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JEL Classification Numbers: G21, D72, D73, P16.

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1 Introduction

What are the consequences of political influence on banking and economic outcomes? Theories of political lending cycles predict that governments use loans by state-owned banks as a strategic tool for re-election purposes. In particular, bank credit can be reallocated around election years with the aim of shifting election outcomes in favour of the ruling party or parties in control of central government. Does such targeted redistribution simply favour certain regions and lead to an increase in credit? Or can it be used to punish others on the basis of their attractiveness to politicians and actually result in financial constraints? If so, does this reallocation of credit have real effects on the economy?

We test theories of political cycles in Turkey using the universe of bank credit. Unlike previous literature, we can draw on quarterly data to identify the exact timing of politically induced lending and shed light on potential mechanisms. We complement this information with administrative data aggregated from balance sheets and income statements of every registered firm to the industry-by-province level. This allows us to quantify the misallocation of financial resources at the aggregate level and the resulting distortions to economic outcomes. We further quantify how politically induced lending leads to local and aggregate total factor productivity (TFP) losses associated with capital and labour misallocation.

We document three main sets of findings. First, we show that state-owned banks engage in strategic lending around local elections when compared with private banks. In contrast to earlier literature, state-owned banks curb credit prior to local elections in the aggregate. This result is driven by cross-sectional reallocation of credit between constituencies defined by their political alignment and the degree of electoral competition. In particular, state bank lending increases in provinces when an incumbent mayor aligned with the ruling party in central government faces competition from opposition parties. In contrast, closely contested provinces get relatively less credit from state banks in the run up to elections if the incumbent mayor is from an opposition party. When we also take into account uncontested provinces, we find that the total effect of local elections on bank lending is small in politically aligned provinces, but significantly large and disruptive in opposition provinces.

We interpret this vastly different behaviour of state banks around elections as strong evidence for the existence of a political lending cycle, consistent with incentives of "tying your enemy's hands in close races" (Brollo and Nannicini, 2012). It appears that the central government – via its control over state banks – strategically targets provinces either to support their own mayors, or to punish opposition mayors, so that their candidates have a better chance in upcoming elections. These results exist only for corporate loans, but not for consumer loans. This suggests that the reallocation of credit aims to influence voting patterns

through local corporate activity, for instance by affecting voters' employment prospects.

Our identification strategy builds on difference-in-differences estimates that exploit the greater susceptibility of state-owned banks to political pressure compared with private banks. We take advantage of the fact that local elections are held on a fixed, five-year schedule across the nation, which prevents politicians' ability to time elections. We use variation across localities in electoral competition and political alignment to identify elements of tactical redistribution and rule out alternative explanations.[1] Our results are robust to alternative specifications that control for unobservable and demand-driven explanations of the lending cycle. In addition, we collect novel data on the rewarding of investment incentives by the central government and new public construction projects to test for an electoral cycle in the distribution of public funds. Our results suggest political lending is mostly driven by supply-side factors, but we cannot fully rule out potential demand-side mechanisms.

In our second set of findings, we present evidence that economic activity is strongly influenced by political lending. Election cycles and close election outcomes provide a quasi-exogenous variation in how aggregate credit is reallocated across the country. We expect this reallocation to affect financial constraints and have real consequences if borrowers are unable to switch lenders. We confirm this conjecture and find that firms in opposition provinces experience substantial reductions in bank debt and no increase in non-bank debt, especially in industries that are more dependent on lending from state banks. In contrast, businesses located in politically contested provinces aligned with the ruling party have increased access to both types of debt.

We find that economic activity suffers in opposition provinces as a result of tightened financial constraints. In particular, industries with a high share of state bank lending located in politically contested provinces experience substantial reductions in assets, employment and sales in the run up to local elections if the incumbent mayor is from an opposition party. We find the exact opposite patterns if the incumbent mayor is from the ruling party, although these estimates are not statistically significant. We do not detect an electoral cycle in business dynamics, suggesting that credit reallocation operates through existing businesses.

In our third set of findings, we document how the political lending cycle leads to misallocation of production factors and TFP losses. First, we show that credit growth in the run up to a local election suffers in industries with initially higher efficiency (or marginal revenue product of capital, MRPK), especially in opposition provinces. Second, we quantify factor misallocation and its impact on TFP across industries within provinces and between provinces. We focus on this level of misallocation given our earlier evidence on how polit-

[1] We use the terms *aligned* and *allied* interchangeably throughout the text. Either terminology refers to incumbent mayors affiliated with the ruling party in central government.

ical lending distorts employment and assets based on political alignment of provinces and state banks' share of lending at the industry level. To guide our measurement, we follow an approach similar to Brandt et al. (2013) and pioneered by Restuccia and Rogerson (2008) and Hsieh and Klenow (2009). This approach helps us document how capital and labour misallocation contribute to within-province and between-province distortions. Finally, we draw on a comprehensive firm-level survey to test for an electoral cycle in the dispersion of MRPK within province-industry pairs.

Over the sample period, we find that misallocation reduces aggregate TFP by around 25%. We show that within-province distortions explain around two thirds of this aggregate reduction, and that capital market imperfections explain the vast majority of distortions both within and between provinces. Our estimates imply that efficient TFP levels are consistently higher in opposition provinces than in politically aligned provinces by 4.9% on average during this period. This means that more labour and capital should be allocated to the former in the absence of distortions. We also document that within-province distortions are higher in opposition provinces; on average, actual TFP was 19.4% lower than efficient TFP in opposition provinces compared with a 13.1% distortion in aligned provinces. We estimate that the electoral cycle leads to actual TFP levels that are 1.9% lower than their efficient levels due to within-province distortions. We also find some evidence that, in local election years, MRPK dispersion across firms increases more in industries with greater state bank presence in politically contested opposition provinces.

The literature on targeted redistribution distinguishes between constant patronage, which refers to rewarding core supporters (Cox and McCubbins, 1986), and tactical redistribution, which aims to achieve electoral gains by targeting politically competitive regions around elections (Dixit and Londregan, 1996). "Patronage" involves awarding areas in which the incumbent party enjoys strong support regardless of the electoral cycle. "Tactical redistribution" predicts that resources will be directed towards swing districts to influence voters' preferences in the run up to elections. Our results pinpoint tactical redistribution as the driver of the credit cycle. They are consistent with a setting in which voters are unable to distinguish the sources of government transfers and political spillovers occur in favour of incumbent mayors (Brollo and Nannicini, 2012).

We contribute to three strands of the literature. First, we provide new evidence on the intersection of political forces on lending and shed light on the underlying mechanisms. Existing studies find that political influence is used to *expand* credit to secure votes.[2] In

[2]Dinç (2005) finds cross-country evidence, while Cole (2009) and Carvalho (2014) provide evidence for India and Brazil, respectively, for increased political lending in national elections. Englmaier and Stowasser (2017) provide evidence for Germany around local elections. Baum et al. (2010) find no evidence for political lending around parliamentary elections in Turkey. Chavaz and Rose (2019) find that private banks receiving

contrast, we document an aggregate *reduction* in credit due to tactical redistribution across regions.[3] Unlike previous studies, in which political pressure is applied by *local* governments on *local* state banks, our setting predicts political influence by the *central* government on *national* state banks. In local elections, a central government's control over state banks leads to different incentives across regions depending both on their political attractiveness and alignment, as it can use transfers to favour political friends or to punish political enemies (Brollo and Nannicini, 2012). Hence, the aggregate impact of political lending cycles is conditional on how countries' political systems and electoral competition interact with their banking sectors.

Second, we contribute to the literature on how government control over banks affects allocation of financial resources and the real economy. While government ownership can help solve credit market failures that arise due to coordination problems or information asymmetries (Stiglitz, 1993), they could also end up serving the private interests of politicians and result in a misallocation of financial resources (Shleifer and Vishny, 1994; Shleifer, 1998; La Porta et al., 2002). We present novel evidence on aggregate financial constraints and quantify the local and aggregate costs of politically induced lending based on administrative corporate balance sheet data. This evidence relates to earlier studies on how politicians influence firms' real decisions, which find that firms tend to increase employment and plant creation under political pressure (Carvalho, 2014; Bertrand et al., 2018). We add to this work by showing how firms in politically misaligned provinces in fact suffer reductions in employment, assets and sales, due to tactical redistribution.

Our third contribution is to quantify the local and aggregate productivity losses from political lending. We identify tactical redistribution as a key driver of factor misallocation within provinces and point out state ownership of banks as an additional government policy that distorts the efficient allocation of resources (Guner et al., 2008). Our work therefore relates to a growing body of work on financial frictions and misallocation (Gilchrist et al., 2013; Midrigan and Xu, 2014; Larrain and Stumpner, 2017; Gopinath et al., 2017; Bai et al., 2018), and sheds light on the causes and costs of misallocation (Restuccia and Rogerson, 2017).

The paper is organised as follows. Section 2 describes the institutional background. Section 3 describes the data. Section 4 presents our empirical methodology and results on tactical reallocation of credit. Section 5 documents the real effects of politically induced lending and estimated efficiency losses from misallocation. We conclude in Section 6.

government funds in the United States increased lending after 2008 in line with political incentives.

[3] Akey et al. (2020) document a similar negative association between political power and consumer credit by private banks in the United States.

2 Institutional Background

2.1 The Turkish banking sector

The Turkish financial system is dominated by deposit-taking banks, which are the primary sources of funding in the economy. Both state-owned and private banks provide banking services through nation-wide branch networks, and there are no local or regional banks. Banks primarily lend to corporates and households with no particular sectoral specialisation, having left behind the episode of funding government deficits in the 1980s and 1990s, when political interference was widespread.[4]

The shift in Turkish banking activity toward private sector financing followed an intensive restructuring phase after currency and banking crises in 1999-2001. An extensive reform package was initiated under the guidance of the International Monetary Fund (IMF) to strengthen the banking sector's operational efficiency and financial stability and to remove political interference. The central bank gained its institutional independence, while an independent Banking Regulation and Supervision Agency (BRSA) was established. In early 2003, BRSA pushed through the early adoption of Basel II capital adequacy standards and a limited deposit insurance scheme was introduced a year later.

These reforms undeniably improved the institutional quality of the Turkish banking sector, which escaped the global financial crisis of 2008-09 unscathed. They also arguably minimised government interference in banking, except via direct ownership. State authorities retain controlling shares in all three deposit-taking state banks – *Ziraatbank, Halkbank,* and *Vakıfbank* –, despite the fact that all three were initially aimed to be privatised as part of the post-crisis restructuring programme. The IMF states explicitly in its 2002 Stand-By Agreement with Turkey that the government should "establish a common and politically independent board for *Ziraat* and *Halk* [...] and appoint new management who will apply commercial criteria to ensure profitability, and who will formulate privatization plans," and "resume privatization process for *Vakıf*" (IMF, 2002). Although both *Halkbank* and *Vakıfbank* eventually floated part of their shares via initial public offerings, the full privatisation of state banks did not materialise in the coming years.[5]

Our sample period starts when these reforms took effect. This constitutes an ideal period to investigate the influence of the central government on state-owned banks, as direct major-

[4]For instance, during the coalition governments of the 1990s, it was common practice to share control of state banks among coalition parties based on their vote shares (Önder and Özyıldırım, 2013).

[5]See IMF (2007): "After many delays, the IPO for 20–25 percent of the government's share in *Halkbank* is underway. Staff urged that the residual government stake be sold within the next year and the privatization of *Ziraat* [...] launched at once. The authorities, however, are reluctant to commit to specific plans, noting that *Ziraat* serves a social function as the only financial institution with branches in rural areas."

ity ownership – which the authorities retained despite earlier commitments to do otherwise – appears to be the only channel through which it can exert pressure on the banking system.

Table 1 shows a snapshot of the banking sector in local election years. The sector shrank in size considerably between 1999 and 2004 following the financial stability programme before resuming growth. State banks became much leaner by shedding branches and personnel, but retain an important role as they typically control around a third of total banking assets. State banks have substantially improved their loan quality and capital buffers, although they continue to operate at a lower ratio of equity to assets. The formation of a uniform supervisory and regulatory system levelled the playing field for private and state banks, as both types of banks converged to similar levels of financial performance.

2.2 Politics and local elections in Turkey

Turkey was a parliamentary democracy with a multi-party political system during our sample period. The Prime Minister, typically leader of the ruling political party, served as the head of central government and exercised executive powers with the Council of Ministers.[6] In general elections, political parties targeted nationwide popularity, as the leader of the party with the most popular vote was given the role to form government. Seats in parliament were determined based on a proportional representation system (the *D'Hondt* or *Jefferson method*), but parties could gain seats only if they obtained at least 10% of national votes.

The current ruling party, AKP (*Adalet ve Kalkınma Partisi*), has been in power since 2002 and retained its parliamentary majority through several general elections thanks to its nationwide popularity. The AKP inherited the IMF-led reforms of 1999-2001 and successfully implemented them, bringing public expenditures under control and strengthening the overall quality of institutions (Acemoglu and Ucer, 2015). As part of its political reform agenda, the AKP increased local governments' administrative and financial autonomy.[7]

Turkey is divided into 81 provinces for administrative purposes, which are further divided into 923 districts. The main local government is the municipality, which delivers a wide range of services. Significant executive powers make the mayor the single most important authority, who presides over a local council that sets municipal budgets. Out of the 81 provinces, 30 are designated as metropolitan municipalities, which are required by law to have at least 750,000 inhabitants and serve all districts within the province. This designation gives a municipality greater autonomy from the central government and political power. Municipal

[6] Turkey switched to an executive presidential system in June 2018, in which the electorate votes for the president alongside members of the parliament. However, the structure of local governments is unaffected.

[7] AKP was established in 2002 as a reformist party rooted in Islamist ideology, whose predecessor movement had first gained power in local governments in the 1990s. Arguably, AKP prioritised reform of local governments to consolidate its dominant position at the time of reforms (Bayraktar and Massicard, 2012).

budgets correspond to 4-5% of GDP, at par with many Western countries (Meyersson, 2014). However, metropolitan municipalities control much larger budgets than non-metropolitan municipalities do thanks to their greater financial autonomy.

In local elections, a mayor and a council are elected based directly on local popular vote (the *first-past-the-post* principle) and no national threshold applies. A metropolitan mayor is elected by the majority of votes cast in that province. In non-metropolitan provinces, local services are delivered by district municipalities and voters only vote for candidates of the district they live in. As a result, the major contest among political parties is to have their candidate elected as the metropolitan mayor in metropolitan provinces, and as the mayor of the central district – which has the largest population – in non-metropolitan provinces.

Local elections are held every five years on the same day throughout the country. Our sample period covers three local elections held in 2004, 2009 and 2014, at the end of March in each case. On the one hand, this means that we cannot exploit time variation across provinces in elections. On the other hand, it removes any bias from endogeneity of election timing, which may arise if early elections are called when the local economy is doing particularly well (Cole, 2009). We focus on political cycles based on local, as opposed to general, elections to identify possible effects on bank lending and economic outcomes for two reasons.[8]

First, as Turkey shifted from coalition governments to a single-party government over the past two decades, local elections have become more instrumental in expanding the power base of the ruling party. Mayors have become more visible in national politics, and some metropolitan mayors have commanded substantial political clout.[9] They represent the local electorate and act as the main local political figure, especially in metropolitan provinces, where nearly 80% of the national population live and 85% (95%) of total lending by state (private) banks is concentrated on average during our sample period.[10] At the same time, local governments – especially metropolitan municipalities – have increasingly controlled larger budgets and become sources of economic rents. They employ a vast number of people and outsource many services to private contractors, which is an indirect way to reward contracts and influence local economic activity.

In addition, the AKP government has rarely faced any competition at national elections

[8]General elections are held in different years from local elections, and frequently called early by the central government opportunistically. There were four national elections in our sample period: July 2007, June 2011, June 2015, and again in November 2015 after the election in June led to a hung parliament.

[9]Most notably, current President Erdoğan served as mayor of Istanbul between 1994 and 1998. Bayraktar and Massicard (2012) argue that the executive authorities of the Islamist metropolitan municipalities served as a training ground for a new generation of national political elites.

[10]The Union of Municipalities of Turkey notes (https://www.tbb.gov.tr/en/local-authorities/municipal-organs/): "The mayor is a politically strong person. [...] The office is so strong that even incumbent parliamentary deputies compete for it; for example, in the 2014 local elections 6 incumbent parliamentary deputies were elected metropolitan mayors."

during our sample period. In contrast, it faced fierce competition in local elections. Figure A.1 in Appendix A shows that the AKP's average vote share in local elections is only slightly higher than that of opposition parties, but they have a commanding lead in general elections. Thus, we expect that any potential reallocation of resources should follow local elections, especially where the AKP faces real competition to win or lose certain provinces.

Second, votes in national elections do not translate directly into seats in parliament, and thereby into political influence over resource transfers. This is due to a high election threshold of 10% at the national level: votes for parties that fail to clear this threshold are redistributed among remaining parties in each province. We believe that such uncertainty regarding the number of parliament seats that can be won at the local level deters the central government from pursuing a regional targeting policy. In contrast, competition in a local election is straightforward to quantify and more visible since winners are determined by popular vote. Hence, the focus on local elections sheds light on tactical reallocation by the central government when it faces a clear competitive threat to win or lose a province.

3 Data

We use four main data sets in our analysis. The first comes from BRSA's FinTürk database, which provides province-level data at the quarterly frequency on both corporate and consumer loans by state and private banks beginning in the fourth quarter of 2007. These data constitute the universe of bank cash and non-cash loans in the country, and include data on non-performing loans (NPLs), bank branches, and deposits by province and bank type. Separately, FinTürk provides a breakdown of corporate lending nationally by bank type and industry of borrower (following NACE Rev. 2) at a monthly frequency since 2005. We use this information to construct credit market shares in each industry by bank type.

Our second data set contains measures of real economic outcomes from establishment-level administrative records. The Turkish Ministry of Industry (MoI) maintains a database that sources data from balance sheets and income statements by all corporates liable to pay tax, capturing the universe of formal activity.[11] For our purposes, we obtain the following variables aggregated at the 2-digit industry (NACE Rev. 2), province, and year level: employment, net sales, total assets and liabilities, short- and long-term bank debt, and total number of establishments underlying these variables. We use this information and a framework we introduce in Section 5.3 to construct measures of local and aggregate productivity;

[11] This database is called *Girişimci Bilgi Sistemi (GBS)* in Turkish. Researchers can submit information requests to the ministry to obtain data aggregated at a higher level than the most detailed level of the establishment. See https://gbs.sanayi.gov.tr/GbsHakkinda.aspx.

we describe our measurement in detail in Appendix A.

Our third data set is the annual firm-level industrial survey conducted by the Turkish Statistical Institute (TurkStat). The industrial survey covers all firms with 20+ employees and a random sample of smaller firms. The survey includes data on investment expenditures, employment, and value added, amongst others. This allows us to calculate firm-level measures of marginal revenue product of capital and its dispersion. We describe these calculations in detail in Appendix A.

Our fourth data set includes local election outcomes. We obtain information on district- and metropolitan-level votes for each political party from TurkStat. Based on these data, we create two political variables. The first is a measure of political contestedness that captures the margin of victory/loss by the ruling-party candidate against the most popular opposition candidate. Formally, we start with a continuous *Competition* variable: $Competition_{p,t} = 1 - |Margin_{p,t}|$, where p stands for province, t indicates the particular election, and *Margin* denotes the difference in the share of votes won by the ruling party's candidate and the most popular opposition candidate. Thus, *Competition* takes values between 0 and 1, with values closer to 1 indicating close electoral competition. To capture province-level competition, we work with the margin in the election of metropolitan mayors in metropolitan areas. For non-metropolitan areas, we use the corresponding value for the central district of the province.

One might worry that electoral contestedness is influenced by the lending behaviour of state-owned banks in the province, which might render *Competition* potentially endogenous. We follow earlier studies in dealing with this issue and define an indicator variable of electoral competition. In particular, we let $Comp_{p,t}$ equal 1 when the variable *Competition* is above its sample median and 0 otherwise. We show below that our results are qualitatively unchanged when using time-invariant measures fixed for each province or alternative definitions.

Our second political variable is a dummy for political alignment (or alliance), which indicates whether the ruling-party candidate wins in that province or not. Recall that voters elect metropolitan mayors in metropolitan provinces, while they elect district mayors in non-metropolitan provinces. However, our credit data are only available at the province level, which means that we need to aggregate voting outcomes to define a province-level measure of alignment. We tackle this problem by concentrating on the metropolitan mayors and, in non-metropolitan provinces, on the central district mayors. This gives us a direct measure of alliance for each province.

However, this procedure is still not ideal for non-metropolitan provinces.[12] If politically induced lending occurs at the level of districts, this may create measurement error and lead to attenuation bias in our estimates. We therefore base our main findings on results from

[12]For instance, Figure A.2 shows how districts may have different political alignment.

metropolitan provinces. Nevertheless, we also report our findings from a full sample that includes non-metropolitan provinces.

Table A.1 in Appendix A presents summary statistics for the main variables in our analysis. During the sample period, just over a half of provinces are classified as politically aligned with the ruling party. There is a fair degree of electoral competition, as the win margin in the median province is 14 percentage points. Figure A.3 shows political competition and mayors' alignment with the governing party for metropolitan provinces in each local election. It shows that electoral contestedness has increased throughout the sample period.

4 Political lending cycles

4.1 Identification strategy

We start with a simple *difference-in-differences* (DD) methodology in a balanced panel setting to investigate political cycles. We use government ownership of banks as our treatment, which captures political influence by the central government over local lending. Our control group includes all privately-owned banks, which are assumed to be fully profit maximising and to adjust their lending flexibly to meet local credit demand.

We search for tactical redistribution by making full use of the time-series and cross-sectional dimensions of our dataset. Formally, we adopt a *triple difference-in-differences* (DDD) model and test whether highly contested provinces get more/less credit from state banks around elections when compared with private banks. We expect political pressure on state-owned banks to *intensify* around election years. If politicians also influence lending by private banks around elections, then our estimates provide a lower bound for the true size of political lending. However, if firms are able to switch costlessly between private and state banks, then our estimates may be magnified.

Our baseline specification has fixed effects for bank type, province and time, which help us capture unobservable, time invariant factors and aggregate shocks. We further include time trends by province and the number of local branches by bank type.[13] These control for any long-term credit demand and supply changes and potential sorting of banks linked to regional unobservables. We include a full set of province×time or bank type×time effects in our DDD regressions to eliminate unobserved province- or bank-specific shocks that may be correlated with election cycles. We cluster standard errors at the province level, since local credit outcomes for both bank types are likely to be correlated across time within localities.

[13] We control for (lagged) branch presence using an ordinal variable by assigning branches into 30 groups, because they might be affected by election cycles (Englmaier and Stowasser, 2017).

