

The Real Effects of Government-Owned Banks: Evidence from an Emerging Market

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ABSTRACT

Using plant-level data for Brazilian manufacturing firms, this paper provides evidence that government control over banks leads to significant political influence over the real decisions of firms. I find that firms eligible for government bank lending expand employment in politically attractive regions near elections. These expansions are associated with additional (favorable) borrowing from government banks. Further, these persistent expansions take place just before competitive elections, and are associated with lower future employment growth by firms in other regions. The analysis suggests that politicians in Brazil use bank lending to shift employment towards politically attractive regions and away from unattractive regions.

GOVERNMENT OWNERSHIP OF banks is widespread around the world (La Porta, Lopez-de-Silanes, and Shleifer (2002)). Given the role of banks as a major source of external finance for firms, it is important to understand the consequences of government control over their lending decisions. Recent work provides support for the idea that political considerations influence the lending decisions of government banks (Sapienza (2004), Dinc (2005), Khwaja and Mian (2006), Claessens, Feijen, and Laeven (2008), and Cole (2009)), but the literature has little to say about the real consequences of these considerations.

This paper investigates lending by government banks and firms' employment decisions, and provides support for the view that politicians use lending by government banks to influence the real behavior of firms. While politicians can obtain private benefits from changes in firms' decisions, such as increasing patronage or electoral support, politicians are often constrained in their ability to exchange favors with firms. By providing politicians with a mechanism to implement such exchange, government control over bank lending can lead to

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greater political influence over decisions in the real economy (Shleifer and Vishny (1994, 1998)).

To study the above issue I employ a sample of Brazilian manufacturing plants of more than 13,000 firms between 1995 and 2005, which represents the universe of Brazilian manufacturing firms with at least 50 employees. Motivated by the fact that government bank lending is controlled by the central government, which is often believed to exert effort to reelect local governments that are politically allied, I show that firms eligible for government bank lending expand employment in regions with allied incumbents near reelection years. These effects are only important for incumbents facing strong competition from the opposition. Moreover, these expansions do not simply change the timing of hiring decisions, but rather represent persistent and economically important expansions to firms' local employment.

Consistent with the interpretation that firms expand in exchange for government bank loans, I find no effects for firms that are ineligible for government bank lending. Also consistent with this interpretation, I find that these real expansions are associated with greater borrowing from government banks. This borrowing comes in the form of loans tied to projects that take place in the same location and period as the firms' expansion. Finally, I find that these persistent local expansions do not lead to persistent expansions in the firms' total employment. Multilocation firms expand in regions with allied incumbents and compensate by growing less in other regions after elections.

The results suggest that politicians use bank lending to shift employment toward politically attractive regions and away from politically unattractive regions. According to this interpretation, politicians provide loans with better terms than private bank loans to firms in exchange for these employment shifts. I test and find support for several additional predictions that follow from this interpretation. The estimated number of jobs shifted is economically important and represents between 18% and 36% of the total number of jobs created by firms in the sample.

There are several advantages to investigating this issue in the context of Brazilian manufacturing. First, only firms in sectors targeted by government bank loans (priority sectors) rely on these loans as a major source of external finance. Constraints on the size of projects financed also imply that government loans are most important for large firms. This provides a benchmark for the behavior of firms in the absence of government lending. Another advantage of this research setting is the existence of detailed plant-level data that track the decisions of firms across different regions.

Additionally, reelection races by allied incumbents provide clear measures of regions' political attractiveness. In my main results, I compare the local employment of firms in regions with allied incumbents around reelection races to the local employment of firms at that time in regions without allied incumbents. I exploit the fact that being elected with a smaller margin of votes is a strong predictor of the competition faced by incumbents at reelection to construct a sample of incumbents facing strong competition from the opposition.

My identification strategy relies on both the timing of the effects and differences across firms and regions. I find economically or statistically important expansion only for priority sector firms that are sufficiently large to rely on government bank loans, and for reelection races of incumbents elected after a close election. I also find that most of the local employment expansion takes place just before those races. Finally, I provide evidence that politicians manage to influence elections with bank lending.

I explain the challenges in reconciling this collection of evidence with alternative interpretations of my results, such as the existence of unobserved economic shocks, the use of other instruments by politicians to influence firms' decisions, social lending by government banks, or unintended real effects by politicians. I provide direct evidence against predictions from these alternative interpretations.

This paper relates to a growing literature on the political economy of finance. The first contribution of this paper is to provide evidence that government control over banks leads to an economically significant influence of political considerations over firms' real decisions. This suggests that political influence over bank lending does not simply lead to a transfer of money or to changes in the timing of firms' decisions. These considerations seem to systematically influence the way financial markets allocate resources. The second contribution of this paper is to provide evidence on the specific mechanism through which this happens: politicians use control over bank lending decisions to influence the decisions of firms and increase employment in politically attractive regions. This evidence has implications for both the causes and consequences of financial market regulation. Previous work on the political economy of financial regulation emphasizes the importance of conflicts between private and public interests in the choice of regulation (Kroszner and Strahan (1999), Rajan and Zingales (2003, 2004)). One explanation for this gap is the idea that large incumbent firms oppose financial development to avoid competition (Braun and Raddatz (2008), Benmelech and Moskowitz (2010)).¹ The analysis in this paper provides support for an additional explanation. Politicians control financial institutions because doing so provides them with greater ability to politically influence the choice of projects being implemented in the economy. This explanation helps rationalize why government participation in financial markets takes the specific form of government ownership of financial institutions.

The idea that politicians value control over firms' real decisions and use ownership choices to achieve this goal is supported by recent work on the decision to privatize nonfinancial firms (Dinc and Gupta (2011)). The current paper adds to this evidence by analyzing how the ownership of banks allows politicians to influence firms' decisions. More broadly, this paper also complements previous work linking politics to the real decisions of firms. Previous research (Julio and Yook (2012)) provides evidence that political uncertainty influences firms'

¹ See Pagano and Volpin (2005), Mian, Trebbi, and Sufi (2010) and Rajan and Ramcharan (2011) for additional evidence on the political economy of financial regulation.

investment decisions. This paper provides support for an alternative and direct channel linking the political process to firms' real decisions.

A related study by Bertrand et al. (2007) provides evidence that politically connected CEOs in France exchange favors with politicians by creating more jobs in politically competitive regions near elections. They find limited evidence that politicians return favors through policies that benefit firms, which is consistent with the view that this behavior reflects personal exchanges between politicians and connected CEOs. In contrast, the current paper analyzes how politicians use bank lending to influence the real decisions of firms. The focus of the paper is on the consequences and causes of government control over lending, and how political factors affect the way financial markets allocate resources.

The rest of this paper is organized as follows. Section I presents the conceptual framework. Section II provides institutional background on bank lending and politics in Brazil. Section III describes the data and presents summary statistics. Section IV presents the results. Section V addresses alternative explanations for the results. Finally, Section VI concludes.

I. Conceptual Framework

If politicians derive private benefits from changing firms' decisions, they may use government bank lending to influence the real decisions of private firms. I focus on increases in electoral support as an important source of private benefits for politicians. A large literature on business cycles emphasizes that incumbent politicians have incentives to change economic policies and influence both economic conditions and voters near elections (Nordhaus (1975), Alesina, Roubini, and Cohen (1997), and Drazen (2000)). While most of this literature focuses on cycles at the national level, recent evidence supports the view that local employment conditions influence local voters' support for incumbents (Wolfers (2002), Bertrand et al. (2007)). Theory suggests that politicians might target resources to regions with swing voters to win elections (Person and Tabellini (2002)). Therefore, incumbent politicians can benefit from improved employment conditions in regions with greater electoral returns near elections, which I refer to as politically attractive regions, and may use government bank lending to achieve this goal. I define politically attractive regions more precisely in Section II, based on the empirical setting used in this paper.

The above logic leads to the following two-way exchange: on the one hand, firms expand employment in politically attractive regions near elections; on the other hand, government banks provide these firms loans with better terms than loans from private commercial banks. In equilibrium, the better terms of these government bank loans compensate firms for the cost of changing their decisions. If politicians face commitment problems when making future promises to firms, government banks will provide loans to firms at the same time that firms expand.

If managers are maximizing shareholder value, this exchange will lead to a less efficient allocation of resources by firms. Managers are changing

value-maximizing decisions in response to a transfer.² The value of the transfer through government bank loans offsets the value of the efficiency loss and thus the exchange creates value for shareholders. However, at the aggregate level, one would internalize the two sides of the transfer. While these efficiency costs might be important, quantifying such costs is beyond the scope of this paper. The analysis in the paper is a first step in analyzing whether these considerations systematically affect the allocation of resources by firms. A full analysis of efficiency would require incorporating potential benefits from government bank lending. For example, cheaper credit could help the credit rating of large firms and allow them to raise funds from foreign investors. This analysis would also require incorporating equilibrium effects. For example, the changes in the allocation of resources by firms might help voters keep politicians accountable and might reduce other distortions.

