ESSAYS IN GOVERNMENT FINANCE

BY

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Preface

Government ownership of banks is widespread around the world. While these do not exist, in the traditional sense, in the United States, they are prevalent in South America, Europe and Asia. Even in highly developed economics such as Germany and France, government banks play a sizeable role in finance. In this dissertation, I contribute to the growing body of evidence on the behavior and impacts of direct government intervention in finance through government ownership of banks.

What is a government-owned bank? To illustrate, think about Bank of America but with the federal government of the United States as the majority shareholder. This majority stake gives the federal government effective control over the operations of the bank. The Chief Executive Officer is a political appointee from the executive branch as well as a fraction of the Board of Directors. Given this, it is perhaps unsurprising that the literature on the behavior of government-owned banks has generally found these banks lending to politically-connected firms, to areas with aligned politicians (and even more so to areas with aligned-close elections), and increasing lending in election years. It would be as if this government-owned Bank of America lent more in Ohio in the summer of 2012, prior to the re-election of President Barack Obama.

The fall of Lehman Brothers and the ensuing financial crisis has renewed interest in understanding the proper role of government in finance, and in particular, the potential role and benefits of government ownership of banks. Anecdotal evidence in Brazil suggests that their government banks may have done something positive in the recent financial crisis. President Dilma Rousseff explained that "During the crisis, after the failure of Lehman Brothers, it was [state-controlled financial] institutions like Banco do Brasil, Caixa Economica Federal and the National Development Bank (BNDES) that prevented the economy from being shipwrecked." A former Chief Financial Officer of Bradesco (a large private bank in Brazil), said that views of state banks are changing and state banks "had a very important role ...

in the government's anti-cyclical policies." Similarly, a former Governor of the Central Bank of Brazil said that the "consensus" is that the state banks played an important role.

Even though the existing academic literature paints a bleak view of government-owned banks, in theory, these banks may be well suited in a financial crisis to provide counter-cyclical support. Because they are explicitly backed by the government, they may not experience the same flight of deposits that a perceived to be unsafe bank may have. They can be re-capitalized quickly, and governments can instruct the banks to lend. In the United States, when the government "bailed-out" the [private] banks, it was not effective in increasing lending. This was largely because the government could not force the banks to lend the money. In theory, a government could solve this principal-agent problem by re-capitalizing the government banks and instructing them to lend. Empirical evidence finds that lending by public banks tends to be less responsive to macroeconomic shocks than private banks, and very recent work on this financial crisis shows that in Latin America, government banks tended to lend less pro-cyclically than private banks.

My dissertation contributes both to the literature on the behavior of government banks and the impacts of these banks. Chapter 1 uses data from Brazil to analyze the lending by government and private banks at the national and local-level and then assesses whether this lending translated into the real economy (e.g. to employment and output). Chapter 2 develops a theoretical model of lending by government and private banks. Then, informed by the political science literature, I create a proxy for political-connectedness of borrowers and test the model using the World Bank Enterprise Surveys with firms and banks around the world. Chapter 3 calculates spillover effects of government transfers using data on Brazil's (government-bank facilitated) Bolsa Familia conditional-cash transfer program.

The contribution from chapter 1 largely comes from the data collected with the help of the Central Bank of Brazil. This data includes monthly bank balance sheets, the bank branch locations for every bank in Brazil, municipality balance sheets, yearly municipality employment censuses, additional controls from the 2000 census, and municipality-level output data from Brazil's Institute of Applied Economic Research (IPEA). While work has been

done to understand government banks in the recent financial crisis across countries, to my knowledge, no work has been able to assess the local-lending and hence the local effects of government banks during the financial crisis.

I find that government banks did, indeed, provide counter-cyclical lending at both the national and the local level, and this lending translated into locations with an above median share of bank branches that are government-owned experiencing a roughly 2% relative increase in output and 1.5% in employment.

It is important to note that this finding should be interpreted with caution. Given the existing literature on government banks (and consist with this dissertation's Chapter 2), we need to be careful about making statements that are too bold. I do not interpret this result as saying that we should nationalize two of the large US banks to buffer against potential financial crises. In fact, given the divergence in lending between government and private banks following the crisis, one needs to be careful about the changes in these banks' market shares, which when we are back to non-crisis times, could leave countries more susceptible to the politically-advantageous behavior for which government banks had previously been criticized. It does appear that this is a potential fiscal policy mechanism, however, much more work would be needed if one were to want to argue that this is the best mechanism.

Chapter 2 expands the literature on the lending behavior of government banks by providing a cross-country analysis. The cited studies either have loan-level data within a single country or have loan volume data across many countries. While I interpret my result as consistent with the previous literature, I am able to connect firms to banks from a large cross-section of countries through the World Bank Enterprise Surveys.

Chapter 3 uses household surveys and data on Brazil's Bolsa Familia program to assess labor market spillovers, but it has broad implications. Understanding the general equilibrium effects of government transfer programs, whether they are conditional-cash transfer programs in developing countries or welfare payments in the United States, is vital to understanding both the decisions that people make and the expected policy implications. This chapter attempts to understand the effects of a government transfer program in a locality

to both the recipients and the non-recipients.

Chapters 1 and 3 are co-authored with Leo Feler from the Johns Hopkins University School of Advanced International Studies (SAIS).

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Chapter 1

Bank Ownership, Lending, and Local Economic Performance During the 2008 Financial Crisis

1.1 Introduction

Government ownership of banks is widespread around the world. In 2005, government banks accounted for about 40% of total bank assets in Germany, 45% of total bank assets in Brazil, and about 42% in Argentina (Barth, Caprio, and Levine, 2004). The recent finance literature has focused on the potential drawbacks to direct government intervention in finance through government-owned banks. This literature finds that government ownership of banks is correlated with poor financial development and is most prevalent in poor countries with inefficient governments and weak institutions (Barth, Caprio and Levine, 2001, and La Porta, Lopez-de-Silanes, and Shleifer, 2002). There is further evidence that these banks come under political pressure to lend in preferential ways or to preferential areas (Sapienza, 2004; Dinc, 2005; Carvalho, 2012).

Government-owned banks, on the other hand, may be well positioned in times of a financial crisis to provide counter-cyclical support. When governments own banks, they can instruct their banks to make loans. Lending by government-owned banks tends to be less responsive

in general to macroeconomic shocks than lending by private banks (Micco and Panizza, 2006; Bertay, Demirguc-Kunt, and Huizinga, 2012; and Cull and Martinez-Peria, 2012). In part, this is due to how government banks are funded, being less reliant on short-term debt and being able to take advantage of government funds to make loans (Ivashina and Scharfstein, 2010) [1]. And in part, this can be explained by politics, with government banks coming under greater political pressure and being more susceptible to political influence to continue to lend. In fact, one cited reason for Brazil's relative success during the recent financial crisis was direct government involvement in the banking sector[2].

This paper explores the argument that government banks can provide counter-cyclical support and help to mitigate economic recessions, using data on Brazil following the collapse of the Lehman Brothers investment bank. It assesses whether government ownership of banks resulted in more lending, higher GDP, more employment and higher incomes, effectively mitigating the effects of the global financial crisis and helping Brazil avoid an economic recession.

In Brazil, the combination of government funding and political pressure resulted in government banks increasing their lending, offsetting the decline in private bank lending following the onset of the financial crisis. As Figure 1.1 shows, the sum of all lending by private-sector banks declined sharply after September 2008, while the sum of all lending by government-owned banks increased. Whereas prior to the onset of the financial crisis, private-sector banks accounted for the majority of all lending, after September 2008, government banks were the majority lenders in Brazil.

Government banks are not spread uniformly throughout Brazil, being concentrated in certain regions, and because lending is highly localized, areas with a high share of government banks were disproportionately stimulated. These areas correspondingly experienced rela-

[1] In the U.S., during the 2008-2010 financial crisis, banks cut their lending less if they had better access to deposit financing (Ivashina and Scharfstein, 2010) and if they relied more heavily on retail deposits rather than wholesale liabilities for funding (Gozzi and Goetz, 2010).

[2] *The Economist* (May 12, 2010) cites the CFO of Bradesco, a large private Brazilian bank, as saying that government banks in the country played a critical role in promoting anti-cyclical policies. Additionally, a former governor of Brazil's Central Bank explained the consensus view in Brazil that government banks were important in propping-up the economy during the financial crisis.

tive increases in employment and incomes, as shown in Figures 1.2 and 1.3, as government banks in these areas increased their lending to offset the decline in lending by private-sector banks. Our empirical strategy is to use a differences-in-differences approach, comparing what happens to lending, GDP, and employment before and after the crisis in areas with a high share of government-owned banks versus areas with a low-share of these banks.

Our results suggest that localities with a high share of government-owned banks experienced better than expected changes in lending, GDP, formal sector employment, and incomes. These localities continued to grow during the 2008-2010 financial crisis and did so faster than otherwise comparable localities with low shares of government banks.

These findings are largely consistent with the existing banking and finance literature. Having more efficient financial markets promotes economic growth, both at the national level, as discussed in King and Levine (1993), as well as at the local level, as discussed in Guiso, Sapienza, and Zingales (2004), Burgess and Pande (2003) and Gilbert and Kochin (1989). Given this literature, it is not unexpected that relieving local financial frictions during a crisis would improve economic outcomes.

The literature also suggests that economic sectors heavily reliant on bank financing grow faster when this financing is efficiently provided (Rajan and Zingales, 1998) and suffer larger declines when it is not (Braun and Larrain, 2005; Chava and Purnanandam, 2006; Kroszner, Laeven, and Klingebiel, 2007; and Dell'Ariccia, Detragiache, and Rajan, 2008). We examine this in our paper, and find that government banks did not respond differently nor disproportionately stimulate localities with higher bank dependence.

We do not find any evidence of government bank lending being politically targeted in Brazil immediately following the crisis, but it is unclear whether this would be the case in countries with weaker political institutions or even in countries with stronger institutions over a longer time horizon. In the Brazilian case, lending was neither targeted to the most bank-dependent sectors, nor was it targeted politically. Instead, it was simply allocated to areas where government banks had a greater presence, consistent with a directive to increase lending broadly in the face of a crisis.

This paper proceeds as follows. Section 1.2 provides background on Brazil's banking sector and the 2008-2010 financial crisis. Section 1.3 describes the data and the empirical strategy. Section 1.4 presents the main results. Section 1.5 explores variations of the main results and offers a series of robustness and specification checks. Finally, Section 1.6 concludes. We include a simple conceptual framework in the appendix for why government banks might allocate credit differently than private-sector banks and how this might affect employment and GDP.

1.2 Brazil's Banking Sector and the 2008-2010 Financial Crisis

Approximately one-third of Brazil's nearly twenty thousand bank branches belong to federal government banks. These include Banco do Brasil, Caixa Economica Federal, or one of several federally-owned regional banks created in the mid-1900s to stimulate regional economic development. Prior to 1997, Brazil also had an expansive system of bank branches owned by individual state governments. Almost all of these state-owned banks were privatized during a consolidation of Brazil's financial sector between 1997 and 2006. Even after this consolidation, government banks continued to account for approximately 45% of total bank assets in Brazil (Barth, Caprio, and Levine, 2004).

State-owned and federally-owned banks in Brazil functioned largely as substitutes. State banks existed in the wealthier states, whereas federally-owned banks had the greatest presence in historically underdeveloped states lacking resources to establish their own banks. With the privatization of state banks beginning in 1997, bank branches that used to be state-owned in wealthier states were transferred to private ownership. Federally-owned banks were never privatized. By 2008, prior to the onset of the financial crisis, this wave of state-bank privatizations and the absence of any privatization of federal banks left Brazil

with bank branches either privately-owned or federally-owned, with many localities having a bank branch of a particular type (private or government) for reasons unrelated to their underlying economic characteristics.

We exploit the variation in bank ownership across localities to estimate how bank ownership during the 2008-2010 financial crisis affected local lending, production, and employment. Figure 1.4 shows the extent of variation in the share of bank branches that are government-owned across localities in Brazil. Some localities have a high share of government-owned bank branches and some have a low share despite having similar economic characteristics. We believe that locations of government and private bank branches do not correspond entirely to local economic conditions, but we use locality fixed-effects and propensity-score matching in the estimates to mitigate potential endogeneity between a location's bank ownership and its underlying characteristics.

With the onset of the financial crisis in September 2008, localities with a high share of government banks experienced increases in lending whereas those with a low share did not. The next section outlines the available data and the empirical strategy.

1.3 Data and Empirical Strategy

1.3.1 Sample

Brazil has 5,565 municipalities as of 2010, which can be combined into 3,659 spatially constant units. These 3,659 units reflect the 1970 municipal borders, which are roughly equivalent in size to a U.S. county. Collapsing the municipalities into 3,659 spatially constant units since the 1970 serves several purposes: first, it allows us to use controls dating back to 1970; second, it more closely reflects spatial areas corresponding to a common area labor market; and third, it mitigates potential issues of firms obtaining loans from outside their municipal borders.

Our analysis focuses on four federally-owned banks and 115 privately-owned ones, which

are together responsible for over 18,000 bank branches in Brazil. We do not include banks owned by individual state governments in our analysis since almost all of these had been privatized by the time of the financial crisis and since they did not enjoy the same soft-budget constraints and explicit backing as federally-owned banks[3].

We exclude from our analysis localities that do not have any bank branches, which tend to be sparsely populated and remote. Our base sample is therefore comprised of 2,601 localities with at least one bank branch, although we experiment with alternative samples. Summary statistics for our base sample are provided in Table 1.1. On average, localities have over 60,000 residents in 2000 and are almost 70% urbanized. Immediately prior to the crisis, they had approximately 15,000 formally-employed workers, 1,100 formal-sector firms, contributed R$1.05 billion to Brazil's GDP, and had over 7 bank branches, of which, on average, 53% were government-owned. Between 2005 and 2007, the average annual locality GDP growth was nearly 5%, employment growth was over 7%, and credit growth was over 22%.

1.3.2 Data

This paper combines data on bank branch locations, municipality-level lending, bank balance sheets, and employment censuses. The bank data were provided by the Central Bank of Brazil, and the employment censuses are from Brazil's Ministry of Labor. Data from the Central Bank document the locations of all bank branches - those currently in operation and those that have ceased operation - for every year since 1900 to the present. This allows us to capture a snapshot of the spatial distribution of bank branches at the onset of the financial crisis and to see historical trends in branch openings and closures prior to the crisis.

[3]We also omit Brazil's National Development Bank (BNDES) from the sample of government banks, for two reasons: BNDES does not have bank branches, and it does not engage in retail lending. The majority of its lending prior to and during the financial crisis is to large, national firms. These firms operate outside the scope of local credit markets, and it would therefore be difficult to attribute local economic fluctuations in lending and economic outcomes to BNDES. Moreover, BNDES does not account for a large fraction of overall lending in Brazil. While it is an important source of long-term lending, it accounts for only 11.6% of total direct lending in Brazil immediately prior to the financial crisis.

Monthly locality-level lending is available from 1989-2012. This data, combined with bank branch locations, allow us to determine the number of bank branches in a locality, the fraction of branches that is government-owned, and the aggregate monthly lending in a locality. We create two measures to reflect the degree to which government banks operate in a locality. The primary one is simply the fraction of bank branches in a locality that is government-owned, although we also experiment with a dummy variable that is equal to 1 if the locality has above the median fraction of government-owned bank branches.

Monthly bank-level balance sheets, aggregated for all of a bank's branches for all of Brazil, allow us to look at aggregate lending of government versus private banks. Using this information, we see that private banks reduced lending and government banks did not at the onset of the financial crisis. Additionally, we can look at banks' liabilities to determine whether the reason for the reduction in lending is due to a reduction in deposits or to a change in the fraction of deposits lent.

To measure the local economic impact of the 2008-2010 financial crisis, we utilize the Brazilian yearly employment census, Relacao Anual de Informacoes Sociais (RAIS). The RAIS identifies all employees on the payroll of formal sector firms as well as the self-employed who pay into the social security system. The data cover approximately 2.5 million establishments and 36 million workers. It is well known that the informal sector in Brazil is non-trivial, with 30% of the overall workforce being informal and the average locality having 34% of its workers in the informal sector, based on 2000 census data. We therefore view our results as a reflection of how lending affects formal-sector employment outcomes in Brazil, although, based on a limited dataset, gains in formal and informal-sector employment are highly correlated [4].

As an additional exercise, we classify localities based on their economies' dependence on external sources of financing, as done by Rajan and Zingales (1998) and Gozzi and Goetz

[4]Using the *Pesquisa Nacional por Amostra de Domicilios (PNAD)*, an annual panel of 817 representative municipalities from 2001-2009, we estimate the elasticity between formal and informal sector employment controlling for year and municipality fixed effects. The estimate (standard error) of the elasticity is 0.024 (0.010).

(2010)[5]. We use a measure of external financial dependence for U.S. economic sectors, match U.S. and Brazilian sectors, and then compute for each locality an employment-weighted average of external financial dependence depending on the locality's pre-crisis level of employment in different economic sectors. We also experiment with the share of small firms (those with 1 to 19 employees) in a locality as a measure of government bank dependence as discussed in section 1.5.1. The intuition, based on Braun and Larrain (2005) and Kroszner, Laeven, and Klingebiel (2007), is that localities whose economies' are heavily dependent on external finance should experience greater changes in employment and GDP as a result of the financial crisis, and in these areas, government banks may play a greater role in mitigating recessions.

Finally, information on locality-level GDP and control variables including measures of urbanization, education, income, population, and exports all come from Brazil's Institute of Applied Economic Research (IPEA).

1.3.3 Quasi-Random Distribution of Bank Branches and Matching

A key assumption of this paper is that whether localities have government or private-sector bank branches is random conditional on fixed locality characteristics. Once we control for observable locality characteristics, whether localities have government bank branches should be uncorrelated with potential economic outcomes. If a locality characteristic, for which we had not controlled, were correlated with both the presence of government bank branches and greater economic resilience to fluctuations in lending, then the observed results might be overly attributed to the presence of these government banks branches.

To minimize this potential for omitted variable bias, we employ several corrections. One

[5]As in Rajan and Zingales (1998) and Gozzi and Goetz (2010), external financial dependence is defined as investment that cannot be financed through internal cash flows generated by the firm. It is capital expenditures minus cash flows from operations divided by capital expenditures. Cash flow from operations is broadly defined as the sum of cash flows from operations plus decreases in inventories, decreases in receivables, and increases in payables. We use data compiled by Gozzi and Goetz (2010) on external financial dependence for U.S. firms based on Compustat data from the 1990s. Using their measure of an industry's dependence on external finance, aggregated from firm-level data up to the 3-digit NAICS sector, we match to Brazilian data based on CNAE codes. We then use the share of a locality's employment in these CNAE sectors to compute measures of external financial dependence for each locality in our sample.

is to include locality fixed effects in our estimates. This controls for any fixed locality characteristic that might influence both a locality's bank branch composition and economic outcomes during a financial crisis. The drawback of including locality fixed effects is that we cannot obtain estimates from time-invariant characteristics in the estimates. Another approach is to match localities based on the propensity to have more than the median share of government bank branches immediately prior to the crisis. We can then obtain a single measure-the propensity score-and match localities with similar characteristics based on this measure. Using this approach, localities have similar characteristics, but some have a high share of government-owned bank branches while others do not. In some specifications, we control directly for the propensity score in addition to fixed locality characteristics.

We calculate the propensity score as a function of the following locality characteristics taken from 2000 census data: years of education, urbanization rate, illiteracy rate, average per capita income, and the natural logarithms of population, total locality income, total locality employment, a measure of total locality human capital, and several interactions of these. Details of the estimation are provided in an appendix. Within each propensity score block, we cannot reject at the 5% significance level that at least 95% of the covariates are statistically indistinguishable across localities.

We estimate whether we can predict a locality's share of government-owned bank branches based on locality characteristics. Table 1.2 shows results from regressing the share of a locality's bank branches that are government owned on the locality's urbanization rate, years of education, shares of industry, services, and agriculture in GDP, average annual GDP and employment growth, and the natural logarithms of total employment, population, GDP, and exports. From column (1), we see that localities that are more urbanized, more educated, less populous, and have higher GDP-essentially, localities that are more developed-have a lower fraction of government-owned bank branches. In column (2), we control for the propensity score. While the propensity score is significant-higher propensity scores are correlated with higher shares of government-owned bank branches-none of the locality characteristics is significant. Once we control for the propensity score, locality characteristics no longer have explanatory power in predicting a locality's fraction of

government-owned bank branches. In column (3) we control for 18 propensity score block dummies. Again, locality characteristics are insignificant. We take these results to imply that once we control for the propensity score, or once we match localities based on the propensity score, the distribution of bank branches is essentially random and therefore uncorrelated with other locality characteristics that may determine economic outcomes during a financial crisis.

1.3.4 Empirical Strategy

Reduced-Form Estimates

Our goal is to assess, first, whether government banks behave differently than private-sector banks and why this may be the case, and second, whether localities with a greater share of government bank branches experience different outcomes in lending, GDP, employment, and income during the 2008-2010 financial crisis.

Using aggregate bank data for all of Brazil, we estimate the following equation:

$$y_{it} = post_t + post_t \times govbank_i + post_t \times X_i + \lambda_i + \tau_t + \varepsilon_{it} \tag{1.3.1}$$

where y_{it} is alternately the natural logarithm of lending or deposits or the share of deposits lent by bank i in month and year t, $post_t$ is a dummy variable equal to 1 for the crisis and post-crisis period, $govbank_i$ is a dummy variable equal to 1 if the bank is one of the four government-owned banks operating in Brazil, X_i are fixed bank characteristics, λ_i are bank fixed-effects, τ_t is time (in this case, months) relative to the onset of the financial crisis, and ε_{it} is the error term. Controlling for $post_t \times X_i$ allows banks with different fixed characteristics to experience differential changes in the post period regardless of whether they are government or privately owned. We are interested in coefficient estimates on $post_t$ and $post_t \times govbank_i$, which respectively tell us how lending, deposits, or the share of deposits lent changes during the crisis, and how this change differs for government-owned

relative to private-sector banks[6]. Standard errors are clustered at the bank level, although we experiment with clustering at the month-year level.

We use a similar empirical strategy, differences-in-differences, to estimate the effects of government bank ownership at the locality level. When estimating effects at the locality level, our data are now annual, and instead of just a binary treatment dummy, we allow for the intensity of treatment to vary depending on the fraction of bank branches in a locality that is government-owned immediately prior to the crisis. When examining locality-level banking, y_{it} is alternately the natural logarithm of lending or deposits or the share of deposits lent in locality i at time t. When we are examining locality-level economic outcomes, y_{it} is alternately the natural logarithm of locality GDP, employment (both gross employment and hours), wages, or number of establishments. As measures of government bank involvement, $govbank_i$ is alternately the fraction of government-owned bank branches or a dummy variable equal to 1 if the locality has above the median share of government bank branches[7]. In some specifications, we include interactions of $post_t$ with the propensity score calculated in Section1.3.3, and in the robustness checks, we include additional interactions with fixed locality characteristics; this allows localities with certain characteristics to experience differential level changes in the post period irrespective of their shares of government bank branches. When estimating using annual data, τ_t is the number of years relative to the onset of the financial crisis. Including τ_t in the estimation detrends the data and allows us to capture effects relative to an overall trend. Of interest in equation (1.3.1) are the coefficient estimates on $post_t$ and $post_t \times govbank_i$, which respectively tell us what happens to the outcome variable in the post period and how this change from the pre to post-period is different for localities with higher shares of government-owned bank branches. For these

[6]In addition, we report estimates from the following equation using random-effects:

$$y_{it} = govbank_i + post_t + post_t \times govbank_i + X_i + post_t \times X_i + \lambda_i + \tau_t + \varepsilon_{it}$$

This equation allows us to estimate a coefficient on $govbank_i$, which is subsumed in the fixed-effects when estimating equation (1.3.1). The assumption for random-effects estimates to be valid is that the error term is uncorrelated with $govbank_i$ conditional on the other regressors.

[7]We also experimented with $govbank_i$ being a dummy equal to 1 if the locality had at least one government-owned bank branch. The issue in doing this is that there are few large and developed localities that do not have at least one government-owned bank branch, which makes it difficult to argue that our treatment and control localities are otherwise similar.

estimates, standard errors are clustered at the locality level, although we experiment with clustering at the state-year level.

We estimate versions of equation (1.3.1) where we alternately include interactions or split the sample to capture a locality's degree of external financial dependence as well as political alignments with the federal government. This allows us to discern whether the effects of having a higher share of government bank branches is greater for certain types of localities.

Instrumental Variables Estimates

We also employ an instrumental variables procedure to estimate the elasticity between locality-level lending and GDP, employment indicators, and number of establishments. We exploit the variation in government bank shares at the locality level and instrument for post-crisis changes in lending with pre-crisis shares of government bank branches. The intuition is that having a higher share of government bank branches should be strongly correlated with changes in lending but should not otherwise affect outcomes such as GDP, employment, and number of establishments except through the channel of lending. The validity of the instrument relies on controlling for how localities with different estimated propensities of having a high share of government bank branches would experience different changes in these outcome variables irrespective of their actual bank ownership structure. After controlling for this estimated propensity score, the share of government bank branches in a locality should be strongly correlated with pre-to-post crisis changes in lending but should be otherwise orthogonal to changes in GDP, employment, and establishments. The structural equation of interest is:

$$lny_{it} = lncredit_{it} + post_t + post_t \times X_i + \lambda_i + \tau_t + \varepsilon_{it} \tag{1.3.2}$$

where y_{it} is alternately locality GDP, measures of employment, or number of establishments in locality i at time t, $credit_{it}$ is total credit operations, and as before, $post_t$ is a dummy variable equal to 1 for the post-crisis period, X_i are fixed-locality characteristics such as the

estimated propensity score for having above the median share of government bank branches, τ_t is a time trend, λ_i are locality fixed effects, and ε_{it} is the error term. We instrument for $lncredit_{it}$ with $post_t \times govbank_i$, where $govbank_i$ is the pre-crisis fraction of bank branches in a locality that is government-owned, although we also experiment with $govbank_i$ being a dummy equal 1 if the locality has above the median share of government bank branches. In an alternate specification, we include $X_i \times \tau_t$ and $post_t \times X_i \times \tau_t$ as exogenous controls in equation (2) to allow for the possibility that localities with different characteristics follow different pre and post-crisis trends irrespective of their banking structure, and we instrument for $lncredit_{it}$ using $post_t \times govbank_i$ as well as $post_t \times govbank_i \times \tau_t$.

A complementary approach is to analyze the pre-to-post crisis change in average lending and economic outcomes. We calculate the difference in average $lncredit_{it}$ and average lny_{it} for 2005-2007 and 2008-2009 and estimate the following equation:

$$\Delta y_i = \Delta lncredit_i + X_i + \varepsilon_i \tag{1.3.3}$$

where $\Delta y_i = ln\bar{y}_{i,2008-2009} - ln\bar{y}_{i,2005-2007}$ and $\Delta lncredit_i = lncr\bar{e}dit_{i,2008-2009} - lncr\bar{e}dit_{i,2005-2007}$. We instrument for $\Delta lncredit_i$ with $govbank_i$, where $govbank_i$ is alternately the fraction of bank branches government-owned or a dummy variable if the locality has above the median share of government bank branches. In the results section, we present several estimates of equations (2) and (3) using different instrumenting strategies.

1.4 Results

In this section, we present results based on the empirical strategy. We first discuss bank-level monthly lending results based on data from Brazil's retail banks with operations during the two years prior through one year following the onset of the crisis. We next discuss locality-level lending, GDP, and employment results. Finally, we discuss variations of the locality-level results based on localities' political alignments and external financial dependence; assess the quality of government bank lending during the crisis; and provide

some robustness checks.

1.4.1 Reduced-Form Bank-Level Results

To assess how banks in Brazil operated during the financial crisis, Table 1.3 shows results from estimating equation (1.3.1). Total credit operations - which include traditional lending as well as lease financing and lines of credit - decline in the post period by approximately 15% relative to previous trends for private-sector banks. For government-owned banks, total credit operations actually increase by about 9% to 14%, as shown in columns (1) and (2). This translates into a difference in the post-crisis period of 24% to 29% between the total credit operations of private-sector and government-owned banks. These results mirror the trends in Figure 1.1. The specifications of columns (1) and (2) control for bank fixed-effects, and in addition, column (2) interacts the $post_t$ dummy with fixed bank-level characteristics to control for the possibility that banks with different characteristics would have experienced different changes in credit operations independent of the financial crisis[8].

One concern is that banks are of vastly different sizes, and specifically, that the average government bank is much larger than the average private-sector bank. While the inclusion of $post_t \times X_i$ allows banks of different sizes to experience different post trends, the estimates still treat every bank equally. In columns (3) and (4), we weight the estimates by banks' total assets in the pre-crisis period, so that banks with a higher level of pre-crisis lending receive more weight. It is still the case that government-owned banks lend more in the post period, between 19% and 28% more, compared to large private-sector banks. We investigate these results further and separate banks into government-owned, large private, and other private banks[9]. Even in the specification where we include $post_t \times X_i$, we obtain estimates that suggest that government-owned banks lend more than large private banks during the

[8]The fixed bank-level characteristics, calculated as of August 2006, are the natural logarithms of total credit and total liabilities, a measure of portfolio quality calculated as a weighted average of credit ratings on loans, and banks' capitalization ratios, all standardized to have mean 0 and standard deviation 1.

[9]Large private banks include Itau/Unibanco, Bradesco, Santander, and HSBC.

financial crisis (results not shown) [10] [11].

The differential changes in credit operations between government and private-sector banks during the financial crisis could be the outcome of changes in behavior or of changes in the amount of loanable funds. Columns (5)-(8) of Table 1.3 show what happens to a bank's total liabilities, which includes retail deposits as well as interbank borrowing and commercial paper issuances. Total liabilities declined by approximately 8.0% in the post period, relative to previous trends, and this decline is not statistically different between private and government-owned banks. Even when we weight the estimates by banks' initial liabilities, we do not obtain significant differences for the change in liabilities between government and private banks during the post period.