Our main identifying assumption is that state-owned and private banks follow similar trends over the election cycle in the absence of electoral considerations and tactical redistribution. We should note that we do not have a "pre-period" typical in DD settings, because we are testing for an election cycle. However, two pieces of evidence suggest that our identifying assumption is likely to hold. First, Figure 1 shows that both state and private banks have steadily increased lending (Panel A), while they achieved similar rates of profitability especially after 2006 (Panel B).[14] Importantly, Panels C-E show similar long-run trends in lending during which state banks increased their market shares across all provinces regardless of political alignment or electoral competition.[15] Second, we conduct placebo tests in Section 4.4 where we randomise election timings, political alignment and competition one at a time. These tests will reveal that state-owned and private banks behave similarly in the absence of political considerations.

4.2 Is there an election cycle in state-bank credit?

We start by testing whether state banks adjust their overall lending behaviour around elections compared with private banks. Consider:

$$Credit_{b,p,t} = \beta_\tau StateBank_b \times Election_{t+\tau} + \delta X_{b,p,t-1} + \theta_b + \gamma_p + \lambda_t + \varepsilon_{b,p,t} \quad (1)$$

where b is an index for bank type, p stands for province, and t denotes year-quarters in the quarterly data. The dependent variable, $Credit_{b,p,t}$, is total cash loans in logs. $StateBank_b$ is a dummy variable indicating state-owned banks. $Election_{t+\tau}$ equals 1 in the quarter that a local election takes place and the preceding three quarters, and 0 otherwise, when $\tau = 0$. With quarterly data, we can pinpoint exactly when state banks alter their lending behaviour. We therefore employ a rolling definition of $Election_{t+\tau}$, where $\tau \in (-10, 10)$ corresponds to the quarters over the five-year election cycle. Our coefficient of interest is β_τ and captures the behaviour of state banks compared with private banks at each point over this cycle.

Table 2 presents our results. We find that state banks reduce lending in the four quarters up to and including elections by 12.2% in the metropolitan sample (column 2) and 5.1% in the full sample (column 6) when compared with private banks on average. Our estimates are stable across alternative specifications and statistically significant. They are also economically large in magnitude, especially in metropolitan provinces where they compare similarly

[14] We show in Figure A.4 in Appendix A that both types of banks display similar trends in their return on equity, net interest margins, tier 2 ratios (capital adequacy), and NPL ratios since 2006.

[15] Exceptions to these trends appear in 2009 and 2016, when private banks cut back on lending due to significant slowdowns in the Turkish economy.

to the sample average for year-on-year credit growth of 17.5% for state banks and 13.0% for private banks (these figures are 15.5% and 14.5%, respectively, for the full sample).

Notice that the inclusion of time fixed effects in Equation (1) prevents the identification of how private banks behave in the run up to an election. We therefore estimate a specification in which we drop time fixed effects but include $Election_{t+\tau}$ (columns 2 and 5). We find that private banks increase their lending by 7.8% (3.2%) in the metropolitan sample (full sample) in the run up to a local election on average. This shows that private banks do not fully compensate for the reduction in lending by state banks. Hence, the total estimated reduction in lending around local elections is 4.2% (s.e.=0.013, p-value=0.003) in metropolitan provinces and 1.8% (s.e.=0.008, p-value=0.023) in the full sample, when compared with non-election periods. These reductions imply a significant slowdown in credit in metropolitan provinces, where typical growth in lending is around 15% over four quarters.

Figure 2 shows the full election cycle. Each plotted coefficient is an estimate of β_τ as τ varies between -10 and +10 and comes from a regression with our baseline controls and province time trends. Hence, estimates for $\tau = 0$ in Panels A and B equal estimates reported in columns (1) and (4), respectively, of Table 2. Lending by state banks hits rock bottom compared with private banks either in the quarter in which elections take place or just before. In metropolitan provinces, state-bank credit hits a trough at -12.9% one quarter before local elections, while it hits a trough at -5.1% in the election quarter in the full sample. This negative effect is estimated with precision in the four quarters leading up to the election and persists for another four to five quarters following it.

4.3 Is there tactical redistribution across provinces?

We now test the existence of political incentives behind the intertemporal reallocation of state-bank credit over the election cycle. Note that redistributing credit is not costless and that the central government's incentive to distort bank policies increases with the marginal utility of receiving additional votes (Englmaier and Stowasser, 2017). Undoubtedly, this marginal utility is highest in closely contested elections. We should therefore find stronger reallocation of credit in provinces with high electoral competition if there is political interference. Consider:

$$\begin{aligned} Credit_{b,p,t} = \beta_\tau Comp_{p,t} \times StateBank_b \times Election_{t+\tau} + \alpha_1 Comp_{p,t} \times StateBank_b \\ + \alpha_2 StateBank_b \times Election_{t+\tau} + \alpha_3 Comp_{p,t} \times Election_{t+\tau} \\ + \alpha_4 Comp_{p,t} + \delta X_{b,p,t-1} + \theta_b + \gamma_p + \lambda_t + \varepsilon_{b,p,t} \quad (2) \end{aligned}$$

where $Comp_{p,t}$ represents the binary competition variable created earlier.[16]

We note that political competition is potentially endogenous in Equation (2). However, this is unlikely to distort our estimates for two reasons. First, it is reasonable to assume that political redistribution of credit would not change election outcomes by such a high margin as to make an election uncompetitive. This does not mean that the central government would not be able to win an election by manipulating credit. It means that any extra lending allocated to a province through state banks would not be able to change the *nature* of the election, making it competitive or uncompetitive. Second, state banks cannot extend or withdraw credit without limit. Expansion of credit is costly in the short term as banks set aside more capital and in the long term as non-performing loans are likely to rise, while a large reduction may have aggregate consequences.[17] Nevertheless, we show in Section 4.4 below that our results are robust to several alternative definitions of political competition.

Our main coefficient of interest in Equation (2) is the triple-interaction effect denoted by β_τ when $\tau = 0$. It captures the difference between state-bank and private-bank lending in the run up to an election in highly contested provinces. The two-way interactions absorb economically important effects and are also of interest. α_1 accounts for the possibility that state-owned banks may differ in their local lending behaviour based on the political attractiveness of a province independent of an election cycle. α_2 captures how state-owned banks adjust their lending relative to private banks in uncontested provinces, while α_3 captures the behaviour of private banks in competitive provinces around elections.

A central government's incentives to redistribute resources across provinces depend not only on political attractiveness, but also on whether the incumbent mayor is a political ally or not. We therefore divide our sample into two subsamples based on current mayoral incumbency and condition our expectations of β_τ on political alliance. If tactical redistribution exists, we expect $\beta_\tau > 0$ in aligned provinces and $\beta_\tau < 0$ in non-aligned provinces.

Table 3 shows estimates of Equation (2) when $\tau = 0$ on a sample of metropolitan provinces, for which our identification strategy is cleanest. We find strong evidence in favour of a tactical redistribution mechanism. On the one hand, state-owned banks increase their lending by 13.2% on average in provinces with high contestedness and an aligned incumbent mayor before an election when compared with private banks. On the other hand, they reduce credit by 8.9% on average in contested provinces but currently under an opposition mayor. These changes are sizable in relation to the sample average of yearly credit growth. Our

[16]Notice that $Comp_{p,t}$ is time-varying and we need to make an assumption on political contestedness for non-election quarters. We assume that competition for the ten quarters after an election is captured by the most recent election outcome, while it is captured by an upcoming election outcome for the ten quarters before an election (Cole, 2009; Englmaier and Stowasser, 2017).

[17]We thank an anonymous referee for this point.

point estimates are mostly unchanged but have less precision when we include the full set of province-time and bank-time fixed effects.

Table 3 reveals further insights. We find that state-owned banks reduce their lending relative to private banks in uncontested provinces regardless of alignment. In these provinces, the relative reduction before an election is estimated to be 14.7% in aligned provinces (Column 1) and 6.1% in non-aligned provinces (Column 5) on average. This explains the average reduction in state bank lending from our DD exercise. Out of the four groups into which provinces are classified by political competition and alignment, state banks cut back on credit in three of them but increase it in contested provinces that are politically aligned. Figure 1 shows why state banks may do so: politically aligned and contested provinces constitute the largest group by lending and economic importance. Hence, support for favoured incumbents in these provinces comes at the expense of all the others. It is also possible that state banks are more aggressive in reducing credit in opposition provinces than increasing it in aligned provinces, as they aim to erode the incumbency advantage of opposition mayors. A rich political science literature documents that incumbent politicians enjoy higher prospects of re-election against external candidates.[18] This incumbency bias may lead the central government to target and hurt opposition mayors' re-election chances in particular.

What are the total effects of local elections on lending? In columns (2) and (6), we drop time fixed effects and include $Election_{t+\tau}$ to capture the behaviour of private banks in uncontested provinces. We find that, relative to non-election periods, private banks reduce lending in an election by 10.5% on average in aligned provinces with high electoral competition, but they increase it by 9.8% in non-competitive provinces. In aligned provinces, the average estimated effect of elections on private bank lending is -0.7% (s.e.=0.041, p-value=0.871), while that on state bank lending is -1.0% (s.e.=0.069, p-value=0.881) relative to private banks.[19] The estimated total effect due to an election is an insignificant -1.7% (s.e.=0.049, p-value=0.727) relative to non-election periods. In non-aligned provinces, the average estimated effect is 8.9% (s.e.=0.024, p-value=0.002) for private banks and -14.8% (s.e.=0.033, p-value=0.000) for state banks relative to private banks, which implies a significant total effect of -5.9% (s.e.=0.026, p-value=0.039). Hence, while overall lending remains unaffected in aligned provinces, it takes a large hit in opposition provinces.

Figure 3 illustrates the presence of tactical reallocation over the full election cycle by plotting estimates of β_τ for $\tau \in (-10, 10)$.[20] There is strong evidence that politically non-aligned provinces suffer from a reduction in lending by state banks before closely contested

[18] See, among many others, Cox and Katz (1996); Gelman and Huang (2008); de Benedictis-Kessner (2018).

[19] We calculate $\hat{\alpha}_3 + \hat{Election}$ for private banks and $\hat{\beta}_\tau + \hat{\alpha}_2$ for state banks.

[20] The exact model used for the estimates shown in the figure includes our baseline controls and province time trends as in Columns (1) and (5) in Table 3.

elections. We see the exact opposite in politically aligned provinces. Targeted redistribution starts around four quarters prior to an election, and is strongest in the immediate run up to it. We report formal tests of differences in coefficients between the two samples in the bottom panel. We reject the equality of coefficients at the 95% level of confidence for $\tau \in (-3, 1)$, confirming an intensified effect before an election.[21]

We believe that this visual representation of state-bank credit reallocation over the election cycle provides strong evidence of political incentives behind state-bank lending. State banks may behave differently around elections, for instance due to uncertainty. However, this implies that state banks should cut back on lending in all contested provinces regardless of political alignment, which is at odds with the evidence. Separately, unobserved shocks to industries in which state banks are more active lenders may correlate with the election cycle. However, such effects should be captured by province-by-time fixed effects, whose inclusion leaves our main estimates unchanged. It is difficult to explain why such cross-sectional relationships vary over time specifically around elections without resorting to an explanation based on political incentives (Cole, 2009).

To shed more light on political incentives, we explore the channels through which the central government engages in tactical redistribution. Our data can be broken down by lending to different segments of the economy, which allows us to test how voters respond to targeted lending. On the one hand, politicians may try to induce a quick and direct impact on voters by raising their instant consumption. On the other hand, politicians may use bank credit to boost or contain corporate activity in a region, especially if voters attribute corporate performance to local politicians and care about their employment prospects.

Figure 4 plots estimates of Equation (2) for corporate and consumer lending in Panels A and B, respectively. We find that tactical redistribution mainly targets corporate loans, while consumer loans show no pattern at all. Before an election, state-owned banks increase corporate lending by 24.1% relative to private banks in aligned and contested provinces, while they reduce it by 18.4% in non-aligned and contested provinces. We reject the equality of coefficients in the two samples for $\tau \in (-3, 1)$. In unreported regressions, we find that the estimated total effect on corporate lending is 0.8% (s.e.=0.076, p-value=0.914) in aligned provinces and -14.4% (s.e.=0.048, p-value=0.008) in non-aligned provinces.

Does tactical redistribution of credit operate through state banks' existing clients or at the margin? As we do not observe the marginal borrower, we adopt an indirect approach and estimate the electoral cycle for NPLs as a share of total cash loans. On the one hand, we

[21] We also note that state banks increase credit relative to private banks in non-aligned provinces in quarters farther away from local elections. This intertemporal reallocation arises because state banks are generally expected to boost credit at the national level near general elections.

would expect post-election default rates to differ between state and private banks if banks grant loans to applicants successively based on credit quality and state banks adjust lending to the marginal borrower before an election. On the other hand, default rates may stay the same if state banks adjust their lending based on firms' political connections and these firms have similar risk profiles as the rest of the population.

Figure A.5 in Appendix A shows that NPL ratios do not differ significantly in aligned provinces over the election cycle, but they are lower for state banks in non-aligned provinces after an election by 2.5 percentage points on average. We can reject the equality of coefficients in the two samples for $\tau = +2$. As the average maturity of loans in Turkey lies between one and two years, this timing in default rates is consistent with state banks cutting back on lending to the marginal borrowers in non-aligned provinces, but increasing it to existing clients in aligned provinces, which are possibly politically connected.

4.4 Tests for identifying assumptions and robustness

We carry out several tests on our identifying assumption and robustness of our results, which are all reported in Appendix B. Our main identifying assumption is that state-owned and private banks would have followed similar trends over the election cycle in the absence of tactical redistribution. This assumption implies that there should be no significant differences in lending between these banks if local elections were held not on a pre-determined five-year cycle, but instead on random dates. It also implies that there should be no discernible differences in lending if either electoral contestedness or political alignment at the province level was random.

We study these implications using placebo tests presented in Figure B.1. We estimate Equation (2) with corporate loans as our dependent variable after randomly generating three local election dates during our sample period (Panel A), electoral competition (Panel B), and political alignment (Panel C), each one at a time and while keeping everything else fixed. Results are statistically indistinguishable from zero on and around randomly generated election dates. We also do not detect any differences in lending around actual elections when either political competition or alignment is randomly generated.

We check the robustness of our main result – that there is tactical redistribution of corporate loans by state banks – to a number of alternative specifications, all of which include our baseline controls and province time trends. First, we re-estimate Equation (2) with alternative definitions of political contestedness. We construct a time-invariant measure to guard against the potential endogeneity of electoral competition by averaging $Comp_{p,t}$ for each province across all local elections. We find that state-owned banks increase corporate

lending in politically aligned provinces, but curb it in non-aligned provinces, both by around 20% compared with private banks just before a competitive election (Figure B.2). Using alternative definitions of the competition dummy all return qualitatively similar results.[22]

Second, our results remain unchanged when we instead control for bank branches in continuous log form (Figure B.7) or customer deposits (Figure B.8) in $X_{b,p,t-1}$.

Third, we confirm that our main results are unchanged when we drop the three largest provinces (Figure B.9), metropolitan provinces in eastern Turkey (Figure B.10), or provinces that changed hands between political parties (Figure B.11).

Fourth, we check our results against a potential bias arising in our standard errors from a low number of clusters. Clustering at the province-by-bank type level doubles our clusters and allows for province-by-bank specific correlation in error terms. This leads to smaller standard errors than our baseline (Figure B.12). We also apply the wild cluster bootstrap-t procedure suggested by Cameron et al. (2008) without specifying a null hypothesis for regressors. The empirical p-values returned from this procedure are lower than our baseline p-values (Table B.1). These checks suggest that our baseline inference is based on conservative estimates.

Finally, we confirm that the political lending cycle holds over a longer time period, 2003-2017, when we estimate Equations (1)-(2) with yearly data. Estimates suggest that state banks curb credit relative to private banks by around 15% in the year before a local election (Table B.2) and increase lending on a larger scale directly afterwards (Figure B.13). A longer period allows us to draw on additional variation due to the 2004 local elections and shows that our results are not driven by any one election during this period (Figure B.15). Tactical redistribution continues to hold in the yearly data (Table B.3) and estimates are slightly higher than our quarterly estimates over the election cycle (Figure B.14).

4.5 Additional mechanisms

We conduct several tests on whether demand-side factors can explain the lending behaviour of state banks. We provide a summary of these tests here and details in Appendix B.

First, we explore the role of political uncertainty, which may affect firms' decisions on investment and borrowing. We construct a Herfindahl index of local political competition to proxy uncertainty for each election. We then run a regression that relates bank lending to the Herfindahl index over the election cycle. Results show no evidence that political uncertainty is associated with a change in lending throughout the election cycle. When we further test

[22]These include defining the dummy as the upper 25% of the continuous variable (Figure B.3), using the 50% cut-off for each election one at a time (Figure B.4), using the continuous competition variable itself (Figure B.5), or measuring competition using the previous election's outcome (Figure B.6).

whether state banks are particularly vulnerable to uncertainty, we find insufficient evidence that uncertainty can explain the observed patterns.

Second, we explore whether the central government allocates public contracts around local elections. If firms receiving these funds use state bank credit relatively more, this can give rise to a lending cycle induced by firm-level demand. We put together two new databases to test this mechanism (see Appendix A). The first contains "investment incentive certificates" issued by the central government, while the second contains permits issued by local municipalities for all new construction projects initiated by the public sector. Our results are mixed. On the one hand, we find some evidence that investment incentive certificates follow an electoral cycle in line with tactical redistribution. However, there is no requirement for recipients to work with state banks rather than private banks. To the extent that this occurs, it can explain part of the variation in the lending cycle. On the other hand, we detect no particular cycle in public construction, which is unlikely to drive the lending cycle.

5 How does political lending affect economic outcomes?

5.1 Evidence from administrative data

To identify the real effects of politically induced lending, we draw on a new administrative dataset (MoI's *GBS*) for the period 2006-2016. Our baseline estimates are based on manufacturing industries, for which previous research documented a strong correlation between employment growth and votes for the incumbent party (Bertrand et al., 2018).[23] We show below that our results extend to other sectors of the economy.

Consider the following DDD model:

$$\begin{aligned} Outcome_{i,p,t} = \beta_\tau Comp_{p,t} \times StateBankShare_i \times Election_{t+\tau} \\ + \alpha_1 Comp_{p,t} \times StateBankShare_i + \alpha_2 StateBankShare_i \times Election_{t+\tau} \\ + \alpha_3 Comp_{p,t} \times Election_{t+\tau} + \alpha_4 Comp_{p,t} + \theta_i + \gamma_p + \lambda_t + \varepsilon_{i,p,t} \end{aligned} \quad (3)$$

where $Outcome_{i,p,t}$ is an economic outcome for industry i, province p, and time t in logs. $StateBankShare_i$ measures the share of state banks in total lending by industry nationally. We construct $StateBankShare_i$ as an industry-level measure that does not vary with time and measure it as of the fourth quarter of 2005 to prevent possible reverse causality. We control for our baseline set of fixed effects and province time trends.

[23] We document this correlation for Turkey in Appendix C. We estimate that a 10% growth in a province's manufacturing employment in the year prior to a local election is associated with up to a 5.1 percentage points increase in the vote share of an incumbent party (Table C.1).

The main coefficient of interest, β_τ, captures how economic outcomes in industries with an initially higher share of state bank lending and located in politically contested provinces move with the election cycle. If the cycle affects real outcomes, then contested provinces with an opposition mayor are expected to suffer lower economic activity, while those with a politically aligned mayor are expected to see a boost. Moreover, if the real effects are indeed driven by politically induced lending, then they should be strongest in industries where state banks play a bigger role relative to private banks.

This research design exploits the heterogeneity in industries' exposure to state banks under the assumption that firms can only imperfectly substitute for a change in credit supply from their main bank.[24] It implies that politically induced lending affects outcomes to the extent that firms' aggregate financial constraints are tightened or relaxed. If firms are able to perfectly switch between state and private banks due to competitive spillovers, then firms in opposition areas need not experience financial constraints.

This is an assumption we can test directly in the data. We estimate Equation (3) with (log) total bank debt as our first dependent variable. Table 4 shows estimates when $\tau = 0$ in Columns (1) and (5) and Figure 5 shows the full election cycle in Panel A.[25] Before an election, industries with a higher share of state bank lending in politically contested provinces experience a bigger slowdown in total borrowing if the incumbent mayor is from an opposition party. We find the opposite in aligned provinces, but these estimates are not statistically significant.

Figure C.1 in Appendix C provides average marginal effects for $Election_t$.[26] In contested and non-aligned provinces, total borrowing in an industry with a 20% (vs. 10%) initial state bank share is lower by 2.9% (vs. higher by 9.7%) in an election year when compared with non-election years. In contrast, total borrowing in an industry with a 20% (vs. 10%) initial state share bank rises by 21.6% (vs. 19.5%) in contested and aligned provinces due to local elections. Figure C.1 also shows the total estimated effects by alignment. For an industry with a 20% initial market share of state banks, the change in total industry borrowing in an election year is estimated to be 3.1% in non-aligned provinces but 10.8% in aligned provinces. The sample average growth of total borrowing is 20.2% in our data. Hence, there is strong evidence that aggregate financial constraints are tightened in non-aligned provinces, especially where political competition is high.

[24] A rich literature documents the stickiness of firm-bank relationships and how firms' access to credit suffers in the face of shocks to their relationship lenders, especially in the case of small business lending. See, for instance, Greenstone et al. (2014) and references therein.

[25] We show in Table C.2 of Appendix C that our estimates remain similar across specifications with different sets of fixed effects.

[26] We calculate average marginal and total effects based on a specification that includes $Election_t$ and drops time dummies. See Columns (2) and (6) in Table C.2 for the underlying coefficients.

Data allow us to dig deeper into how corporate borrowing is affected by the political lending cycle. First, we provide a further check on how aggregate constraints diverge by alignment. Table C.3 in Appendix C reports estimates when the outcome is corporates' total liabilities excluding bank debt, which primarily captures credit from suppliers and shareholder loans. We do not find any evidence that corporates in opposition provinces are able to substitute their bank debt with other creditors, while those in contested and aligned provinces increase their non-bank borrowing.

Second, we present estimates when the outcome of interest is short-term or long-term bank debt (Figure C.2). We observe a similar cycle in both types of borrowing, although estimates are statistically significant only for long-term bank debt. In Turkey, loans with longer maturities are typically used for capital investment purposes, while those with shorter maturities are used for working capital purposes. We therefore expect economic outcomes to be affected mainly through an investment channel, but we do not rule out other channels.

Table 4 shows estimates of Equation (3) when our outcome variable is total assets (Columns 2 and 6), employment (Columns 3 and 7), or net sales (Columns 4 and 8).[27] We observe a consistent pattern in all of these outcomes. In the run up to local elections, industries that are more dependent on state bank lending and located in contested opposition provinces suffer a significant and negative impact on their assets, employment, and sales. In contrast, the same industries located in politically contested and aligned provinces experience a statistically insignificant increase in these economic outomes.