Another important consideration is that, in principle, politicians could use other tools to increase employment in politically attractive regions. First, they could use their control over government-owned nonfinancial firms. As discussed by La Porta, Lopez-de-Silanes, and Shleifer (2002), owning banks gives politicians extensive control over the projects being financed while leaving the implementation of these projects to the private sector.³ Consistent with this view, Section II.A discusses how government bank lending continued to be important in Brazil after a period of substantial privatization of nonfinancial firms. Second, politicians could use alternative policy instruments to influence private firms. For example, they could use tax breaks. A potentially important advantage of bank loans comes from their greater ability to address commitment problems. Politicians might not be able to make credible promises to provide tax benefits for a given number of years in the future. However, they might be able to commit to a transfer to firms by providing a loan today with a below-market interest rate. Additionally, politicians might prefer to use bank lending to influence firms if the costs associated with government loans are not transparent to voters (Coate and Morris (1995), Alesina, Baqir, and Easterly (2000)). While it is not clear on theoretical grounds if politicians use government banks to influence firms' real decisions, the main goal of this paper is to test this hypothesis.

II. Background: Bank Lending and Politics in Brazil

A. Government Bank Lending

Government bank credit was a major source of finance for manufacturing firms in Brazil over the sample period studied in this paper (1995 to 2006) and

² This will be the case unless firms are financially constrained and the government bank loans are allowing firms to reduce the cost of external finance. The analysis in this paper suggests that this reduction in the cost of external finance is not of first-order importance for the firms impacted by government bank loans. See Section IV.C.

³ See Frydman et al. (1999), La Porta and Lopez-de-Silanes (1999), Meggison and Netter (2001), and Gupta (2005) for evidence on the performance benefits of privatizing nonfinancial firms.

was allocated through the Brazilian National Development Bank (BNDES).⁴ This bank was created in the 1950s and, as part of a broader government program to stimulate longer-horizon investments in strategic industries, acquired significant importance as a source of external finance for private firms in the 1960s and 1970s. Even after a process of economic reform and liberalization in the early 1990s, where other components of this strategy lost importance through substantial reductions in trade barriers and extensive privatization, government bank lending continued to play a key role in the financing of manufacturing firms. Approximately 30% of all manufacturing investment in the country over the sample period was part of a project (partially) financed through these loans, which represented the largest source of (implicit) subsidies to manufacturing firms.⁵

The bank provided credit to firms through both direct loans and indirect loans, each comprising 50% of the bank's total lending. Direct loans were equivalent to loans by commercial banks, except for the fact that they were not financed through deposits. Firms applied for loans directly to the government bank, which evaluated their projects and credit risk on a one-by-one basis. The government bank negotiated loan terms directly with firms and had substantial discretion over these terms. Loans were typically associated with lower interest rates and better repayment terms than loans from privately owned banks. Loans were also tied to specific investment projects, which specified the location of investments. Only sufficiently large loans were financed this way.

Indirect loans were intermediated through private commercial banks. In these operations, firms applied to commercial banks for loans. The commercial banks screened borrowers, took all the credit risk, and negotiated loan terms with firms. Some of these loans were automatically approved by the government bank if they satisfied eligibility conditions. Other loans required analysis by government bank loan officers. Because of the large number of indirect loans, the government bank had less room to negotiate loan terms on a one-by-one basis and lending decisions were made in a more decentralized way inside the bank.⁶

I exploit two sources of variation across firms on the importance of government bank lending. First, I exploit the fact that government bank lending is historically concentrated in certain sectors (priority sectors) and that this concentration is persistent over time. Federal law required the federal government to list a group of sectors explicitly favored by government bank lending. I define

⁴ La Porta, Lopez-de-Silanes, and Shleifer (2002) find that an important share of the government ownership of banks around the world takes the form of ownership of development banks.

⁵ See Pinheiro and Oliveira Filho (2007) for a broader survey of the role of the government in Brazilian credit markets and *The Economist* (August 5, 2010) for a recent account of the Brazilian Development Bank.

⁶ The bank also provided equity financing for firms, becoming a minority shareholder. This creates an additional channel through which the bank can affect the decisions of firms. I would like to thank Campbell Harvey (the Editor) for emphasizing this point to me. In this paper, I focus on the lending channel. See the Internet Appendix, available in the online version of the article on the *Journal of Finance* website, for robustness on this issue.

priority sectors as sectors included on this list at the beginning of the sample period. I interpret this list as capturing sectors in which one should expect a lower cost of government bank lending. The allocation of government bank lending across the priority and nonpriority sectors during the sample period is described in Table I. The average amount of government bank credit going to firms in priority sectors (scaled by firm capital) is four times larger than the same amount for firms in nonpriority sectors. Government bank loans typically fund a large share of the investment made in the projects financed. These projects represent on average 21.5% of the capital of priority sector firms every three years. This contrasts with a ratio equal to 3.6% of the capital for firms in nonpriority sectors and suggests that government bank loans are economically important, mostly for firms in priority sectors.⁷

Second, I exploit the fact that government bank loans are mostly important for larger firms. This comes from the fact that only sufficiently large firms have access to direct loans. Larger firms have on average three times more loans (combining direct and indirect) from the government bank. These firms have long-term relationships with the bank, constantly financing their investment projects. On average, these firms have a new loan every 3.3 years. Most of their borrowing is through direct loans.

B. Politicians and Political Alignments

I exploit the fact that government bank lending to manufacturing is controlled by the central government to construct measures of regions' political attractiveness. Both the CEO of the bank and the bank's board of directors are nominated by the president and can be fired at his discretion. The lending decisions of the bank are commonly seen as reflecting strategies determined by the central government. During the sample period, Brazil had a democratic regime with a federal structure and a presidential multiparty political system, which included a president, national congress, governors, mayors, and assemblies at both the state and the municipality levels. The president and the 27 state governors were directly elected by voters every four years and all those elections took place at the same time. State governors (and a few mayors) controlled an important amount of resources and were influential figures. The central government was often seen as valuing the reelection of allied governors and engaged in both direct and visible forms of support during reelection campaigns.⁸

⁷ This list was compiled only for the purposes of government bank lending. There are at least two reasons to expect a lower cost of lending to historically favored sectors. First, there is specialization of the expertise of the bank and its loan officers in some sectors. Second, there are long-term relationships between the bank and firms. See the Internet Appendix for more evidence and a detailed discussion of the lending concentration and the choice of priority sectors.

⁸ These alignments are based on partisan affiliations. Previous evidence suggests the importance of these partisan affiliations for explaining intergovernmental transfers (Bugarin and Ferreira (2006)).

Table I
How Concentrated Is Government Bank Credit across Sectors?

This table presents the allocation of loans to private firms across sectors by the Brazilian National Development Bank (BNDES) during 1995 to 2006. All numbers are averages over the years in this period. For each year, *Total Govt. Bank Credit* is the total amount of funds disbursed to finance projects by large private firms in that sector. *Total Credit/Capital* is *Total Govt. Bank Credit* divided by the aggregate capital of large firms in that industry. Large firms are firms with average employment above 500 workers over the sample period (see Section III.B). Aggregate capital for these firms is not directly available and is computed as the product of their aggregate employment by the sector-wide capital to labor ratio. *Ratio of Total Employment* is the ratio of sector employment to total employment in (priority or nonpriority) sectors. *Average Total Credit/Capital* is the weighted average of *Total Credit/Capital* in priority and nonpriority sectors, where the weights are the ratios of total employment. Loans are typically tied to investment projects, and a typical government participation in project financing is 50%. *Average Govt. Bank Financed Investment/Capital* (per year) = $2 \times$ *Average Total Credit/Capital* (per year). The three-year period reported reflects a typical project. *Fraction of Investment Financed with Govt. Bank* is given by the ratio of *Average Govt. Bank Financed Investment/Capital* to the average *Total Investment/Capital* among priority and nonpriority sectors. *Average Total Credit/Total Debt* is *Total Govt. Bank Credit* divided by the total outstanding debt of large firms. Total debt for these firms is estimated based on the leverage ratios of firms registered with the Brazilian Security Exchange Commission (see the Internet Appendix for details).

Sector	Total Govt. Bank Credit (R\$1995 Millions/ Year)	Total Credit/Capital (%)	Ratio of Total Employment
Panel A: Nonpriority Sectors			
Tobacco	6.1	0.26	0.03
Medical and Precision Equipment	7.2	0.39	0.01
Informatics	18.8	1.88	0.01
Apparel and Apparel Accessories	25.3	0.22	0.06
Publishing and Printing	27.8	0.28	0.07
Miscellaneous	51.1	0.43	0.04
Footwear, Leather, and Hide Products	81.6	0.36	0.19
Petroleum Refining and Other Fuels	81.8	1.26	0.07
Wood Products	86.3	1.49	0.04
Electronic Equipment	97.0	2.11	0.05
Metal Products	111.0	1.11	0.06
Electric Equipments and Components	115.0	1.36	0.08
Mineral Products (nonmetallic)	136.0	1.40	0.07
Plastic Products	149.0	1.15	0.07
Textiles	180.0	0.99	0.18
Average Total Credit/Capital (per year) = 0.90%			
Average Govt. Bank Financed Investment/Capital (over 3 years) = 5.42%			
Fraction of Investment Financed with Govt. Bank = 14.93%			
Average Total Credit/Total Debt (per year) = 1.79%			
Panel B: Priority Sectors			
Chemical Products	318.0	2.15	0.10
Machinery and Equipment	408.0	2.67	0.09
Pulp and Paper Products	414.0	7.03	0.05
Metallurgy	580.0	4.40	0.10
Automobile, Truck, and Bus	816.0	3.37	0.18
Food and Beverage	952.0	1.32	0.48
Other Transportation Equipment	1,430.0	56.56	0.02
Average Total Credit/Capital (per year) = 3.58%			
Average Govt. Bank Financed Investment/Capital (over 3 years) = 21.47%			
Fraction of Investment Financed with Govt. Bank = 48.68%			
Average Total Credit/Total Debt (per year) = 6.27%			

I use the reelection races by incumbent governors allied to the central government to construct a measure of regions' political attractiveness. The idea that local employment expansions by private firms helped local incumbents is supported by the fact that local governments typically exerted considerable effort to promote these expansions. If the central government was biased towards helping the reelection of local allied incumbents, only allied incumbents facing some competition from the opposition should have been helped. I exploit this fact to refine the measure of regions' political attractiveness.

