the competition in the baseline.

We provide an alternative estimation method to verify that our results are not coming from a potentially erroneous interpretation and weighting in the two-way fixed effects model. Although in our benchmark estimation we are relying on differential treatment time across units to estimate our effects, our setting is different than that of Goodman-Bacon (2018). For each merger, we observe the municipalities that are and not affected by it - and thus we can use a control only of unaffected municipalities at each moment in time (relative to each merger). For that, we use the stacking method of Gormley and Matsa (2011) and Deshpande and Li (2019). For each merger c, we construct a sample of affected and unaffected municipalities. We then stack all of the datasets together and estimate Eq.(6)

$$y_{m,r,c,t} = \gamma_{m,c} + \gamma_{r,c,t} + \gamma_s + \delta_0 T_{m,r,c,t} + \delta_{POST} T_{m,r,c,t} \times P_{m,r,c,t} + \varepsilon_{m,r,c,t} \tag{6}$$

where $y_{m,r,c,t}$ is the outcome for municipality m, at region r, at time t, for merger c. We add the merger dimension to our previous set of municipality and region-time fixed effects, that is: we use $\gamma_{m,c}$ municipality-merger and $\gamma_{r,c,t}$ as region-calendar time-merger fixed effects. We add the distance to the merger fixed effects, γ_s that captures the dynamics of treatment and control pre-and-post merger with respect to the distance to it. The variable $T_{m,r,c,t}$ is one if the municipality m is treated in merge c at time t. Finally, the variable $T_{m,r,c,t} \times P_{m,r,c,t}$ is one if the municipality m is already treated in merge c at time t (i.e., the interaction of treatment with post periods).

The results are in Table 5 for a 36 month window. We conduct the stacked analysis using three different control and treatment samples. In Column 1 we use all municipalities, in Column 2 we use only municipalities never exposed to a merge as control (to avoid the 'bad' controls issue) and in Column 3 we drop the data of a treated municipality if it is treated again in a 36 month window after a merge to avoid double counting. All results are quantitatively consistent with those in our benchmark estimates in Table 2.

IV.2. Robustness

The results of this section are robust in various dimensions. First, as the minimum amount of a loan to be included in the SCR dataset changed over time from 5,000 BRL to 200 BRL, we consider subsamples of loans above 5,000 BRL or 1,000 BRL. The results are in Table

H.3 and are quantitatively close to our benchmark specification in Table 2, indicating that the changes in data collection are not responsible for our results. Due to these changes and branch expansion, more municipalities are observed in the data in recent years, which means our panel composition could be changing non-randomly over time and drive our results. In Table H.4 we show that our results are robust including municipalities that have at least 8 years of data from 2005-2015. Further, to guarantee that our results are not generated by composition effects of municipalities that are subject to multiple M&A's being systematically different, we show that our results are robust using municipalities exposed to at most one M&A episode in our sample in Table H.5.

Second, as to be exposed to a bank M&A a municipality must have branches of both banks, our exposed municipalities are generally larger and richer than our treatment municipalities. Even though we do not find any pre-trend in our event study analysis, a different shock could affect larger municipalities more severely (such as the 2008 crisis). To alleviate this concern, we re-do our analysis with municipalities that had between 2 and 6 private banks in the beginning of our sample. We show in Table H.6 that within this sample, treatment and control municipalities have similar characteristics. In particular, the difference in GDP per capita between treatment and control is less than 5%, while the difference in the number of private banks is approximately 3%. Table H.7 shows that our benchmark results in Table 2 are robust within this sample.

Third, as we focus on new credit in each month-year t at each municipality, we could be over-weighting short term loans in our sample. For instance, if a firm has a long-term credit once and keeps getting new working capital loans, the working capital loans will reappear in our sample while the long-term credit appears only once. To adjust for this, we compute our results for spreads using a volume times maturity weight for each loan (instead of only volume). With this new weighting, the spreads in our sample have an average of 22 p.p.. As can be seen in Table H.8, the results are consistent in terms of % of spreads that increase after the M&A episode.

Fourth, our result is robust to which municipalities we include in the sample and the market definition we use. To estimate the effect of competition in local markets, we exclude municipalities with more than 20 private banking conglomerates in our benchmark specification. In Table H.9 we show that our results are robust to the inclusion of these municipalities in our sample. As our benchmark regressions are weighted and municipalities with more than 20 private banking conglomerates are relatively large, the results are quantitatively smaller. This is expected given our results of Table 4 where we show that the competition effect is smaller for markets with more participants in the baseline. Moreover, our result is robust to the market definition. The median municipality in our sample is still small compared to an average US county.[18] We use then as a robustness microregions as local banking markets, which are a much broader definition of a market, which is the microregion concept used in the Brazilian Census (which is the labor market definition in Adão (2015)). The results are in Table H.10. We still observe significant coefficients, but quantitatively smaller than those in Table 2, which is consistent with the fact that banking markets are local. We explore in detail the role of geographical spillovers in Section IV.5.

Finally, we show that our results are also present using only publicly available data. In Appendix E we provide a case study of the largest merger in our sample - the merge between Itaú and Unibanco. At the moment the merger, Itaú was the second largest private bank in Brazil, while Unibanco was the third. As our results so far rely on administrative data, we do a case study for this merger to show are results are replicable with publicly available data and to we simplify the analysis of the most important event in our sample. As can be seen in Appendix E, we also find in the case study a decrease in total credit in treated municipalities, and this decrease is larger for less competitive municipalities at the moment of the merger.

IV.3. Alternative Explanations and Relationship Lending

In this section we explore and rule out alternative explanations to our findings. First, we focus on changes in bank structure by comparing markets with only one of the exposed banks versus those with both. Second, we compare loan and firm characteristics, as well as the number of branches between exposed and non-exposed markets. Third, we estimate the effect of the episode on defaults to test if lending risk explains our results.

After an M&A episode, banks involved can change lending and pricing policies to restructure their operations. Therefore, our results from the previous section could hypothetically be a consequence of changes in bank structure and not competition. To test for this

[18]The median municipality in our sample has 25,388 people (see Table 1), while US counties have on average 100,000.

hypothesis, we run the same regression as in Eq.(2), but only comparing markets that had only one of the merging banks versus those with none of the merging banks, i.e., those that are affected by the M&A episode but do not see changes in competition. An exposed market in this case is a municipality m in region r that had one - and not both - of the banks involved in an M&A episode, while a non-exposed market is one that had neither of the merging banks.

Our results for markets with only of the banks involved in a M&A episode and those with none are in Table 6. As we will discuss in more detail in Section IV.5, there are potential geographic spillovers between municipalities, and in columns 2-4 we include a control for M&A episodes in the same microregion. In Columns 1 and 2 of Table 6 we see no significant effect on quantity of credit 3 years after an M&A episode, which suggests that our benchmark result on credit quantity is unlikely to be a result of bank re-structuring or different policies between merging banks. In Columns 3 and 4 we observe that there is a negative and significant effect on spreads. This effect can be a consequence of efficiency gains following an M&A (e.g., Sapienza (2002)), although in other settings efficiency gains also lead to increases in lending volume (Stiroh and Strahan (2003), Mian, Sufi and Verner (2019)). Alternatively, it could be the case that banks compete more for loans in now relatively more competitive markets, but our source of variation in competition does not allow to distinguish between these channels. Overall, these results imply that our benchmark results could be underestimating the true effect of bank competition on interest rates. As we argue later on, this does not affect our estimates of the effect of lending rates and spreads on quantity and labor market outcomes.

Another potential explanation for our results is that banks involved in M&A episodes close or re-structure their branches. Nguyen (2019) argues that branch closures destroy lending relationships and thus can hypothetically reduce the volume of loans and increase spreads, mainly in terms of small business lending. We show in Table 7 that there is no effect on branches from private banks per 100,000 inhabitants in affected markets, while we observe a small, but non-persistent increase from branches of public banks. For reference, In our sample, the average number of private and total branches per 100,000 inhabitants is, respectively, 8.58 and 14.41 (Table 1). Therefore, the effects estimated in Table 7 are not only statistically, but also economically insignificant.

Table 7 also explores other dimensions of relationship lending. First, we show that as expected, loans have a maturity 10 days larger with the reduced competition, as in models of relationship based lending. However, this result is unlikely to be driving our main finding. First, the average maturity of loans in our sample is 250 days, which indicates that there is a less than 4% increase in maturity 3 years after the episode. Second, one would expect less competition to allow for banks to form deeper bonds and thus increase maturity, but reduce spreads and increase access for loans, when we find exactly the opposite. Importantly, we show that the share of relationship loans monotonically decreases over time in exposed municipalities, that is, less competition is not helping banks and firms to form lending relationships even after 4 years after the M&A episode.

We also show that there is no economically significant change in the age of firms taking loans in exposed municipalities or in the share of loans to small firms, which could be behind our results if firms form exposed cities were significantly younger/older (as shown in Zarutskie (2006)). Firms in exposed municipalities 4 months and a half younger (Table 7), which is not economically meaningful compared to a sample average of 14 years. Finally, one would expect small firms to be more dependent on bank relationships (as in Nguyen (2019)). We investigate this by computing the share of loans made to small firms and the relative spread of loans to small and large firms. The relative spread is given by the log of the ratio of spreads of loans to small firms to all loans. We show that there is no significant change in the share of loans to small firms, and that the effect of the relative spread of loans to small firms is not persistent. Taken together, the results in Table 7 suggest that branch closures, loan maturity and relationship lending in general are not behind our findings.

The last alternative channel we investigate is defaults. In our sample, we have that 2% of credit is on default one year after the loan is granted. We estimate Eq.(1) with the share of loans and total volume under default. We find in Table 8 that there is a increase in volume defaults, but that this is focused on markets with less than 20 private banks in 2005. This result is not significant in terms of the number of defaults. This effect on defaults is not surprising, given that loan volume is decreasing and interet rates increasing. We show in Appendix D that even when taking into account this increase, at most 15-20% of the change in spreads can be attributed to defaults in our sample.

IV.4. Sector Heterogeneity and Real Outcomes

Having established our main results in terms of spreads and lending volume, we show that our results are robust across sectors and have implications for real variables, such as employment, wages and output. In the traditional IO view that less competition in a market reduces quantities and increases prices, one can interpret a change in competition as a credit supply shock. As highlighted in Mian, Sufi and Verner (2019) and others, these can affect firms and the real economy through two channels. First, as lending becomes more expensive and scarce for firms, firms reduce hiring and investment. Second, bank competition is also likely to affect households, which implies that the local demand for goods and services also decreases with less competition in the banking sector. Mian, Sufi and Verner (2019) finds strong evidence that the latter is likely to explain the dynamics of employment and output following the 1980's geographic bank deregulation in the US.

To test if the household channel is driving our results, we follow Mian, Sufi and Verner (2019) and others and separate our firms (and loans) in each municipality in four sectors: tradable, non-tradable, construction and agriculture. In the presence of the household channel, firms in the non-tradable sector of exposed municipalities face two shocks: (i) credit is more expensive *and* (ii) the demand for their product is also lower when compared to firms in the tradable sector. If households are driving the results, we would expect larger effects in the non-tradable sector in terms of credit volume, spreads and employment. We estimate Eq.(1) for each sector separately to show our effect is not mainly caused by household demand, since we find strong effects in lending, spreads, employment and output for firms in the tradable sector.

We extend Eq.(2) with sector specific controls, that is, we estimate Eq.(7)

$$y^{s}_{m,r,t} = \gamma_m + \gamma_{r,t} + X_{m,r}\beta_t + X^{s}_{m,r,t}\beta_s + \delta_0 T_{m,r,t} + \delta_{POST} T_{m,r,t} \times P_{m,r,t} + \varepsilon^{s}_{m,r,t} \tag{7}$$

by sector s, on municipality m at time t, where we add to our benchmark controls the sector specific controls $X^{s}_{m,r,t}$, namely the $Size^{s}_{m,r,t}$ of the average firm size on sector s in market m and $AA^{s}_{m,r,t}$, $A^{s}_{m,r,t}$ and $B^{s}_{m,r,t}$ are the share of credit in a sector s of market m with rating AA, A and B, respectively. We also take this equation to the data with two differences compared to our benchmark estimation. First, as not all municipalities have firms from all sectors receiving new loans monthly, our sample is reduced for this estimation. Second, as different sectors have different external finance needs with different maturity structures and spread levels, we exclude the first 12 months post M&A in this estimation and use log of spread.[19]

The results on lending and spreads by sector 3 years after the M&A episode are in Table 9. For comparison purposes, in Column 1 of Table 9 we re-estimate our effect within the subsample of markets within these samples and with these changes in window and depend variable. As can be seen comparing the columns in Table 9, the effect on spreads is consistent across sectors. The effect of new credit from private banks in the tradable sector of 17.42% is larger than considering all firms (column 2), while the effect on new credit for non-tradable firms is much larger at 34.17% (column 3).[20]

Initially, one might be inclined to conclude that the difference between tradables and non-tradables is the household demand. Table 10 displays the results of estimating Eq.(7) with employment and average wage in a given sector as dependent variables. We can see comparing columns 2 and 3 that the effect on tradable employment and wages is larger than that of non-tradables. [21] We observe in Column 2 of panel A in Table 10 a 6.14% effect on employment in the tradable sector, compared with an effect of 5.34% in the non-tradable. This result is robust if we include municipalities with more than 20 private banks in 2005, as can be seen in panel B. Importantly, we find a positive, but statistically insignificant, effect on employment in the agricultural sector, which is expected in the Brazilian economy given the large share of agricultural credit that is subsidized (approximately 75%).

There are a few explanations to why we observe larger effects on tradable and construction sector employment coupled with an equivalent effect of competition changes on spreads and smaller effects on credit compared to the non-tradable sector. First, bank capital is more relevant to the tradable sector. In our sample, the share of bank capital to output in tradable

[19]In our sample, loans to firms from the tradable sector, for instance, have an average maturity of 188 days, with a standard deviation of 268 days across municipalities, while loans to firms of the non-tradable sector have an average maturity of 222 days with a standard deviation of 177 days across municipalities.

[20]As in Mian and Sufi (2014), more than half of our firms are not classified in any sector, and thus the effect by sector does not need to be within the sector specific effects.

[21] As explained in detail in Appendix C, employment is computed monthly by considering the stock of workers employed in a given month given that we observe which workers were hired or fired in a given month. We do not observe monthly wages, but we do observe average wages for workers hired in a given month-year. This implies that our wage measure may suffer from measurement error and thus may attenuate our results.

and construction is 48% larger in our sample than for the non-tradable sector. Second, it is possible that labor and capital or bank capital and other sources of financing are more substitutable in the non-tradable sector. [22] Albeit an interesting question per se, understanding this heterogeneity is outside the scope of this project.

To interpret our findings and compute the average causal response of real variables to competition, we can estimate the effect of competition on real variables using the DiD-IV estimation of Eq.(3). Ultimately, as we are interested in how competition shapes prices in the banking sector, we use lending spreads as the competition measure. Table 11 shows that the effect of lending spreads on real variables. We find for a 1% increase in spread causes a -0.3% reduction in non-agricultural employment in our benchmark sample and -.22% in the sample with all municipalities.

We confirm our findings on employment and wages by estimating the effect on sectorial and total output. As we observe output annually, we assume that a given market is exposed in year t if it is exposed for more than 6 months in that year to the M&A episode. Table H.11 shows that, consistent with our employment results, we find no effect on agricultural output (column 1), a negative effect of 7.7% on Industry and Construction (column 2) and a negative effect of 1.58% on the services sector (column 3), and a negative effect of 2.17% on total local GDP (column 4).

Finally, we also estimate the effects on those municipalities that had only one of the merging banks (but not both) compared to those that had none to check if potential efficiency gains in the banking sector are passed through to the corporate sector. We show the results in Table H.12. Overall, we find negligible results in terms of employment and output in municipalities that had only one of the merging banks compared to those with none. Given we do not see an effect of credit in these municipalities with only one of the merging banks (Table 6), this result is not surprising.

[22] For instance, With a CES production function with elasticity of substitution σ, the relative response of bank capital k_b and labor l to a change in the lending rate r^l and wages w would be given by $d\ln\left(\frac{k_b}{l}\right) = -\sigma d\ln\left(\frac{r^l}{w}\right)$, that is, for the same relative change in prices, the relative change in bank capital and labor vary due to the elasticity σ.

IV.5. Geographic Spillovers

When focusing on local markets, the IO literature generally focuses on small, isolated markets (Bresnahan and Reiss, 1991). We do not make this restriction on this paper, since we want to understand the aggregate effect of bank competition. A natural question is, thus, what is the geographic spillover effects of an M&A episode between large banks.

For concreteness, consider the northeast region of the state of Parana (Figure 2) we discussed in our empirical framework of Section III. This region is an example what is defined as a mesoregion in the Brazilian Census. Brazil has over 5,500 municipalities, aggregated across 558 microregions, which are themselves aggregated in 137 mesoregions. In our benchmark estimation, we use a municipality as a market and focus on the within mesoregion variation (by controlling for time-mesoregion fixed effects). The microregions and municipalities of the mesoregion of the northeast state of Parana are in Figure 6.

We compute the spillover effects of exposition to M&A events by comparing the outcome of municipalities that are not directly exposed to the mergers, but that are in either microregions or mesoregions that are themselves exposed. For instance, for the municipalities in Figure 6, we would compare the outcomes of municipalities in the two microregions affected by the merger with those of the microregion not affected to estimate the micro-region level spillover. To estimate this effect, we estimate Eq.(2) with markets not exposed to a given merger, with the treatment intensity being given by the number of merger episodes in the micro/meso region of the municipality. More specifically, for the microregion spillovers, we estimate Eq.(8)

$$y_{m,r,t} = \gamma_m + \gamma_{r,t} + X_{m,r,t}\beta + \sum_n \delta_n \mathbb{1}\{D_{m,r,t} = n\} + \delta_{POST} T_{m,r,t} \times P_{m,r,t} + \varepsilon_{m,r,t} \tag{8}$$

where $D_{m,r,t} \equiv \sum_{m \in M_r} \sum_\tau \mathcal{M}_{m,r,t-\tau}$ represents the number of mergers municipality m, in a microregion M_r in mesoregion r is in the window analysis at time t, and the term $\sum_n \delta_n \mathbb{1}\{D_{m,r,t} = n\}$ is a dummy for each potential number n of mergers in a region at a time t . The idea is to allow a different level of the outcome before the merge and capture the merge effect (and not a sample composition issue). The variable $T_{m,r,t} \times P_{m,r,t} \equiv \sum_{m \in M_r} \sum_{\tau > 0} \mathcal{M}_{m,r,t-\tau}$ is the interaction of the number of treated municipalities in microregion M_r, in mesoregion r, with the post

M&A period ($\tau > 0$), such that δ_{POST} is the spillover effect of one M&A episode in the same region.

We observe in Table 12 significant spillovers from M&A episodes in terms of financial variables. In Columns 2 and 4 of Table 12 we observe a decrease of 1.86% in new credit with an M&A episode in the same mesoregion and 5.45% in the same microregion for municipalities not directly exposed to the episode. We observe similar results for lending spreads. As we use mesoregion-time fixed effects in our benchmark estimates, the results in Table 12 suggest that our estimates could be underestimating the true effect of competition (since non-exposed municipalities also observe similar qualitative effects), although this spillover effect is small when compared to our benchmark estimates.

As highlighted in Adao, Arkolakis and Esposito (2019), there is a potential role of spatial linkages between markets that determine the aggregate effect of large shocks to the economy. Although a DiD design in this case is appropriate to capture differential effect with respect to shock, it misses the aggregate effect due to the indirect effects across markets. We test if there is a role for spatial linkages in real variables between markets by running the regression in Eq.(8) for real outcomes. In a gravity equation model of spatial linkages, such as Monte, Redding and Rossi-Hansberg (2018), we should observe effects on close municipalities in terms of employment if there are spatial linkages. We also estimate Eq.(8) without region-time fixed effects as they could be endogenous if the spatial linkages are homogeneous within each region.