We report average marginal and total effects for $Election_t$ in Figures C.3-C.5 of Appendix C. According to our estimates, total assets in an industry with a 20% initial state bank share is lower by 10.4% on average in contested and non-aligned provinces due to local elections. In contrast, they are up by 11.5% in contested and aligned provinces when compared with non-election years. This discrepancy remains, but is smaller in size, when we also take into account changes in non-contested provinces. An industry with a 20% initial state bank share located in aligned provinces sees its total assets rise by 0.5% on average, but the same industry in non-aligned provinces sees total assets decline by 6.3% in an election year.

We observe a similar picture when looking at how local elections impact aggregate employment and sales. We find that, in contested and non-aligned provinces, an industry with a 20% initial state bank share suffers an average reduction of 5.3% in employment and 9.6% in net sales in an election year relative to non-election years. However, in contested and aligned provinces, the same industry sees its employment rise by 3.5% and its net sales by 1.3% due to local elections. In the data, the average year-on-year growth rate is 17.5% for

[27] Tables C.5-C.6 in Appendix C show that our estimates remain similar across specifications with different sets of fixed effects.

total assets, 6.7% for employment and 14.9% for net sales. In relation to these figures, local elections induce a significant distortion to the allocation of corporate resources.

Figure 5 shows the full election cycle for total assets in Panel B. The negative impact on total assets in contested and non-aligned provinces is already significant one year ahead of a local election. We also find that the impact on assets in these provinces is reversed in the two years following local elections, although these estimates are much smaller in size. There is a similar story in Panels C and D, which show the full election cycle for employment and net sales, respectively. Contested opposition provinces start seeing a decline in employment a year ahead of an election in industries with greater state bank lending. At the same time, the negative impact of local elections on both employment and sales in these provinces are reversed away from election years; but these estimates are again very small.

We report additional results in Appendix C. Figure C.6 replicates the analysis with the number of enterprises as an outcome, which shows that firm entry and exit dynamics are unaffected by the electoral cycle. Figure C.7 extends our results to all sectors of the economy, while Figure C.8 extends them to the full sample of provinces.

5.2 Evidence on efficiency of credit allocation

In this sub-section, we ask whether the electoral cycle distorts the efficient allocation of financial resources in Turkey. Consider the following model:

$$\Delta Credit_{i,p,t} = \beta_\tau Efficiency_{i,p} \times Election_{t+\tau} + \alpha_1 Efficiency_{i,p} + \theta_i + \gamma_p + \lambda_t + \varepsilon_{i,p,t} \quad (4)$$

where $\Delta Credit_{i,p,t}$ captures the year-on-year change in (log) total bank credit of all firms located in province p and operating in industry i.

We use two alternative proxies for province-industry efficiency. First, we define the asset turnover rate, which captures the additional sales an industry uses its capital to generate. For each industry-province pair, we define $Efficiency_{i,p} = Net\,Sales_{i,p}/Total\,Assets_{i,p}$ using the beginning-of-sample information from 2006. This ensures that our efficiency measure is independent of future credit trends in the sample period. Second, we define $MRPK_{i,p} = NetSales_{i,p}/RealFixedAssets_{i,p}$. The coefficient β identifies how credit growth at the industry-province level varies with initial productivity over the election cycle.

Table 5 shows results from this exercise across different specifications when $\tau = 0$. Columns (1)-(3) indicate that industry-province pairs with greater initial efficiency experience greater credit growth during the sample period, unless there are upcoming local elections. Industry-province pairs that are initially more productive experience lower credit growth in election years. This result is more precisely estimated with a full set of province-

time and industry-time fixed effects. For instance, a 1% increase in asset turnover (MRPK) is associated with 2.7 (5.5) percentage points higher credit growth in non-election years, but 6.9 (7.5) percentage points lower credit growth in election years on average (Column 3). These numbers suggest that the political lending cycle identified earlier leads to a considerable misallocation of aggregate bank credit.

We replicate this exercise for the sample of politically aligned provinces in Columns (4)-(6) and non-aligned provinces in Columns (7)-(9). We find similar patterns in both samples, but estimates are greater in magnitude and more precise for non-aligned provinces. Politically induced financial misallocation appears stronger in these regions, where it likely distorts aggregate efficiency more than in aligned provinces. It is possible that such misallocation is not concentrated around local elections in aligned provinces, if state banks favour these regions in non-election years due to constant patronage of government strongholds.

We extend our estimates of Equation (4) to the full election cycle in Figure C.9. We do not find evidence for misallocation of credit in any of the non-election years. Instead, we find that misallocation during election years is especially pronounced for non-aligned provinces. In light of earlier findings, this suggests that province-industry pairs that are initially more efficient are also those experiencing the largest increases in aggregate credit constraints in opposition regions.

5.3 Aggregate misallocation

A natural implication of our earlier results is that capital and labour are misallocated across provinces and industries due to a political lending cycle. In this sub-section, we measure aggregate productivity losses from this misallocation and decompose them into factor market distortions within provinces (across industries) and between them. To do so, we build a simple framework in the spirit of Brandt et al. (2013). We summarise the framework here and provide details in Appendix A.

Consider an economy with m provinces indexed by $p = 1, ..., m$, and I industries indexed by $i = 1, ..., I$. We assume a Cobb-Douglas production technology with the same factor elasticities in all provinces and industries:

$$Y_{pi} = A_{pi} L_{pi}^{\alpha} K_{pi}^{1-\alpha} \tag{5}$$

where Y_{pi}, A_{pi}, L_{pi}, K_{pi} indicate real GDP, TFP, employment, and real capital stock, respectively, in province p and industry i. Assume that GDP in a province is a CES aggregate of goods from each industry i, and aggregate GDP is a CES aggregate of province-level output:

$$Y_p = \left(\sum_{i=1}^{I} Y_{pi}^{1-\phi} \right)^{\frac{1}{1-\phi}} \; ; \; Y = \left(\sum_{p=1}^{m} \omega_p Y_p^{1-\sigma} \right)^{\frac{1}{1-\sigma}} \tag{6}$$

where ϕ^{-1} and σ^{-1} indicate the elasticities of substitution between industries and provinces, respectively, and ω_p is province p's weight in aggregate output.

We take total employment and capital stock as given and focus on their allocation. Let $L_p = \sum_{i=1}^{I} L_{pi}$ and $K_p = \sum_{i=1}^{I} K_{pi}$ denote employment and capital in province p and $L = \sum_{p=1}^{m} L_p$ and $K = \sum_{p=1}^{m} K_p$ denote aggregate employment and capital. Define $l_{i|p} = L_{pi}/L_p$, $k_{i|p} = K_{pi}/K_p$, $l_p = L_p/L$, and $k_p = K_p/K$ as the shares of employment and capital. For given TFP in each province-industry pair, A_{pi}, we can calculate province-level and aggregate TFP as:

$$A_p = \left[\sum_{i=1}^{I} \left(A_{pi} l_{i|p}^{\alpha} k_{i|p}^{1-\alpha} \right)^{1-\phi} \right]^{\frac{1}{1-\phi}} \; ; \; A = \left[\sum_{p=1}^{m} \omega_p \left(A_p l_p^{\alpha} k_p^{1-\alpha} \right)^{1-\sigma} \right]^{\frac{1}{1-\sigma}}$$

The allocation of employment and capital $\{l_{i|p}, k_{i|p}, l_p, k_p\}$ that maximises aggregate TFP (or output) is called the efficient allocation. For any given L and K, it is given by:[28]

$$l_{i|p} = k_{i|p} = \left(\frac{A_{pi}}{A_p^*} \right)^{\frac{1-\phi}{\phi}} \; ; \; l_p = k_p = \frac{\omega_p^{\frac{1}{\sigma}} \left(A_p^* \right)^{\frac{1-\sigma}{\sigma}}}{\sum_{p=1}^{m} \omega_p^{\frac{1}{\sigma}} \left(A_p^* \right)^{\frac{1-\sigma}{\sigma}}}$$

$$A_p^* = \left(\sum_{i=1}^{I} A_{pi}^{\frac{1-\phi}{\phi}} \right)^{\frac{\phi}{1-\phi}} \; ; \; A^* = \left[\sum_{p=1}^{m} \omega_p^{\frac{1}{\sigma}} \left(A_p^* \right)^{\frac{1-\sigma}{\sigma}} \right]^{\frac{\sigma}{1-\sigma}}$$

where * indicates the efficient level. Hence, A_p^* and A^* are province-level and aggregate TFP in the absence of distortions.

The efficient allocation means that labour and capital should be allocated to province-industry pairs in proportion to their relative productivities. In the presence of factor market distortions, actual TFP deviates from efficient TFP, both at the province level and in the aggregate. We define TFP losses due to distortions at the province level as $D_p = log(A_p^*/A_p)$, and in the aggregate as $D = log(A^*/A)$.

Tactical redistribution implies that opposition provinces that are politically contested in local elections are punished and industries with greater dependence on state banks suffer especially more. We therefore consider a single distortion to the economy, which is a province-

[28]The efficient allocation is obtained by solving the social planner's problem of maximising aggregate output s.t. (5)-(6) and market clearing conditions. See Appendix A.

by-industry specific capital wedge.[29] The representative firm that operates in province p and industry i solves the profit maximisation problem:

$$\underset{K_{pi},\, L_{pi}}{max} \; P_{pi} A_{pi} L_{pi}^{\alpha} K_{pi}^{1-\alpha} - wL_{pi} - \tau_{pi}^{k} r K_{pi}$$

where w denotes the wage, r the rental price of capital, and τ_{pi}^{k} is the province-by-industry specific capital wedge. The first-order condition to this problem relative to capital means that we can identify the capital wedge, which is proportional to MRPK, up to a scalar: $\tau_{pi}^{k} \propto \frac{P_{pi} Y_{pi}}{K_{pi}}$.

This framework helps us quantify TFP losses in the economy based on observable changes in factor shares across province-industry pairs. When there are no distortions to capital, the allocation of production factors follows relative productivities and implies no dispersion across MRPK's or factor shares. However, when τ_{pi}^{k} vary across province-industry pairs, we observe greater dispersion in factor shares both across industries within a province and across provinces in aggregate. This dispersion means that relatively more (less) productive province-industry pairs employ less (more) capital and labour than the levels implied by the efficient allocation. This leads to a reduction in potential output given total endowments of capital and labour, which we refer to as a TFP loss.

In our empirical implementation, we assume that the technology parameter is best measured in a relatively undistorted economy and set $\alpha = 0.67$, which corresponds to the average labour share in the US (Hsieh and Klenow, 2009). We do not use the labour share observed in Turkey, since it likely deviates from the true output elasticity of labour due to factor market distortions.[30] We set both ϕ and σ equal to 0.67, which implies an elasticity of substitution of 1.5 both across industries within a province and across provinces. We choose ω_p so that the first order condition to the social planner's problem holds on average over our sample period and hence it is time-invariant.[31]

We can now document the impact of factor misallocation on aggregate TFP. Figure 6 plots our measure of aggregate distortions, D, in Panel A and shows that there is a persistent, but declining level of factor market distortions during our sample period. Actual

[29] To simplify the discussion and exposition, we do not consider the other two distortions – on output and labour – that are typically discussed in studies of misallocation. We can do so, because factor allocations would be unaffected by any proportional change in wedges (capital, labour or output) common across all province-industry pairs.

[30] The average labour share at the industry-province level in our dataset is 0.62. This implies a slightly higher output elasticity of capital than what we use in our exercise, meaning that capital misallocation across industries becomes more important for aggregate distortions. In unreported analysis, we confirm that our results are little changed when $\alpha = 0.62$.

[31] We conduct our analysis on metropolitan provinces. Replicating this analysis on all provinces returns very similar results, which are available upon request.

TFP is around 25% lower than efficient TFP on average. Panel A decomposes the aggregate distortion into its between-province and within-province components.[32] We find that the within-province component explains around two thirds of aggregate distortions during this period, while the between-province component accounts for around one third. Eliminating within-province distortions only would bring actual TFP to around 8% of efficient TFP, which would constitute a huge boost to aggregate output.

We can further disaggregate the within- and between-province components into the contributions of labour and capital market distortions.[33] Panels B and C show that capital market distortions explain the vast majority of how within- and between-province distortions contribute to aggregate distortions. For instance, capital market distortions across industries within provinces account for just over a half of aggregate TFP losses, while capital misallocation between provinces account for an additional fifths of aggregate distortions on average. Labour market distortions within- and between-provinces together explain around a quarter of aggregate TFP losses.

Panel D depicts the average of province-level efficient TFP, A_p^*, by political alignment. With the exception of 2010-2013, efficient TFP levels are higher in non-aligned provinces than in aligned provinces, by 4.9% on average over the period 2006-2016. This implies that, in the absence of distortions, more labour and capital would be allocated to non-aligned provinces. There is also a significant difference in the level of within-province distortions, D_p, between politically aligned vs. non-aligned provinces as shown in Panel E. On average, actual TFP is 13.1% lower than efficient TFP in aligned provinces, but it is 19.4% lower than efficient TFP in non-aligned provinces. This points to persistent misallocation of production factors across industries within opposition provinces. It also means that more efficient industries in these provinces operate with a lower-than-efficient allocation of labour and capital.

In light of these findings, we test for the presence of an electoral cycle in within-province distortions. Consider the simple model:

$$D_{p,t} = \beta_\tau Election_{t+\tau} + \gamma_p + \lambda \times t + \varepsilon_{p,t} \tag{7}$$

where $D_{p,t}$ is the within-province distortion in year t, and we include province fixed effects and a linear time trend. To quantify the impact of how a change in D_p due to an electoral

[32] We define between-province distortions as $D_b = log(A^*/A_b)$, where A_b is measured TFP in the absence of within-province wedges (i.e. when $l_{i|p}$ and $k_{i|p}$ follow the efficient allocation). Likewise, we define within-province distortions as $D_w = log(A^*/A_w)$, where A_w is measured TFP in the absence of between-province wedges (i.e. when l_p and k_p follow the efficient allocation). $D - D_b$ gives us the contribution of between-distortions and $D - D_w$ does so for within- distortions.

[33] For within-province distortions, we define A_{wl} (A_{wk}) as measured TFP when $k_{i|p}$ ($l_{i|p}$) follows the efficient allocation but $l_{i|p}$ ($k_{i|p}$) does not and calculate $log(A_{wl}/A)$. We do likewise for between-province distortions, but working with each province's share of labour and capital instead.

cycle affects the aggregate TFP loss, D, we weight observations by province weights, ω_p.

Figure 7 shows the full electoral cycle for province-level TFP distortions in the left panel. We find that, on average, actual province-level TFP is a statistically significant 1.9% lower than its efficient level in election years when compared with off-election years. In other words, the average within-province distortion over our sample period, which equals 15.4%, increase by 1.9 percentage points due to local elections. Since within-province distortions account for two-thirds of aggregate distortions, our estimate implies that the electoral cycle accounts for just under 10% of aggregate distortions (or TFP losses) through its effect on within-province misallocation.

We further test for an electoral cycle in the sources of within-province misallocation. For each province and year, we calculate the correlation between $k_{i|p}$ and $\left(A_{pi}/A_p^*\right)^{(1-\phi)/\phi}$ and likewise for $l_{i|p}$.[34] Figure 7 shows estimates of Equation (7) when the dependent variable is the correlation between factor shares and TFP shares. In election years, we find that the correlation for capital shares drops by 0.01, and that for labour shares drops by 0.024. These correspond to 7.6% (11.5%) of the sample standard deviation of correlation for capital (labour). We find that labour market distortions begin a year earlier, while they seem to contribute positively to efficient allocation in years further away from elections. These trends are consistent with our earlier findings on distortions to total assets and employment in the run up to and after elections.

5.4 Misallocation across firms

The misallocation literature typically studies the dispersion of marginal revenue products across production units to quantify economy-wide distortions, but it remains silent on the sources of these distortions. We provide a reduced-form test of whether tactical redistribution leads to misallocation of capital across firms within province-industry pairs using firm-level data. Our data source for this exercise is TurkStat's Annual Industrial and Services Statistics, which we describe in more detail in Appendix A. We use these confidential firm-level data to construct two variables for each province-industry pair by year: (i) dispersion of (log) MRPK across firms, and (ii) the share of firms carrying out physical investments. We conduct our analysis for the sample of metropolitan provinces as the survey's coverage of province-industry pairs in non-metropolitan provinces is sparse.

Table 6 shows the results when we estimate Equation (3) with the standard deviation of (log) MRPK as our dependent variable. We find that MRPK dispersion across firms tends to increase relatively more in election years in politically contested and non-aligned provinces

[34] In the absence of factor market distortions, both correlations should be 1. However, in the data these correlations are 0.87 for capital and 0.79 for labour.

when industries rely more on state bank lending. An industry with a 20% initial state bank share sees its MRPK dispersion rise by 4.5% more in an election year when compared with an industry with a 10% initial state bank share in the same province. In contrast, MRPK dispersion across firms is unaffected in aligned provinces. Figure 8 shows the full electoral cycle, which does not reveal any changes to the dispersion of MRPK in off-election years. These findings suggest that financial constraints imposed on opposition provinces mainly affect firms that use capital more productively than other firms.

We do not find any meaningful effect of tactical redistribution when the dependent variable is the share of firms carrying out physical investments in either aligned or non-aligned provinces (see Table C.7 in Appendix C). This finding can be due to higher political uncertainty in election years, especially for provinces with an incumbent mayor from the opposition. We find strong evidence that a greater share of firms in politically contested and non-aligned provinces invest in physical capital during non-election years if they operate in an industry more dependent on state-bank lending. For instance, in an average non-election year, the share of firms investing in these provinces is a statistically significant 3.2 percentage points higher than in an election year for an industry with a 20% initial state bank share. In light of earlier findings, these results suggest that local elections affect misallocation of capital less so across firms within province-industry pairs, but more so across industries within provinces and between provinces.

6 Conclusion

In this paper, we document politically motivated distribution of state bank lending around local elections in Turkey. This lending cycle is particularly salient in corporate loans and targeted at politically contested provinces based on incumbent mayors' affiliation. In aggregate, lending by state banks is lower prior to elections compared with private banks, which constitutes a first piece of evidence that political involvement in banks leads to a drop in access to credit. High frequency data allow us to differentiate between pre-election tactical redistribution and post-election rewards or punishment mechanisms. Our findings strongly support theories of tactical redistribution to manipulate voters for re-election prospects. An important implication is that low frequency data may not be optimal to explain mechanisms underlying electoral cycles in bank lending.

It is crucial to understand the distributive implications of political lending to inform policies about circumscribing the latitude of governments to intervene in the economy (Cole, 2009). Our findings imply that tactical redistribution is not simply a minor cost of the democractic process, but it can lead to substantial misallocation of resources. Aggregate

credit constraints induced by the political lending cycle distort the efficient allocation of production factors and reduce aggregate productivity. We find that capital market imperfections explain the vast majority of aggregate productivity losses and present evidence that local election cycles can partly explain them. Future research can explore how such distortions in financial markets affect long-run productivity and the policies to minimise these distortions.

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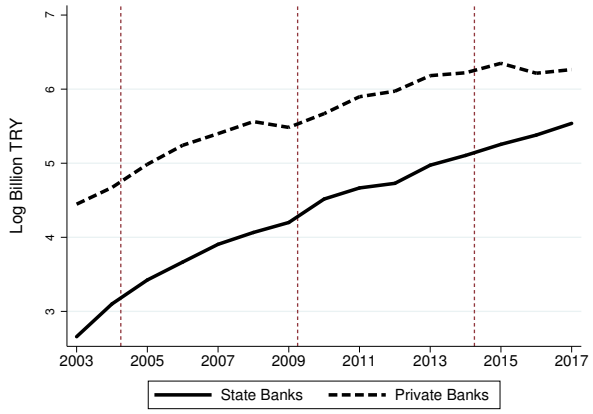
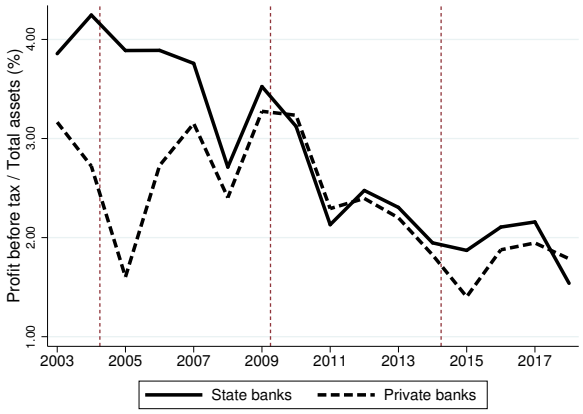
Figures and Tables

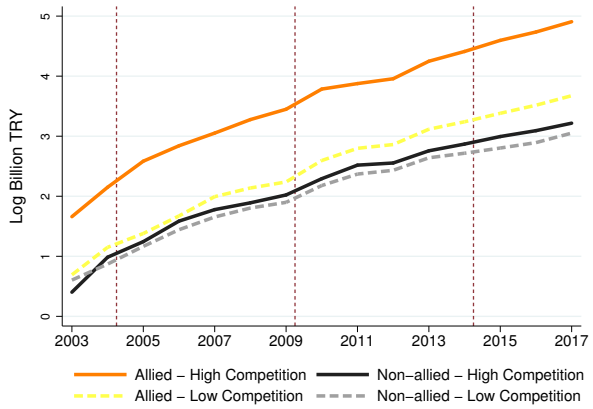
Figure 1: Aggregate credit and financial performance by bank type



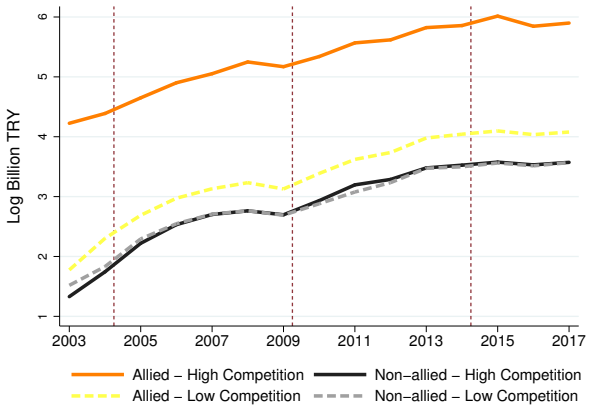
(a) Aggregate credit



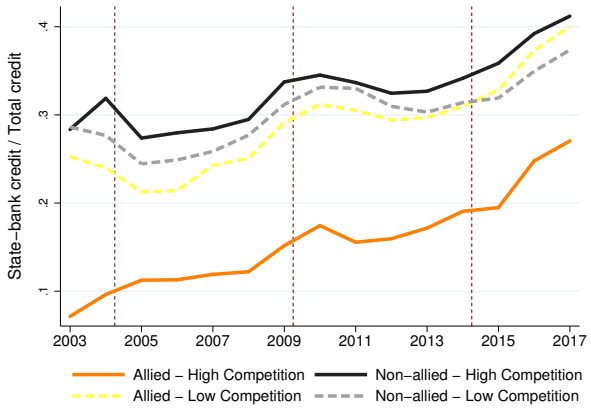
(b) Return on assets



(c) State bank lending



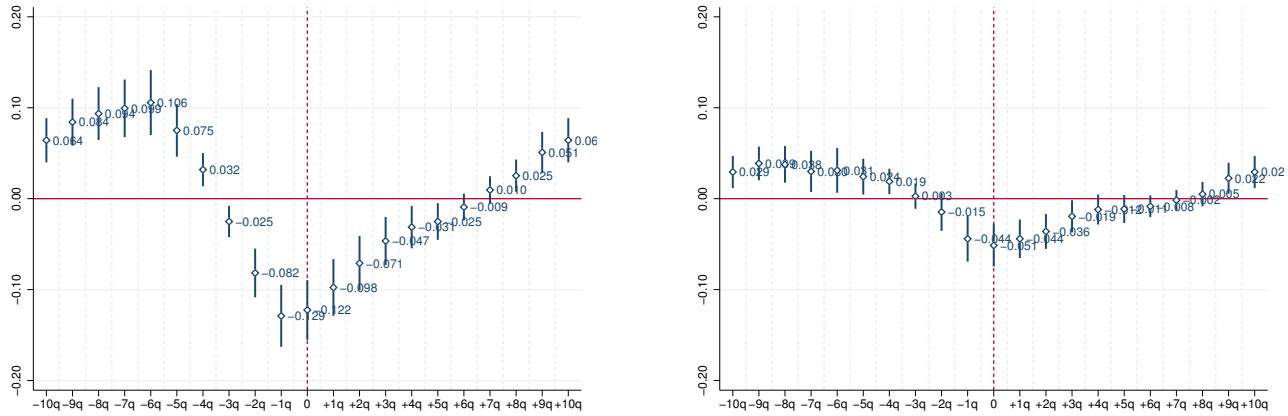
(d) Private bank lending



(e) Market share of state banks

Notes: Sample includes metropolitan provinces in panels (c)-(e).

