Politics in Financial Intermediation: Evidence from Brazil*

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Abstract

Using highly granular banking data from Brazil, we reveal a political bias in the intermediation of liquidity windfalls. Following a rise in deposits due to natural resource booms, state-owned banks increase credit supply outside of resource-rich areas by more if the destination municipality is politically aligned with the ruling president. In contrast, private banks redistribute significantly less funds to a destination municipality if it exhibits this same alignment. Unlike previous studies, our results are not driven by changes in lending policies during election years or amidst strong political competition faced by an allied incumbent. Instead, we find stronger effects when the allied incumbent was elected with a large margin of victory, suggesting that our results primarily arise from favoritism towards strong allies. Political lending by state-owned banks has a negative impact on bank profitability, indicating a misallocation of credit.

Keywords: Banking, Liquidity reallocation, State-owned banks, Political lending

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1 Introduction

A well-functioning financial system where liquidity flows to the most promising projects is fundamental for sustained economic growth (Rajan and Zingales, 1998). Nonetheless, the allocation of credit is often determined by factors other than creditworthiness. A growing literature finds political factors to influence bank lending decisions. This body of work typically documents political lending cycles, where state-owned banks (or even private banks[1]) increase credit in election years to affect voting behavior (Dinç, 2005), especially when elections are closely contested (Cole, 2009; Carvalho, 2014; Englmaier and Stowasser, 2017; Bircan and Saka, 2021). However, we know much less about political biases in bank lending outside of an election context. Do banks make lending decisions based on political factors such as party alignment also in non-election years, and even in the absence of political competition? If so, what are the consequences?

In the present paper, we address this gap in the literature by studying the role of politics in the intermediation of liquidity windfalls. We focus on Brazil, a global economic heavyweight with a large banking sector but also a developing country suffering from institutional weaknesses and poverty. To capture liquidity windfalls, we use exogenous shocks to the value of Brazil's large endowment of non-renewable natural resources, over the period 2001-2022. Brazil is among the largest producers worldwide of oil and several minerals such as iron ore, and the oil and gas and mining sector jointly account for more than 10% of the country's GDP.[2] Our approach exploits that banks are differently exposed to natural resource booms via their spatial branch network, implying variation in deposit inflows and banks' ability to adjust credit supply.

Our first finding is that boom-exposed banks do not only re-invest windfalls locally, but also use rising deposits from resource-rich areas to raise credit in other parts of the country. This result complements Bustos et al. (2020), who find that agricultural productivity growth leads to higher savings and capital outflows from rural areas of Brazil. However, we reveal

[1] See Fungáčová et al. (2023).

[2] Brazil exports 2.6% of the World's traded oil but is not a member of OPEC. In iron ore, Brazil supplies 21% of global supply, behind only Australia (54%). Brazil also exports copper, manganese, nickel, bauxite, and various other minerals. Source: The Observatory of Economic Complexity (OEC), 2021.

a political bias in the spatial redistribution of funds: faced with two non-resource ("destination") municipalities, state-owned banks (which account for 43% of total bank credit in Brazil) raise lending by more in the municipality whose local government is aligned with the ruling central government at the time.[3] Does this reflect misallocation, or might it be explained by efficiency-related factors? Additional results clearly speak against the latter hypothesis. First, we find that privately-owned banks grant *less* extra credit to municipalities that are aligned with the center, compared to non-aligned destination municipalities (perhaps because rising state bank credit crowds out private credit); and second, we find weak but statistically significant evidence that political lending negatively affects bank profitability. Taken together, these results suggest a misallocation of funds by state-owned banks.

Unlike most other studies in the literature on politically motivated bank lending, our results are not driven by election cycles. First, we find that the effect of political alignment on state-owned banks' credit supply does not intensify during municipal election years (in which the alignment may be at risk of breaking down).[4] In fact, there is no statistically significant difference in the strength of the political lending effect across the four years of the municipal election cycle. Second, the political lending effect is stronger in locations where the municipal election that created (or maintained) the political alignment was won by a *large* margin, reflecting a high degree of alignment. This novel evidence suggests that lending to politically aligned places reflects favors to strong allies, rather than attempts to preserve alignment in politically competitive environments. Complementary results show that the extent of political lending is not a function of a destination municipality's level of economic development or factors such as urban/rural status. However, we do find that the results vary to some degree by the political orientation of the ruling central government. Under presidents whose party is classified as centrist, there is no political bias in state-owned bank lending, while the effect does occur under both left- and right-wing presidents,

[3] We define alignment as a time-varying dummy variable which equals one if the coalition that nominated the incumbent mayor (who is the key figure in the municipal government) includes the political party of the ruling president. See Section 2.3 for details and institutional background.

[4] Municipal elections are typically held in October. This implies that there is enough time prior to the election to pick up lending patterns in the election year that aim to influence the election outcome.

in a magnitude that is not statistically distinguishable.

Our empirical strategy combines highly granular data on the universe of banking in Brazil with novel data on municipal-level resource endowment and information on political outcomes. Our rich and unique data set allows us to capture cross-bank and cross-time variation in exposure to natural resource booms and busts throughout the country, and to address various threats to identification. Intuitively, a bank that focuses its operations in municipalities that host large endowments of natural resources, whose global prices are currently booming, is highly exposed to aggregate resource windfalls in the given period. Other banks might be over-represented in municipalities hosting commodities that are busting in the same period, or may not locate in resource-endowed regions at all. Banks thereby differ in terms of deposit inflows and thus extra liquidity, creating variation in their ability to grant extra credit in resource-endowed ("origin") or non-endowed ("destination") municipalities. By focusing on destination municipalities that host at least two banks, we can saturate our specifications with municipality-year fixed effects, which control for credit demand effects or potentially endogenous election outcomes. Our results are robust to a wide range of robustness checks, which further address these and other concerns. We also discuss how our empirical strategy is related to shift-share designs and how we leverage the recent literature in this area (e.g. Goldsmith-Pinkham et al., 2020).

Our paper contributes to several literatures. First, we add to a body of work on politically motivated credit supply, by showing that political lending extends beyond efforts to influence contested elections. In this context, Cole (2009) finds that agricultural lending by state-owned banks in India rises during local election years, particularly when elections are close; Bircan and Saka (2021) find that prior to tight local elections in Turkey, state-owned banks increase credit supply if the local incumbent is politically aligned with the central government, and decrease credit otherwise; and Carvalho (2014) use Brazilian data to show that ahead of competitive elections, state-owned banks increase lending to shift employment to regions with politically aligned incumbents.[5] A rare study that also analyzes political lending beyond elections is Chavaz and Rose (2019), who show that private recipient banks of the 2008 Troubled Asset Relief Program (TARP) increase lending by more in US census tracts that are located just inside their home-representative's district.

Our study also adds to a literature showing that local commodity booms create liquidity windfalls which banks use to increase credit supply in nonboom regions. Bustos et al. (2020) show such effects in the context of agricultural productivity growth in Brazil due to the adoption of genetically engineered soy, and Gilje et al. (2016) in the context of oil and shale gas discoveries in the United States. We contribute to these studies by highlighting a political dimension in the spatial redistribution of funds.

The remainder of the paper is structured as follows. Section 2 describes our data sources and provides relevant background information, Section 3 outlines our empirical strategy, Section 4 presents our results and robustness checks, and Section 5 concludes.

2 Background and Data

Three different data sets form the backbone of our empirical analysis. These contain information on natural resource endowment, bank- and branch-level data, and election data, respectively. In this section we describe these data sets and relevant background information. Other data used in the analysis are described as we employ them throughout Section 4. Our sample period lasts from 2001 to 2022, due to data availability and reasons we discuss in Section 2.3. Table A1 in the Appendix provides descriptive statistics.

[5] Englmaier and Stowasser (2017) use German data to show that lending policies that respond to electoral cycles are not unique to developing and emerging economies. Kumar (2020) finds that a politically motivated rise in lending to farmers before elections crowds out lending to manufacturing firms. In a rare study finding no effects, Baum et al. (2010) find that the behavior of state-owned banks does not meaningfully differ from that of private-sector banks before, during or after elections. More generally, this literature traces back to Dinç (2005), who employ cross-country bank-level data to show that state-owned banks increase their lending in election years relative to private banks.

2.1 Natural resource endowment

Brazil has vast endowments of non-renewable natural resources. The country ranges among the top five producers worldwide of iron ore, bauxite, lithium, and niobium, and among the top 10 producers of oil, nickel, and magnesium (as of 2022). The oil and gas sector has seen huge investments over the last years and accounts for about 10% of the country's GDP, while mining accounts for 2.5% of GDP.

A key data ingredient of our empirical strategy is non-renewable natural resource endowment at the municipal level as of 2000, thus one year before the start of our sample period. Non-renewable natural resources include oil and gas as well as metals such as gold or copper. While we do not have data on coal endowment, Brazil is a minor coal producer, accounting for less than 0.1% of global production. Following Allcott and Keniston (2018), we use commodity-specific average (real 2000 USD) prices over our sample period to express reserves in monetary terms, enabling us to sum reserves across commodities. We use different data sources for oil and gas versus metals, which we outline separately next. Details can be found in the Online Appendix.