Table 13 displays the results of estimating Eq.(8) for wages and employment by sector. We find no strong evidence of spatial linkages in real variables as the small drop in employment and wages observed in consistent with the credit reduction we observe in these municipalities. This result is not surprising: the average merge in our sample affects directly only 3% of municipalities and 15% of the population, which is a much smaller shock in magnitude than the China shock analyzed in Adao, Arkolakis and Esposito (2019). Importantly, Adao, Arkolakis and Esposito (2019) show that in their framework, absent spatial linkages (i.e., if markets are in fact segmented), the differential response of local aggregate outcomes captured by the DiD framework determines the aggregate effects in general equilibrium. We take this approach in our model in the next section.

V. An Economy with Bank Concentration and Financial Frictions

In Section IV we show the reduced form effects of bank competition. Our objective now is to impose more structure to understand if the coefficients found can be quantitatively understood through the lens of a model of bank competition and conduct counterfactuals. As our data, our model has several markets, each with a different level of bank competition and productivity level. For simplicity, we assume that markets do not interact or trade with each other.

Each market has two main economic actors: firms and banks. Firms are heterogeneous in their wealth and cost of capital. Firms choose labor and capital to be used in production subject to a financial constraint that capital in production is limited by wealth (as in Moll (2014) and others). Banks compete with each other by choosing the quantity of credit in a given market (Cournot competition). The total quantity of credit offered by all banks together will imply an interest rate through the demand for credit. The demand for credit comes from aggregating the solution to each firm's individual optimization problem. The demand for credit is downward sloping due to more firms deciding to not produce (extensive margin) instead of taking smaller loans (intensive margin), which is consistent with the data. To close the model, we have workers that supply labor for firms. Our model is static, but we show that there is potential for the amplification for the mechanisms we present here dynamically through increased profits and savings in the corporate sector, in line with the intertemporal distortion from financial frictions in Itskhoki and Moll (2019). As described in Itskhoki and Moll (2019), due the presence of financial frictions, the return to capital in the corporate sector is larger than the interest rate in the economy and policies that increase savings in the corporate sector are optimal due to an increase capital accumulation and output.

Given our functional form assumptions, the demand for credit in each market has a Constant Semi-Elasticity (CSE) with respect to interest rates. We estimate this semi-elasticity to be -3.17: for each 1 p.p. increase in lending rates, credit volume falls by 3.17%. This semi-elasticity is the key parameter in our model. Individual optimization from each bank in each market implies that spreads are given by concentration over the (absolute) semi-elasticity, which is a testable implication of the model. We test this implication for the CSE demand and find that we fail to reject the null implied by the CSE.

Given model success in capturing the relation between lending rates and credit, we take it one step further to evaluate the model implied effect of lending rates on real outcomes. We show that in partial equilibrium, the firm level production function can be aggregated to market level production function and that the semi-elasticity is a sufficient statistic of the effect of competition on output in partial equilibrium. Through a calibrated version of the model, we show that the response of the model is consistent with that observed in the data for real variables.

We evaluate three counterfactuals within this model in partial and general equilibrium (allowing local wages to adjust). First, we evaluate what would happen if spreads in all markets fell down to a world average of 5.43 p.p.. Second, we evaluate the effect of introducing one extra bank in each market. Third, we evaluate what would be the effect of public banks competing with private banks in markets they are already present. In our calibration, where 14% of capital depends on competition across banks, changing Brazilian spreads to those at the world level would be equivalent to 4.83% increase in output and corporate profits over output would increase 6.51 percentage points, facilitating capital accumulation.

Finally, although there is limited evidence in our sample of increased efficiency, we use our model to discuss potential shocks to the banking system that reduce competition but potentially increase efficiency of a few or all banks (such as M&A episodes). We show that the local concentration channel is quantitatively large: cost reductions of the entire banking system of approximately 30% would be required to undo it for municipalities affected by both the efficiency gains and the reduction in local concentration.

V.1. Setting

Our economy is static and has $m = 1, ..., M$ municipalities. Firms are heterogeneous in their wealth, a. Firms combine labor l and capital k to generate output as in the constant returns to scale (CRS) production function in Eq.(9)

$$y(k,l) = (z_m k)^{\alpha} l^{1-\alpha} \tag{9}$$

where z_m is a general productivity factors of municipality m and $\alpha \in (0,1)$. Following the literature, we assume firms face a financial friction of [23]

$$k \leq \lambda a$$

with $\lambda > 1$. Larger values of λ mean that firms can use more external capital in production by leveraging their own wealth.

Firms have two sources of external funding: (i) free credit from banks (where interest rates are not regulated, as in our empirical sample) and (ii) other sources of external finance (subsidized credit, corporate bonds etc.). Firms can borrow λ_b from banks and λ_o from other sources of external finance, such that

$$\lambda_b + \lambda_o + 1 = \lambda$$

Firms in our economy however do not have to borrow from external sources, and can choose their own capital structure. Let r be the average deposit rate across banks in the economy, $\overline{r}$ be the rate of non-bank external financing and r^l_m the cost of bank capital. Let λ^+_b be a variable that is λ_b if the firm uses bank capital and zero otherwise (and, analogously, λ^+_o for external funding). Given a capital structure, we assume firm i faces a cost of capital

$$r^{cc}_{i,m} = r^{cc}_m - \xi_i \tag{10}$$

where ξ_i is firm level idiosyncratic cost of capital shock, i.i.d. across firms, and r^{cc}_m is given by Eq.(11)

$$r^{cc}_m = \frac{\lambda^+_b}{1+\lambda^+_b+\lambda^+_o} r^l_m + \frac{\lambda^+_o}{1+\lambda^+_b+\lambda^+_o}\overline{r} + \frac{1}{1+\lambda^+_b+\lambda^+_o} r \tag{11}$$

where r^l_m is the lending rate from banks in market m and $\overline{r}$ is the borrowing rate for other sources of external finance, which we assume is the same across all markets. We introduce the shock ξ_i to allow for heterogeneity in firm capital structure choices and to be consistent

[23]The important feature of the financial friction is that it is linear in wealth a. This constraint can be micro-founded from a limited commitment problem where λ is the inverse of probability of collateral recovery. For a more detailed discussion of the micro-foundations possible generalizations see Moll (2014). What this constraint does not encompass are dynamic incentive contracts or endogenously incomplete markets with optimal contracts (as in Joaquim, Townsend and Zhorin (2019)) . Moll (2014), Itskhoki and Moll (2019) and others.

with the variation we observe in cost of funding within municipalities. The microfoundation of this shock is not relevant for our results, and that similar results could be obtained in our model with firm level heterogeneity in productivity, fixed production costs (paid if the firm decides to produce), distance to banks (as in Joaquim, Townsend and Zhorin (2019)) etc.. Importantly, this cost is not paid to banks and does not change the bank maximization beyond the implied functional form for the demand.

The cost of capital in Eq.(11) is simply the volume weighted cost of capital considering the sources of funding each firm chooses to use. For now, we will assume r_m^{cc} is given and compute the optimal decisions of the firm. Later, we will come back to how firms optimally choose their capital structure. For a cost of capital $r_{i,m}^{cc}$, wages w_m, wealth a and a productivity z, the production profit of a firm that chooses to produce is given by Eq. (12)

$$\pi(r_{i,m}^{cc}, w_m \mid a, z_m) \equiv \max_{k \leq \lambda a,\, l} \; (z_m k)^{\alpha} l^{1-\alpha} - r_{i,m}^{cc} k - w_m l \tag{12}$$

The CRS production function implies that each firm will either choose to not produce (and use zero capital and labor) or be exactly at the constraint. The solution to the problem of each manager in terms of input choices is given by (See Appendix F.1):

$$k(r_{i,m}^{cc}, w_m \mid a, z_m) = \lambda a \mathbb{1}\left[\hat{z}_m \geq r_{i,m}^{cc}\right]$$

$$l(r_m^{cc}, w_m | a, z_m) = \left[\frac{(1-\alpha)}{w_m}\right]^{1/\alpha} z_m k(r_{i,m}^{cc}, w_m | a, z_m)$$

where

$$\kappa(w_m) \equiv \alpha \left[\frac{(1-\alpha)}{w_m}\right]^{\frac{1-\alpha}{\alpha}} \text{ and } \hat{z}_m \equiv z\kappa(w_m)$$

External finance allows firms to use more capital - which implies that they will also more labor in production (in the same proportion). Using the optimal inputs, we can rewrite the profit for firm i in market m as Eq.(13) [24]

$$\pi(r^{cc}_{i,m}, w \mid a, z) = \lambda a \ \max\left\{\hat{z}_m - r^{cc}_{i,m}, \ 0\right\} \tag{13}$$

The firm returns for each extra unit of capital are constant and given by $\hat{z}_m$. Therefore, their capital structure choice is given by the relation of $\hat{z}_m$ and $r, \overline{r}$ and r^l_m. A firm uses a given type of funding to produce if $z_m \kappa(w_m)$ is larger than the cost of a given type of funding, as represented in Figure 1 for the case bank finance is the most expensive source of outside funding.

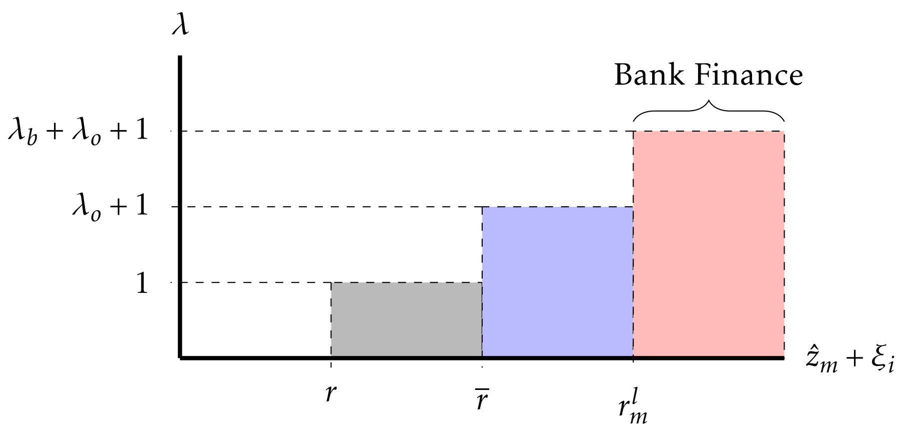

Figure 1: External Finance Choice for Firms

Note: Capital structure for firms assuming that bank finance is the most expensive source of financing and that firms borrow from other sources before using bank finance.

Each market m has B_m banks. As the firm problem is linear in a and we assume banks observe the level of wealth of firms, we can solve for for the bank equilibrium by considering the full demand for credit in a given market. We show in Appendix F.2 that under some convenient functional form assumptions, we can write the inverse demand function for credit Q_m as

$$r_m(Q_m) = \eta^{-1}\left[\gamma_m - \ln(Q_m)\right] \tag{14}$$

where γ_m is a term that depends on local productivity, wealth and model parameters, but not directly on any decision from banks (such as the lending rate).

[24] Note that Eq. (13) is linear in wealth a, which is what allows for an easy aggregation. This comes from the CRS assumption coupled with and a linear constraint $k \leq \lambda a$ in wealth.

Each bank b has a marginal cost in market m given by $c_{b,m}$ (monitoring and overhead costs, default provision etc.) and the deposit rate r, common to all banks and markets. Bank b chooses a quantity Q_b to offer in each market to maximize profits as in Eq.(15)

$$\max_{Q_b} \; [r_m(Q_m) - c_{b,m} - r] \, Q_b \tag{15}$$

where $Q_m \equiv \sum_b^{B_m} Q_b$ is the total quantity of credit in market m and $r_m(Q_m)$ is the inverse demand function from Eq.(14).

Finally, we assume in the general equilibrium version of our model that there is a representative worker per market m in our model that has a labor supply given by Eq. (16), which can be microfounded with GHH (Greenwood–Hercowitz–Huffman) preferences. We assume that banks do not take this adjustment into account when making choices, i.e., they do not anticipate that their changes in lending volume will affect local wages, which would affect how they optimally choose their credit policy in the first place.

$$l_m^s = w_m^{-\varphi} \tag{16}$$

As our model is static, the consumption level of the representative worker is given by $w_m \mathcal{L}_m$, where $\mathcal{L}_m$ is the equilibrium labor in market m.

V.2. Model Implications

We provide now two model implications that are testable in the data. First, we focus on partial equilibrium outcomes. We show that semi-elasticity of demand is a sufficient statistic for the effect of concentration on spreads and of spreads on payroll and output. Second, we discuss how to compute the same predictions in a general equilibrium setting numerically.

Let $\mu_{b,m}$ be the market share of bank b in market m. Let $s_{m,b} = r_b^l - r$ be the spreads of bank b with respect to the deposit rate. We focus here on market share weighted spread in market m, that is

$$s_m \equiv \sum_b \mu_{b,m} s_{m,b}$$

Lemma 1 shows how bank optimization (together with the Cournot structure) implies that spreads are given by a market specific constant and the ratio of concentration and the semi-elasticity. Even if market are very concentrated, the sensitivity of demand to prices can drive down equilibrium prices. The extreme case would be if there is only one bank in a market, but all firms can immediately switch to an online credit provider if it offers lower prices, thus driving margins to zero even with one unique bank in the market.

Given a semi-elasticity, Lemma 1 provides a testable prediction of the model (which we take to the data in the next subsection): changes in concentration that do not affect the relative cost structure of a market should have a $1/\eta$ effect on spreads.

Lemma 1. ***Bank Optimization.*** *In our Cournot competition, individual bank maximization of Eq.(15) implies that market share weighted spread s_m is given by*

$$s_m = c_m + \frac{HHI_m}{\eta} \tag{17}$$

where $HHI_m \equiv \sum_b \mu_{b,m}^2$ is the local HHI and $c_m \equiv \sum_b \mu_{b,m} c_{b,m}$ is the average marginal cost in market m

Proof. Appendix F.3. ■

As our final objective is to understand the effect of bank competition in macro aggregates, we must first understand how macro aggregates depend on the individual output of each firm. As each firm has a CRS production technology and faces a linear financial constraint, we can write aggregate output $\mathcal{Y}_m$ as in Eq.(18)

$$\mathcal{Y}_m = z_m^{\alpha} \mathcal{K}_m^{\alpha} \mathcal{L}_m^{1-\alpha} \tag{18}$$

where $\mathcal{K}_m$ and $\mathcal{L}_m$ are, respectively, aggregate capital and labor in market m (see Appendix F.1 for derivations).

More useful for our purposes, however, is the aggregation in Lemma 2. We show that output can be written as a function of wages, local wealth and interest rates in each type of loan. In a partial equilibrium model (were wages are fixed), the percentage change in output from changes in spread depends on the change in capital times the share of capital affected by this change. Percentage changes in capital in our model come from the semi-elasticity demand (Eq. 14), which implies the semi-elasticity of demand is a sufficient statistic for the effect of concentration on output in our model. Given our aggregate production function, we have that total payroll is equal to a share $1-\alpha$ of output, which implies the effect on output is the same as the effect on total payroll - and thus the consumption of the representative worker.

Lemma 2. ***Aggregation, Spreads and Real Outcomes.*** *We can decompose aggregate output as*

$$\ln(\mathcal{Y}_m) = \zeta_m(\hat{z}_m, w_m) + \theta_m(r_m^l) \tag{19}$$

where $\zeta_m(\hat{z}_m, w_m)$ depends on local wages and productivity, but not on interest rates, and

$$\theta_m(r_m^l) \equiv \ln\left(e^{-\eta r} + \lambda_o e^{-\eta \bar{r}} + \lambda_b e^{-\eta r_m^l}\right) \tag{20}$$

Thus, in partial equilibrium, the immediate response of output and consumption to spreads is given by Eq.(21)

$$d \ln \mathcal{Y}_m = d \ln \mathcal{C}_m = -\eta \omega d s_m \tag{21}$$

where ω is the share of capital in the economy provided competitively by banks and $\mathcal{C}_m$ is the consumption of the representative worker in market m.

Proof. Appendix F.4. ■

Lemma 1 and 2 show the effect of concentration on real variables in partial equilibrium. However, for large events (as the M&A episodes we are exploring) and some of the counterfactuals we simulate, there is potential for wages to adjust with changes in competition. In our calibration, the quantitative effect of wage adjustments in the prediction of Lemma 1 is small, but significant for Lemma 2. See Appendix G for details on GE computation and parameters.

Dynamics. So far we have focused exclusively on the static distortion from lack of competition in banking, which is through less credit due to market power. We could consider a different model where bank competition does not affect credit allocation, but only the cost of finance. In this case, bank competition would not have any effect in today's output, only on the share of output that is retained in the corporate sector versus the share that goes to the bank sector (i.e., how to share the production surplus). Even if this is case, market power in banking would still cause a dynamic distortion.

As shown in Itskhoki and Moll (2019), when the corporate sector is financially constrained, the rate of return for investments is larger than the cost of capital in the economy - and thus increasing capital in the corporate sector can be welfare enhancing compared to laissez-faire. Policies that increase the ability of the corporate sector to save (interest rate subsidies, wage suppression etc.) are thus optimal. In our setting, if lending is more expensive, the corporate sector profit is lower, and thus accumulates less capital over time.

Joaquim and Sandri (2019) makes the distinction between the static and dynamic distortions clearly. For the local level of bank competition (or any credit policy in general) to maximize current output, the average productivity in the economy where each manager's productivity level is weighted by their leverage. On the other hand, in the steady state, bank competition affects output by the static channel and though the reduction in the aggregate cost of finance (Lemma 3 in Joaquim and Sandri (2019)). Although we do not provide a full-blown dynamic model in this paper, we compute the implications of bank competition to corporate profits and thus to the capital accumulation channel in Itskhoki and Moll (2019) and Joaquim and Sandri (2019).

V.3. Semi-Elasticity of Demand for Bank Credit and Testable Implications

In this section we test the predictions of the model in Lemma 1 and 2 in the data. For that, we must first estimate the semi-elasticity of demand, which is a sufficient statistic for the partial equilibrium effect of concentration on spreads and output.

We estimate the semi-elasticity and elasticity of demand by using an empirical version of Eq. (14) given by Eq.(22), where we separate local wealth in fixed effects and an error with local productivity, which is correlated with spreads in that market.

$$\ln(Credit_{m,r,t}) = \gamma_m + \gamma_{r,t} + X_{m,r}\beta_t + \eta^r Spread_{m,r,t} + \varepsilon_{m,r,t} \tag{22}$$

Our coefficient of interest is η_r, the semi-elasticity of demand for credit. As spreads are potentially endogenous, we supplement Eq.(22) with the first stage equation Eq.(23), where we instrument spreads with exposure to the M&A episodes.

$$Spread_{m,r,t} = \gamma_m + \gamma_{r,t} + X_{m,r}\beta_t + \delta_0 T_{m,r,t} + \delta_{POST} T_{m,r,t} \times P_{m,r,t} + \vartheta_{m,r,t} \tag{23}$$

where $T_{m,r,t}$ and $P_{m,r,t}$ are treatment and post M&A episode, respectively, as explained in more detail in Section III. In this section, we focus on the results for windows of 18 months before and 36 months after the M&A episode, as we want a window that is large enough such that all effects are realized, but potentially not too large (as 48 months) to allow for other changes in market structure at each market. The results of estimating Eqs. (22)-(23) using a window of 36 months after the M&A episode are in Table 14. We estimate that for a change of interest rates of 1 p.p., the demand for credit falls 3.17%. The results are consistent considering only loans for private banks or both public and private banks. From now on, we use $\hat{\eta} = 3.17\%$.