There is indeed anecdotal evidence consistent with this type of preferential treatment. In one instance, the nonallied governor of Sao Paulo (Geraldo Alckmin) protested in the press against the constant refusal of the Development Bank to finance projects in his state. The governor contrasted this refusal against the speed with which projects were approved in regions governed by the presidential party. In another instance, the governor of Rio de Janeiro (Rosinha Matheus) explicitly pressured President Lula to reward the electoral support given to his reelection campaign by her political group with a greater supply of government bank credit for projects in her state.⁹

III. Data and Summary Statistics

The empirical analysis in this paper combines different data sources on government bank credit, plants, firms, and political variables.

A. Government Bank Loan Data

I use three different sources of information on the loans made by BNDES during the period between 1995 and 2006. First, I use the aggregate volume of funds disbursed to privately owned firms in each year by a two-digit sector code, similar to the SIC code, and by the type of loan (direct loans to firms and indirect loans through commercial banks). Table I above is based on this information, which I use to check that government bank credit is strongly concentrated in priority sectors. Second, I use the aggregate volume of funds disbursed to finance projects by (privately owned) manufacturing firms in each state-year. This information is used to estimate the credit allocation results. Finally, I use summary statistics on individual loans for different groups of firms based on firm size (number of employees). I use this information to check that government bank loans are most important for large firms and confirm the actual size range over which this happens.

B. Plant and Firm Data

The main data source used for information on plants and firms in this paper is a registry of all formal workers in Brazil (RAIS), obtained from the Brazilian Ministry of Labor. This registry contains annual information on all formal labor

⁹ See *Valor Economico* (January 6, 2003) and *Valor Economico* (April 1, 2005).

contracts, including unique plant and firm identifiers, months of hiring or separation for each worker, and the plant's sector. By law, all firms hiring workers in Brazil must complete the information on this registry. The manufacturing sector relies intensely on workers with formal labor contracts and anecdotal evidence suggests this is particularly true for the largest firms. Based on this registry, I construct an annual and continuous measure of firm-state employment. For any given firm-state-year, I track all workers who were employed in that firm-state-year and compute the fraction of the year that the worker was attached to the firm-state. I then aggregate this variable for all existing workers. The firm-state sample covers the period 1995 to 2005. I start the sample in 1995 because there was very high inflation prior to this period, and focus on all the periods with available data from 1995 on.

Firms are covered in the main sample used in the paper if they have average total employment above 500 workers over this sample period. I track these firms in all their states of operation over the entire sample period. Firms drop out of the data only when they leave the registry. This happens when there is either a true exit (bankruptcy or acquisition) or when there is a change in the firm's tax code (unique firm identifier). The choice of this sample is motivated by the fact that borrowing from the government-owned bank was most important for large firms.¹⁰ To check the robustness of my results, I also construct samples of smaller firms. I first construct a sample of firms between 500 and 200 workers. The 200-worker threshold is chosen to obtain a similar number of firms as the main sample. I also construct a sample of firms with employment between 200 and 50 employees.

C. Political Data and Variables

State and national elections take place at the same time every four years. Data for the 1994, 1998, 2002, and 2006 elections are available from the Brazilian national election court (TSE). I use these data to construct measures of alignments between state governors and the presidential coalition. I capture whether an incumbent governor is allied with the federal government based on his/her political party and political coalitions at the time he/she was elected. I obtain information on these alignments from press sources and election websites. During the sample period, there were two major parties in the presidential coalition (including the president's party): PSDB and PFL between 1995 and 2002, and PT and PMDB between 2003 and 2006. Table II presents the distribution of alignments across states over the sample period.

I construct a sample of allied incumbents expected to face greater competition from the opposition. This is the sample of incumbents elected after a close election. In this sample, nonallied incumbents elected after a close election are

¹⁰ As discussed before, this is owing to the fact that only sufficiently large firms have access to direct loans. The average size of firms relying on direct government bank loans is close to the average size of firms in this sample and government bank credit becomes particularly important for firms with more than 500 employees. See the Internet Appendix for more details.

Table II
Description of Incumbents' Alignments

This table presents the entire distribution of political alignment between governors and the central government over the sample period (1995 to 2006). Listed below are all Brazilian states over the three electoral cycles covered in the sample. Local (gubernatorial) and national elections took place together at the end of 1994, 1998, 2002, and 2006.

State	Allied During 1995–1998?	Allied During 1999–2002?	Allied During 2003–2006?
Rondonia		Yes	
Acre			Yes
Amazonas		Yes	
Roraima			
Para	Yes	Yes	
Amapa			
Tocantins		Yes	
Maranhao	Yes	Yes	
Piaui			Yes
Ceara	Yes	Yes	
Rio Grande do Norte			
Paraiba		Yes	
Pernambuco			Yes
Alagoas			
Sergipe	Yes	Yes	
Bahia	Yes	Yes	
Minas Gerais	Yes		
Espirito Santo		Yes	
Rio de Janeiro	Yes		
Sao Paulo	Yes	Yes	
Parana		Yes	Yes
Santa Catarina		Yes	Yes
Rio Grande do Sul	Yes		Yes
Mato Grosso do Sul			Yes
Mato Grosso		Yes	
Goias		Yes	
Distrito Federal			Yes

used as a control group for allied incumbents elected after a close election. I classify a state election as close if it satisfies two criteria. First, the two top candidates must include one allied and one nonallied candidate. Second, the election must be decided by a sufficiently small margin of votes. In the Internet Appendix, I provide evidence that incumbents elected with a smaller margin of votes face greater competition when running for reelection.

During the sample period there was a two-round system for state elections. If no candidate received the absolute majority of votes in the first round, the two top candidates ran in a second round of voting. For my sample of close elections, I use elections won by a margin of votes smaller than 20% of valid votes, in either the first or the second round. The majority of the elections in the sample (80%) were decided in the second round. The median absolute

margin of victory for the first round is 7.55% of valid votes. I provide a list of all incumbents elected after close elections in the Internet Appendix.

D. Summary Statistics

Table III presents summary statistics for the sample of firms with an average size above 500 employees, the main sample used in this paper. There are approximately 40,000 firm-state-year observations, corresponding to around 1,500 firms across two to three states and over a period of 11 years. Firms in the sample are large, employing on average 2,267 workers. The number of firm-state-year observations is relatively balanced across states with allied incumbents and states with nonallied incumbents.

IV. Results

A. Local Firm Employment

I use firm-state employment data to test whether firms *eligible* for government bank loans expand employment in states with allied incumbents near reelection years. I also test whether this effect is not important for firms ineligible for government bank lending. I further test whether these expansions are most important for incumbents facing strong competition, take place largely before reelection races, and have economically important effects on the allocation of resources by firms.

A.1. Employment Growth

To test these predictions, I begin by estimating

$$Y_{istj} = \theta_{tj} + \gamma_1 \text{Allied}_{st} + \gamma_2 \text{Allied}_{st} \times \text{Reelection}_t + \beta_1 \text{Allied}_{st} \times \text{Priority}_j + \beta_2 \text{Allied}_{st} \times \text{Reelection}_t \times \text{Priority}_j + \varepsilon_{istj}, \quad (1)$$

where Y is the local employment growth of firm i in industry j and state s at time t . The term θ_{tj} denotes industry-year fixed effects. *Allied* is an indicator that equals one if the incumbent is allied with the central government. *Reelection* is an indicator that equals one if there will be a local reelection race within two years. Local elections occur every four years and all races take place at the same time. Since elections take place in the last months of the year, this variable is equal to one in both the calendar year of reelection races and the previous calendar year. *Priority* indicates whether a firm belongs to a priority sector.

The coefficients γ_1 and γ_2 compare the employment growth of nonpriority sector firms in regions with and without allied incumbents. The first coefficient makes this comparison in the two years farthest from reelection races. The second coefficient estimates the difference between this comparison in the two years closest to the reelection races and the two years farthest from the

Table III
Summary Statistics

This table presents summary statistics for the main sample of firms used in the paper. The sample consists of firms that had average total formal employment above 500 employees. Observations are at the firm-state-year level. Variables are averaged over all observations in the sample over the period 1995 to 2005. Allied incumbents are incumbents politically allied with the central government. Employment is a full-time equivalent of all workers with formal labor contracts. Employment growth in year t is computed as the difference between employment in year t and year $t - 1$ divided by the average employment across these two periods.