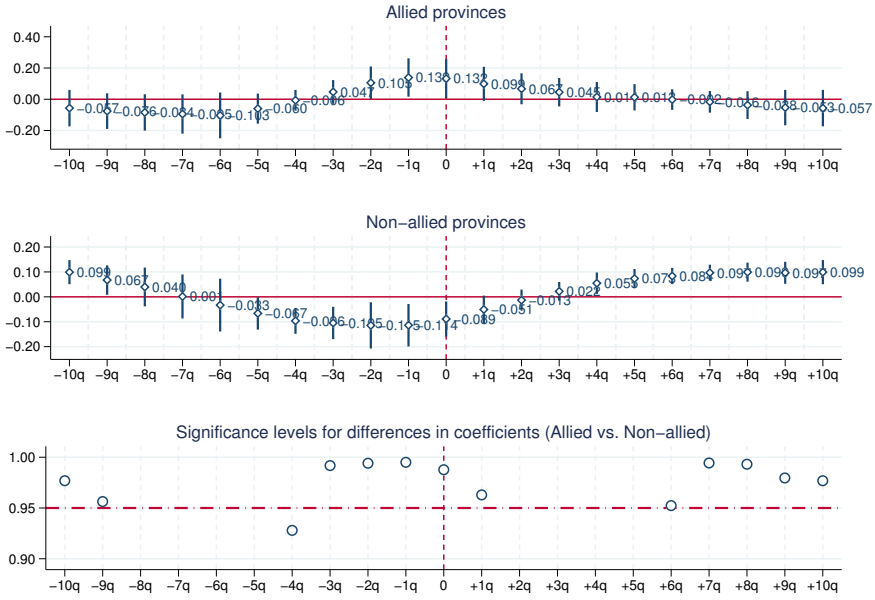
Figure 2: State bank lending relative to private banks over the election cycle



(a) Metropolitan provinces (b) All provinces

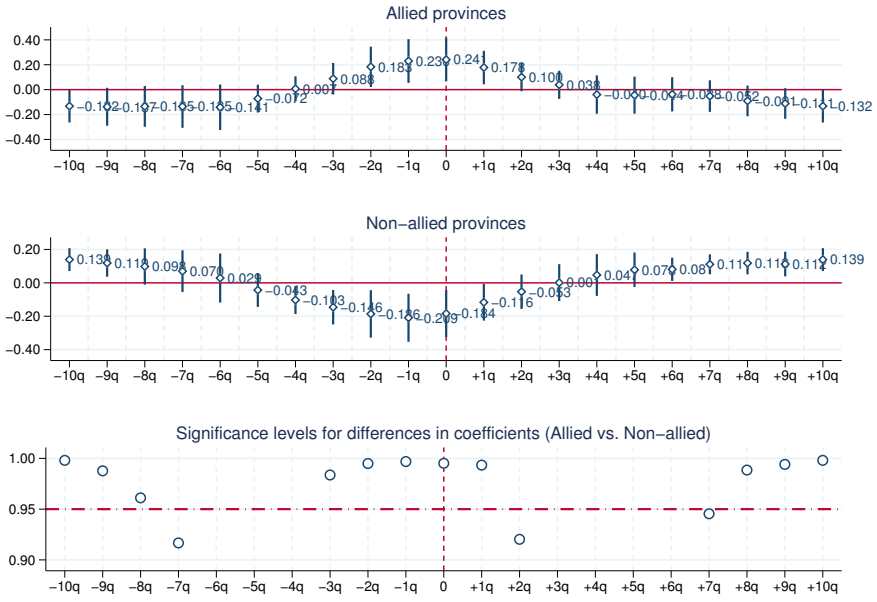
Notes: This figure shows results of Equation (1) estimated on quarterly data. Each coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends. Panel A includes metropolitan provinces and panel B includes the full sample.

Figure 3: Tactical redistribution of state bank lending over the election cycle

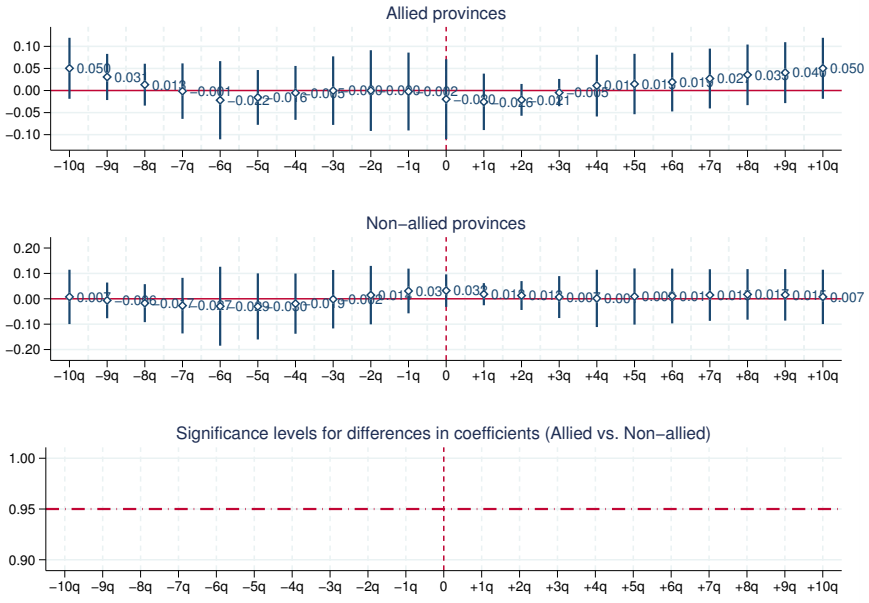


Notes: This figure shows results of Equation (2) estimated on quarterly data. Sample includes metropolitan provinces. Each coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure 4: Tactical redistribution of corporate vs. consumer loans



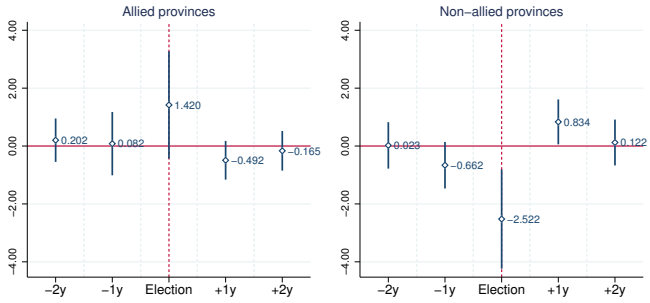
(a) Corporate loans



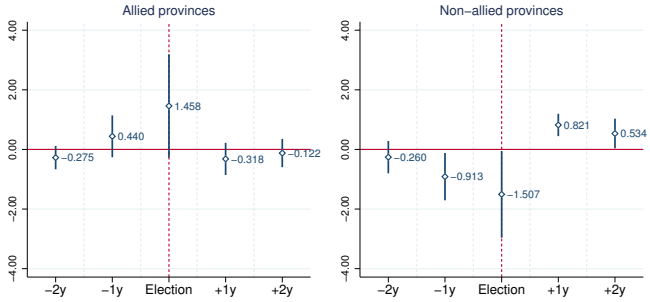
(b) Consumer loans

Notes: This figure shows results of Equation (2) estimated on quarterly data. Sample includes metropolitan provinces. Each coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

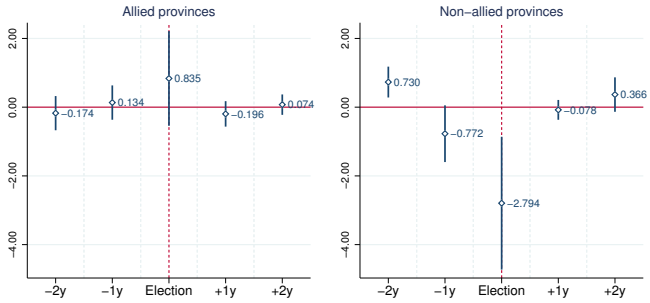
Figure 5: Effects of political lending on corporate outcomes



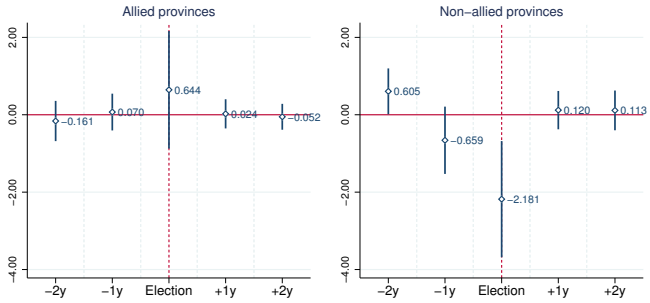
(a) Bank debt



(b) Total assets



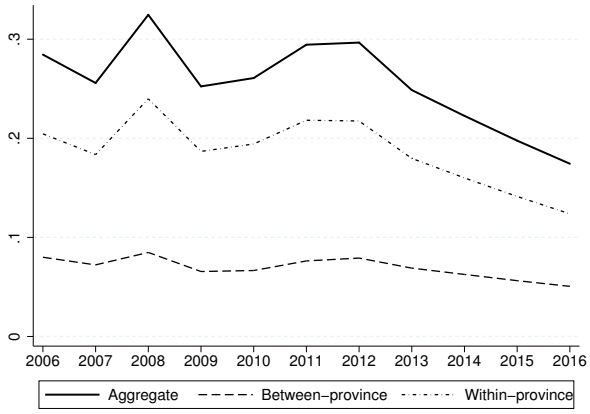
(c) Employment



(d) Net sales

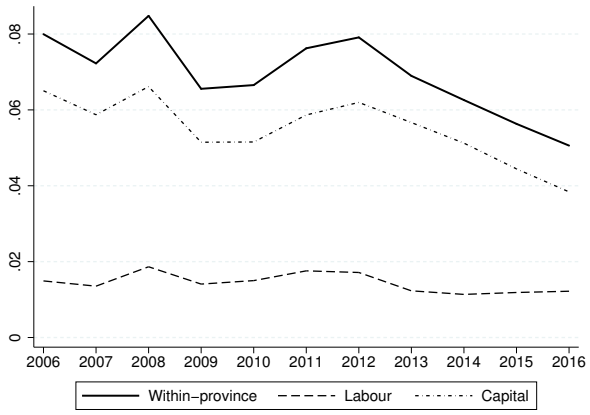
Notes: This figure shows results of Equation (3) for manufacturing industries in metropolitan provinces. Each coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Equality of coefficients between allied vs. non-allied provinces is rejected for Election and +1y in Panel A; for -1y, Election and +1y in Panel B; for -2y and Election in Panel C; and for Election in Panel D at the 90% confidence level.

Figure 6: Aggregate TFP distortions and efficient TFP by alignment

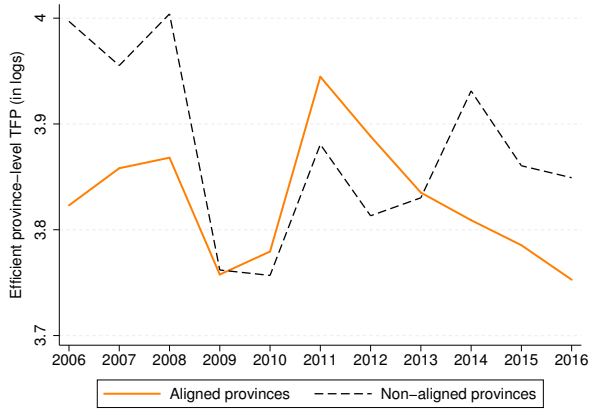


(a) Aggregate TFP distortions over time

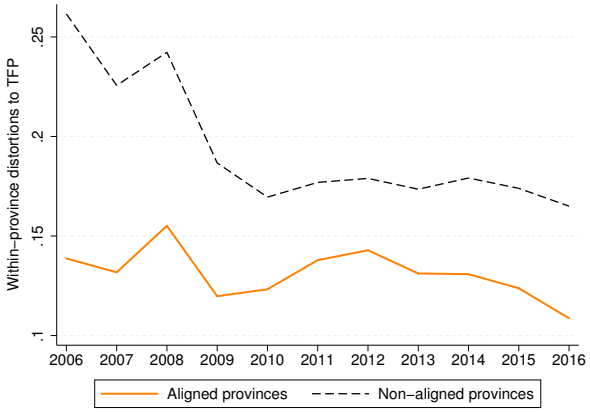
(b) Within-province distortions



(c) Between-province distortions



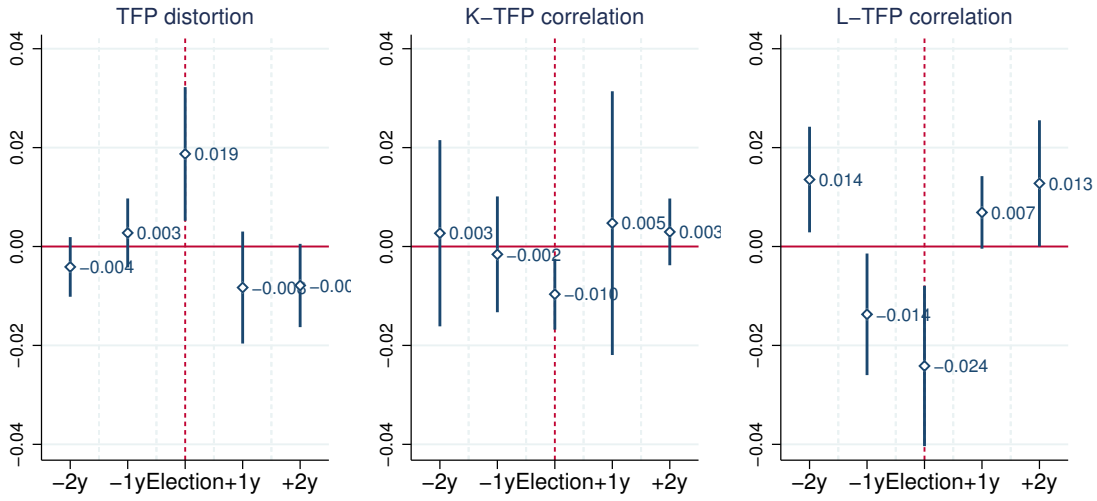
(d) Efficient TFP by alignment



(e) Deviation from efficient TFP

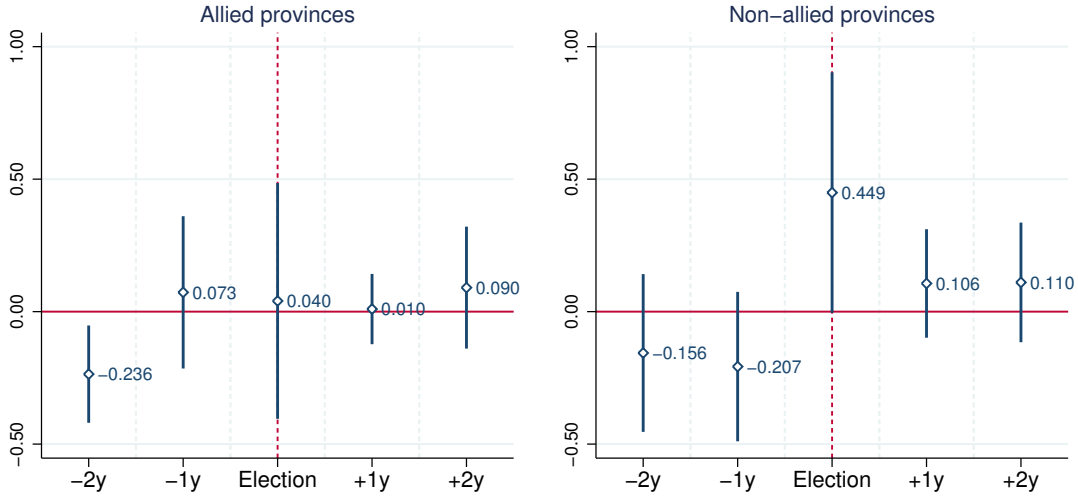
Notes: Panel A shows aggregate TFP losses and their decomposition into within-province and between-province distortions. Panels B and C decompose these latter distortions into those driven by capital and labour misallocation. Panel D shows the average model-implied efficient province-level TFP and Panel E shows average deviation of actual TFP from the efficient level. Sample includes manufacturing industries in metropolitan provinces.

Figure 7: Electoral cycle in province-level TFP distortions and allocation



Notes: This figure shows results of Equation (7). Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for province fixed effects and a linear time trend. Sample includes metropolitan provinces.

Figure 8: MRPK dispersion across firms within industry-province pairs



Notes: This figure shows results of Equation (3) for all industries in metropolitan provinces. The dependent variable is the standard deviation of (log) MRPK within each province-industry pair by year. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for our baseline set of fixed effects and province time trends.

Table 1: Snapshot of banks by ownership in local election years

		1999	2004	2009	2014
Panel A: Composition					
Number of banks	State	4	3	3	3
	Private	50	31	28	30
Number of branches	State	2,865	2,149	2,530	3,500
	Private	4,081	3,938	6,452	7,681
Panel B: Performance					
NPLs / Loans	State	10.0%	11.1%	4.5%	3.0%
	Private	3.6%	4.9%	6.0%	2.8%
Return on Assets	State	1.1%	2.5%	2.6%	1.4%
	Private	4.5%	1.6%	2.3%	1.2%
Equity / Assets	State	4.1%	9.4%	9.4%	10.7%
	Private	12.9%	15.8%	13.4%	11.3%

Table 2: State bank lending in the run up to local elections

	Metropolitan sample			*Full sample*		
	(1)	(2)	(3)	(4)	(5)	(6)
State Bank x Election	-0.122***	-0.120***	-0.122***	-0.051***	-0.050***	-0.050**
	[0.019]	[0.019]	[0.027]	[0.014]	[0.014]	[0.020]
Election		0.078***			0.032***	
		[0.012]			[0.009]	
Local branches; bank type FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes		Yes	Yes	
Time FE	Yes			Yes		
Province-Time trends	Yes	Yes		Yes	Yes	
Province x Time FE			Yes			Yes
N	2,460	2,460	2,460	6,642	6,642	6,642
R^2	0.924	0.900	0.934	0.839	0.806	0.857

Notes: This table shows results of Equation (1) estimated on quarterly data. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table 3: Tactical redistribution of credit in metropolitan provinces

	Aligned provinces				*Non-aligned provinces*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Comp x StateBank x Election	0.132*	0.133*	0.132	0.127	-0.089**	-0.087**	-0.088	-0.089
	[0.073]	[0.073]	[0.100]	[0.091]	[0.040]	[0.040]	[0.055]	[0.065]
Comp x StateBank	-0.299*	-0.302*	-0.300	-0.281	0.041	0.046	0.043	0.042
	[0.171]	[0.171]	[0.233]	[0.234]	[0.082]	[0.079]	[0.111]	[0.121]
StateBank x Election	-0.147***	-0.143***	-0.146***		-0.061**	-0.062**	-0.061	
	[0.024]	[0.024]	[0.032]		[0.028]	[0.028]	[0.038]	
Comp x Election	-0.086*	-0.105**			0.037*	0.053*		
	[0.043]	[0.043]			[0.019]	[0.027]		
Comp	0.117	-0.001			0.033	-0.001		
	[0.096]	[0.088]			[0.054]	[0.057]		
Election		0.098***				0.037		
		[0.016]				[0.022]		
Local branches	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank type FE	Yes	Yes	Yes		Yes	Yes	Yes	
Province FE	Yes	Yes			Yes	Yes		
Time FE	Yes				Yes			
Province-Time trends	Yes	Yes			Yes	Yes		
Province x Time FE			Yes	Yes			Yes	Yes
Bank x Time FE				Yes				Yes
N	1,380	1,380	1,380	1,380	1,080	1,080	1,080	1,080
R^2	0.917	0.899	0.855	0.859	0.952	0.926	0.920	0.922

Notes: This table shows results of Equation (2) estimated on quarterly data. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table 4: Effects of tactical redistribution on real outcomes

	Aligned provinces				*Non-aligned provinces*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	*Bank Debt*	*Total Assets*	*Employment*	*Net Sales*	*Bank Debt*	*Total Assets*	*Employment*	*Net Sales*
Comp x State Bank Share x Election	1.420	1.458	0.835	0.644	-2.522**	-1.507*	-2.794**	-2.181**
	[1.080]	[1.009]	[0.802]	[0.885]	[0.983]	[0.833]	[1.107]	[0.860]
Comp x State Bank Share	-1.380	-0.986	-1.356	-1.600	-0.046	-1.261	-0.220	-1.634
	[2.641]	[2.103]	[1.573]	[2.265]	[1.657]	[1.615]	[1.086]	[1.658]
State Bank Share x Election	-1.218***	-0.805	-0.717*	-0.837	1.252	0.253	1.756*	1.260
	[0.431]	[0.476]	[0.390]	[0.618]	[0.753]	[0.468]	[0.894]	[0.728]
Comp x Election	-0.115	-0.114	-0.059	-0.017	0.298**	0.168*	0.371**	0.274**
	[0.123]	[0.110]	[0.086]	[0.088]	[0.102]	[0.085]	[0.139]	[0.107]
Comp	0.059	0.121	0.152	0.196	0.050	0.186	0.000	0.172
	[0.294]	[0.215]	[0.163]	[0.238]	[0.172]	[0.172]	[0.104]	[0.156]
Industry, Province, Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province-Time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,330	2,378	2,512	2,514	1,628	1,662	1,789	1,798
R^2	0.516	0.535	0.627	0.538	0.513	0.546	0.619	0.547