Given an estimate for η, we can use the results in Lemma 1 and 2 to recover partial equilibrium implications of the model, or calibrate a full version of the model to recover the general equilibrium effects of a simulated M&A episode. We present both in this section, but as shown in Section IV.4, the effect on wages is less significant than the effect on employment, and the partial and general equilibrium predictions are close for an M&A episode. Apart from calibrations using aggregate data and other standard parameters in the literature, we calibrate the labor supply elasticity to match the relative response of wages to total payroll in the data. For details on parameters and numerical solution of the general equilibrium model, see Appendix G.

We estimate the effect of concentration on spreads as we estimate the semi-elasticity of demand, where spreads are the dependent and concentration is the right hand variable in the second stage (Eq.22). Column 1 of Table 15 displays the estimated effect of changes in concentration in changes in spreads. Columns 4 and 5 show the partial and general equilibrium prediction and the p-value of testing that the estimated effect is equal to the model predicted effect. As we can see in Columns 4 and 5, we fail to reject our model implication on the relationship of concentration and spreads in Lemma 1. This relationship is not mechanical: it appears as the result of our Cournot competition assumption and bank optimization (Lemma 1) given the demand structure.

We also test the model implications for the relation between lending spreads (or lending rates, given that our estimation includes time fixed effects) in terms of total payroll, that is, the prediction in Lemma 2. The results are in Columns 2 and 3 of Table 15. Columns 4 and 5 have the predicted effect on total payroll in partial and general equilibrium and the p-values of testing the model implications in parenthesis. The effect on partial equilibrium is given by the multiplication of the semi-elasticity (-3.17) and the share of capital that is competitively provided by banks, 14% (see Appendix G). The effect of general equilibrium uses a calibration based on aggregate data and the relative response of wages and employment - i.e., we do not calibrate the level of responses, which is exactly what we test on Table 15. See Appendix G for details on GE computation and parameters.

V.4. Counterfactuals

Given our success of the model to replicate key moments in the data in Section V.3, we now conduct three counterfactuals on local competition in banking markets. First, we solve the model with one extra bank in each market (that has a cost equivalent to the market average). Second, we solve the model considering what would happen if public banks were actively competing with private banks. Third, we solve the model assuming that spreads on all markets falls to the current world level of 5.43 p.p. The idea of the first and second counterfactuals is to understand how much could be gained with feasible policies that encourage local competition, either directly or through the use of public banks, while the third provides an international comparison of how much the lack of competition in the banking sector can be detrimental to welfare.

Before diving into aggregate responses, we focus on the mechanics of the counterfactuals in our model. Consider the first counterfactual: one extra bank in each market. The response for each market will depend on its concentration in the baseline. The competition return of the marginal bank is decreasing in the initial number of banks. For instance, in a model with N symmetric competitors, the concentration is $1/N$, and thus an entry shifts concentration from $1/N$ to $1/(N+1)$. For a market going from a monopoly to a duopoly, this implies that concentration changes from 1 to .5, while a market going from 4 to 5 banks concentration goes from .25 to .2. As in our model local concentration is what determines spreads (Lemma

1) and spreads affect output (Lemma 2), the effect of the marginal bank will be increasing (and convex) in initial concentration. We show this result in our model in Figure H.12. To recover aggregate responses, we aggregate individual market responses using population as weights.

Our results for each counterfactual are in Table 16. We show the effect of each on for outcomes: output, bank capital, wages and profits of the corporate sector as a share of output. In Panel A of Table 16 we show that one extra bank locally can increase competitive bank capital by 13.75% and has an effect of 1.32% in output. The effect on output is modest compared to the effect on competitive bank capital (9%) due to fact that approximately only 14% depends on bank capital in our calibration.

In Panel B of Table 16 we show the effect of increasing the competition level by public banks actively competing with private banks. As studied in Coelho, De Mello and Rezende (2013) and Sanches, Silva Junior and Srisuma (2018), public banks are not directly competing with private counterparts in Brazil. Our counterfactual focuses on changes in credit concentration that is competitively provided to the level of credit concentration overall (including earmarked loans and public banks).[25] We find that this feasible change in local concentration could increase output by almost 1%. Finally, we show that if spreads fell to 5.43 p.p. in all banking markets in Brazil, we would observe an increase of bank capital of almost 40%, which would translate to a 4.83% output gain (Panel C).

Finally, we also show that the dynamic savings channel of Itskhoki and Moll (2019) is relevant in all of our counterfactuals, although a quantitative dynamic statement is outside the scope of this project. For instance, one extra bank increases profits over output for the corporate sector in .96 p.p., which will then lead to more savings and investment in the future.

[25] Sanches, Silva Junior and Srisuma (2018) focuses on the effect of banking privatization, and what would happen with banking markets that would potentially have branch closures following it (as some markets with the presence of public banks are not profitable enough for private banks, as shown in Coelho, De Mello and Rezende (2013)). Our exercise in here is different in spirit, as we want to focus on the competitive role between private and public banks.

V.5. Efficiency Gains

Although we find limited efficiency gains in our sample by comparing municipalities with only one bank involved in the M&A with those with none, there is evidence that banks can increase their efficiency (e.g., Jayaratne and Strahan (1996) and Sapienza (2002)) following mergers in different settings. We provide now an extension of our model that can encompass changes in efficiency by merging banks and weight the efficiency channel of mergers versus the local competition channel of this paper. From Lemma 1, we can write the levels of spreads in each market according to

$$s_m = c_m + \frac{HHI_m}{\eta}$$

Spreads are composed by the market share weighted cost of providing loans, c_m and the concentration/semi-elasticity term. Therefore, if a shock affects costs and concentration together, we have that spreads in market m will change according to Eq.(24)

$$\Delta s_m = \Delta c_m + \frac{\Delta HHI_m}{\eta} \tag{24}$$

So far, we assumed that $\Delta c_m = 0$, i.e., that the cost of banks involved in mergers was not affected by the merger. When $\Delta c_m \neq 0$, we will have that the M&A will have two effects: the increase in concentration, that increases spreads, and the potential efficiency gains, that decreases spreads. Municipalities with both banks involved in the merger will be subject to both - and which effect is larger will determine the net result. Municipalities with one bank involved in merger are only subject to the efficiency gains, and thus spreads will decrease. The net aggregate effect of a merger will thus depend on the relative share of municipalities with both and only one of the banks involved in the merger. For instance, if the share of municipalities with both is larger, the local concentration increase channel will be larger.

To quantitatively assess the efficiency gains channel, we conduct one final counterfactual exercise. For simplicity, we assume that before the merger, all banks had a cost

$$c_{b,m} = \bar{c} + \bar{c}_m$$

that is, all banks in a given market had the same cost $\bar{c}$ across markets and there is a market specific cost $\bar{c}_m$. We assume that, among the many banks in our economy, there are two banks, A and B, that are merging, and their costs post merger will be given by $\hat{c}_{b,m} = \bar{c}(1-\chi)$, where $\chi \in [0,1]$ for $b = A, B$. The parameter χ represents the percentage reduction in costs: for $\chi = 1$, the costs of banks post merger would be zero. In this case, the market share of the merging banks will increase, and market share weighted costs will reduce by more than χ. We also consider a scenario where the cost of *all* banks in market m, not only A and B, is reduced by χ. The idea is to capture a situation where due to banks A and B efficiency gains, other banks look for avenues to increase their efficiency and thus the banking system as a whole becomes more efficient.

The term $c_{b,m}$ in our model captures the marginal cost of a loan apart from the funding cost of the bank, r. For the purposes of this exercise, we will consider that this cost is comprised off two terms: defaults and administrative costs of banks. According to the BCB, [26] these two terms are responsible for 65% of spreads - which we use in our calibration to estimate $\bar{c}$. A reduction in $c_{b,m}$ thus means that either the bank becomes more efficient in screening or recovering collateral, or simply that it reduces its administrative costs of reducing the loans. See Appendix G for details on calibration and computation of counterfactuals.

We present our results in Figure 7. We plot the percentage increase in aggregate output $\mathcal{Y}$ for municipalities with only one or both of the merging banks when only the merging banks increase their efficiency (Left Panel) or when the banking system as a whole increases their efficiency (Right Panel) by χ. We see in the left panel of Figure 7 that for those municipalities with both banks involved in the merger, even efficiency gains of 50% are not enough to compensate for the loss of concentration. The intuition is that the loss of concentration affects the market as a whole, while the efficiency gains of these two banks, even when they gain market share in equilibrium, is not enough to sufficiently move the cost of the market. In the right panel of Figure 7, when other banks follow suit and increase their efficiency by the same amount as the merging banks, we see that a cost of reduction of almost 30% would be needed to compensate for the increase in local concentration. Overall, even with potentially large efficiency gains, our model and estimates suggest that local competition is quantitatively relevant for spreads and aggregate output in given municipalities.

[26] See the BCB's Banking Report for 2018.

VI. Conclusion

This paper presents new evidence on the causal effect of bank competition in financial and real outcomes using a comprehensive dataset of Brazilian loans and firms. We use heterogeneous exposure to M&A episodes by large banks to identify the effect of competition in banking by comparing exposed versus non-exposed municipalities.

Two empirical results stand out. First, we show that a reduction in bank competition is responsible for a significant increase in lending spreads (the difference between lending rates and the deposit rate) and decrease in loan volume. This decrease in volume occurs fully through the extensive margin - i.e., less loans in equilibrium, and not smaller loans. We show that our results are robust in a subset of loans made to firms with or without a previous relation to the bank, and that there is no significant change in geographic presence of banks, which indicates that the relationship lending channel is not relevant in our setting. We also show that these results are likely due to competition, and not a different change by merging banks, as we do not find the same outcomes comparing markets with only one of the merging banks as those with zero. Second, we show that the effect on spreads and credit carries over to the real economy. We show that employment decreases substantially across sectors. Importantly, we do not observe a systematic difference in the agricultural sector, which has more than 75% of its credit provided through government programs and subsidies.

We propose a simple model of bank competition and show that the semi-elasticity for the demand for credit is a sufficient statistics for the effect of competition (and, in the model, concentration) on spreads and the effect of spreads on output. Using our instrument, we estimate this semi-elasticity to be around -3.17. We take the model implications to the data and fail to reject them, and provide various conterfactuals. If spreads fell to world levels in all markets, for instance, we would observe a 4.83% increase in output and 6.51 p.p. increase in profits of the corporate sector over GDP. Finally, we show that potential efficiency gains from M&A's (or alternative policies) would have to be extremely large to compensate for the reduction in local bank competition.

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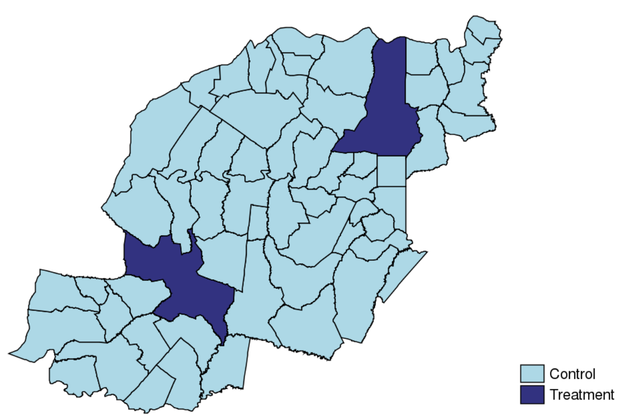
Appendix

A. Figures

Figure 2: Treatment and Control Municipalities in Itaú-Unibanco merger (Aug/2010)

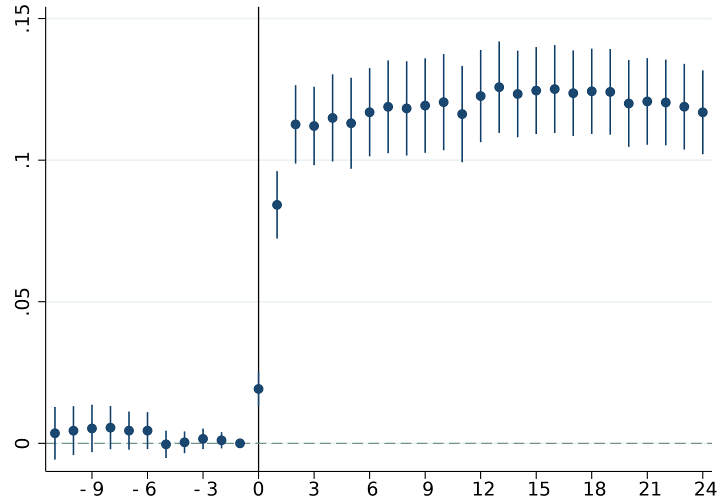
(a) All municipalities

(b) Northeast of Parana



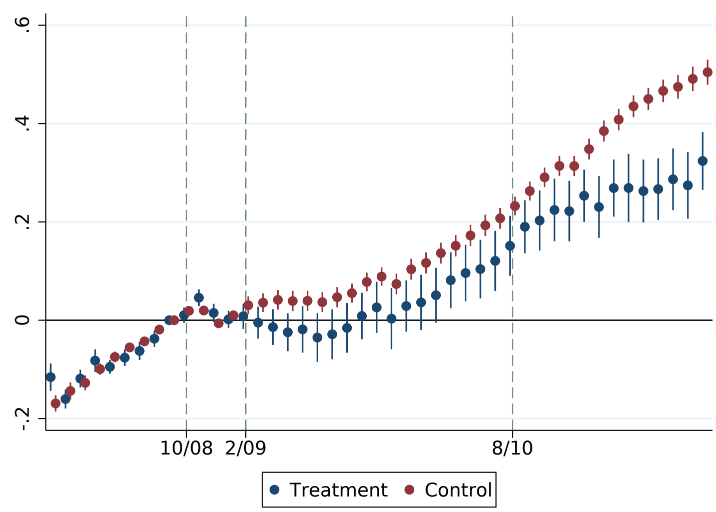
Note: Exposure to the largest M&A episode in our sample (Itaú-Unibanco), which received its final approval in Aug/2010. A municipality is considered exposed to the episode if it has at least one branch (from ESTBAN) of both banks at the moment of the episode. Panel A is for all municipalities in the country, while Panel B is for the municipalities is in the northeast region of the state of Parana. We use the within-region variation (as in Panel B) in our estimation.

Figure 3: Concentration of Private Credit Around M&A Episodes



Note: Coefficients δ_τ from Eq.(1), estimated at the monthly-municipality level. Regression outcomes is the HHI of private credit from bank-municipality balance sheets (from ESTBAN). Standard errors computed clustering by municipality (treatment unity). Bars show 99% confidence intervals. We normalize $\delta_{-1} = 0$. Exposed (Treatment) municipalities are those that had at least one branch of both banks at the time of the M&A episode. Vertical lines represent the time of the bank identifier change in ownership data.

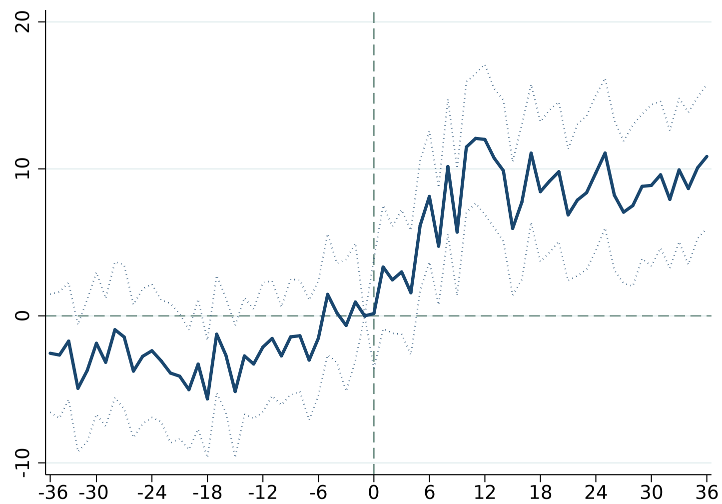
Figure 4: Stock of Private Credit Over Time: Itau-Unibanco Merger



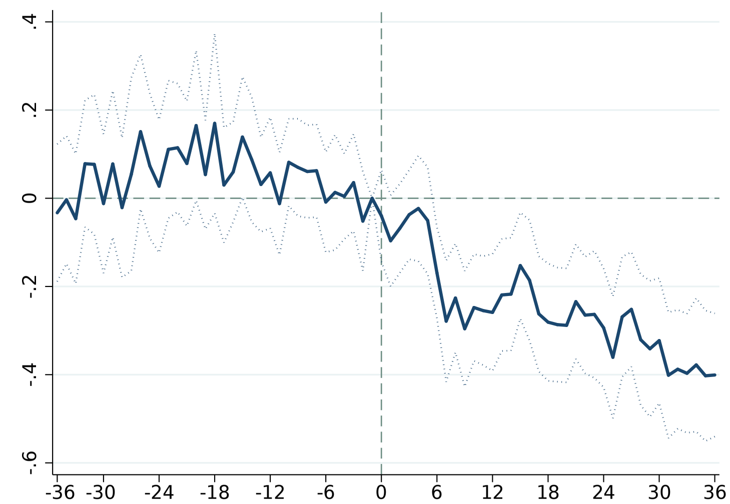
Note: Time dummies γ_t from Eq.(4) with log of total private credit as an outcome, estimated at the monthly-municipality level. Standard errors computed clustering by municipality (treatment unity). Bars show 90% confidence intervals. Exposed (Treatment) municipalities are those that had at least one branch of both Itaú and Unibanco at Oct/2008, when merger is announced. All of the data for the case study comes from the publicly available ESTBAN dataset (see Section II). The vertical lines represent the following dates in the merger: Oct/2008 is the date the merger is announced, Feb/09 is when the BCB approves, and Aug/2010 is the CADE approval (final approval).

Figure 5: Effect of M&A Episode on Lending Spreads and Credit Volume: 36 month window

(a) $Spread^{Pr}$

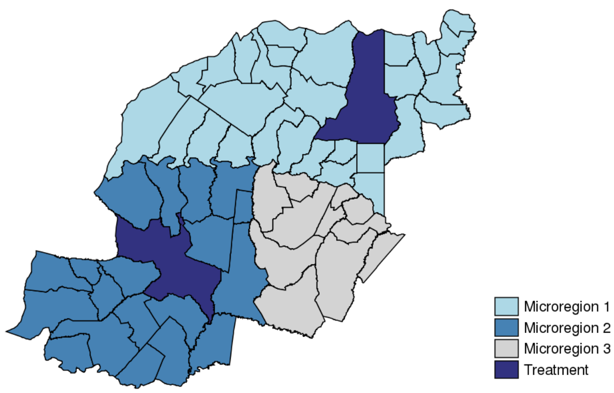


(b) $\ln\left(Credit^{Pr}\right)$



Note: Coefficients δ_τ from Eq.(1), estimated at the monthly-municipality level. Regression outcomes are the lending spreads (local interest rates minus country level deposit rate) on left panel, $Spread^{Pr}$, and log of new credit on right panel, $\ln\left(Credit^{Pr}\right)$, both computed only for loans made by private banks. For each municipality, interest rates are aggregated using loan size as weights and loan volume corresponds to the sum of value of all loans, computed through the SCR credit registry (see Section II) from 2005-2015. Standard errors computed clustering by municipality (treatment unity). Dotted lines represent 99% confidence intervals. We normalize $\delta_{-1} = 0$. Exposed (Treatment) municipalities are those that had at least one branch of both banks at the time of the M&A episode. Vertical lines represent the time of the final approval of the M&A episodes. Sample of municipalities is those that had at least one and not more than 20 private banks in Dec/2005 and all periods in the 36 month window for a given M&A. Credit regressions are weighted by population in 2005 and spreads are weighted by credit volume from public banks in ESTBAN in 2005.