	All Observations				Allied Incumbent				Nonallied Incumbent			
	Mean	Median	Std. Deviation	Obs.	Mean	Median	Std. Deviation	Obs.	Mean	Median	Std. Deviation	Obs.
Employment	535.2	131.2	1,151.9	40,464	585.8	211.8	1,245.5	21,900	475.5	73.8	1,027.3	18,564
Log(Employment)	4.54	4.88	2.31	40,464	4.74	5.36	2.29	21,900	4.31	4.30	2.31	18,564
Total Firm Employment	2,267.1	1,134.0	3,733.5	40,464	2,081.2	1,076.9	3,339.3	21,900	2,486.5	1,210.3	4,140.1	18,564
Employment Growth (%)	-0.6	0.0	46.3	33,892	-1.5	0.0	45.7	17,831	0.4	0.1	46.9	16,061

reelection races. This second effect captures any differential behavior of employment growth in allied states near reelection races for nonpriority firms. The coefficients β_1 and β_2 estimate the difference between these two effects for priority firms and nonpriority firms.

I first estimate this specification using the sample of firms with at least 500 employees. In this sample, the results for priority sector firms capture the behavior of firms eligible for government bank lending. The results are reported in column (1) of Table IV. Employment growth is not higher in states with allied incumbents for nonpriority sector firms, nor is it higher for priority sector firms in the years farthest from the reelection races. Employment growth becomes higher in states with allied incumbents only for priority firms and next to reelection races. While this result is economically important, it is not statistically significant.

I further analyze the timing of the effects by estimating

$$\begin{aligned}
 Y_{istj} = & \theta_{tj} + \gamma_1 \text{Allied}_{st} + \gamma_2 \text{Allied}_{st} \times \text{Reelection}(-1)_t + \gamma_3 \text{Allied}_{st} \\
 & \times \text{Reelection}(0)_t + \beta_1 \text{Allied}_{st} \times \text{Priority}_j + \beta_2 \text{Allied}_{st} \times \text{Reelection}(-1)_t \\
 & \times \text{Priority}_j + \beta_3 \text{Allied}_{st} \times \text{Reelection}(0)_t \times \text{Priority}_j + \varepsilon_{istj}, \quad (2)
 \end{aligned}$$

where $\text{Reelection}(0)$ is an indicator that equals one in the calendar year of a reelection race, $\text{Reelection}(-1)$ is an indicator that equals one in the calendar year before a reelection race, and all other variables are defined as before.

The coefficients now break down the previous comparisons between the employment growth of firms in regions with and without allied incumbents separately for one and two years before reelection races. Since local elections take place in the last months of the calendar year, the comparison in the calendar year of reelection races represents a comparison within one year of reelection races, and the comparison in the calendar year before a reelection race represents the comparison between one and two years before the races.

The results are reported in column (2) of Table IV. As in the previous results, employment growth is not higher in states with allied incumbents for nonpriority sector firms, nor is it higher for priority sector firms in the years farthest from reelection races. However, the breakdown shows that priority sector firms expand employment between one and two years before reelection races in states with allied incumbents. This expansion is economically and statistically significant. The absence of an effect within one year of reelection races implies that priority firms do not offset the initial expansion by growing less in this period. The previous results, when looking at the average employment growth during these two years, missed this pattern, which is consistent with previous evidence that suggests opportunistic political cycles have a short length in emerging markets (Akhmedov and Zhuravskaya (2004)).

To refine these results I first control for fixed differences across firms-states' employment growth rates. This test addresses the possibility that firms in allied regions have higher average growth rates. Columns (3) and (4) replicate

Table IV
Local Employment Results: Employment Growth
Dependent Variable: Employment Growth in a Firm-State-Year

This table presents results from the estimation of equations (1) and (2). The unit of observation is a firm-state-year. Employment growth in year t is the difference in employment between years t and $t - 1$ divided by the average employment across those two years. *Allied* is an indicator that equals one if the incumbent is politically allied with the central government. *Reelection* is an indicator that equals one if there will be a local reelection race within two years. *Reelection(0)* and *Reelection(1)* are, respectively, indicators that equal one in the calendar year of reelection races and the previous calendar year. *Priority* is an indicator that equals one for priority sector firms. The overall sample includes all firms with average employment above 500 workers between 1995 and 2005. The sample of close elections restricts the analysis to incumbents elected after a close election. The sample of nonclose elections contains all other incumbents. The smaller firms sample contains firms with average employment above 200 and below 500 workers between 1995 and 2005. Standard errors (in parentheses) are heteroskedasticity-robust and clustered at the firm level. *, **, and *** respectively indicate significance at the 10%, 5%, and 1% levels.

	Sample of All Observations										
	Close Elections					Smaller Firms			Nonclose Elections		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
<i>Allied</i>	0.005 (0.011)	0.005 (0.011)	0.001 (0.011)	0.001 (0.011)	0.022 (0.015)	0.022 (0.015)	0.016 (0.014)	0.016 (0.014)	-0.012 (0.009)	-0.008 (0.020)	-0.001 (0.018)
<i>Allied</i> × <i>Reelection</i>	-0.013 (0.015)		-0.024 (0.014)		-0.032 (0.021)		-0.042** (0.020)		-0.004 (0.011)	0.011 (0.028)	-0.021 (0.025)
<i>Allied</i> × <i>Reelection(-1)</i>		-0.025 (0.016)		-0.036 (0.015)		-0.030 (0.022)		-0.049*** (0.020)			
<i>Allied</i> × <i>Reelection(0)</i>		0.014 (0.020)		0.005 (0.020)		-0.020 (0.030)		-0.0130 (0.028)			
<i>Allied</i> × <i>Priority</i>	-0.015 (0.015)	-0.015 (0.015)	-0.017 (0.015)	-0.017 (0.015)	-0.025 (0.018)	-0.025 (0.015)	-0.031* (0.018)	-0.032* (0.018)	-0.001 (0.012)	-0.002 (0.026)	0.003 (0.022)
<i>Allied</i> × <i>Reelection</i> × <i>Priority</i>	0.030 (0.020)		0.050*** (0.019)		0.062** (0.027)		0.063*** (0.024)		0.006 (0.016)	-0.007 (0.038)	0.037 (0.034)
<i>Allied</i> × <i>Reelection(-1)</i> × <i>Priority</i>		0.045** (0.021)		0.066*** (0.021)		0.065** (0.027)		0.075*** (0.025)			
<i>Allied</i> × <i>Reelection(0)</i> × <i>Priority</i>		-0.008 (0.027)		0.008 (0.026)		0.033 (0.041)		0.014 (0.038)			
Industry-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm-State FE			YES	YES	YES	YES	YES	YES	YES	YES	YES
R^2	0.08	0.08	0.42	0.42	0.09	0.09	0.44	0.44	0.38	0.02	0.39
Observations	33,033	33,033	33,033	33,033	17,734	17,734	17,734	17,734	17,860	15,299	15,299

the two previous results with firm-state fixed effects. These results confirm the previous findings, which become economically and statistically stronger.

To refine the results above further I restrict my analysis to the sample of allied incumbents elected after a close election. This test is intended to identify the incumbents expected to face strong competition from the opposition. If politicians are using government bank lending to help incumbents in reelection races, the previous effects should be more pronounced in this sample. I use nonallied incumbents elected after close elections as a control group for allied incumbents elected after close elections.¹¹ Columns (5) to (8) replicate all previous results using this sample. These results also confirm the previous findings, which again become economically and statistically stronger. Priority sector firms expand employment between one and two years before reelection races of allied incumbents. As a robustness check, I also estimate the results based on the sample of incumbents elected after nonclose elections. Columns (10) and (11) report the results. The results show economically much smaller effects that are statistically insignificant.

I next restrict my analysis to the sample of firms with more than 200 and less than 500 employees. In this sample, government bank lending is not as important for priority sector firms. Column (9) reports the results. Consistent with the view that the firms in this sample are less influenced by political incentives, the results show no economically or statistically important effects for both priority and nonpriority sector firms.

The estimates in Table IV imply that large priority sector firms expand employment between 6.6% and 10.0% in regions with allied incumbents. Over the entire sample, these coefficients imply expansions between 132,865 and 201,311 jobs, which represents between 10.2% and 15.4% of the total employment of these firms at the beginning of the sample period. Over the sample period, these firms created 721,177 jobs. Relative to this benchmark, the estimates represent between 18.4% and 27.9% of the jobs created by firms. Given the importance of industry shocks, I also compute this benchmark based on industry-adjusted employment. Based on this approach, the estimates represent between 20.8% and 36.3% of all jobs created by firms.¹²

A.2. Cumulative Effect on Employment

Finally, I analyze the cumulative employment expansion of firms around allied incumbents' reelection races. On the one hand, the previous results might simply capture changes in the timing of firms' employment decisions. If this is the case, these results might not capture important changes in the allocation of resources by firms. On the other hand, these results might reflect systematic changes in the operations of firms across regions. I address this issue by

¹¹ See Section III.C for greater detail and discussion of how this sample is constructed.