Notes: This table shows results of Equation (3) for the sample of metropolitan provinces and manufacturing industries. The dependent variable is (log) total bank debt in columns (1) and (5), (log) total assets in columns (2) and (6), (log) employment in columns (3) and (7), and (log) net sales in columns (4) and (8). Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table 5: Allocation of bank credit by initial efficiency

	All provinces			*Aligned provinces*			*Non-aligned provinces*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A:									
Asset turnover x Election	-0.041	-0.059**	-0.096***	-0.011	-0.050	-0.102***	-0.077**	-0.069**	-0.094**
	[0.026]	[0.026]	[0.023]	[0.039]	[0.038]	[0.035]	[0.032]	[0.034]	[0.037]
Asset turnover	0.017*	0.020**	0.027***	0.006	0.014	0.023	0.028*	0.024	0.031**
	[0.010]	[0.010]	[0.010]	[0.014]	[0.016]	[0.015]	[0.014]	[0.015]	[0.015]
R^2	0.069	0.204	0.245	0.072	0.205	0.255	0.075	0.198	0.277
Panel B:									
MRPK x Election	-0.070**	-0.111***	-0.130***	-0.022	-0.090*	-0.091*	-0.131***	-0.134***	-0.175***
	[0.031]	[0.032]	[0.038]	[0.044]	[0.047]	[0.052]	[0.040]	[0.043]	[0.062]
MRPK	0.045***	0.051***	0.055***	0.024	0.037*	0.038*	0.070***	0.066**	0.076***
	[0.015]	[0.015]	[0.016]	[0.020]	[0.022]	[0.022]	[0.024]	[0.025]	[0.025]
R^2	0.070	0.206	0.245	0.072	0.206	0.253	0.078	0.201	0.279
Industry FE	Yes	Yes		Yes	Yes		Yes	Yes	
Province FE	Yes			Yes			Yes		
Time FE	Yes			Yes			Yes		
Province x Time FE		Yes	Yes		Yes	Yes		Yes	Yes
Industry x Time FE			Yes			Yes			Yes
N	5,341	5,341	5,341	3,228	3,228	3,228	2,113	2,113	2,113

Notes: This table shows results of Equation (4) for the sample of all provinces and manufacturing industries. The dependent variable is year-on-year change in (log) total bank debt, defined as the sum of short-term and long-term bank debt on corporate balance sheets. Efficiency is defined as (log) net sales to total assets in Panel A and (log) net sales to real fixed assets in Panel B for each province-industry pair in 2006. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table 6: MRPK dispersion across firms within industry-province pairs

	Aligned provinces				*Non-aligned provinces*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Comp x State Bank Share x Election	0.046	0.040	0.061	-0.001	0.441	0.449	0.483*	0.578**
	[0.256]	[0.259]	[0.263]	[0.331]	[0.262]	[0.260]	[0.253]	[0.267]
Comp x State Bank Share	0.253	0.256	0.250	0.253	0.041	0.026	0.018	-0.004
	[0.202]	[0.202]	[0.204]	[0.216]	[0.189]	[0.187]	[0.194]	[0.180]
State Bank Share x Election	0.239	0.250	0.241		-0.496**	-0.506**	-0.548**	
	[0.192]	[0.193]	[0.194]		[0.229]	[0.225]	[0.222]	
Comp x Election	-0.011	-0.001			-0.145***	-0.164***		
	[0.044]	[0.047]			[0.036]	[0.035]		
Comp	-0.057*	-0.062*			0.004	-0.008		
	[0.031]	[0.031]			[0.029]	[0.035]		
Industry FE	Yes	Yes	Yes		Yes	Yes	Yes	
Province FE	Yes	Yes			Yes	Yes		
Time FE	Yes	Yes			Yes	Yes		
Province-Time trends		Yes				Yes		
Province x Time FE			Yes	Yes			Yes	Yes
Industry x Time FE				Yes				Yes
N	3,595	3,595	3,595	3,595	2,440	2,440	2,440	2,440
R^2	0.268	0.279	0.295	0.357	0.234	0.245	0.264	0.339

Notes: This table shows results of Equation (3) for the sample of metropolitan provinces and all industries. The dependent variable is the standard deviation of (log) MRPK within each province-industry pair by year. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Online Appendix for

Lending Cycles and Real Outcomes: Costs of Political Misalignment

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Appendix A

A.1 Data description

We provide details on our data sources in this sub-section.

Investment Incentive Certificates

Using publicly available data from the Turkish Ministry of Economy's websie, we put together a database that contains investment incentive certificates issued by the central government from 2003 onwards. These incentives are administered by the Ministry of Economy and constitute Turkey's main investment promotion programme. The government maintains a website in English and provides details on this scheme here: http://www.invest.gov.tr/en-US/investmentguide/investorsguide/Pages/Incentives.aspx. The scheme is available to both foreign and domestic investors through an "Investment Incentive Certificate", which is obtained from the Ministry following an evaluation of the investment project. Recipients are published in the *Official Gazette* every month alongside the amount of their proposed capital investment, number of jobs they promise to create, and the particular incentives they are entitled to receive based on the region of investment.

Over the 2003-2017 period, a total of 56,241 incentive certificates have been issued with a total of TRY 824 billion in capital investment and just over 2 million new jobs proposed by recipients. As domestic investors are more likely to respond to political influence than foreign investors, we focus on the incentive certificates rewarded to the former. Indeed, the vast majority of incentive certificates – 53,134 out of a total of 56,241 – have been awarded to local investors during the sample period.

Firm-level survey data

We use a confidential firm-level data set, the Annual Industrial and Services Statistics (AISS), at TurkStat's premises to construct two variables for each province-industry pair by year: (i) share of firms carrying out physical investments, and (ii) dispersion of (log) MRPK across firms. These data cover the period 2006-2015, as TurkStat discontinued carrying out these surveys in 2016. We should note that the survey provides data at the level of the firm but not the establishment. Although most firms in Turkey have a single establishment, this aspect of the data may add some noise to our estimation. In contrast, the administrative *GBS* database provides data aggregated from the level of establishment and a cleaner picture of province-industry activity and the aggregate economy.

We observe yearly investment figures – which include businesses' spending on plants,

physical equipment, tangible and intangible fixed assets, and other investment goods – for firms included in the annual industrial survey. We use total business spending on physical investment to calculate firm-level capital stock with the perpetual inventory method, assuming that firms are on their balanced growth path and a depreciation rate of 7% applies annually. We carry back the calculation of capital stock for firm-year observations prior to the first year in which physical investment is reported. We remove all firms with missing or negative capital stock. We then calculate (log) MRPK and trim it at the bottom and top 1% of its distribution before calculating its standard deviation at the province-by-industry level in each year.

A.2 A framework for productivity and misallocation

We closely follow Brandt et al. (2013) in building our measures of local and aggregate productivity and misallocation. The main difference in our setup is that we have multiple industries, instead of two, within each province. To simplify the discussion and exposition, we consider a single distortion to the economy, which is a province-by-industry specific capital wedge, and leave out possible output and labour distortions.

Model Setup

Consider an economy with m provinces, indexed by $p = 1, ..., m$, and I industries indexed by $i = 1, ..., I$. We assume a Cobb-Douglas production technology with the same factor elasticities in all provinces and industries:

$$Y_{pi} = A_{pi} L_{pi}^{\alpha} K_{pi}^{1-\alpha} \tag{8}$$

where Y_{pi}, A_{pi}, L_{pi}, K_{pi} indicate the real GDP, total factor productivity (TFP), employment, and real capital stock, respectively, in province p and industry i.

Assume that GDP in a province is a CES (constant elasticity of substitution) aggregate of goods produced in each industry i, and that aggregate GDP is a CES aggregate of output produced in each province:

$$Y_p = \left(\sum_{i=1}^{I} Y_{pi}^{1-\phi} \right)^{\frac{1}{1-\phi}} \tag{9}$$

$$Y = \left(\sum_{p=1}^{m} \omega_p Y_p^{1-\sigma} \right)^{\frac{1}{1-\sigma}} \tag{10}$$

where ϕ^{-1} and σ^{-1} indicate the elasticities of substitution between industries and provinces, respectively, and ω_p is province p's weight in aggregate output.

Determining efficient allocation

We are interested in understanding how the allocation of employment and capital across provinces affects local and aggregate TFP. We therefore take total employment and capital stock as given in each year and focus on their allocation. Let $L_p = \sum_{i=1}^{I} L_{pi}$ and $K_p = \sum_{i=1}^{I} K_{pi}$ denote employment and capital in province p and $L = \sum_{p=1}^{m} L_p$ and $K = \sum_{p=1}^{m} K_p$ denote total employment and capital in the country. Define $l_{i|p} = L_{pi}/L_p$, $k_{i|p} = K_{pi}/K_p$, $l_p = L_p/L$, and $k_p = K_p/K$ as the shares of employment and capital of each industry i in province p, and then of each province p in the country. We take quantity TFP for each province-industry pair, A_{pi}, as exogenous and calculate province-level TFP, A_p, and aggregate TFP, A, as:

$$A_p = \frac{\left(\sum_{i=1}^{I} Y_{pi}^{1-\phi}\right)^{\frac{1}{1-\phi}}}{L_p^{\alpha} K_p^{1-\alpha}} = \left[\sum_{i=1}^{I} \left(A_{pi} l_{i|p}^{\alpha} k_{i|p}^{1-\alpha}\right)^{1-\phi}\right]^{\frac{1}{1-\phi}}$$

$$A = \frac{\left(\sum_{p=1}^{m} \omega_p Y_p^{1-\sigma}\right)^{\frac{1}{1-\sigma}}}{L^{\alpha} K^{1-\alpha}} = \left[\sum_{p=1}^{m} \omega_p \left(A_p l_p^{\alpha} k_p^{1-\alpha}\right)^{1-\sigma}\right]^{\frac{1}{1-\sigma}}$$

The allocation of employment and capital that maximises aggregate TFP is called the efficient allocation. In order to determine the efficient level of TFP and allocation, we solve the social planner's problem: $\underset{L_{pi}, K_{pi}}{max}\, Y$ s.t. (8)-(10) and market clearing conditions $\sum_{p,i} L_{pi} = L$ and $\sum_{p,i} K_{pi} = K$. One can then show that, for any given L and K, the allocation that maximises Y is given by:

$$\pi_{pi} = \frac{L_{pi}}{L_p} = \frac{K_{pi}}{K_p} = \left(\frac{A_{pi}}{A_p^*}\right)^{\frac{1-\phi}{\phi}} \;;\; \pi_p = \frac{L_p}{L} = \frac{K_p}{K} = \frac{\omega_p^{\frac{1}{\sigma}} \left(A_p^*\right)^{\frac{1-\sigma}{\sigma}}}{\sum_{p=1}^{m} \omega_p^{\frac{1}{\sigma}} \left(A_p^*\right)^{\frac{1-\sigma}{\sigma}}}$$

$$A_p^* = \left(\sum_{i=1}^{I} A_{pi}^{\frac{1-\phi}{\phi}}\right)^{\frac{\phi}{1-\phi}} \;;\; A^* = \left[\sum_{p=1}^{m} \omega_p^{\frac{1}{\sigma}} \left(A_p^*\right)^{\frac{1-\sigma}{\sigma}}\right]^{\frac{\sigma}{1-\sigma}}$$

where $*$ indicates the efficient level. What this allocation shows is that labour and capital should be allocated to province-industry pairs in proportion to their relative productivities. In the presence of factor market distortions – in our case we have in mind distortions induced by bank lending, and therefore to capital – the actual allocation may deviate from the efficient

allocation. This means that actual aggregate TFP may be lower than the efficient TFP, both at the province level and in the aggregate. We follow Brandt et al. (2013) and define TFP losses due to distortions at the province level and in the aggregate, respectively, as follows:

$$D_p = log(A_p^*/A_p)\,; \quad D = log(A^*/A)$$

Distortions to capital

Tactical redistribution implies that opposition provinces that are politically contested in local elections are punished and industries with greater dependence on state banks suffer especially more. We therefore consider a single distortion to the economy, which is a province-by-industry specific capital wedge.

The representative firm that operates in province p and industry i solves the profit maximisation problem:

$$\underset{K_{pi},\, L_{pi}}{max}\; P_{pi} A_{pi} L_{pi}^{\alpha} K_{pi}^{1-\alpha} - wL_{pi} - \tau_{pi}^{k} r K_{pi}$$

where w denotes the wage, r the rental price of capital, ad τ_{pi}^{k} is the province-by-industry specific capital wedge. The first-order condition to this problem relative to capital helps us define MRPK and yields:

$$(1-\alpha) P_{pi} A_{pi} L_{pi}^{\alpha} K_{pi}^{-\alpha} = \tau_{pi}^{k} r$$

We can therefore identify the capital wedge, which is proportional to MRPK, up to a scalar: $\tau_{pi}^{k} \propto \frac{P_{pi} Y_{pi}}{K_{pi}}$. Since allocation of factors is not affected by any proportional changes in wedges common to all provinces and sectors, we can set the capital wedge as the average value product of capital.

Notice that this profit maximisation problem implies a province-by-industry specific price faced by the firm. In our empirical exercise, we do not observe price deflators at such detailed level. However, we can use a method similar to Hsieh and Klenow (2009) and Brandt et al. (2013) to infer price information from nominal output shares. With a CES aggregate production function to determine province-level output, the profit maximisation problem is:

$$\underset{Y_{pi},\, i=1,\ldots,I}{max}\; P_p \left(\sum_{i=1}^{I} Y_{pi}^{1-\phi} \right)^{\frac{1}{1-\phi}} - \sum_{i=1}^{I} P_{pi} Y_{pi}$$

which yields the first-order condition:

$$P_{pi} = P_p \left(\frac{Y_{pi}}{Y_p} \right)^{-\phi} \tag{11}$$

One can then show that the prices satisfy the following relationship:

$$\frac{P_{pi}}{P_p} = \left(\frac{Y_{pi}^{nominal}}{\sum_{i=1}^{I} Y_{pi}^{nominal}} \right)^{-\frac{\phi}{1-\phi}}$$

We can therefore calculate province-industry specific price indices using the province-level price index and each province-industry's nominal output share. This helps us construct real output at the province-industry level:

$$Y_{pi} = \frac{Y_{pi}^{nominal}}{P_p} \left(\frac{Y_{pi}^{nominal}}{\sum_{i=1}^{I} Y_{pi}^{nominal}} \right)^{\frac{\phi}{1-\phi}} \tag{12}$$

In a similar manner, we derive an expression to determine the province weights. The profit maximisation problem to determine aggregate output is given by:

$$\underset{Y_p, p=1,\ldots,m}{max} \; P \left(\sum_{p=1}^{m} \omega_p Y_p^{1-\sigma} \right)^{\frac{1}{1-\sigma}} - \sum_{p=1}^{m} P_p Y_p$$

which yields the first-order condition:

$$P_p = \omega_p P \left(\frac{Y_p}{Y} \right)^{-\sigma} \tag{13}$$

Parameter choices and data

We follow Brandt et al. (2013) in setting our parameters. We assume that the technology parameter is best measured in a relatively undistorted economy (Hsieh and Klenow, 2009) and set $\alpha = 0.67$, which corresponds to the average labour share in the US. We set ϕ and σ equal to 0.67, which implies an elasticity of substitution of 1.5 both across industries within a province and across provinces. This value is at the lower end of what is commonly used in the international trade and macro literature. Many studies use a higher elasticity of substitution equal to 3 (e.g. Hsieh and Klenow, 2009), which would imply larger estimated TFP losses.

We choose ω_p such that Equation (13) holds on average during our sample period:

$$\omega_p = \frac{1}{m} \sum_{t=2006}^{2016} \left(\frac{P_{pt} Y_{pt}^{\sigma}}{\sum_{p=1}^{m} P_{pt} Y_{pt}^{\sigma}} \right)$$

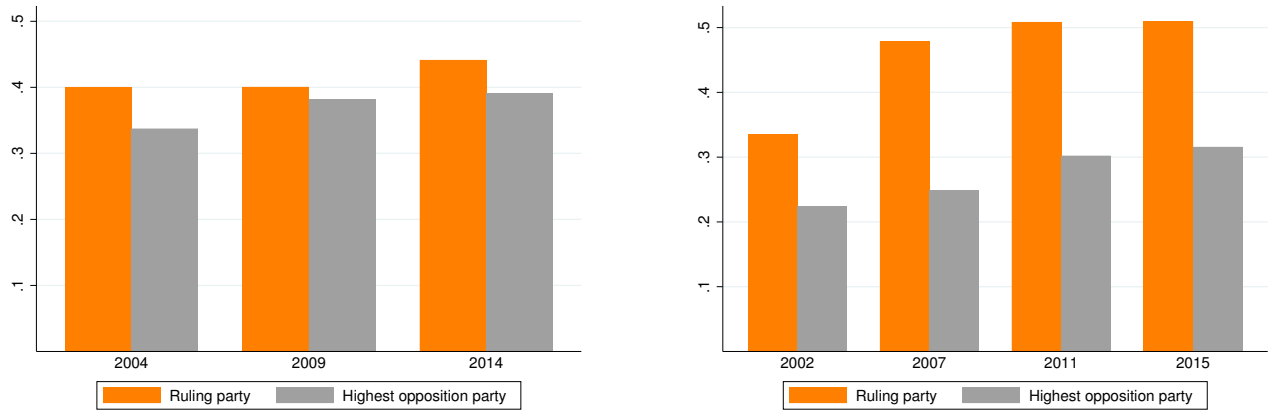
where m equals 30 for the sample of metropolitan provinces and 81 for the sample of all provinces.

The data provided by the MoI's *GBS* following our information request do not contain information on investment expenditures. We also do not observe the breakdown between fixed and current assets at the level of province, industry and year. Instead, we can work with two pieces of information. First, we have the book value of total assets at original purchase prices aggregated from the firm-level balance sheets up to the level of province, industry and year. Second, the GBS regularly publishes on its website an aggregated balance sheet at the level of industry and year. We use the aggregated information at the industry-year level to estimate the ratios of fixed to total assets. Based on this ratio, we proportionately rescale total assets at the province, industry and year level. We deflate fixed assets by the aggregate price index for investment goods to arrive at our measure of capital stock at the province-industry level.

Our measure of output is net sales. Unfortunately *GBS* does not provide measures of value added or purchases of intermediates. To the extent that variation is small across provinces within a sector in their use of intermediate inputs, this should not affect our measure of TFP. We calculate real output by taking advantage of Equation (12) and using nominal output values alongside province-specific CPI indices from TurkStat.

A.3 Figures and tables

Figure A.1: Average vote shares in local and general elections

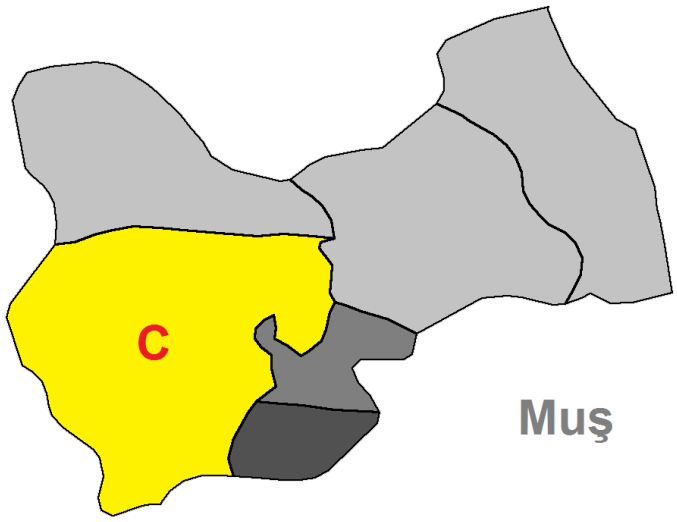


(a) Local elections

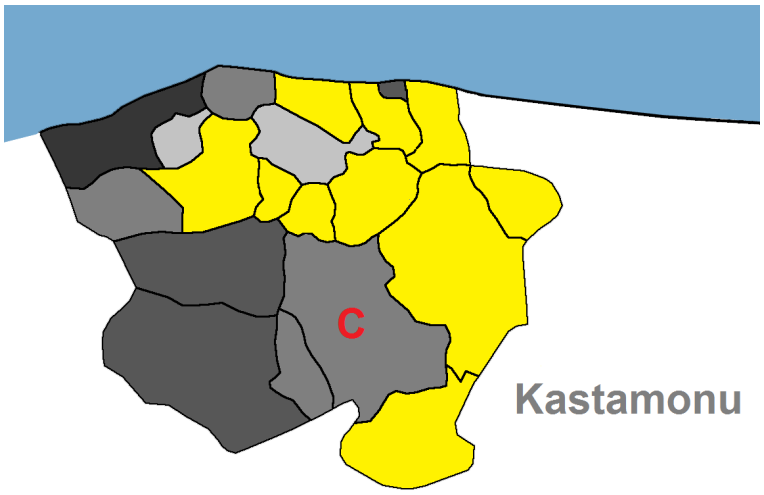
(b) General elections

Notes: Panels A and B show the average vote shares of the governing party and the highest opposition party for each province in local and general elections, respectively.