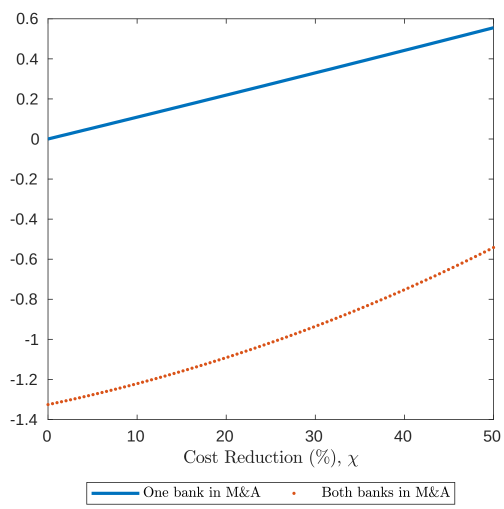
Figure 6: Microregions and Merge Exposure for the Northeast of Parana



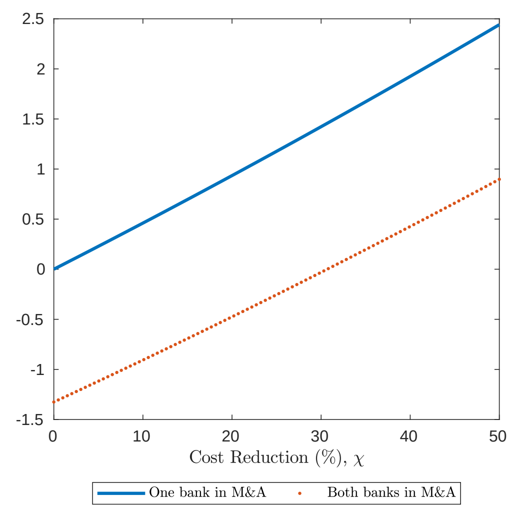
Note: Exposure to the largest M&A episode in our sample (Itaú-Unibanco), which received its final approval in Aug/2010, by microregion, in the mesoregion of the Northeast of Parana. A municipality is considered exposed to the episode if it has at least one branch (from ESTBAN) of both banks at the moment of the episode.

Figure 7: Percentage Change in Aggregate Output from an M&A: Local Competition and Efficiency Gains

(a) M&A Banks Efficiency Gains



(b) Banking System Efficiency Gains



Note: Percentage change in aggregate output (aggregated across banking markets) following an M&A with potential efficiency gains. The efficiency gains are measured by parameter χ, which presents the percentage reduction in administrative and default costs from banks. The effects are computed for municipalities that had only one of the merging banks (continuous blue line), and thus are only subject to the cost reduction effect on spreads, and for those that had both banks, and thus face both an increase in efficiency and local concentration (red dashed line). We plot the results in two scenarios: in the left panel is the case where only the merging banks become more efficient and in the right panel is the case where the banking system as a whole becomes more efficient. For details on the model, see Section V. For details on computation and details on the counterfactual, see Appendix G.

B. Tables

Table 1: Descriptive Statistics

	Mean	Med	S. D.
		SCR	
# Loans (1,000)	54.14	1.66	203.02
Loan Volume (US$ 1,000,000)	234.09	12.55	638.28
Loan Size	13104.18	10534.46	17886.14
Maturity (days)	250.19	201.32	230.93
Spread	35.86	33.74	19.75
Collateral	.46	.47	.22
Relationship Loans	.55	.56	.16
Loans to Small Firms	.13	.95	.13
Firm Age	15.81	15.23	6.58
HHI (Private)	.61	.54	.28
HHI	.52	.44	.27
		ESTBAN	
# Banks	3.84	3	3.29
# Private Banks	2.22	1	2.69
Branches (Private, per 100,000)	8.58	6.92	6.74
Branches (per 100,000)	14.41	12.6	8.75
HHI (Private)	.75	1	.29
HHI	.59	.51	.28
		RAIS	
# Firms	2492.13	759	14600.47
# Employees	18042.07	3488	124544.52
Wages (US$, monthly)	446.78	420.05	127.17
		IBGE	
Population	74122.2	25388	326312.01
GDP/Pop (US$ 1,000)	6.62	5.11	6.80

Note: SCR data from 2005-2015 and for all other datasets from 2002-2018. For each municipality, we aggregate all SCR variable using loan size as weights. Collateral is a dummy equal to one if a loan requires collateral. Relationship loans is the share of loans made to firms that had at least a 2 year relationship with a given bank (where a relationship starts at the firms loan). Loans to small firms is the share of total credit that goes to firms with less than $\$240,000$ Brazilian Reais in revenue. We show SCR statistics weighted my population, apart from market concentration. Loan volume refers to the sum of all loans made in a given municipality-month observation. Spreads are the lending rate minus the deposit rate at the national level. All monetary variables are in 2010 BRL. See Section II and Appendix C for details.

Table 2: Financial Outcomes: Lending Spreads and Total Credit

	Months post M&A Episode			
	12	24	36	48
	(1)	(2)	(3)	(4)
$\ln\left(Credit^{Pr}\right)$	-.0707**	-.1167**	-.1713**	-.2156**
	(.0136)	(.0178)	(.0211)	(.024)
$Spread^{Pr}$	2.6433**	4.7973**	5.8816**	6.9869**
	(.4042)	(.4979)	(.5707)	(.6257)
$\ln(Credit)$	-.0214**	-.0557**	-.1024**	-.1443**
	(.0114)	(.0143)	(.0167)	(.019)
$Spread$	1.1684**	3.3619**	4.2009**	5.0988**
	(.3079)	(.3838)	(.4289)	(.4636)
Controls	Y	Y	Y	Y
Month-Year × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	238,286	236,511	232,269	229,122

Note: **, *, + indicate significance significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(2) estimated at the monthly-municipality level using 2005-2015 data from 18 months before and 12, 24, 36 and 48 months after the M&A episode, as described in Sections II and III. Regression outcomes are the log of total credit, $\ln(Credit)$, and lending spreads (local interest rates minus country level deposit rate), $Spread$, from new loans in a municipality m, in region r and time t. The superscript Pr denotes the variables computed using loans originated on private banks only. For each municipality, interest rates are aggregated using loan size as weights and loan volume corresponds to the sum of value of all loans. Treatment municipalities are those that had at least one branch of both banks at the time of the M&A episode. The controls used are GDP and credit per capita in 2005 interacted with time dummies and the local exposure to the business cycle, computed as the the covariance of local growth rate with country level growth rate over 2002-2018. All regressions include time-region (mesoregion IBGE concept) and municipality fixed effects. Credit regressions are weighted by population in 2005 and spreads are weighted by credit volume from public banks in ESTBAN in 2005.

Table 3: Decomposition of Loan Volume Effect: Intensive vs Extensive Margin

	Months post M&A Episode			
	12	24	36	48
	(1)	(2)	(3)	(4)
$\ln\left(Credit^{Pr}\right)$	-.0707**	-.1167**	-.1713**	-.2156**
	(.0136)	(.0178)	(.0211)	(.024)
$\ln\left(\#Loans^{Pr}\right)$	-.0851**	-.1198**	-.164**	-.193**
	(.0116)	(.0146)	(.0178)	(.0204)
$\ln\left(LoanSize^{Pr}\right)$	.0144	.0031	-.0073	-.0226
	(.0089)	(.0106)	(.0135)	(.016)
Controls	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	238,286	236,511	232,269	229,122

Note: **, *, + indicate significance significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(2), estimated at the monthly-municipality level using 2005-2015 data from 18 months before and 12, 24, 36 and 48 months after the M&A episode. Regression outcomes are the log of total credit, $\ln\left(Credit^{Pr}\right)$, log of the number of private loans, $\ln\left(\#Loans^{Pr}\right)$, and log of the size of loans, $\ln\left(LoanSize^{Pr}\right)$. The superscript Pr denotes the variables computed using loans originated on private banks only. For details on controls, fixed effects, treatment control definitions, and regression weights see the notes on Table 2.

Table 4: Total Credit and Competition in the Baseline 36 months after M&A episode

	Panel A. $\ln\left(Credit^{Pr}\right)$				
	None (1)	N_B^{Pr} (2)	HHI^{Pr} (3)	N_B^{Pr} (4)	HHI^{Pr} (5)
δ_{POST}	-.2614**	-.4112**	.0278	-.398**	-0.0488
	(.0243)	(.054)	(.0454)	(.0730)	(.0828)
δ_C		.0707**	-.3338**	.0715**	-.305**
		(.0092)	(.107)	(.00954)	(.108)
δ_μ				-.0653	.239
				(.218)	(.219)
	Panel B. $Spread^{Pr}$				
	None (1)	N_B^{Pr} (2)	HHI^{Pr} (3)	N_B^{Pr} (4)	HHI^{Pr} (5)
δ_{POST}	7.49**	14.71**	-.1092	14.25**	2.311
	(.5945)	(1.2921)	(1.1171)	(1.730)	(2.084)
δ_C		-2.32**	11.98**	-2.375**	10.82**
		(.235)	(2.4506)	(.246)	(2.509)
δ_μ				2.795	-7.044
				(5.515)	(5.497)
Controls	Y	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y
Obs	232,269	232,269	232,269	232,269	232,269

Note: $**$, $*$, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(5), and the differential effect based on each competition variable in the baseline, δ_C. For Columns (4) and (5) we also add the triple interactions with the market share of merging banks in the baseline, δ_μ. We estimate Eq.(5) at the monthly-municipality level using 2005-2015 data from 18 months before 36 months after the M&A episode. The competition variables are: number of private banking conglomerates, N_B^{Pr}, and concentration of private credit (stock) from ESTBAN, HHI^{Pr}. For details on dependent variables, others controls, fixed effects and treatment control definitions, see the notes on Table 2. We add number N_B^{Pr} and HHI^{Pr} at the moment of the M&A episode as controls beyond those of Table 2.

Table 5: Financial Outcomes with Stacking (36mo Window)

	(1)	(2)	(3)
$\ln\left(Credit^{Pr}\right)$	-.173^{+}	-.223*	-.2117*
	(.0826)	(.085)	(.0768)
$Spread^{Pr}$	5.0878*	6.5938**	6.0711*
	(1.9223)	(1.9285)	(2.1842)
Distance to Merge FE	Y	Y	Y
Month × Region × Cohort	Y	Y	Y
Municipality × Cohort FE	Y	Y	Y
Obs	2,384,920	1,941,967	872,224

Note: $**$, $*$, + indicate significance significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality-cohort (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(6) estimated at the monthly-municipality-merge level using 2005-2015 data from 18 months before and 36 months after the M&A episode, as described in Sections IV. Regression outcomes are the log of total credit, $\ln(Credit)$, and lending spreads (local interest rates minus country level deposit rate), $Spread$, from new loans in a municipality m, in region r and time t. The superscript Pr denotes the variables computed using loans originated on private banks only. For each municipality, interest rates are aggregated using loan size as weights and loan volume corresponds to the sum of value of all loans. For each merger, we construct a sample of treatments and controls, and then stack all of them to estimate the desired effects, as described in Deshpande and Li (2019). Treatment municipalities for a given merger are those that had at least one branch of both banks at the time of the M&A episode. All regressions include time-region-cohort (mesoregion IBGE concept) and municipality-cohort fixed effects, as well as a distance (in months) to the merger. Credit regressions are weighted by population in 2005 and spreads are weighted by credit volume from public banks in ESTBAN in 2005. Column 1 includes all municipalities, Column 2 uses only municipalities with zero mergers in the control and Column 3 does not use the data of a municipality after it is exposed to a new merger inside the merger window of a different merger.

Table 6: Financial Outcomes: only one merging bank versus none (36mo window)

	$\ln\left(Credit^{Pr}\right)$	$\ln\left(Credit^{Pr}\right)$	$Spread^{Pr}$	$Spread^{Pr}$
	(1)	(2)	(3)	(4)
δ_{POST}	-0.0143	-0.0132	-1.734**	-1.861**
	(0.00973)	(0.00970)	(0.281)	(0.289)
δ_{SPILL}		-0.0416**		0.647**
		(0.00714)		(0.187)
Controls	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	212,805	212,805	212,805	212,805

Note: $**$, $*$, + indicate significance significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(2) controlling for the spillover effect (a dummy variable if any other municipality in the same microregion is exposed to an M&A episode, with an associated coefficient δ_{SPILL}), estimated at the monthly-municipality level using 2005-2015 data from 18 months before and 36 months after the M&A episode. Treatment municipalities are those that had at least one branch of only one of the involved banks in the M&A episode and control are those that had none. For details on outcome variables, controls, and regression weights, see notes in Table 2.

Table 7: The effect on Branches and firm and loan characteristics

	Months post M&A Episode			
	12	24	36	48
	(1)	(2)	(3)	(4)
Branch (Pr) per 100,000	.0033	.0431	.0119	-.0228
	(.0226)	(.0294)	(.036)	(.0424)
Branch (Total) per 100,000	.0608*	.1236**	.1006*	.0759
	(.0276)	(.0368)	(.0463)	(.0554)
Maturity	4.3145	4.4063	10.5339**	9.1628**
	(3.1377)	(2.8515)	(3.4299)	(3.5054)
Relative Spread of Loans to Small Firms	.0291+	.0625**	.0441*	.0294
	(.0166)	(.0183)	(.021)	(.0232)
Share of Loans to Small Firms	.0063*	.0139**	.0104**	.0067*
	(.0022)	(.0026)	(.0028)	(.0029)
Firm Age	-.254**	-.337**	-.383**	-.351* *
	(.0956)	(.1003)	(.1141)	(.1249)
Share of Relationship Loans	-.004	-.0146**	-.0191**	-.0214**
	(.0042)	(.0044)	(.0047)	(.0051)

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(1) estimated at the monthly-municipality level using 2005-2015 data from 18 months before and 12, 24, 36 and 48 months after the M&A episode, as indicated in the headers of each column. Regression outcomes are, in order with the source in parenthesis, branches of private banks per 100,000 inhabitants (ESTBAN), branches of any bank per 100,000 inhabitants (ESTBAN), average maturity of loans in days (SCR), log of the ratio of spreads to small firms versus spreads over all loans (SCR), share of loan volume to small firms (SCR), average age of firms in years (SCR) and share of loan volume made to banks and firms with at least a 2 year relationship since their first loan (SCR). For control, treatment/control definition, and regression weights see notes of Table 2. All regressions include time-region (mesoregion IBGE concept) and municipality fixed effects.

Table 8: Effect of M&A episodes on Defaults

	Months post M&A Episode					
	$\leq$ 20 Private Banks			All Markets		
	24	36	48	24	36	48
	(1)	(2)	(3)	(4)	(5)	(6)
% Credit in Default	.0058**	.0085**	.0122**	.002**	.0044**	.0068**
	(.0015)	(.0015)	(.0017)	(.0009)	(.0011)	(.0013)
% Loans in Default	.0015	.0039**	.0089	-.0004	.0009	.0038**
	(.0012)	(.0011)	(.0011)	(.0007)	(.0007)	(.0009)
Controls	Y	Y	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y
Obs	152,482	151,039	147,602	153,399	151,936	148,491

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(1) estimated at the monthly-municipality level using 2005-2015 data from 18 months before and 24, 36 and 48 months after the M&A episode, as indicated in the headers of each column. We estimate the effect on our benchmark sample without the municipalities with more than 20 private banking conglomerates in 2005 and all municipalities. Regression outcomes are share of credit (in terms of volume) and share of loans (in terms of number) in default one year after their initial date. For controls, treatment/control definition, and regression weights see notes of Table 2.

Table 9: Financial Outcomes: Sector Heterogeneity (36mo window)

	All Sectors	Tradable	Non-Tradable	Construction
$\ln\left(Credit^{Pr}\right)$	-.1422**	-.1742**	-.3417**	-.1940**
	(.0242)	(.0475)	(.0313)	(.0639)
$\ln\left(Spread^{Pr}\right)$	.1712**	.1829**	.206**	.2895**
	(.0242)	(.0348)	(.0289)	(.0349)
$\ln(Credit)$	-.1266**	-.1566**	-.2479**	-.1875**
	(.0223)	(.045)	(.0261)	(.0548)
$\ln(Spread)$	.1752**	.1903**	.1924**	.2543**
	(.022)	(.0346)	(.0256)	(.0344)
Controls	Y	Y	Y	Y
Year × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	162,051	162,051	162,051	118,363

Note: **, *, + indicate significance significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(2) estimated at the monthly-municipality level using 2005-2015 data from 12 months before and 36 months after the M&A episode (excluding the first 12), as described in Sections II and III. Regression outcomes are the log of total credit, $\ln(Credit)$, and log of lending spreads (local interest rates minus country level deposit rate), $Spread$, from new loans in a municipality m, in region r and time t in each sector. Sectors are defined as in Mian and Sufi (2014). The superscript Pr denotes the variables computed using loans originated on private banks only. For each municipality, spreads are aggregated using loan size as weights and loan volume corresponds to the sum of value of all loans. For details on controls and treatment/control definition, and fixed effects, see notes of Table 2.

Table 10: Effect on Employment and Wages by Municipality (36mo window)

	Agriculture (1)	Tradable (2)	Non-Tradable (3)	Construction (4)
Panel A. ≤ 20 Private Banks				
Employment	.03	-.0614**	-.0534**	-.0933**
	(.0307)	(.0136)	(.0084)	(.0275)
Wages	.0169	-.0189**	-.0022	-.0038**
	(.0124)	(.008)	(.0039)	(.0103)
Panel B. All Municipalities				
Employment	.0356	-.0429**	-.0397**	-.0643**
	(.0281)	(.0124)	(.0086)	(.0236)
Wages	.0192+	-.0095	-.0033	.0073
	(.0114)	(.0079)	(.0036)	(.0091)
Controls	Y	Y	Y	Y
Year × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(2) estimated at the monthly-municipality level using 2005-2015 data from 12 months before and 36 months after the M&A episode (excluding the first 12), as described in Sections II and III. Regression outcomes are total employment and average annual wage of all workers employed at a given month-sector computed from RAIS. Sectors are defined as in Mian and Sufi (2014). Panel A is our benchmark sample with municipalities with at least one and no more than 20 private banking conglomerates in 2005. Panel B has the same results for all municipalities. For details on controls and treatment/control definition, and fixed effects see notes of Table 2. We use population in 2005 as weights in the regression. Regressions in Panel A have each 178,993 observations, while those in Panel B have 195,536.

Table 11: Spread Effect on Payroll by Municipality (36mo window)

	Panel A. $\leq$ 20 Private Banks				
	Tradable (1)	Non-Tradable (2)	Construction (3)	Total (4)	Total – Agr (5)
$\ln\left(Spread^{Pr}\right)$	-0.619**	-0.238**	-0.341*	-0.161*	-.3008**
	(0.216)	(0.0484)	(0.158)	(0.0719)	(.0918)
	Panel B. All Municipalities				
	Tradable	Non-Tradable	Construction	Total	Total – Agr
$\ln\left(Spread^{Pr}\right)$	-0.383*	-0.198**	-0.214	-0.0939	-.2248**
	(0.149)	(0.0504)	(0.204)	(0.0768)	(.08440)
Controls	Y	Y	Y	Y	Y
Year × Region FE	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the average causal effect, that is, the coefficient δ_{IV} from Eq.(3) estimated at the monthly-municipality level using 2005-2015 data from 12 months before and 36 months after the M&A episode (excluding the first 12), as described in Sections II and III. Regression outcome in the second stage is total payroll by sector computed as the multiplication of total employment and average annual wage of all workers employed at a given month-sector computed from RAIS. Sectors are defined as in Mian and Sufi (2014). The endogenous variable, $\ln\left(Spread^{Pr}\right)$, is the log of spread by municipality from new loans for firms of a given sector by private banks. For each municipality, interest rates are aggregated using loan size as weights and loan volume corresponds to the sum of value of all loans. Panel A is our benchmark sample with municipalities with at least one and no more than 20 private banking conglomerates in 2005. Panel B has the same results for all municipalities. Column 5 has the total payroll minus that of agriculture. For details on controls and treatment/control definition, and fixed effects see notes of Table 2. We use population in 2005 as weights in the regression. Regressions in Panel A have each 152,457 observations, while those in Panel B have 172,847.