2.1.1 Data on metal endowment

To compute the metal endowment of each Brazilian municipality in 2000, we use proprietary deposit-level information from S&P Global's *Raw Materials Data* (RMD). Most importantly, this data set provides us with economically recoverable reserves at the deposit level. Data on deposit location are not available, but we retrieve them (as well as other data points which are missing for several deposits) using a variety of sources.

We identify 143 deposits with positive reserves, distributed across 88 municipalities and 16 out of Brazil's 27 provinces. In terms of main metal, 53 deposits contain iron (these reserves make up 69% of Brazil's total metal endowment), 37 gold (2.6%), 10 nickel (3.3%), 9 aluminum (11.1%), 8 copper (2.6%), 4 manganese (<0.1%), 4 phosphorus (0.7%), 4 tin (0.2%), 4 zinc (0.5%), 2 niobium (9.1%), 2 uranium (<0.1%), 1 lithium (0.3%), 1 potassium (0.1%), 1 diamonds, 1 tantalum, 1 tungsten, and 1 vanadium (all <0.1%).[6] Across municipalities with positive metal endowment, the average reserves are worth $21 billion, while the median stands at $3 billion.

2.1.2 Data on oil and gas

We construct a novel database on Brazil's oil and gas endowment at the municipal level by combining multiple publicly available data sets. Our point of departure is data on field-specific "volume of oil and gas in place" (which is more broadly defined than economically recoverable reserves), provided by the Brazilian National Agency for Petroleum, Natural Gas and Biofuels (ANP). We then merge this data set with field-specific information on recovery rates (from GlobalData) and annual production (from ANP), to arrive at economically recoverable reserves at the field level in 2000. Thereafter, we match fields to municipalities. While this is straightforward for onshore fields, for offshore fields we follow previous literature (e.g. Caselli and Michaels, 2013) and use Brazil's oil and gas royalty allocation scheme, which assigns fields to municipalities based on extending municipalities' terrestrial borders onto the continental shelf. Across the 244 municipalities with oil and gas endowment, the average reserves equal $18 billion, and the median equals $1.4 billion.

2.2 Brazil's banking sector and data

Brazil hosts Latin America's five largest banks, two of which are state-owned. Throughout our sample period 2001-2022, state-owned banks accounted for 33% of total bank assets and 43% of total bank credit. Banco do Brasil and Caixa Econômica Federal jointly account for more than 90% of state-owned bank assets and state-owned bank credit. For these and other banks that are majority-owned by the central government, the CEO and the board of directors are traditionally appointed by the president. This opens the door for substantial political influence on lending decisions. Before 2000, the majority of Brazil's 27

[6] Deposit-level reserves are reported as of one specific year (rather than annually), which is always greater 2000. We therefore compute deposit-level reserves in 2000 by taking the sum of reserves in the reported year and previous, post-2000 production. Since reserves and production are measured in terms of metal *ore*, we transform ore reserves into contained metal reserves using deposit-specific grade information.

states (provinces) operated their own state banks, but many of these were privatized in the late 1990s (see for example Beck et al., 2005).[7]

We construct a panel data set that includes banks' balance sheet and income statement data and information on the individual branches of these banks across Brazil's municipalities. Bank-level data come from call reports published by the Brazilian Central Bank, while the (unconsolidated) branch data come from the ESTBAN database. The latter includes data on credit, which allows us to measure lending by a specific bank in a specific municipality. Note that multiple branches (offices) of the same bank within a single municipality are considered a single entity in the original data. Consequently, the branch-level data we use in the analysis represent the sum of the balance sheet items of all branches (offices) of a bank per municipality.

We define state-owned banks as those that are majority-owned by the central government or a provincial government, during our entire sample period. In a robustness check, we adjust our definition to only include banks owned by the central government. We drop one bank that was privatized during our sample period, as this event forbids a clear ownership classification.

We restrict the sample in several ways, for different reasons. First, we drop foreign-owned banks, because (i) these may be differently affected by external shocks such as the 2007-08 financial crisis or the Covid-19 pandemic; and (ii) we thereby simplify our analysis by not needing to contrast state-owned banks with two different types of private banks.[8] We obtain information on bank ownership from the database of Claessens and Van Horen (2014). Second, we need to account for the fact that while certain banks effectively close down or are taken over by other banks, they occasionally remain in the data thereafter (for one or several years). We do so by dropping bank-years in which the bank either (i) has zero deposits; (ii) is reported as inactive in the Claessens and Van Horen (2014) data; or (iii) is no longer active in any of the municipalities in which it operated in the beginning of our sample period. Third, we drop bank-years in which a bank only operates

[7] Note that throughout the paper, we use the term state-owned bank as a synonym for government-owned bank, rather than referring to state-owned banks as provincially-owned banks.

[8] We classify banks as foreign-owned if they are foreign-owned in at least one year during our sample period.

in one municipality, given our focus on redistribution of liquidity within a bank's spatial branch network. Fourth, we drop bank-municipality-years in which deposits and/or credit equal zero, which typically occurs when a branch has formally been established in the municipality but is not yet operational. Fifth, we drop observations for which we do not have bank- or branch-level data on all variables used in the analysis. Finally, we only keep municipality-years in which at least two banks are locally active, as this allows us to saturate our bank-municipality-level credit regressions with municipality-year fixed effects (see Section 3).

This sampling procedure leaves us with 70 banks (of which 11 are state-owned) that are active in more than one year over our sample period and therefore enter our analysis. 41 of these are active in 152 resource-endowed municipalities (resulting in 730 bank-municipality pairs), while 65 of the banks are active in 2,279 non-resource municipalities (more than 9,000 bank-municipality pairs). Our restricted sample still includes Brazil's largest banks, and represents 74% of the total banking assets and 73% of total credit in the country as of 2000.

2.3 Election data and institutional background

Municipal elections are held every four years in Brazil. The elections take place at the same time (usually in October) all over the country. Voters directly elect a mayor (*Prefeito*) and the municipal chamber (*Câmara Municipal*), which jointly manage the municipal government. In municipalities with more than 200,000 eligible voters, a runoff election among the top two mayor candidates takes place, unless one of them achieves a majority in the first round. The newly elected local government takes office on January 1 of the year following the election. For this reason, we choose to start our sample period in 2001, when the local governments elected in 2000 came into office (we do not have data on local government elections prior to 2000). Our sample period of 2001-2022 thus covers the municipal governments elected in the years 2000, 2004, 2008, 2012, 2016, and 2020. Since the 2000 elections, the term limit for mayors has been extended from one to two terms. A political party can nominate a mayoral election candidate either alone or as part of a pre-electoral coalition

(PEC; see Silva, 2022), where the latter is more common.

We source data on election outcomes from the *Tribunal Superior Eleitoral* (TSE). The most important variable we construct from these data indicates the political alignment between Brazil's central government and the municipal government. Due to the widespread use of PECs in municipal elections, we define political alignment as a dummy variable that equals one if the president's party is part of the PEC that nominated the winner of the mayoral election. This dummy equals one in around 30% of municipality-years in our sample. In an alternative specification, we use a more narrowly defined alignment variable, which equals one if the president and the mayor belong to the same party (this is true for 10% of municipality-years).

3 Empirical Strategy

The goal of our empirical strategy is to capture a bank's exposure to natural resource booms and associated windfalls across Brazil, and link this exposure variable to the bank's credit supply decisions in a plausibly causal fashion. However, the very first step is to establish that local resource booms lead to an increase in the deposits of local bank branches, as otherwise there is no scope for redistribution. Therefore, we first describe how we test this hypothesis, before we proceed with presenting our bank-level resource boom exposure and our main estimating equations.

3.1 Local resource booms and deposit inflows

To test whether local natural resource booms lead to an increase in the deposits of local bank branches, we estimate the following specification, where we define resource booms similarly to Pelzl and Poelhekke (2021):

$$\begin{aligned} ihs(Deposits_{i,j,t}) = \ & \beta_0 + \beta_1[RESendow_{j,t=0} \times ihs(RESprice_{j,t})] \\ & + \gamma_{i,j} + \mu_{i,t} + \varepsilon_{i,j,t} \end{aligned} \tag{1}$$

$Deposits_{i,j,t}$ are deposits of bank i in municipality j in year t, $\gamma_{i,j}$ are bank-municipality fixed effects, and $\mu_{i,t}$ are bank-year fixed effects. $RESendow_{j,t=0}$ equals municipality j's natural resource endowment, computed as:

$$RESendow_{j,t=0} = \sum_c RESendow_{c,j,t=0}$$

where $RESendow_{c,j,t=0}$ equals the endowment of commodity c (which is a specific metal, oil, or natural gas) in municipality j in the year 2000, expressed in terms of US dollars (see Section 2.1). We measure endowment in 2000 and thus one year before the start of our sample period because current endowment in year t is a function of exploration effort, which may be endogenous to unobserved local developments. We scale $RESendow_{j,t=0}$ by the variable's standard deviation to ease the interpretation of β_1, which we discuss further below. $RESprice_{j,t}$ is a municipality-level price index that captures the price level at time t of the commodities that make up $RESendow_{j,t=0}$:

$$RESprice_{j,t} = \sum_c P_{c,t} \frac{RESendow_{c,j,t=0}}{RESendow_{j,t=0}} \quad \text{if } RESendow_{j,t=0} > 0, \text{ 0 otherwise}$$

where $P_{c,t}$ equals the annual average world price of commodity c in year t, normalized by the price of c in 2000. For the estimation of Equation (1), we transform $RESprice_{j,t}$ by the inverse hyperbolic sine function to be able to include municipalities without resource endowment (for which $RESprice_{j,t} = 0$) into our sample. For consistency, we also apply the inverse hyperbolic sine transformation to $Deposits_{i,j,t}$, although our sampling choices imply that this variable is always positive (see Section 2.2). We cluster standard errors at the bank level and at the meso-region level. The 137 meso-regions in Brazil (and in our sample) are subdivisions of the Brazilian states, grouping together municipalities in proximity and with common characteristics. We cluster at this geographic level to account for potential serial correlation in the standard errors across municipalities within the same meso-region (see Cameron and Miller, 2015).[9] β_1 in Equation (1) can be interpreted as the impact of

[9] An even more conservative choice would be to cluster at the state level, but with 27 states this would come at the risk of having too few clusters. Meso-regions contain around 40 municipalities on average, with substantial variation across meso-regions. Meso-regions are further divided into micro-regions, of which there are 540 in our sample. Note that the concept of meso- and micro-regions was abolished towards the end of our sample period in 2017, and replaced by "Intermediate and Immediate Geographic Regions".

an increase in the price of locally found commodities by 100 log points, in the municipality with resource endowment equal to one standard deviation.