¹² I split the sample into two periods and identify each firm-state-period as a separate observation. I then aggregate the total number of jobs created over all observations for which there was an employment expansion.

estimating

$$Y_{istj} = \alpha_{is} + \theta_{tj} + \sum_{\tau=-3}^4 \gamma_{\tau} \text{AlliedReelection}_{st}(\tau) + \sum_{\tau=-3}^4 \beta_{\tau} \text{AlliedReelection}_{st}(\tau) \times \text{Priority}_j + \varepsilon_{istj}, \quad (3)$$

where Y is the log of employment of firm i in industry j and state s at time t . The terms α_{is} and θ_{tj} respectively denote firm-state fixed effects and industry-year fixed effects. As before, *Priority* indicates whether a firm belongs to a priority sector.

The variable *AlliedReelection*(τ) is an indicator that equals one if there was a reelection by an allied incumbent τ years ago in that state. For example, *AlliedReelection*(0) equals one in the calendar year of an allied reelection race, *AlliedReelection*(1) equals one in the calendar year after that reelection race, and *AlliedReelection*(-1) equals one in the calendar year before that race. The γ coefficients capture the local employment level of nonpriority sector firms before and after allied incumbents' reelection races. The β coefficients capture the difference between the local employment level of priority firms and nonpriority firms before and after these events.

Between years -3 and 0, this specification compares the employment of firms in states with allied incumbents to the employment of firms in states without allied incumbents. Between years 1 and 4, this specification compares the employment of firms in states with an allied incumbent in the past term to the employment of firms in states without an allied incumbent in the past term. In making this last comparison, the specification controls for whether the current incumbent is allied.¹³ I restrict the analysis to two years after reelection races. Since new elections occur every four years and firms expand two years before reelection races, results based on longer windows might be influenced by new expansions.

Previous results show that employment growth increases in states with allied incumbents predominantly in year -1. A shock to employment growth that starts in year -1 should have an impact on employment *levels* from year 0 forward. Therefore, I collapse the estimates from years -3 to -1 and from years 0 to 2. These two estimates respectively represent the effects more than one year before reelection races and from one year before reelection races and beyond.

The results are reported in Table V. As in Table IV, I first focus on the sample of firms with at least 500 employees. Column (1) of Table V reports the results based on all incumbents. For nonpriority firms, there is an economically small and statistically insignificant effect that goes to zero over time. In contrast, there is a statistically significant 8.0% increase in the local employment of priority sector firms from near reelection races forward. Column (2) of Table V

¹³ This is due to the fact that I control for *AlliedReelection*(-4) to *AlliedReelection*(-1), while I estimate the impact of *AlliedReelection*(1) to *AlliedReelection*(4).

Table V
Local Employment Results: Cumulative Effects

Dependent Variable: Log of Firm-State-Year Employment

This table reports the results from the estimation of equation (3). The unit of observation is a firm-state-year. $AlliedReelection(\tau)$ is an indicator that equals one if there was a reelection by an allied incumbent τ years ago in that state. $AlliedReelection(i\ to\ j)$ is equal to one if any of the indicators between i and j is one. $Priority$ is an indicator that equals one if a firm is in a priority sector. The overall sample contains all firms with average employment greater than or equal to 500 workers between 1995 and 2005. The sample of close elections restricts the analysis to reelection races that took place with an incumbent governor elected after a close election. For each of these reelection races, the sample goes from two years after the reelection race until the first year of the previous incumbent's term. Only firms initially in the state are included. In this sample, $AlliedReelection(\tau)$ is an indicator that equals one τ years after the reelection race took place, if the incumbent running for reelection was allied. Only data before the reelection race are used to estimate firm-state FEs and the results control for the alignment of the previous and next incumbent. The results for the sample of nonclose elections replicate the results for the sample of close elections using incumbents elected after a nonclose election. The sample of smaller firms includes all firms with average employment above 200 and below 500 between 1995 and 2005. The sample of smallest firms includes firms with average employment above 50 and below 200 in this same period. Standard errors (in parentheses) are heteroskedasticity-robust and clustered at the firm level. ** indicates significance at the 5% level.

	Close Elections			Close Elections		
	All Observations (1)	(2)	(3)	Nonclose Elections (4)	Close Elections (5)	Smallest Firms (7)
$AlliedReelection(-3\ to\ -1)$	0.020 (0.025)	-0.001 (0.036)	-0.023 (0.035)	-0.047 (0.043)		
$AlliedReelection(0\ to\ 2)$	-0.003 (0.030)	-0.073 (0.062)	-0.079 (0.058)	-0.010 (0.065)		
$AlliedReelection(-3\ to\ -1) \times$ $Priority$	0.001 (0.031)	0.001 (0.046)	0.023 (0.049)	0.014 (0.055)	0.053 (0.077)	-0.049 (0.042)
$AlliedReelection(0\ to\ 2) \times$ $Priority$	0.080** (0.037)	0.168** (0.085)	0.168** (0.078)	0.060 (0.086)	0.184** (0.079)	-0.052 (0.061)
$AlliedReelection(-1)$			YES YES	YES YES	YES YES	YES YES
$AlliedReelection(0\ to\ 2)$			YES YES	YES YES	YES YES	YES YES
$AlliedReelection(-1) \times$ $Priority$			0.63 32,412	0.33 25,047	0.75 32,412	0.71 31,961
$AlliedReelection(0\ to\ 2) \times$ $Priority$			32,412	25,047	32,412	105,648
Firm-State FE	YES	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES	YES
State-Year FE	0.52	0.64	0.63	0.33	0.75	0.71
R^2	40,460	32,412	32,412	25,047	32,412	105,648
Observations						

reports the results based on the sample of incumbents elected after a close election. These results confirm the previous findings, which become economically and statistically stronger. Panel A in Figure 1 plots the estimated response of priority sector firms by year. The figure suggests a sharp and persistent expansion in local employment from one year before reelection races and beyond. These results imply magnitudes that are similar to those analyzed in Table IV and suggest that employment expansions represent systematic changes in the operations of firms across regions.

Estimation of the effects for years -3 and -2 requires several years of data before reelection races. Since the estimates for these years are economically much smaller and statistically insignificant, I estimate (3) looking only at the effects between years -1 and 2 . Column (3) presents the estimates based on this approach. Panel B in Figure 1 plots the estimates separately by year. While the results are more precisely estimated, both the magnitudes and the qualitative pattern of the expansions are very similar to those from the previous results. This is the basic specification that I use in the remainder of the paper.

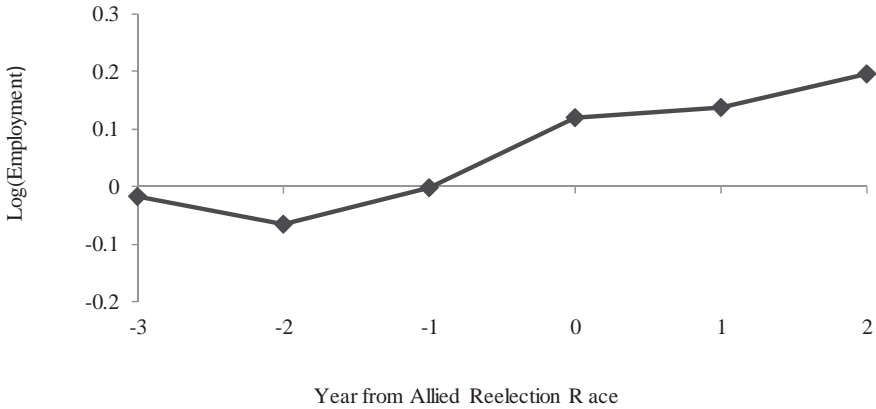
Column (4) presents these estimates with the sample of incumbents elected after nonclose elections. There is an economically small and statistically insignificant effect on the local employment of priority firms located in states with allied incumbents. Column (5) shows that the results reported in column (3) are robust to the inclusion of both industry-year and state-year fixed effects. Columns (6) and (7) report the same results based on the samples of smaller firms, which rely less on government bank credit. I find no economically or statistically important effects for priority firms in these samples.

B. Credit Allocation Results

I test if employment expansions by firms are associated with additional (favorable) borrowing from government-owned banks. As discussed in Section I, if politicians face commitment problems in making promises of future transfers to firms, they should compensate firms with additional loans at the time that firms expand. Since government bank loans are tied to specific projects with specified locations, these additional loans might be financing those exact expansions by firms in states with allied incumbents near reelections. This strategy can reduce politicians' cost of monitoring firms' decisions while influencing their behavior. The data allow me to test this hypothesis. I can track the aggregate disbursement of funds by year to projects by private firms located in different states. Government bank lending is very concentrated on priority sector firms. Therefore, at the aggregate *state* level, large expansions in lending during a given period should go to priority firms operating in that state during the same period. I test for this expansion in lending by estimating

$$\text{Log}(\text{Credit})_{st} = \alpha_s + \theta_t + \sum_{\tau=-3}^4 \beta_{\tau} \text{AlliedReelection}_{st}(\tau) + \varepsilon_{st}, \quad (4)$$

Panel A: Local Employment of Priority Firms Near Allied Reelection Races (All Years)



Panel B: Local Employment of Priority Firms Near Allied Reelection Races (Main Specification)