Table 12: Geographic Spillovers on Financial Variables: Microregions and Mesoregions

	Mesoregions		Microregions	
	24 mo (1)	36 mo (2)	24 mo (3)	36 mo (4)
$\ln\left(Credit^{Pr}\right)$	-.0133**	-.0186**	-.048**	-.0545**
	(.0034)	(.0034)	(.0124)	(.0136)
$Spread^{Pr}$	.4702**	.6012**	.8244*	.9732*
	(.1031)	(.1004)	(.4105)	(.4157)
Controls	Y	Y	Y	Y
Month	Y	Y	Y	Y
Month × Region FE	N	N	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	200,525	200,525	198,434	198,434

Note: **, *, + indicate significance significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(8) estimated at the monthly-municipality level using 2005-2015 data from 18 months before and 24 and 36 months after the M&A episode, as indicated in the headers of each column. For details on outcome variables and controls, see notes in Table 2. The sample of municipalities is this table excludes those exposed to M&A episodes. Treatment municipalities are those that had at least one market in their meso or microregions (geographic concepts in the Brazilian Census) exposed to the M&A episode. Regressions for micro-region spillovers (Columns 3 and 4) include time-region (mesoregion IBGE concept) and municipality fixed effects, while regressions for mesoregion spillovers only time and municipality fixed effect. Credit regressions are weighted by population in 2005 and spreads are weighted by credit volume from public banks in public banks in ESTBAN in 2005.

Table 13: Geographic Spillovers on Microregions: Employment and Wages (36mo Window)

	Time-Region FE			
	Agriculture	Tradable	Non-Tradable	Construction
Employment	-0.00583	-0.00130	-0.00136	0.000230
	(0.0110)	(0.00307)	(0.00161)	(0.00882)
Wages	-0.0000	-0.00417	-0.00417**	-0.00915**
	(0.00527)	(0.00258)	(0.00168)	(0.00378)
	No Time-Region FE			
Employment	-0.0112	-0.00880***	-0.00517***	-0.0126*
	(0.00792)	(0.00161)	(0.000974)	(0.00648)
Wages	0.000162	-0.00461**	-0.00635***	-0.0102***
	(0.00493)	(0.00212)	(0.00133)	(0.00327)

Note: **, *, + indicate significance significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(8) estimated at the monthly-municipality level using 2005-2015 data from 18 months before and 36 months after the M&A episode, as indicated in the headers of each column. For details on outcome variables and controls, see notes in Table 2. The sample of municipalities is this table excludes those exposed to M&A episodes. Treatment is the number of municipalities in their meso or microregions (geographic concepts in the Brazilian Census) exposed to the M&A episode. Panel A Regressions include time-region (mesoregion IBGE concept) and municipality fixed effects, while regressions in Panel B include only time and municipality fixed effects. Regressions are weighted by population in 2005.

Table 14: Semi-Elasticity and Elasticity of Demand for Bank Credit (36mo window)

	$\ln\left(Credit^{Pr}\right)$	$\ln(Credit)$	$\ln\left(Credit^{Pr}\right)$	$\ln(Credit)$
$Spread^{Pr}$	-0.0317**			
	(.0041)			
$Spread$		-0.0295**		
		(.0046)		
$\ln\left(Spread^{Pr}\right)$			-1.311**	
			(.1574)	
$\ln(Spread)$				-1.0507**
				(.1633)
Controls	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	268,725	268,725	239,475	268,725

Note: **, *, + indicate significance significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the average causal effect, that is, the coefficient δ_{IV} from Eq.(3) estimated at the monthly-municipality level using 2005-2015 data from 12 months before and 36 months after the M&A episode (excluding the first 12), as described in Sections II and III. This table uses data from all municipalities, even those we do not use in our benchmark sample. For variable definitions, treatment/control definition, and fixed effects see notes of Table 2.

Table 15: Data vs Model Predictions: Concentration, Spreads and Payroll

	$Spread^{Pr}$	Payroll	PE	GE
HHI^{Pr}	29.2904**		31.54	32.42
	(5.9367)		(.7048)	(.5981)
$Spread$		-.0034*	-.0045	-.0036
		(.0015)	(.4863)	(0.9184)
Controls	Y	Y		
Month × Region FE	Y	Y		
Municipality FE	Y	Y		
Obs	266,098	226,243		

Note: **, *, + indicate significance significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the average causal effect, that is, the coefficient δ_{IV} from Eq.(3) estimated at the monthly-municipality level using 2005-2015 data from 12 months before and 36 months after the M&A episode (excluding the first 12). The second stage dependent variable is in each column, while the endogenous are in the rows This table uses data from all municipalities, even those we do not use in our benchmark sample. Payroll is computed as the multiplication of total employment and average annual wage of all workers employed at a given month computed from RAIS. HHI^{Pr} is computed by stock of credit of private banks in ESTBAN. For other variable definitions, treatment/control definition, and fixed effects see notes of Table 2. Columns 3 and 4 have the partial equilibrium (PE) and general equilibrium (GE) predictions and p-values in parenthesis. For more details on how the PE predictions are computed, see Section V.3. For calibration, model solution and GE predictions, see Appendix G.

Table 16: Aggregate Counterfactual Outcomes

	$\%\Delta\mathcal{Y}$	$\%\Delta\mathcal{K}_B$	$\Delta\frac{\Pi}{Y}$	$\%\Delta w$	$\Delta Spread$
	Panel A. One Extra Bank				
Partial Eq.	2.44	14.04			-4.43
General Eq.	1.32	13.75	.96	0.55	-4.43
	Panel B. Public Bank Competition				
Partial Eq.	1.79	10.46			-3.3
General Eq.	0.97	10.24	0.66	0.4	-3.3
	Panel C. Spreads at World Level				
Partial Eq.	8.96	40.8			-12.87
General Eq.	4.83	39.7	6.51	2.02	-12.87

Note: Percentage changes in Output, $\%\Delta\mathcal{Y}$, bank capital, $\%\Delta\mathcal{K}_B$, wages, %, and p.p. changes in profits of all firms over GDP , $\Delta\frac{\Pi}{Y}$, and spreads, $\Delta Spread$, for each of our counterfactual exercises. For details on the model, see Section V. For details on the numerical solution, calibration and specifics of each counterfactual see Appendix G.

C. Dataset Construction

In this section we provide details on the data.

Data Sources for descriptive statistics throughout the text.

- 5-Asset Share of Banks: World Bank Global Financial Development Database.
- Finance as a major constraint and share of investments financed with banks: World Bank Enterprise survey.
- External Finance: Beck, Demirgüç-Kunt and Levine (1999), available in World Bank Financial Structure Database. External funding defined as sum of bank credit, private bond market and stock market capitalization (Moll (2014)).
- DOJ's HHI criteria: The U.S. Department of Justice guidelines indicates that markets with an HHI above .25 are *very concentrated*, see https://www.justice.gov/atr/herfindahl-hirschman-indexhere.
- World Spreads: World Bank WDI dataset.
- Share of credit subsidized in agriculture: BCB time series management system

Credit Registry. For each loan in SCR, we have various characteristics and entries in the dataset that are relevant for our analysis. We start this section by explaining each of them and how they enter into our final dataset. We drop the loan if it has either missing or a negative value for a categorical positive variable in any of these variables.

- *Loan index rate and base rate:* loans in the data can have several interest rate structures, such as inflation + premium, LIBOR + premium etc.. The loan index rate variable shows what is the economic indicator (if any). We observe the economic indicator (inflation, LIBOR, target rate etc.) and the premium (which is denoted the loan base rate). We use the observed value of the economic indicator to compute the final interest rate of each loan by summing the value of the indicator with the premium (loan base rate). We exclude loans with negative or larger than 1000 % (yearly) interest rates.
- *Loan Resource*: loans in the data can be either funded with government resources (even in private banks) and by using the bank's own resources. We keep only the second type of loan - given that loans with government resources are subject to several allocation regulations and interest rate caps - and drop those that do not have any resource information.
- *Firm Size*: firms in the dataset are divided in four categories: micro, small, medium, large, depending on the gross revenue in a given year. In our sample, we call a small firm what the

BCB defines as a micro firm (i.e., their smallest category). [27].

- *Loan Type*: loans are classified according to their intended usage by firms (working capital, export financing etc..). We use this information to exclude real-state loans and loans to banks from our sample.
- *Firm Industry*: firms are classified according to IBGE's sector classification, called CNAE (*Classificação Nacional de Atividades Econômicas*). We use the first two digits of the CNAE code and match the sector of the firm to those in Mian and Sufi (2014), as detailed below.
- *Maturity*: From the loan start/end date, we compute the maturity of loans in our sample in terms of days. We exclude loans with maturities of less than one day
- *Loan Size*: To compute the loan size, we use the total of the amount outstanding, unreleased and credit line, that is: the total amount available for the firm.
- *Loan Rating*: Loans in our dataset have a rating system. Resolution $2,682/1999$ of the BCB established that all financial institutions should classify their credit exposures into nine levels of risk, varying from AA and A to H. Rating AA represents the best rating a loan can achieve (lowest credit risk) and H represents the worst rating a loan can be assigned (highest credit risk). All banks have to maintain an internal credit rating scheme based on the guidelines set by the Central Bank. We keep only loans with ratings from $AA-C$, as loans classified D or lower are those already in default/renegotiation. Loans from category AA have zero provisions for default, while those at C (lowest in our sample) have 3% provisions. [28]
- *Default*: Loans in our sample are considered in default if they are more than 90 days late. To create a time-structure for defaults for the DiD analysis, loans are determined to be in default if they are in default one year later than the loan start date.

To avoid results drive by outliers, we winsorize our sample by removing the 1% highest and lowers interest rates and loan amounts.

The data has several dates associated with each loan: loan start/end date, firm start date, and bank-firm start date (first loan recorded). Each loan can appear more than once in the sample: every time a credit line is used, for instance, or when a loan goes into default it reappears in the sample. Additionally, some data provided by the banks can contain errors. Therefore, for each month-year, we determine loans to be part of our sample if all of the conditions below are satisfied:

1. Firm start date is smaller than firm-bank relationship start date.

[27] The categorization is as follows: micro: one whose gross annual income is equal to or less than R$ 240,000.00, small: one whose gross annual revenues exceed R$ 240,000.00 and is equal to or less than R$ 2,400,000.00, medium: one whose gross annual revenues exceed R$ 2,400,000.00 and is equal to or less than R$ 300,000,000.00.

[28] For more details: https://www.bcb.gov.br/pre/normativos/res/1999/pdf/res_2682_v$_{L}.pdf$

2. Firm-bank relationship start date smaller or equal than loan start date.
3. Loan start date smaller or equal than loan end date.
4. Loan start date equals to the month and year in question.

Relationship Loans. A loan is defined a relationship-loan if the firm-bank relationship start date is at least two years before the current loan.

Aggregation. We aggregate loans to zip codes (5 digits) by using the firm zip code, and then aggregate to the municipality level by using a dataset that links which zip code belongs to each municipality. For 81 out of 20,334 zip codes, the 5 digit zip code is divided between two municipalities. In this case, we allocate the share of loans for each municipality randomly corresponding to the share of 8 digit zip codes that is in each municipality.

Unicad. Unicad contains bank and conglomerate ids over time, as well as bank ownership (public vs private, for instance). For banks without conglomerate data, we assume that the conglomerate id is the same as the bank id. If bank ownership data is missing, we assume that the bank is a private bank. All major public banks in Brazil are captured by this definition.

Estban. ESTBAN [29] contains the balance sheet of each banking conglomerate as well as the number of branches per municipality. To determine the amount of credit, we use the following accounting entry: *verbete_160_operacoes_de_credito*, which translates to "credit operations" in the asset of each bank. [30] ESTBAN has two measures of branches: expected and realized. We use the realized measure of branches in a given year.

RAIS and IBGE. The labor market dataset, RAIS, is available publicly (without worker or firm identifiers). We drop firms that are not operating or that have zero registered employees. [31]. Municipality level output and population is available at IBGE's Sidra system. [32]

Alternative M&A measure. As exposure to M&A episodes is key in our paper, we construct an alternative measure for robustness. Since there are no large bank exits in Brazil from 2005-2015, we indirectly identify a M&A in our sample if both of the following conditions are satisfied:

1. The financial conglomerate had at least 10 % market share in at least 10 % of the markets.
2. The financial conglomerate in the database had a reduction of 95 % in loan volume for 99 % of the markets. between two years

The idea of condition 1 is to pin down large enough banks that either merged or were acquired, while the idea of condition 2 is to determine if this conglomerate stopped providing loans in all

[29] For data access: https://www.bcb.gov.br/estatisticas/estatisticabancariamunicipios
[30] For accounting definitions and balance sheet entries: https://www3.bcb.gov.br/aplica/cosif/completo
[31] For data access: . The data must be accessed through a Brazilian IP.
[32] For data access: https://sidra.ibge.gov.br

markets. Results are robust to variations in the thresholds above. A market us exposed to an episode if it had the bank that exited the market satisfying the conditions above. The advantage of our main measure is transparency, even though we may lose a potential M&A episodes is both banks change their conglomerate ids, for instance. The advantage of this alternative, more indirect measure, is that we capture directly exits in the market, but at the cost of transparency and identifying which banks were involved in an M&A episode. Overall, the measures are extremely close, with a correlation of .8 and our results are robust to the use of either (available upon request).

Sector Classification. We follow the Mian and Sufi (2014) sector classification. We use IBGE's CNAE classification [33]. We use the first 2 digits of the CNAE code to determine the industry of a firm according to Table C.1.

Table C.1: Sector Classification From the 2 First Digits of CNAE Code

Sector	Cnae (2 First Digits)
Agriculture	1
Construction	16, 41-43, 71
Tradable	5-8, 10-13, 15, 22-31
Non-Tradable	45-47 55-56

D. Default and Spreads

In this section we explore how much the effect on defaults can explain the increase in spreads we observe in the data. For that, we first introduce some notation. Let π be the bank profit per unit of capital, s the level of spreads, p the probability of repayment, r the deposit rate and c the share of loans in default that is recovered. We can write the bank profit per unit of capital, π as

$$\pi = (1 + s + r)[p + (1 - p)c] - (1 + r)$$

The idea is that the bank must repay to depositors $(1 + r)$ regardless. In case of repayment by the firm, it gets $1 + s + r$, and in case of default it recovers c of the repayment owed to the bank (here, we could have assumed the bank receives a share on the loan principal times interest, without the spread. We opt for this version to guarantee that if $c = 1$, changes in default have no effect in the profit per unit of the capital of the bank). With values for c, r, p and s, we can compute the bank profit per unit of capital and compare how it changes pre and post an M&A episode. We use $s = 35p.p.$ and $p = .98$ based on our markets in the sample. The results for $r = 13.75\%$ (average over

[33] Available at: https://concla.ibge.gov.br/classificacoes/por-tema/atividades-economicas/classificacao-nacional-de-atividades-economicas

sample period), $c = .13$ [34] for our sample of municipalities with less than 20 banks in Dec/2005 are that default adjusted spreads increase 4.2 p.p. (compared to 4.79 p.p.) 24 months after than M&A episode, 5 p.p. (compared to 5.88 p.p.) 36 months afterwards and 5.5 p.p. (compared to 6.89 p.p.) 48 months afterwards.

An alternative way of considering the effect of defaults is to consider that defaults act instantly, while spreads are only fully accrued over the year. We use an approximation that our average maturity over the 365 days, $250/365 \approx .68$ of spreads are only accrued. In this case, for the 36 month window in our sample of municipalities with less than 20 banks in Dec/2005, we have that the increase in bank profit per unit of capital would be equivalent to an increase of 4.7 p.p. in spreads.

E. Case Study: Itaú-Unibanco

In this section we show a case study of the largest merger in our sample - the merge between Itaú and Unibanco. At the moment the merger, Itaú was the second largest private bank in Brazil, while Unibanco was the third. Before the merger, Itaú and Unibanco had jointly over 3600 branches, which represented 36% of branches from all private banks in Brazil (and 20% of all branches, including public banks). Jointly, the banks had a 24% market share in private credit. The new bank, creatively named Itaú-Unibanco, was among the 20 largest banks in the world and had close to BRL 1 trillion in assets (in 2019 BRL). The Itaú-Unibanco merger was announced of Oct/2008, approved by the Brazilian Central Bank in Feb/09 and finally approved by the antitrust authority (CADE) in Aug/2010.

We use the same identification strategy and interpretation as in Section III for this case-study, but focusing on the unique merger of Itaú-Unibanco. Through this section, we thus estimate Eq.(25)

$$y_{m,t} = \alpha_m + \alpha_{r,t} + \sum_{\tau} \delta_\tau \mathcal{I}_{m,t-\tau} + \varepsilon_{m,t} \tag{25}$$

where $y_{m,t}$ is the outcome of municipality m in month t, α_m are municipality fixed effects, $\alpha_{r,t}$ are region-time fixed effects and $\mathcal{I}_{m,t-\tau}$ is a dummy variable that is one if municipality m was exposed to the Itaú-Unibanco merge $t-\tau$ periods before. Under the identifying assumptions, the δ_τ's coefficients in Eq.(25) are the effect of changes in competition over time on the exposed municipalities (treatment effect on the treated). According to the BCB rules, no bank identifying information from the credit registry can be used, so we use only publicly available municipality level balance sheet data (ESTBAN) form banks in this case study. We focus on branches and credit

[34] As estimated by the World Bank. Source: http://pubdocs.worldbank.org/en/252851536597936709/Policy-Note-Credit-Markets.pdf.

(total and from Itaú-Unibanco only) as outcomes.

First, we can see in Figure H.8 that there is no pre-trend and no long-term significant effect on branches per 100,000 inhabitants. Even though though there is a restructuring process where the number of branches increases after the merger, all of the increase is eventually undone and the number of branches is close the the pre-merger level.

Second, we focus on the credit from Itaú-Unibanco versus other private and other public banks pre and post merger. We can see in Figure H.9 that other private banks reduce their credit between exposed and non-exposed municipalities, but less so than Itaú-Unibanco, while cred from public banks increases. This is consistent with changes in local competition. For other private banks, the are conflicting channels at play. The merger increases their own market power and potentially increases the efficiency or merged banks (Jayaratne and Strahan (1996)), which would lead them to decrease quantities in equilibrium. On the other hand, if merged banks reduce their credit level, more clients are available and other private banks increase their credit supply. For public banks, the later channel seems to be the dominant one, which is consistent with the evidence in Sanches, Silva Junior and Srisuma (2018) that public banks are not directly competing with private banks in Brazil.

Third, we provide a visual representation of the merger effect with more or less banks in the baseline. We separate the treatment and control municipalities in those with 3 or less private banks or 5 or more private banks at the moment of the merge and re-run our analysis. We can see in Figure H.10 that the effect is larger when there are less banks in the baseline, that is, the marginal bank is more relevant for credit outcomes when there are less banks competition, as implied in the competition model of Joaquim, Townsend and Zhorin (2019). There is no pre-trend and for both the effect is persistent. Quantitatively, the effect with 3 or less private banks is approximately 2.5 times larger.