Figure A.2: District-level political alignment in two non-metropolitan provinces



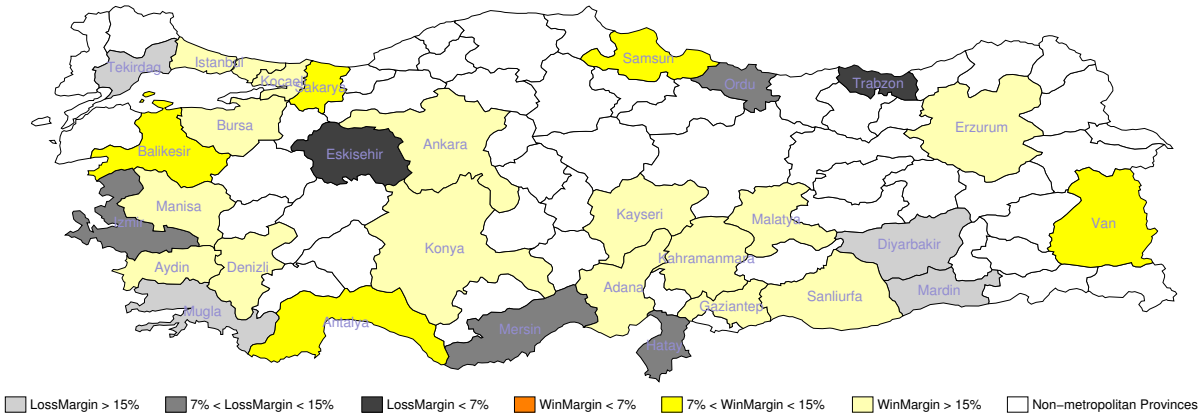
(a) A politically aligned province in 2004 elections



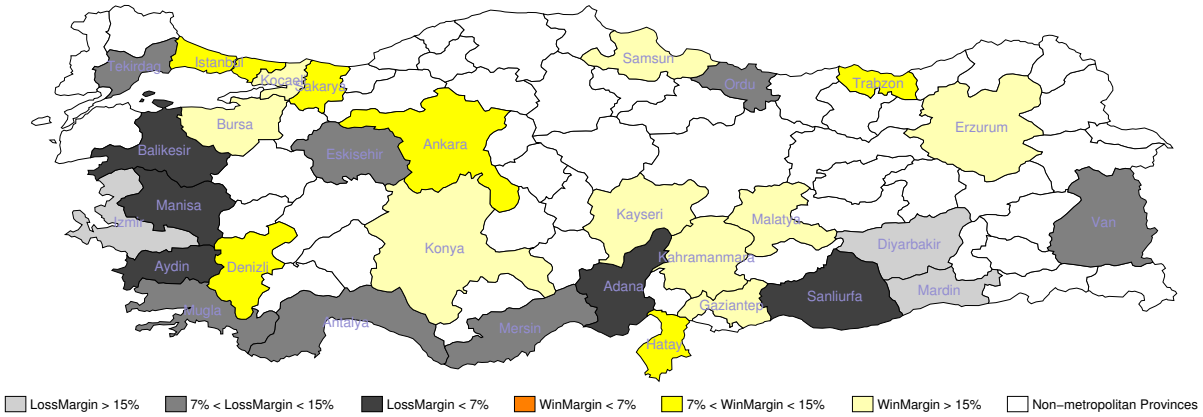
(b) A politically non-aligned province in 2004 elections

Notes: Panel A shows a province in which the elected central district mayor is aligned with the central government and Panel B shows a province in which the elected central district mayor is non-aligned. "C" in red colour stands for the central district. Politically aligned districts are given in yellow and non-aligned districts are given in varying shades of gray corresponding to dierent opposition parties.

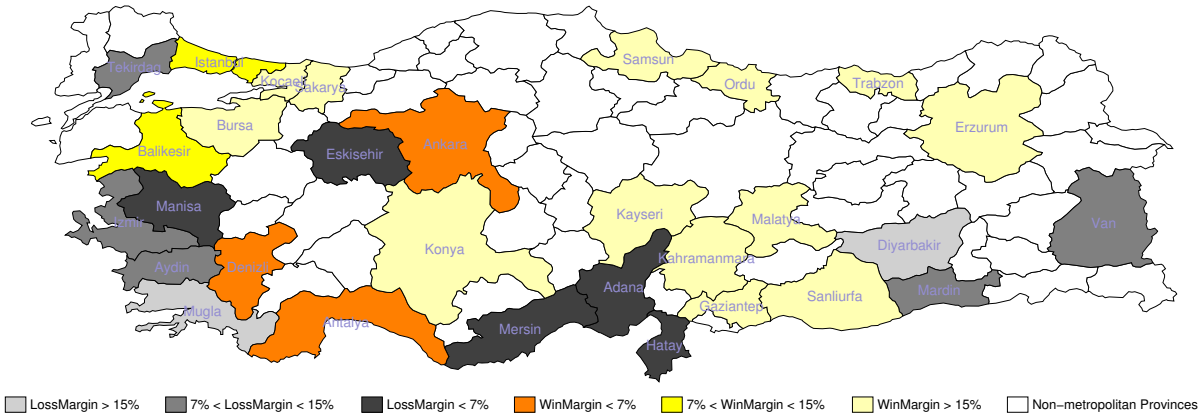
Figure A.3: Political competition and alignment in metropolitan provinces



(a) 2004 local elections



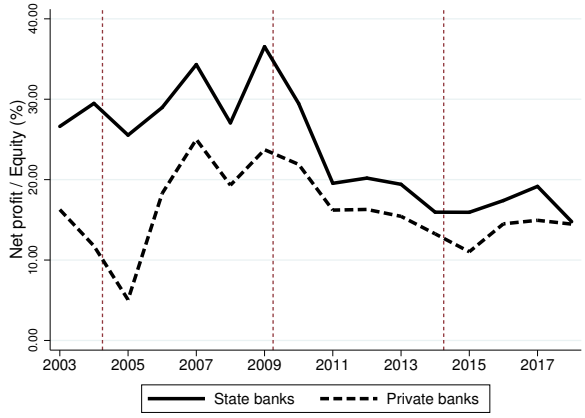
(b) 2009 local elections



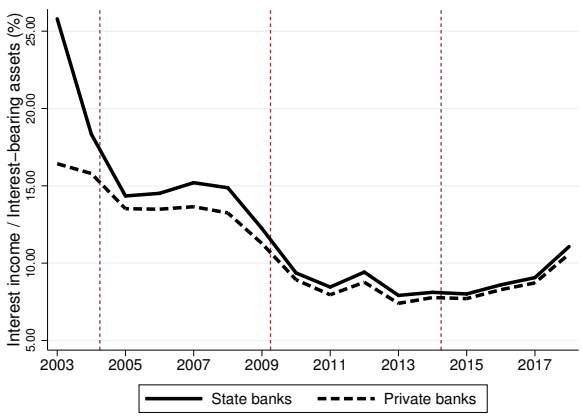
(c) 2014 local elections

Notes: Panels A, B, and C show the win/loss margins for the governing party in 2004, 2009, and 2014 local elections, respectively. Politically aligned provinces are in shades of yellow and non-aligned provinces are in shades of gray.

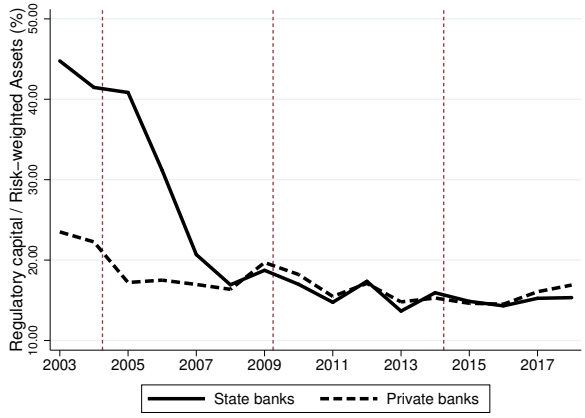
Figure A.4: Aggregate performance ratios by bank type



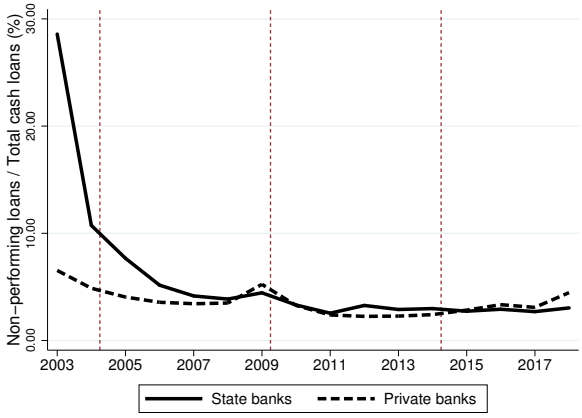
(a) Return on equity



(b) Interest margin



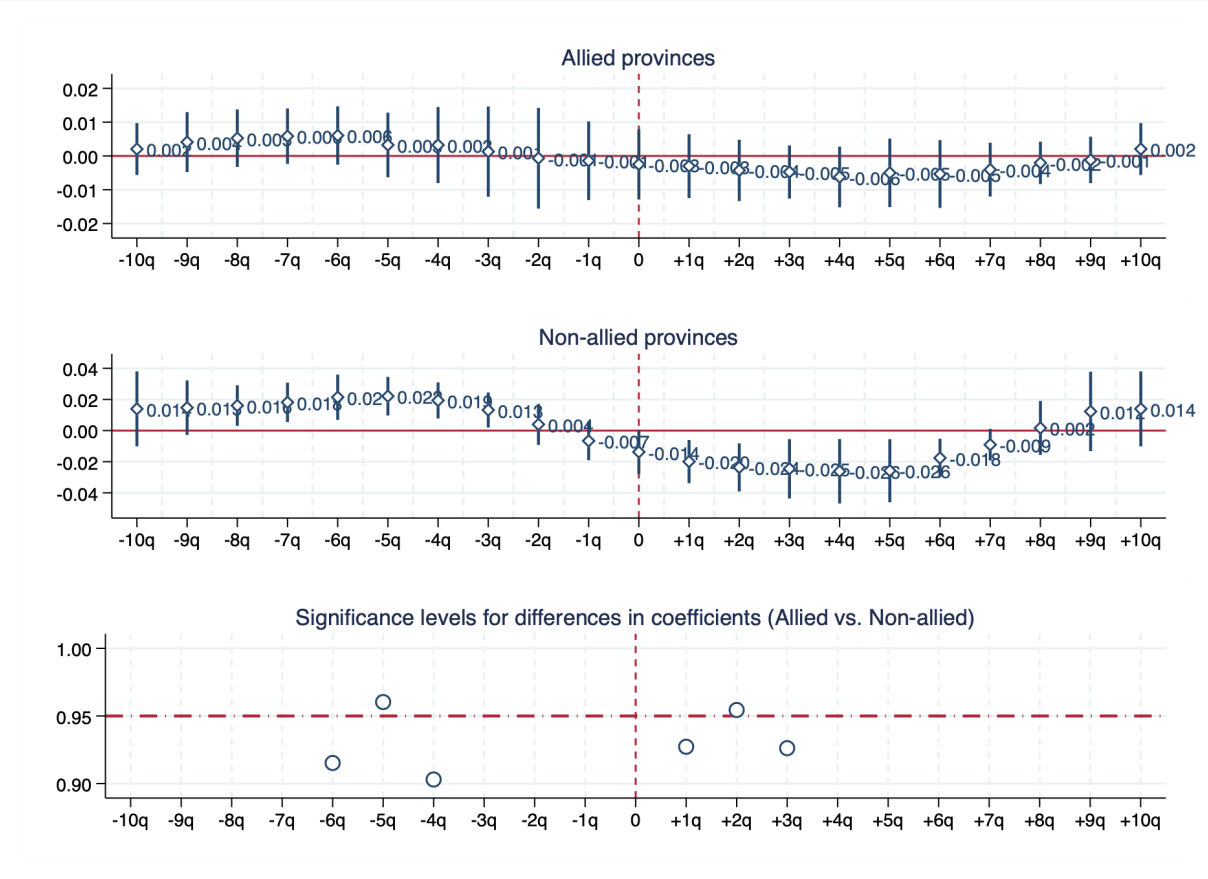
(c) Tier 2 ratio



(d) NPL ratio

Notes: Return on equity is defined as net profits (after tax) divided by equity. Interest margin is defined as total interest income divided by total interest-bearing assets. Tier 2 ratio is defined as equity divided by risk-weighted assets. NPL ratio is defined as gross non-performing loans divded by total cash loans.

Figure A.5: Share of non-performing corporate loans over the election cycle



Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Table A.1: Summary statistics for the main variables

	Mean	Median	S.D.	Min	Max	Obs.	Source
Panel A: Quarterly cash loans (2007q4-2017q4)							
log Total loans	13.50	13.39	1.40	9.24	19.28	6,642	FinTürk
log Corporate loans	12.23	12.15	1.61	7.16	18.58	6,642	FinTürk
log Consumer loans	12.61	12.54	1.28	8.67	17.69	6,642	FinTürk
log Non-performing loans	10.03	9.94	1.51	4.37	15.67	6,642	FinTürk
Panel B: Election data							
Aligned (dummy)	0.60	1.00	0.49	0.00	1.00	243	TurkStat
Competition	0.82	0.85	0.15	0.24	0.99	243	TurkStat
Panel C: Annual economic data (2006-2016)							
log Number of establishments	3.85	3.71	1.48	0.00	9.96	9,785	MoI
log Bank debt	16.91	16.95	2.40	8.11	23.14	7,646	MoI
log Employment	6.22	6.17	1.98	0.00	12.77	9,785	MoI
log Net sales	18.16	18.07	2.27	12.03	24.85	9,677	MoI
log Total assets	18.75	18.68	2.03	13.65	24.80	7,972	MoI

Appendix B

B.1 Testing for additional mechanisms

In Sub-section 4.5 of the main text, we briefly discuss the exercises that we conducted to test whether demand-side factors can explain the lending behaviour of state banks. In this appendix, we provide more detail on these exercises and links to the results referenced in the main text.

First, we explore the importance of political uncertainty, which may affect firms' decisions on investment and borrowing. A challenge to this argument is that uncertainty should affect corporate decision-making similarly in both aligned and non-aligned provinces. Consider:

$$Credit_{b,p,t} = \beta_\tau HI_{p,t} \times Election_{t+\tau} + \alpha_1 HI_{p,t} + \delta X_{b,p,t-1} + \theta_b + \gamma_p + \lambda_t + \varepsilon_{b,p,t} \quad (14)$$

where $HI_{p,t}$ is an indicator variable for the upper half of a Herfindahl index of local political competition and proxies uncertainty. The Herfindahl index is defined as: $1-\sum_{i\in I}(VoteShare_{i,p,t})^2$, where $VoteShare_{i,p,t}$ denotes each political party's vote share in province p and time t. We use a dummy variable instead of the continuous index to guard against the possibility that credit reallocation might affect political uncertainty. Figure B.16 shows no evidence at all – from regressions on either quarterly (Panel A) or yearly (Panel B) data – that political uncertainty is associated with a change in lending at any point of the election cycle.

Are state banks particularly vulnerable to political uncertainty? If state banks tend to work relatively more with firms that have greater sensitivity to local politics, then they may cut back on lending due to reduced credit demand. We estimate the following model:

$$\begin{aligned} Credit_{b,p,t} = \beta_\tau HI_{p,t} \times StateBank_b \times Election_{t+\tau} + \alpha_1 HI_{p,t} \times StateBank_b \\ + \alpha_2 StateBank_b \times Election_{t+\tau} + \alpha_3 HI_{p,t} \times Election_{t+\tau} \\ + \alpha_4 HI_{p,t} + \delta X_{b,p,t-1} + \theta_b + \gamma_p + \lambda_t + \varepsilon_{b,p,t} \quad (15) \end{aligned}$$

Figure B.17 shows that β_τ is estimated with a negative, but statistically insignificant, sign in the quarters (year) leading up to a local election in Panel A (Panel B). This suggests that while state banks may indeed be more cautious prior to local elections, there is not sufficient evidence that political uncertainty would explain our findings.

Second, we explore the possibility that the central government allocates public contracts or funds around local elections. If firms receiving these funds use state bank credit relatively more, this can give rise to a lending cycle induced by firm-level demand. We put together two new databases from publicly available data to test this particular mechanism.

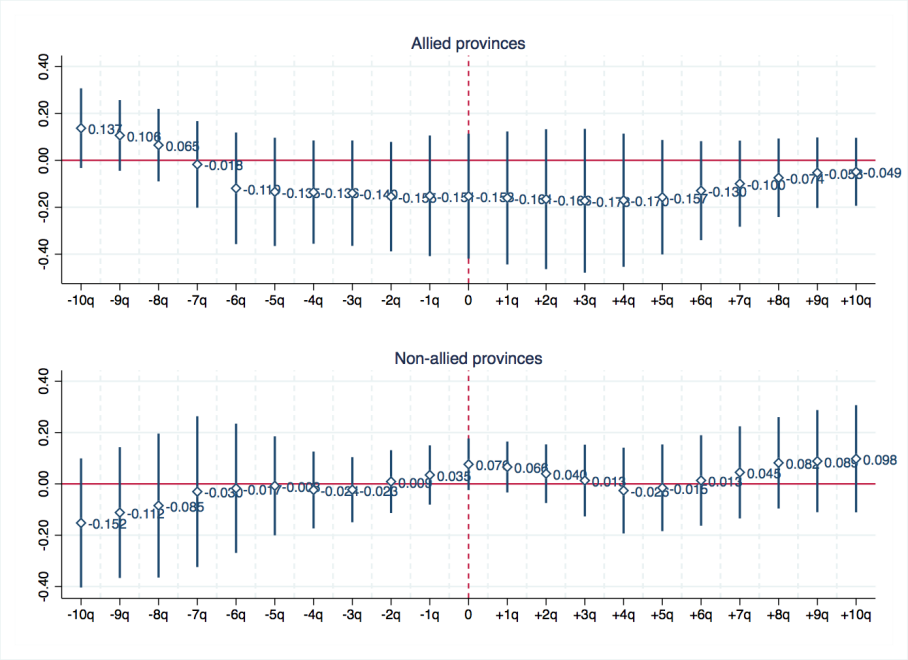
The first database contains "investment incentive certificates" issued by the central government from 2003 onwards. We describe these data in greater detail in Appendix A. If these certificates are allocated based on political incentives, then promised job creation and investments should go up in politically aligned provinces and down in non-aligned provinces prior to a local election when there is high electoral competition. We aggregate the data to the province level at a quarterly frequency and estimate the following model in logs:

$$PublicFunds_{p,t} = \beta_\tau Comp_{p,t} \times Election_{t+\tau} + \gamma_p + \lambda_t + \gamma_p \times t + \varepsilon_{p,t} \tag{16}$$

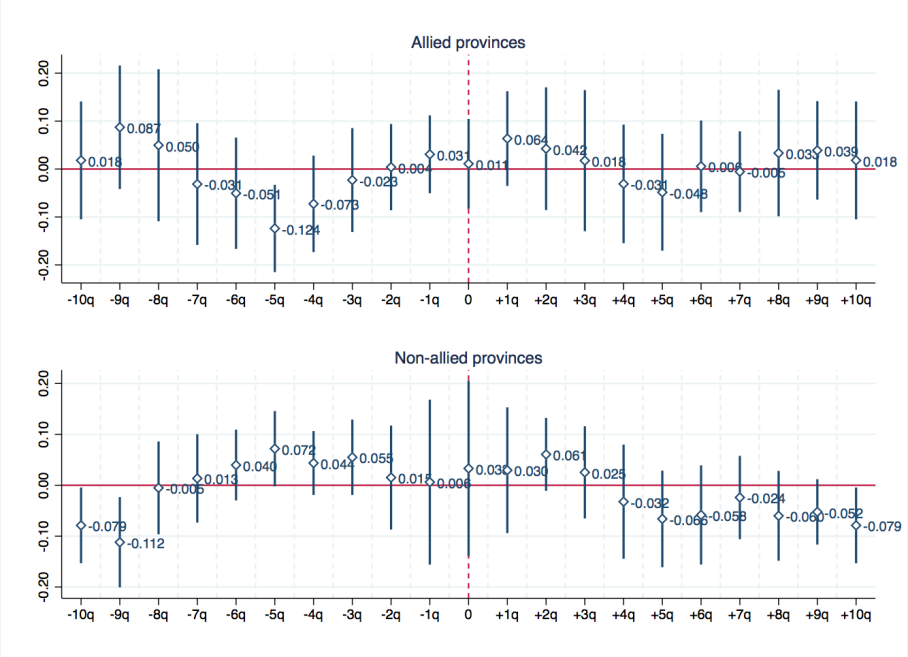
Figure B.18 shows the results. In the run up to local elections, we find a drop in the number of certificates and promised job creation and capital investment in provinces that are politically contested and non-aligned. The expected increase in aligned provinces is not always estimated with statistical signicance, except for promised job creation. Importantly, there is no requirement for recipients to work with state banks rather than private banks. However, to the extent that this occurs, the reallocation of government incentives around elections can explain part of the variation in the lending cycle.

The second database contains construction permits issued by local municipalities. We collect data on the number of buildings and building area covered on all new construction projects initiated by the public sector during the 2003-2017 period. We aggregate these data to the province level at a quarterly level and estimate Equation (16). Figure B.19 shows no particular cycle in public construction in either politically aligned or non-aligned provinces. It is therefore unlikely that the public sector's construction activity drives the lending cycle identified earlier.