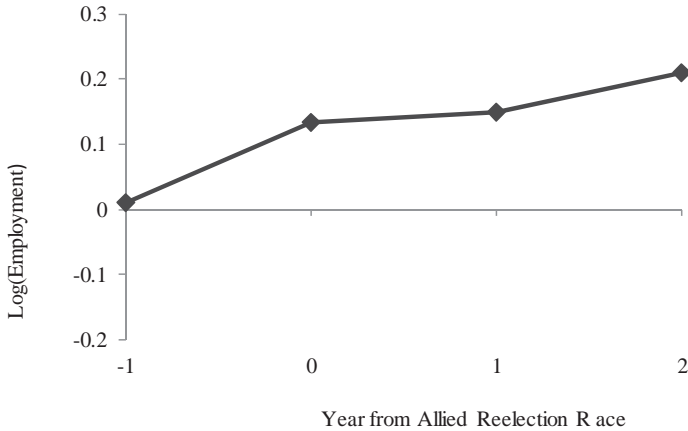


Figure 1. Timing of employment effects. This figure plots the estimated impact of a reelection race by an allied incumbent on the local employment of firms eligible for government bank loans over time. The results are based on the estimation of equation (3). The sample period is 1995 to 2005. The overall sample includes all firms with average employment greater than or equal to 500 workers over this period. The results are based on the sample of close elections, which restricts the analysis to reelection races that took place with an incumbent governor elected after a close election. For each of these reelection races, the sample goes from two years after the reelection race until the first year of the previous incumbent's term. Only firms initially in the state are included. The reported effect τ years after the reelection race is the estimated coefficient of $Allied\ Reelection(\tau) \times Priority$. $Allied\ Reelection(\tau)$ is an indicator that equals one τ years after the reelection race took place, if the incumbent was allied. $Priority$ indicates if the firm is in a priority sector. Only data before the reelection race are used to estimate firm-state FEs and the results control for the alignment of the previous and next incumbent. Panel A reports results that include years -3 to 2 in the estimation of equation (3) and have $AlliedReelection(-3)$ to $AlliedReelection(2)$ as controls, as well as industry-year FEs. Panel B reports results that include years -1 to 2 in the estimation of equation (3) and have $AlliedReelection(-1)$ to $AlliedReelection(2)$ as controls, as well as industry-year FEs.

Table VI
Credit Allocation Results

Dependent Variable: Log of Government Bank Credit in a State-Year

This table reports the results from the estimation of equation (4). The unit of observation is a state-year. Government bank credit is the amount of funds disbursed to finance investment projects by private firms in a given state-year. The sample period is 1995 to 2006. *Allied Reelection*(τ) is an indicator that equals one if there was a reelection by an allied incumbent τ years ago in that state. *Allied Reelection*(i to j) is equal to one if any of the indicators between i and j is one. The sample of close elections restricts the analysis to the reelection races of incumbents elected after a close election. For each of these reelection races, the sample goes from two years after the reelection race until the first year of the previous incumbent's term. In this sample, *Allied Reelection*(τ) is an indicator equal to one τ years after the reelection race took place, if the incumbent running for reelection was allied. Only data before the reelection race are used to estimate state FEs and results control for the alignment of the previous and next incumbent. The results for the sample of nonclose elections replicate this same analysis for the sample of incumbents elected after a nonclose election. Standard errors (in parentheses) are heteroskedasticity-robust and clustered at the state level. ** indicates significance at the 5% level.

	All Observations (1)	Close Elections		Nonclose Elections
		(2)	(3)	
<i>AlliedReelection</i> (-3 to -2)	-0.070 (0.133)	0.152 (0.249)		
<i>AlliedReelection</i> (-1 to 2)	0.272** (0.131)	0.533** (0.209)	0.460** (0.222)	0.083 (0.194)
Year FE	YES	YES	YES	YES
State FE	YES	YES	YES	YES
R^2	0.90	0.93	0.92	0.45
Observations	304	135	135	169

where *Credit* is the amount of funds disbursed to projects by private firms in state s in year t . The terms α_s and θ_t respectively denote state and year fixed effects. The variable *AlliedReelection*(τ) is defined as before: an indicator that equals one if there was a reelection by an allied incumbent τ years ago in that state. The β coefficients capture the amount of credit disbursed to projects in a state before and after reelection races by allied incumbents.

There are two reasons to track the amount of funds disbursed after the reelection races. First, there is the same concern as in the context of employment that results may simply reflect the timing of decisions. Second, loans are tied to three- to four-year projects. Given that firms begin expanding only between one and two years before reelection race years, loans approved at that time should impact the amount of funds disbursed for at least one to two years after reelection races. Following the approach with the employment results, I follow credit until two years after reelection races.

Column (1) in Table VI reports the credit results for the sample of all incumbents. If loans are funding expansions, the disbursement of funds should expand at the same time as employment growth. Since the increase in employment growth begins between one and two years before the reelection race year

(year -1), I collapse the estimates into years farthest from reelection races (years -3 to -2) and years closest to reelection races and beyond (years -1 to 2). I find an economically small and statistically insignificant negative effect on the amount of funds going to states with an allied incumbent in the years farthest from reelection. In contrast, there is an economically important and positive effect on this amount from the years closest to reelection forward. Column (2) reports the same results for the sample of incumbents elected after a close election. These results confirm the previous findings, which become economically stronger.

Following the same approach as Table V, since the estimates for the first years (years -3 to -2) are economically much smaller and are statistically insignificant, I estimate the effects only between years -1 and 2 . Column (3) reports the estimates based on this approach. There is an economically and statistically important effect from near reelections forward. As a robustness check, column (4) reports the same estimate for the sample of incumbents elected after nonclose elections. The results show an economically much smaller and statistically insignificant expansion in government bank lending.

The estimates in Table VI imply that government bank lending expands by 27% over four years in regions with allied incumbents. Over the entire sample, these coefficients imply a total of R\$8,744 million (R\$1995) in funds. This represents 9.8% of the total funds disbursed to manufacturing private firms during the sample and 6.7% of the initial capital of large priority sector firms. Over the sample period, there is an overall expansion of R\$28,923 million (R\$1995) in government bank lending to these firms. Relative to this benchmark, the estimates represent 30.2% of the total expansion in government bank lending.¹⁴

I interpret this evidence as providing support for the view that employment expansions are reciprocated with greater access to (subsidized) government bank loans. Government bank lending financing projects in states with allied incumbents expands at the same time that priority firms expand employment. While I cannot track individual loans, at the aggregate state level the observed expansions in lending need to be associated with substantially greater access to lending by the exact same firms that expand (priority firms). If these large aggregate lending expansions are driven only by nonpriority firms, they should be associated with very large increases in borrowing by nonpriority firms from government banks. Since loans are tied to projects, nonpriority firms should be making large real expansions. However, I do not observe this behavior in the data. Additionally, lending expansions are only important in regions in which incumbents face competition, the exact same regions in which employment expansions by priority firms are also important.

¹⁴ This overall expansion is computed as the difference between the total funds disbursed over the sample minus the predicted amount of funds based on initial values. Given a participation ratio of 50% in projects, these numbers imply projects equal to 13.4% of firms' initial capital, consistent with employment magnitudes in Section IV.A.1.

C. Are Firms Shifting Employment across Regions?

Assuming that firms do not make substantial changes to their capital to labor ratio, they can make persistent expansions to their local employment in two ways. First, firms can implement additional projects and expand their overall size. Second, firms can shift the location of future planned projects toward politically allied regions and away from politically unattractive regions. In principle, firms could substitute by actually reducing employment in regions that are less politically attractive, as opposed to shifting new expansions. However, if reducing capacity by firing workers and selling capital is costly, this will be a less attractive alternative.

I provide evidence consistent with the view that firms mostly shift the location of future projects.¹⁵ First, I test if the results are driven by firms initially located both in states with allied incumbents and in states without allied incumbents (multialignment firms). Panel A in Table VII reports the results, which show that the effects estimated in Table V are driven by multialignment firms. In the Internet Appendix, I show that the results from Table V are primarily important for firms in growth industries and firms with most of their initial employment outside allied regions. These are exactly the firms with greater margin to shift the location of future expansions toward politically allied regions.¹⁶ I also show in the Internet Appendix that firms' overall employment experiences only a temporary expansion near reelection races.

Finally, I separately estimate employment expansions in allied regions and employment contractions in nonallied regions. I exploit the fact that effects only obtain for multialignment firms. In states with allied incumbents, I compare multialignment firms to multilocation firms located only in states with allied incumbents. In states with nonallied incumbents, I compare multialignment firms to multilocation firms located only in states with nonallied incumbents. These comparisons respectively provide us with estimates for employment expansions and contractions.

I estimate the employment expansions (contractions) by analyzing a sample of states initially nonallied (allied) that elect an allied (nonallied) incumbent. For each of those two samples, I estimate the following specification:

$$Y_{istj} = \alpha_{is} + \theta_{ij} + \sum_{\tau=-3}^4 \gamma_{\tau} \text{Multialignment}_i \times \text{Reelection}_{st}(\tau) + \sum_{\tau=-3}^4 \beta_{\tau} \text{Multialignment}_i \times \text{Reelection}_{st}(\tau) \times \text{Priority}_j + \varepsilon_{istj}, \quad (5)$$

where Y is the employment of firm i in sector j and state s at time t . The variable *Multialignment* indicates whether the firm is located in both states

¹⁵ In the Internet Appendix, I discuss how this outcome emerges under plausible assumptions. This outcome is consistent with firms being financially constrained.