Finally, we show that the initial market share of Itaú and Unibanco combined has no persistent effect in terms of the total quantity credit in a munipality beyond the exposure, that is, we estimate

$$y_{m,t} = \alpha_m + \alpha_{r,t} + \alpha_{r,t} \times \mu_{IU} + \sum_{\tau} \delta_\tau \mathcal{I}_{m,t-\tau} + \sum_{\tau} \delta_\tau^\mu \mu_{IU} \mathcal{I}_{m,t-\tau} + \varepsilon_{m,t} \tag{26}$$

where μ_{IU} is the combined share of Itaú and Unibanco in Oct/2008 and plot the coefficients δ_τ^μ in Figure H.11. There is a large positive differential effect of a large market share of Itaú and Unibanco on total credit right after the M&A announcement, but this effect eventually becomes zero and is not significant 5 months after BCB approval of the merger in Feb/2009. Taken together, we observe that the results obtained in the credit registry in terms of credit volume, heterogeneous competition in the baseline and relationship lending in this M&A episode.

F. Proofs and Derivations

F.1. Firm Problem and Aggregation

We keep implicit the dependence of the key terms on m to simplify the notation. Taking the FOC in the profit maximization problem of the firm, Eq. (12), with respect to l:

$$(1-\alpha)(zk)^{\alpha} l^{-\alpha} = w \Rightarrow l(a,z) = \left(\frac{(1-\alpha)}{w}\right)^{1/\alpha} zk(a,z) \tag{27}$$

In the profit function :

$$\begin{aligned}
\pi(r_i^{cc}, w|a,z) &= (zk)^{\alpha} l^{1-\alpha} - wl - r_i^{cc} k \\
&= \left[z\left(\frac{(1-\alpha)}{w}\right)^{(1-\alpha)/\alpha} - zw\left(\frac{(1-\alpha)}{w}\right)^{1/\alpha} - r_i^l \right] k \\
&= \left[z\alpha\left(\frac{(1-\alpha)}{w}\right)^{(1-\alpha)/\alpha} - r_i^{cc} \right] k = \left[\kappa(w)z - r_i^{cc}\right] k
\end{aligned}$$

Therefore, as long as $z\kappa(w) > r_i^{cc}$, the firm wants to scale the production up to the collateral constraint. The firms uses their own capital as long as $z\kappa(w) > r - \xi_i$, uses non-bank sources of external finance if $z\kappa(w) > \overline{r} - \xi_i$ and uses bank lending if $z\kappa(w) > r^l - \xi_i$, as in Figure 1. This is a consequence of constant returns to scale: the profit is linear in k, and thus managers either bind at one of the financial constraints or do not produce at all. Finally, the expected output if a firm is in fact producing with leverage λ_i

$$y_i(a,z) = (zk)^{\alpha} l^{1-\alpha} = \lambda_i za\left[(1-\alpha)/w\right]^{\frac{1-\alpha}{\alpha}} = \frac{\kappa(w)}{\alpha}\lambda_i za \tag{28}$$

From optimal input decisions for each firm and the assumption that $\mathbb{P}[\xi_i \geq a] = C_0 - e^{-\eta\xi}$ for a constant C_0, the optimal input choice made by firms implies that aggregate capital is

$$\begin{aligned}
\mathcal{K} = \int k_i(a,z)di &= \mathcal{X}\left[\mathbb{P}(z\kappa(w) > r - \xi_i) + \lambda_o \mathbb{P}(z\kappa(w) > \overline{r} - \xi_i) + \lambda_b \mathbb{P}(z\kappa(w) > r^l - \xi_i)\right] \\
&= \mathcal{X} e^{\eta\hat{z}-1}\left[e^{-\eta r} + \lambda_o e^{-\eta\overline{r}} + \lambda_b e^{-\eta r^l}\right] = \mathcal{X} e^{\eta\hat{z}-C_0} e^{\theta}
\end{aligned} \tag{29}$$

where $\hat{z} \equiv z\kappa(w)$ and $\theta \equiv \ln\left(e^{-\eta r} + \lambda_o e^{-\eta\overline{r}} + \lambda_b e^{-\eta r^l}\right)$. Moreover, aggregate labor demand is given by

$$\mathcal{L} = \int l_i(a,z)di = \left[\frac{(1-\alpha)}{w}\right]^{1/\alpha} z\mathcal{K} = \left[\frac{\kappa(w)}{\alpha}\right]^{1/(1-\alpha)} z\mathcal{K} \tag{30}$$

Therefore

$$\kappa(w) = \alpha \mathcal{L}^{1-\alpha} \mathcal{K}^{\alpha-1} z^{\alpha-1}$$

Moreover, we know that aggregate output can be written as

$$\mathcal{Y} \equiv \int y_i(a,z,\sigma)di = \frac{\kappa(w)}{\alpha} z\mathcal{K} \tag{31}$$

Replacing $\kappa(w)$ in the above equation yields Eq.(18). ■

F.2. Inverse Demand (Eq. 14)

Let ξ_i have a distribution such that $\mathbb{P}[\xi_i \geq a] = C_0 - e^{-\eta\xi}$ for a constant C_0. Given a lending rate r^l, the total demand for bank credit $\mathcal{D}_m(r^l_m)$ is given by

$$\mathcal{D}_m(r^l_m) = \frac{\lambda_b}{1 + \lambda_b + \lambda_o} \mathcal{X}_m \mathbb{P}\left[\xi_i \geq r^l_m - z_m \kappa(w_m)\right] \tag{32}$$

where $\mathcal{X}_m \equiv \int a_i di$ is total wealth in region $\mathcal{X}_m$. Substituting the distribution of ξ_i and taking logs implies that

$$\ln\left(\mathcal{D}_m(r^l_m)\right) = \gamma_m - \eta r^l_m \tag{33}$$

for

$$\gamma_m \equiv \ln\left(\frac{\lambda_b}{1 + \lambda_b + \lambda_o}\right) - C_0 + \ln(\mathcal{X}_m) + \eta \hat{z}_m \tag{34}$$

which is Eq.(14).

F.3. Lemma 1

Proof. Replacing the inverse demand of Eq.(14) on the maximization problem of the bank in Eq.(15), we have that the bank problem can be re-written as

$$\max_{Q_b} \left(\eta^{-1}[\gamma_m - \ln(Q_m)] - c_{b,m} - r\right) Q_b \tag{35}$$

where $Q_m \equiv \sum_b Q_b$ is the total quantity in the market. The first order condition implies [35]

$$\left(\eta^{-1}\left[\gamma_0 + \gamma_m - \ln(Q_m)\right] - c_{b,m} - r\right) - \eta^{-1}\frac{\partial \ln(Q_m)}{\partial Q_b} Q_b = 0$$

Thus

$$\left(r_m^l - c_{b,m}\right) - \eta^{-1}\frac{\partial \ln(Q_m)}{\partial Q_m} Q_m \mu_{m,b} = 0 \Rightarrow r_m^l - c_{b,m} - r = \eta^{-1}\mu_{m,b}$$

Aggregating over all banks (with market shares as weights), spreads $s_m \equiv r_m^l - r$ is given by

$$s_m = \sum_b^{B_m} \mu_{m,b}\left[c_{b,m} + \eta^{-1}\mu_{m,b}\right]$$

which implies $s_m = c_m + \frac{HHI_m}{\eta}$ for $c_m = \sum_b \mu_{b,m} c_{b,m}$. ■

F.4. Lemma 2

Proof. We keep implicit the dependence of the key terms on m to simplify the notation. Replacing Eq. (29) in Eq.(31) we have that

$$\mathcal{Y} \equiv \int y_i(a, z, \sigma) di = \frac{\kappa(w)}{\alpha} z \mathcal{X} e^{\eta \hat{z} - 1} e^{\theta}$$

Taking logs

$$\ln(\mathcal{Y}) = \ln\left(\frac{\hat{z}}{\alpha}\right) + \ln(\mathcal{X}) + \ln\left(e^{\eta \hat{z} - 1}\right) + \theta$$

Collecting terms we have Eq. (19). Statically and in partial equilibrium, the stock of wealth $\mathcal{X}$ and wages w (and thus $\hat{z}$) do not change. Thus

$$\frac{d\ln(\mathcal{Y})}{ds} = \frac{d\ln(\mathcal{Y})}{dr^l} = \frac{d\theta}{dr^l} = -\eta \frac{\lambda_b e^{-\eta r^l}}{e^{-\eta r} + \lambda_o e^{-\eta \bar{r}} + \lambda_b e^{-\eta r^l}} = -\eta\omega$$

[35] which is sufficient for the maximum in this case. Note that the objective function is concave. Let:

$$\pi^b(Q_b) \equiv \left(\eta^{-1}\left[\gamma_m - \ln(Q_m)\right] - c_{b,m} - r\right) Q_b \Rightarrow \frac{\partial^2 \pi^b(Q_b)}{\partial Q_b} = -\eta^{-1}\frac{1}{Q_m} - \eta^{-1}\frac{Q_m - Q_b}{Q_m^2} < 0$$

Finally, from the firm optimization problem

$$y_i(a,z) = \lambda_i az\left[\frac{1-\alpha}{w}\right]^{\frac{1-\alpha}{\alpha}} = \left[\frac{1-\alpha}{w}\right]^{-1} l_i(a,z) \Rightarrow \frac{1-\alpha}{w} y_i(a,z) = l_i(a,z) \tag{36}$$

$$\Rightarrow (1-\alpha)\mathcal{Y} = w\mathcal{L} \tag{37}$$

which implies that the total payroll ($w\mathcal{L}$) response is the same as the output. ■

G. Numerical Solution of the Model

In this section we detail how we solve, calibrate and compute the counterfactuals in the model presented in Section V.

Groups. As our model focuses on regional differences on the banking market, we aggregate all municipalities that have the same concentration of private credit (HHI from ESTBAN). For that, we use the first two digits of the HHI, that is, we categorize municipalities in 100 categories, going from 0 to 1 in their HHI of private credit for ESTBAN in December of 2007, which is before the wave of consolidation that started in 2008 in the Brazilian market. Denotes each group by $g = 1, \ldots, 100$ and let M_g be the set of municipalities in group g. For each group g, we compute total population, P_g, total output, Y_g and total bank lending (stock from ESTBAN) from private and public banks, K_g^B. We compute the average productivity by group g inverting Eq.(18), assuming that bank capital is a share of total capital, that is:

$$z_g = \exp\left\{\alpha^{-1}\ln Y_g - \ln K_g^B - (1-\alpha)\alpha^{-1}\ln P_g\right\}$$

and normalize the average productivity (weighted by population of each group to one). We do not attempt to model here the difference in scales in each municipality, and simply assume that all have initial average wealth of $X_m = 1$. We will then later aggregate the effect on each municipality using population as weights for their outcomes.

We initialize average spreads pre-counterfactuals at 18.3 p.p., which comes from the BCB report on spreads for the Brazilian economy for 2011-2016. As discussed in Section IV.1, these number is significantly smaller than the one in our sample due to the fact that we use new credit in each month, while the BCB uses the full stock of credit to compute spreads in a given month. For that, we compute $\bar{s}$, which is given by the difference of 18.3 and the weighted average HHI^{Pr} over the elasticity of demand, that is

$$\bar{s} \equiv .183 - \eta^{-1}\sum_g \left[\frac{P_g}{\sum_{\hat{g}} P_{\hat{g}}}\right] HHI_g^{Pr}$$

Solution. Given z_g, we solve the model as each municipality is independent, that is, we solve the model once for each group g, for a representative municipality within this group using the productivity z_g and concentration of this group HHI_g^{Pr}. For that, we guess an initial level of wages w_m, compute the optimal decision for each firm and aggregate to compute total municipality output $Y_m(w_m)$ and use a bysection algorithm to find the equilibrium wage in a given market. We compute excess labor demand, given by

$$EL_m(w_m) = (1-\alpha)\frac{Y_m(w_m)}{w_m} - w_m^{-\psi}$$

where we use the fact that total payroll is equal to $(1-\alpha)Y$ (see Section F.1). To compute the results in Table 16, we compute changes in output, bank capital etc. for each representative municipality of group g and aggregate using population P_g of each group as weights.

Calibration. The value and source of the parameters we use in our model are in Table G.1. A few comments are in order. First, we solve the model repeatedly to match two parameters: the elasticity of labor supply φ and ability to self finance λ. To estimate φ, we replicate the results of Table 15 for waves and recover that the relative response of wages to payroll is .42. We calibrate λ to match the External Finance/GDP ratio observed in the Brazilian economy according to Beck, Demirgüç-Kunt and Levine (1999).

We compute the share of competitive bank capital as follows. From Brazil's Institute of Applied Economic Research, we have that that capital over output, K/Y, is 2.49. and from Beck, Demirgüç-Kunt and Levine (1999), we have that external finance over GDP, E/Y, for Brazil is 1.43 and that banks account for 45% of that from 2005-2015. From BCB aggregate data, we have that 55% of bank loans are non-earmarked in Brazil. Therefore, the share of competitive bank capital from all capital is given by

$$\omega = \frac{E/Y}{K/Y} \times .45 \times .55 \approx 14.21\%$$

we calibrate λ_o and λ_b to match $\omega = 14.21\%$ and that the $E/Y = 1.43$. This implies a $\lambda = 8.24$. In the literature, λ is generally calibrated to be around 1.5-2.5 for developing economies. However, λ has a different meaning in our model. For Moll (2014), for instance, every entrepreneur that chooses to produce will be against the constraint of $k = \lambda a$. In our model, firms can use their own capital, use other sources of finance and banks. Therefore, the average leverage of entrepreneurs that do choose to produce will be lower than the maximum leverage, λ, given that large shares of them will not use the sources of external finance.

Merger Simulation and Counterfactuals. We simulate a merger in our economy by changing local concentration on all municipalities by computing the new HHI as:

$$HHI_g^{'} = HHI_g^{Pr} - .0588 \times \eta$$

Table G.1: Parameter Values for Quantitative Exercise

Parameter	Model	Value	Source
		Estimated	
η	Semi-Elasticity of Demand	-.0317	Table 14
φ	Elasticity of Labor Supply	1.4	Calibrated
		Brazil Specific	
r	Deposit Rate	11.25%	Brazil's Policy Rate (Dec/2007)
λ_o	Fin Friction	6.08	$\omega = 14.21\%$ and $E/Y = 1.43$
λ_b	Fin Friction	2.15	$\omega = 14.21\%$ and $E/Y = 1.43$
$\overline{r}$	Subsidized rate for loans	.16	BCB
		Standard	
α	Mg. Prod of k	.4	

that is, to guarantee that the change in spreads will be as observed in Table 2 for 36 months. We have also shifted the HHI distribution by multiplying local HHI by a constant and calibrate the constant to match the 5.88 reduction in spreads, as seen in 2 and the results are not sensitive to this choice. The important outcome here is not the absolute level of responses, but rather the average causal response of payroll to spreads in the model. To compute the first counterfactual, we change HHI for each group g as follows:

$$HHI_g^1 = \frac{1}{\frac{1}{HHI_g^{Pr}} + 1}$$

that is, we compute the HHI-equivalent number of banks (its inverse, add one extra bank and invert it again to recover the new HHI. For the second counterfactual, we simply use the HHI of all credit (private and public, subsidized or not) from municipality-bank balance sheet data (ESTBAN). Finally, for the third counterfactual, we compute the HHI that would be necessary to guarantee spreads in all municipalities were equal to 5.43 p.p., that is $HHI_g^3 = \eta^{-1}(.053 + r - \overline{s})$.

Efficiency Gains. Following the 65% of spreads come from administrative costs or default. We use this number to compute our baseline level of $\overline{c}$, in particular, we assume that $\overline{c} = .183 \times .65 = .1189$, that is, that 11.89 p.p. of our baseline level of 18.3 p.p. of spreads are due to administrative costs and defaults.

For each municipality, we assume that the benchmark concentration is the case where there is one extra bank (such that we can remove a bank from all municipalities), that is:

$$HHI_m^{Bench} = \frac{1}{\frac{1}{HHI_g^{Pr}} + 1}$$

We assume that the market is initially composed of symmetric banks, with cost $\bar{c}$.

For the municipalities with only one bank involved in the merger (either A or B, but not both), we compute the changes in spreads for two cases. The first case (left panel of Figure 7) is the one where only the cost of bank b is reduced to $\bar{c}(1-\chi)$. The structure of our model implies that the market share weighted cost - and thus spread - in this case is gonna be reduced by a combination of three effects: (i) the reduction in cost of the bank, (ii) the gain of market share from this bank, reducing even more the market share weighted cost of the market, and (iii) the increase in concentration from this gain in efficiency. Mathematically, from the optimization of each bank in Lemma 1, we have that

$$s_m = c_{m,b} + \eta^{-1}\mu_{m,b} = \bar{c} + c_m + \eta^{-1}\mu_{m,b}$$

Let b_0 be a bank involved in the merger in a market with N banks. We have that:

$$\bar{c} + c_m + \eta^{-1}\frac{(1-\mu_{m,b_0})}{N-1} = \bar{c}(1-\chi) + c_m + \eta^{-1}\mu_{m,b_0} \Rightarrow \mu_{m,b_0} = \eta\bar{c}\chi\frac{N-1}{N} + \frac{1}{N}$$

For the banks not involved in the merger, their market share will be given by $\mu_{m,b} = \frac{(1-\mu_{m,b_0})}{N-1}$, from where we can compute the new level of spreads and concentration in the market after the efficiency gains.

$$\Delta s_m = -\bar{c}\chi\mu_{b,m_0} + \eta^{-1}\left[\mu_{m,b_0}^2 + (N-1)(1-\mu_{m,b_0})^2 - \frac{1}{N}\right]$$

The second case (right panel of Figure 7) is where all banks have their costs reduced by χ, so the change in spreads is given by

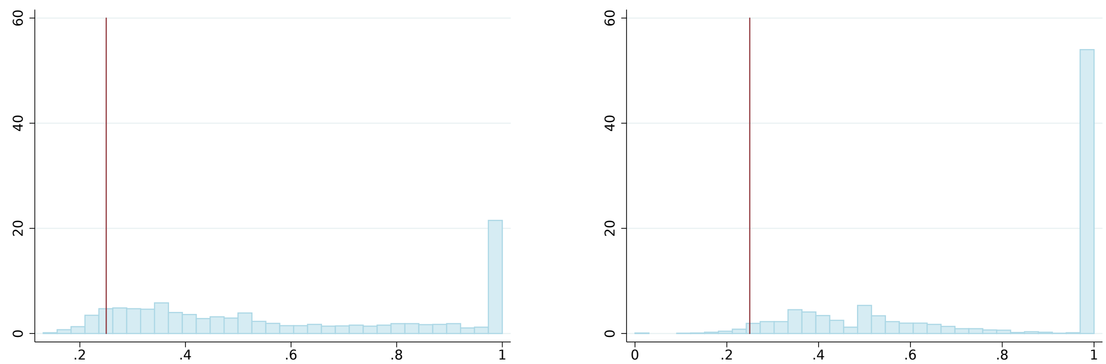
$$\Delta s_m = -\chi\bar{c}$$

For municipalities with both banks involved in the merger (both A and B), we compute the change in spreads as the sum of the efficiency gains described in the previous paragraph with the change in concentration of moving from HHI_m^{Bench} to HHI_m (the observed one) with the equation in Lemma 1. We aggregate the results across municipalities as previously descried in this section.