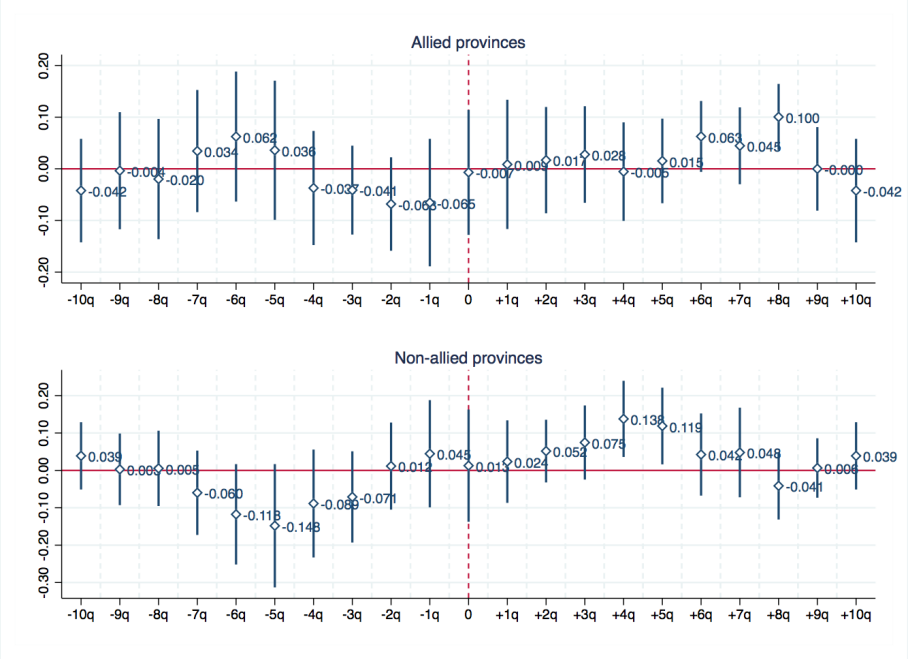
Figure B.1: Placebo tests for tactical redistribution of state-bank credit



(a) Local election dates generated randomly



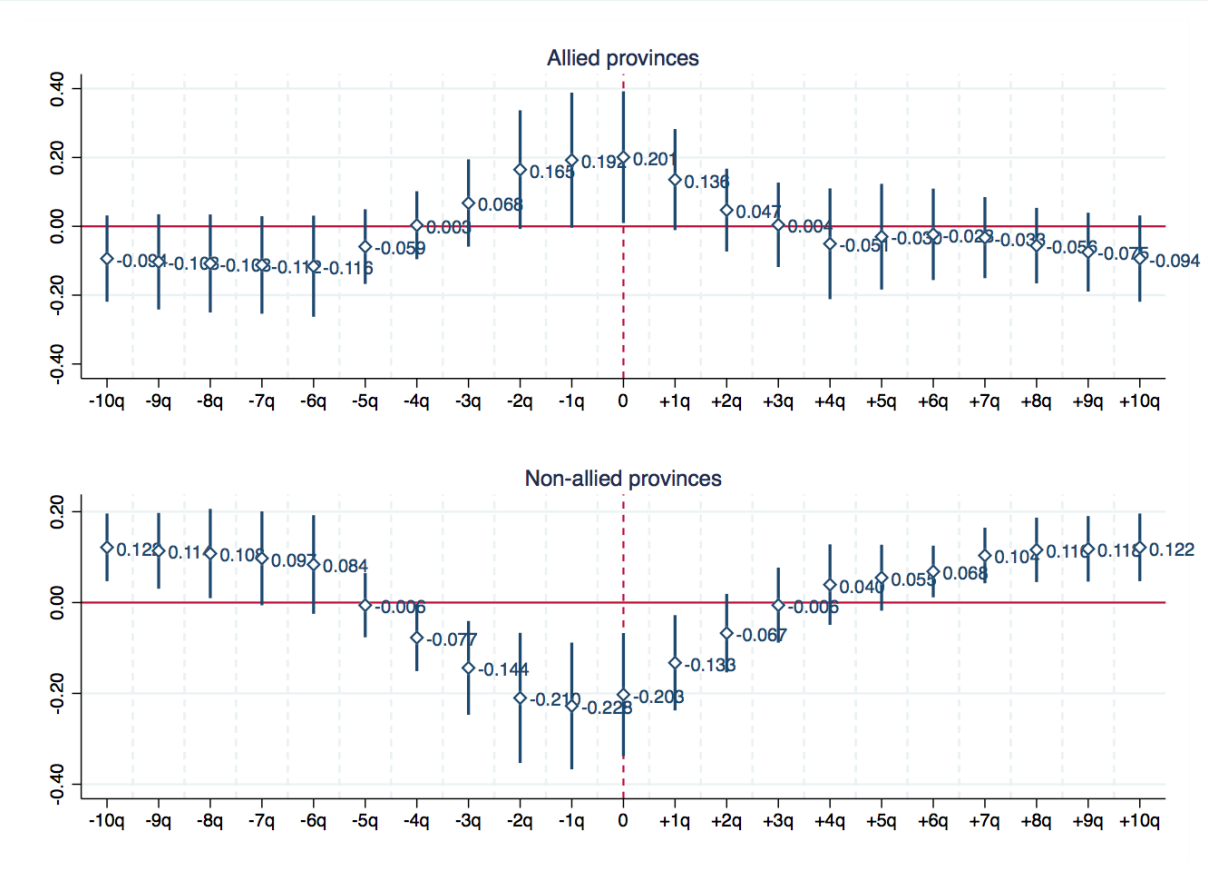
(b) Political competition generated randomly



(c) Political alignment generated randomly

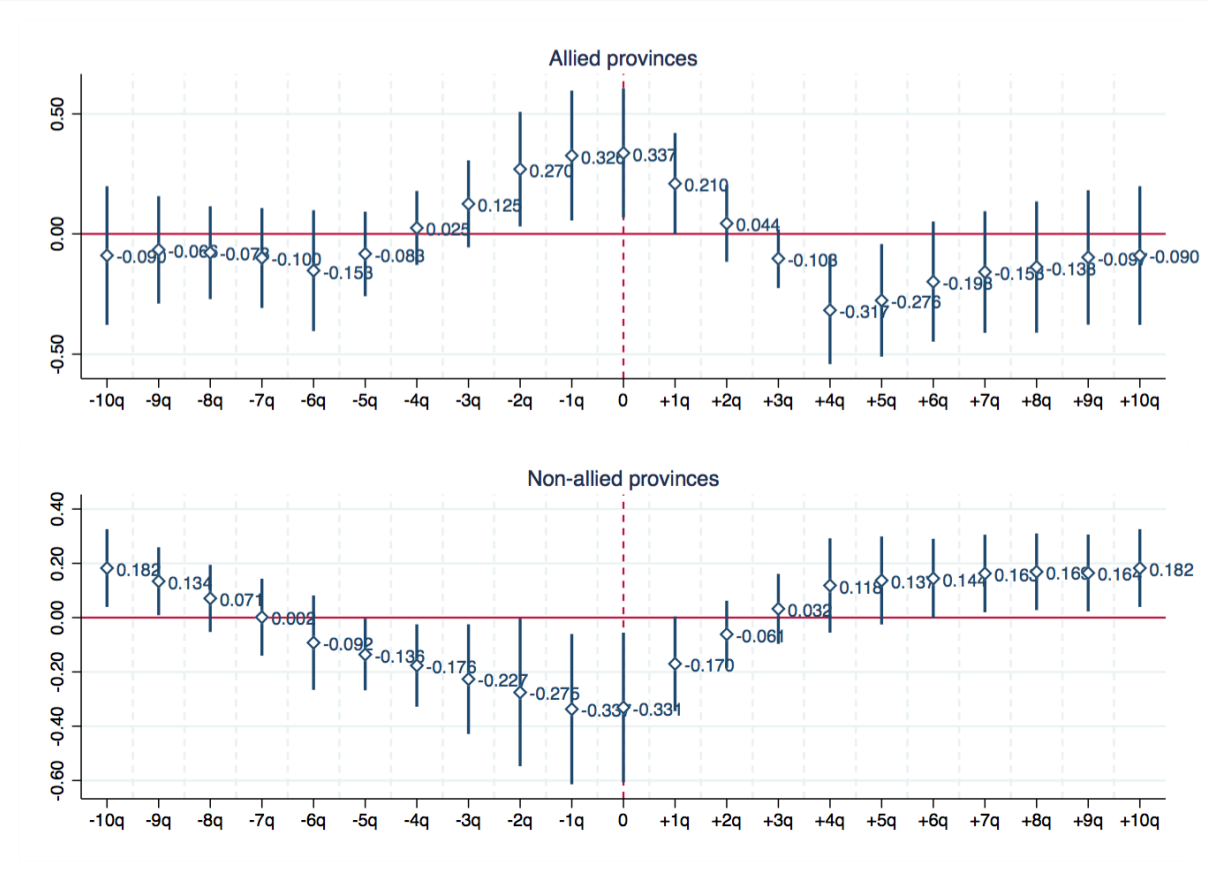
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Panel (a) randomises the timing of local elections; panel (b) randomises the political competition dummy; and panel (c) randomises the political alignment dummy, while keeping everything else fixed. Sample includes metropolitan provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.2: Tactical redistribution of state-bank credit over the election cycle: Competition dummy based on average margin across three elections



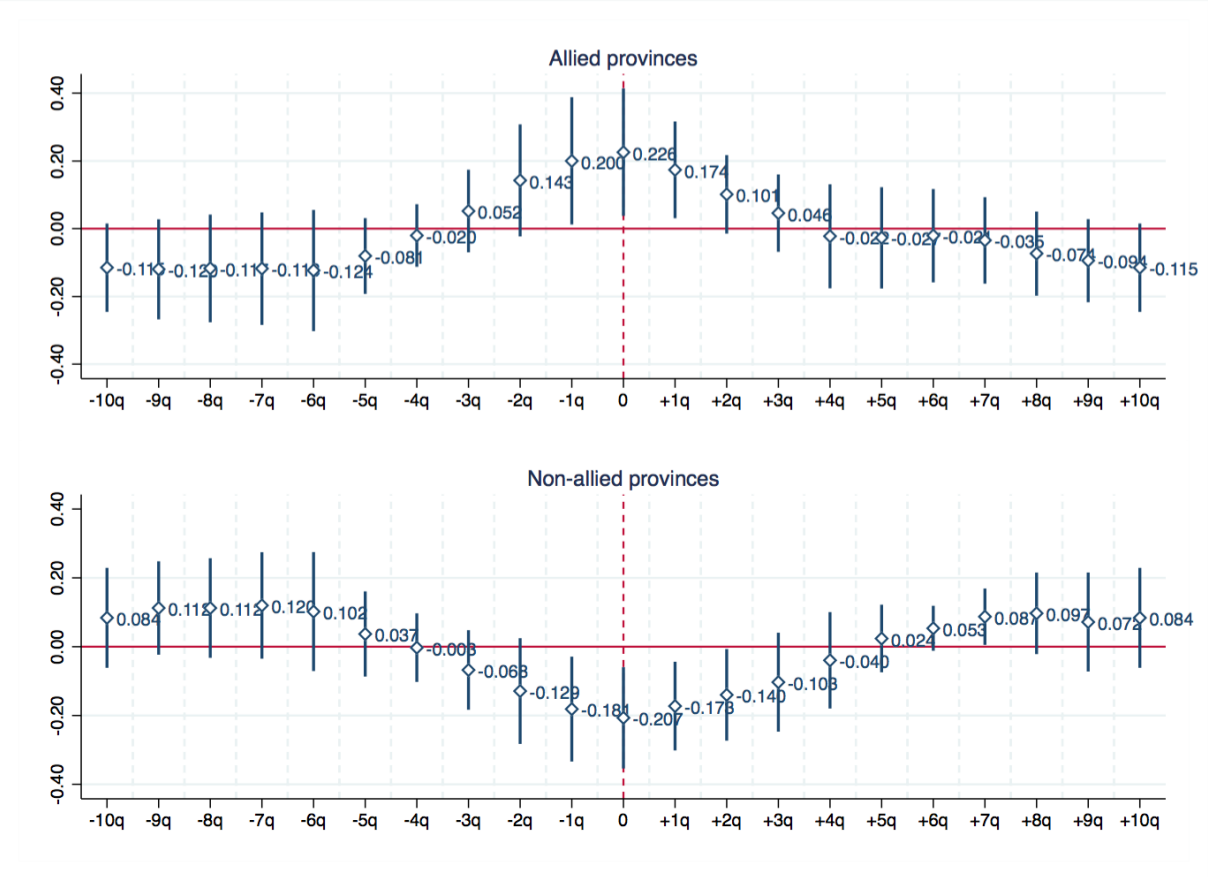
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces. The competition variable equals 1 if a province has above median electoral competition defined as the win margin averaged across three local elections (2004, 2009, 2014), and 0 otherwise. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.3: Tactical redistribution of state-bank credit over the election cycle: Competition dummy defined by top 25% of distribution



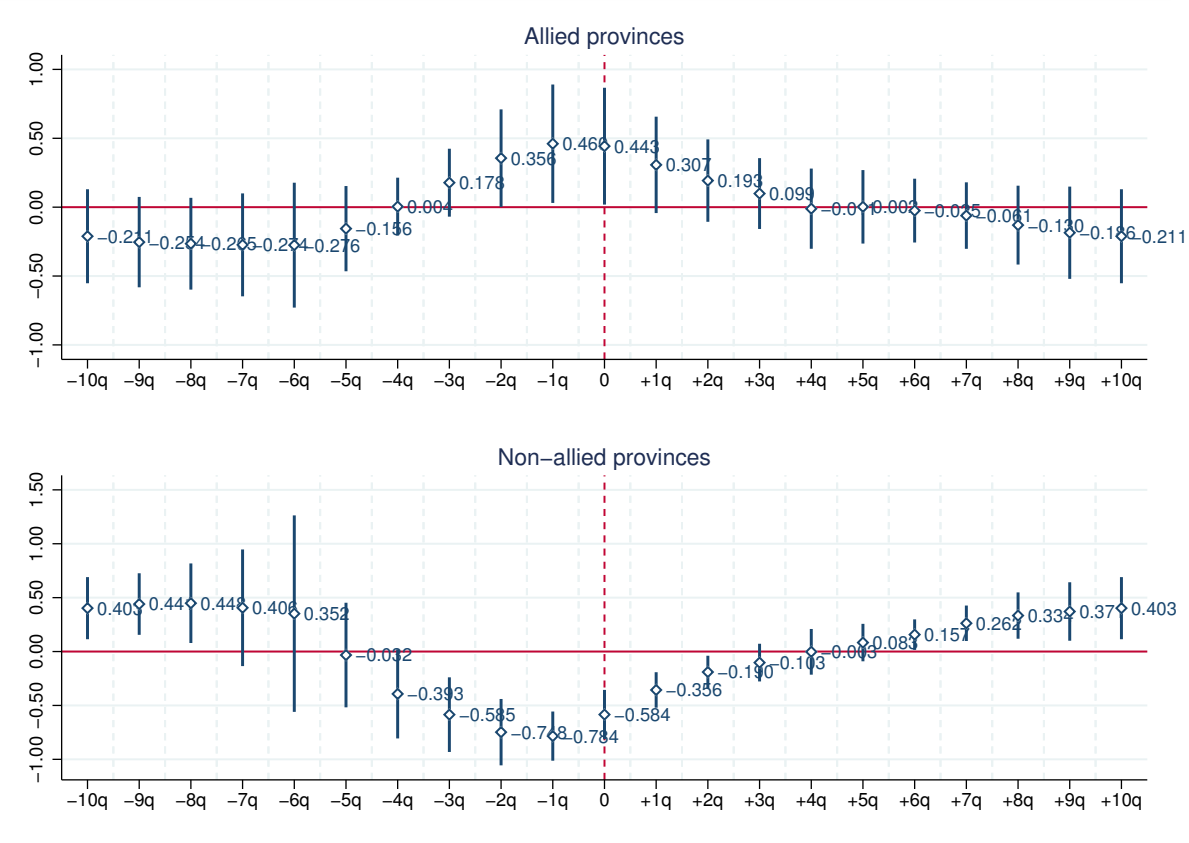
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces. The competition variable equals 1 for the upper 25% of the continuous competition variable in the pooled sample of province-years, and 0 otherwise. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.4: Tactical redistribution of state-bank credit over the election cycle: Competition dummy defined by top 50% of distribution for each election



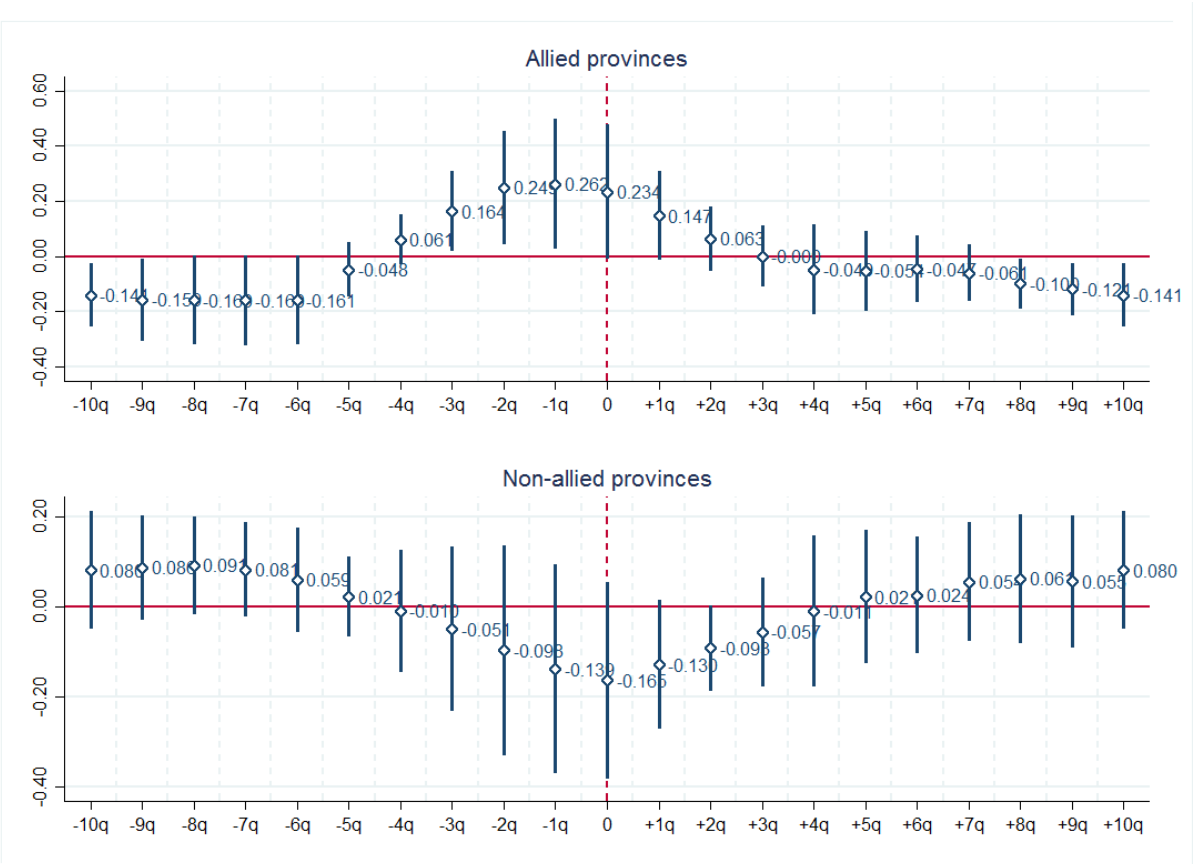
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces. The competition variable equals 1 for the upper 50% of the continuous competition variable for each local election separately treated, and 0 otherwise. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.5: Tactical redistribution of state-bank credit over the election cycle: Continuous competition



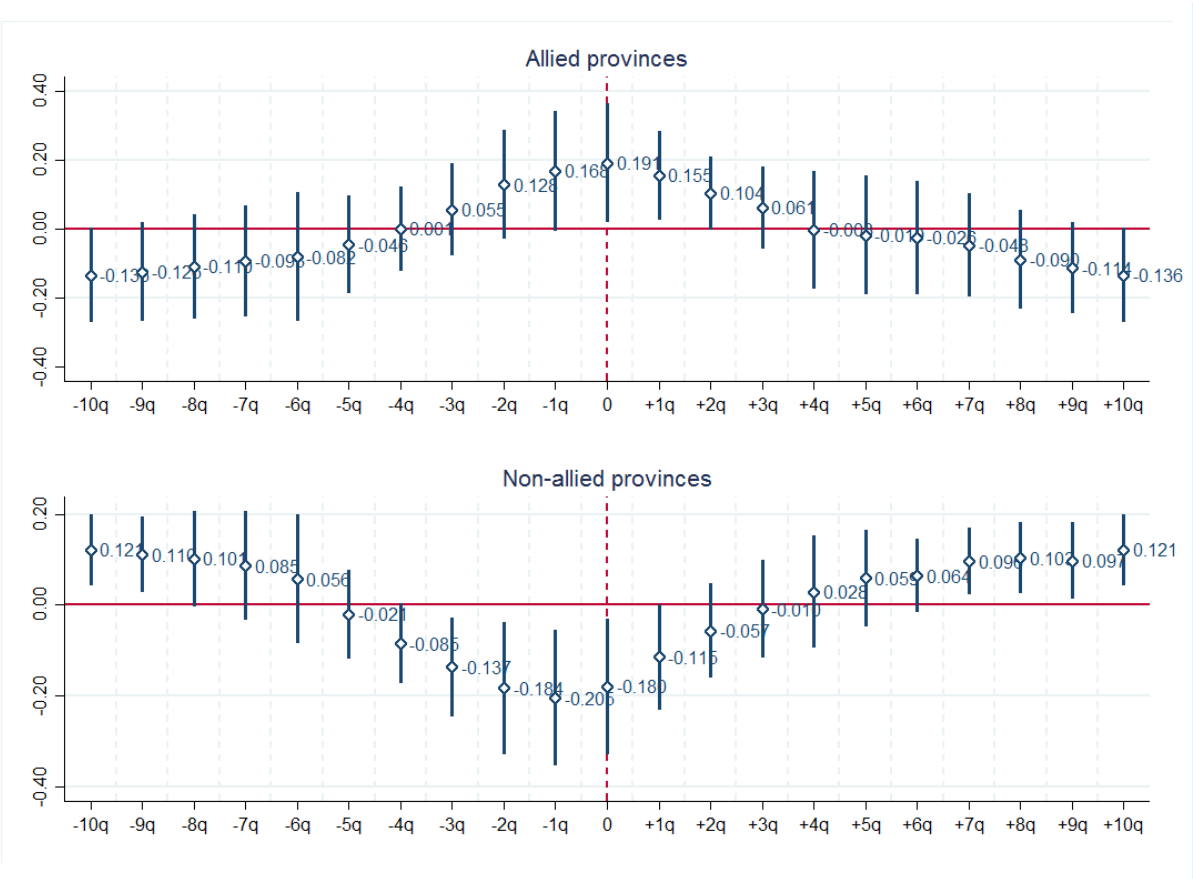
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces. The competition variable is used in its continuous form. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.6: Tactical redistribution of state-bank credit over the election cycle: Using previous election outcomes to define competition



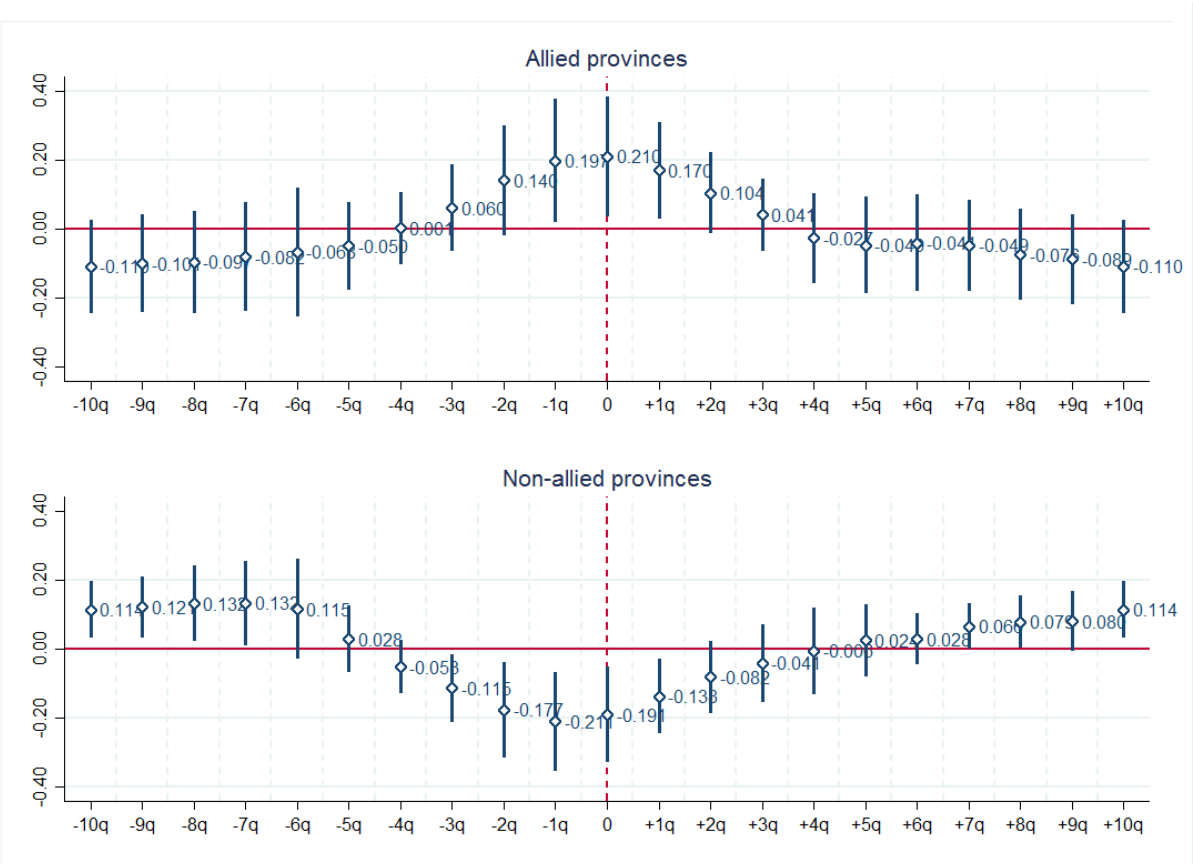
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces. The competition variable equals 1 for the upper 50% of the continuous competition variable based on previous election's outcome, and 0 otherwise. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.7: Tactical redistribution of state-bank credit over the election cycle: Controlling for bank branches in continuous form