We expect β_1 to be positive through several mechanisms that channel natural resource revenue into bank-branch deposits. First, theory (Corden and Neary, 1982) suggests that a booming resource sector results in an aggregate ‘spending effect’, both from rising wages (due to higher labor demand by the booming sector) and from spending of resource revenue.[10] Local resource revenue can arise through local ownership of land on which extraction takes place, as happens in the US (Gilje et al., 2016), or via fiscal transfers, which have been found to raise municipal incomes and GDP for oil-producing municipalities in Brazil (Caselli and Michaels, 2013; Cavalcanti et al., 2019). Second, in the case of Brazil, many upstream firms supply to the mostly offshore oil and gas sector, such that employment in oil-related sectors is many times larger than direct employment in oil drilling. Similar to spending effects, these inter-industry linkages spread the boom beyond workers and firms in the resource sector (Katovich et al., 2023). Third, non-oil metals and minerals mining has been found to positively affect local businesses within 150km of operating mines due to improved infrastructure and regional revenue spending (De Haas and Poelhekke, 2019). In sum, these local short to medium run effects likely result in a rise of local bank deposits, notwithstanding that longer term aggregate effects may be negative through various resource-curse mechanisms, as surveyed in van der Ploeg (2011) and Cust and Poelhekke (2015).

3.2 Bank-level resource boom exposure

The next step is to define natural resource boom exposure at the bank level, which is a time-varying variable that depends on a number of characteristics. Thereafter, we can proceed to test whether banks with larger boom exposure experience more deposit inflows than others at the level of the entire bank, and whether they use this extra liquidity to increase credit supply in origin and/or destination municipalities. Intuitively, a bank's resource boom exposure is very high if the bank is very active in resource-endowed mu-

[10] This holds even when the boom results in sectoral reallocation and a decline of manufacturing exports.

nicipalities (relative to other municipalities), if the endowments in these municipalities are very large, and if the prices of the commodities that make up these endowments are very high. Put differently, if a certain municipality j accounts for a large share of the bank's overall activities and hosts large endowments of natural resources whose prices are currently booming, then j clearly drives the bank's resource boom exposure upwards. Three elements thus influence a bank's resource boom exposure: the relevance of different municipalities for the bank, their natural resource endowments (if any), and prices. We first describe these elements separately and at the municipality level, and then show how we aggregate them across municipalities to arrive at a bank-level measure of resource boom exposure.

Element #1 of bank-level resource boom exposure: Municipality relevance

The major banks in Brazil typically operate in multiple, but not all municipalities of the country. Total deposits of the bank equal the sum of deposits across all municipalities where the bank is active. Bustos et al. (2020) argue in the context of agricultural booms across Brazil that the ratio of bank-specific deposits in municipality j to total deposits of the bank captures to what extent a resource boom in j (if any) matters for the bank. While this share varies over time and could be endogenous to the boom, Bustos et al. (2020) address this source of endogeneity by fixing the ratio at the beginning of their sample period. We follow their approach and compute the ratio $\frac{Deposits_{i,j,t=0}}{TotalDeposits_{i,t=0}}$ for all municipalities where bank i is active, where $t = 0$ stands for the year 2000.[11,12] We use j's share in total bank deposits rather than j's asset or credit share because the asset share may reflect accounting factors rather than real representation, and the credit share appears too close to our main outcome variable, bank-municipality credit. That said, the results are robust to using these alternatives.

[11] Fixing the ratio at $t = 0$ addresses for example the potential concern that banks which expand credit by more than others in a given period for unobserved reasons also open up more branches in booming natural resource municipalities (compare to Equation 4 below). Specifically, if we allowed municipalities' shares in total bank deposits to vary throughout our sample period, then endogenous branch openings during booms would drive up the exposure variable and bias our results, but with shares as of $t = 0$, such branch openings leave the exposure variable unaffected.

[12] Fixing a municipality's share in total bank deposits as of 2000 implies that our sample does not include banks that were not yet active in 2000, as all initial municipality shares are missing in that case.

Element #2: Municipality endowment of natural resources

In Section 3.1 we defined a municipality's natural resource endowment as the sum of reserves of all locally found commodities in the year 2000. We use this same definition, $RESendow_{j,t=0}$, as the second element of our bank-level resource boom exposure.

Element #3: Prices of commodities that make up municipality endowment

To capture the price level of commodities found in municipality j, we use $RESprice_{j,t}$, which we defined in Section 3.1. The variable is a weighted average of normalized prices across all locally found commodities c, where the weight of c equals its share in total reserves of municipality j.

Equipped with these three elements, we compute bank-level resource boom exposure by first multiplying deposit share, reserves, and prices at the level of a single municipality, and then aggregating across all municipalities for a given bank:

$$ResBoomExposure_{i,t} = \sum_{j}^{J} \frac{Deposits_{i,j,t=0}}{TotalDeposits_{i,t=0}} \times RESendow_{j,t=0} \times RESprice_{j,t} \quad (2)$$

This variable is computed for each bank i in each year t based on all municipalities j in which the bank has a branch. 45 of the 70 banks in our sample are represented in at least one municipality with natural resource endowment in the year 2000; for the remaining 25 banks, $ResBoomExposure_{i,t} = 0$.

3.3 Main estimating equations

We first use *ResBoomExposure* to test whether larger exposure to natural resource booms has a positive effect on deposit inflow at the bank level. We do so via the following specification, which complements Equation (1) (we cluster standard errors at the bank level):

$$ihs(Deposits_{i,t}) = \beta_0 + \beta_1 ihs(ResBoomExposure_{i,t}) + \gamma_i + \mu_t + \varepsilon_{i,t} \quad (3)$$

where γ_i are bank fixed effects and μ_t are year fixed effects. The following step is to analyze credit: as deposits rise, banks may use the extra liquidity to raise lending. They may do so in resource-endowed municipalities, and/or redistribute "boom deposits" to other areas of the country, as evidenced by Bustos et al. (2020) in the context of agricultural booms. In testing for such effects, we define our dependent variable at the bank-municipality level, since a rise in a bank's credit supply is operationalized by the branches that are located in the borrower's municipality. More specifically, we estimate the following specification, using two different samples – bank-municipality pairs in origin municipalities, and bank-municipality pairs in destination municipalities[13]:

$$ihs(Credit_{i,j,t}) = \beta_0 + \beta_1 ihs(ResBoomExposure_{i,t}) + C_{i,j,t-1} + \gamma_{i,j} + \mu_{j,t} + \varepsilon_{i,j,t} \qquad (4)$$

$\mu_{j,t}$ are municipality-year fixed effects, which control for local time-varying factors that may be correlated with the exposure of local banks to resource booms across Brazil. For example, lending in a destination municipality may not only be influenced by the resource boom exposure of local banks, but also by credit demand, which could correlate with local banks' boom exposure for instance if local industries have supply chain linkages to the booming resource sector in origin municipalities.[14] With the inclusion of municipality-year fixed effects we control for such confounding factors. Intuitively, β_1 is then identified off the difference in resource boom exposure across the different banks that locate in a given municipality, while time-varying factors like credit demand are accounted for by the fixed effects – to the extent that these factors affect all local banks in the same way.

$C_{i,j,t-1}$ is a vector of lagged bank-municipality-level controls, which are mainly included to control for bank-specific credit demand. For example, in the above scenario, firms that

[13] Note that in-between these steps, we analyze the difference between deposit inflow and lending in origin municipalities for boom-exposed banks, to gauge the potential for fund reallocation to non-endowed municipalities; see Section 4 for details.