The relative increase in government banks' credit operations, shown in columns (1)-(4) is not due to a relative increase in the availability of loanable funds. As columns (9)-(12) show, whereas private-sector banks reduced their share of liabilities lent by up to 2.1% relative to previous trends, government banks increased their share of liabilities lent by 5.1% to 6.4%. This is a significant difference of 6.6% to 8.5% in the share of liabilities lent.

Government banks behaved differently from private-sector banks following the onset of the financial crisis. While we cannot separate whether this difference in behavior is due to differences in risk aversion, outlook, or potential loan losses, we can argue that it leads government banks to increase credit operations during the crisis. We now examine how these differences in national-level bank behavior translate into local-level outcomes.

[10] When we look at a previous crisis period, immediately following the Asian financial crisis in 1997-1998, we find a similar pattern of lending at the national level, but not as stark, between government and private banks. And when we conduct a placebo exercise by randomly assigning years between 2000 and 2007 as counterfactual crisis years, we find no difference in government and private bank lending relative to previous trends, as would be expected (results not shown).

[11] It is widely known that government banks earmark credit for specific types of projects. While our data does not specifically identify credit that is earmarked, we re-estimate the credit results omitting categories most likely to have a high share of earmarked credit, such as housing, agriculture, and infrastructure. Omitting these categories does not significantly alter our results.

1.4.2 Reduced-Form Locality-Level Results

The credit results discussed in the previous subsection are based on national aggregates. In this subsection, we first examine whether these credit results also hold when we look at locality-level credit operations. An issue here is that for localities with more than one bank branch, we cannot ascribe credit operations to a particular type of bank, i.e., government or private. Instead, we assess whether localities with a higher share of government bank branches experience different credit outcomes following the crisis. We do this collapsing the monthly data into annual averages to make them comparable to our annual data on GDP, employment, and firms. We then show and discuss results on employment and GDP at the locality-level before proceeding to variations of these locality-level results.

Credit Operations and Liabilities

Table 1.4 shows results from estimating equation (1.3.1) using locality-level data on average annual credit operations. As shown in columns (1) and (2), the average locality without any government bank branches experienced declines in lending of approximately 46% relative to previous trends following the onset of the financial crisis. Every ten percentage point increase in the share of bank branches that is government owned mitigates these declines by 6.6%. The inclusion of $post_t \times X_i$ in column (2) allows for the possibility that localities with different initial characteristics would experience different outcomes in the post-period irrespective of their shares of government bank branches. For the locality estimates, we use the propensity score calculated in Section 1.3.3 as the sole control variable in X_i, although we also experiment in the robustness checks with an extensive list of locality characteristics as additional controls.

In columns (3) and (4), we weight the estimates by a locality's total population in 2000. This more closely reflects the outcomes for where the average person lives; without weighting, each locality carries the same importance in the estimation regardless of whether they are minimally populated or major population centers. Based on results from these weighted

regressions, we obtain that total credit operations declined between 14% and 20% following the onset of the crisis, but every ten percentage point increase in the share of government-owned bank branches mitigated these declines by 1.8% to 2.6%. When estimating equations (1)-(4) with $govbank_i$ being a dummy variable equal to 1 if the locality has above the median share of government-owned bank branches, the results are similar (shown in Panel B). For the average locality, having a high share of government-owned bank branches increases total credit operations during the financial crisis by 40% to 42% relative to previous trends; and in the population-weighted estimates, these increases are 7% to 9%. The impact of the crisis on total credit operations and the mitigating effects of government-owned bank branches were smaller in more populous areas, but the effects were nonetheless present and highly significant [12].

While we do not report locality-level results for total liabilities and share of liabilities lent, we obtain results that correspond to the bank-level ones discussed previously. Localities with a high share of government-owned bank branches experience increases in total credit operations because they experience increases in the share of liabilities lent during the financial crisis.

Employment and GDP

Localities with a high share of government-owned bank branches likewise experience less severe declines in employment and GDP following the onset of the financial crisis. Table

[12] As a matter of interpretation, there are two potential responses by government banks that are consistent with these results. First, government banks may be responding differently than private banks to financial crises. Second, government banks may be responding to the decrease in credit provided by private banks. To address these interpretations, we divide our sample into three- one has localities with only private bank branches, one has localities with only government bank branches, and one has localities with both government and private bank branches - and we compare how post-crisis lending differs from pre-crisis lending. For localities with only private bank branches, lending declines relative to previous trends during the post-crisis period. In localities with only government bank branches, lending does not decline relative to previous trends. And in localities with mixed bank branch composition, lending declines but not as much as in localities with only private bank branches (results not shown). If government bank branches were fully compensating for the decline in lending by private bank branches in these localities, their post-crisis lending would have followed the same trajectory as in localities with only government bank branches. This is not the case. While we cannot rule out that government bank branches react to the lending provided by private bank branches, we can rule out that they react fully by increasing their lending to exactly compensate for the decline in lending by private banks.

1.5 shows results for the effects of having a high share of government-owned bank branches on GDP, industrial value-added, and services value-added at the locality level. Depending on the specification, declines in GDP vary from almost zero to 1.6% (1.6% to 2.6% in the population-weighted regressions) in localities with no government-owned bank branches, and declines are mitigated by 0.34% to 0.57% (0.51% to 0.65% in the population-weighted regressions) for every ten percentage point increase in the share of bank branches that is government-owned. These results are shown in columns (1)-(4). The inclusion of $post_t \times X_i$ in the estimates potentially absorbs some of the post effects and some of the effects of government bank ownership on the outcome variable during the post-period, leading us to underestimate the true coefficients on both $post_t$ and $post_t \times govbank_i$. However, to the extent that localities with different characteristics may experience different post effects for reasons correlated with the presence of government bank branches, excluding $post_t \times X_i$ would cause us to overestimate the true coefficients on $post_t$ and $post_t \times govbank_i$. The estimates we obtain provide plausible lower and upper bounds of the true effects, and even the most conservative of these suggests that government bank branches mitigated contractions in local GDP.[13].

GDP is calculated as the sum of value added in industry, services, agriculture, and government. Industry and services together account for 75% (81% when population-weighted) of localities' economies. Although the coefficients are more noisily estimated when we disaggregate GDP, industrial value-added would have declined by 0.8% to 2.5% and services value-added would have declined by 1.8% to 2.7% following the onset of the financial crisis, but these declines were reversed for areas with a high share of government bank branches. For every ten percentage point increase in the share of government-owned bank branches

[13]When we estimate using random effects, we obtain a coefficient (standard error) of 0.041 (0.065) on the dummy variable for having above the median share of government-owned bank branches. The coefficient estimates on and are statistically identical to those for the corresponding fixed effects specification in Table 1.5, Panel B, column (2). The random effects specification includes controls for the propensity of localities to have above the median share of government-owned bank branches as well as this propensity score interacted with $post_t$. The fact that the coefficient estimate on $govbank_i$ is not significantly different from zero suggests that once we control for the propensity score, localities with high versus low shares of government-owned banks have similar pre-crisis levels of production. Moreover, the fact that estimates on $post_t$ and $post_t \times govbank_i$ hardly change between the random and fixed effects specifications suggests that having a high share of government-owned bank branches is orthogonal to fixed locality characteristics after we control for the propensity score.

in a locality, industrial value-added is 0.14% (insignificant) to 0.45% higher and services value-added is 0.22% to 0.39% higher.

The mitigating effects of government bank branches on economic outcomes are reflected in employment numbers, as shown in Table 1.6. Employment declines by 3.2% to 3.7% in the post period relative to previous trends for areas with no or only a low share of government bank branches. The mitigating effects of having all or a high share of these branches in a locality are on the order of 1.8% to 3.8%, as shown in columns (1)-(4), although depending on the specification, they are not always significant at conventional levels. When examining only private sector employment and when examining total hours and wages, however, we obtain significant coefficient estimates on $post_t \times govbank_i$[14]. Declines in total hours range from 3.9% to 4.8% in the post-period, but having all government bank branches or a high share of government bank branches mitigates these declines by 1.5% to 5.9%. Similarly, when analyzing the total wage bill of a locality, the positive effects of having a high share of government bank branches during the post-crisis period are large and significant, especially for more populous localities.[15].

Finally, as shown in Table 1.7, the number of establishments declines between zero to 3.2% relative to previous trends for areas with no or only a low share of government bank branches. Every ten percentage point increase in the share of government bank branches mitigates these declines by 0.15% to 1.0%. These effects of government-owned bank branches on the number of establishments are highly significant and are larger for more populous localities.

We can break down these results on employment, hours, wages, and firms across 17 sectors for each of the 2,601 localities[16]. We re-estimate equation (1.3.1) for these variables and

[14]When examining private-sector employment, the estimates (standard errors) on $post_t \times govbank_i$ range from 0.024 (0.017) to 0.040 (0.023) when $govbank_i$ is the fraction of bank branches government-owned and when the estimation includes $post_t \times X_i$. We fail to obtain significant estimates when examining public-sector employment.

[15]Estimation with random effects yields the following coefficient estimates (standard errors) on the dummy variable for having a high share of government-owned bank branches when the dependent variables are respectively the natural logarithms of employment, hours, wages, and number of firms: 0.038 (0.066), 0.024 (0.067), 0.024 (0.073), and 0.021 (0.063), implying that after controlling for the propensity to have a high share of government-owned bank branches, localities with a high share of these branches do not have different levels of employment, total working hours, wages, or firms during the pre-crisis period compared to localities with a low share of government-owned bank branches.

[16]We unfortunately do not have locality-level data on GDP or lending disaggregated across these sectors.

include locality-by-sector fixed effects. In doing so, we are comparing what happens within a specific sector to employment, hours, wages, and firms in localities with high versus low shares of government-owned banks. Results are shown in Table 1.8. The coefficient estimates suggest that sectors in localities with high shares of government bank branches experience relative increases in employment, hours, wages, and number of establishments compared to the same sectors in locations with low shares of government bank branches. Coefficient estimates are not always significant, especially when the estimation includes $post_t \times X_i$, although the inclusion of $post_t \times X_i$ in addition to locality-by-sector fixed effects saturates the estimation. The results in Table 1.8 nonetheless provide some assurance that the reason localities with higher shares of government bank branches experience better outcomes in the post-crisis period is not simply because they having different economic compositions. Even within the same economic sectors, localities with higher shares of government bank branches perform better during the financial crisis.

A related exercise is to examine how neighboring localities perform in the post-crisis period given their differences in shares of government bank branches. Neighboring localities face similar climatic and agronomic conditions, have similar access to infrastructure such as ports and railways, and are in the same state (we exclude neighboring localities across state lines). However, labor and some lending may flow across locality boundaries, confounding the results, even though lending is highly localized in Brazil[17] When we estimate a version of equation (1.3.1) where our variables of interest are $post_t$ and $post_t \times \Delta govbank_i$, and $\Delta govbank_i$ is the difference in shares of government bank branches between neighboring localities, we obtain the results shown in Table 1.9. These estimates include fixed effects for 5,841 locality pairs covering 2,562 localities[18]. The results are largely in line with those discussed previously: in the post-crisis period, neighbors with a greater share of government-owned bank branches experience larger relative increases in lending, GDP, employment, and number of establishments.

[17]Since Brazil lacks central credit registries, information on the credit worthiness of borrowers is based on borrowers' long-standing relationships with their banks. This limits the ability of borrowers to obtain loans from financial institutions with whom they do not normally bank.

[18]Localities with neighbors that do not have a bank branch are dropped from the estimation.

These reduced-form results collectively suggest that the increased lending provided by government-owned banks during the financial crisis not only propped-up production and prevented a greater number of firms from failing, but also buttressed workers' labor hours and income.

1.4.3 Instrumental Variables Results

Using as an instrument the variation in the shares of government bank branches across localities, we directly estimate the relationship between lending and economic outcomes such as GDP, measures of employment, and number of firms in the post-crisis period. Results are shown in Table 1.10. Columns (1)-(5) contain estimates from an over-identified instrumentation of equation (1.3.2), where we instrument for $lncredit_{it}$ using both $post_t \times govbank_i$ and $post_t \times govbank_i \times \tau_t$. Columns (6)-(10) contain estimates from the cross-sectional version in equation (1.3.3) , where we instrument for $\Delta lncredit_{it}$ using only the fraction of bank branches in a locality that is government-owned. The results suggest that the elasticities between lending and GDP, hours worked, and number of firms are all approximately 0.05. Since relative increases in lending are on the order of 66% for localities where all bank branches are government-owned, these estimates translate into increases of approximately 3% in GDP, hours worked, and number of firms. Results are insignificant for total employment and wages. The magnitudes of these results are equivalent to those obtained in corresponding reduced-form estimations.

We experiment with using a dummy variable for whether localities have above the median fraction of government-owned bank branches and with using different combinations of instruments to estimate equations (1.3.2) and (1.3.3). We plot coefficient estimates and confidence intervals on the elasticities from these different instrumentation strategies in Figure 1.5. Estimates are stable, although setting equal to a dummy variable as opposed to a continuous fraction leads to larger standard errors and causes some estimates of the elasticity between lending and hours worked to become insignificant. These instrumental variables estimates reinforce our previous results that increases in lending tend to increase

GDP and prop up firms, and have positive but less significant impacts on employment.

1.5 Variations of Locality-Level results, Quality of Loans, and Robustness

In this section, we explore variations on the locality-level results presented in Section 4, assess loan quality, and provide some robustness checks. Specifically, we are interested in whether the increased credit provided by government-owned banks flows to where it might be needed most and where it might be most productive, or whether these flows are determined by political considerations. Additionally, we examine banks' balance sheets to assess whether government-owned banks experience a deterioration in their loan portfolio as a result of extending credit during the crisis. We also perform some robustness checks by trimming our sample to ensure that our results are not driven purely by outliers. Finally, and more speculatively, we conduct an exercise to assess what happens to local productivity, as measured by the Solow residual, as a result of government bank lending. While the presence of government banks may attenuate the recessionary effects of the financial crisis, it may also prevent Schumpeterian creative destruction and the reallocation of resources to more productive firms.

1.5.1 Local External Financial Dependence

We begin by estimating a variant of equation (1.3.1) that includes interactions with a measure of the fraction of firms in a locality that are dependent on banking or external finance. As discussed previously, we follow Rajan and Zingales (1998) and Gozzi and Goetz (2010), and adapt their measures of external financial dependence to Brazil. This is admittedly an imperfect measure: sectors that cannot fund themselves from retained earnings and are dependent on external financing in the U.S. are unlikely to fully approximate dependent sectors in Brazil, where financial markets are much less developed.

To approximate the degree to which Brazilian firms are reliant on financing from specifically government banks, we utilize the World Bank Enterprise Surveys. The World Bank surveys firm managers to try to understand the economic environment in which the firm operates. Surveys include firm-level characteristics such as sales, employment, industry codings and information on their financing needs and loans. For a subset of countries, these surveys include a question which asks if a firm's most recent loan was from a government or private bank. This question was asked in Brazil in the 2009 survey.

Of the 1,802 firms in the survey, 1,176 firms have (or have had) a loan and 379 received this loan from a government bank. Among the small firms (firms with fewer than 20 employees) that use bank financing, 41% are connected to government banks. Of medium size firms, with 20-100 employees, 32% have government bank loans and among large firms only 19% have government bank loans[19]. There is a monotonically decreasing relationship between firm size and the fraction of firms that use bank financing that are connected to government banks. We therefore proxy for local government bank dependence with the fraction of a locality's firms that have fewer than 20 employees in 2005.

Results are shown in Table 1.11. The coefficient estimates on $post_t \times govbank_i$ are generally positive and significant, and, as before, they suggest that areas with a higher fraction of government-owned bank branches experience relative increases in lending, GDP, hours, wages, and number of firms following the financial crisis. However, the coefficient estimates $post_t \times govbank_i \times$ high external dependence$_i$ or on $post_t \times govbank_i \times$ high frac. small firms$_i$ are generally mixed, even when we do not control for $post_t \times X_i$ and when we weight the estimations by locality population. If government banks targeted lending to localities whose firms were most reliant on external sources of financing, then we should obtain significantly positive estimates, and we do not. Our measures of external financial dependence are imperfect, and we experimented with several alternatives, including a continuous variable to capture the fraction of firms that are small or externally dependent rather than using a dummy variable for whether the locality is above the median in these measures. While we cannot rule out that government banks targeted lending to where it might have had the

[19] Author's calculation

greatest impact, we have no convincing evidence that they followed such a strategy.

1.5.2 Local Political Alignments

To test the political economy motive, we estimate a variant of equation (1.3.1) to include interactions with mayors' political affiliations around the time of the crisis. We find no evidence that credit was targeted based on political connections, despite previous evidence that lending is often allocated politically in Brazil, especially around elections (Carvalho, 2012). We are interested in whether mayors are politically affiliated with the executive branch of the federal government because it is the executive branch that appoints the directors of federally-owned banks. We code a locality as being politically affiliated around the time of the financial crisis if its mayor is from the Worker's Party, which controlled the executive branch, or if its mayor is from a party that is a member of the coalition government[20]. For localities that are comprised of multiple municipalities, we use the electorate-weighted share of mayors that belong to either the Worker's Party or a coalition party. Results are shown in Table 1.12. We report results for whether the locality had elected a politically-affiliated mayor in either the 2004 or the 2008 elections, and we experiment with separating political affiliations in these periods (not shown) rather than considering them together. In general, we do not obtain any significant coefficient estimates on $post_t \times govbank_i \times affiliated_i$. Regardless of whether we control for $post_t \times X_i$, weight the estimates by locality population, or separately consider political affiliations during different election cycles, we fail to obtain robustly significant coefficient estimates to suggest that lending is allocated based on political affiliations. To examine this point further, we look alternately at three subsamples: the 991 localities with one bank branch, for which we can directly attribute lending to either a government or private-sector bank; the 799 localities with only government and no private bank branches; and the 427 localities where Worker's Party or coalition candidates were engaged in close elections [21]. For these three subsamples, there is still no indication that government banks are allocating resources based on political

[20] We consider coalition parties to be those that have a ministerial appointment in the executive branch.

[21] We define close elections as those in which the margin of victory is less than five percent, although we experiment with different cutoffs without significant differences to the results.

affiliations (results not shown). Instead, we find that the coefficient estimates on $post_t \times govbank_i$ remain positive and significant, suggesting that more credit is allocated during the financial crisis to localities with a higher fraction of government-owned banks irrespective of these localities' political affiliations with the federal government.

1.5.3 Loan Quality

Even though government-owned banks provide credit during a time when private banks are reducing their lending, they do not appear to be sacrificing their lending standards. As shown in Table 1.13, while government-owned banks may reduce their capitalization ratios slightly, between zero and 4.3% relative to private-sector banks during the post-crisis period, the quality of their loans actually appears to improve, with borrowers' credit ratings being between 1.4% (insignificant) to 3.9% higher[22].This is consistent with even high-quality borrowers being unable to obtain credit from private-sector banks and instead shifting to government-owned banks. A caveat is that these credit ratings are self-reported, and so it is unclear whether government-owned banks are simply becoming relatively more lax in their ratings, although we have no reason to believe this is the case. There appears to be some evidence that government-owned banks actually reduced their loan loss provisions, especially when we weight the estimates by banks' assets in the pre-crisis period, and there is no significant difference in the post-period between the credit earnings of government relative to private-sector banks, again suggesting that government banks have not significantly relaxed their lending standards in an effort to provide more credit during the crisis.

[22]Banks provide a breakdown of the credit ratings of their loans. We apply a numerical value to these letter ratings and then use these numerical values to derive a weighted average of the overall credit quality of the loan portfolio. If the portfolio is comprised entirely of the highest-rated credits, the "Borrowers' Credit Rating" variable takes the value 1, if it is comprised entirely of the lowest-rated credits, this variable takes the value 0. Banks' capitalization ratio is calculated as total equity capital as a share of total liabilities. Loan loss provisions is capital as a share of total credit operations set aside to cover potential loan losses. Credit earnings is earnings from credit as a share of total credit operations

1.5.4 Robustness to Clustering, Controls, Sample Selection, and Alternative Explanations

Our base case results are robust to alternative ways of clustering, to the inclusion of additional controls, to different sample selection, and to alternative explanations, as shown in Tables 1.14 and 1.15. When we cluster at the state-year level, as opposed to the locality level, standard errors change slightly but without adversely affecting the significance of our coefficient estimates (Table 1.14, Row B) [23]. When we include additional or alternative controls, our estimates remain robust. In Table 1.14, Row C, we control for locality-specific pre-crisis trends; in Row D, we include an interaction between $post_t$ and the fraction of a locality's workers that are public sector employees to address the possibility that areas with more government sector employees receive more lending and experience better economic outcomes irrespective of their bank composition; in Row E, we match localities based on their propensity scores and control for an interaction between $post_t$ and these match fixed effects in lieu of controlling for $post_t \times pscore_i$; and in Row F, we control for the interaction between and a list of fixed locality controls in lieu of controlling for $post_t \times pscore_i$. In all these specifications, we continue to obtain positive and significant coefficient estimates for the effects of government bank branches on lending and firms during the post crisis period, and effects are positive for GDP and employment indicators although we lose significance as we add more controls and lose degrees of freedom. In Row G, we trim the top and bottom 10% of our sample with respect to the propensity score to address the possibility that outliers are driving our results; they are not. In Row H, we collapse spatial units into metro areas. These are areas defined as commuting zones and common labor markets by Brazil's statistics agency (IBGE). The majority of our localities are already unique commuting zones and labor markets, although 441 belong to a larger metro area. Finally, in Row I, we conduct a placebo exercise. We randomly assign localities to have a high or low fraction of government bank branches based on their estimated propensity scores and

[23] To address issues of spatial correlation, we also cluster standard errors at the level of own locality plus immediate neighbors. This assumes errors are correlated between localities that share borders and over time, but are otherwise independently distributed. Neither clustering at the own locality plus neighbor level nor clustering at the state-year level alters the significance of our coefficient estimates.

regardless of their actual bank branch composition. We do this in a way that maintains the number of counterfactual high government bank branch localities equal to the real number of such localities, and then we estimate the effects of having a counterfactually assigned high fraction of government bank branches on outcomes during the post-crisis period. As would be expected, the effects are close to zero.

It is conceivable that there are other government programs that might stimulate local economies that coincide with the presence of government bank branches. To test for this, we estimate equation (1.3.1) to obtain the effects of having a high fraction of government bank branches on government transfers, public sector employment, and construction employment during the post-crisis period. These results are shown in Table 1.15. If it were the case that the federal government was differentially transferring more funds directly to localities that have a greater share of government bank branches, we would obtain positive coefficient estimates on $post_t \times govbank_i$. Depending on the specification, we obtain negative estimates, statistically insignificant estimates, or small positive estimates. We also do not find any evidence that the number of government workers or construction workers was differentially increasing in areas with more government bank branches. The result on construction workers mitigates the possibility that some other type of stimulus program or infrastructure investment program was differentially driving economic outcomes in localities with a high fraction of government bank branches during the post-crisis period.

1.5.5 Productivity

As a final exercise, we attempt to estimate the effect of government-bank involvement on productivity in local economies during the financial crisis. For each locality, for 2000-2009, we estimate the following equation:

$$\ln GDP_{it} = \ln K_{it} + \ln L_{it} + \varepsilon_{it} \tag{1.5.1}$$

where GDP_{it} is the equivalent of gross domestic product in locality i at time t, K_{it} is total credit operations, L_{it} is alternately employment or total labor hours, and ε_{it} is the error term. We obtain coefficient estimates on $\ln K_{it}$ and $\ln L_{it}$, and use these estimates to compute the residuals for each locality for 2005-2009. Note that each locality is constrained to having the same production technology throughout the period, although localities can have a different optimal mix of capital and labor. With the Solow residuals as the dependent variable, we estimate equation (1.3.1). Results are shown in Table 1.16. We find that greater government presence in a locality is associated with a zero to 3.8% increase in productivity, as measured by the Solow residual. It is important to note that these results only reflect short-term outcomes. While the greater presence of government-owned banks may serve to prop-up lending, GDP, and firms, it is unclear whether in the long-term, this prevents structural adjustment in the economy and hampers productivity. In the short-term, however, it does not appear that government-bank intervention led to a relative decline in local productivity; in the most optimistic of estimates, local productivity actually increases during the post-crisis period in areas with the highest shares of government-owned bank branches.

1.6 Conclusion

While the onset of the 2008-2010 financial crisis resulted in a sharp decline in lending, production, and employment in many countries around the world, this decline was comparatively mild in Brazil. This higher lending does not appear to have been allocated politically, although there is also no evidence that it was allocated strategically to areas that are more bank dependent. Instead, it appears that government banks simply lent more in the areas where they operated without necessarily targeting specific localities or sectors.

In areas with high government bank presence, the local economy was disproportionately stimulated. Total credit operations, GDP, labor hours, income, and the number of establishments were all higher in these localities than in corresponding ones with only a low share of government-owned bank branches. Our estimates suggest that economic growth,

incomes, and the number of firms would have declined in Brazil relative to previous trends if not for the involvement of government-owned banks. A counter-argument is that government bank lending may have prevented Schumpeterian creative destruction in the local economy and therefore have hampered productivity growth in the longer term. At least in the short term, within two years following the onset of the crisis, there appears to be no negative effect on an area's productivity as a result of government bank involvement.

These results should be interpreted with some caution. There is ample evidence that government banks are subject to political capture and that their lending can become politically motivated, with detrimental effects to the allocation of productive inputs and financial development (Dinc, 2005; Khwaja and Mian, 2005; Carvalho, 2012; and Barth, Caprio, and Levine, 2001). Even in Brazil, this has previously been the case during the period when individual state governments owned banks (Feler, 2012). Futher, increased government involvement in the banking sector during financial crises affects the market shares of private banks even after the crisis is over which could potentially exacerbate these issues. While federal government banks propped-up the economy in Brazil and prevented a deeper recession from occurring following the onset of the 2008 financial crisis, it is unclear what the longer-term implications of government bank intervention might be. During this crisis, government bank lending had significantly positive and fairly immediate effects on GDP, employment, and incomes, and helped firms remain in business.

1.7 Figures

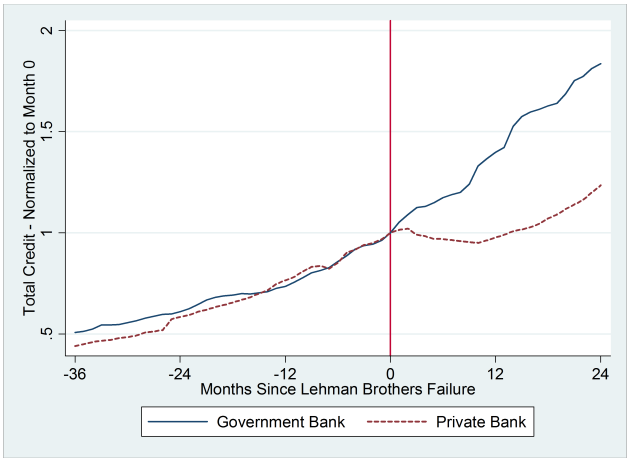


Figure 1.1: Total Credit Operations

Notes: This figure shows the total credit operations normalized to be 1 at the onset of the financial crisis in September 2008. Total credit is based on aggregated balance sheets of four federal government banks and 115 private-sector banks.

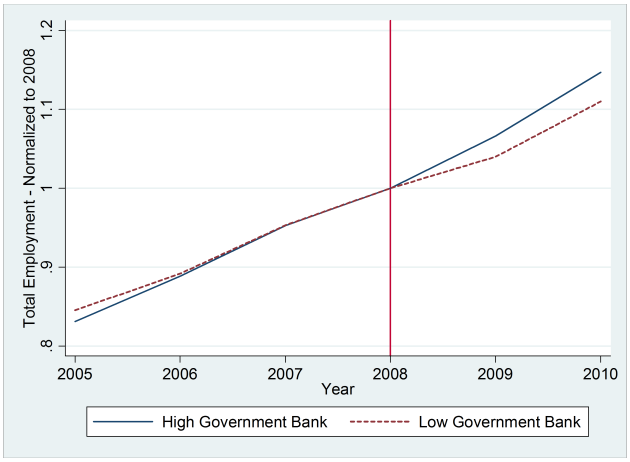


Figure 1.2: Total Employment

Notes: This figure shows locality-level employment normalized to be 1 at the onset of the financial crisis in 2008 for the 2,601 localities with bank branches. High government bank localities are defined as localities that have above the median share of government-owned bank branches.

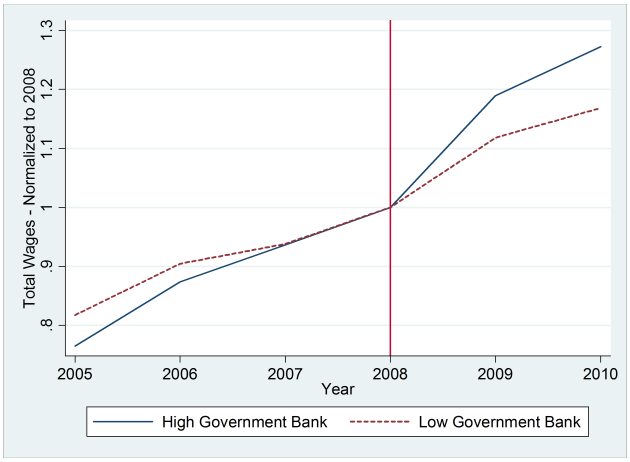


Figure 1.3: Total Wages

Notes: This figure shows locality-level wages normalized to be 1 at the onset of the financial crisis in 2008 for the 2,601 localities with bank branches. High government bank localities are defined as localities that have above the median share of government-owned bank branches.