H. Additional Figures and Tables

Figure H.1: Market Concentration (HHI) in Dec/2010

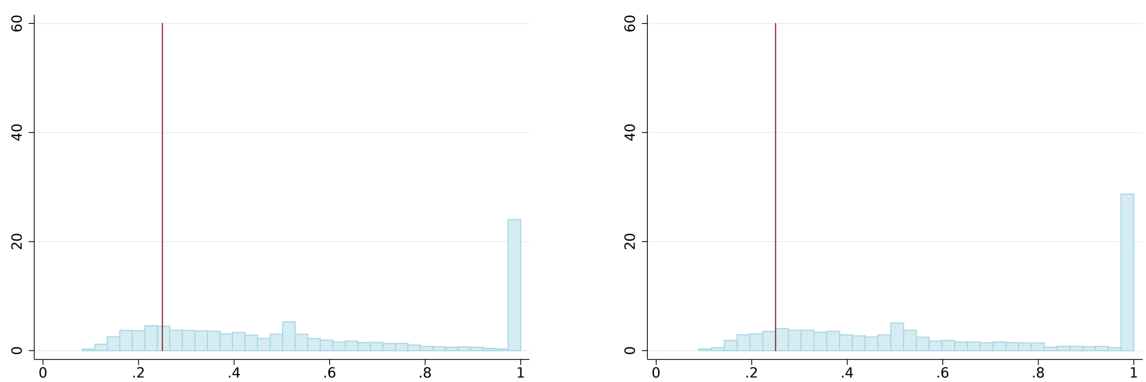
(a) HHI (Total) (b) HHI (Private)



Note: HHI index distribution for Brazilian municipalities in Dec/2010. Data comes from ESTBAN and the level of credit is computed through the municipality-month balance sheet of banks. HHI of private banks computed by excluding public banks for each municipality. See Appendix C for details. The vertical red line indicates .25, which is the DOJ threshold to define very concentrated markets.

Figure H.2: Market Concentration (HHI) in Dec/2010 (SCR)

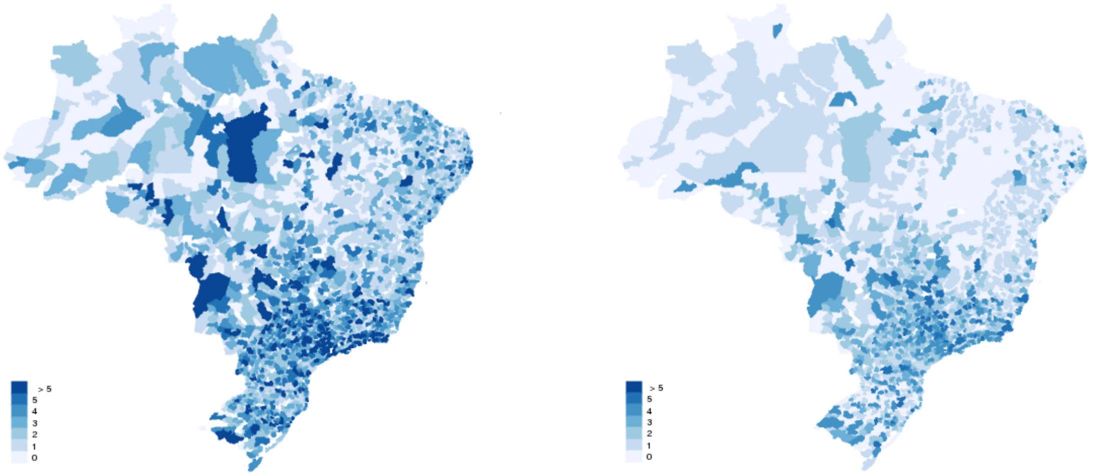
(a) HHI (Total) (b) HHI (Private)



Note: HHI index distribution on new credit for Brazilian municipalities in Dec/2010. Data from the credit registry (SCR). HHI of private banks computed by excluding public banks for each municipality. See Appendix C for details. The vertical red line indicates .25, which is the DOJ threshold to define very concentrated markets (if above).

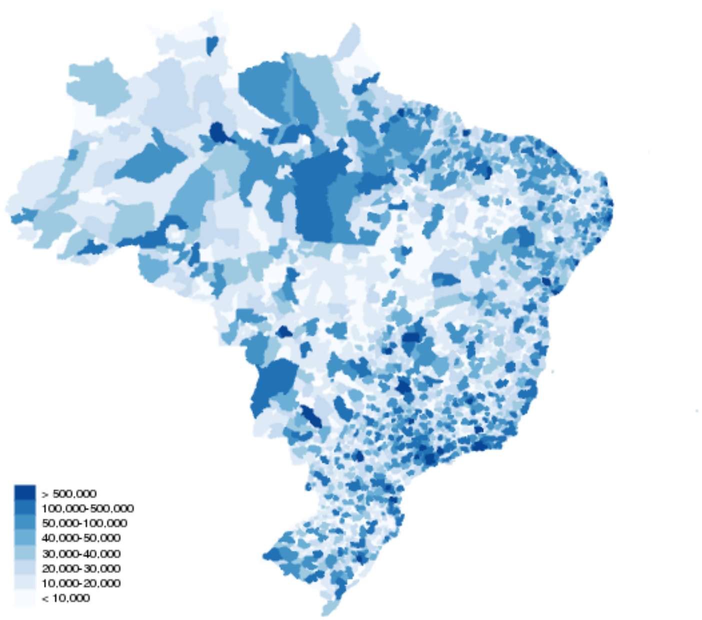
Figure H.3: Geographic Presence of Banks in Dec/2010

(a) # Banks (Total) (b) # Banks (Private)



Note: Bank refers to a banking conglomerate (and not on the number of branches). Data of physical presence for each bank comes from ESTBAN, while bank ownership comes from the Unicad Dataset.

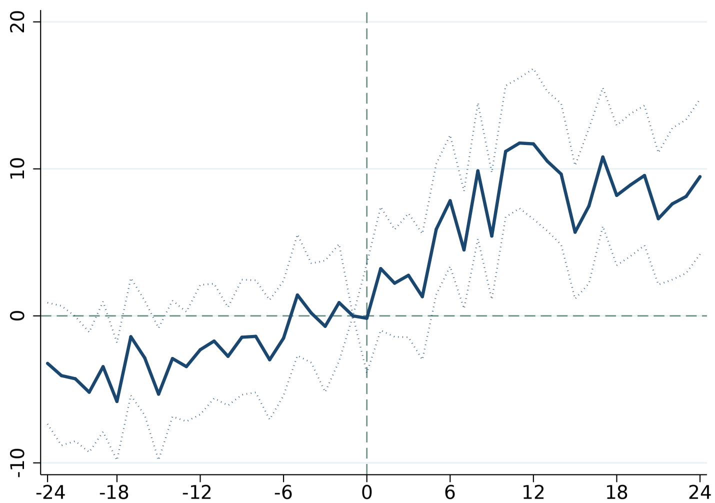
Figure H.4: Population in 2010



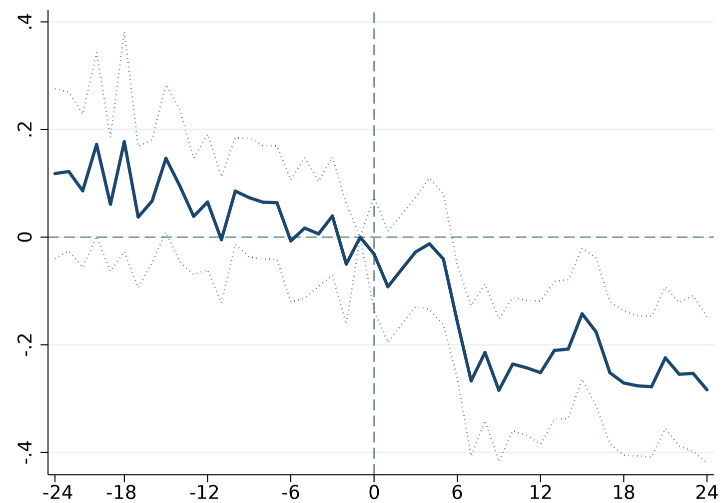
Note: Population by Municipality in Brazil for 2010. Data Source: IBGE.

Figure H.5: Effect of M&A Episode on Lending Spreads and Credit Volume: 24 month window

(a) $Spread^{Pr}$



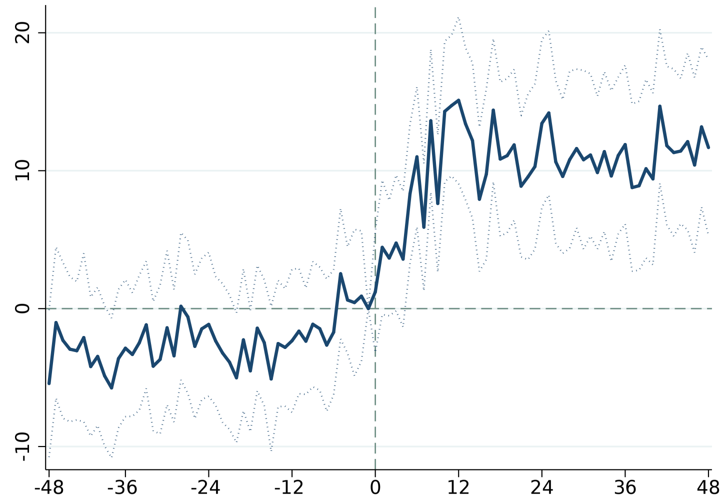
(b) $\ln\left(Credit^{Pr}\right)$



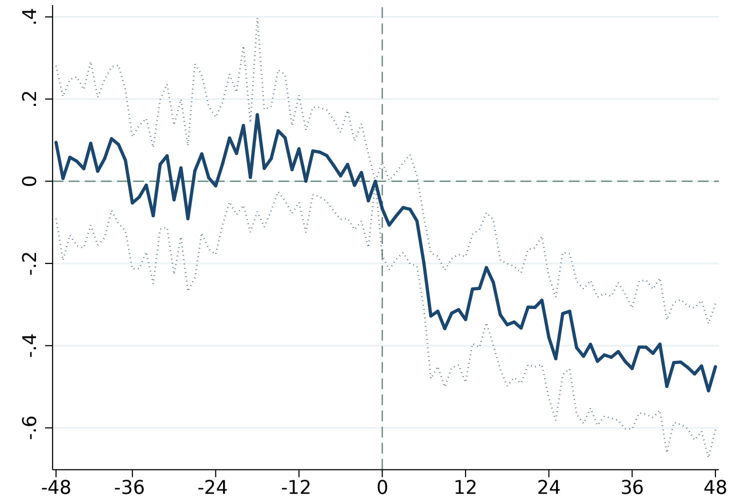
Note: Coefficients δ_τ from Eq.(1), estimated at the monthly-municipality level. Regression outcomes is the lending spreads (local interest rates minus country level deposit rate) on left panel, $Spread^{Pr}$, and log of new credit on right panel, $\ln\left(Credit^{Pr}\right)$, both computed only for loans made by private banks. For each municipality, interest rates are aggregated using loan size as weights and loan volume corresponds to the sum of value of all loans, computed through the SCR credit registry (see Section II) from 2005-2015. Standard errors computed clustering by municipality (treatment unity). Dotted lines represent show 99% confidence intervals. We normalize $\delta_{-1} = 0$. Exposed (Treatment) municipalities are those that had at least one branch of both banks at the time of the M&A episode. Vertical lines represent the time of the final approval of the M&A episodes. Sample of municipalities is those that had at least one and not more than 20 private banks in Dec/2005 and all periods in the 24 month window for a given M&A.

Figure H.6: Effect of M&A Episode on Lending Spreads and Credit Volume: 48 month window

(a) $Spread^{Pr}$



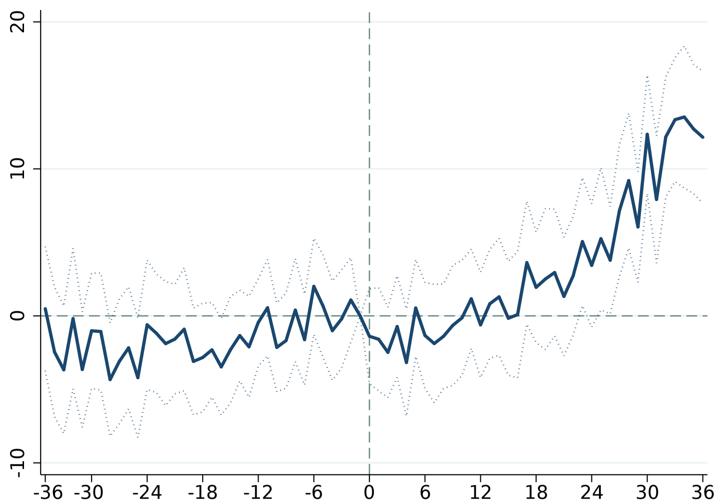
(b) $\ln\left(Credit^{Pr}\right)$



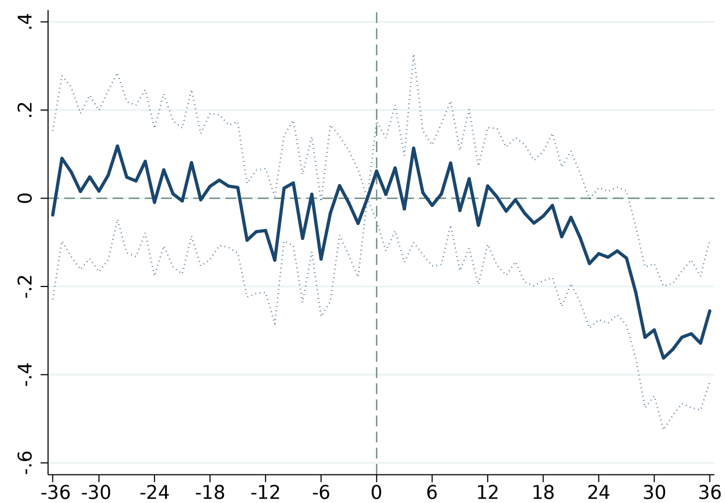
Note: Coefficients δ_τ from Eq.(1), estimated at the monthly-municipality level. Regression outcomes are the lending spreads (local interest rates minus country level deposit rate) on left panel, $Spread^{Pr}$, and log of new credit on right panel, $\ln\left(Credit^{Pr}\right)$, both computed only for loans made by private banks. For each municipality, interest rates are aggregated using loan size as weights and loan volume corresponds to the sum of value of all loans, computed through the SCR credit registry (see Section II) from 2005-2015. Standard errors computed clustering by municipality (treatment unity). Dotted lines represent show 99% confidence intervals. We normalize $\delta_{-1} = 0$. Exposed (Treatment) municipalities are those that had at least one branch of both banks at the time of the M&A episode. Vertical lines represent the time of the final approval of the M&A episodes. Sample of municipalities is those that had at least one and not more than 20 private banks in Dec/2005 and all periods in the 48 month window for a given M&A. Credit regressions are weighted by population in 2005 and spreads are weighted by credit volume from public banks in ESTBAN in 2005.

Figure H.7: Effect of M&A Episode on Lending Spreads and Credit Volume: 36 month window from identifier change

(a) $Spread^{Pr}$

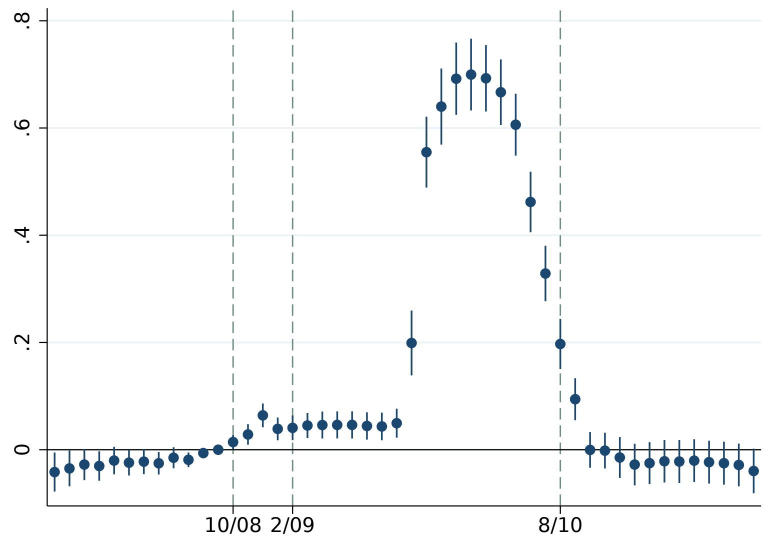


(b) $\ln\left(Credit^{Pr}\right)$



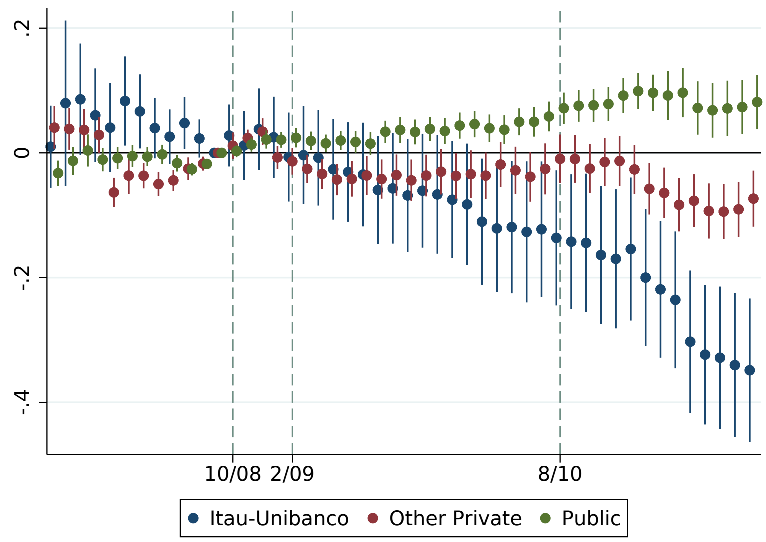
Note: Coefficients δ_τ from Eq.(1), estimated at the monthly-municipality level. Regression outcomes are the lending spreads (local interest rates minus country level deposit rate) on left panel, $Spread^{Pr}$, and log of new credit on right panel, $\ln\left(Credit^{Pr}\right)$, both computed only for loans made by private banks. For each municipality, interest rates are aggregated using loan size as weights and loan volume corresponds to the sum of value of all loans, computed through the SCR credit registry (see Section II) from 2005-2015. Standard errors computed clustering by municipality (treatment unity). Dotted lines represent show 99% confidence intervals. We normalize $\delta_{-1} = 0$. Exposed (Treatment) municipalities are those that had at least one branch of both banks at the time of the M&A episode. Contrary to Figure 5, where vertical lines represent the time of the final approval of the M&A episodes, the vertical line here represents the date we observe the change in conglomerate ID in the Unicad dataset. Sample of municipalities is those that had at least one and not more than 20 private banks in Dec/2005 and all periods in the 24 month window for a given M&A. Credit regressions are weighted by population in 2005 and spreads are weighted by credit volume from public banks in ESTBAN in 2005.

Figure H.8: Itaú-Unibanco Branches



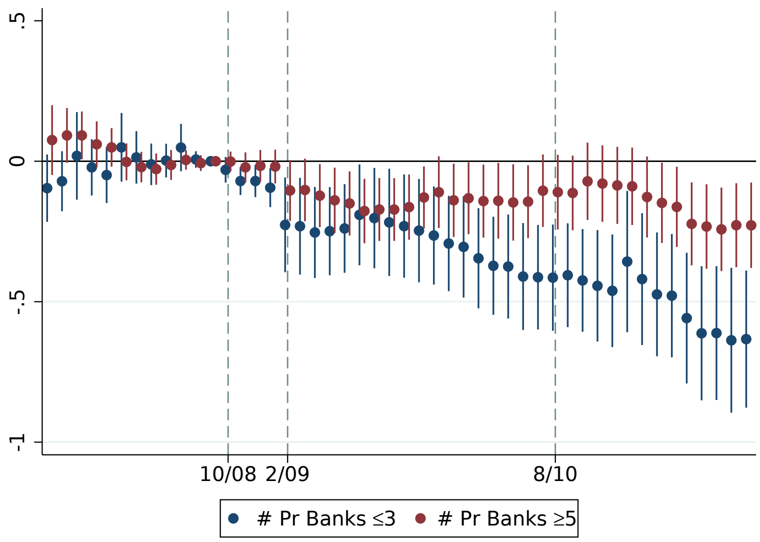
Note: Coefficients δ_τ from Eq.(25) with Itaú-Unibanco Branches (per 100,000 inhabitants) as an outcome, estimated at the monthly-municipality level. Standard errors computed clustering by municipality (treatment unity). Bars show 90% confidence intervals. Exposed municipalities are those that had at least one branch of both Itaú and Unibanco at Oct/2008, when merger is announced. All of the data for the case study comes from the publicly available ESTBAN dataset (see Section II). The vertical lines represent the following dates in the merger: Oct/2008 is the date the merger is announced, Feb/09 is when the BCB approves, and Aug/2010 is the CADE approval (final approval). Regression weighted by population in 2005.