¹⁶ This type of flexibility has been emphasized in previous work on the internal allocation of resources inside the firm (Williamson (1975), Stein (1997), Matsusaka and Nanda (2002)).

Table VII
Are Firms Shifting Employment Across Regions?

Dependent Variables: Log of Firm-State-Year Employment and Firm-State-Year Employment

Panel A reports results that replicate the estimates in column (3) of Table V for two separate subsamples, multialignment firms and not multialignment firms. Multialignment firms are firms initially located in states that (at the time of the reelection race) had allied and nonallied incumbents. Panel B reports results from the estimation of equation (4). The unit of observation is a firm-state-year. The sample of allied incumbents contains all state reelection races in which the incumbent was allied and the past incumbent was not. The sample of nonallied incumbents contains all state reelection races in which the incumbent was nonallied and the past incumbent was allied. For each reelection race, the sample goes from three years after the reelection until the first year of the previous incumbent's term. Only firms initially in the state are included. In each sample, $Reelection(\tau)$ is an indicator that equals one τ years after the reelection race took place. *Multialignment* indicates if the firm is located in states that (at the time of the reelection) have allied and nonallied incumbents. This measure is based on the employment of firms across states until three years before the reelection race. *Priority* is an indicator that equals one if a firm is in a priority sector. Only data before the reelection races are used to estimate firm-state FEs. The results for the sample of allied incumbents control for *Multialignment* \times *Reelection* (-1) and *Multialignment* \times *Reelection* (0 to 3). The results for the sample of no-allied incumbents control for *Multialignment* \times *Reelection* (-1), *Multialignment* \times *Reelection* (0 to 1), and *Multialignment* \times *Reelection* (2 to 3). Standard errors (in parentheses) are heteroskedasticity-robust and clustered at the firm level. * and ** respectively indicate significance at the 10% and 5% levels.

	Panel A: Are the Employment Results Driven by Multialignment Firms?			
	Log of Employment		Sample of Close Elections	
	Multialignment (1)	Not Multialignment (2)	Multialignment (3)	Not Multialignment (4)
<i>AlliedReelection(0 to 2) × Priority</i>	0.201** (0.093)	0.007 (0.141)	133.6** (62.3)	5.8 (121.6)
Firm-State FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
R^2	0.75	0.71	0.75	0.71
Observations	22,701	9,711	22,701	9,711

(Continued)

Table VII—Continued

	Panel B: Employment Expansions in Allied Regions vs. Employment Contractions in Nonallied Regions			
	Log of Employment		Employment	
	Allied Incumbents (1)	Nonallied Incumbents (2)	Allied Incumbents (3)	Nonallied Incumbents (4)
<i>Multialignment</i> × <i>Reelection</i> (−1) × <i>Priority</i>	0.005 (0.079)		1.3 (21.8)	
<i>Multialignment</i> × <i>Reelection</i> (0 to 3) × <i>Priority</i>	0.197** (0.101)		54.3** (28.1)	
<i>Multialignment</i> × <i>Reelection</i> (−1) × <i>Priority</i>		−0.021 (0.079)		−6.7 (24.9)
<i>Multialignment</i> × <i>Reelection</i> (0 to 1) × <i>Priority</i>		−0.006 (0.119)		−1.8 (37.8)
<i>Multialignment</i> × <i>Reelection</i> (2 to 3) × <i>Priority</i>		−0.225* (0.130)		−71.6* (43.0)
Firm-State FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
R ²	0.63	0.56	0.63	0.56
Observations	16,302	13,418	16,302	13,418

with allied incumbents and states with nonallied incumbents at the time the new incumbent sits in office, and *Priority* indicates whether a firm belongs to a priority sector. The terms α_{is} and θ_{tj} respectively denote firm-state fixed effects and industry-year fixed effects.

The variable $Reelection(\tau)$ is an indicator that equals one τ years after the reelection race of the new incumbent in the state. For example, $Reelection(-1)$ equals one three years after the new incumbent sits in office, and $Reelection(0)$ equals one in the calendar year the new incumbent runs for reelection. The estimation of the γ coefficients compares over time the local employment of multialignment and other firms in the nonpriority sector. I focus on the β coefficients that estimate the difference between this effect for priority and nonpriority firms. In contrast with the previous results, which analyzed effects only until year 2, I extend the horizon by one year to capture the lag in the employment contractions, which comes from the prediction that firms are shifting expected expansions.

Panel B in Table VII reports the results. Columns (1) and (3) report the estimates for the employment expansions. Columns (2) and (4) report the estimates for the employment contractions. Figure 2 plots the estimated effects by year. The results show that, after an allied incumbent is elected, priority firms expand their employment in the region only near the incumbent's reelection race. This expansion is persistent and matches the pattern of the previous results. On the other hand, it takes longer for priority firms to contract their employment in a region after a nonallied incumbent is elected. Priority firms do not contract their employment before the incumbent runs for reelection. They reduce their employment only after reelection races.

While some of those estimates are not very precisely estimated, they support the view that firms expand in regions with allied incumbents by shifting the location of future projects away from states with nonallied incumbents. These results also suggest that politicians are not using lending to adversely affect nonallied incumbents in reelections or punish nonallied states.¹⁷ They suggest that contractions are a consequence of expansions in regions with allied incumbents.

D. Are Politicians Influencing Elections with Credit?

In the Internet Appendix, I provide evidence that politicians manage to influence electoral outcomes with government bank lending.¹⁸ I analyze the share of votes by incumbents in reelection races relative to when last elected. I first show that allied incumbents have higher voting shares in reelections. I then show that this effect is driven by regions where priority sectors are historically

¹⁷ I thank the Associate Editor for highlighting these two possibilities. In the sample, approximately 50% of the states in which priority firms operate have an allied incumbent. If firms contract in states with nonallied incumbents by the same magnitude (in terms of number of workers) that they expand in states with allied incumbents, they are only substituting labor across regions.

¹⁸ I would like to thank an anonymous referee for encouraging this analysis and suggesting directions.

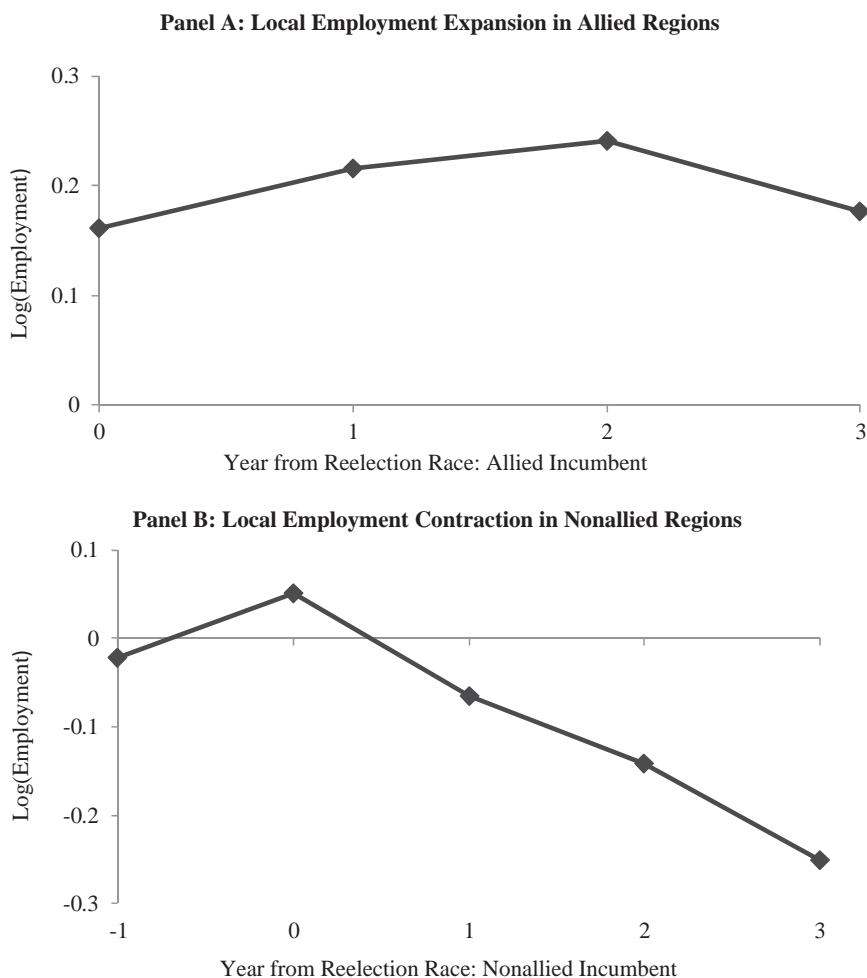


Figure 2. Timing of employment expansions and contractions. This figure plots the estimated impact of a reelection race on the local employment of firms eligible for government bank loans across regions with allied and nonallied incumbents. The results are based on the estimation of equation (4). The sample period is 1995 to 2005. The overall sample includes all firms with average employment greater than or equal to 500 workers over this period. Panel A reports results based on a sample of allied incumbents, which includes all state reelection races in which the incumbent is allied and the previous incumbent is not. Panel B reports results based on a sample of nonallied incumbents, which includes all state reelection races in which the incumbent is not allied and the previous incumbent is allied. For each reelection race, the sample goes from two years after the reelection race until the first year of the previous incumbent's term. Only firms initially in the state are included. The reported effect τ years after the reelection race is the estimated coefficient of $Reelection(\tau) \times Multialignment \times Priority$. $Reelection(\tau)$ is an indicator that equals one τ years after the reelection race took place. $Multialignment$ indicates if the firm is located in states that (at the time of the reelection) are allied and nonallied. This variable is based on the employment of firms across regions until three years before the reelection race. $Priority$ indicates if the firm is in a priority sector. Only data before the reelection race are used to estimate firm-state FEs. The results include controls for $Reelection(-1) \times Multialignment$, $Reelection(0) \times Multialignment$, $Reelection(1) \times Multialignment$, $Reelection(2) \times Multialignment$, and $Reelection(3) \times Multialignment$, as well as industry-year FEs.

more important. This analysis is at the municipality level and there is variation in the importance of priority sectors both within and across states. The estimates suggest that a substantial number of elections are influenced by the effects analyzed in this paper. I also provide evidence that higher growth in the amount of government bank lending financing projects in a state is generally associated with higher voting shares by incumbents in reelection. This effect is also driven by regions in which priority sectors are historically more important.