[14] Suppose for instance that a destination municipality hosts a manufacturing industry which supplies more goods to iron ore-producing origin municipalities in times of high iron ore prices and therefore demands more credit from local banks. Moreover, suppose that banks that are more active in the iron ore business are not only more exposed to iron ore booms through their branch network, but also settle more often in destination municipalities that supply goods to iron ore producers elsewhere. In this scenario there is a clear (positive) correlation between *ResBoomExposure* of banks that locate in the destination municipality and unobserved local credit demand. Without municipality-year fixed effects, a positive β_1 might then simply reflect higher credit demand rather than higher credit supply.

do not locate in origin municipalities but are connected to them via supply chains might not uniformly demand more credit from all of their lenders during resource booms. Instead, they might demand more from lenders with certain characteristics. If those characteristics are correlated with banks' boom exposure, then β_1 would be contaminated by bank-specific credit demand. In our baseline specification, $C_{i,j,t-1}$ thus includes lagged size (measured as total assets of bank i in municipality j), which we find to positively correlate with bank-level resource boom exposure.[15] We cluster standard errors at the bank level and at the meso-region level in Equation (4).

The main focus of our paper is to analyze the political dimension of banks' liquidity redistribution across space. In this spirit, we estimate the following extended version of Equation (4), which allows for differential effects across politically aligned and non-aligned municipalities, and across state-owned and private banks:

$$\begin{aligned} ihs(Credit_{i,j,t}) = \; & \delta_0 + \delta_1 ihs(ResBoomExposure_{i,t}) \\ & + \delta_2 ihs(ResBoomExposure_{i,t}) \times PolAlignment_{j,t} \\ & + \delta_3 ihs(ResBoomExposure_{i,t}) \times PolAlignment_{j,t} \times StateOwned_i \\ & + \omega OtherRelevantInteractions + C_{i,j,t-1} + \gamma_{i,j} + \mu_{j,t} + \varepsilon_{i,j,t} \end{aligned} \tag{5}$$

where $PolAlignment_{j,t}$ is a dummy variable that equals one if the political party of Brazil's president in year t is part of the electoral coalition that nominated the mayor who rules in municipality j in year t. $StateOwned_i$ is a dummy indicating that bank i is majority-owned by the central or provincial government, in all years during our sample period. $OtherRelevantInteractions$ is a vector including all other interactions whose inclusion is made necessary by adding the double and triple interaction in Equation (5). Note that $PolAlignment$ may be affected by time-varying, municipality-level factors; however, these

[15] We use the first lag to avoid the "bad control" problem (Angrist and Pischke, 2009), given that current bank-level boom exposure might affect current bank-municipality-level size. Our results are robust to including additional bank-municipality-level controls such as a lagged liquid asset ratio and deposit ratio. We only include size in our baseline specification because unreported regressions show that among the mentioned controls, it is the only bank-municipality-level variable that both correlates with bank exposure and has a statistically significant effect on bank-municipality-level credit.

are captured by municipality-year fixed effects in Equation (5), and we further address this potential concern via a robustness check (see Section 4.3).

4 Results

We start this section by establishing the auxiliary result that local natural resource booms lead to an increase in local bank deposits. Thereafter, we present our baseline results, which show how larger resource boom exposure at the bank level leads to more deposit inflows, higher credit supply, and politically motivated lending (see Section 4.1). In the following, we explore the nature and motivation of political lending decisions, by studying heterogeneous effects across election-related variables, municipality characteristics, and political parties' ideological orientation. Furthermore, we test for financial consequences of politically motivated lending (see Section 4.2). Finally, we discuss a large and diverse range of robustness checks (see Section 4.3).

4.1 Baseline Results

Before we discuss how banks might use natural resource-induced liquidity windfalls, we must first establish that such windfalls exist, in terms of higher deposit inflows. Intuitively, we expect that higher natural resource prices raise the revenue of the resource sector in endowed ("origin") municipalities, part of which will enter the financial system through deposits. We test this hypothesis by regressing bank-municipality-level deposits on our municipality-level resource boom measure and controlling for bank-year level unobservables, as displayed in Equation (1). Given that this exercise only serves as a first step to motivate our analysis, we present the results in Appendix-Table A2 (see column 1), which contains different auxiliary results that are discussed throughout Section 4. We find that an increase in local resource prices by 100 log points leads to a statistically significant rise in deposits by 3.2%, in the origin municipality with resource endowment equal to one standard deviation.

Next, we test whether this rise in local deposits during booms is sufficiently relevant to raise deposits at the overall bank level. We do so by estimating Equation (3) and present the results in column 1 of Table 1, which displays the impact of bank-level boom exposure on different outcomes. We find that larger exposure to resource booms across Brazil leads to a statistically significant rise in bank-level deposits. While the coefficient size is hard to interpret directly, it will enable us to put the credit results discussed in the next paragraphs into perspective.

A rise in bank-level deposits should enable banks to increase their credit supply. They may do so in origin municipalities (directly or by redistributing funds between them), or may instead reallocate funds to raise credit in other municipalities. We test the prior hypothesis first, by estimating Equation (4) on the sample of origin municipalities. We find that an increase in resource boom exposure that raises bank-level deposits by 69% leads to a 47% rise in credit by branches of the same bank in resource-endowed regions, on average (see column 3 of Table 1).

Given our results that boom exposure raises deposits but also credit supply in resource-endowed regions, the question arises if banks have sufficient "boom funds" left to redistribute to non-endowed regions of the country. In column 3, we address this question by studying how resource boom exposure affects bank-municipality-level deposits minus granted credit (scaled by lagged total assets) in origin municipalities. We find a positive effect, which Bustos et al. (2020) interpret as an outflow of capital from origin municipalities, in the context of their study. In column 4, we estimate Equation (4) on the sample of destination municipalities. Consistent with the evidence in column 3, we find a positive effect: a rise in exposure that increases bank-level deposits by 69% raises credit supply in non-endowed municipalities by 37%, on average. This evidence complements previous findings of a redistribution of funds across space via banks' branch networks following liquidity windfalls (Gilje et al., 2016; Bustos et al., 2020).

We proceed by testing for the role of politics in the redistribution of bank funds across space, which constitutes our main contribution to the above-cited literature. We start by interacting $ihs(ResBoomExposure)$ with $PolAlignment$, thereby evaluating whether political alignment between the president and the local government influences the lending decisions of the average bank in our sample. The results in column 5 show that this is not the case: the interaction term is virtually zero and not statistically significant. In column

6, we estimate Equation (5), which allows us to explore whether political alignment has different implications for the lending policies of state-owned banks versus private banks. The results reveal highly interesting patterns. First, the triple interaction term in column 6 reveals that boom-exposed banks that are *state-owned* increase credit by *more* if the destination municipality is politically aligned with the president's party. This raises the question, do the results of column 6 simply reflect more lending to political allies, or could they (also) reflect efficiency-related factors? For example, might political alignment imply a more frictionless and thus more profitable lending prospect? If that were the case, then we would also expect privately-owned banks to redistribute more funds to municipalities where the local government is politically aligned with the ruling president. However, the coefficient on $ihs(ResBoomExposure) \times PolAlignment$ is *negative* (and statistically significant). This indicates that privately-owned banks also factor in political variables in their lending decision, but in the opposite direction: faced with two destination municipalities, they raise lending by *less* in the politically aligned one. This clearly speaks against efficiency-related reasons for the observed lending behavior of state-owned banks, and might reflect crowding-out of private bank loans due to increased lending by state-owned banks in aligned municipalities. This hypothesis receives some support from our finding that political lending by state-owned banks reduces bank profitability (see Section 4.2), as this is consistent with below-market interest rates which private banks struggle to compete with.[16]

Finally, note that state-owned banks generally increase credit supply in destination municipalities by less than privately-owned banks: the coefficient on $ihs(ResBoomExposure) \times State-owned$ is negative and statistically significant. Therefore, it is not the case that among all possible combinations of political alignment and bank ownership, the redistribution of funds from origin to destination municipalities is strongest for state-owned banks in politically aligned municipalities. In contrast, the effects need to be interpreted in a relative sense: faced with two destination municipalities, a state-owned bank raises credit by more

[16] Unfortunately, we do not observe interest rates on local bank loans.

in the aligned one.[17]

4.2 Heterogeneous effects and financial consequences

The results of column 6 of Table 1 indicate that state-owned banks use increased liquidity to supply more credit in politically aligned destination municipalities (we refer to this result as the 'baseline alignment effect' below). In this section, we mainly study whether the strength of this effect depends on the timing of elections, political competition, municipality characteristics, the political orientation of the ruling president's party, or the degree of political alignment between the center and a municipal government. These exercises will help us understand the mechanism(s) behind the baseline alignment effect. In terms of empirical strategy, we estimate an extended version of Equation (5), which includes an additional four-tuple interaction term $ihs(ResBoomExposure) \times PolAlignment \times State-owned \times X$, where X varies by regression (we also include all other relevant interaction terms that result from this four-tuple interaction). The results are reported in Table 2. Due to limited space, we only include the coefficients on this four-tuple interaction as well as the triple interaction term $ihs(ResBoomExp.) \times PolAlignment \times State-owned$.