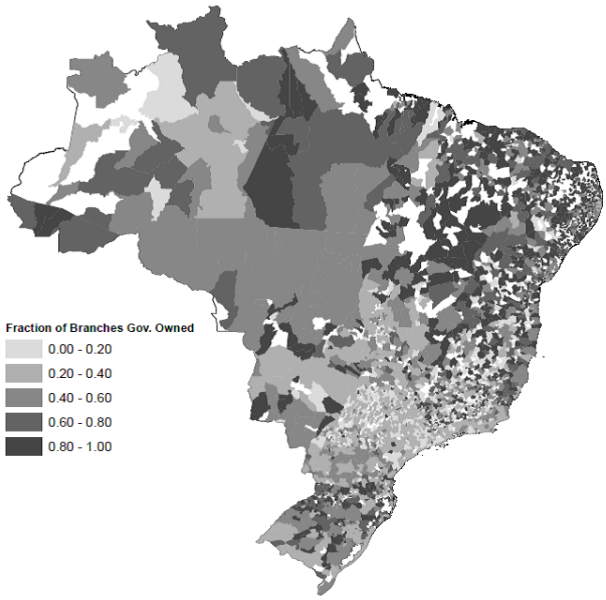


Figure 1.4: Variation in Government Ownership of Bank Branches

Notes: This figure shows the share of bank branches that are government-owned in 2007 for the 2,601 Brazilian localities that have at least one bank branch.

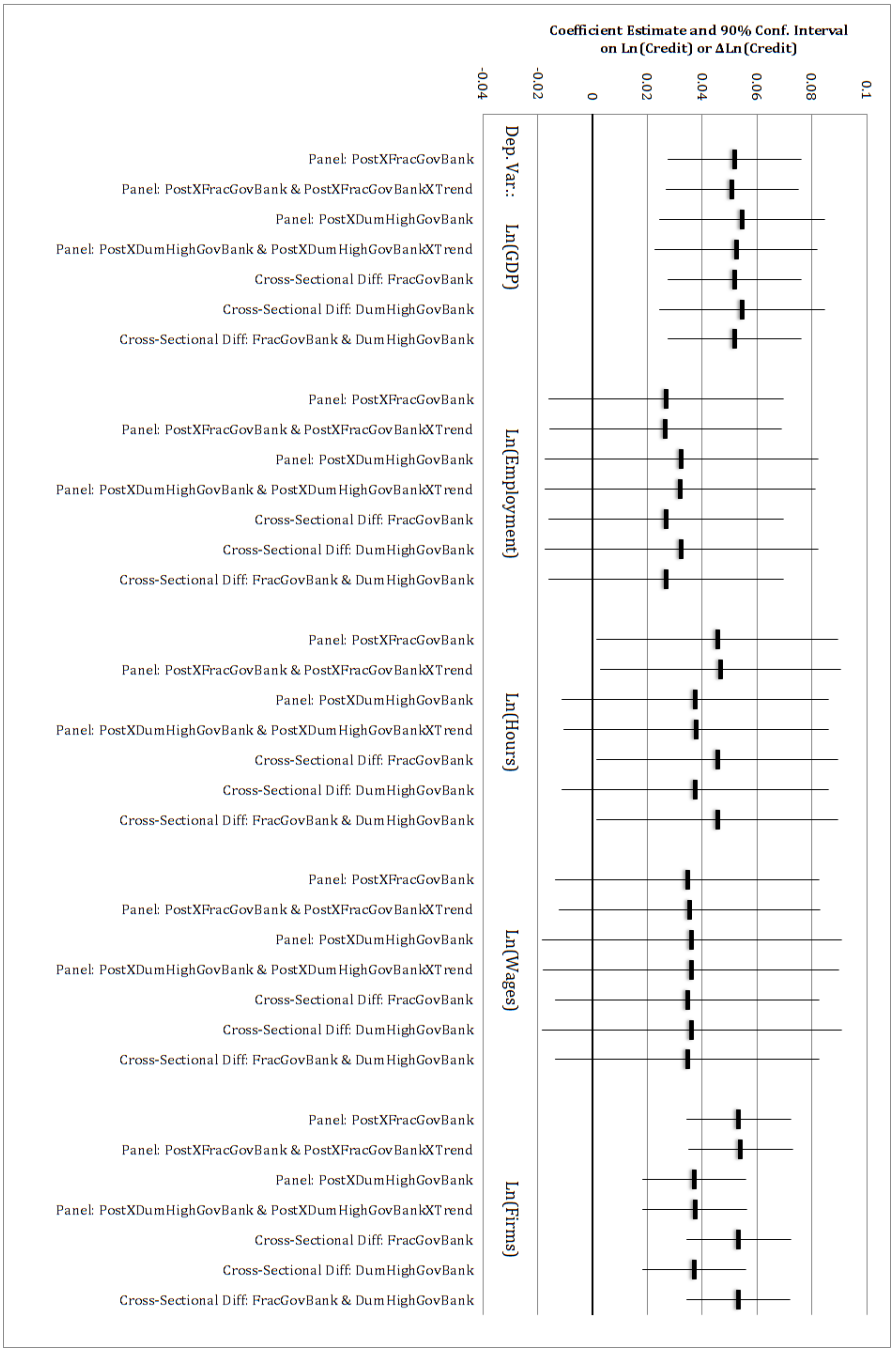


Figure 1.5: IV Estimates and Confidence Intervals

Notes: This figure shows the coefficient estimates and confidence intervals for various instrumental variables specifications.

1.8 Tables

Table 1.1: Summary Statistics

	Mean	Median	Std. Dev.
Bank and Credit Variables:			
Fraction Branches Government Owned	0.53	0.50	0.37
Total Bank Branches	7.6	2.0	65.6
Government Bank Branches	2.5	1.0	11.8
Private Bank Branches	5.1	1.0	54.4
Yearly Real Credit Growth	22.1%	16.6%	41.5%
Locality Variables:			
GDP in 2007 (in R$ millions of 2000)	1,050	81	25,600
Yearly GDP Growth	4.9%	4.4%	7.9%
Yearly Industry Value-Added Growth	4.6%	2.9%	17.9%
Yearly Services Value-Added Growth	5.9%	5.4%	6.2%
Total Employment in 2007	14,218	2,247	105,455
Yearly Employment Growth	7.1%	5.6%	14.3%
Total Hours in 2007 (in 10,000s)	2,288	357	16,613
Yearly Hours Growth	7.5%	6.0%	13.7%
Total Monthly Wages in 2007 (in R$10,000s)	1,866	161	18,748
Yearly Wage Growth	11.5%	8.6%	18.4%
Total Firms in 2007	1,108	256	6,049
Yearly Growth in Number of Firms	5.0%	4.3%	5.6%
Control Variables:			
GDP in 2000 (in R$ millions)	447	61	3,783
Population in 2000	62,075	21,231	277,809
Fraction Population Urban	0.68	0.70	0.21
Years of Schooling in 2000	4.47	4.56	1.29
Total Exports in 2007 (in R$10,000s)	6,144	0	34,195

Notes: Summary statistics are based on the sample of 2,601 localities with at least one bank branch in 2007, prior to the onset of the financial crisis. Growth rates are averages for 2005-2007. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.2: Determinants of a Locality's Share of Government Bank Branches

	(1)	(2)	(3)
Dep. Variable:	Fraction Branches Gov Owned		
Ln(Total Employment)	0.0039	-0.0098	-0.0069
	(0.0220)	(0.0198)	(0.0200)
Frac. Urban	-0.3862***	-0.0792	-0.0415
	(0.0552)	(0.0554)	(0.0566)
Ln(Population)	0.1157***	0.0326	0.0289
	(0.0215)	(0.0204)	(0.0204)
Ln(GDP)	-0.0610***	0.0079	-0.0024
	(0.0220)	(0.0208)	(0.0209)
Years of Education	-0.0700***	-0.0023	-0.0065
	(0.0104)	(0.0104)	(0.0109)
Ln(Exports)	-0.0005	-0.0006	-0.0005
	(0.0012)	(0.0011)	(0.0011)
Industry VA/Total GDP	0.2095	0.1429	0.1577
	(0.1648)	(0.1501)	(0.1501)
Services VA/Total GDP	0.0225	0.1228	0.1148
	(0.1676)	(0.1559)	(0.1561)
Agric VA/Total GDP	0.0294	0.0222	0.0459
	(0.1574)	(0.1445)	(0.1454)
Yearly GDP Growth (2005-2007)	0.1518	0.0569	0.0537
	(0.1019)	(0.0918)	(0.0919)
Yearly Employment Growth (2005-2007)	-0.0059	0.0435	0.0455
	(0.0444)	(0.0413)	(0.0411)
Propensity Score		0.6390***	
		(0.0335)	
P-Score Block Dummies			x
Number of Localities	2,601	2,601	2,601

Notes: Robust standard errors in parentheses. Results are shown from regressing the fraction of total branches in a locality that are government owned in 2007 on locality characteristics. Column (2) includes a control for the propensity to have above the median share of government-owned bank branches and column (3) includes propensity score block dummies. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.3: Total Credit Operations (Bank Balance Sheets)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Variable:	Total Credit Operations				Total Liabilities				Total Credit/Total Liabilities			
Post	-0.1517***	-0.1481***	-0.1014***	0.0007	-0.0800***	-0.0777***	0.0044	-0.0561	-0.0205**	-0.0146	-0.0271*	-0.0249
	(0.0385)	(0.0463)	(0.0345)	(0.1144)	(0.0264)	(0.0249)	(0.0336)	(0.0616)	(0.0084)	(0.0093)	(0.0146)	(0.0231)
PostXGovbank	0.2430***	0.2924**	0.2815***	0.1917*	-0.0258	0.0038	-0.0135	-0.0827	0.0845***	0.0660**	0.0743***	0.0656***
	(0.0685)	(0.1315)	(0.0469)	(0.1052)	(0.0577)	(0.0819)	(0.0407)	(0.0558)	(0.0257)	(0.0272)	(0.0157)	(0.0164)
PostXControls		x		x		x		x		x		x
Bank Fixed Effects	x	x	x	x	x	x	x	x	x	x	x	x
Asset Weighted			x	x			x	x			x	x
Observations	4,403	4,403	4,403	4,403	4,403	4,403	4,403	4,403	4,403	4,403	4,403	4,403
Number of Banks	119	119	119	119	119	119	119	119	119	119	119	119

Notes: Robust standard errors, clustered at the bank level, in parentheses. Bank characteristics included as controls and interacted with post are total assets, capitalization ratio, a weighted average of borrowers' credit ratings, and credit as a share of assets, all for August 2006, prior to the onset of the financial crisis. We restrict the sample to 119 banks in continuous operation between 2006 and 2009. All estimations control for a linear time trend. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.4: Total Credit Operations (Municipality Balance Sheets)

	(1)	(2)	(3)	(4)
Dep. Variable:		Total Credit Operations		
Panel A: Govbank=Frac. Branches Govt Owned				
Post	-0.4570***	-0.4570***	-0.1419***	-0.1993***
	(0.0536)	(0.0578)	(0.0227)	(0.0312)
PostXGovbank	0.6623***	0.6623***	0.1780***	0.2640***
	(0.0845)	(0.0937)	(0.0443)	(0.0560)
Panel B: Govbank=High Govt Bank Dummy				
Post	-0.3389***	-0.3296***	-0.0903***	-0.1058***
	(0.0381)	(0.0402)	(0.0146)	(0.0195)
PostXGovbank	0.4156***	0.3990***	0.0695***	0.0898***
	(0.0504)	(0.0567)	(0.0186)	(0.0235)
For both panels:				
PostXPscore		x		x
Locality Fixed Effects	x	x	x	x
Population Weighted			x	x
Observations	13,005	13,005	13,005	13,005
Number of Localities	2,601	2,601	2,601	2,601

Notes: Robust standard errors, clustered at the locality level, in parentheses. Results are shown from regressing the natural logarithm of deflated total credit operations at the average annual level on post, postXgovbank, and depending on the specification, on postXpscore. All estimations include locality fixed effects and a linear time trend, and columns (3) and (4) are population-weighted. In Panel A, govbank is the fraction of total bank branches in a locality that is government-owned, and in Panel B, govbank is a dummy equal to 1 if the fraction of government-owned bank branches is above the median for all localities. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.5: GDP and Value-Added

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable:	GDP				Industry Value-Added		Services Value-Added	
Panel A: Govbank=Frac. Branches Govt Owned								
Post	-0.0156***	-0.0034	-0.0255***	-0.0161*	-0.0246**	-0.0080	-0.0269***	-0.0175***
	(0.0055)	(0.0062)	(0.0075)	(0.0089)	(0.0100)	(0.0116)	(0.0042)	(0.0045)
PostXGovbank	0.0574***	0.0343***	0.0649***	0.0509***	0.0450***	0.0138	0.0394***	0.0217***
	(0.0081)	(0.0095)	(0.0109)	(0.0144)	(0.0152)	(0.0185)	(0.0063)	(0.0073)
Panel B: Govbank=High Govt Bank Dummy								
Post	-0.0084*	0.0026	-0.0096	-0.0020	-0.0193**	-0.0041	-0.0217***	-0.0131***
	(0.0047)	(0.0054)	(0.0061)	(0.0067)	(0.0083)	(0.0101)	(0.0035)	(0.0039)
PostXGovbank	0.0413***	0.0218***	0.0342***	0.0242**	0.0332***	0.0062	0.0280***	0.0126**
	(0.0058)	(0.0074)	(0.0073)	(0.0095)	(0.0109)	(0.0148)	(0.0045)	(0.0057)
For both panels:								
PostXPscore		x		x		x		x
Locality Fixed Effects	x	x	x	x	x	x	x	x
Population Weighted			x	x				
Observations	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005
Number of Localities	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601

Notes: Robust standard errors, clustered at the locality level, in parentheses. Results are shown from regressing the natural logarithms of locality-level GDP (columns (1)-(4)), industry value-added (columns (5) and (6)), and services value-added (columns (7) and (8)) on post, postXgovbank, and depending on the specification, on postXpscore. All estimations include locality fixed effects and a linear time trend, and columns (3) and (4) are population-weighted. In Panel A, govbank is the fraction of total bank branches in a locality that is government-owned, and in Panel B, govbank is a dummy equal to 1 if the fraction of government-owned bank branches is above the median for all localities. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.6: Total Employment, Hours and Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Variable:	Total Employment				Total Hours				Total Wages			
Panel A: Govbank=Frac. Branches Govt Owned												
Post	-0.0370***	-0.0363***	-0.0316***	-0.0343**	-0.0482***	-0.0440***	-0.0391***	-0.0423***	-0.0827***	-0.0661***	-0.0941***	-0.0518***
	(0.0093)	(0.0126)	(0.0083)	(0.0160)	(0.0095)	(0.0123)	(0.0077)	(0.0146)	(0.0114)	(0.0144)	(0.0088)	(0.0167)
PostXGovbank	0.0191	0.0177	0.0343**	0.0384	0.0382***	0.0302	0.0545***	0.0592**	0.0542***	0.0229	0.1335***	0.0703***
	(0.0131)	(0.0189)	(0.0136)	(0.0251)	(0.0144)	(0.0191)	(0.0139)	(0.0233)	(0.0160)	(0.0213)	(0.0143)	(0.0254)
Panel B: Govbank=High Govt Bank Dummy												
Post	-0.0345***	-0.0341***	-0.0249***	-0.0313***	-0.0411***	-0.0363***	-0.0263***	-0.0292***	-0.0768***	-0.0620***	-0.0630***	-0.0361***
	(0.0073)	(0.0104)	(0.0055)	(0.0102)	(0.0074)	(0.0099)	(0.0050)	(0.0093)	(0.0090)	(0.0118)	(0.0072)	(0.0122)
PostXGovbank	0.0136*	0.0129	0.0233***	0.0318**	0.0235***	0.0149	0.0302***	0.0340***	0.0407***	0.0144	0.0755***	0.0400***
	(0.0080)	(0.0133)	(0.0074)	(0.0141)	(0.0086)	(0.0129)	(0.0073)	(0.0125)	(0.0098)	(0.0146)	(0.0082)	(0.0142)
For both panels:												
PostXPscore		x		x		x		x		x		x
Locality Fixed Effects	x	x	x	x	x	x	x	x	x	x	x	x
Population Weighted			x	x			x	x			x	x
Observations	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005
Number of Localities	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601

Notes: Robust standard errors, clustered at the locality level, in parentheses. Results are shown from regressing the natural logarithms of locality-level GDP (columns (1)-(4)), industry value-added (columns (5) and (6)), and services value-added (columns (7) and (8)) on post, postXgovbank, and depending on the specification, on postXpscore. All estimations include locality fixed effects and a linear time trend, and columns (3) and (4) are population-weighted. In Panel A, govbank is the fraction of total bank branches in a locality that is government-owned, and in Panel B, govbank is a dummy equal to 1 if the fraction of government-owned bank branches is above the median for all localities. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.7: Total Establishments

	(1)	(2)	(3)	(4)
Dep. Variable:		Number of Firms		
Panel A: Govbank=Frac. Branches Govt Owned				
Post	-0.0314***	-0.0137***	-0.0322***	-0.0087
	(0.0038)	(0.0043)	(0.0053)	(0.0073)
PostXGovbank	0.0687***	0.0353***	0.0999***	0.0648***
	(0.0061)	(0.0071)	(0.0104)	(0.0125)
Panel B: Govbank=High Govt Bank Dummy				
Post	-0.0207***	-0.0033	-0.0078**	0.0099**
	(0.0029)	(0.0034)	(0.0031)	(0.0048)
PostXGovbank	0.0459***	0.0148***	0.0530***	0.0297***
	(0.0038)	(0.0047)	(0.0057)	(0.0067)
For both panels:				
PostXPscore		x		x
Locality Fixed Effects	x	x	x	x
Population Weighted			x	x
Observations	13,005	13,005	13,005	13,005
Number of Localities	2,601	2,601	2,601	2,601

Notes: Robust standard errors, clustered at the locality level, in parentheses. Results are shown from regressing the natural logarithm of total establishments on post, postXgovbank, and depending on the specification, on postXpscore. All estimations include locality fixed effects and a linear time trend, and columns (3) and (4) are population-weighted. In Panel A, govbank is the fraction of total bank branches in a locality that is government-owned, and in Panel B, govbank is a dummy equal to 1 if the fraction of government-owned bank branches is above the median for all localities. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.8: Sector-Level Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable:	Employment		Hours		Wages		Firms	
Panel A: Govbank=Frac. Branches Govt Owned								
Post	-0.0094*	-0.0079	-0.0367**	-0.0120	-0.0590***	-0.0215	-0.0061**	-0.0031
	(0.0054)	(0.0062)	(0.0186)	(0.0211)	(0.0157)	(0.0178)	(0.0026)	(0.0030)
PostXGovbank	0.0154**	0.0126	0.0710***	0.0245	0.1301***	0.0594**	0.0199***	0.0143***
	(0.0077)	(0.0093)	(0.0262)	(0.0317)	(0.0220)	(0.0265)	(0.0038)	(0.0046)
Panel B: Govbank=High Govt Bank Dummy								
Post	-0.0075	-0.0064	-0.0267*	-0.0031	-0.0458***	-0.0135	-0.0045**	-0.0026
	(0.0047)	(0.0054)	(0.0159)	(0.0181)	(0.0134)	(0.0153)	(0.0023)	(0.0026)
PostXGovbank	0.0112**	0.0092	0.0493***	0.0073	0.0995***	0.0419**	0.0161***	0.0127***
	(0.0053)	(0.0069)	(0.0173)	(0.0223)	(0.0147)	(0.0188)	(0.0027)	(0.0035)
For both panels:								
PostXPscore		x		x		x		x
SectorXLocality Fixed Effects	x	x	x	x	x	x	x	x
Observations	221,085	221,085	221,085	221,085	221,085	221,085	221,085	221,085
Number of Sectors	17	17	17	17	17	17	17	17
Number of Localities	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601

Notes: Robust standard errors, clustered at the locality-sector level, in parentheses. Results are shown from regressing the natural logarithms of total employment (columns (1) and (2)), total hours (columns (3) and (4)), total wages (columns (5) and (6)), and total firms (columns (7) and (8)) on post, postXgovbank, and depending on the specification, on postXpscore. All estimations include localityXsector fixed effects and a linear time trend. In Panel A, govbank is the fraction of total bank branches in a locality that is government-owned, and in Panel B, govbank is a dummy equal to 1 if the fraction of government-owned bank branches is above the median for all localities. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.9: Neighboring Localities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Variable:	Credit		GDP		Employment		Hours		Wages		Firms	
Post	0.0039	0.0041	0.0050*	0.0050*	-0.0005	-0.0003	-0.0020	-0.0018	-0.0020	-0.0018	0.0049***	0.0049***
	(0.0139)	(0.0139)	(0.0028)	(0.0028)	(0.0041)	(0.0041)	(0.0043)	(0.0044)	(0.0054)	(0.0054)	(0.0016)	(0.0016)
PostXΔGovbank	0.6371***	0.6440***	0.0248***	0.0217***	0.0107	0.0198**	0.0205**	0.0307***	0.0204*	0.0287**	0.0109**	0.0106**
	(0.0564)	(0.0591)	(0.0063)	(0.0067)	(0.0089)	(0.0097)	(0.0103)	(0.0112)	(0.0111)	(0.0121)	(0.0043)	(0.0045)
PostXΔPscore		x		x		x		x		x		x
Locality Pair Fixed Effects	x	x	x	x	x	x	x	x	x	x	x	x
Observations	29,205	29,205	29,205	29,205	29,205	29,205	29,205	29,205	29,205	29,205	29,205	29,205
Locality Pairs	5,841	5,841	5,841	5,841	5,841	5,841	5,841	5,841	5,841	5,841	5,841	5,841
Localities	2,562	2,562	2,562	2,562	2,562	2,562	2,562	2,562	2,562	2,562	2,562	2,562

Notes: Robust standard errors, clustered at the locality-pair level, in parentheses. Results are shown from regressing the difference in the natural logarithms of total credit, GDP, total employment, total hours, total wages, and number of establishments between neighboring localities on post, postXΔgovbank, and in some specifications postXΔpscore. Δgovbank is the difference in the fraction of bank branches that are government-owned and Δpscore is the difference in the standardized propensity score between neighboring localities. Localities with neighbors that do not have a bank branch and locality pairs that cross state boundaries are excluded from the sample. All estimations include locality-pair fixed effects and a linear time trend. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.10: IV Estimates of the Effects of Credit on Production, Employment, Wages and Firms

Dep. variable:	(1) Ln(GDP)	(2) Ln(Emp)	(3) Ln(Hours)	(4) Ln(Wages)	(5) Ln(Firms)	(6) Δ Ln(GDP)	(7) Δ Ln(Emp)	(8) Δ Ln(Hours)	(9) Δ Ln(Wages)	(10) Δ Ln(Firms)
	Panel Fixed Effects					Cross-Sectional Pre-to-Post Crisis Differences				
Ln (Credit)	0.0508***	0.0266	0.0467*	0.0355	0.0540***					
	(0.0146)	(0.0256)	(0.0266)	(0.0289)	(0.0116)					
Δ Ln (Credit)						0.0518***	0.0268	0.0456*	0.0345	0.0533***
						(0.0147)	(0.0259)	(0.0267)	(0.0292)	(0.0115)
Locality Fixed Effects	x	x	x	x	x					
PostXP-score	x	x	x	x	x					
P-score						x	x	x	x	x
First Stage F-Stat	31.22	31.22	31.22	31.22	31.22	32.12	32.12	32.12	32.12	32.12
Hansen-Sargan P-val.	0.26	0.88	0.48	0.55	0.16	--	--	--	--	--
Observations	13005	13005	13005	13005	13005	2601	2601	2601	2601	2601
Number of Localities	2601	2601	2601	2601	2601	2601	2601	2601	2601	2601

Notes: Robust standard errors in parentheses. For columns (1)-(5), standard errors are clustered at the locality level. Columns (1)-(5) show results from an instrumental variables regression of ln y = ln(credit)+post+postXpscore+trend+postXtrend+postXpscoreXtrend+localityFE+ ϵ, where we have instrumented for ln(credit) using postXfrac. branches gov.-owned and postXfrac. branches gov.-ownedXtrend. Columns (6)-(10) show results from regressing the difference between average pre and post-crisis ln y on the difference between average pre and post-crisis ln(credit) and the pscore: Δln y = Δ ln(credit) + pscore +ϵ, where we instrument for Δ ln(credit) with the frac. of bank branches that are government-owned in the locality prior to the crisis. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.11: Interactions with Credit Dependence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Variable:	Credit	GDP	Employment	Hours	Wages	Firms	Credit	GDP	Employment	Hours	Wages	Firms
High:	High Fraction of Small Firms						High External Dependence					
Panel A: Govbank=Frac. Branches Govt Owned												
Post	-0.4105***	0.0037	-0.0338*	-0.0380*	-0.0618***	0.0065	-0.4855***	-0.0094	-0.0463**	-0.0594***	-0.0789***	-0.0097*
	(0.0939)	(0.0095)	(0.0205)	(0.0195)	(0.0224)	(0.0061)	(0.0983)	(0.0085)	(0.0196)	(0.0187)	(0.0213)	(0.0059)
PostXGovbank	0.6529***	0.0196	0.0085	0.0155	0.0106	0.0319***	0.6844***	0.0363***	0.0295	0.0528*	0.0424	0.0407***
	(0.1406)	(0.0150)	(0.0308)	(0.0305)	(0.0340)	(0.0105)	(0.1410)	(0.0117)	(0.0288)	(0.0292)	(0.0317)	(0.0095)
PostXHigh	-0.0866	-0.0122	-0.0040	-0.0101	-0.0072	-0.0376***	0.0503	0.0106	0.0176	0.0271	0.0226	-0.0071
	(0.1314)	(0.0109)	(0.0187)	(0.0187)	(0.0216)	(0.0069)	(0.1338)	(0.0107)	(0.0174)	(0.0176)	(0.0204)	(0.0068)
PostXHighXGovbank	0.0065	0.0257	0.0166	0.0261	0.0218	0.0016	-0.0350	-0.0015	-0.0209	-0.0423	-0.0367	-0.0138
	(0.1726)	(0.0167)	(0.0290)	(0.0310)	(0.0344)	(0.0121)	(0.1742)	(0.0166)	(0.0272)	(0.0294)	(0.0329)	(0.0120)
Panel B: Govbank=High Govt Bank Dummy												
Post	-0.2500***	0.0124*	-0.0360**	-0.0350**	-0.0663***	0.0151***	-0.3520***	0.0021	-0.0411***	-0.0461***	-0.0679***	0.0013
	(0.0618)	(0.0075)	(0.0153)	(0.0144)	(0.0167)	(0.0043)	(0.0700)	(0.0072)	(0.0151)	(0.0142)	(0.0164)	(0.0044)
PostXGovbank	0.3478***	0.0034	0.0123	0.0099	0.0188	0.0157**	0.4183***	0.0152	0.0190	0.0275	0.0216	0.0195***
	(0.0773)	(0.0111)	(0.0201)	(0.0194)	(0.0217)	(0.0065)	(0.0844)	(0.0094)	(0.0187)	(0.0182)	(0.0202)	(0.0060)
PostXHigh	-0.1565	-0.0180**	0.0038	-0.0020	0.0082	-0.0375***	0.0402	0.0012	0.0124	0.0174	0.0104	-0.0088*
	(0.0988)	(0.0086)	(0.0135)	(0.0134)	(0.0157)	(0.0052)	(0.1004)	(0.0085)	(0.0127)	(0.0127)	(0.0149)	(0.0051)
PostXHighXGovbank	0.0939	0.0326***	0.0009	0.0087	-0.0079	-0.0004	-0.0344	0.0144	-0.0109	-0.0240	-0.0135	-0.0116
	(0.1031)	(0.0118)	(0.0178)	(0.0188)	(0.0212)	(0.0076)	(0.1038)	(0.0117)	(0.0164)	(0.0175)	(0.0200)	(0.0075)
For both panels:												
PostXPscore	x	x	x	x	x	x	x	x	x	x	x	x
Locality Fixed Effects	x	x	x	x	x	x	x	x	x	x	x	x
Observations	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005	13,005
Number of Localities	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601