V. Alternative Explanations

I interpret my findings above as driven by politicians using government bank lending to politically influence the decisions of firms. In this section, I examine whether alternative explanations can plausibly explain my findings.

A. Are the Results Driven by the Supply of Government Bank Credit?

One can argue that the results are not driven by the *supply* of government bank lending. One possible explanation is that firms expand in regions with allied incumbents in response to unobserved economic shocks. In the Internet Appendix, I show that the election of an allied incumbent in a state is not associated with past or contemporaneous changes in local employment. Rather, it is only associated with future changes in local employment at the time of reelection races. These local employment expansions are only important for firms eligible for government bank lending and incumbents facing strong competition in reelection. It is unlikely that unobserved economic shocks would match all these patterns.

A more likely alternative explanation for the results is that politicians use other unobserved instruments to influence firms' decisions. As discussed in Section II.A, priority sectors are sectors historically targeted by the federal government for the purposes of government bank lending. The motivation is to capture sectors in which there is a lower cost of lending by the government bank and government credit is concentrated in these sectors during the sample period. To the extent that large priority sector firms are also targeted by other government policies, in principle this story could explain the results. My main strategy to address this concern is to directly examine the major policies targeted at manufacturing firms. The results imply economically large effects over manufacturing firms' allocation of labor. In order to explain these findings, unobserved instruments must be able to produce large transfers to firms.

In the Internet Appendix, I describe government reports that quantify the major sources of transfers (implicit and explicit) from the federal government to manufacturing firms. One of these reports was prepared by economists in the Ministry of Finance. This report measured implicit transfers based on opportunity costs and relied on government records. I examine three broad groups of policies: trade policies, regional policies, and policies targeted at specific sectors. More than 90% of the transfers associated with trade policies come from pro-export policies. Based on customs data on the universe of legally registered

export operations, I estimate the main results of the paper for different samples, sorted by the importance of exports over revenues. I find that the results in the paper are more important for firms that do not significantly rely on exports. I also find that exporting firms do not expand their employment more in allied regions near reelection years. Another potential source of transfers from trade policies are import taxes. Based on customs data on import operations, I show that the results are not driven by importing firms. I then estimate the main results in the paper excluding the specific regions and sectors targeted by alternative policies. These results are similar to those reported in the paper using all sectors and regions. Motivated by government reports and anecdotal evidence, I also examine the major source of transfers from local governments (local tax breaks) and procurement contracts. Based on firm-level data on local taxes and an industry-level measure of the importance of procurement contracts, I show that controlling for the importance of these instruments does not significantly impact the main results of the paper.

This analysis suggests that the main alternative sources of transfers to firms cannot explain the results in the paper. However, I cannot completely rule out the possibility that other instruments (not listed in government documents or commonly mentioned in the press) could explain the results. As an additional robustness check, I provide direct evidence against the view that priority sectors are chosen with politically sensitive regions in mind. In the Internet Appendix, I show that priority sectors are not more important in regions with allied incumbents or in regions with allied incumbents elected after a close election.

B. Social Lending and Unintended Lending Effects

Another alternative explanation for the results is a social lending story. According to this explanation, the results in the paper are driven by the supply of government bank loans. However, government banks allocate credit in this way in response to social objectives. There might be synergies between the central government and allied incumbents in the implementation or screening of socially desirable projects by firms. Additionally, since it might take time to find and finance these projects, they may get financed near the end of incumbents' terms. Based on such assumptions, one can rationalize a loan expansion near reelection races in states with allied incumbents.

The first challenge for this explanation is the fact that expansions are only important near reelection races of incumbents facing political competition. In principle, economic conditions in regions with stronger political competition could be different and this could lead a social planner to lend more in these regions. For example, political competition could capture regions in the bottom of the local business cycle and government loans could be used to stabilize business cycles (Micco and Panizza (2006)). As discussed in Section III.C, political competition in reelection races is predicted based on the outcome of elections when incumbents were last elected. In the Internet Appendix, I show that political competition is weakly related to local business cycles but is stronger in

regions with lower growth. I then show that results in the paper are not driven by states with lower growth or states at the bottom of the business cycle.

This analysis suggests that political competition is not capturing economic conditions. In principle, political competition could be correlated with other factors that would also lead social objectives to move together with strong competition. While I cannot completely rule out this possibility, there are two other challenges for this explanation. Since an important explicit social objective of government bank lending is to mitigate firms' financing constraints, this explanation predicts that an important share of firms should be increasing their overall size and that smaller firms should be impacted. I find no support for these predictions. Additionally, employment expansions are sharp and mostly concentrated before reelection races. This is consistent with previous evidence on opportunistic political cycles in emerging markets (Akhmedov and Zhuravskaya (2004)). A social lending story can explain a sharp expansion to loan approval before reelections but, given that loans are typically associated with multiyear investment projects, faces a challenge in explaining why firms also need to expand employment this way.

A final possibility is that firms expand local employment in response to the political allocation of credit but politicians are not trying to influence firms' decisions. For example, politicians might increase government bank lending to help allied incumbents raise additional campaign contributions or to gain further support from businesses. Local expansions are driven by multilocation firms that receive additional loans from government banks for projects located in the exact *same regions* where they expand. If politicians are not trying to influence firms' decisions, it is not clear why they would finance projects located in the same regions where allied incumbents are located. Similar to the social lending story, this explanation predicts that loans would get *approved* near reelection races but it also faces a challenge when explaining why firms' expansions are mostly concentrated before reelection. While motives are inherently difficult to measure, the findings in this paper are consistent with the view that politicians use lending by government banks to influence the real behavior of firms.

VI. Conclusion

In this paper, I investigate lending by government banks and employment decisions of firms, and provide support for the view that government control over banks leads to an economically significant influence of political considerations over the real decisions of firms. I also provide evidence on the specific mechanism through which this happens: politicians use control over bank lending decisions to influence the decisions of firms and increase employment in politically attractive regions.

I study this issue based on a sample of Brazilian manufacturing plants that represents the universe of Brazilian manufacturing firms with at least 50 employees. I exploit the fact that government bank lending is controlled by the central government and show that firms eligible for government bank

lending expand employment in regions with allied incumbents near reelection race years. These expansions are persistent, only important when incumbents face political competition, and associated with greater borrowing from government banks. I find no effects for firms that are ineligible for government bank lending.

Consistent with the view that politicians use bank lending to shift employment towards politically attractive regions and away from unattractive regions, I find that local employment expansions do not lead to persistent expansions of firms' total employment. Multilocation firms expand in regions with allied incumbents and compensate by growing less afterwards in other regions. Moreover, employment expansions in attractive regions are (partially) financed with government loans tied to projects in those exact same regions. Finally, I also provide evidence that politicians manage to influence elections with bank lending.

This evidence suggests that political considerations systematically affect the way financial markets allocate resources. Governance considerations seem to have direct real consequences through their influence on the allocation of credit. While this paper suggests that these effects are economically important for firms, the analysis does not allow me to estimate potential efficiency costs. To the extent that managers are maximizing shareholder value, the effects analyzed in this paper are leading to a less efficient allocation of resources by firms and government loans are compensating firms for this cost. Analyzing the efficiency costs of these considerations is a promising area for future research.

The evidence in this paper also has implications for the causes and consequences of financial market regulation. Previous work on the political economy of financial regulation has emphasized the importance of the gap between private and public interests in the choice of regulation (Kroszner and Strahan (1999) and Rajan and Zingales (2003, 2004)). The results in this paper suggest the importance of an explanation for this gap. Politicians control financial institutions because this provides them with greater ability to politically influence the choice of projects being implemented in the economy. This explanation helps rationalize why government participation in financial markets takes the specific form of government ownership of financial institutions and is consistent with previous evidence on the privatization of nonfinancial firms. While this paper focuses on government control over banks' lending decisions, other forms of regulation over banks might also lead to greater political influence over the real economy. The recent financial crisis and the potential for greater government regulation over financial institutions have increased the importance of addressing this issue.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

Appendix S1: Internet Appendix.