Municipal election timing

The focus of the existing literature on political bank lending has been to document political lending cycles, where (predominantly) state-owned banks increase credit supply in election years to influence the election outcome. Therefore, we start our analysis by testing whether such effects drive the baseline alignment effect we find. In column 1, we do so by interacting our triple interaction term from Equation (5) with a dummy variable that takes one in the municipal elections years 2004, 2008, 2012, 2016, and 2020 (recall that these elections were held in October or later). The coefficient on this four-tuple interaction is close to zero and

[17] We explore the reasons for the generally weaker effects for state-owned banks in Appendix-Table A2. One possible explanation is our finding that state-owned banks' deposits rise by a smaller percentage upon a given boom exposure (see column 3), which might be explained by state-owned banks having a larger deposit-to-asset ratio in general (see column 4), implying that a given deposit inflow during a given boom (in absolute terms) possibly has a smaller impact on state-owned banks' lending capacity. As a possible alternative explanation, state-owned banks might increase lending in *origin* municipalities by more than privately owned banks, but we do not find evidence for this (see column 5).

statistically insignificant. This speaks against the hypothesis that state-owned banks use windfall-induced liquidity to increase the re-election chances of a politically aligned mayor. However, one may hypothesize that election-motivated fund redistribution does take place, but occurs already before the election year. To test for that, in column 3 we analyze political lending effects across the entire election cycle. We do so via three four-tuple interactions, where each time we interact our baseline triple interaction with a dummy indicating a certain distance to the municipal election year, in terms of calendar years: one year before the election, two years, and three years (the baseline triple interaction thus reflects the effect in the election year). The results show that the four-tuple interactions are not statistically significant, suggesting that the strength of political lending by state-owned banks does not depend on the distance to the election. This provides additional evidence that our results are not (primarily) driven by election-related considerations of state-owned banks.

Electoral competition

In columns 3 and 4, we test whether the baseline alignment effect differs across mayors that won the last election (which created or preserved the alignment) by a large versus narrow margin.[18] In column 3, we define X as a dummy that equals one if the margin of victory is larger than the median margin across all mayor elections in our sample period (which equals 12%). In column 4, we instead use the actual vote share difference (in percentage points) between the winner and the runner-up, scaled by the variable's standard deviation (which equals 19ppt). The results in both columns give a consistent and clear answer: municipalities where the aligned mayor won by a large margin receive *more* credit by state-owned banks than others. This speaks against the hypothesis that while "weakly aligned" mayors do not receive more support in election years (see column 1), they are supported more throughout their term to maintain the political alignment in the following term. More generally, the evidence exploiting victory margins further suggests that the influence of elections is not the main objective of the political lending decisions we find. In contrast, our findings suggest that the ruling central government uses its control over state-owned banks to channel funds to regions that are strong political allies.

[18] We calculate the vote share difference between the winner and the runner-up based on the second-round election results whenever a second round was held.

Municipality-level corruption

If the (main) purpose of state banks' lending behavior is not to raise local economic activity and thereby please voters (such as for example in Carvalho, 2014), then might it reflect corrupt activities, where political friends potentially pocket (parts of) the money? While it is difficult to test this hypothesis directly, we can study whether the baseline alignment effect is larger in municipalities where corrupt activities have been detected in another context during our sample period. In column 5 of Table 2, we therefore use a municipality-level corruption indicator defined and provided by an influential study of Brollo et al. (2013). This indicator is based on random audits by Brazil's central government of the use of fiscal transfers received by the municipal government, and equals the share of audited funds for which severe irregularities were detected. While some municipalities in the data were audited twice, implying audits on the fund use of different municipal governments (2001-2004 versus 2005-2008), auditors always collected documents from the period 2001 until the time of the audit. We use the resulting data set – which includes 1,422 municipalities – to define a time-invariant dummy variable that equals one if in at least one audit, severe irregularities were detected (which is true for 42% of municipalities).[19] The coefficient on the four-tuple interaction term is not statistically significant, thus this exercise does not produce evidence in favor of the corruption hypothesis.

Financial consequences of political lending: lender profitability

Another way to test for a misuse of funds, as well as for the financial consequences of the observed lending practises, is to study lender profitability. Since lending decisions are operationalized by a bank's branches, we do so by estimating Equations (4) and (5) with bank-municipality-level return on assets as dependent variable. In alternative specifications, we replace banks' resource boom exposure with the variable's first lag, given that it probably takes time for credit decisions to affect lender profitability. The results show that the state-owned banks' branches which increase credit in politically aligned municipalities

[19] In general, corruption is surely more attributable to municipal governments than to municipalities per se. Nonetheless, those municipalities for which the dummy equals zero can be regarded as less corrupt than their counterparts during (the first half of) our sample period.

(relative to other branches of the same bank) suffer a small but statistically significant decrease in profitability (see Table 3, column 4). This is suggestive of a politically motivated misallocation of funds.

Further sources of heterogeneity at the municipal level

We proceed by testing for further sources of heterogeneity in the baseline alignment effect. In column 6 of Table 2, we study whether the effect is stronger in economically more developed municipalities, via a dummy that equals one if GDP per capita in 2000 exceeds the median; and in column 7, we test whether the effect differs across more urban versus rural municipalities, using the "Hierarquia urbana do Brasil" classification.[20] The results indicate that the baseline alignment effect occurs across the board, rather than only in richer versus poorer or more versus less urban municipalities.

Political orientation of the ruling president

Since Brazil has had presidents with different political orientations throughout our sample period (left, center, and right), it is worthwhile exploring whether the extent of politically motivated lending differs along this dimension. We first do so via a dummy variable that takes one if the president's party is classified as (relatively) centrist by Bolognesi et al. (2022). This applies to the presidency of Fernando Henrique Cardoso (1995-2002; PSDB) and Michel Temer (2017-2018; MDB).[21] The coefficient on the four-tuple interaction term is negative and statistically significant (see column 8), and the (unreported) marginal effect is virtually zero. This indicates that our baseline alignment effect is driven by left-wing and right-wing presidencies. In column 9, we extend the specification of column 8 by further including a four-tuple interaction term with a dummy for right-wing presidencies (Jair Bolsonaro, 2019-2022, PSL), leaving left-wing presidencies as baseline. The right-wing interaction is positive but not statistically significant, suggesting that the effect is not

[20] Municipalities are classified as being (part of) a Metropolis (6% of municipalities in our sample), Regional Capital (7%), Sub-regional Center (which are medium-sized cities; 6%), Center of Zone (21%), or Local Center (59%), in descending order of urbanity. We classify a municipality as urban if it is at least a Sub-regional Center.

[21] Michel Temer took office from Dilma Rousseff in August 2016 rather than on January 1 as usual, but since our data is annual, we consider 2017 as the start of his presidency (which lasted until December 31, 2018).

distinguishable in size across left-wing and right-wing presidencies.[22]

Variation in the degree of political alignment

Finally, we test whether our baseline alignment effect is stronger if we use an alignment definition that arguably reflects a stronger alliance between the central and municipal government. The corresponding dummy equals one if the president's party is not only included in the coalition that nominated the winning mayor (which is our baseline definition), but is in fact the party of the mayor him- or herself.[23] The interaction term is virtually zero and not statistically significant (see column 10 of Table 2), which speaks against alliance strength effects.

4.3 Robustness Checks

In this section we show the results of various robustness checks on the results of Table 1. The results are reported in Table 4. Although some of our robustness checks imply the inclusion of additional variables into the baseline specifications underlying Table 1, due to limited space we only report the coefficients on the variables displayed in Table 1.

Addressing the recent shift-share literature

In column 1, we address the recent developments in the literature on Bartik-style "shift-share" designs (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). This literature is relevant to our study because our natural resource boom exposure variable (see Equation 2) can be re-written to take the following form:

$$ResBoomExposure_{i,t} = ResExposure_i \times P_{i,t} \tag{6}$$

where

[22] Note that the interaction term featuring the centrist presidency dummy is smaller in magnitude and turn insignificant when using this specification. Combining the evidence of columns 8 and 9, we might therefore conclude that if any ranking were to be made, then right-wing presidencies would appear to engage the most in politically motivated lending via state-owned banks, followed by left-wing presidencies and (thereafter) centrist presidencies.

[23] This is true for 38% of municipality-years with a political alignment according to the baseline definition.

$$ResExposure_i = \sum_j RESendow_j \times \frac{Deposits_{i,j,t=0}}{TotalDeposits_{i,t=0}} \tag{7}$$

$$P_{i,t} = \sum_j RESprice_{j,t} \times \left(\frac{RESendow_j \times \frac{Deposits_{i,j,t=0}}{TotalDeposits_{i,t=0}}}{\sum_j RESendow_j \times \frac{Deposits_{i,j,t=0}}{TotalDeposits_{i,t=0}}} \right) \tag{8}$$

$ResExposure_i$ captures a bank's exposure to natural resource *endowment* (as opposed to booms) across Brazil as of 2000 (recall that $RESEndow_j$ equals total resource endowment of municipality j, as defined in Section 3.1). Intuitively, resource endowment in a certain municipality matters more for a bank if the municipality assumes a larger share in the bank's portfolio. $P_{i,t}$ is a weighted average of the normalized natural resource price $RESprice_{j,t}$ (as defined in Section 3.1) across all origin municipalities j where bank i is present. Intuitively, the prices of the municipality's resources are more relevant to the bank the larger the endowment is and the more important the municipality is for the bank.[24] While Equation (6) is unnecessarily complex (see footnote 24 for an illustrative example on its component $P_{i,t}$), it visualizes that $ResBoomExposure_{i,t}$ is a shift-share instrument, where $ResExposure_i$ is the share variable and $P_{i,t}$ the shift variable. Applied to our setting, Goldsmith-Pinkham et al. (2020) show that to ensure a causal interpretation of $\hat{\beta}_1$ in Equation (4), it is sufficient that the shares, $ResExposure_i$, are exogenous to other bank characteristics *conditional on controls*. In order to evaluate the robustness of our results, we therefore need to feed Equation (4) with additional interaction terms, each of which consists of $P_{i,t}$ times a control variable that correlates with $ResExposure_i$, and evaluate whether $\hat{\beta}_1$ remains statistically significant. We examine how $ResExposure_i$ is related to other bank-level variables by regressing it on a vector including bank size (=total assets), deposit to asset ratio, capital ratio, and liquid to total assets ratio, all measured in 2000