Notes: Robust standard errors, clustered at the locality level, in parentheses. Results are shown from regressing the natural logarithms of total credit, GDP, total employment, total hours, total wages, and number of establishments on post, postXgovbank, postXhigh, and postXhighXgovbank, where high is alternately a dummy equal to 1 if the locality has above the median fraction of small firms (columns (1)-(6)) or a dummy equal to 1 if the locality has above the median measure of externally dependency (columns (7)-(12)). All estimations include locality fixed effects and a linear time trend. In Panel A, govbank is the fraction of total bank branches in a locality that is government-owned, and in Panel B, govbank is a dummy equal to 1 if the fraction of government-owned bank branches is above the median for all localities. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.12: Political Economy and Lending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable:	Total Credit Operations							
Alignment:	PT Mayor				Coalition Mayor			
Panel A: Govbank=Frac. Branches Govt Owned								
Post	-0.4540***	-0.5081***	-0.0781***	-0.1983***	-0.3699***	-0.4307***	-0.0742***	-0.1981***
	(0.0457)	(0.0509)	(0.0195)	(0.0287)	(0.0654)	(0.0709)	(0.0247)	(0.0337)
PostXGovbank	0.7224***	0.8281***	0.1922***	0.3754***	0.6380***	0.7493***	0.1978***	0.3819***
	(0.0590)	(0.0709)	(0.0289)	(0.0476)	(0.0787)	(0.0909)	(0.0412)	(0.0583)
PostXAlignment	0.0515	0.0295	0.0132	-0.0019	-0.1216	-0.1170	0.0037	-0.0004
	(0.1015)	(0.0996)	(0.0453)	(0.0443)	(0.0860)	(0.0857)	(0.0358)	(0.0325)
PostXAlignmentXGovbank	0.0376	0.0553	0.0485	0.0536	0.1418	0.1383	-0.0055	0.0025
	(0.1613)	(0.1594)	(0.1105)	(0.1095)	(0.1109)	(0.1106)	(0.0578)	(0.0535)
Panel B: Govbank=High Govt Bank Dummy								
Post	-0.3299***	-0.3696***	-0.0180	-0.0663***	-0.2658***	-0.3103***	-0.0158	-0.0678***
	(0.0375)	(0.0415)	(0.0151)	(0.0181)	(0.0534)	(0.0579)	(0.0233)	(0.0258)
PostXGovbank	0.4609***	0.5341***	0.0666***	0.1333***	0.4131***	0.4902***	0.0644**	0.1315***
	(0.0381)	(0.0480)	(0.0173)	(0.0246)	(0.0523)	(0.0626)	(0.0267)	(0.0325)
PostXAlignment	0.1275*	0.1149	0.0240	0.0186	-0.0696	-0.0675	0.0065	0.0081
	(0.0737)	(0.0729)	(0.0228)	(0.0226)	(0.0677)	(0.0675)	(0.0289)	(0.0281)
PostXAlignmentXGovbank	-0.1041	-0.0971	-0.0022	-0.0130	0.0503	0.0499	-0.0011	-0.0014
	(0.0876)	(0.0868)	(0.0419)	(0.0420)	(0.0718)	(0.0716)	(0.0334)	(0.0336)
For both panels:								
PostXPscore		x		x		x		x
Locality Fixed Effects	x	x	x	x	x	x	x	x
Population Weighted			x	x			x	x
Observations	96,237	96,237	96,237	96,237	96,237	96,237	96,237	96,237
Number of Localities	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601

Notes: Robust standard errors, clustered at the locality level, in parentheses. Results are shown from regressing the natural logarithm of total monthly credit on post, postXgovbank, postXalignment, and postXalignmentXgovbank, where alignment is an electorate-weighted average of a municipality's mayoral alignment with the presidential party, the PT, (columns (1)-(4)), or with one of the parties in the federal government's coalition (columns (5)-(8)) in either 2004 or 2008. All estimations include locality fixed effects and a linear time trend. In Panel A, govbank is the fraction of total bank branches in a locality that is government-owned, and in Panel B, govbank is a dummy equal to 1 if the fraction of government-owned bank branches is above the median for all localities. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.13: Quality of Loans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable:	Capitalization Ratio		Borrowers' Credit Rating		Loan Loss Provisions		Credit Earnings	
Panel A: Non Asset Weighted								
Post	-0.0015	-0.0055	-0.0221***	-0.0219**	0.0603**	0.0649*	-0.0018	-0.0017
	(0.0062)	(0.0075)	(0.0062)	(0.0091)	(0.0282)	(0.0351)	(0.0063)	(0.0072)
PostXGovbank	-0.0001	-0.0115	0.0137	0.0165	-0.0252	-0.0697	0.0117	-0.0097
	(0.0123)	(0.0190)	(0.0107)	(0.0165)	(0.0271)	(0.0945)	(0.0076)	(0.0130)
Panel B: Asset Weighted								
Post	0.0427**	-0.0496***	-0.0227**	-0.0123	0.0203***	0.0078	-0.0034	-0.0101
	(0.0199)	(0.0181)	(0.0103)	(0.0163)	(0.0071)	(0.0282)	(0.0027)	(0.0096)
PostXGovbank	-0.0434**	-0.0010	0.0392***	0.0185	-0.0270***	-0.0315*	0.0007	0.0090
	(0.0214)	(0.0203)	(0.0048)	(0.0153)	(0.0051)	(0.0162)	(0.0018)	(0.0076)
PostXControls		x		x		x		x
Bank Fixed Effects	x	x	x	x	x	x	x	x
Observations	4,403	4,403	4,403	4,403	4,403	4,403	3,922	3,922
Number of Banks	119	119	119	119	119	119	106	106

Notes: Robust standard errors, clustered at the bank level, in parentheses. Results are shown from regressing the monthly capitalization ratio (columns (1) and (2)), borrowers' average credit ratings (columns (3) and (4)), loan loss provisions (columns (5) and (6)), and credit earnings (columns (7) and (8)) on post, postXgovbank, bank fixed effects, a linear time trend, and depending on the specification, on postXcontrols, where controls include total assets, capitalization ratio, a weighted average of borrowers' credit ratings, and credit as a share of assets, all for August 2006, prior to the onset of the financial crisis. We restrict the sample to 119 banks in continuous operation between 2006 and 2009; the sample size is limited by data availability to 106 banks in columns (7) and (8). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.14: Alternative Clustering, Controls, Sample Selection, and Placebos

	(1)	(2)	(3)	(4)	(5)	(6)	
Dep. Variable:	Ln(Credit)	Ln(GDP)	Ln(Emp.)	Ln(Hours)	Ln(Wages)	Ln(Firms)	Localities
(A) Base Case	0.2640***	0.0509***	0.0384	0.0592**	0.0703***	0.0648***	2,601
	(0.0560)	(0.0144)	(0.0251)	(0.0233)	(0.0254)	(0.0125)	
Standard Errors							
(B) S.E. Clustered at State-Year	0.2640***	0.0509***	0.0384**	0.0592***	0.0703**	0.0648***	2,601
	(0.0738)	(0.0154)	(0.0172)	(0.0189)	(0.0284)	(0.0116)	
Controls and Matching							
(C) Include Locality Specific	0.2278**	0.0184	-0.0053	0.0074	0.0737**	0.0582***	2,601
Pre-Trends	(0.1046)	(0.0188)	(0.0290)	(0.0295)	(0.0349)	(0.0144)	
(D) Include Post X FracGovWorkers	0.2525***	0.0409***	0.0338	0.0437*	0.0413	0.0506***	2,601
	(0.0608)	(0.0146)	(0.0286)	(0.0258)	(0.0283)	(0.0124)	
(E) Post X PScore Block Dummies	0.2492***	0.0511***	0.0353	0.0544**	0.0663**	0.0594***	2,601
(no Post X Pscore)	(0.0539)	(0.0145)	(0.0259)	(0.0239)	(0.0261)	(0.0122)	
(F) Post X Fixed Locality Controls	0.3227***	0.0434***	0.0270	0.0438*	0.0525**	0.0453***	2,601
(no Post X PScore)	(0.0537)	(0.0146)	(0.0252)	(0.0230)	(0.0257)	(0.0101)	
Samples and Spatial Units							
(G) Trim by Propensity Score	0.3839***	0.0346**	0.0308	0.0499**	0.0588**	0.0515***	2,081
	(0.0587)	(0.0141)	(0.0204)	(0.0212)	(0.0236)	(0.0103)	
(H) Collapse to Metro Areas	0.2412***	0.0565***	0.0459	0.0647**	0.0645**	0.0696***	2,239
	(0.0827)	(0.0160)	(0.0284)	(0.0263)	(0.0286)	(0.0154)	
Placebo							
(I) Counterfactually Assign	0.0355*	0.0052	0.0008	-0.0011	0.0067	-0.0078	2,601
"Govbank" Based on P-score	(0.0190)	(0.0126)	(0.0092)	(0.0086)	(0.0111)	(0.0057)	

Notes: Robust standard errors in parentheses. Each cell reports the coefficient on postXgovbank from a different estimation, where govbank is the fraction of government-owned bank branches in a locality (except in row (I), where it is a dummy equal to 1 that is randomly assigned based on the pscore). All estimations are population-weighted and include locality fixed effects as well as a linear time trend. All estimations include postXpscore as a control except in row (D), where postXblock dummies are included, and row (E), where postXfixed locality characteristics are included. Rows (A)-(F) present results using the base-case sample, row (G) presents results from trimming the sample by the top and bottom ten percent of the propensity score distribution, and row (H) presents results from collapsing localities to metro areas based on Brazilian statistical definitions of commuting zones and labor markets. Standard errors are clustered at the locality level, except in row (B), where they are clustered at the state-year level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.15: Alternative Explanations

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable:	Ln (Gov. Transfers)		Ln (Gov. Workers)		Ln (Const. Workers)	
Panel A: Govbank=Frac. Branches Govt Owned						
Post	0.0854***	0.1338***	-0.0607**	0.0056	0.0923*	0.1043
	(0.0038)	(0.0217)	(0.0246)	(0.0614)	(0.0521)	(0.0645)
PostXGovbank	0.0130**	-0.0762*	-0.0144	0.0071	-0.0394	-0.0640
	(0.0061)	(0.0424)	(0.0365)	(0.0536)	(0.0789)	(0.1019)
Panel B: Govbank=High Govt Bank Dummy						
Post	0.0885***	0.0989***	-0.0702***	-0.0140	0.0851*	0.0484
	(0.0032)	(0.0065)	(0.0213)	(0.0477)	(0.0442)	(0.0515)
PostXGovbank	0.0068	-0.0119	0.0033	0.0414	-0.0246	0.0368
	(0.0045)	(0.0103)	(0.0262)	(0.0281)	(0.0565)	(0.0740)
For both panels:						
PostXP-Score	x	x	x	x	x	x
Locality Fixed Effects	x	x	x	x	x	x
Population Weighted		x		x		x
Observations	13,005	13,005	13,005	13,005	13,005	13,005
Number of Localities	2,601	2,601	2,601	2,601	2,601	2,601

Notes: Robust standard errors, clustered at the locality level, in parentheses. Results are shown from regressing the natural logarithms of federal government transfers to localities, the number of public sector workers in a locality, and the number of construction workers in a locality on post, postXgovbank, postXpscore, a linear time trend, and locality fixed effects. Columns (2), (4), and (6) show results from population-weighted estimations. In Panel A, govbank is the fraction of total bank branches in a locality that is government-owned, and in Panel B, govbank is a dummy equal to 1 if the fraction of government-owned bank branches is above the median for all localities. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.16: Productivity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Variable:	TFP				Change in TFP		TFP				Change in TFP	
	TFP Calculated Using Labor=Total Employment						TFP Calculated Using Labor=Total Hours					
Panel A: Govbank=Frac. Branches Govt Owned												
Govbank					0.0142*	0.0235**					0.0149*	0.0241**
					(0.0075)	(0.0114)					(0.0076)	(0.0117)
Post	0.0065**	0.0148***	-0.0065	0.0094*			0.0053	0.0138***	-0.0086*	0.0082*		
	(0.0033)	(0.0037)	(0.0051)	(0.0048)			(0.0033)	(0.0038)	(0.0050)	(0.0050)		
PostXGovbank	0.0247***	0.0090*	0.0341***	0.0104			0.0265***	0.0104*	0.0375***	0.0123*		
	(0.0042)	(0.0054)	(0.0057)	(0.0063)			(0.0043)	(0.0055)	(0.0058)	(0.0064)		
Panel B: Govbank=High Govt Bank Dummy												
Govbank					0.0074	0.0084					0.0081	0.0098
					(0.0052)	(0.0068)					(0.0053)	(0.0068)
Post	0.0102***	0.0183***	0.0021	0.0147***			0.0092***	0.0174***	0.0005	0.0128***		
	(0.0027)	(0.0031)	(0.0042)	(0.0038)			(0.0028)	(0.0032)	(0.0042)	(0.0042)		
PostXGovbank	0.0168***	0.0024	0.0171***	0.0006			0.0182***	0.0035	0.0200***	0.0039		
	(0.0029)	(0.0041)	(0.0035)	(0.0035)			(0.0029)	(0.0042)	(0.0036)	(0.0040)		
For both panels:												
PostXP-Score		x		x				x		x		
Locality Fixed Effects	x	x	x	x			x	x	x	x		
Population Weighted			x	x		x			x	x		x
Observations	26,010	26,010	26,010	26,010	2,601	2,601	26,010	26,010	26,010	26,010	2,601	2,601
Number of Localities	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601	2,601

Notes: Robust standard errors, clustered at the locality level, in parentheses. Results are shown from regressing yearly Solow residuals on post, postXgovbank, and depending on the specification, on postXpscore. The Solow residual is alternately calculated using total employment (columns (1)-(6)) or total hours (columns (7)-(12)) as a measure of labor. Estimations reported in columns (1)-(4) and (7)-(10) include locality fixed effects, and columns (3), (4), (6), (9), (10), and (12) are population-weighted. Columns (5), (6), (11), and (12) show results from regressing the 2007-2009 change in the Solow residual on govbank. In Panel A, govbank is the fraction of total bank branches in a locality that is government-owned, and in Panel B, govbank is a dummy equal to 1 if the fraction of government-owned bank branches is above the median for all localities. * significant at 10%; ** significant at 5%; *** significant at 1%.

1.9 Appendix A: Propensity Score

This appendix provides information for the propensity score matching. The propensity to have more than the median share of government-owned bank branches is calculated using the following locality characteristics taken from 2000 census data: years of education, urbanization rate, illiteracy rate, average per capita income, and the natural logarithms of population, total locality income, total locality employment, a measure of total locality human capital, and several interactions of these. The results of the logit estimation are shown in Table 1.17. Localities are stratified into 18 propensity score blocks. Within each propensity score block, we cannot reject at the 5% significance level that at least 95% of the covariates are statistically indistinguishable across localities. Figure 1.5 shows the overlap in the box plots of the estimated propensity scores for localities above and below the median share of government-owned bank branches.

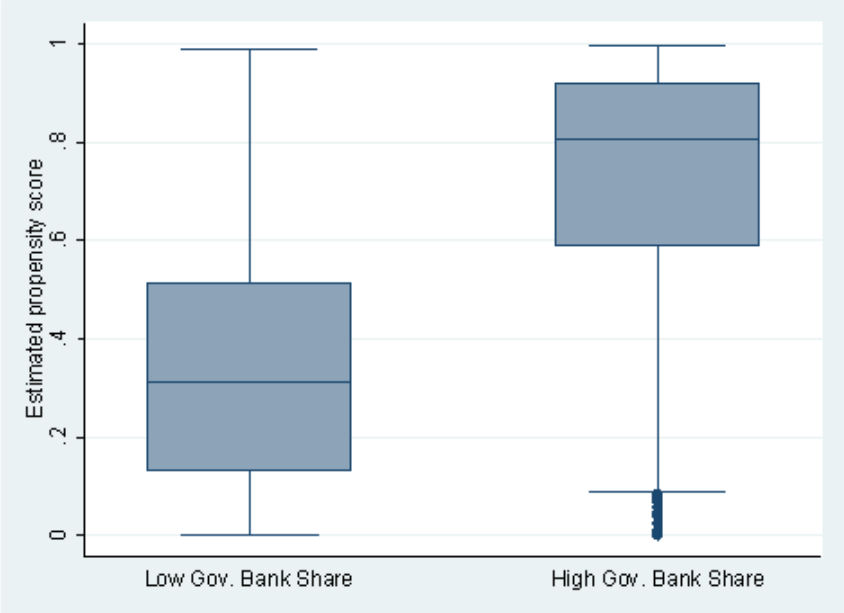


Figure 1.6: Box Plot of Estimated Propensity Scores for Localities with High and Low Shares of Government-Owned Bank Branches

Table 1.17: Estimation of Propensity Score

Years of Schooling	1.0434***
	(0.2258)
Urbanization	18.5914***
	(3.7240)
Ln(Population)	6.7971***
	(1.4356)
Ln(GDP)	6.9334***
	(1.2559)
Ln(Total Employment)	-0.2015
	(1.2783)
Ln(Human Capital)	-4.3854***
	(0.9187)
Income per Capita	0.0355***
	(0.0043)
Illiteracy Rate	0.1205***
	(0.0159)
Ln(GDP)XLn(Population)	-0.3832***
	(0.1342)
Ln(GDP)XLn(Total Employment)	-0.4279***
	(0.0819)
Ln(Total Employment)XLn(Population)	0.4883***
	(0.1321)
Ln(Population)XUrbanization	-1.3643***
	(0.3816)
Income per CapitaXUrbanization	-0.0439***
	(0.0045)
Income per CapitaXIlliteracy Rate	-0.0004***
	(0.0001)
Number of Localities	2601
Pseudo R-squared	0.33

Notes: Standard errors in parentheses. Estimation is based on a logit of the propensity of a locality to have above the median share of government-owned bank branches. All regressors are for 2000. * significant at 10%; ** significant at 5%; *** significant at 1%.

1.10 Appendix B: Conceptual Framework

Appendix B presents a conceptual framework to explain why private banks might lend differently than government banks when faced with a financial crisis, and how this differential lending might affect economic outcomes such as production, employment, and incomes. The intuition is that private banks may lend less because of funding constraints or because of a combination of greater risk aversion, loan losses, or a more pessimistic world outlook. If lending is used for capital investment and if capital complements labor in production, then declines in lending can lead to declines in GDP, employment, and income, with the magnitude of declines depending on the wage elasticity and the capital intensity of production.

1.10.1 Banks

There are three agents in this framework: banks, firms, and workers. Banks pay a deposit rate, r_d , on deposits, D. They lend out a fraction, γ, of deposits at interest rate r_l. There are two states of the world. A good state occurs with probability p. In this state, loans are repaid with interest. A bad state occurs with probability $(1-p)$. In this state, banks do not receive interest on their loans; they lose a fraction, δ, of what was lent; and they must recapitalize, contributing $\delta\gamma D$ from their own capital. Banks profits in the good, g and bad, b, states can be written as:

$$\begin{aligned} \pi_g &= r_l\gamma D - r_d D \qquad (a) \\ \pi_b &= 0 - r_d D - \delta\gamma D \qquad (b) \end{aligned} \tag{1.10.1}$$

Banks have an original valuation, V . Their objective is to maximize a welfare function of their expected valuation by choosing what fraction, γ, of deposits to lend. This welfare function, capturing risk aversion, can expressed as:

$$W = p\frac{(V + r_l\gamma D - r_d D)^{1-\sigma}}{1-\sigma} + (1-p)\frac{(V + 0 - r_d D - \delta\gamma D)^{1-\sigma}}{1-\sigma} \tag{1.10.2}$$

Maximizing this welfare function with respect to γ and simplifying yields:

$$\gamma^* = \frac{(V - r_d D)\left(\phi^{\frac{1}{\sigma}} - 1\right)}{D\left(r_l + \delta\phi^{\frac{1}{\sigma}}\right)} \tag{1.10.3}$$

where $\phi = \frac{pr_d}{(1-p)\delta}$ and σ is a measure of risk aversion. The expression in(1.10.3) provides the optimal fraction of deposits banks are willing to lend. Assuming banks are risk averse ($\sigma > 1$) and their original valuation is always greater than the dividends paid to depositors ($V - r_d D > 0$), then in order for banks to lend, it must be the case that $\phi = \frac{pr_d}{(1-p)\delta} > 1$; this condition simply states that expected gains must be greater than expected losses.

We can now analyze what happens to the optimal fraction of deposits lent. As the probability of the good state of the world increases, banks are willing to lend more (i.e., $\frac{\partial \gamma^*}{\partial p} > 0$); as risk aversion increases, banks are willing to lend less (i.e., $\frac{\partial \gamma^*}{\partial \sigma} < 0$); and as potential loan losses in the bad state of the world increase, banks are willing to lend less (i.e., $\frac{\partial \gamma^*}{\partial \delta} < 0$)[24]. Since lending is equal to a fraction of deposits, $C = \gamma D$, lending can decline if either γ or D declines. In the empirical section, we examine whether the reason for decreased lending by private banks is due to declines in γ or D.

1.10.2 Firms, Employment, and Output

Lending is assumed to be transformed one-for-one into capital, $K = C = \gamma D$, which firms rent and use in production. Firms produce a globally traded good priced at 1, using capital and labor, according to a Cobb-Douglas production function. Firms maximize the following

[24] Assuming $V - r_d D > 0$ and $\phi > 1$, so that banks are willing to lend,

$$\frac{\partial \gamma^*}{\partial p} = \frac{(r_l + \delta)(V - r_d D)\,\phi^{\frac{1}{\sigma}}}{p(1-p)\sigma D\left(r_l + \delta\phi^{\frac{1}{\sigma}}\right)^2} > 0$$

$$\frac{\partial \gamma^*}{\partial \sigma} = \frac{\phi^{\frac{1}{\sigma}} \ln \phi\,(r_l + \delta)(V - r_d D)}{\sigma^2 D\left(r_l + \delta\phi^{\frac{1}{\sigma}}\right)^2} < 0$$

$$\frac{\partial \gamma^*}{\partial \delta} = \frac{\phi^{\frac{1}{\sigma}}\left(r_l + \delta + \delta\sigma\phi^{\frac{1}{\sigma}} - \delta\sigma\right)(V - r_d D)}{\sigma\delta D\left(r_l + \delta\phi^{\frac{1}{\sigma}}\right)^2} < 0$$

profit function:

$$\pi = K^{\alpha} L^{1-\alpha} - r_l K - wL \tag{1.10.4}$$

where K is capital, L is labor, r_l is the rental rate of capital, and w is wages. Since production is constant returns to scale, there are an indeterminate number of firms of indeterminate size, and factor markets are competitive, with capital and labor paid their marginal products:

$$\begin{aligned} r_l &= \alpha K^{\alpha-1} L^{1-\alpha} \qquad (a) \\ w &= (1-\alpha) K^{\alpha} L^{-\alpha} \qquad (b) \end{aligned} \tag{1.10.5}$$

In the short-run, local labor supply is inelastic, and for simplicity, normalized to 1. With full employment, initial period wages are $w_1 = (1-\alpha)(\gamma_1 D_1)^{\alpha}$ and initial period output is $y_1 = (\gamma_1 D_1)^{\alpha}$.

We are interested in a shock to lending, either through a reduction in γ or D. In the second period, there is a decline in lending, with $\gamma_2 D_2 < \gamma_1 D_1$. Given that labor is inelastically supplied, if wages can freely adjust downward, then $w_2 = (1-\alpha)(\gamma_2 D_2)^{\alpha}$ and labor demand in the second period is 1, with full employment. Unemployment will only arise if wages are downwardly rigid. Letting $\eta \in [0,1]$ be a measure of wage elasticity, with $\eta = 1$ implying that wages are completely elastic, then wages in the second period can be expressed as $w_2 = (1-\alpha)\left[\eta\left(\gamma_2 D_2\right)^{\alpha} + (1-\eta)\left(\gamma_1 D_1\right)^{\alpha}\right]$. We can now write an expression for labor demand in the second period as a function of lending, the wage elasticity, and the technology parameter, α :

$$L_2 = \left(\frac{1}{\eta + (1-\eta)\left(\frac{\gamma_1 D_1}{\gamma_2 D_2}\right)^{\alpha}} \right)^{\frac{1}{\alpha}} \tag{1.10.6}$$

Given $0 < \eta < 1$ and $\gamma_2 D_2 < \gamma_1 D_1$, then there will be unemployment, with $L_2 < 1$. Assuming $D_1 = D_2$, with the reason for the decline in lending due to a reduction in γ, we can simplify equation (1.10.6) and take the natural logarithm to obtain

$$\ln L_2 = -\frac{1}{\alpha} \ln\left(\eta + (1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha} \right) \tag{1.10.7}$$

Similarly, we can write the following expression for output in the second period:

$$\ln y_2 = \alpha \ln (\gamma_2 D_2) - \frac{1-\alpha}{\alpha} \ln \left(\eta + (1-\eta) \left(\frac{\gamma_1}{\gamma_2} \right)^{\alpha} \right) \tag{1.10.8}$$

1.10.3 Comparative Statistics

We can now perform comparative statics on equations (1.10.7) and (1.10.8) and analyze how employment and output respond to a change in γ_2 depending on the wage elasticity, η , and technology parameter, α.

As γ_2 declines, in other words, as lending declines, both employment and output decline. A higher wage elasticity (a higher value of η) mitigates the decline in both employment and output due to a decline in lending. Finally, as production becomes more capital intensive (with higher values of α), a given decline in lending results in larger declines in output and smaller declines in employment.[25] The comparative statics are fairly intuitive. With greater wage elasticity, shocks to lending are transmitted to wages rather than to employment and output. Since wages can adjust, employment and output can remain high despite a decline in lending. Moreover, for industries that are more capital intensive, a decline in lending has less of an effect on employment and a greater effect on output. Since in more capital-intensive industries, labor comprises a smaller share of productive inputs, a decline in lending leads to more of a decline in output and less of a decline in employment, as compared to a similar lending shock to less capital-intensive industries.

[25] We can differentiate equations (1.10.7) and (1.10.8) to obtain the following

$$\frac{\partial \ln L_2}{\partial \gamma_2} = \frac{(1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha}}{\gamma_2 \left(\eta + (1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha}\right)} > 0 \qquad \frac{\partial \ln y_2}{\partial \gamma_2} = \frac{\alpha\eta + (1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha}}{\eta + (1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha}} > 0$$

$$\frac{\partial^2 \ln L_2}{\partial \gamma_2 \partial \eta} = \frac{-\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha}}{\gamma_2 \left(\eta + (1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha}\right)^2} < 0 \qquad \frac{\partial^2 \ln y_2}{\partial \gamma_2 \partial \eta} = \frac{-(1-\alpha)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha}}{\gamma_2 \left(\eta + (1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha}\right)^2} < 0$$

$$\frac{\partial^2 \ln L_2}{\partial \gamma_2 \partial \alpha} = \frac{-\eta(1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha} \ln\left(\frac{\gamma_1}{\gamma_2}\right)}{\gamma_2 \left(\eta + (1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha}\right)^2} < 0 \quad \text{for} \quad \gamma_2 < \gamma_1$$

$$\frac{\partial^2 \ln y_2}{\partial \gamma_2 \partial \alpha} = \frac{\eta\left(\eta + (1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha} + (1-\eta-\alpha+\alpha\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha} \ln\left(\frac{\gamma_1}{\gamma_2}\right)\right)}{\gamma_2 \left(\eta + (1-\eta)\left(\frac{\gamma_1}{\gamma_2}\right)^{\alpha}\right)^2} < 0 \quad \text{for } \gamma_2 < \gamma_1 \quad \text{and } \alpha, \eta \in (0,1)$$

This conceptual framework yields several testable implications. First, we can test whether a decline in lending is due to a decline in a bank's loanable funds or to a decline in the share of funds a bank is willing to lend. Second, we can test whether declines in lending lead to declines in employment and output, and whether these declines are greater depending on the capital intensity of industries and the labor market rigidities in an area.

To test an implication of the conceptual framework, we estimate a variant of equation (1.3.1) that includes interactions with measures of local capital intensity and labor market flexibility. In localities with more capital-intensive industries, the impact of a decline in lending should be reflected more in GDP than in labor since labor is a less important input in production. In areas with more flexible labor markets, a decline in lending should be reflected less in GDP and possibly more in employment and wages. As a measure of capital intensity, we consider the fraction of a locality's workforce initially employed in heavy industries or manufacturing. As a measure of labor market flexibility, we consider the pre-crisis share of workers in the informal sector and the pre-crisis share of the working-age population that is not employed.

Results are not reported, but in more capital-intensive localities, post-crisis declines in GDP are indeed slightly larger while declines in employment are similar to those in less capital-intensive areas. While having a higher share of government-owned bank branches buttresses GDP in these areas, their effect on GDP is smaller than in less capital-intensive localities. This coincides with the previous finding that government-owned banks were not necessarily targeting more credit to localities where this credit might have had greater effects. In localities with more flexible labor markets, the declines in GDP and employment following the onset of the crisis are smaller in magnitude. As predicted by the empirical framework, in areas where there is more slack in the labor market, there appears to be smaller declines in both GDP and employment since firms can presumably substitute more easily between labor and capital.

Chapter 2

Government vs. Private Bank Borrowing and the Cost of Political Lending

2.1 Introduction

Government ownership of banks is widespread around the world. In 2005, government banks accounted for about 40% of total bank assets in Germany, 45% of total bank assets in Brazil, and about 42% in Argentina (Barth, Caprio and Levine, 2006). The prevalence of government-owned banks makes understanding their role in the economy important. This paper develops a model and uses a dataset of firms from across the world to understand more clearly the role of government banks.

Two competing views of government-sponsored enterprise emerge from the literature. The social view, consistent with Atkinson and Stiglitz (1980) and Stiglitz (1993), holds that government enterprises aim to maximize social welfare. According to this view, intervention in the banking sector is justified when the social benefits of curing financial market failures are sufficiently high. Put differently, these banks can provide firms financing for projects that may produce large positive externalities to society when the project would otherwise go unfunded.

The political view, on the other hand, argues that while it may be socially beneficial for government enterprises to exist to cure market failures and to maximize social welfare, politicians aim to maximize their individual utility. The political view argues that government enterprises broadly, and government banks specifically, exist to serve the private interests of politicians. According to this view, loans provided by government banks are made strategically to attain the specific goals of incumbent politicians, whether that is to maximize the probability of re-election, to maximize campaign contributions or to maximize employment, as discussed in Shleifer and Vishny (1994).

The empirical academic literature on government banks is more consistent with the political view of government enterprise. At the macroeconomic level, Barth Caprio and Levine (2001) finds that widespread state ownership of banks is correlated with poor financial development. Additionally, La Porta, Lopez-de-Silanes, and Shleifer (2002) finds that government ownership of banks is larger in poor countries, countries with inefficient governments and poor protection of property rights.

Using bank-level loan volumes around the world, Dinc (2005) finds that politics influences the lending by government banks as these banks increase their lending in election years. Building on Beck, Demirguc-Kunt and Levine (2006), Houston, Lin and Ma (2011) finds that higher levels of government bank lending is associated with higher levels of perceived bank corruption. Further, this effect is more pronounced when the media is government-owned. While this study is compelling, an important limitation is that the corruption measure that is used does not distinguish between government and private banks nor does it connect firms to the type of bank (government versus private) from which a firm has borrowed.