Figure H.9: Total Credit: Itaú-Unibanco and Other Banks



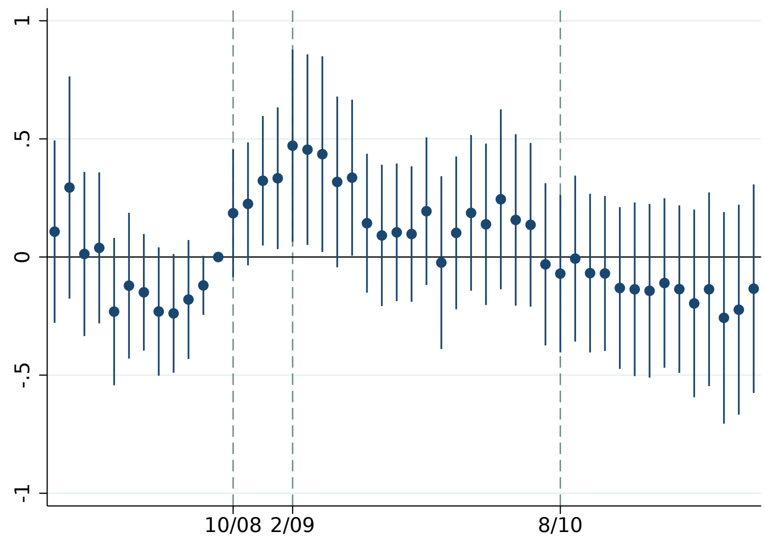
Note: Coefficients δ_τ from Eq.(25) with Itaú-Unibanco, Other private Banks, and Public Banks log of credit stock as an outcome, estimated at the monthly-municipality level. Standard errors computed clustering by municipality (treatment unity). Bars show 90% confidence intervals. Exposed municipalities are those that had at least one branch of both Itaú and Unibanco at Oct/2008, when merger is announced. All of the data for the case study comes from the publicly available ESTBAN dataset (see Section II). The vertical lines represent the following dates in the merger: Oct/2008 is the date the merger is announced, Feb/09 is when the BCB approves, and Aug/2010 is the CADE approval (final approval). Regressions are weighted by population in 2005. Regressions weighted by population in 2005.

Figure H.10: Credit from Itaú-Unibanco and Number of Banks in the Baseline



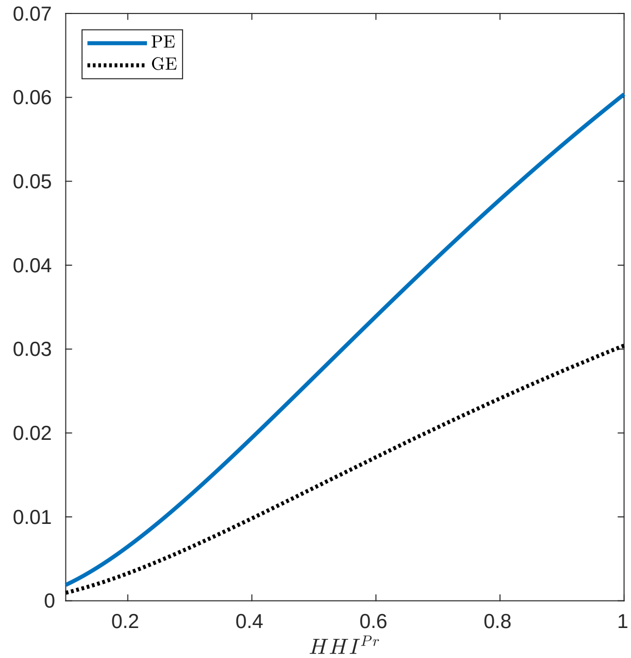
Note: Coefficients δ_τ from Eq.(25) with Itaú-Unibanco log of credit stock as an outcome, estimated at the monthly-municipality level for two different subsamples: (i) municipalities with at least 5 private banking conglomerates in the Oct/2008 and (ii) those with no more than 3. Standard errors computed clustering by municipality (treatment unity). Bars show 90% confidence intervals. Exposed municipalities are those that had at least one branch of both Itaú and Unibanco at Oct/2008, when merger is announced. All of the data for the case study comes from the publicly available ESTBAN dataset (see Section II). The vertical lines represent the following dates in the merger: Oct/2008 is the date the merger is announced, Feb/09 is when the BCB approves, and Aug/2010 is the CADE approval (final approval). Regression weighted by population in 2005.

Figure H.11: Total Credit: Differential Effect of Itaú-Unibanco Market Share in the Baseline



Note: Coefficients δ_τ^μ from Eq.(26) with log of credit stock from private banks as an outcome, estimated at the monthly-municipality level for two different subsamples: (i) municipalities with at least 5 private banking conglomerates in the Oct/2008 and (ii) those with no more than 3. Standard errors computed clustering by municipality (treatment unity). Bars show 90% confidence intervals. Exposed municipalities are those that had at least one branch of both Itaú and Unibanco at Oct/2008, when merger is announced. All of the data for the case study comes from the publicly available ESTBAN dataset (see Section II). The vertical lines represent the following dates in the merger: Oct/2008 is the date the merger is announced, Feb/09 is when the BCB approves, and Aug/2010 is the CADE approval (final approval). Regressions weighted by population in 2005.

Figure H.12: Model Implied Effect of Output of One extra Bank



Note: Effect of one extra bank in output of a municipality with HHI^{Pr} in the horizontal axis before the entry. For model details, see Section V. For details on the model, see Section V. For details on the numerical solution, calibration and specifics of each counterfactual see Appendix G.

Table H.1: M&A Episodes from Conglomerate Identifiers

Target	Buyer	Date Unicad	Approval(s)
BBVA BRASIL	BRADESCO	05/2003	05/2003
BANCO DO MARANHAO	BRADESCO	01/2004	02/2004
BANCO DO CEARA	BRADESCO	12/2005	12/2005
INTER AMEX	BRADESCO	05/2006	05/2006
BANKBOSTON	ITAU	08/2006	08/2006
UBS	UBS PACTUAL	11/2006	11/2006
BMC	BRADESCO	08/2007	08/2007
ABN AMRO REAL	SANTANDER	07/2008	07/2008
BANCO DE SANTA CATARINA	BANCO DO BRASIL	08/2008	02/2009
UNIBANCO	ITAU	10/2008	08/2010
BONSUCESSO	SANTANDER	01/2015	01/2015
HSBC	BRADESCO	06/2016	06/2016

Note: M& A episodes in our sample constructed directly through bank conglomerate changes in Unicad Dataset. We define an M&A episode as a situation where (i) one bank has changed conglomerates and has more than BRL 10 bn in assets, (ii) the conglomerate of this bank exits the market. The date of the episode is the date the bank changes conglomerates in Unicad. We suppose that the bank conglomerate that changed its code is the target, while the one that kept their code is the acquirer. Approvals date are dates where both the BCB and CADE have approved the merge.

Table H.2: M&A Episodes: Number of Banks and Concentration

	Months since M&A			
	12	24	36	48
	(1)	(2)	(3)	(4)
# Banks	-1.3127**	-1.364**	-1.4075**	-1.4684**
	(.0449)	(.0503)	(.0528)	(.0552)
# Private Banks	-1.2014**	-1.2068**	-1.2224**	-1.2558**
	(.0425)	(.047)	(.0493)	(.0515)
HHI (ESTBAN)	.0236**	.026**	.0315**	.0387**
	(.0028)	(.0038)	(.0041)	(.0043)
HHI^{Pr} (ESTBAN)	.1065**	.115**	.1189*	.1229**
	(.0054)	(.0057)	(.0058)	(.0058)
HHI^{Pr} (SCR)	.0549**	.0722**	.0836**	.1028**
	(.0058)	(.0059)	(.0058)	(.0061)
Month × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	238,286	236,511	232,269	229,122

Note: $**$, $*$, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). Standard errors computed clustering by municipality (treatment unity). The table shows the coefficient δ_{POST} from Eq.(2) estimated at the monthly-municipality level using 2005-2015 data from 18 months before and 12, 24, 36 and 48 months after the M&A episode (as measured with changes in bank identifiers in Unicad dataset), as described in Sections II and III. Regression outcomes with sources in parenthesis are, in order, number of banking conglomerates (ESTBAN), number of private banking conglomerates (ESTBAN), HHI of stock of credit (ESTBAN), HHI of of stock of credit from private banks (ESTBAN), and HHI of private credit of new loans (SCR). Treatment municipalities are those that had at least one branch of both banks at the time of the M&A episode. The controls used are GDP and credit per capita in 2005 interacted with time dummies and the local exposure to the business cycle, computed as the the covariance of local growth rate with country level growth rate over 2002-2018. All regressions include time-region (mesoregion IBGE concept) and municipality fixed effects.

Table H.3: Financial Outcomes: Lending Spreads and Total Credit by Loan Size

	> 5,000 BRL		> 1,000 BRL	
	24 mo (1)	36 mo (2)	24 mo (3)	36 mo (4)
$\ln\left(Credit^{Pr}\right)$	-.108**	-.1479*	-.1149**	-.1671**
	(.0173)	(.0204)	(.0177)	(.021)
$Spread^{Pr}$	4.5551**	5.3421**	4.7654**	5.8232**
	(.4973)	(.5825)	(.4993)	(.5752)
$\ln(Credit)$	-.0494**	-.0863**	-.0546**	-.0998**
	(.014)	(.0163)	(.0143)	(.0167)
$Spread$	3.3283**	4.117**	3.374**	4.2316**
	(.3894)	(.4435)	(.3866)	(.4341)
Controls	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	236,379	232,137	236,507	232,265

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). For details on outcome variables, treatment/control definition, fixed effects and regression weights see notes on Table 2. This table replicates the results in Table 2 for subsamples of loans above 1,000 BRL and 5,000 BRL, using data from 18 months pre M&A episode and 24 and 36 months afterwards, as indicated in the column headers.

Table H.4: Financial Outcomes: Markets with at least 8 years of Private Loans

	Months post M&A Episode			
	12 mo (1)	24 mo (2)	36 mo (3)	48 mo (4)
$\ln\left(Credit^{Pr}\right)$	-.0666**	-.1127**	-.1643**	-.2062**
	(.0134)	(.0176)	(.0209)	(.0237)
$Spread^{Pr}$	2.613**	4.7584**	5.8301**	6.9224**
	(.4025)	(.4954)	(.5683)	(.6236)
$\ln(Credit)$	-.0193+	-.0533**	-.0971**	-.1369**
	(.0113)	(.0141)	(.0164)	(.0186)
$Spread$	1.174**	3.358**	4.1977**	5.0955**
	(.3087)	(.384)	(.4298)	(.4654)
Controls	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	222,644	220,877	216,643	213,500

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). For details on outcome variables, treatment/control definition, fixed effects and regression weights see notes on Table 2. This table replicates the results in Table 2 for a subsample of municipalities (both treatment and control) with at least 8 years of data on private loans in SCR from 2005-2015.

Table H.5: Financial Outcomes: Markets with at Most One M&A Episode

	Months post M&A Episode			
	12 mo	24 mo	36 mo	48 mo
	(1)	(2)	(3)	(4)
$\ln\left(Credit^{Pr}\right)$	-.1715**	-.23**	-.274**	-.3013**
	(.0284)	(.0367)	(.0406)	(.0429)
$Spread^{Pr}$	8.7671**	11.0613**	11.1818**	11.9005**
	(1.1127)	(1.1524)	(1.1979)	(1.1877)
$\ln(Credit)$	-.1137**	-.1771**	-.2159**	-.2451**
	(.0232)	(.0298)	(.0328)	(.0346)
$Spread$	5.8555**	8.919**	8.8654**	9.3412**
	(.9391)	(.9767)	(1.0467)	(1.0325)
Controls	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	216,896	215,832	213,298	211,016

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). For details on outcome variables, treatment/control definition, fixed effects and regression weights see notes on Table 2. This table replicates the results in Table 2 for a subsample of municipalities (both treatment and control) with at most one M&A episode from 2005-2015.

Table H.6: Treatment vs Control Characteristics: Markets with 2-6 Private Banks in Dec/2005

	Control (1)	Difference (2)
Credit/Pop.	2722.68	233.58**
		(35.10)
GDP/Pop.	18.63	.8867**
		(.2597)
Spread	29.33	1.7822**
		(.5845)
# Banks	5.31	.1752**
		(.0218)
# Private Banks	3.13	.0984**
		(.0192)

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. We estimate $f_{m,r} = \alpha + \sigma_r + \beta Treatment_{m,r} + \epsilon_{m,r}$ where $f_{m,r}$ is a pre-merger characteristics of municipality m, in region r, $Treatment_{m,r}$ is a dummy equal to one if municipality m is exposed and σ_r are region fixed effects. We show the results of α and β in Column 1 and 2, respectively. Outcomes variables with their sources in parenthesis are, in order, credit stock per capita (ESTBAN) in 2010 BRL price level, GDP per capita (IBGE) in 1,000s of 2010 BRL price level, volume weighted average lending spreads (SCR), number of banking conglomerates (ESTBAN), and number of private banking conglomerates (ESTBAN).

Table H.7: Financial Outcomes: Lending Spreads and Total Credit for Markets with 2-6 Private Banks in Dec/2005

	Months post M&A Episode			
	12 mo (1)	24 mo (2)	36 mo (3)	48 mo (4)
$\ln\left(Credit^{Pr}\right)$	-.0833**	-.1156**	-.1418**	-.169**
	(.0185)	(.0219)	(.0259)	(.029)
$Spread^{Pr}$	4.7349**	6.8000**	7.2765**	8.1877**
	(.6492)	(.7432)	(.8415)	(.8866)
$\ln(Credit)$	-.0531**	-.0848**	-.1118**	-.1393**
	(.0164)	(.0191)	(.0223)	(.0249)
$Spread$	3.2797**	5.6537**	6.0311**	6.7859**
	(.5203)	(.6004)	(.6694)	(.6959)
Controls	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	122,260	121,001	117,974	115,655

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). For details on outcome variables, treatment/control definition, fixed effects and regression weights see notes on Table 2. This table replicates the results in Table 2 for a subsample of municipalities (both treatment and control) with 2-6 Banks in Dec/2005. For key characteristics in control and treatment groups in this subsample, see Table H.6.

Table H.8: Spreads: Volume Weighted vs Maturity-Volume Weighted

	Volume Weighted		Volume-Maturity Weighted	
	Coefficient	% Mean	Coefficient	% Mean
Spread	4.20**	11.71%	2.09**	11.40%
	(.4289)		(.2793)	
Controls	Y		Y	
Month × Region FE	Y		Y	
Municipality FE	Y		Y	
Obs	232,269		232,269	

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). For details on treatment/control definition, fixed effects and regression weights see notes on Table 2. The table shows the coefficient δ_{POST} from Eq.(2) estimated at the monthly-municipality level using 2005-2015 data from 18 months before and 36 months after the M&A episode, as described in Sections II and III. Regression outcomes are and lending spreads (local interest rates minus country level deposit rate), $Spread$, from new loans in a municipality m, in region r and time t. For each municipality, we aggregate interest rates in two ways. First, as in Table 2, interest rates are aggregated using loan size as weights and loan volume corresponds to the sum of value of all loans. Second, interest rates are aggregated using loan size times maturity as weights, as to not potentially over-weight short term loans.

Table H.9: Financial Outcomes: Lending Spreads and Total Credit (All Municipalities)

	Months post M&A Episode			
	12 mo	24 mo	36 mo	48 mo
	(1)	(2)	(3)	(4)
$\ln\left(Credit^{Pr}\right)$	-.0451**	-.0744**	-.1103**	-.1426**
	(.0103)	(.0144)	(.0183)	(.0205)
$Spread^{Pr}$	1.8556**	3.535**	4.394**	5.266**
	(.3307)	(.434)	(.477)	(.5224)
$\ln(Credit)$	-.013**	-.0318**	-.0614**	-.0939**
	(.0078)	(.0107)	(.0137)	(.0156)
$Spread$	.7845**	2.4133**	3.0655**	3.7826**
	(.2561)	(.3398)	(.3669)	(.4047)
Controls	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	239,203	237,408	233,158	230,011

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. For details on outcome variables, treatment/control definition, fixed effects and regression weights see notes on Table 2. We estimate the effect on our benchmark sample (Table 2) without the municipalities with more than 20 private banking conglomerates in 2005 and all municipalities. In this table, we replicate the results with all municipalities.

Table H.10: Financial Outcomes: Microregions as Markets

	Months post M&A Episode			
	12 mo	24 mo	36 mo	48 mo
	(1)	(2)	(3)	(4)
$\ln\left(Credit^{Pr}\right)$	-.0519^{+}	-.0789*	-.1016**	-.1279**
	(.027)	(.0324)	(.0384)	(.0429)
$Spread^{Pr}$	2.2986**	3.4977**	3.7373**	4.299**
	(.5048)	(.6505)	(.7471)	(.8213)
$\ln(Credit)$	.006	-.0225	-.0568^{+}	-.0877*
	(.022)	(.0261)	(.0304)	(.0342)
$Spread$	1.2739**	2.6827**	2.8188**	3.2643**
	(.3808)	(.4951)	(.554)	(.6017)
Controls	Y	Y	Y	Y
Month × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	56,543	55,636	53,814	52,740

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. For details on outcome variables, fixed effects and regression weights see notes on Table 2. The difference from our benchmark results is that we consider the IBGE microregion concept (larger than a municipality) as a market. A microregion is exposed if it has at least one branch of both banks involved in an M&A episode, and non-exposed otherwise.

Table H.11: Effect on Output by Municipality 3 years after M&A episode

	Agriculture	Industry	Services	Total
	.0012	-.0775**	-.0158*	-.0217**
	(.013)	(.0179)	(.008)	(.0082)
Controls	Y	Y	Y	Y
Year × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Obs	20,684	20,684	20,684	20,684

Note: **, *, + indicate significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). This table displays δ_{POST} from Eq.(2) with annual output by sector in each municipality from IBGE. The industry sector includes both industry and construction output. We assume that a municipality is exposed to an M&A in a given year if it is exposed in a month up to June.

Table H.12: Employment and Wages: only one merging bank versus none (36mo window)

	Agriculture (1)	Tradable (2)	Non-Tradable (3)	Construction (4)
Employment	0.000323	-0.00336	-0.00514^{+}	-0.00191
	(0.0107)	(0.00684)	(0.00300)	(0.0144)
Wages	0.00405	0.00515^{+}	-0.00286*	-0.00121
	(0.00350)	(0.00296)	(0.00122)	(0.00506)
Controls	Y	Y	Y	Y
Year × Region FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Observations	173,261	173,261	173,261	173,261

Note: $**$, $*$, $+$ indicate significance significance at, respectively, 1%, 5% and 10%. Standard errors computed clustering by municipality (treatment unity). The table shows the treatment effect on the treated, that is, the coefficient δ_{POST} from Eq.(2), estimated at the monthly-municipality level using 2005-2015 data from 18 months before and 36 months after the M&A episode. Treatment municipalities are those that had at least one branch of only one of the involved banks in the M&A episode and control are those that had none. Regression outcomes are total employment and average annual wage of all workers employed at a given month-sector computed from RAIS. Sectors are defined as in Mian and Sufi (2014). For details on controls and treatment/control definition, and fixed effects see notes of Table 2. We use population in 2005 as weights in the regression.