[24] As an illustrative example, consider a bank that operates in three municipalities, two of which are resource-endowed (origin) municipalities. In one of those, there is a small resource endowment (gold; \$5 million) and the bank's local deposits only constitute 1% of total bank deposits. The indexed gold price $P_{c=gold,t}$ (see Section 3.1) equals 1.05 in year t (thus, the gold price is 5% higher than in $t = 0$, the year 2000). In the other origin municipality where the bank operates, there is a large resource endowment (iron ore; \$100 million) and the bank's local deposits make up 50% of the bank's total deposits. $P_{c=iron\ ore,t} = 1.2$. This implies that the bank-specific resource price index $P_{i,t}$ is heavily skewed towards the price of iron ore:
$P_{i,t} = 1.05 \times \frac{5\times0.01}{5\times0.01+100\times0.5+0\times0.49} + 1.2 \times \frac{100\times0.5}{5\times0.01+100\times0.5+0\times0.49} + 0 \times \frac{0\times0.49}{5\times0.01+100\times0.5+0\times0.49} = 1.19985.$

(same as $ResExposure_i$). The results (see Table A2, column 6) show that there is a statistically significant correlation between a bank's exposure to natural resource endowment across Brazil and a bank's size and liquidity ratio. Conceptually, one concern could thus be that our baseline findings in column 4 of Table 1 simply reflect that larger banks' credit supply responds differently to business cycle fluctuations that correlate with global commodity prices. However, column 1 of Table 4 shows that our results are robust to the inclusion of bank size (and bank liquidity ratio, both interacted with $P_{i,t}$ and then subjected to the inverse hyperbolic sine transformation) into Equation (4).[25] This is reassuring from an identification perspective, in light of the findings of Goldsmith-Pinkham et al. (2020).

Dropping municipalities where political alignment is created through municipal elections
One might be concerned that both the credit supply decisions of banks and our political alignment variable are affected by time-varying factors at the municipality level. For example, a municipality-specific economic boom may not only increase the number of creditworthy projects, but also increase the popularity of the ruling president and thereby lead to the election of a politically aligned mayor. While this concern is resolved by the inclusion of municipality-year fixed effects into Equation (5), we also address it via estimating Equation (5) on a restricted sample, in which political alignment is only created via presidential elections. Specifically, we show that if we only include municipalities where in no single year during our sample period, political alignment is created by a municipal election, our results are very robust: despite the much smaller sample size, the size of our key triple interaction coefficient remains almost identical, only the significance level is lowered due to a larger standard error (see column 3 of Table 4).

Is resource boom exposure a proxy for other bank-level variables which drive our results?
Following our narrative, the triple interaction coefficient in column 6 of Table 1 can be interpreted as follows: among two state-owned banks, the one with higher resource boom exposure increases lending in politically aligned municipalities by more, because the higher

[25] Given Equation (6), we can express $ihs(ResBoomExposure_{i,t})$ as $ihs(ResExposure_i \times P_{i,t})$. Since $ResExposure_i$ correlates with a bank's total assets and liquidity ratio, the additional interaction terms we include in column 1 of Table 4 are $ihs(TotalAssets_i \times P_{i,t})$ and $ihs(LiquidityRatio_i \times P_{i,t})$.

boom exposure implies more excess liquidity. One potential concern with this interpretation is that banks that are more represented in resource-rich regions (in the sense of Equation 7), and therefore also have more resource *boom* exposure on average (in terms of Equation 6), also differ along other dimensions, which might drive our results. As discussed, the results in column 6 of Table A2 show for instance that banks with more resource endowment exposure are larger than other banks. Meanwhile, larger banks' lending decisions might be more strongly influenced by political factors such as whether a municipality is politically aligned with the president or not. For example, large banks' CEOs might be more connected to the central government, and/or larger banks may have a bigger capacity to shift their lending focus to places that prosper as a consequence of the central government channeling more funds to them through non-banking channels. In the extreme case, our results might simply reflect that more boom-exposed state-owned banks increase lending by more in politically aligned municipalities because they are larger and thus "follow political lending trends" more strongly, rather than because they have more excess liquidity. We account for this potential concern by including additional interaction terms that contain bank size (as of 2000) whenever the original term in Equation (4) contains *ResBoomExposure*. Based on the results of column 6 of Table A2, we do the same for banks' liquidity ratio. The results are highly robust to this modification (see column 4 of Table 4), which speaks against the illustrated concern.

Dropping banks that are owned by a state (provincial) government

In our baseline specification, we define state-owned banks as those that are owned by the central government or by one of Brazil's 27 states (provinces). Since our definition of political alignment is centred around the ruling party in central government – which most likely exerts more influence over centrally owned banks than over provincially owned banks – we drop the latter in a robustness check. While this reduces the number of state-owned banks in our sample from eleven to five, our results are robust to this modification (see column 5 of Table 4).

Addressing the concern of endogenous resource prices

Out of the 152 origin municipalities that enter our sample (see Table 1, column 2), 24 host iron ore reserves, and iron ore endowment makes up 29% of total resource endowment across all 152 origin municipalities. Globally speaking, this abundance of iron ore endowment makes Brazil the second-largest producer of this key commodity. One might therefore be concerned that local developments in very iron ore-rich municipalities affect not only local credit supply, but also the global iron ore price, and thereby the resource boom exposure of banks that are active in iron ore regions. We address this concern with a very demanding robustness check in which we drop banks whose share of iron ore in total resource endowment exposure (see Equation 7) exceeds 1%. Despite a drastic sample size reduction, our key triple interaction coefficient is robust to this modification (see column 6 of Table 4).

Further addressing concerns around counfounding credit demand

As already eluded to in Section 3 (see footnote 14 in particular), one might be concerned about selection effects where banks with higher resource boom exposure are over-represented in destination municipalities where local firms have higher credit demand when commodity prices are high, for instance due to supply chain linkages. While municipality-year fixed effects in Equation (4) account for such effects by "forcing" the specification to exploit variation across the multiple banks in a municipality, we further address this concern by dropping destination municipalities with an above-median share of manufacturing in local GDP. The underlying idea is that if manufacturing is less important for a municipality, then also varying credit demand by manufacturing firms that supply goods elsewhere is less relevant. The results are very robust to this exercise (see column 7 of Table 4).

Another potential concern is bank-specific credit demand, where borrowers demand more credit from the branches of banks that are more exposed to natural resource booms across Brazil. One scenario could be that a specific credit type is in higher demand during boom times: for instance, suppliers to the resource sector that locate in destination municipalities might demand more commercial loans during booms, and banks that tend to be more boom-exposed might be over-represented in this particular credit market segment. We address such concerns by breaking down bank-municipality-year-specific total credit into three sub-categories (commercial credit; consumer credit; mortgages and other credit), and defining the unit of observation as bank-municipality-credit type. This allows us to include municipality-credit type-year fixed effects, which control for potentially higher demand for one of the three credit types during boom times.[26] The results are robust to the use of this alternative specification (see column 8 of Table 4).

5 Conclusion

In this paper, we used highly granular banking data from Brazil to reveal a political bias in the financial intermediation of liquidity windfalls. Following a rise in deposits due to natural resource booms, state-owned banks increase credit supply outside of resource-rich areas by more if the destination municipality's local government is politically aligned with the ruling president. In contrast, private banks redistribute significantly less funds to aligned municipalities, compared to other destination municipalities. Unlike previous studies, our results are not driven by changes in lending policies during election years or when an allied incumbent faces strong political competition. Instead, we find stronger effects when the allied incumbent was elected with a large margin of victory, suggesting that favoritism towards strong allies is a key driving factor of our results. These findings provide novel evidence that political lending extends beyond attempts to secure election victories. We also show that the political bias in state-owned bank lending only occurs under left-wing and right-wing presidencies, but is absent under centrist ones. Additional evidence reveals that politically motivated lending by state-owned banks has a negative impact on bank profitability, indicating a misallocation of credit. Overall, our results suggest that developing countries struggle to efficiently absorb large financial gains due to institutional weaknesses, especially when state-owned banks play a large role in the domestic financial market.

[26] We replace bank-municipality fixed effects with bank-municipality-credit type fixed effects in this specification.