Within-country studies have found similar evidence consistent with the political view of government enterprise. Sapienza (2004) uses data on individual bank loans to firms in Italy. This study finds that government banks charge lower interest rates than private banks. Additionally, it finds that government banks prefer to lend to firms in depressed areas, to larger firms, and that electoral politics significantly impacts lending - the stronger

is the political party in the area where the firm is located, the lower the interest rate. Carvalho (2010) uses plant-level data in Brazil and suggests that politicians use loans to shift employment into politically (electorally) beneficial regions of Brazil. In Pakistan, Khwaja and Mian (2005) finds that politically-connected firms borrow higher amounts and default at higher rates than non-politically-connected firms. This effect is only present in government banks.

This paper complements the existing literature on the behavior of government-owned banks by making two contributions. First, it develops a model of private versus government bank lending. The maximization problem for government banks is a convex combination of profits, social benefits, and private political benefits. The model predicts as the cost of political lending – political lending is defined as loans made for the political benefits – changes, the characteristics of borrowers from government banks will also change. The characteristics of borrowers from private banks will remain constant, however, as private banks are not making loans for political purposes.

The second contribution is to use a rich dataset of firms from across the world to directly test the lending decisions of government versus private banks as the cost of political lending varies. With the dataset I have compiled, I am able to analyze government bank lending at the firm-level for a large cross-section of countries. The empirical analysis is done at the firm level both across countries and within sub-national regions across countries. To my knowledge, no model has been developed nor any cross-country, firm-level study has looked at the characteristics of the firms that received government and private loans and how this changes with the cost of political lending.

To capture the cost of political lending at the country level, I use the concentration of media ownership (private versus government-owned media). Brunetti and Weder (2003), Djankov et. al. (2003), and Leeson (2008) provide evidence that having a freer press and having a higher concentration of press that is privately-owned increases the cost of political corruption. Directly it increases the chance of being exposed and indirectly it creates a more interested, active political base that will be more successful at removing corrupt politicians.

At the sub-national region, I use hand-collected data on the contestability of parliamentary elections. Peters and Welch (1980), Krause and Mendez (2009), and Ferraz and Finan (2008) show that corruption and accusations of corruption impact re-election hopes, especially in close elections.

In this chapter, I provide evidence that (a) government-owned banks lend to firms with motivations that are different than privately-owned banks, (b) at the country level, while private banks do not vary the characteristics of the firms in their lending portfolio as the concentration of government or private press varies, government banks do, and (c) at the sub-national region level, government banks similarly vary the characteristics of the firms in their lending portfolio as the closeness of parliamentary elections varies whereas private banks do not.

Point (a) is consistent with both views of government banks. Importantly, these theories state the existence of government banks for the purpose of serving a different set of firms than is being lent to already. The contributions of points (b) and (c), however, help to distinguish the political from the social view. The results are consistent with the political view of government-owned enterprise.

Section 2.2 presents the model. Section 2.3 summarizes the available data and provides summary statistics, section 2.4 presents the methodology and regression results, and section 2.5 discusses potential issues and future research.

2.2 Model of Private vs. Government Bank Lending

This section develops a model to generate theoretical predictions for how lending by government and private banks may diverge as the cost of political lending varies.

In period 0, a risk-neutral firm has the opportunity to invest in one unit of capital, V, which will produce cash-flows, C, in periods 1 and 2 where $C_1 = C_2$ and is immediately observable to all parties. The firm has retained earnings and is liquidity constrained, i.e. $V > R$. To

invest, the firm must post collateral K to take out a bank loan $L = V - R$ at interest rate r to be repaid in period 1.

If the firm is unable to pay back the loan in period 1, the bank will seize the collateral and the capital, V, which will be assumed to have zero re-deployable value. The firm will keep the first period cash-flow, C^L, but the collateral will be seized and the capital utilized by the bank at a fraction, $0 < f < 1$, of the level that the firm operated. This could be thought of as the bank incurring a cost to collect collateral and run the firm leading to efficiency losses.

Cash flows will be "high" with probability λ or "low" with probability $(1 - \lambda)$ where $C^L < (1+r)L < C^H$ and $E[C] = \lambda C^H + (1-\lambda)C^L$. For the firm to desire to invest in this project we must assume that $2E[C] - (1 + r)L - R > 0$ and to ensure that the project and loan are incentive compatible, i.e. both parties are better off in the "good" state than the "bad" state, we require that $(1 + r)L > f(K + C^L)$ for banks and $2C^H - (1 + r)L > C^L - K$ for firms.

Expected profit of the bank will then be: $\lambda(1+r)L+(1-\lambda)f(K+C^L)-L$ and the expected profit for the firm will be: $\lambda\left[2C^H - (1 + r)L\right]+(1-\lambda)(C^L-K)$. Alternatively, allowing the bank to seize the first period cash-flow, allowing for positive redeployment of the capital, and allowing for differences in the efficiency of collateral seizure and the bank operation of the firm will change the payoffs to the bank and firm but will not change the implications of the model.

Bank Maximization Problem:

The bank will maximize expected profits, given that the (IR) constraint of the firm is satisfied:

$$\max E[\Pi] = \lambda(1 + r)L + (1 - \lambda)[f(K + C^L)] - L$$

$$s.t. \;\; \lambda[2C^H - (1 + r)L] + (1 - \lambda)(C^L - K) \geq 0 \;\; (IR_{firm})$$

Because bank profit is increasing in the collateral requirement, the firm's IR constraint will

bind and therefore, $K^* = \frac{\lambda}{1-\lambda}\left[2C^H - (1+r)L\right] + C^L$. With perfect foresight, the bank will be able to earn expected profits equal to: $E[\Pi] = f\left[\lambda 2C^H + (1-\lambda)2C^L\right] + \lambda(1+r)L(1-f) - L$ and the expected profit of the firm is simply zero. For simplicity I am not allowing for renegotiation in period 1. Renegotiation will lead to a more efficient outcome, but will not change the implications of the model.

Additionally, there are ways we could modify the model so that the firm will get profits for itself. For example, assuming a competitive banking sector could yield this result, but the current set-up is sufficient to get the theoretical predictions for this paper.

2.2.1 Government-Owned Banks

Whereas it seems reasonable to assume that privately-owned banks are trying to maximize their bottom-line, analyzing government bank behavior with this same assumption is problematic. Indeed, both of the theories of government sponsored enterprises highlight the existence of these institutions for reasons other than simply maximizing profits.

The political theory, for example, emphasizes that government-owned banks are a way for politicians to extract private benefits. Therefore, the utility function of the government bank will take the form of a convex combination of profits, social welfare/curing market failures and political benefits:

$$\max \alpha\Pi + \beta\,(\text{Social Welfare \& Market Failures }) + (1-\alpha-\beta)\,(\text{Private Political Benefits})$$

The choice variable for the government banks is the firms to which they lend. We can immediately see that our assumption about private banks implies that for private banks $\alpha = 1$ as it is not concerned with social welfare nor the political benefits that politicians receive. The implication of $\alpha = 1$ for private banks is that (in general) the composition of borrowers will differ between private and government sources.

In addition to this more general utility function, I will define private political benefits as

$PB(E, M)$ where private benefits are increasing in the size of firm to which the government lends, E, and is decreasing in the cost of political lending, M, $PB_E > 0$, $PB_M < 0$. Remember that the cost of political lending is increasing in privatized media and closeness of elections.

Private political benefits increasing in the size of the firm is consistent with the evidence that larger firms are more politically active (Boddewyn and Brewer, 1994; Hansen and Mitchell, 2000; Masters and Keim, 1985; and Schuler, 1996) and is in line with the literature showing that larger firms having more disposable resources to use for political purposes (Hillman and Hitt, 1999 and Schuler and Rehbein, 1997). Private political benefits decreasing in the cost of political lending is trivially analogous to private media increasing the cost of corruption.

I will assume that these benefits are increasing in firm size at a decreasing rate, $PB_{EE} < 0$, and that as the cost of political lending increases, increases in firm size have a smaller change in private benefits, $PB_{EM} < 0$. This amounts to it being harder to extract incrementally higher private benefits from a larger firm as the media environment lends itself less to political lending (or elections are close). I will assume that $\beta = 0$ to find theoretical predictions to analyze whether government bank actions are consistent with the political view. Empirically this will not have an effect as long as the variation in the firms that will cure market failure or maximize social welfare across countries is uncorrelated with the concentration of media.

Lastly, I will redefine the fraction of collateral that is seized (and the ability of the bank to run the firm), as a decreasing function of firm size $f^g = f(E) \leq f^p$ where $f^g_E < 0$. Importantly, the government bank will only repossess fraction $f(E)$ and the firm will keep the difference between the private bank f^p and $f(E)$ which comes from it being more costly for government banks to repossess collateral. You can think of f^p as the fraction of efficiency that is retained by a bank and then $(f^p - f(E)) > 0$ as rents that the government bank leaves to the firms that they lend, because it is politically costly to repossess collateral. $(1 - f^p)$ is the efficiency loss when a government bank seizes collateral which is the same as

a private bank. The difference is that part of the rents that government bank potentially could seize is left for the firm.

Note: An implication of assumptions in this model is that the decision to request a government bank loan as opposed to a private bank loan does not reveal any information about the firm itself, and it does not affect the composition of firms that will be requesting loans, i.e. the assumptions rule out the possibility that because government banks begin lending to larger firms, private banks will have fewer large firms to lend. This implication will be discussed in the next section, but because of the low rejection rate of loans and low number of loan applications per firm, it does not appear that firms are going to private banks after being rejected from government banks or vice versa.

Government Bank Maximization Problem

With these modifications from the private bank maximization problem, we can write the government bank's maximization problem as:

$$\max E[\Pi] = \alpha \left[\lambda(1+r)L + (1-\lambda)[f(E)(K+C^L)] - L\right] + (1-\alpha)PB(E,M) - L$$

$$s.t.\ \ \lambda[2C^H - (1+r)L] + (1-\lambda)\left(C^L - K + (f^p - f(E))\,K\right) \geq 0 \ \ (IR_{firm})$$

Note again that government bank utility is increasing in collateral, so the firm's IR constraint will bind and $K^g = \frac{1}{1+f(E)-f^p}\left[\frac{\lambda}{1-\lambda}\left(2C^H - (1+r)L\right) + C^L\right]$, which is decreasing in $f(E)$ and increasing in firm size. We can also see that if $\alpha = 1$ and the government bank is lending to small firms, i.e. $f(E) \approx f^p$ then the function for collateral will be the same between the private banks and government banks.

Instead of only making loans that will maximize profits, the government bank will maximize its expected utility by optimally choosing the size of the firm such that the first order condition is satisfied which leads to the following:

Lemma 1 *For Government banks:*

1. *If* $\alpha < \alpha^*$ *then:* $\frac{\partial E}{\partial M} < 0$*, whereas if* $\alpha \geq \alpha^*$ *then* $\frac{\partial E}{\partial M} = 0$
2. *If* $\alpha < \alpha^*$ *then:* $\frac{\partial E}{\partial \alpha} < 0$*, whereas if* $\alpha \geq \alpha^*$ *then* $\frac{\partial E}{\partial \alpha} = 0$

   For Private Banks:

3. $\frac{\partial E}{\partial M} = 0$

(Proof in appendix 2.9)

Intuitively, (1) says that government banks that are sufficiently politically motivated will lend to smaller firms as the cost of political lending increases. (2) says that as the degree to which government banks care about political benefits increases, they will lend to larger firms. (3) says simply that as the cost of political lending increases, there will be no changes in the composition of firms borrowing from private sources. The important empirical prediction from the lemma is: *if government banks are politically motivated, as the cost of political lending changes, they will lend to a different composition of borrowers whereas private banks will not.*

The remainder of the paper will empirically test whether there is support for modeling government banks differently than private banks in this way. If it is true that government banks are (to a sufficient degree) maximizing private political benefits, then as media ownership becomes more privatized (or elections become closer), we will see government banks lending to smaller firms. If they are not, however, then as the media-ownership environment changes, the composition of firms that are borrowing from government banks will remain constant. The next section describes the available data.

2.3 Available Data and Summary Statistics

2.3.1 Firm-Level Data

The firm level data come from the World Bank Enterprise Surveys. The Enterprise Surveys focus on emerging economies, but these surveys have been completed in over 125 countries, with a total of over 120,000 firms since 2002, when the surveys began. The purpose of the survey is to try to understand how the business environment affects the performance of businesses. The questionnaires include questions about the ownership structure of the firm, the firm's infrastructure, firm sales, obstacles to firm growth, and the firm's financing, among others.

One of the issues with this dataset is that not all questions are asked in each year for each country which, depending on the questions you wish to use, can severely limit the sample size. For example, the question of whether the firm received a government bank loan was not asked in the 2003 survey for Brazil, but it was in the 2009 survey.

The question of interest for this paper is the source of the most recent loan that a firm received. I define *Government Loan* equal to 1, if the most recent loan was from a government source, and 0 otherwise. *Private Loan* equals 1 if the most recent loan was from a private source, and 0 otherwise. Because the question only refers to the most recent loan, there are three potential outcomes: 1 for *Government Loan* with a 0 for *Private Loan*. 1 for *Private Loan* and 0 for *Government Loan*, and a 0 for both *Government* and *Private Loan*. A 0 for both implies that the firm has never had a bank loan. Unfortunately, this question was only asked in the files starting from 2006 onward and was not asked in all countries. Prior to 2006, firms were asked whether they had a bank loan, which included both private and government banks.

Table 2.3 provides the summary statistics[1]. We see that the average firm is about 13 years

[1]The 21 countries that are in the cross-country tables are: Argentina, Armenia, Brazil, Bulgaria, Chile, Colombia, Croatia, Czech Republic, Estonia, Ghana, Hungary, Kazakhstan, Lithuania, Peru, Poland, Russia, Slovak Republic, Slovenia, South Africa, Tanzania and Uganda.

old and has about 29 employees, so these are not huge firms. They are not Wal-Mart. Additionally, 8% of firms reported that their last loan was from a government bank whereas 41% reported it was from a private bank, which means that just under half of our sample has ever had a bank loan. Admittedly, using the most recent loan is potentially problematic if the firms are receiving many loans. However, the average time since the firms received their last loan is 1.75 years. This means that these firms are accessing banks infrequently which lessens this potential issue.

Another potential issue is that firms may be sequentially applying for loans, i.e. a firm may first apply to a government bank, and if rejected apply to a private bank. But this does not appear to be the case because the average number of loan applications is .67 loan applications per firm. Conditional on having a loan, the application rate is 1.13 per firm with only .11 loan rejections.

Conditional on having a loan, we see that firms tend to be a bit older and larger, which is as expected. They also appear to be more productive. Interestingly, the fraction of firms that reported corruption or finance as an obstacle to growth is fairly consistent, whether they have a bank loan or not. About 30% of firms reporting either as an obstacle. Panels C and D of Table 2.3 are the same as Panels A and B, but including only firms in the sub-national region analysis.

2.3.2 Country Data

To understand the impact of differences in the cost of political lending on the composition of borrowers from government versus private sources, I use the concentration of government- versus privately-owned press from Djankov et. al. (2003). The four variables of interest are: *Government Press - Count, Government Press - Share, Private Press - Count, Private Press - Share.* These measures were constructed by taking the top 5 press outlets in each country by circulation. *Government Press - Count* is the number of the top five press outlets owned by the government. *Government Press - Share* is the market share of the top five press outlets owned by the government. These measures are then normalized so a

value of 0.2 for *Government Press - Count* in Russia means that 1 of the top 5 press outlets is owned by the government. Uganda's 0.58 for *Government Press - Share* means that the government owns 58% of the market share of the top 5 press outlets. Note: government press is not just one minus private press. The Djankov et. al (2003) data also has *Other Press - Count* and *Other Press- Share*. The sum of government, private and other is one.

To control for economic development, I control for the natural logarithm of per capita gross domestic product from the World Development Indicators, and to control for the development of the financial system, I control for the total market capitalization as a fraction of GDP from Beck et al. (2001). Additionally, to control for the prevalence of government ownership of banking assets, I include the percentage of assets in the banking system that are more than 50% owned by the government from Barth et al. (2006). Depending on the specification, I include the *Control of Corruption Index* from Kaufmann, et. al (2008) which is a measure of freedom from corruption, meaning that higher values for corruption mean less corruption and *Democracy* from the Freedom House Polity IV database which takes a value from 0 to 10 where 10 is the most democratic (Cheibub and Gandhi, 2004).

2.3.3 Sub-National Region Data

Admittedly, running cross-country regressions, albeit at the firm level, creates issues about the comparability of the government banking system's goals across countries. For example, government banks in Argentina may have different goals than government banks in Brazil.

To quell this issue, it would be helpful to have another "cost" of political lending that can be collected within country. The cost that I focus on is the potential threat of losing an election. To proxy for this, I collect data on the closeness (or contestability) of elections. I define Election Contestability $= 1 - \frac{\text{first place votes}-\text{second place votes}}{\text{first place votes}+\text{second place votes}}$, which takes a value of 0 if the first place vote-getter gets all of the votes to 1, where first and second place get the same number of votes. The advantage of this measure is that it can be collected within country, at the regional level, whereas the media ownership data is only at the country-level. Because the firm-level data is coded to the regional-level, i.e. I know whether a firm from

Argentina is located in Buenos Aires or Cordoba or one of the other provinces, I am able to match the location of the firm with the contestability of the elections in its region.

To collect the regional data, I first use Election Resources (http://electionresources.org), which has formatted election data for the most developed countries, to collect data on the parliamentary election that is nearest to the year of the survey for each country. When formatted data was unavailable on Election Resources, I collected by hand the data from the bureau of elections for each country. Table 2.2 has the election specific details and the sources for each of the 17 countries from which I was able to collect.

I merge the regional election data to the Enterprise Surveys which includes firm regional identifiers. The identifier is imperfect because the level that the regions are defined varies between countries. For example, Brazil has identifiers at the state level whereas Ukraine has directional identifiers, e.g. East-Ukraine vs. West-Ukraine. Therefore, for all of the regional variables I aggregate to the level of the Enterprise Surveys.

The average election contestability for the whole sample is .73 which means that: $.27 \times (First + Second) = (First - Second)$ the first place vote getter on average received about 65% of the total vote between the first and second place candidates and the second place vote getter earned about 35%. In the median election, however, the first place vote getter only earned 60% of the vote.

Table2.4 shows t-statistics of differences in the firm characteristics from high to low government owned media and close versus not-close elections. We see that there are significant differences in all the firm characteristics between high and low government ownership of the media for government bank loans and differences in age and size for private bank loans. The magnitude of the differences in firm size is larger for government banks, but the t-statistic is larger for private banks. This is perhaps because there are more firms with private bank loans. The differences in the election measures do not show up significant for government or private banks. However, as elections are closer, government banks lend to smaller firms, whereas private banks seem to lend to larger. The t-statistics are low, but the direction is as expected for government banks.

Figures 2.1 and 2.2 show scatter plots of the size of the firm and government concentration of media and election contestability, respectively. These figures mimic the results in Table 2.4. Variable definitions and summary statistics for all of the variables are provided in Tables 2.1 and 2.3, respectively. The next section provides econometric methodology and results.

2.4 Methodology and Results

2.4.1 Initial Specification

In Table 2.5 Panel A I use all of the firms in the sample and run two separate probit regressions that predict the probability getting a government (private) loan versus not getting a government (private) loan.

$$Pr\left(\text{Government Loan} = 1|\mathbf{X},\mathbf{Z}, \delta_{industry}\right) = \Phi\left(\alpha + \mathbf{X}\beta + \mathbf{Z}\mu + \delta_{industry} + u_i\right)$$

$$Pr\left(\text{Private Loan} = 1|\mathbf{X},\mathbf{Z}, \delta_{industry}\right) = \Phi\left(\alpha + \mathbf{X}\beta + \mathbf{Z}\mu + \delta_{industry} + u_i\right)$$

Where $\mathbf{X}$ is a vector of firm-level characteristics: $\ln\left(age\right)$, $\ln\left(\#\text{ of employees}\right)$, $\ln\left(\frac{sales}{employees}\right)$ and $\ln\left(\frac{electricity}{employees}\right)$. And $\mathbf{Z}$ is a vector of country-level characteristics: $\ln\left(\text{GDP per Capita}\right)$, *Stock Market Capitalizations to GDP*, and *Government Bank Assets*. All of the probit regressions in this and the next section also include industry-level fixed effects and robust standard errors are clustered at the country level.

In Panel B, I use only the firms that have either a government bank loan or a private bank loan. I run the following regression:

$$Pr\left(\text{Government Loan} = 1|\mathbf{X},\mathbf{Z}, \delta_{industry}\right) = \Phi\left(\alpha + \mathbf{X}\beta + \mathbf{Z}\mu + \delta_{industry} + u_i\right)$$

where now *Government Loan* takes a value of 1 if the firm has a government bank loan, and 0 if the firm has a private bank loan.

Panel A shows that the probability of getting a government loan is increasing in the size of the government banking sector and is increasing in the productivity of the firm. The probability of a government loan increasing in firm productivity seems consistent with stories where government banks lend to firms that could have received private banking financing, which does not seem consistent with the social view. The probability of getting a private loan is decreasing in government bank assets and increasing in financial development. Panel B shows similar results, namely that conditional on having a bank loan, the size of the government bank sector is positively correlated with the probability of getting a government bank loan. Interestingly, as firm size increases, the probability of getting a government bank loan decreases.

2.4.2 Cross-Country Regressions: Effect of Media Ownership

Well functioning, civic-minded media can effectively act as a fourth branch of government, a branch whose purpose is to inflict a cost of corruption. As shown in the model, if government banks are politically motivated, more intense (or varying) media scrutiny will change the optimal firm to which the bank will lend. This will not be true for private banks.

To test the model, I add interaction terms into the initial specification of the regressions. I therefore run two separate regressions:

$$Pr\left(\text{Government Loan} = 1|\mathbf{X},\mathbf{Z}, Press, \delta_{industry}\right) = \Phi\left(\alpha + \mathbf{X}\beta_g + \mathbf{Z}\mu_g + \gamma Press + \phi_g \mathbf{X} Press + \delta_{industry} + u_i\right)$$

$$Pr\left(\text{Private Loan} = 1|\mathbf{X},\mathbf{Z}, Press, \delta_{industry}\right) = \Phi\left(\alpha + \mathbf{X}\beta_p + \mathbf{Z}\mu_p + \gamma Press + \phi_p \mathbf{X} Press + \delta_{industry} + u_i\right)$$

If the hypothesis is true that government banks are optimizing differently from private banks, conditional on the media environment, we would expect that:

$$\phi_g \neq 0 \text{ and that } \phi_p = 0$$

Table 2.6 presents the first evidence that this is indeed true. The first important point from this table is that we do not see an effect for private banks. If we observed that the interactive effect for private banks was significant, then this would present problems with

either the model which assumes that private banks are not maximizing political benefits or the measure we are using (media) to isolate the cost of political lending is wrong. Because we do not see an effect for private banks, we can be more confident that the model and specification we are running is not inherently flawed from the start.

Given that the interaction terms on the firm-level characteristics and the media environment for private banks are insignificant, we can try to assess whether there is an effect for government banks. We observe that these interaction terms are significant and that with more government-owned media, firms receiving government loans are differentially older and larger (more employees), whereas privately owned media is associated with a decrease in the size of firms receiving loans. This result holds for both the *Count* and *Share* definitions of the press variables.

In Table 2.7, I restrict the sample of firms to those that have received a bank loan so the dependent variable takes a 1 if government bank loan and 0 if private bank loan[2]. We again see that $\phi_g \neq 0$ with a significant negative coefficient on the interaction between private press concentration and firm size (-0.0266) implying that as the cost of political lending increases the characteristics of the borrowers from government and private banks diverge.

2.4.3 Robustness of Cross-Country Regressions

A valid criticism of the regressions in Table 2.6 and Table 2.7 is that they do not rule out the possibility that the interaction of press ownership with firm level characteristics is significant because of the correlation between the role government-ownership of banks, the media environment, and some other variable like income per capita or financial development. For example, it is plausible that rich countries (or countries with more developed financial markets) tend to have more private media and a role for government owned-enterprises that is different than poor countries. Further, as documented by Alfaro, Charlton and Kanczuk

[2]Note: The rest of the cross-country results presented in the tables only include firms that have a government or private bank loan. One may be concerned about doing this because perhaps the firms that do not have any bank loans may drive the results and so running separate probit regressions for government and private loans may be in order. For space, I do not include these tables, but the bottom line is that the results do not change.

(2008), the distribution of the size of firms is correlated with economic development which may be the effect we are capturing in Tables 2.6 and 2.7. If either of these were the case, excluding the interaction between income per capita and the firm-level characteristics may lead to significant interaction terms in Tables 2.6 and 2.7 even if the interactions are not significant.

In Table 2.8, I run the same regressions as in Table 2.7, but instead of the interactions of media concentration with the firm level characteristics, I include interactions between firm characteristics and income per capita. The results show that none of the interactions is significant. Alternatively, including interactions with conventional financial market development measures yields the same insignificant results (not shown).

Table 2.9 then includes these interactions to the specification in Table 2.7. We see that the interactions between media concentration and the firm characteristics remain significant, and in many cases get larger while the income per capita and financial development (not shown) interactions remain insignificant.

While many "good" outcomes, such as the absence of corruption, are highly correlated with income per capita, one could still criticize Table 2.9 by arguing that there could be another variable, that is independently correlated, driving the results. In order to address this problem, in Table 2.10 I run the same regressions as in Table 2.9, but include additional interaction terms between firm-level characteristics and the Kaufmann corruption index. Note that in these tables the interactions of the firm-level characteristics and income per capita (columns (1)-(4)) and financial development (columns (5)-(8)) are suppressed. We see again that the interaction terms with media concentration and the firm characteristics are highly significant. The interactions between corruption and the firm characteristics are also significant, but there appears to be a robust independent effect of government concentration of the media and the firms to which they are lending.

As a final note on the cross-country regressions, it is important to mention that these results are robust to different specifications as well as the different controls. Including fewer firm-level controls will allow us to expand the sample as, for example, electricity expenditure

per employee may be missing for a set of firms. Running these same regressions but with different combinations of firm-level controls does not change the results. As an additional robustness check, I rerun Table 2.7 but omitting one country at a time to see if this changes the results. The coefficients on the interaction between media concentration and firm size remain remarkably stable and remain significant. (Tables are omitted but available upon request from author)

In summary, the cross-country regressions show strong and consistent support for the hypothesis that government banks lend to a different composition of borrowers depending on media concentration. Not only do the interaction terms between media concentration and the firm-level characteristics show up significant in the regressions, but the interactions with other country-level measures including income per capita and financial development do not. Using private banks as a counterfactual (Table 2.6) to ensure that we are capturing the effect of government versus private banks as opposed to just country-effects, shows that this effect is driven by differences in government and private banks, not country-effects.

2.4.4 Sub-National Region Regressions: Effect of Election Contestability

The potential issues with running cross-country regressions in this framework are numerous. I will try to address some on them in the next section, but if this same result were to hold for government versus private bank lending at the sub-national level, then we can be more confident in the results. To try to understand the cost of political lending within country I exploit regional election data.

In Table 2.11, I run the same probit regressions as in Table 2.6 but instead of the media variables at the country level, I use election contestability at the regional level. The theory is that in areas with close elections, there is a higher cost to political lending than in areas with landslide elections. To put this into the context of the model in Section 2.2, having a landslide election has the same comparative static effect as having high government-owned media, but calculated at the regional level. Therefore, the prediction is that the interaction between the firm-level characteristics and election contestability will show up insignificant

for private but not government banks.

Econometrically, the specification in Table 2.11 is:

$$Pr\left(\text{Government Loan} = 1|\mathbf{X}, Election, \delta_{industry}, \delta_{country}\right) =$$

$$\Phi\left(\alpha + \mathbf{X}\beta_g + \gamma Election + \phi_g \mathbf{X} Election + \delta_{industry} + \delta_{country} + u_i\right)$$

$$Pr\left(\text{Private Loan} = 1|\mathbf{X}, Election, \delta_{industry}, \delta_{country}\right) =$$

$$\Phi\left(\alpha + \mathbf{X}\beta_p + \gamma Election + \phi_p \mathbf{X} Election + \delta_{industry} + \delta_{country} + u_i\right)$$

Where again $\mathbf{X}$ is the set of firm-level controls, but instead of country-level controls I include country fixed-effects. *Election* is the measure of election contestability. If the hypothesis is true that government banks are optimizing differently conditional on the regional electoral environment than private banks, this would imply that:

$$\phi_g \neq 0 \text{ and that } \phi_p = 0$$

Looking at columns (1) and (2) of Table 2.11, we see that the contestability of elections has a negative and insignificant coefficient for government banks and positive and insignificant for private banks. This means that as elections get closer, firms are less likely to receive a government loan and more likely to receive a private loan.

Looking at the interactive effects of elections on firm-level characteristics (columns (3) and (4)), we can see that the interactions between the firm-level characteristics and election contestability in the private bank regressions are insignificant except for productivity. This significant interactive effect of elections and productivity in the private bank regressions is very consistent. For government banks, the results are consistent with the cross-country tables. The interactions for size and age show up significant and in the expected direction for the government borrowers.

While the intuition behind the result is the same as the cross-country regressions, the result

is in some ways is stronger because using within country variation allows us to add country fixed effects. A large obstacle to understanding Tables 2.6 is that the goals of government banks may vary across countries. Table 2.11 is able to control for this and yields a similar result.

Similar to the criticisms of Table 2.6, a valid criticism of columns (3) and (4) is that it does not eliminate potential correlations between the way that government banks react to the closeness of elections depending on corruption or democracy. It is reasonable to argue that corrupt governments (or undemocratic) governments will react differently to electoral results. Columns (5) and(6) add in interaction terms between corruption and democracy with the firm characteristics to see if this changes the results.

The interaction between elections and age remains very strong in all of these regressions for government bank loans. The interaction between size and elections similarly remains significant with a consistent point estimate. Columns (7) and (8) of Table 2.11 runs the same regressions as columns (3) and (4) but only includes firms that have a bank loan. Interestingly, for the most part the interactions do not show up significant. The direction of the coefficients is as predicted by the model with a t-statistics above 1, but the result is not robust to this specification.

In summary, the cross-country regressions are remarkably robust to different specifications, using all the firms or just firms with loans and different subsets of countries. The sub-national regional election results appear to be less robust, and when conditioned on having a bank loan, the results disappear. The signs on the coefficients remain consistent with the model.

2.5 Potential Issues and Future Research

While this study has clear advantages over previous work, there are still limitations and concerns that are worth noting. A first concern is that the question that I am using to determine from where the firm borrows only covers its most recent loan. It is conceivable,

and probably true in some cases, that firms received their last loan from a government source, whereas they also have previous loans from private banks or vice versa. While this is a legitimate concern, it is reasonable to assume that on average where firms got their most recent loan is correlated with the composition of loans they have. Further, as discussed in section 2.3, it does not appear that firms are borrowing frequently, so while the average firm may have a few outstanding loans, it does not appear to be the case that they will have hundreds.

A second concern is the degree to which firms are requesting loans from government versus private sources. There is no data on the number of government loans for which firms applied. Perhaps it is the case that firms are selectively applying for loans as opposed to government banks lending strategically. For this to negate the findings of this paper, there would have to be a correlation between the "type", e.g. size, age, etc., of the firms applying to government banks and the media/electoral environment. While this could be true, it seems more plausible that the type of firm applying for government resources would be correlated with financial development or income per capita, in general. Regressions in Table 2.9 control for these interactions which does not diminish the interactive effect through media concentration, and the economic development and financial development interactions are not significant. This does not rule out the possibility that the "type" of firm that requests a government loan is correlated with media concentration. It does make the case harder to make, however - especially considering the low loan rejection rate reported in the data.

Third, while I do have data on the firms that borrowed from government banks, I do not have data on a complete set of loan characteristics. The dataset provides information on the size, duration, and collateral requirements, but it does not include interest rate data. Previous studies, namely Sapienza (2004), conclude that the interest rate is an important difference between government and private loans - government banks may be able to fidget with interest rates to provide benefits for borrowing from them as opposed to private banks. The model ignores this by assuming a correctly risk-adjusted interest-rate for both private and government banks. In reality, this may not to be true; however, the contribution of

this paper is to assess the composition of the firms that are connected to government banks (which has not been done), not the realized cost of capital associated with these loans (which has been done).

Fourth, one may argue that while it appears that there is evidence that government banks change their lending portfolio as the cost of political lending varies, this does not rule out the social view. This is plausible, but, for this to be consistent one would need to argue that market failures are uncorrelated with financial development or economic development, but correlated with the media concentration. This is a difficult case to make, but one could try.

Concerning the sub-national region analysis, a fifth concern is whether elections are a good way to quantify the "cost of political lending." What comes with elections is big benefits to the winner and big losses to the loser. Apart from all the endogeneity in what causes close elections, politically lending may provide benefits that are differentially larger in close elections. This would imply that my discussion of close elections being a "cost" to political lending is incorrect as it is actually a "benefit". However, if elections increase the potential benefits from political lending and government banks are political lending, this too would cause changes in the composition of firms that borrow from government banks in areas with close versus land-slide elections. Including the same interactions as in Table 2.11 would be correct, but the interpretation would be different.

Even if my use of elections as a cost is correct, one may be concerned that elections are conducted differently in different countries. For example, some countries use proportional representation voting as opposed to direct elections. The interpretation used in this paper regarding the contestability of elections is that it changes the cost imposed on politicians of political lending. While it is true that the cost structure may be different between direct and proportional representation systems, all that is needed for my interpretation to be correct is that as election results are closer the cost of political lending increases. This is consistent regardless of the electoral system, but it is worth noting the differences in voting structure. In the end, country fixed-effects are included which controls for these differences in electoral

systems and exploits variations within country in the contestability of elections.

A final concern is the estimation procedure used. As Ai and North (2003) points out, it is potentially problematic to use interaction terms in probit analysis. As an additional robustness check, I ran the tables with a linear probability model which yielded the same qualitative results as the probit analysis. The significance of the interactions in the media regressions was weakened (but for the most part remained significant) and remained in the election regressions. One may also argue that instead of running two separate regressions, running a multinomial logit may be more correct with three choices: government loan, private loan and no bank loan. Theoretically, this may be problematic because two of the choices are quite similar - the Independence of Irrelevant Alternative (IIA) assumption of the multinomial logit is that adding another alternative or changing the characteristics of one of the alternatives does not affect the relative odds between two alternatives under consideration, which could be problematic in the context of similar alternatives. Having the option to apply to a government bank or private bank as opposed to only a government bank or only a private bank clearly violates this assumption.

Future research on this topic could include trying to identify the channel through which private benefits from large firms to government officials flow. This paper is agnostic to the channel. Government banks may be lending to obtain lobbying or campaign contribution dollars, but they may also be lending to large firms as a way to promote excess employment which will be helpful in politically contestable elections. Understanding more deeply the precise channel would help in the interpretation of the results.

Further, trying to understand empirically and theoretically how electoral politics influences government banks would be useful. While my use of elections may not be the best way to measure the cost of political lending, the innovation in using regional election data in this analysis is something that can be built on in future work. For example, one could potentially look at close elections as a way to identify (a component) of exogenous variation in which political party was elected to look at differences to whom parties prefer to lend.

2.6 Conclusion

This paper looks directly at firms which have government versus private bank loans to gain insights into the lending behavior of banks as the cost of political lending changes. It contributes both theoretically by developing a model of government banking and empirically by using a rich cross-country dataset that allows me to directly connect firms who borrowed from government banks as opposed to private banks. The empirical work uncovers three basic facts:

First, motivations of government-owned banks are different from privately-owned banks. As discussed, this is highly expected as neither of the theories of the existence of public enterprise predict similar lending patterns.

Second, in a cross-section of countries, while private banks do not change their lending portfolio as the concentration of government or private press varies, government banks do. The interpretation of this is that as the cost of political lending changes, government banks react, but private banks do not.

And third, looking at within country variation by electoral region, government banks similarly change their lending portfolio as the closeness of parliamentary elections changes whereas private banks do not.

I interpret these results as being consistent with the political view of government-owned enterprise.

2.7 Figures

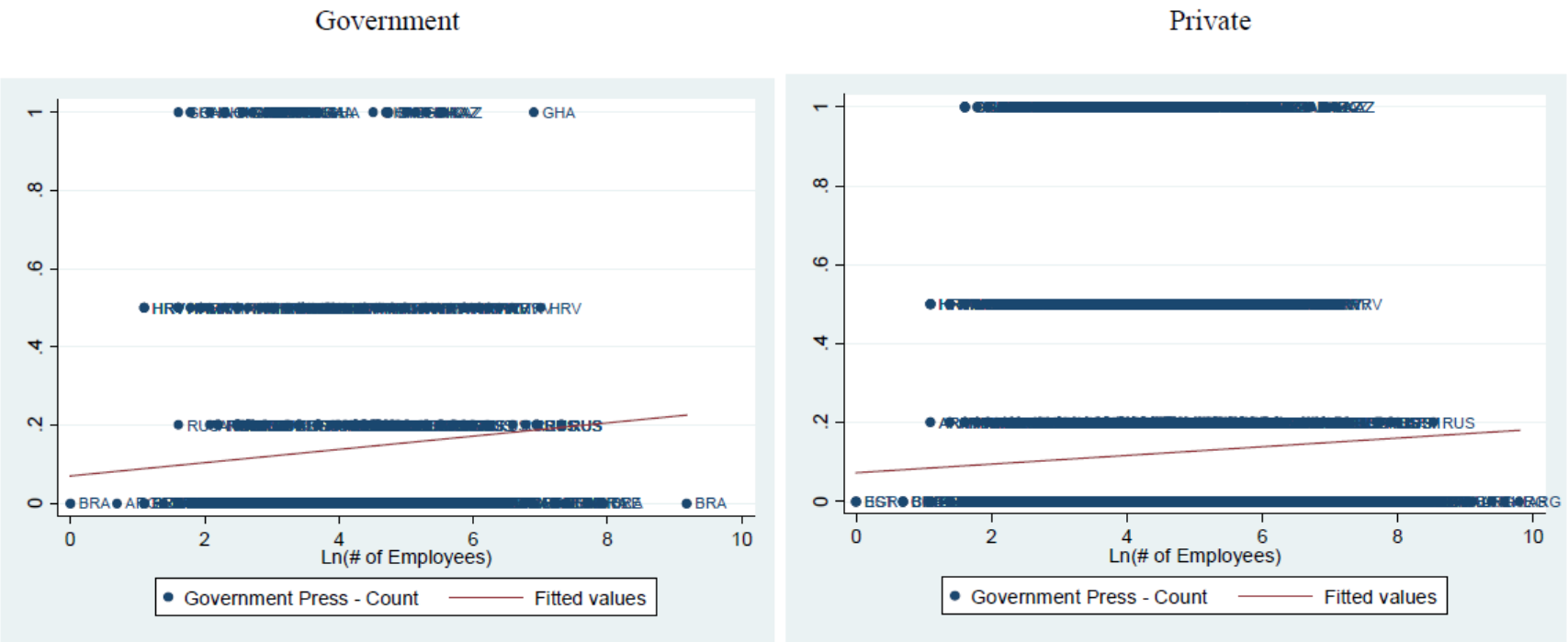


Figure 2.1: Government Press (Count) vs. Ln(Number of Employees)

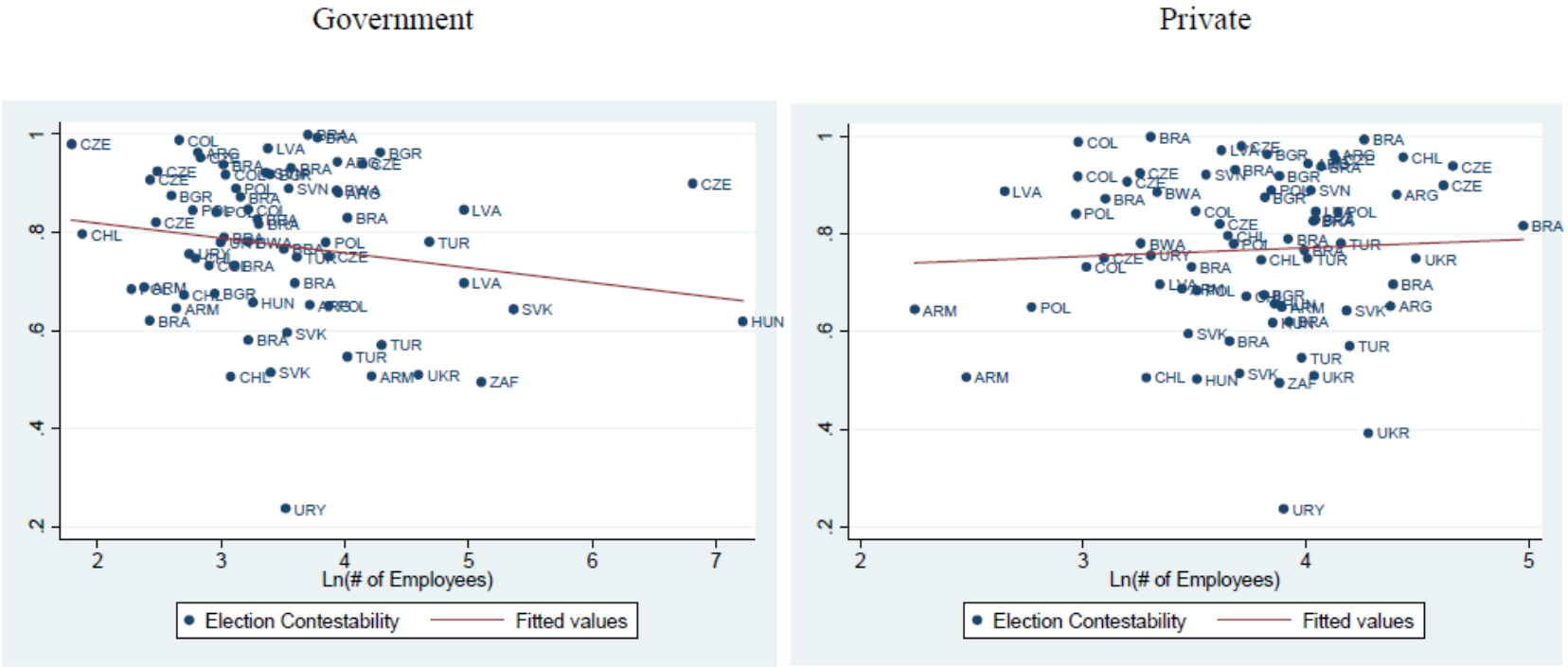


Figure 2.2: Election Contestability vs. Ln(Number of Employees)

2.8 Tables

Table 2.1: Variable Definitions

Variable	Definition	Source
Age	Ln(# of years the firm in operation)	World Bank Enterprise Surveys
Employees	Ln(# of permanent employees of the firm)	World Bank Enterprise Surveys
Productivity	Ln(Total Sales/Employment)	World Bank Enterprise Surveys
Capital Intensity	Ln(Electricity Expenditure/Employment)	World Bank Enterprise Surveys
Government Loan	Equals 1 if the last loan of the firm was from a government bank	World Bank Enterprise Surveys
Private Loan	Equals 1 if the last loan of the firm was from a private bank	World Bank Enterprise Surveys
Time Since Last Loan	Number of years since last loan	World Bank Enterprise Surveys
Loan Applications	# of Loan Applications Submitted	World Bank Enterprise Surveys
Loan Applications Rejected	# of Loan Applications Rejected	World Bank Enterprise Surveys
Corruption Obstacle	Equals 1 if firm reported that corruption is an obstacle to growth	World Bank Enterprise Surveys
Finance Obstacle	Equals 1 if firm reported that finace is an obstacle to growth	World Bank Enterprise Surveys
GDP per Capita	Ln(GDP per Capita)	World Development Indicators
Government Press - Share	The government sector's market share of the top 5 daily newspapers	Djankov et al. (2003)
Government Press - Count	The fraction of the top 5 daily newspapers that are government owned	Djankov et al. (2003)
Private Press - Share	The private sector's market share of the top 5 daily newspapers	Djankov et al. (2003)
Private Press - Count	The fraction of the top 5 daily newspapers that are privately owned	Djankov et al. (2003)
Government Banks - Assets	Fraction of banking system's assets that are 50% or more government owned	Barth et al (2006)
Market Capitalization to GDP	Ratio of Stock Market Capitalization to GDP	Beck et al. (2001)
Control of Corruption - Estimate	"Captures preceptions of the extent to which public power is exercisd for private gain, including both petty and grand forms of corruption as well as 'capture' of the state by elites and private interests"	Kaufmann et al. (2008)
Democracy	Country-level democracy score ranges from 0-10 with 10 being most democratic	Freedom House Polity IV
Election Contestability	A measure of how closeness of the nearest parliamentery election	Author's Calculation

Table 2.2: Election Sources:

Country	Survey Year	Election Year	Election
Argentina	2006	2007	Chamber of Deputies
Armenia	2009	2007	Parliamentary
Botswana	2006	2009	Parliamentary
Brazil	2009	2010	Chamber of Deputies
Bulgaria	2007	2005	National Assembly
Chile	2006	2005	Chamber of Deputies
Colombia	2006	2006	Chamber of Representatives
Czech Republic	2009	2010	Chamber of Deputies
Hungary	2009	2010	National Assembly
Latvia	2009	2010	Parliamentary
Poland	2009	2007	Polish Sejm
Slovak Republic	2009	2010	National Council
Slovenia	2009	2008	National Assembly
South Africa	2007	2009	National Assembly
Turkey	2008	2007	National Assembly
Ukraine	2008	2006	Supreme Council
Uruguay	2006	2004	National Assembly

Table 2.3: Descriptive Statistics
Panel A: Cross-Country Data - All Firms

Variable	Mean	Median	Observations	Countries
Ln(Age)	2.56	2.64	12,131	21
Ln(Employees)	3.36	3.26	12,131	21
Ln(Sales/Employees)	10.92	10.70	12,131	21
Ln(Elec. Exp/Employees)	6.30	6.05	12,131	21
Government Loan	0.08	0.00	12,131	21
Private Loan	0.41	0.00	12,131	21
Ln(GDP per Capita)	8.05	8.39	12,131	21
Government Press - Count	0.16	0.00	12,131	21
Private Press - Count	0.79	1.00	12,131	21
Government Press - Share	0.16	0.00	12,131	21
Private Press - Share	0.80	1.00	12,131	21
Government Banks - Assets	0.12	0.03	12,131	21
Market Capitalization to GDP	50.68	31.45	12,131	21
Control of Corruption	-0.08	-0.14	12,131	21
Democracy	7.87	8.58	12,131	21
Time Since Last Loan	1.75	1.00	12,131	21
Loan Applications	0.67	0.00	12,131	21
Loan Applications Rejected	0.08	0.00	12,131	21
Corruption Obstacle	0.32	0.00	12,131	21
Finance Obstacle	0.28	0.00	12,131	21

Table 2.3: Descriptive Statistics
Panel B: Cross-Country Data - Only Firms with Loan

Variable	Mean	Median	Observations	Countries
Ln(Age)	2.62	2.64	5,950	21
Ln(Employees)	3.74	3.65	5,950	21
Ln(Sales/Employees)	11.16	10.88	5,950	21
Ln(Elec. Exp/Employees)	6.45	6.15	5,950	21
Government Loan	0.15	0.00	5,950	21
Private Loan	0.85	1.00	5,950	21
Ln(GDP per Capita)	8.05	8.39	5,950	21
Government Press - Count	0.16	0.00	5,950	21
Private Press - Count	0.79	1.00	5,950	21
Government Press - Share	0.16	0.00	5,950	21
Private Press - Share	0.80	1.00	5,950	21
Government Banks - Assets	0.12	0.03	5,950	21
Market Capitalization to GDP	50.67	31.45	5,950	21
Control of Corruption	-0.08	-0.14	5,950	21
Democracy	7.87	8.58	5,950	21
Time Since Last Loan	1.78	1.00	5,950	21
Loan Applications	1.13	0.00	5,950	21
Loan Applications Rejected	0.11	0.00	5,950	21
Corruption Obstacle	0.33	0.00	5,950	21
Finance Obstacle	0.29	0.00	5,950	21

Table 2.3: Descriptive Statistics
Panel C: Sub-Nat'l Region Data - All Firms

Variable	Mean	Median	Observations	Countries	Regions
Ln(Age)	2.62	2.67	8,031	17	75
Ln(Employees)	3.39	3.26	8,031	17	75
Ln(Sales/Employees)	10.70	10.62	8,031	17	75
Ln(Elec. Exp/Employees)	6.08	5.97	8,031	17	75
Government Loan	0.11	0.00	8,031	17	75
Private Loan	0.41	0.00	8,031	17	75
Ln(GDP per Capita)	8.44	8.53	8,031	17	75
Election Contestability	46.82	27.69	8,031	17	75
Market Capitalization to GDP	0.73	0.81	8,031	17	75
Control of Corruption	0.11	0.10	8,031	17	75
Democracy	8.64	9.33	8,031	17	75
Time Since Last Loan	2.09	1.00	8,031	17	75
Loan Applications	0.57	0.00	8,031	17	75
Loan Applications Rejected	0.05	0.00	8,031	17	75
Corruption Obstacle	0.36	0.00	8,031	17	75
Finance Obstacle	0.25	0.00	8,031	17	75

Table 2.3: Descriptive Statistics
Panel D: Sub-Nat'l Region Data - Only Firms with Loan

Variable	Mean	Median	Observations	Countries	Regions
Ln(Age)	2.68	2.71	4,322	17	75
Ln(Employees)	3.67	3.58	4,322	17	75
Ln(Sales/Employees)	10.86	10.85	4,322	17	75
Ln(Elec. Exp/Employees)	6.14	5.89	4,322	17	75
Government Loan	0.19	0.00	4,322	17	75
Private Loan	0.81	1.00	4,322	17	75
Ln(GDP per Capita)	8.44	8.53	4,322	17	75
Election Contestability	46.82	27.69	4,322	17	75
Market Capitalization to GDP	0.74	0.81	4,322	17	75
Control of Corruption	0.11	0.10	4,322	17	75
Democracy	8.64	9.33	4,322	17	75
Time Since Last Loan	2.11	1.00	4,322	17	75
Loan Applications	0.96	0.00	4,322	17	75
Loan Applications Rejected	0.05	0.00	4,322	17	75
Corruption Obstacle	0.35	0.00	4,322	17	75
Finance Obstacle	0.25	0.00	4,322	17	75

Table 2.4: T-Statistics of Differences in Means

Cross-Country	Government Bank Loan				Private Bank Loan			
Variable	High Gov't Ownership	Low Gov't Ownership	Difference	T-Statistic	High Gov't Ownership	Low Gov't Ownership	Difference	T-Statistic
Ln(Age)	2.69	2.80	-0.11	-2.16	2.61	2.71	-0.1	-3.98
Ln(Employees)	4.06	3.48	0.59	6.08	4.2	3.72	0.48	10.42
Ln(Sales/Employees)	11.35	12.14	-0.80	-4.86	11.02	10.97	0.05	0.88
Ln(Elec. Exp/Employees)	6.63	7.49	-0.86	-4.87	6.19	6.21	-0.02	-0.36
Regional	Government Bank Loan				Private Bank Loan			
Variable	High Election Contestability	Low Election Contestability	Difference	T-Statistic	High Election Contestability	Low Election Contestability	Difference	T-Statistic
Ln(Age)	2.64	2.73	-0.09	-0.73	2.73	2.66	0.08	1.07
Ln(Employees)	3.38	3.60	-0.22	-0.95	3.8	3.64	0.16	1.3
Ln(Sales/Employees)	11.33	11.10	0.23	0.61	11.43	11.09	0.34	1.14
Ln(Elec. Exp/Employees)	6.65	6.36	0.29	0.68	6.9	6.39	0.51	1.54

Table 2.5: Baseline Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable:	Gov't Loan	Priv. Loan	Gov't Loan	Priv. Loan	Gov't Loan	Priv. Loan
Panel A: All Firms						
Ln(# of Employees)	-0.000191	0.0874***	-0.000761	0.0890***	-0.000586	0.0885***
	(0.00251)	(0.0114)	(0.00201)	(0.0122)	(0.00199)	(0.0118)
Ln(Age)			0.00357	-0.0104	0.00333	-0.0101
			(0.00438)	(0.00994)	(0.00438)	(0.00972)
Ln(Sales/Employees)					0.00747***	0.0240**
					(0.00148)	(0.0112)
Ln(Elec. Exp./Employees)					0.00261*	-0.0074
					(0.00152)	(0.00664)
Stock Market Cap./GDP	-0.000136	0.000818***	-0.000133	0.000810***	-0.000133	0.000804***
	(0.000164)	(0.000287)	(0.000164)	(0.000289)	(0.000162)	(0.000288)
Gov't Bank Assets	0.214***	-0.768***	0.213***	-0.766***	0.211***	-0.761***
	(0.0546)	(0.0965)	(0.0549)	(0.0968)	(0.0541)	(0.0956)
Ln(GDP per Capita)	0.0392**	-0.0382	0.0383**	-0.0362	0.0386**	-0.0361
	(0.0172)	(0.0289)	(0.0173)	(0.0284)	(0.0169)	(0.0289)
Observations	12,110	12,110	12,110	12,110	12,110	12,110
Panel B: Firms with Gov't or Private Loan						
Ln(# of Employees)	-0.0251***		-0.0278***		-0.0273***	
	(0.00633)		(0.00521)		(0.00494)	
Ln(Age)			0.0146		0.014	
			(0.00964)		(0.00950)	
Ln(Sales/Employees)					0.00817	
					(0.00500)	
Ln(Elec. Exp./Employees)					0.00694	
					-0.00454	
Stock Market Cap./GDP	-0.000491*		-0.000481*		-0.000477*	
	(0.000273)		(0.000270)		(0.000263)	
Gov't Bank Assets	0.605***		0.604***		0.601***	
	(0.0886)		(0.0890)		(0.0850)	
Ln(GDP per Capita)	0.0826***		0.0787***		0.0787***	
	(0.0260)		(0.0261)		(0.0251)	
Observations	5,930		5,930		5,930	

Notes: This table presents marginal-effects probit regressions of having a government (or private) loan on firm-level and country-level variables. "Govt. Loan" takes a value of 1 if their most recent loan was from a government bank and 0 otherwise. "Priv. Loan" takes a value of 1 if their most recent loan was from a private bank and 0 otherwise. The regressions include industry fixed effects and standard errors are clustered at the country level. All regressions include the following controls: stock market capitalization to GDP ratio, income per capita, and the fraction of bank assets government-owned. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.6: Cross-Country: Media Regressions - All Firms
Panel A: Government and Private Press - Count

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable:	Gov't Loan	Priv. Loan	Gov't Loan	Priv. Loan	Gov't Loan	Priv. Loan	Gov't Loan	Priv. Loan
Panel A: All Firms								
Ln(# of Employees)	-0.0005	0.0881***	-0.00306*	0.0839***	-0.000398	0.0879***	0.0110**	0.0973***
	(0.00202)	(0.0118)	(0.00181)	(0.0128)	(0.00204)	(0.0117)	(0.00527)	(0.0218)
Ln(Age)	0.00316	-0.00984	-0.000858	-0.0165	0.00303	-0.00985	0.0214***	0.0311
	(0.00427)	(0.00971)	(0.00461)	(0.0102)	(0.00420)	(0.00975)	(0.00503)	(0.0299)
Ln(Sales/Employees)	0.00705***	0.0263**	0.00746***	0.0240**	0.00676***	0.0258**	0.00764**	0.0247***
	(0.00159)	(0.0125)	(0.00196)	(0.0155)	(0.00160)	(0.0123)	(0.00368)	(0.00813)
Ln(Elec. Exp./Employees)	0.00252*	-0.00706	0.00362***	-0.0102	0.00246	-0.00709	-0.00335	0.00419
	(0.00152)	(0.00632)	(0.00128)	(0.00680)	(0.00154)	(0.00634)	(0.00287)	(0.0123)
Government Press - Count	0.00148	-0.0817*	-0.0790	-0.469**				
	(0.0321)	(0.0877)	(0.0847)	(0.253)				
Private Press - Count					-0.0231	0.064	0.0347	0.341
					(0.0276)	(0.0857)	(0.0902)	(0.240)
Press X Ln(# of Employees)			0.0165**	0.0352			-0.0143**	-0.0144
			(0.00645)	(0.0274)			(0.00575)	(0.0262)
Press X Ln(Age)			0.0292***	0.0529			-0.0227***	-0.0516
			(0.00990)	(0.0499)			(0.0084)	(0.0348)
Press X Ln(Sales/Employees)			0.000548	0.0513			-0.00043	0.00083
			(0.00472)	(0.0177)			(0.0045)	(0.0168)
Press X Ln(Elec. Exp./Employees)			-0.00569**	0.0119			0.00750***	-0.0155
			(0.00284)	(0.0159)			(0.00305)	(0.0141)
Observations	12,110	12,110	12,110	12,110	12,110	12,110	12,110	12,110

Notes: This table presents marginal-effects probit regressions of having a government (or private) loan on firm-level and country-level variables. "Govt. Loan" takes a value of 1 if their most recent loan was from a government bank and 0 otherwise. "Priv. Loan" takes a value of 1 if their most recent loan was from a private bank and 0 otherwise. The regressions include industry fixed effects and standard errors are clustered at the country level. All regressions include the following controls: stock market capitalization to GDP ratio, income per capita, and the fraction of bank assets government-owned. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.6: Cross-Country: Media Regressions - All Firms
Panel B: Government and Private Press - Count

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable:	Gov't Loan	Priv. Loan	Gov't Loan	Priv. Loan	Gov't Loan	Priv. Loan	Gov't Loan	Priv. Loan
Panel A: All Firms								
Ln(# of Employees)	-0.000578	0.0882***	-0.00272	0.0840***	-0.000402	0.0879***	0.0120**	0.0982***
	(0.00200)	(0.0119)	(0.00175)	(0.0129)	(0.00204)	(0.0117)	(0.00552)	(0.0214)
Ln(Age)	0.00332	-0.0101	0.000244	-0.0145	0.00305	-0.0098	0.0240***	0.0364
	(0.00435)	(0.00965)	(0.00441)	(0.00969)	(0.00422)	(0.00973)	(0.00523)	(0.0346)
Ln(Sales/Employees)	0.00743***	0.0271**	0.00737***	0.0246**	0.00677***	0.0261**	0.00792**	0.0236**
	(0.00156)	(0.0125)	(0.00186)	(0.0157)	(0.00159)	(0.0113)	(0.00353)	(0.00707)
Ln(Elec. Exp./Employees)	0.00260*	-0.00688	0.00345***	-0.00964	0.00245	-0.00703	-0.00346	0.00503
	(0.00149)	(0.00622)	(0.00123)	(0.00667)	(0.00153)	(0.00630)	(0.00292)	(0.0121)
Government Press - Share	0.00189	-0.126*	-0.131*	-0.506**				
	(0.0353)	(0.0760)	(0.0684)	(0.221)				
Private Press - Share					-0.0233	0.0745	0.0474	0.351
					(0.0287)	(0.0860)	(0.0855)	(0.236)
Press X Ln(# of Employees)			0.0190***	0.0418*			-0.0152**	-0.0121
			(0.00648)	(0.0215)			(0.00603)	(0.0262)
Press X Ln(Age)			0.0296**	0.0436			-0.0251***	-0.0565
			(0.0126)	(0.0568)			(0.0086)	(0.0393)
Press X Ln(Sales/Employees)			0.0028	0.0049			-0.000736	0.000734
			(0.00452)	(0.0181)			(0.00426)	(0.0169)
Press X Ln(Elec. Exp./Employees)			-0.00531*	0.0117			0.00747***	-0.0162
			(0.00321)	(0.0149)			(0.00311)	(0.0141)
Observations	12,110	12,110	12,110	12,110	12,110	12,110	12,110	12,110