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6 Tables

Table 1: Baseline Results

Dependent Variable →	ihs(Deposits)	ihs(Credit)	$\frac{Deposits-Loans}{Assets_{t-1}}$	ihs(Credit)		
Unit of Observation →	Bank-year	Bank-municipality-year				
Sample →	All Banks	Resource-endowed (=origin) municipalities		Non-resource-endowed (=destination) municipalities		
	(1)	(2)	(3)	(4)	(5)	(6)
ihs(Exposure)	0.689***	0.467*	0.809*	0.371*	0.370*	0.428**
	(0.129)	(0.238)	(0.445)	(0.196)	(0.195)	(0.210)
ihs(Exposure) × Political alignment					0.002	-0.016**
					(0.007)	(0.007)
ihs(Exposure) × State-owned						-0.189**
						(0.091)
ihs(Exp.) × Pol. alignm. × State-owned						0.048***
						(0.008)
Fixed effects	Bank, Year	Bank-Muni, Muni-Year	Bank-Muni, Muni-Year	Bank-Muni, Muni-Year	Bank-Muni, Muni-Year	Bank-Muni, Muni-Year
Other relevant interaction terms	n/a	n/a	n/a	n/a	n/a	Yes
Lagged bank-muni-level controls	n/a	Yes	Yes	Yes	Yes	Yes
Observations	880	11,382	11,382	139,134	139,134	139,134
# Banks	70	41	41	65	65	65
# Municipalities	n/a	152	152	2,279	2,279	2,279
Effect of political alignment on state-owned banks' credit in destination municipalities						0.031***
						(0.007)

Notes: In this table we study the impact of natural resource booms on banks' deposits (column 1), how these additional funds are being used by banks in terms of credit supply (columns 2-4), and to what extent political factors drive these credit decisions (columns 5-6). The unit of observation is a bank-year in column 1 and a bank-municipality-year in columns 2-6. In column 1, we aggregate deposits across all of the bank's branches and test whether deposits rise with banks' exposure to natural resource booms across Brazil. In column 2, we test whether higher boom exposure induces a bank to raise credit supply in resource-endowed municipalities. In column 3, we analyze whether for banks with higher boom exposure, deposits rise by more than lending, in resource-endowed municipalities. This serves to test whether funds from these municipalities are available for redistribution to other regions. In columns 4-6, we study the impact of banks' boom exposure on credit supply in municipalities without resource endowment ("destination municipalities"). *Political alignment* is a dummy variable which equals one if the ruling president's party is part of the electoral coalition that nominated the mayor who currently rules in the destination municipality. *State-owned* is a dummy taking one if the bank is owned by the central or a provincial government. Our sample period is 2001-2022, but we use data from 2000 for the lagged bank-municipality-level controls. The marginal effect at the bottom of column 6 equals the sum of the second and the fourth coefficient in the column. Standard errors in parentheses are clustered at the bank level in column 1 and at the bank- and meso region-level in columns 2-6. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level.

Table 2: Heterogeneous effects

Dependent Variable →	ihs(Credit)									
Sample →	Non-resource-endowed (destination) municipalities									
Explored Heterogeneity →	Election Year	Full Election Cycle (Baseline= Elec year)	Political competition		Corruption measure	Level of economic development	Urban vs. Rural	Political orientation of president party		Narrower definition of political alignment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ihs(Exposure) × Political alignment × State-owned	0.044***	0.048**	0.028***	0.025***	0.043***	0.050***	0.041***	0.042***	0.031*	0.066***
	(0.009)	(0.019)	(0.007)	(0.007)	(0.014)	(0.010)	(0.011)	(0.011)	(0.017)	(0.011)
ihs(Exp.) × Pol. alignment × State-owned × Municipal election year	0.003									
	(0.022)									
ihs(Exp.) × Pol. alignment × State-owned × 1Y before municipal election year		0.007								
		(0.011)								
ihs(Exp.) × Pol. alignment × State-owned × 2Y before municipal election year		0.004								
		(0.026)								
ihs(Exp.) × Pol. alignment × State-owned × 3Y before municipal election year		-0.036								
		(0.030)								
ihs(Exp.) × Pol. alignment × State-owned × Last election won by large margin			0.040***							
			(0.010)							
ihs(Exp.) × Pol. al. × State-o. × Margin of victory in last election (sc. by sdev)				0.024***						
				(0.006)						
ihs(Exp.) × Pol. alignment × State-owned × Corrupt municipality					-0.031					
					(0.033)					
ihs(Exp.) × Pol. alignment × State-owned × Large GDP per capita						-0.005				
						(0.013)				
ihs(Exp.) × Pol. alignment × State-owned × Urban municipality							0.009			
							(0.018)			
ihs(Exp.) × Pol. alignment × State-owned × Centrist president								-0.041*	-0.030	
								(0.024)	(0.026)	
ihs(Exp.) × Pol. alignment × State-owned × Right-wing president									0.037	
									(0.033)	
ihs(Exp.) × Pol. alignment × State-owned × Alignment more narrowly defined										-0.002
										(0.060)
Observations	139,134	139,134	138,804	138,804	37,357	139,134	139,134	139,134	139,134	139,134

Notes: In this table we analyze whether the degree to which state-owned banks supply more credit in politically aligned destination municipalities depends on the timing of and distance to elections (columns 1-2), political competition (columns 3-4), municipality characteristics (columns 5-7), the political orientation of the ruling president's party (columns 8-9), or the degree of political alignment (column 10). To do so, we estimate an extended version of equation (5), which includes an additional four-tuple interaction term $ihs(Exposure) \times Political\ alignment \times State-owned \times X$ (where X varies by column), as well as all other relevant interaction terms that result from this inclusion. Due to limited space, we only include the coefficients on this four-tuple interaction as well as the triple interaction term $ihs(Exp.) \times Pol.\ alignm. \times State-owned$. *Municipal election year* is a dummy that equals one in the years in which municipal elections are held: 2004, 2008, 2012, 2016, and 2020 (these elections were held in October, except for November in 2020). *Last election won by large margin* is a dummy taking one if the difference between the winner and the runner-up in the last municipal mayor election was larger than the median difference across all mayor elections in our sample period (which equals 12.0 percentage points). *Margin of victory in last election (scaled by sdev)* equals the winner's margin of victory in percentage points, scaled by the variable's standard deviation, which equals 19.1ppt. *Corrupt municipality* is a dummy variable based on the narrow corruption measure of Brollo et al. (2013), who compute this variable for 1,422 Brazilian municipalities. The dummy takes one if in at least one year, audits by Brazil's government of the use of fiscal transfers received by the municipality revealed severe irregularities (which is true for 42% of municipalities). *Large GDP per capita* equals one if GDP per capita in the year 2000 is larger than the median. *Urban municipality* is a dummy that equals one if the "Hierarquia urbana do Brasil" classifies the municipality as "Sub-regional center" (which are medium-sized cities), "Regional capital", or "Metropolis", as opposed to "Local center" or "Center of Zone". In columns 8-9, we classify the orientation of the president's party into Centrist (PSDB, 2000-02; MDB, 2017-18), Left (PT, 2003-16), and Right (PSL, 2019-22). *Alignment more narrowly defined* equals one if the president's party is not only part of the coalition backing the mayor, but in fact equal to the party of the mayor him- or herself. Standard errors in parentheses are clustered at the bank- and meso region-level. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level.

Table 3: Lending decisions and bank-municipality-level profitability

Dependent Variable →	Bank-municipality-level Return on Assets in %			
Sample →	Non-resource-endowed (=destination) municipalities			
Timing of *Exposure* →	Current			(t–1)
	(1)	(2)	(3)	(4)
ihs(Exposure)	1.063	1.045	-2.538	-2.589
	(0.941)	(0.905)	(1.827)	(1.849)
ihs(Exposure) × Political alignment		0.129		0.021
		(0.126)		(0.021)
ihs(Exposure) × State-owned		-0.062		0.183
		(0.068)		(0.180)
ihs(Exposure) × Political alignment × State-owned		-0.169		-0.080*
		(0.124)		(0.046)
Observations	139,134	139,134	139,134	139,134

Notes: In this table we estimate equation (4) (see columns 1-2) and an alternative version (see columns 3-4) for bank-municipality-level profitability as dependent variable. We define profitability as return on assets, in percent. In columns 3 and 4, *Exposure* refers to $Exposure_{t-1}$, thus banks' resource boom exposure in the previous year. Standard errors in parentheses are clustered at the bank- and meso region-level. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level.

Table 4: Robustness Checks

Dependent Variable →	ihs(Credit)							
Unit of Observation →	Bank-municipality-year							Bank-muni-credit type-year
Sample →	Non-resource-endowed (destination) municipalities							
Nature of Robustness Check →	Shift-share robcheck	Repeat baseline (T1, c6), for comp.	Drop mayor-driven alignment	Is Exp. a proxy for other bankvars?	Drop prov-gov owned banks	Drop banks w. non-neglig. IronOre exp.	Drop very industr-ialized muni's	Unit of obs. = bank-muni-credit type-year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ihs(Exposure)	0.586**	0.428**	0.196	0.428**	0.449*	-0.033	0.482***	1.293*
	(0.272)	(0.210)	(0.221)	(0.210)	(0.227)	(0.087)	(0.150)	(0.680)
ihs(Exposure) × Political alignment		-0.016**	-0.021**	-0.013*	-0.014*	-0.015**	-0.011	0.009
		(0.007)	(0.009)	(0.008)	(0.008)	(0.007)	(0.012)	(0.016)
ihs(Exposure) × State-owned		-0.189**	-0.153*	-0.191**	-0.210**	0.130	-0.233**	-0.029
		(0.091)	(0.086)	(0.090)	(0.097)	(0.108)	(0.103)	(0.168)
ihs(Exposure) × Political alignment × State-owned		0.048***	0.047*	0.057***	0.085***	0.055*	0.053***	0.039**
		(0.008)	(0.027)	(0.013)	(0.025)	(0.031)	(0.009)	(0.017)
Observations	139,134	139,134	40,540	139,134	127,202	16,750	55,805	417,402