Notes: This table presents marginal-effects probit regressions of having a government (or private) loan on firm-level and country-level variables. "Govt. Loan" takes a value of 1 if their most recent loan was from a government bank and 0 otherwise. "Priv. Loan" takes a value of 1 if their most recent loan was from a private bank and 0 otherwise. The regressions include industry fixed effects and standard errors are clustered at the country level. All regressions include the following controls: stock market capitalization to GDP ratio, income per capita, and the fraction of bank assets government-owned. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.7: Cross-Country: Media Regressions - Only Firms with Bank Loans

VARIABLES	(1) Govt. Loan	(2) Govt. Loan	(3) Govt. Loan	(4) Govt. Loan	(5) Govt. Loan	(6) Govt. Loan	(7) Govt. Loan	(8) Govt. Loan
Ln(# of Employees)	-0.0269***	-0.0302***	-0.0265***	-0.00521	-0.0271***	-0.0294***	-0.0265***	-0.00295
	(0.00515)	(0.00500)	(0.00529)	(0.00440)	(0.00500)	(0.00508)	(0.00525)	(0.00440)
Ln(Age)	0.0129	0.00337	0.0126	0.0483**	0.0135	0.00533	0.0126	0.0543***
	(0.00897)	(0.00978)	(0.00877)	(0.0188)	(0.00923)	(0.00954)	(0.00882)	(0.0198)
Ln(Sales/Employees)	0.00611	0.00674	0.00570	0.0101	0.00674	0.00645	0.00567	0.0126
	(0.00542)	(0.00511)	(0.00543)	(0.00998)	(0.00526)	(0.00518)	(0.00543)	(0.00973)
Ln(Elec. Exp./Employees)	0.00673	0.00838*	0.00671	-0.00761	0.00679	0.00814*	0.00663	-0.00925
	(0.00446)	(0.00430)	(0.00442)	(0.00707)	(0.00449)	(0.00431)	(0.00442)	(0.00692)
Stock Market Capitalization to GDP	-0.000554**	-0.000658**	-0.000674**	-0.000805***	-0.000495*	-0.000553**	-0.000635**	-0.000748**
	(0.000270)	(0.000274)	(0.000302)	(0.000308)	(0.000266)	(0.000270)	(0.000290)	(0.000293)
Government Banks - Assets	0.610***	0.613***	0.624***	0.630***	0.608***	0.610***	0.628***	0.637***
	(0.0877)	(0.0832)	(0.0896)	(0.0838)	(0.0873)	(0.0844)	(0.0896)	(0.0841)
Ln(Income per Capita)	0.0897***	0.0910***	0.0884***	0.0884***	0.0877***	0.0894***	0.0885***	0.0882***
	(0.0304)	(0.0299)	(0.0303)	(0.0290)	(0.0304)	(0.0302)	(0.0301)	(0.0289)
Government Press - Count	0.0816	-0.113						
	(0.0536)	(0.151)						
Private Press - Count			-0.0886**	0.0487				
			(0.0439)	(0.147)				
Government Press - Share					0.0662	-0.235*		
					(0.0621)	(0.133)		
Private Press - Share							-0.0911**	0.0915
							(0.0464)	(0.144)
Press x Ln(# of Employees)		0.0214**		-0.0266***		0.0212**		-0.0286***
		(0.0100)		(0.00645)		(0.00919)		(0.00686)
Press x Ln(Age)		0.0718***		-0.0441*		0.0873***		-0.0500**
		(0.0249)		(0.0245)		(0.0322)		(0.0252)
Press x Ln(Sales/Employee)		0.00270		-0.00423		0.00964		-0.00699
		(0.0113)		(0.00896)		(0.0121)		(0.00903)
Press x Ln(Elec. Exp./Employees)		-0.0132*		0.0172***		-0.0149*		0.0186***
		(0.00729)		(0.00635)		(0.00849)		(0.00647)
Observations	5,930	5,930	5,930	5,930	5,930	5,930	5,930	5,930
Pseudo R-squared	0.178	0.182	0.180	0.186	0.177	0.180	0.180	0.186

Notes: This table presents marginal-effects probit regressions of having a government (or private) loan on firm-level and country-level variables. "Govt. Loan" takes a value of 1 if their most recent loan was from a government bank and 0 if from a private bank. The regressions include industry fixed effects and standard errors are clustered at the country level. All regressions include the following controls: stock market capitalization to GDP ratio, income per capita, and the fraction of bank assets government-owned. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.8: Cross-Country: Income per Capita

Dep. Variable:	(1) Gov't Loan	(2) Gov't Loan	(3) Gov't Loan	(4) Gov't Loan
Panel A: All Firms				
Ln(# of Employees)	-0.0577	-0.0493	-0.0655	-0.0501
	(0.0534)	(0.0483)	(0.0592)	(0.0489)
Ln(Age)	0.132	0.136*	0.131	0.136*
	(0.0835)	(0.0805)	(0.0847)	(0.0809)
Ln(Sales/Employees)	-0.046	-0.0466	-0.0422	-0.0459
	(0.0466)	(0.0448)	(0.0479)	(0.0450)
Ln(Elec. Exp./Employees)	0.0308	0.0291	0.0382	0.029
	(0.0443)	(0.0438)	(0.0463)	(0.0437)
Government Press - Count	0.101*			
	(0.0575)			
Private Press - Count		-0.106**		
		(0.0443)		
Government Press - Share			0.0807	
			(0.0652)	
Private Press - Share				"-0.108**
				(0.0475)
Ln(GDP per Capita) X Ln(# of Employees)	0.00367	0.00272	0.00456	0.00282
	(0.00646)	(0.00588)	(0.00713)	(0.00595)
Ln(GDP per Capita) X Ln(Age)	-0.0142	-0.0148	-0.014	-0.0147
	(0.0102)	(0.00981)	(0.0103)	(0.00985)
Ln(GDP per Capita) X Ln(Sales/Employees)	0.0064	0.00644	0.00597	0.00635
	(0.00544)	(0.00525)	(0.00561)	(0.00526)
Ln(GDP per Capita) X Ln(Elec. Exp./Employees)	-0.00287	-0.00266	-0.00377	-0.00266
	(0.00522)	(0.00517)	(0.00546)	(0.00516)
Observations	5,930	5,930	5,930	5,930

Notes: This table presents marginal-effects probit regressions of having a government (or private) loan on firm-level and country-level variables. "Govt. Loan" takes a value of 1 if their most recent loan was from a government bank and 0 if from a private bank. The regressions include industry fixed effects and standard errors are clustered at the country level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.9: Cross-Country: Media and Income per Capita

Dep. Variable:	(1) Gov't Loan	(2) Gov't Loan	(3) Gov't Loan	(4) Gov't Loan
Government Press - Count	-0.195			
	(0.313)			
Private Press - Count		0.0379		
		(0.297)		
Government Press - Share			-0.479**	
			(0.229)	
Private Press - Share				0.111
				(0.322)
Press X Ln(# of Employees)	0.0282**	-0.0304***	0.0341**	-0.0332***
	(0.0112)	(0.00538)	(0.0148)	(0.00589)
Press X Ln(Age)	0.0631***	-0.0363*	0.0821***	-0.0408**
	(0.0224)	(0.0192)	(0.0303)	(0.0199)
Press X Ln(Sales/Employees)	0.0142	-0.00749	0.0355	-0.0133
	(0.0251)	(0.0231)	(0.0231)	(0.0257)
Press X Ln(Elec. Exp./Employees)	-0.0210***	0.0245***	-0.0265***	0.0263***
	(0.00786)	(0.00482)	(0.00964)	(0.00488)
Ln(GDP per Capita) X Ln(# of Employees)	0.00721	0.00656	0.00906	0.00692
	(0.00697)	(0.00496)	(0.00881)	(0.0511)
Ln(GDP per Capita) X Ln(Age)	-0.00561	-0.0101	-0.00164	-0.00961
	(0.00804)	(0.00835)	(0.00702)	(0.00808)
Ln(GDP per Capita) X Ln(Sales/Employees)	0.00784	0.00574	0.013	0.00694
	(0.00881)	(0.00801)	(0.00935)	(0.00881)
Ln(GDP per Capita) X Ln(Elec. Exp./Employees)	-0.00674	-0.00754	-0.00817	-0.00769
	(0.00655)	(0.00547)	(0.00747)	(0.00557)
Observations	5,930	5,930	5,930	5,930

Notes: This table presents marginal-effects probit regressions of having a government (or private) loan on firm-level and country-level variables. "Govt. Loan" takes a value of 1 if their most recent loan was from a government bank and 0 if from a private bank. The regressions include industry fixed effects and standard errors are clustered at the country level. All regressions include the following controls: stock market capitalization to GDP ratio, income per capita, and the fraction of bank assets government-owned. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.10: Cross-Country: Media and Corruption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable:	Gov't Loan	Gov't Loan	Gov't Loan	Gov't Loan	Gov't Loan	Gov't Loan	Gov't Loan	Gov't Loan
Government Press - Count	-0.0406				-0.0897			
	(0.290)				(0.203)			
Private Press - Count		-0.139				-0.0152		
		(0.250)				(0.196)		
Government Press - Share			-0.337*				-0.274*	
			(0.204)				(0.145)	
Private Press - Share				-0.0677				0.0351
				(0.279)				(0.201)
Press X Ln(# of Employees)	0.0339***	-0.0315***	0.0425***	-0.0344***	0.0363***	-0.0366***	0.0419***	-0.0385***
	(0.00772)	(0.00505)	(0.0125)	(0.00524)	(0.00922)	(0.00669)	(0.00954)	(0.00676)
Press X Ln(Age)	0.0590***	-0.0337**	0.0797**	-0.0384**	0.0615***	-0.0447**	0.0754***	-0.0478**
	(0.0216)	(0.0165)	(0.0323)	(0.0176)	(0.0216)	(0.0195)	(0.0291)	(0.0201)
Press X Ln(Sales/Employees)	0.00759	0.000596	0.0281	-0.00459	0.0109	-0.00797	0.0209	-0.0111
	(0.0236)	(0.0202)	(0.0214)	(0.0227)	(0.0154)	(0.0152)	(0.0153)	(0.0155)
Press X Ln(Elec. Exp./Employees)	-0.0270***	0.0275***	-0.0323***	0.0293***	-0.0262***	0.0290***	-0.0271**	0.0295***
	(0.00750)	(0.00543)	(0.0103)	(0.00569)	(0.00849)	(0.00769)	(0.0106)	(0.00797)
Corruption X Ln(# of Employees)	0.0215**	0.0194**	0.0221**	0.0194**	0.0215***	0.0194**	0.0229**	0.0195***
	(0.0100)	(0.00926)	(0.0106)	(0.00927)	(0.00778)	(0.00629)	(0.00917)	(0.00647)
Corruption X Ln(Age)	-0.0146**	-0.0146**	-0.0149**	-0.0152**	-0.0128	-0.0123	-0.0131	-0.0133
	(0.00679)	(0.00689)	(0.00644)	(0.00675)	(0.00878)	(0.00824)	(0.00881)	(0.00831)
Corruption X Ln(Sales/Employees)	0.0121*	0.0121*	0.0124*	0.0121*	0.0135*	0.0137**	0.0145*	0.0138*
	(0.00724)	(0.00689)	(0.00744)	(0.00698)	(0.00744)	(0.00696)	(0.00786)	(0.00705)
Corruption X Ln(Elec. Exp./Employees)	-0.0114**	-0.0108**	-0.0115**	-0.0106**	-0.0146***	-0.0149***	-0.0145***	-0.0146***
	(0.00496)	(0.00473)	(0.00519)	(0.00460)	(0.00431)	(0.00375)	(0.00473)	(0.00375)
Observations	5,930	5,930	5,930	5,930	5,930	5,930	5,930	5,930

Notes: This table presents marginal-effects probit regressions of having a government (or private) loan on firm-level and country-level variables. "Govt. Loan" takes a value of 1 if their most recent loan was from a government bank and 0 if from a private bank. The regressions include industry fixed effects and standard errors are clustered at the country level. Columns (1)-(4) include interactions with income per capita and firm characteristics and columns (5)-(8) includes interactions with financial development and firm characteristics. All regressions include the following controls: stock market capitalization to GDP ratio, income per capita, and the fraction of bank assets government-owned. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.11: Sub-National Region: Election Contestability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable:	Gov't Loan	Priv. Loan	Gov't Loan	Priv. Loan	Gov't Loan	Priv. Loan	Gov't Loan	Gov't Loan
Panel A: All Firms								
Ln(# of Employees)	-0.00195	0.0885***	0.00771	0.0535	-0.0356	0.0452	-0.0103	-0.0192
	(0.00241)	(0.00922)	(0.00563)	(0.0427)	(0.0345)	(0.0782)	(0.0176)	(0.0688)
Ln(Age)	0.0025	-0.0107	0.0255***	0.0372	-0.0126	-0.263**	0.0252*	0.0286
	(0.00581)	(0.0111)	(0.00587)	(0.0347)	(0.0590)	(0.111)	(0.0139)	(0.0126)
Ln(Sales/Employees)	0.00466***	0.0202	0.00959	0.116**	0.0237	0.165	-0.0177	0.0522
	(0.00133)	(0.0171)	(0.0124)	(0.0503)	(0.0264)	(0.135)	(0.0304)	(0.0746)
Ln(Elec. Exp./Employees)	-0.00171	-0.00065	0.00972	-0.0466	-0.0449	-0.0687	0.0359	-0.0588
	(0.00224)	(0.00791)	(0.0120)	(0.0424)	(0.0320)	(0.109)	(0.0241)	(0.0798)
Election Contestability	-0.0523	0.0891	0.247**	1.085	0.256**	1.077	0.0371	0.0725
	(0.0646)	(0.142)	(0.0969)	(0.694)	(0.106)	-0.713	(0.244)	(0.265)
Contestability X Ln(# of Employees)			-0.0123**	0.0464	-0.0132**	0.0423	-0.021	-0.0213
			(0.00604)	(0.0513)	(0.00658)	(0.0515)	(0.0189)	(0.0168)
Contestability X Ln(Age)			-0.0318***	-0.0653	-0.0326***	-0.0367	-0.0274	-0.0410
			(0.00932)	(0.0446)	(0.0101)	(0.0504)	(0.0201)	(0.0257)
Contestability X Ln(Sales/Employees)			-0.00578	-0.124**	-0.00668	-0.124**	0.0303	0.0301
			(0.0150)	(0.0553)	(0.0140)	(0.0554)	(0.0352)	(0.0313)
Contestability X Ln(Elec. Exp./Employees)			-0.0142	0.062	-0.0132	0.0531	-0.0493*	-0.0490*
			(0.0142)	(0.0483)	(0.0139)	(0.0500)	(0.0281)	(0.0281)
Observations	7,963	8,021	7,963	8,021	7,963	8,021	4,289	4,289

Notes: This table presents marginal-effects probit regressions of having a government (or private) loan on firm-level and country-level variables. In columns (1)-(6), "Govt. Loan" takes a value of 1 if their most recent loan was from a government bank and 0 otherwise and "Priv. Loan" takes a value of 1 if their most recent loan was from a private bank and 0 otherwise. In columns (7) and (8), "Govt. Loan" takes a value of 1 if their most recent loan was from a government bank and a 0 if from a private bank. The regressions include country and industry fixed effects and standard errors are clustered at the country level. * significant at 10%; ** significant at 5%; *** significant at 1%.

2.9 Appendix

2.9.1 Proof for (1)

Re-written, the government bank maximization problem is

$$\max E[U] = \alpha\left[\lambda(1+r)L + (1-\lambda)\left[\frac{f(E)}{1+f(E)-f^p}\phi + f(E)C^L\right]\right] + (1-\alpha)PB(E,M)$$

where

$$\phi = \left[\frac{\lambda}{1-\lambda}\left(2C^H - (1+r)L\right) + C^L\right]$$

The two necessary conditions for an interior optimum are

$$\begin{aligned}
(1) \quad \tfrac{\partial E[U]}{\partial E} &= \alpha(1-\lambda)\left[\frac{f_E(E)(1-f^p)}{(1+f(E)-f^p)^2}\phi + f_E(E)C^L\right] + (1-\alpha)PB_E(E,M) = 0 \\
(2) \quad \tfrac{\partial^2 E[U]}{\partial E^2} &= \alpha(1-\lambda)\eta + (1-\alpha)PB_{EE}(E,M) = 0
\end{aligned}$$

where

$$\eta = (1-\lambda)\left[\frac{f_E(E)(1-f^p)(1-f^p)\left[(1+f(E)-f^p)\,f_{EE}(E) - 2f_E(E)^2\right]}{(1+f(E)-f^p)^4}\phi + f_{EE}(E)C^L\right]$$

Defining $V = \frac{\partial E[U]}{\partial E} = 0$, we can use the implicit theorem to find $\frac{\partial E}{\partial M}$ at the government bank optimum:

$$\frac{\partial E}{\partial M} = -\frac{\frac{\partial V}{\partial M}}{\frac{\partial V}{\partial E}}$$

Because

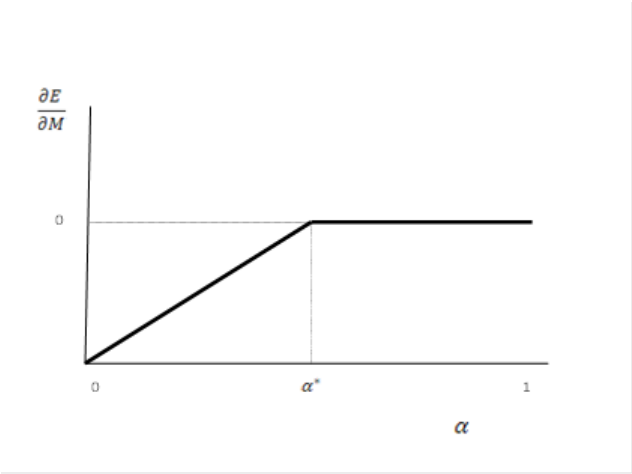
$$\frac{\partial V}{\partial M} = (1-\alpha)PB_M(E,M) < 0$$

in order for us to be at an interior optimum $\frac{\partial V}{\partial E}$ must be less than 0, which is true as long as

$$\alpha^* < \frac{PB_{EE}(E,M)}{PB_{EE}(E,M) - \eta}$$

If however $\alpha \geq \alpha^*$ then we will no longer be at an interior optimum. This implies that the optimum would be at a corner solution where the government bank will provide the loans

to the smallest firms. Graphically

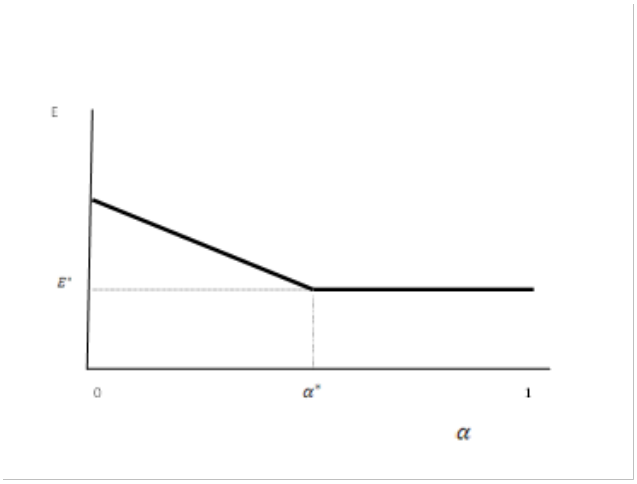


2.9.2 Proof for (2)

Again using the implicit function theorem and defining $V = \frac{\partial E[U]}{\partial E} = 0$:

$$\frac{\partial E}{\partial \alpha} = -\frac{\frac{\partial V}{\partial \alpha}}{\frac{\partial V}{\partial E}} = -\frac{(1-\alpha)\left[\frac{f_E(E)(1-f^p)-PB_E(E,M)<0}{(1+f(E)-f^p)^2}\phi\right]}{[] < 0 \text{ if } \alpha < \alpha^*}$$

In order to be at an interior optimum, again, we need $\alpha < \alpha^*$. If, however, $\alpha \geq \alpha^*$ the we will no longer be at an interior optimum. This implies that the optimum would be at a corner solution where the government bank will provide the loans to the smallest firms regardless of their preference for private benefits. Graphically:



2.9.3 Proof for (3)

The result is immediate because for private banks, the derivative of profit with respect to the media environment is zero.

Chapter 3

Multiplier Effects and Spillovers from Cash-Transfers to the Poor

3.1 Introduction

Means-tested cash-transfers to the poor have been widely studied and evaluated as tools to alleviate immediate poverty and encourage investment in human capital [1]. The focus of these studies is generally on the direct effects of cash-transfers and their conditionalities on beneficiaries. Often times, however, cash-transfers involve a substantial influx of money into local communities, making it likely that there is an impact on the entire community rather than just on individual beneficiaries. While conditional cash-transfer programs have been extensively studied, an unexplored facet is how the economies of local communities are affected by a sudden and substantial influx of money to its poorest members. The focus of this paper is on precisely this aspect of conditional cash-transfer programs: how do cash-transfer programs affect the local economy, do these transfers produce a local multiplier

[1] See, for example, Fiszbein and Schady (2009) for a comprehensive review of conditional cash-transfers in several countries. Specific country studies examining conditional cash-transfer implementation or outcomes for education, health, labor, risk-sharing, and fertility include Schultz, 2004; Soares, Ribas, and Osorio, 2010; Lindert et al., 2007; De La Briere and Lindert, 2005; Lindert 2005; Macours, Schady, and Vakis, 2011; Fernald, Gertler, and Neufeld, 2008; Angelucci and De Giorgi, 2008; Ardington, Case, and Hosegood, 2008; Attanasio and Mesnard, 2006; Barham, 2005a and 2005b; Behrman, Sengupta, and Todd, 2005; Chaudhury and Parajuli, 2008; Coady and Parker, 2004; Glewwe and Olinto, 2004; Rivera et al., 2004; Schady and Araujo, 2008; and Skoufias and Parker, 2001.

effect, and are there income and consumption spillovers to non-beneficiaries. In other words, does one dollar of cash transferred to the poorest members of a community generate more than one dollar of income for that community?

There are many reasons why we may think that cash-transfers have local multiplier and spillover effects. The poor are often thought to have a high marginal propensity to consume [2]. If most of the income received from cash-transfers is spent locally, this will increase demand for local goods and services, which should stimulate local employment and increase earnings in the community. If, however, cash-transfers increase consumption of tradables, imported from outside the locality, there might be negligible local multiplier and spillover effects.

Accounting for multiplier and spillover effects in this sense allows us to assess whether the whole is more than the sum of the parts. As with Miguel and Kremer (2004) and Angelucci and De Giorgi (2009), not accounting for the spillovers or multiplier effects can lead to gross underestimates of program effects. Given the possible existence of multiplier and spillover effects of localized cash injections, measuring them can help determine whether cash-transfers essentially fund themselves or at least substantially offset their costs through the extra economic activity and tax revenue they may help generate.

There are key issues complicating the estimation of local multiplier and spillover effects. First, it can be difficult to separate these from general economic trends. In cases where these transfers are implemented simultaneously across several communities with similar characteristics, it can be difficult to separate what is a multiplier or spillover effect attributable to the transfers and what is simply economic growth that would have occurred anyway, even absent the transfers. Second, since these transfers are means-tested, they are likely to go to communities with particular characteristics, and very likely, there will be omitted variables

[2]The assumption that the poor have a high marginal propensity to consume, and specifically, that the extreme poor have a marginal propensity to consume near 1, is related to the assumed concavity of the consumption function, as argued by Keynes (1935). As discussed in Carroll and Kimball (1996), there is "little recent direct empirical evidence on whether the consumption function is concave," although they mention two papers (Souleles (1995) and Lusardi (1992)) showing that lower income groups in the U.S. generally have a higher marginal propensity to consume. In addition, Murphy, Shleifer, and Vishny (1989) not only argue that the poor have a higher marginal propensity to consume, but specifically, that they consume more locally produced rather than manufactured (tradable) goods.

that drive both the amount of transfer received and subsequent income and employment growth in a community. Failure to account for these might lead us to attribute the effects of these omitted variables to the transfers. To circumvent these issues, one ideally needs exogenous heterogeneity in the amount of transfers across communities.

Brazil's implementation of the Bolsa Familia conditional cash-transfer program provides a source of plausibly exogenous heterogeneity in transfer amounts across otherwise similar communities. Part of this heterogeneity is due to the fact that both income eligibility rules and benefit amounts are constant across all of Brazil despite vast spatial differences in costs of living. Another part is due to how benefit quotas were originally determined. Using data from one point in time, the federal government estimated the number of individuals who were poor and potentially eligible for the cash-transfer program in each municipality. With these estimates, the federal government established quotas for how many poor individuals each municipality was allowed to enroll in the Bolsa Familia program. However, transitory, spatially-correlated noise in any given year can affect local measures of poverty, leading to quotas that are too restrictive or generous and that persist over time. Although the quotas are periodically updated, they always reflect poverty at a particular moment, which can be a noisy estimate of true poverty given transitory, spatially-correlated shocks to income. Together, these spatial differences in costs of living and transitory, spatially-correlated shocks to income provide exogenous heterogeneity in the number of beneficiaries and transfer amounts across otherwise similar communities. This heterogeneity can be exploited to measure how cash-transfers affect the local economy, whether there is a local multiplier effect, and whether the benefits spill over to non-beneficiaries in the community.

3.2 Background on the Bolsa Familia Program

The Bolsa Familia program, officially introduced in 2003, consolidated and expanded four cash-transfer programs that had been in place in various parts of Brazil. Fewer than 15 million people received transfers under these original programs, and there were vast regions of the country not covered. Under Bolsa Familia, cash transfers expanded rapidly. While

only encompassing the 15 million original beneficiaries when first rolled out, it quickly expanded within a year to reach 27 million beneficiaries, within two years to reach 36 million beneficiaries, and within three years to 46 million beneficiaries, approximately one-quarter of the Brazilian population. Unlike the original cash-transfer programs on which it was based, Bolsa Familia covers families across all of Brazil.

Public spending on Bolsa Familia is not substantial. From 2003 to 2006, spending on Bolsa Familia amounted to less than 0.5% of Brazil's GDP (Lindert et al., 2007). Despite its expansive reach and coverage, it is a relatively inexpensive program. For the beneficiary families, however, the cash-transfers represent a substantial increase in their incomes. For a family making less than R$60 (about US$40), the monthly transfer can range from R$50 to R$95, depending on the number of eligible children in the household and whether the mother is pregnant or breastfeeding. For a family making between R$60 and R$120, the monthly transfer can range from R$15 to R$45 (Lindert et al., 2007). This amounts to an increase of at least 12.5% in income, with income possibly doubling or tripling for the extreme poor. In addition, the cash-transfers provide a stable and predictable stream of income not subject to the variance of agricultural or informal sector incomes.

To remain in the program, families need to satisfy two types of conditionalities. First, for the health conditionalities, beneficiary children need to remain up-to-date on vaccines and have regular health check-ups, and pregnant or lactating mothers must have pre and post-natal check-ups as well as participate in a nutrition seminar. Second, for the education conditionalities, children ages 6-15 must be enrolled in school and attend at least 85% of class days each month while mothers must inform the school of absences or if children switch schools. According to Lindert et al. (2007) and Soares, Ribas, and Osorio (2010), these conditionalities have led to increased enrollment and improved school attendance, although school quality is still an issue. There have not been significant gains in health, however. Data collection on potential beneficiaries is decentralized to the municipalities. The municipalities must identify as well as gather and enter data on potential beneficiaries. The federal government checks this data provided by the municipalities against employment and income databases, determines eligibility, and establishes a payroll. Transfers are

then made by the Caixa Economica Federal, a government-owned bank, to beneficiaries' accounts on a monthly basis. Beneficiaries have a debit card they use to withdraw funds at a variety of service points: Caixa bank branches, affiliated bank branches, ATM machines, and lottery houses. They have 90 days to withdraw funds, otherwise these are returned to the federal government; in 2005, 93% of all transfers were actually disbursed, of which 97% were disbursed within the first 30 days (Caixa Economica Federal, 2006).

While municipalities are free to gather and enter data on as many families as they want, each municipality has a quota as to how many of their residents can be enrolled in the Bolsa Familia program. These municipal quotas were originally determined by comparing eligibility criteria (per capita income thresholds) to data from the 2001 national household survey and the 2000 census. Using these data sources, original program quotas resulted in a target of 11.2 million families, about 46 million beneficiaries, when the Bolsa Familia program was first launched in October 2003. By 2006, these targets were met. The quotas were revised in 2006 based on data from the 2004 national household survey, which showed a slight decline in poverty. Within municipalities, spatial poverty maps and living standard indices are used to identify geographic concentrations of the poor, who are then targeted for data gathering and potential enrollment into the Bolsa Familia program.

3.3 Relationship to Previous Literature

There is a rich and growing literature on local labor markets and local multiplier and spillover effects that both models and estimates how changes in local and global economic conditions have ramifications in systems of local economies. Moretti (2010a) models spatial equilibrium and the effect of a productivity shock on local labor markets in the case of tradable and non-tradable goods production, as in the current paper. Similarly, Carrington (1996), Moretti (2010b), Greenstone, Hornbeck, and Moretti (forthcoming), Black, McKinnish, and Sanders (2005), Bound and Holzer (2000) and Autor, Dorn, and Hanson (2011) estimate the effects of various employment, trade, and productivity shocks on local employment and wages, looking specifically at heterogeneous effects on the tradable and

non-tradable goods industries and on subgroups of the population.

These papers are part of a broader literature on place-based policies and localized effects of shocks. Whether the shocks stem from an opening of markets to trade (as in Autor, Dorn, and Hanson (2011)), the introduction of a policy (such as tax incentives, in Greenstone, Hornbeck, and Moretti (forthcoming)) or a commodity boom or construction (as in Black, McKinnish, and Sanders (2005) and Carrington (1996)), what they have in common is that they alter local labor demand and prices. How these changes in labor demand and prices propagate through the economy and translate into a supply response are broadly discussed in the literature on place-making policies. Specifically, Bartik (2002), Glaeser and Gottlieb (2008), and Blanchard and Katz (1992), discuss welfare effects of shocks and policies introduced to help a location develop. The general view from these papers is that, over the medium to long-term, changes in labor supply (through in or out-migration of workers) mitigates any positive or negative effect of a policy so that locations are typically just as well off after the policy or shock in the long-term as they were before. As Bartik (2002), Glaeser and Gottlieb (2008), and Kline (2010) point out, if agglomeration economies are non-linear, then what might be necessary to help locations achieve a stable equilibrium of development, broadly defined, is a "big push" along the lines of Rosenstein-Rodan (1943). With a large enough shock, locations might be able to surmount stable equilibrium poverty traps to achieve greater development.

There is skepticism in the literature about this "big push" view, however, with locations showing tremendous persistence in their relative development even after weathering substantial but temporary shocks (Davis and Weinstein, 2002)). Glaeser and Gottlieb (2008) likewise argue that large-scale, place-oriented policies have had little discernible impact or are expensive relative to their achievements, and that the greatest promise for increasing welfare through place-based policies is to facilitate migration to highly productive areas by discouraging inefficient land-use restrictions.

Given the existing literature, it is questionable whether a policy like the one analyzed in this paper of transferring income to the poorest residents of a community and vastly increasing

the amount of wealth circulating in that community can have any long-term multiplier or spillover effects and increase residents' welfare beyond the amount of the transfer. First, such a policy can produce a concentrated increase in area income, and thus, like in other papers (e.g., Busso, Gregory, and Kline (2010)), it is a large and concentrated treatment effect, unlike many regional development initiatives whose resources are too small or whose area of coverage is too large to have any measureable impacts (such as the Appalachian Regional Commission in the U.S. discussed in Bartik (2002) and Glaeser and Gottlieb (2008)). If transferring income to the poorest residents does indeed produce multiplier or spill-over effects, they should be measureable, at least in the short-run, given the magnitude of the transfers being analyzed. Second, given individual preferences and the relative immobility of the poor (see Kline (2010), Bound and Holzer (2000), and Notowidigdo (2010)), it is possible that even in the medium to long-term, low-skilled labor supply will not fully adjust, suggesting that any transfer-induced multiplier may sustain itself in terms of higher wages for low-skilled workers rather than larger low-skilled population. Lastly, there may be some unintended effects of a policy meant to help poor locations and their residents. If such an income-transfer policy induces out-migration of high-skilled workers, as discussed in the conceptual framework, it can reduce human capital spillovers and undermine productivity and wages (analogous to Moretti (2004a, 2004b, 2004c, 2010a)).

Along these lines, this work also builds on several recent papers regarding prices and regional variation in welfare. In the U.S., controlling for price levels leads to vastly different estimates of poverty and welfare (Slesnick, 2002 and Moretti, 2010a), and there is debate as to whether transfer payments should be indexed based on local prices (see Glaeser (1998)). In the current paper, eligibility requirements and transfers are not indexed despite large regional price variations. While this provides quasi-exogenous variation in the intensity of treatment across locations needed for estimation, it also potentially creates unintended economic inefficiencies. For example, if providing higher real transfers to individuals in poor, unproductive locations discourages migration to high-cost, but productive areas, on aggregate, this can undermine economic productivity as fewer individuals benefit from larger agglomeration economies . And for individuals, remaining in poor, low-cost areas because

of larger real transfers can prevent them from experiencing the rapid wage growth due to learning and spillovers more common in developed labor markets with higher average education (as in Moretti (2004a) and Glaeser and Mare (2001)).

Finally, this paper extends the literature on income transfer programs in developing countries. Conditional cash transfers have been broadly studied as mechanisms to reduce immediate poverty (Fiszbein and Schady, 2009 and Soares, Ribas, and Osorio, 2010), encourage schooling, increase individual employment opportunities and wages, and reduce child labor (Schultz, 2004), and improve health outcomes (Macours, Schady, and Vakis, 2011 and Fernald, Gertler, and Neufeld, 2008). There have also been studies, similar to this one, on the indirect effects of cash-transfers on non-beneficiaries, but through the channel of improved localized informal insurance arrangements (Angelucci and De Giorgi, 2009). This paper extends this literature by looking at aggregate local economic effects of conditional cash transfers. Aside from possibly encouraging changes in individual behaviors, such transfers can also have aggregate effects on local employment, prices, wages, and productivity (ostensibly through human capital spillovers and agglomeration economies). A deeper understanding of these effects of conditional cash transfer programs is important especially given the growing implementation of such programs throughout developing countries [3].

3.4 Conceptual Framework

This conceptual framework aims to provide intuition and testable predictions of the general equilibrium effects of a government transfer to low-skilled households on prices, consumption decisions, overall utility and migration decisions.

[3]Between the mid-1990s and the present, conditional cash transfer programs have been implemented in Mexico, Brazil, Guatemala, El Salvador, Costa Rica, Panama, Ecuador, Peru, Chile, Bolivia, Argentina, Paraguay, Colombia, Nicaragua, Honduras, Jamaica, Dominican Republic, Burkina Faso, Nigeria, Turkey, Yemen, Kenya, Pakistan, India, Bangladesh, Cambodia, Indonesia, and the Philippines (Fiszbein and Schady, 2009).

3.4.1 Set-up

Consider a local economy with low and high-skilled individuals. Both types of individuals have identical Cobb-Douglas preferences over consumption,

$$u(x,z) = x^{1-\gamma}z^{\gamma} \tag{3.4.1}$$

where x is a tradable good and z is a locally-produced, non-tradable good. Low-skilled workers have budget constraint w_L and high-skilled workers have budget constraint w_H, where w_L and w_H respectively denote wages of low and high-skilled workers.

Both types of goods are produced in the local economy. The non-tradable good, z, is produced according to the production function,

$$y_z = F_z(L_L) = L_L^{\alpha} \tag{3.4.2}$$

and the tradable good, x, is produced according to the production function,

$$y_x = F_x(L_H) = L_H^{\beta} \tag{3.4.3}$$

Where L_L and L_H denote low and high-skilled employment. Production of the local good, z, uses low-skilled labor as an input whereas the tradable good, x, uses high skilled labor. In equilibrium, firms maximize profits and workers are paid their marginal product. Prices for the tradable good are fixed and normalized at 1. Prices for the local good, p_z, are endogenously determined by local demand and supply conditions. Solving for the wages yields:

$$\begin{aligned} w_L &= \alpha p_z L_L^{\alpha-1} \\ w_H &= \beta L_H^{\beta-1} \end{aligned} \tag{3.4.4}$$

3.4.2 Worker's Maximization

Each worker $i \in \{High, Low\}$ solves the maximization problem:

$$\begin{aligned} Max u_i(x,z) \quad &= \quad x_i^{1-\gamma} z_i^{\gamma} \\ s.t. \qquad w_i \geq \; & p_z z_i + x_i \end{aligned} \tag{3.4.5}$$

The market clearing condition for the non-tradable good is simply that the amount consumed is equal to the amount produced. This implies that: $z_L + z_H = y_z$. The solution to the maximization problem for both the high and low skilled workers gives $z_L = \frac{w_L^{\gamma}}{p_z}$ and $z_H = \frac{w_H^{\gamma}}{p_z}$. Imposing the market clearing condition and solving for the price of the non-tradable good gives $p_z^* = \frac{\gamma\beta L_H^{\beta-1}}{L_L^{\gamma}\left(1-\frac{\alpha\gamma}{L_L}\right)}$[4]. From this, we can solve for the equilibrium level of consumption of each good by each type of worker:

$$x_H^* = (1-\gamma)\beta L_H^{\beta-1} \tag{3.4.6}$$

$$z_H^* = L_L^{\alpha}\left(1-\frac{\alpha\gamma}{L_L}\right) \tag{3.4.7}$$

$$x_L^* = \frac{1-\gamma}{L_L-\alpha\gamma}\alpha\gamma\beta L_H^{\beta-1} \tag{3.4.8}$$

$$z_L^* = \alpha\gamma L_L^{\alpha-1} \tag{3.4.9}$$

3.4.3 Including Government Transfers and No Migration

We now introduce government transfers, T. It is assumed that revenues for these transfers are external to the local economy; there is no local taxation to fund these transfers. These transfers, as is done through the Bolsa Familia program, go to the low skilled workers. Low skilled total income is therefore the sum of their wages, w_L, and the government transfer, T: $\alpha p_z L_L^{\alpha-1} + T$. High skilled total income remains just the wage income from the previous section.

[4] $p_x = 1 > p_z$ if $L_H < \left(\frac{\gamma\beta}{L_L^{\gamma}\left(1-\frac{\alpha\gamma}{L_L}\right)}\right)^{\frac{1}{1-\beta}}$

Imposing the same market clearing condition for the non-tradable good yields the following:

$$p_z^{**} = \frac{\gamma\left(\beta L_H^{\beta-1} + T\right)}{L_L^{\alpha}\left(1 - \frac{\alpha\gamma}{L_L}\right)}; w_L^{**} = \frac{\alpha\gamma\beta L_H^{\beta-1} + TL_L}{L_L - \alpha\gamma} + T; w_H^{**} = \beta L_H^{\beta-1}$$

We see immediately that this will have a price effect on the non-tradable good and an effect on wages for the low skilled workers. High skilled workers, however, will have the same income as the first period. Re-solving for the consumption of both goods for both types of workers yields:

$$x_H^{**} = (1-\gamma)\beta L_H^{\beta-1} \tag{3.4.10}$$

$$z_H^{**} = \frac{(L_L - \alpha\gamma)\beta L_H^{\beta} L_L^{\alpha-1}}{TL_H + \beta L_H^{\beta}} \tag{3.4.11}$$

$$x_L^{**} = \frac{1-\gamma}{L_L - \alpha\gamma}\left(\alpha\gamma\beta L_H^{\beta-1} + L_L T\right) \tag{3.4.12}$$

$$z_L^{**} = \frac{\alpha\gamma L_L^{\alpha-1}\beta L_H^{\beta} + L_L^{\alpha} L_H T}{TL_H + \beta L_H^{\beta}} \tag{3.4.13}$$

With expressions (3.3.10)-(3.3.13), we can compute simple comparative statics to look at the effects that a transfer amount T to a local economy will have on prices, consumption and utility.

Comparative statics

1. $\frac{\partial p_z^{**}}{\partial T} = \frac{\gamma}{L_L^{\alpha}\left(1-\frac{\alpha\gamma}{L_L}\right)} > 0$
2. $\frac{\partial w_L^{**}}{\partial T} = 1 + \frac{L_L}{L_L - \alpha\gamma} > 0$
3. $\frac{\partial w_H^{**}}{\partial T} = 0$
4. $\frac{\partial u_H^{**}}{\partial T} < 0$[5]
5. $\frac{\partial u_L^{**}}{\partial T} > 0$

[5] If $L_L > \alpha\gamma$

The interpretation of these is simple. In line with the Stolper-Samuelson theorem (Stolper and Samuelson, 1941), the price of the local good increases. The government transfer and the increase in wages for low-skilled workers results in an increase in demand for the local good, which raises prices. As the transfer to a locality increases, so will the price of the non-tradable good and the total income of the low-skilled workers. Note as well that the increase in income for the low-skilled workers is larger than 1, implying that there is a multiplier effect to the program. The wages of the high skilled workers, however, will remain the same. The increased wage of the low skilled workers through the government transfer allows these workers to consume more of both goods, despite the increase in the price of the non-tradable goods. This gives the low skilled workers increased utility after the government transfer. However, the wage of the high-skilled workers remains fixed and the increase in the price of the non-tradable good, induced by the government transfer, allows the high-skilled workers to consume less of it, thereby decreasing their overall utility.

3.4.4 Allowing for Migration

We assume an initial no-arbitrage condition where high-skilled and low-skilled workers in a given locality have equal utility across all localities. If this were not the case, given free mobility, workers would simply move. The government transfer to low skilled workers, however, will create a utility differential between localities that received this transfer and those that did not.

Given a cost of migration parameter equal to $c_M > 0$, a high-skilled worker in a locality that receives this transfer will migrate to a locality that did not receive this transfer if $u_H^* - u_H^{**} > c_M$. Similarly, a low-skilled worker in a locality that did not receive this transfer will move to a locality that does get this transfer if $u_L^{**} - u_L^* > c_M$. Therefore, a government transfer to the low-skilled workers in a locality could yield general equilibrium effects which creates cities that are relatively attractive to high-skilled workers and cities that are relatively attractive to a low-skilled worker.

This simple conceptual framework provides testable implications. First, we can calculate

multiplier and spillover effects in terms of employment and wages. We expect to see a multiplier effect to the low-skilled workers and the workers in the non-tradable sectors. A second implication is that we expect to see a price effect of the program. As we see in the comparative statics, the conceptual framework suggests that we should see an increase in the price of the non-tradable goods, but not the tradable goods. Lastly, we can test whether there is migration of specific types of workers from relatively high-skilled localities to relatively low-skilled localities (or vice versa). Given current data availability, we will only be able to empirically test the first implication at this time.

3.5 Data and Summary Statistics

The data for this paper come largely from Pesquisa Nacional por Amostra de Domicilios (PNAD) - Brazil's annual household surveys. Using these surveys, we obtain information about the number of transfer recipients and the amount of transfers received in selected municipalities. We are additionally able to create municipality-level controls.

We supplement the PNAD surveys with data on rainfall, from Brazil's National Meteorological Institute (Inmet), to create an instrument for local shocks at the state-level to income and poverty during the time when quotas for Bolsa Familia transfers were initially determined. Figure 3.1 shows a map of all of the weather stations in Brazil.

At this point, the PNAD surveys include a municipality identifier that we are unable to match to the census. Once this match is made, we will be able to use additional data sources including Relacao Anual de Informacoes Sociais (RAIS), a database maintained by Brazil's Ministry of Labor, which provides annual formal sector employment and wages for each municipality by occupational sector and worker education category. We will further be able to use data on prices from the Indice Nacional de Precos ao Consumidor (INPC) which are supplemented with data from the PNAD and Pesquisa de Padroes de Vida (PPV). This will allows us to quantify price effects. The PNAD surveys include state identifiers which at this point we use to calculate state-level rainfall shocks.

3.5.1 Direct and Indirect Measures of Household Participation in Bolsa Familia

The annual PNAD surveys do not usually ask households whether they directly receive income from conditional cash transfer programs. There is a question annually, however, on "other income", which includes interest, dividends, and income received from social programs. Since the poorest 50% of households don't usually report interest or dividend income, any income reported in this "other income" category can be assumed to be from social programs like Bolsa Familia. In two years, 2004, shortly after Brazil consolidated several conditional cash-transfer programs into Bolsa Familia, and 2006, when Bolsa Familia had been fully expanded to reach its target of over 11 million families, the PNAD did directly ask whether families received transfers from the Bolsa Familia program. Using information from the 2004 and 2006 PNAD, both the direct question of whether families receive income from Bolsa Familia and the indirect measure of the number of Bolsa Familia beneficiaries based on reports of "other income", it is possible to assess whether reports of "other income" serve as a good proxy for whether families really are beneficiaries of the Bolsa Familia program (this closely follows the approach of Foguel and Paes de Barros (2010)).

The direct measure is based on whether households answered yes to having at least one member receiving benefits from Bolsa Familia. The indirect measure is based on whether the family is in the poorest half of the national income distribution and if their income from "other income" sources is typical of values received if they participate in the Bolsa Familia program. The correlation between the direct and indirect measures is 0.87 in 2004, and Table 3.1 provides a breakdown of how households in the PNAD sample are coded based on these measures. Table 3.1 shows that approximately 84% of households in 2004 do not receive benefits under both measures while approximately 13% of households in 2004 do receive benefits under both measures. Approximately 2% of households in 2004 are truly beneficiaries but are classified as not receiving benefits under the indirect measure, and approximately 1% in 2004 are not truly beneficiaries but are classified as receiving benefits

under the indirect measure.

Using the direct and indirect measures of Bolsa Familia beneficiaries, Table 3.2 reports the average share of households for the PNAD sample of 817 municipalities that receive Bolsa Familia transfers, the average share of household income that comes from Bolsa Familia transfers for families that are beneficiaries, and the share of total municipal income that is comprised of these transfers. This information is reported for two years, 2004 and 2006, based on both the direct and indirect measures of Bolsa Familia participation.

According to the direct (indirect) measure of program participation, approximately 19% (18%) of households were beneficiaries of Bolsa Familia in 2004. Approximately 14% (15%) of beneficiaries' household income in 2004 were from Bolsa Familia transfers. And the transfers increased municipal income by approximately 1.6% (1.5%) in 2004. Not surprisingly, the direct and indirect measures of being a Bolsa Familia household yield summary statistics that are practically identical and statistically indistinguishable from one another.

3.5.2 Summary Statistics

Table 3.3 reports selected summary statistics using 2000 Census data and 2001 and 2009 PNAD data. The Census includes information for all of Brazil's 5,507 municipalities. The PNAD includes information for a sample of 817 municipalities; this sample includes all of the largest metropolitan areas, and then remaining municipalities are selected with probability weights depending on their populations. The PNAD is consequently more heavily weighted toward larger municipalities, with more populous municipalities having a higher probability of being selected. The PNAD is the same panel of 817 municipalities between 2001 and 2009.

The purpose of reporting both 2000 Census and 2001 PNAD summary statistics is to compare whether data for all 5,507 municipalities is comparable to the PNAD sample of 817 municipalities, even though the data differ by one year. Many variables are similar, although given that the Census is more heavily weighted toward less populous (i.e., more rural) mu-

nicipalities, the summary statistics show a greater share of poor, a smaller proportion of the working-age population that is working, and fewer years of schooling and lower wages for all categories of workers.

Based on 2001 PNAD sample data, the mean share of the population that is poor in a municipality is 28%. Approximately 70% of the working-age population is working. Of those that are working, 21% work in the tradable goods sector while 79% work in the non-tradable goods sector. Here, tradable goods are those for which wages are likely determined in general equilibrium by demand and supply conditions for the good that are external to the city (i.e., manufactured goods) and non-tradables are those for which wages are likely determined by demand and supply conditions for the good internal to the city (i.e., retail sale, restaurants). Years of schooling and wages are higher for workers in the tradable good sector than in the non-tradable good sector. Approximately 53% of workers of working-age are low-skilled and 47% are high-skilled. Low-skilled workers are those with less than 6 years of education in 2001 and less than 7 years of education in 2009. The share of high-skilled workers increases between 2001 and 2009, with high-skilled workers becoming a majority of the working-age workforce in the PNAD sample of cities by 2009. As expected, years of schooling and wages are substantially lower for low-skilled than for high-skilled worker. A principal aim of this paper is to examine how the share of a municipalities' population that participates in the Bolsa Familia program and how the increase in municipality income directly attributable to Bolsa Familia affects wages for the municipalities' tradable and non-tradable and low- and high-skilled workers.

3.6 Empirical Strategy and Results

3.6.1 Empirical Strategy

The principal empirical question of this paper involves examining how a change in a municipalities' overall income, that is, the sum of the income of all its residents, affects employment and wages of different types of workers in the municipality. There is an obvious issue of

endogeneity: workers' wages, in part, determine the total income of a municipality and total income of a municipality, through demand channels, determines workers' wages. In order to measure causality from a change in a municipalities' income to a change in employment and wages, it is necessary to have some component of a change in a municipalities' income that is exogenous and independent of any change in employment and wages.

The strategy of this paper is to use the change in a municipalities' income due to Bolsa Familia receipts. Transfers from Bolsa Familia increase total income for the average municipality by 1.9%, with a standard deviation of 2.5%. The question becomes how this increase in average municipality income due to Bolsa Familia affects employment and wages conditional on city characteristics. For this to be a valid strategy, it must be the case that, conditional on observable city characteristics, there is some randomness to the amount of Bolsa Familia transfers a city receives. With this randomness, one can isolate how an exogenous change in total municipal income affects municipal employment and wages.

An issue is the possibility of omitted variables that might jointly determine Bolsa Familia transfers and municipal employment and wages, even conditional on observable city characteristics. If an omitted variable, such as low amenities, which might be partially reflected in a municipality's price levels and poverty rate, causes Bolsa Familia transfers to be higher but also causes employment and wage growth to be lower, then an estimate for how a change in municipal income affects employment and wages will be biased downward.

To address this potential for omitted variable bias, we can instrument for Bolsa Familia transfers. We use, as instruments, state-level weather shocks in 2001-2002 and a generalized agricultural shock. Local weather shocks should affect income and poverty levels in a municipality in a given year. For the years when Bolsa Familia quotas are calculated, these weather shocks will, for the long term, determine the number of Bolsa Familia recipients and amount of transfers a city can receive. Even once the shock dissipates, the quotas remain in force, causing the shocks to have a lasting impact on the amount of transfers. Several years from the shock, however, wages and employment are likely to have returned to their original trends. In this way, local weather shocks from prior years, when quotas are

calculated, are likely to affect Bolsa Familia transfers without otherwise affecting current employment and wages in the municipality, thus satisfying the exclusion restriction. The primary estimating equation is:

$$\Delta y_i = \alpha + \beta \Delta BFPIncome_i + \mathbf{X_i'}\gamma + \varepsilon_\mathbf{i}$$

where Δy_i is the change in the outcome variable, employment or wages, in municipality i, $\Delta BFPIncome_i$ is by how much a municipality's income increases because of transfers to its residents from Bolsa Familia, $\mathbf{X_i}$ is a vector of initial time-period controls for municipality i, and ε_i is the error term. We estimate the first stage equation:

$$\Delta BFPIncome_i = \tilde{\alpha} + \tilde{\beta}_1 \Delta AgricultureShock_i + \tilde{\beta}_2 \Delta RainfallShock_j + \mathbf{X_i'}\tilde{\gamma} + \tilde{\varepsilon}_\mathbf{i}$$

Where the agriculture shock instrument is a Bartik-style calculation using state-level agricultural shocks for each type of agricultural output which is then attributed to each municipality based on the agricultural composition of that municipality. The rainfall shock is calculated as average yearly rainfall in 2001-2002 minus average yearly rainfall over the sample period (2000-2009) scaled by average yearly rainfall. This is calculated for each weather station in Brazil. To calculate the state-level shock, the rainfall shock instrument is then averaged across all stations within the state. For one state (with 11 municipalities in the sample), there is no weather station so the reported results omit this state, leaving a sample of 806 municipalities.

Obtaining the predicted increase in municipality income due to Bolsa Familia from this equation, we can obtain instrumental variable estimates of the effect of an increase in municipality incomes due to Bolsa Familia on the outcome variables.

3.6.2 Results

The conceptual framework provides the simple intuition that the effects of the Bolsa Familia program will be seen largely in the low-skilled workers and to the workers who are employed

in the non-tradable sectors. The transfer to the poor will give them an inital jump in income, but then this will compound as this money is then spent in both the tradable and non-tradable sector. In theory, because there will be no price-effect in the tradable sector, this will not have a wage effect for those employed in the tradable sector, but it will for those employed in the non-tradable sector.

Mitigating the potential endogeneity of transfer amounts to localities hinges upon finding a valid instrument that predicts Bolsa Familia transfer amounts. Column (1) of Table 3.4 shows that our proposed agriculture and rainfall instruments are, indeed, significant in the first stage. After controlling for the median household income, population, education and urbanization, the coefficients on Agriculture Shock and Rainfall Shock are significant (negative) predictors of Bolsa Familia cash-transfers. According to these estimates, given a 10% (negative) deviation in yearly rainfall in 2001 and 20002, Bolsa Familia transfers in 2004 increase by roughly 4%.

Columns (2)-(7) report the second-stage results for changes in both employment and wages from 2004-2006. Consistent with the conceptual framework, the spillover effects for overall employment is transmitted through the low-skilled workers and in the non-tradable sector. For low-skilled employment, a 10% increase in Bolsa Familia transfers in 2004 is associated with a 14% increase in overall employment for low-skilled workers. High-skilled employment, however, does not see this same increase. A 10% increase in Bolsa Familia transfers in 2004 is associated with a 2% (insignificant) increase for high-skilled employment. The distinction between employment in tradable and non-tradable sectors appears to also be important. The non-tradable sector is associated with large multiplier effects whereas the tradable sector is statistically insignificant.

3.7 Conclusion

Much of the academic research on conditional-cash transfer programs has focused on analyzing the effects of cash transfer programs directly on the recipients, e.g. asking if giving

transfers to poor families helps to keep children enrolled in schools and/or vaccinated. Exploiting exogenous variation in transfer amounts to municipalities, this paper calculates multiplier and spillover effects to recipients and non-recipients.

With additional datasets, we are currently expanding this project to assess the price-effects on tradable and non-tradable goods as well as the effect of this transfer program on migration.

3.8 Figures

Figure 3.1: Weather Stations

Notes: This figure shows the dispersion of weather stations across Brazil.

3.9 Tables

Table 3.1: PNAD Households in BF based on Direct and Indirect Methods, 2004

		HH in BF based on "Other Income"		
		No	Yes	Total
HH in BF based on direct PNAD question	No	94,505	1,442	95,947
	Yes	2,294	14,475	16,769
	Total	96,799	15,917	112,716

Table 3.2: Households and Income from Bolsa Familia

	2004 Direct	2004 Indirect	2006 Indirect
Share of HHs in BF	0.191	0.182	0.204
	(0.154)	(0.15)	(0.152)
Share of HH income that is from BF (conditional on being in BF)	0.141	0.147	0.222
	(0.092)	(0.094)	(0.198)
Share of municipal income from BF transfers	0.016	0.015	0.019
	(0.022)	(0.02)	(0.025)
Number of municipalities	817	817	817

Table 3.3: Summary Statistics

	2000 Census	2001 PNAD	2009 PNAD
Share of municipal pop. with income data	0.997	0.985	0.979
	(0.008)	(0.031)	(0.035)
Share poor	0.393	0.280	0.177
	(0.223)	(0.194)	(0.148)
Share working age	0.441	0.457	0.485
	(0.053)	(0.055)	(0.047)
Share working age who worked	0.558	0.698	0.735
	(0.111)	(0.076)	(0.075)
Of these, share in tradables	0.187	0.211	0.205
	(0.095)	(0.111)	(0.117)
Years of schooling	5.115	5.812	7.549
	(1.366)	(1.855)	(1.653)
Years of experience	n/a	22.367	22.402
	n/a	(2.256)	(2.241)
Average hourly wage	2.276	2.434	5.008
	(1.017)	(1.36)	(2.204)
Of these, share in non-tradables	0.813	0.789	0.795
	(0.095)	(0.111)	(0.117)
Years of schooling	6.553	7.260	8.490
	(1.471)	(2.022)	(1.944)
Years of experience	n/a	20.963	21.035
	n/a	(3.857)	(3.695)
Average hourly wage	2.978	3.542	6.608
	(1.58)	(2.349)	(3.455)
Of these, share low-skilled	0.622	0.530	0.403
	(0.134)	(0.189)	(0.166)
Years of schooling	2.754	2.803	3.324
	(0.667)	(0.758)	(0.692)
Years of experience	n/a	25.360	26.911
	n/a	(2.499)	(2.742)
Average hourly wage	1.701	1.678	3.578
	(0.696)	(0.798)	(1.601)
Of these, share high-skilled	0.378	0.470	0.597
	(0.136)	(0.189)	(0.166)
Years of schooling	9.722	9.819	10.689
	(0.526)	(0.771)	(0.571)
Years of experience	n/a	18.309	18.805
	n/a	(2.532)	(2.219)
Average hourly wage	3.492	3.560	6.289
	(1.502)	(1.989)	(2.643)
Number of municipalities	5507	817	817

Table 3.4: Empirical Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Skilled and Unskilled	First-Stage	Second-Stage					
			Employment			Wages	
Dep. Variable:	ShareTransfers	Total	High-Skilled	Low-Skilled	Total	High-Skilled	Low-Skilled
Agriculture Shock	-0.2592***						
	(0.0201)						
Rainfall Shock	-0.0407***						
	(0.0065)						
ShareTransfers		0.5388	0.2053	1.441**	2.419***	2.589**	1.9164**
		(0.389)	(0.4707)	(0.6103)	(0.6622)	(1.104)	(0.9564)
Panel B: Tradables and Non-Trables							
			Employment			Wages	
Dep. Variable:	ShareTransfers		Tradable	Non-Tradable	Total	Tradable	Non-Tradable
Agriculture Shock	-0.2592***						
	(0.0201)						
Rainfall Shock	-0.0407***						
	(0.0065)						
ShareTransfers			1.2506	3.383***		-1.744	1.071
			(1.696)	(0.7656)		(1.815)	(1.059)
Observations	806	806	806	806	806	806	806

Notes: Robust standard errors in parentheses. Column (1) provides the first-stage result of Bolsa Familia transfers on the agriculture shock and rainfall shock instruments. Columns (2)-(7) show the second-stage results for employment and wages. All regressions include controls which are not shown. The controls are median household income, population, schooling and urbanization. The dependent variables in columns (2)-(7) and all controls are calculates as changes from 2004 to 2006. * significant at 10%; ** significant at 5%; *** significant at 1%.

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