Notes: In this table we perform several robustness checks on the results of column 4 of Table 1 (see column 1) and of column 6 of Table 1 (see columns 3-8). For convenience, in column 2 we repeat the results of column 6 of Table 1. In column 1, we address the findings of recent shift-share literature (Goldsmith-Pinkham et al., 2020), by including the interaction terms $ihs(Banksize \times P_{i,t})$ and $ihs(Bank\ liquidity\ ratio \times P_{i,t})$ into Equation (4) (see Section 4.3 for details). In column 3, we exclude all municipalities where at least once during our sample period, *Political alignment* changes from zero to one due to a municipal election, considering the election years 2004, 2008, 2012, 2016, and 2020. In column 4, we include the following interaction terms into Equation (5): $ihs(BankSize) \times Political\ alignment \times State-owned$ and $BankLiquidityRatio \times Political\ alignment \times State-owned$ (where $Size$ and $LiquidityRatio$ are measured in 2000). In column 5, we drop banks that are owned by one of Brazil's state (province) governments; in this specification $State-owned = 1$ thus implies that the bank is owned by the central government. In column 6, we drop banks for which exposure to iron ore endowment makes up more than 1% of their total resource endowment exposure (see Equation 7). In column 7, we drop municipalities for which the share of manufacturing in total GDP of the year 2000 exceeds the median share across municipalities. In column 8, we break down total credit into three categories (commercial loans; consumer loans; mortgages and other loans), and re-define the unit of observation from bank-municipality-year to bank-municipality-credit type-year. This specification includes bank-municipality-credit type fixed effects and municipality-credit type-year fixed effects. Standard errors in parentheses are clustered at the bank- and meso region-level. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level.

Appendix

Table A1: Summary Statistics

	Mean	Median	Min	Max	sdev	N
Panel I: Bank-year-level variables						
Total assets (in billion 2000 USD)	126.46	6.34	0.03	2,737.07	345.58	880
Deposits (in billion 2000 USD)	3.09	0.02	0.00	66.96	9.56	880
Total credit (in billion 2000 USD)	7.36	0.58	0.00	135.37	21.86	880
ihs(Deposits)	19.58	19.15	4.32	27.43	3.43	880
ihs(Exposure)	5.73	7.85	0.00	12.20	4.79	880
Panel II: Bank-municipality-year-level variables, and variables used in bank-municipality-year-level regressions						
Assets (in million 2000 USD)	609.93	15.33	0.00	1.52e+06	17,221.99	150,516
Deposits (in million 2000 USD)	17.47	3.84	0.00	8,182.76	142.66	150,516
Total credit (in million 2000 USD)	41.78	4.51	0.00	73,692.98	718.31	150,516
ihs(Total credit) [Sample = origin municipalities]	17.42	17.41	6.26	25.55	2.01	11,382
(Deposits – Loans) / Lagged Assets [Sample = origin mun.]	0.07	-0.01	-254.80	894.32	8.81	11,382
ihs(Total credit) [Sample = destination municipalities]	17.30	17.31	4.95	27.67	1.88	139,134
Return on Assets [Sample = destination municipalities]	0.01	0.00	-6.24	0.18	0.04	139,134
State-owned	0.57	1	0	1	0.50	139,134
Political alignment [S. = d-mun]	0.30	0	0	1	0.46	139,134
Last election's winner's margin of victory in % [S. = d-mun]	18.28	12.30	0.00	100.00	19.23	138,804
Last election won by large margin [S. = d-mun]	0.51	1	0	1	0.50	138,804
Corrupt municipality [S. = d-mun]	0.41	0	0	1	0.49	37,357
Large GDP per capita [S. = d-mun]	0.61	1	0	1	0.49	139,134
Urban municipality [S. = d-mun]	0.33	0	0	1	0.47	139,134
Centrist president [S. = d-mun]	0.19	0	0	1	0.39	139,134
Right-wing president [S. = d-mun]	0.18	0	0	1	0.39	139,134
Alignment more narrowly defined [S. = d-mun]	0.12	0	0	1	0.33	139,134

Notes: This table provides summary statistics on the variables used in our analysis. For variable descriptions, see the main text and/or the Notes of Tables 1-3. For variables that are used directly in the analysis, we show descriptive statistics using the same sample as in the regressions, as also indicated in square brackets above.

Table A2: Auxiliary Results

Dependent Variable →	ihs(Deposits)		ihs(Deposits)	Deposits /Assets	ihs(Credit)	Bank Resource Endowment Exposure (see Eq. 7)
Unit of Observation →	Bank-muni- -year		Bank-year		Bank- muni- -year	Bank
Sample →	All		All		Origin munis	All
	(1)	(2)	(3)	(4)	(5)	(6)
Resource endowment × ihs(Resource price)	0.032**	0.051**				
	(0.014)	(0.022)				
Res. end. × ihs(Res. price) × State-owned		-0.036				
		(0.021)				
ihs(Exposure)			0.711***		0.491*	
			(0.122)		(0.264)	
ihs(Exposure) × State-owned			-0.590***		-0.050	
			(0.202)		(0.098)	
State-owned				0.046***		
				(0.009)		
ihs(Bank Total Assets in 2000)						0.921***
						(0.222)
Bank Deposits/Assets in 2000						-0.370
						(0.688)
Bank Capital/Assets in 2000						2.489
						(2.151)
Bank Liquid Assets / Total Assets in 2000						-0.294***
						(0.110)
Fixed effects	Bank-Muni, Muni-Year	Bank-Muni, Muni-Year	Bank, Year	None	Bank-Muni, Muni-Year	None
Other relevant interaction terms	n/a	n/a	n/a	n/a	Yes	n/a
Lagged bank-muni-level controls	n/a	n/a	n/a	n/a	Yes	n/a
Observations	154,289	154,289	880	880	11,382	70

Notes: In this table we present several auxiliary results which are discussed throughout Section 4. In column 1, we estimate Equation (1). In column 2, we estimate Equation (1) including an additional interaction term. In column 3, we estimate Equation (3) including an additional interaction term. In column 4, we pool all bank-years (of banks in our sample) and test whether state-owned banks have a higher deposit-to-asset ratio. In column 5, we estimate Equation (4) including an additional interaction term. In column 6, we regress a bank's resource endowment exposure (see Equation 7), which is time-invariant and measured in the year 2000, on several bank characteristics, also measured in 2000. The last three variables in column 6 are scaled by their standard deviation. Note that in columns 4 and 6 we do not include any fixed effects because we are interested in the simple correlation between the studied variables. Standard errors in parentheses are clustered at the bank- and meso region-level in columns 1, 2, and 5, and are clustered at the bank level in column 3. In columns 4 and 7, we use robust standard errors. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level.

Online Appendix

"The Role of Politics in Financial Intermediation: Evidence from Brazil"

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OA1 Metal data

OA1.1 Metal prices data

For gold, copper, iron ore, nickel, tin, uranium, and zinc, we use the IMF Primary Commodity Prices dataset. For all other metals, we use data from the United States Geological Survey (USGS).

OA2 Oil and gas data

We construct a novel database on oil and gas endowment at the municipality level by combining multiple, publicly available data sets. Our point of departure is annual data on all Brazilian oil and gas fields, provided by the Brazilian National Agency for Petroleum, Natural Gas and Biofuels (ANP). While this data set includes information on initial "Volume of Oil in Place" (VOIP) and "Volume of Gas in Place" (VGIP), these concepts indicate the total amount of oil and gas in the field rather than economically recoverable reserves (henceforth, ERR), and we don't know the field-specific recovery rate.[1] We therefore compute initial ERR by dividing cumulative production – which is available in the data set – with field-specific information on which fraction of ERR has been recovered (as of the same year), which is available in a publicly available section of the GlobalData fields database.[2] Afterwards, we subtract pre-2000 production, which we compute using ANP data, from initial ERR to arrive at economically recoverable reserves in the year 2000 at the field level. Finally, we need to match fields to municipalities. For onshore fields, this is simple because the municipality is listed in ANP's production data. For offshore fields, we follow previous literature and use Brazil's oil and gas royalty allocation scheme. Specifically, Petrobras pays extraction royalties (through ANP) to municipal governments, and the royalty allocation formula is such that a certain percentage of each offshore field's production value is paid to the "municipalities facing the oil fields". To define these municipalities and their royalty share, Brazilian law extends each municipality's terrestrial borders onto the

[1] VOIP×recovery rate=economically recoverable reserves. Recovery rates (also referred to as recovery factors) vary widely across fields around the world.

[2] We obtain the relevant information from the GlobalData fields database from the associated and freely accessible portal offshore-technology.com.

continental shelf and then assigns to each municipality the fraction of the oil field that lies within these extended borders (Caselli and Michaels, 2013). We download the resulting data from ANP website and use them to allocate a field's reserves to municipalities. We then aggregate a municipality's onshore and offshore oil reserves, and do the same for gas. In a final step, we transform physical to monetary reserves by multiplying with average prices over 2000-2022, and add up oil and gas reserves for each municipality. Across the 244 municipalities with oil and/or gas endowment, the average reserves equal \$18 billion, and the median equals \$1.4 billion.