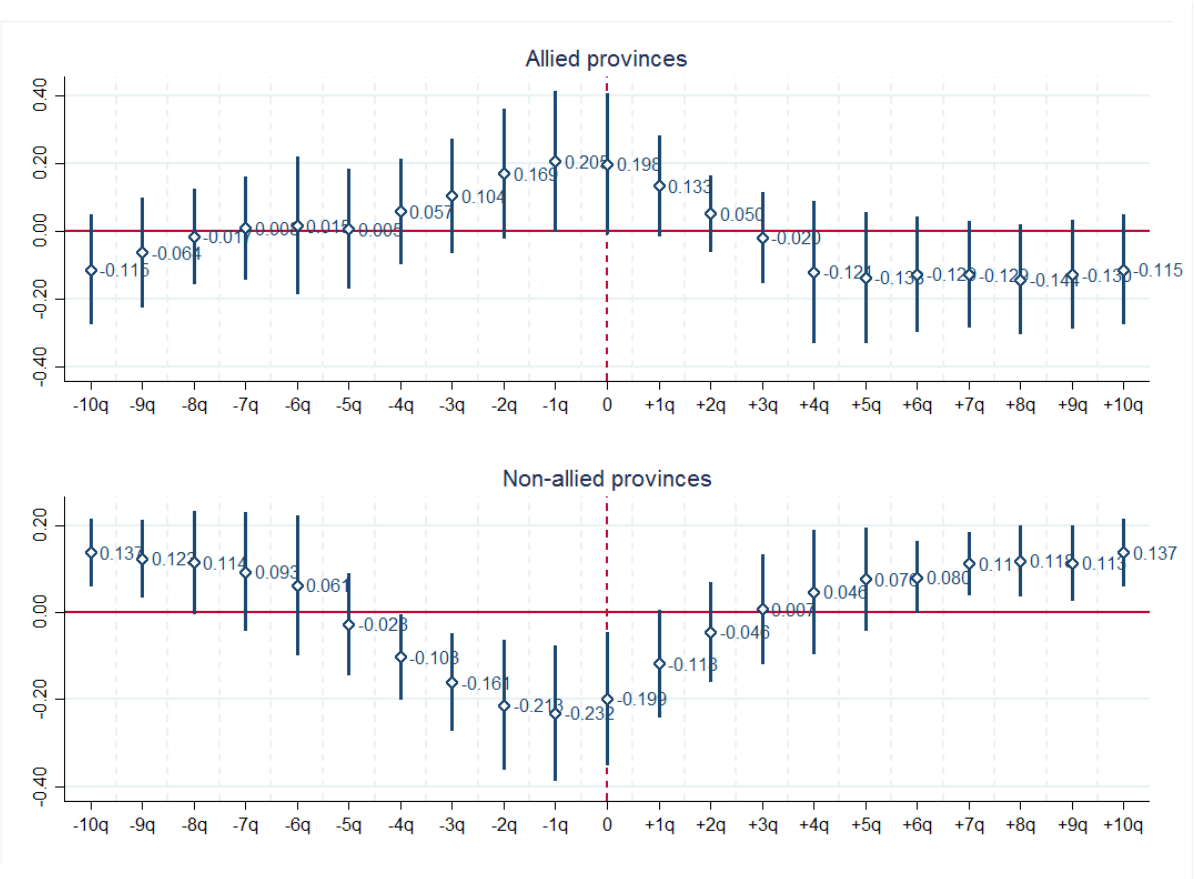
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces. Bank-province-time controls include bank branches in continuous log form. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.8: Tactical redistribution of state-bank credit over the election cycle: Controlling for customer deposits



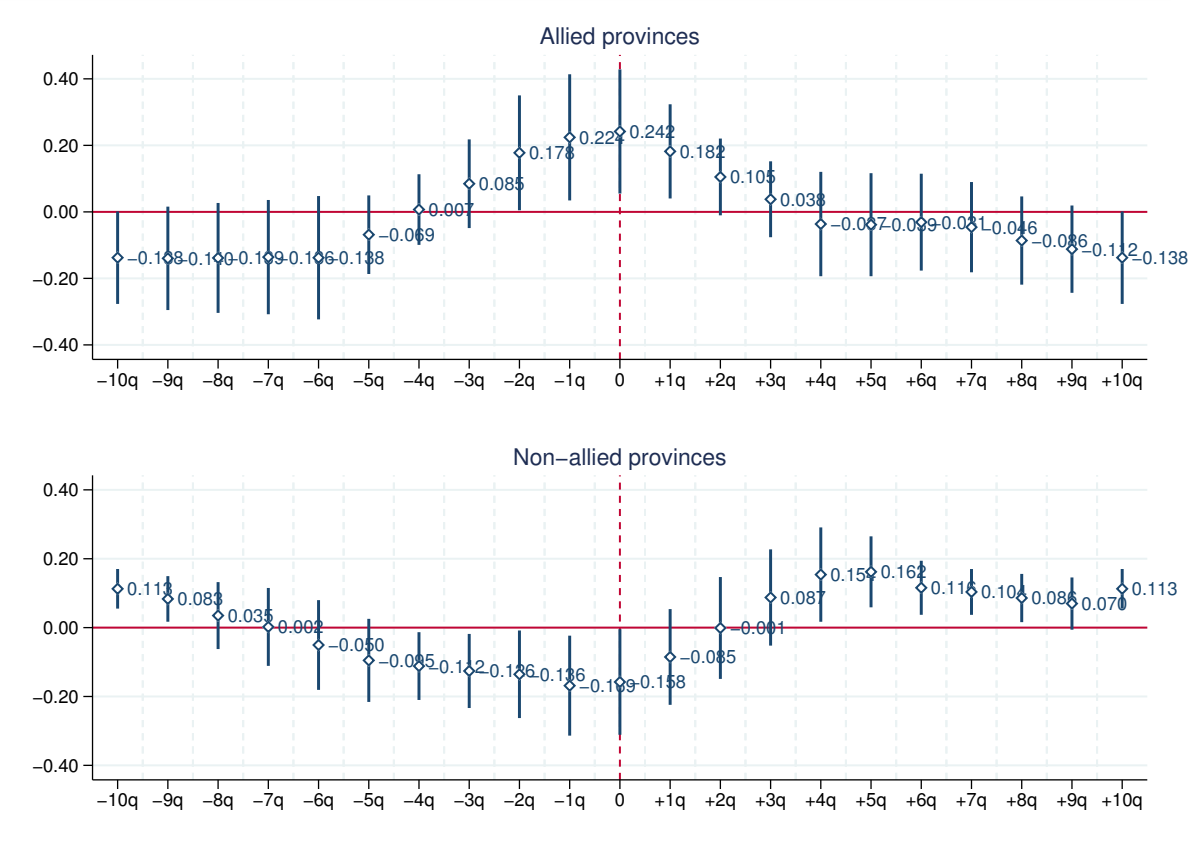
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces. Bank-province-time controls include customer deposits in continuous log form. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.9: Tactical redistribution of state-bank credit over the election cycle: Dropping three largest metropolitan cities



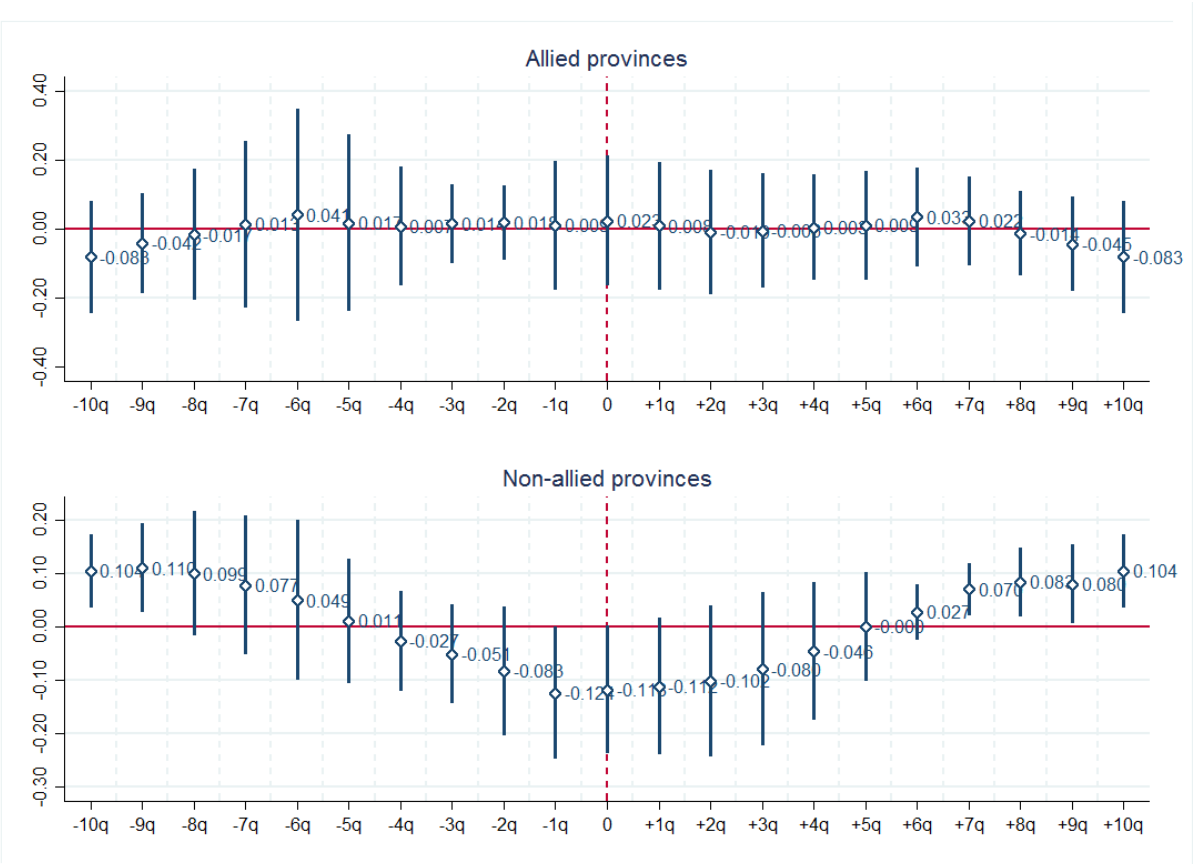
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces except the three largest ones. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.10: Tactical redistribution of state-bank credit over the election cycle: Dropping eastern metropolitan cities



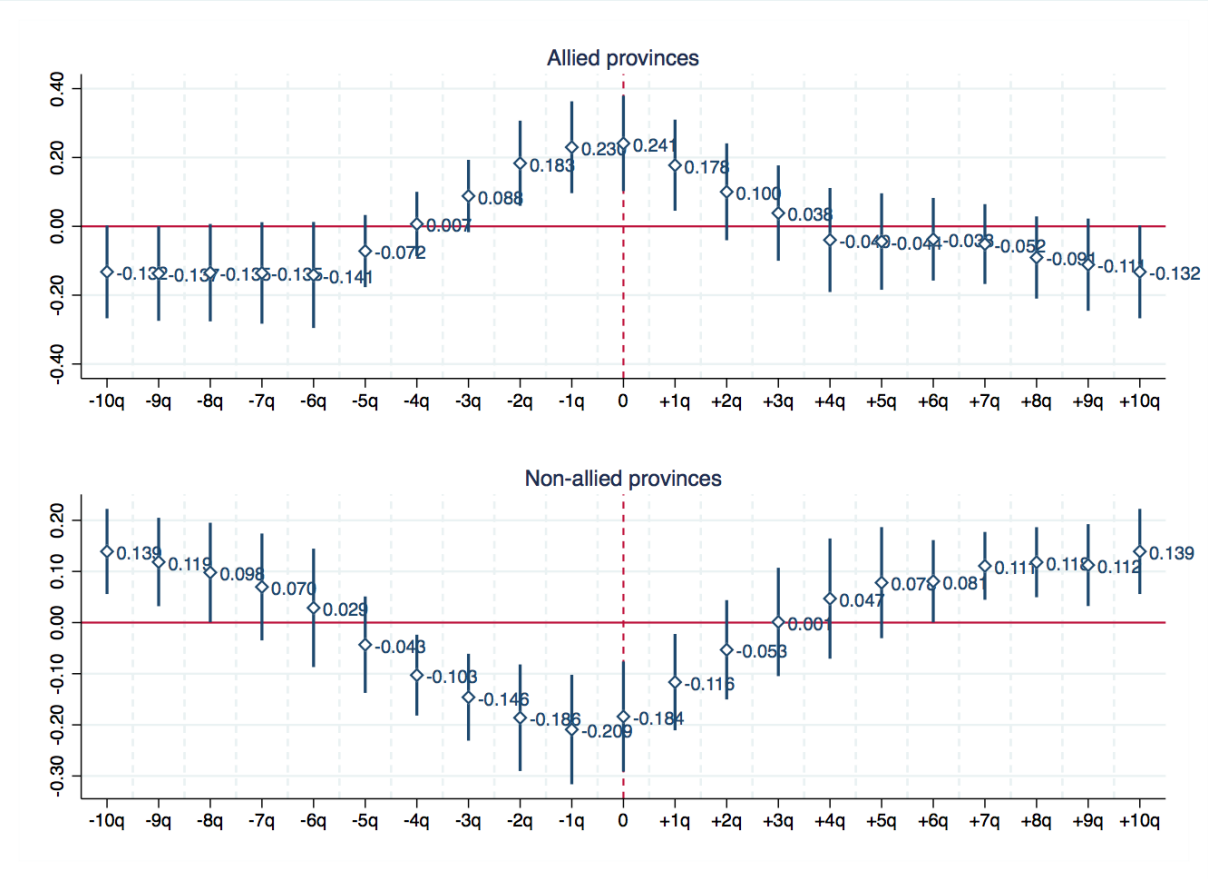
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces except four in eastern Turkey. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.11: Tactical redistribution of state-bank credit over the election cycle: Dropping metropolitan cities that changed hands



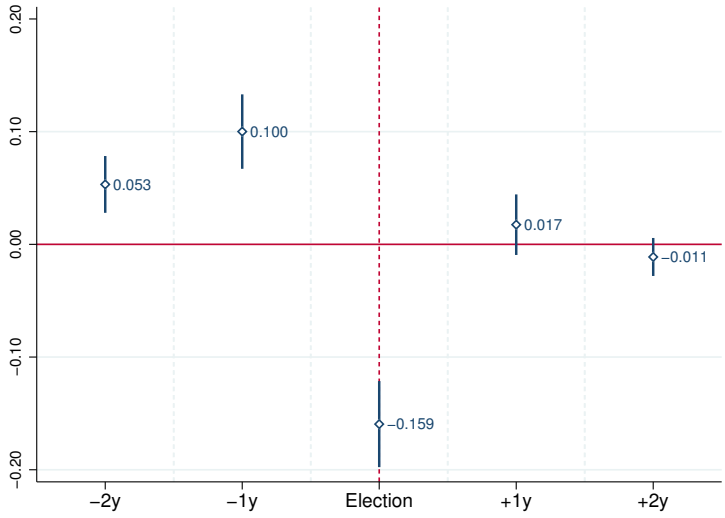
Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes metropolitan provinces except those that changed hands from one political party to another during the sample period. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.12: Tactical redistribution of state-bank credit over the election cycle: Clustering s.e.'s at province-by-bank level

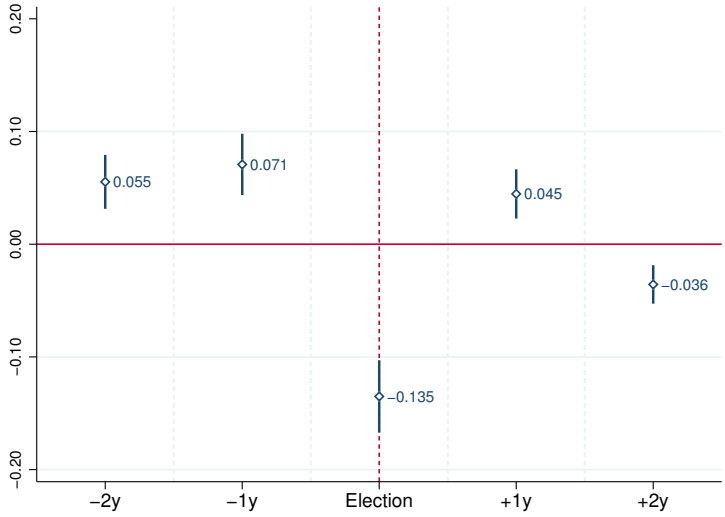


Notes: This figure shows results of Equation (2) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Standard errors are clustered at the level of province-by-bank. Sample includes metropolitan provinces. The estimation sample excludes the three largest metropolitan cities in the country. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.13: State bank lending relative to private banks over the election cycle: Yearly estimates (2003-2017)



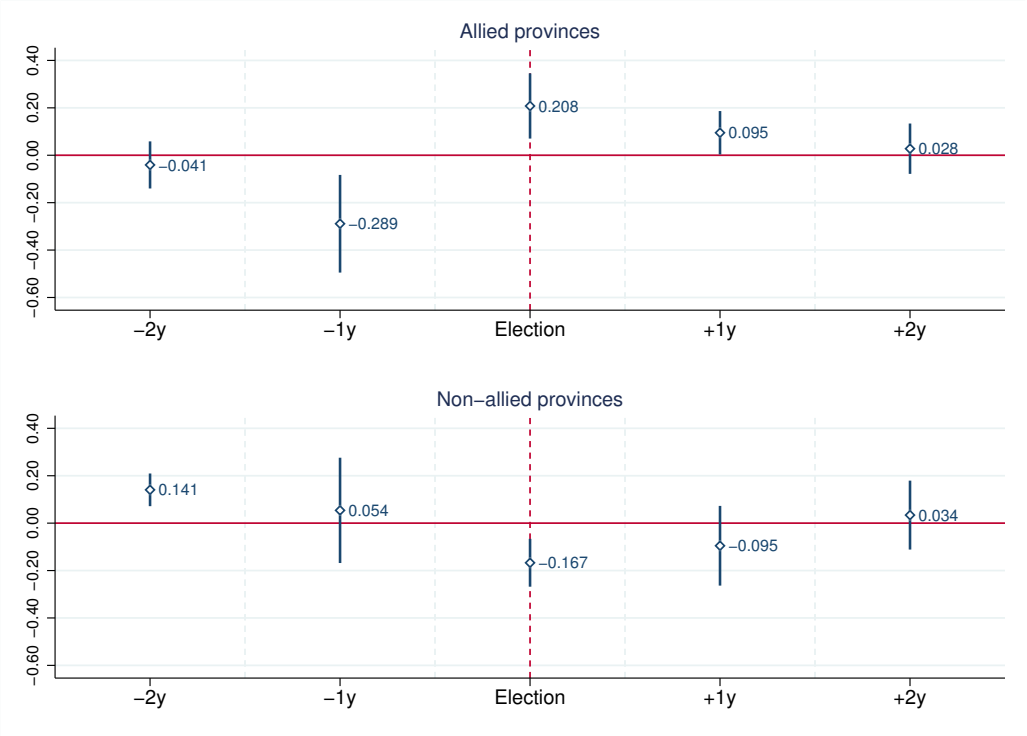
(a) Metropolitan provinces



(b) All provinces

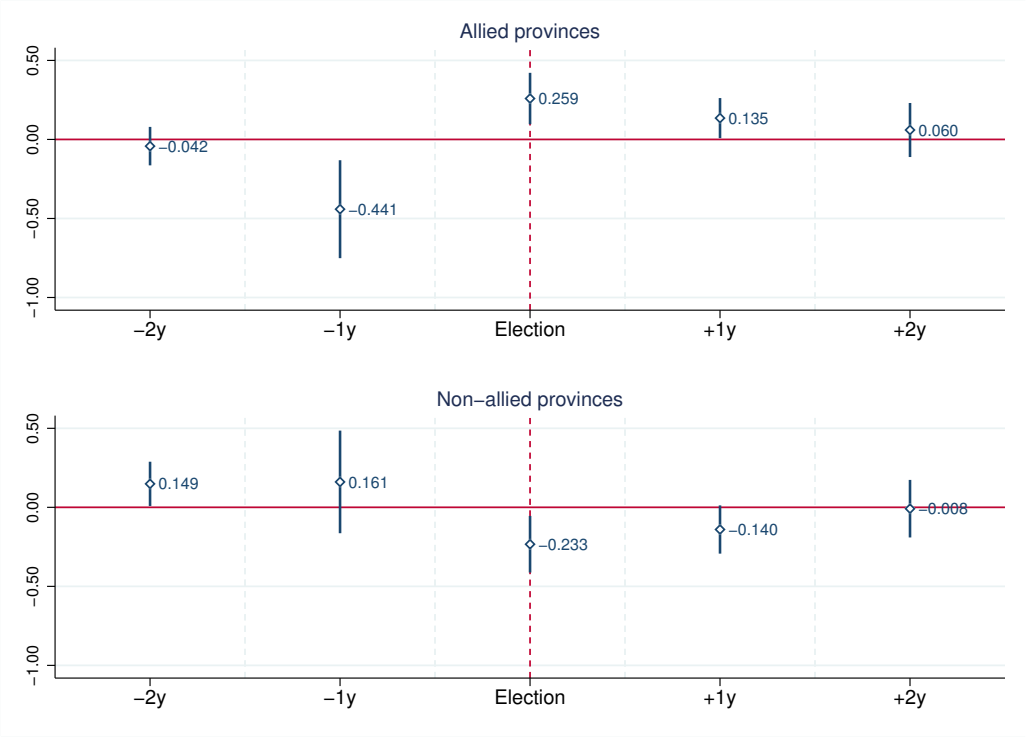
Notes: This figure shows results of Equation (1) estimated on yearly data when τ takes values from -2 to +2, indicating the number of years around elections. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends. Panel A includes metropolitan provinces and Panel B includes the full sample.

Figure B.14: Tactical redistribution of state-bank credit over the election cycle: Yearly estimates (2003-2017)

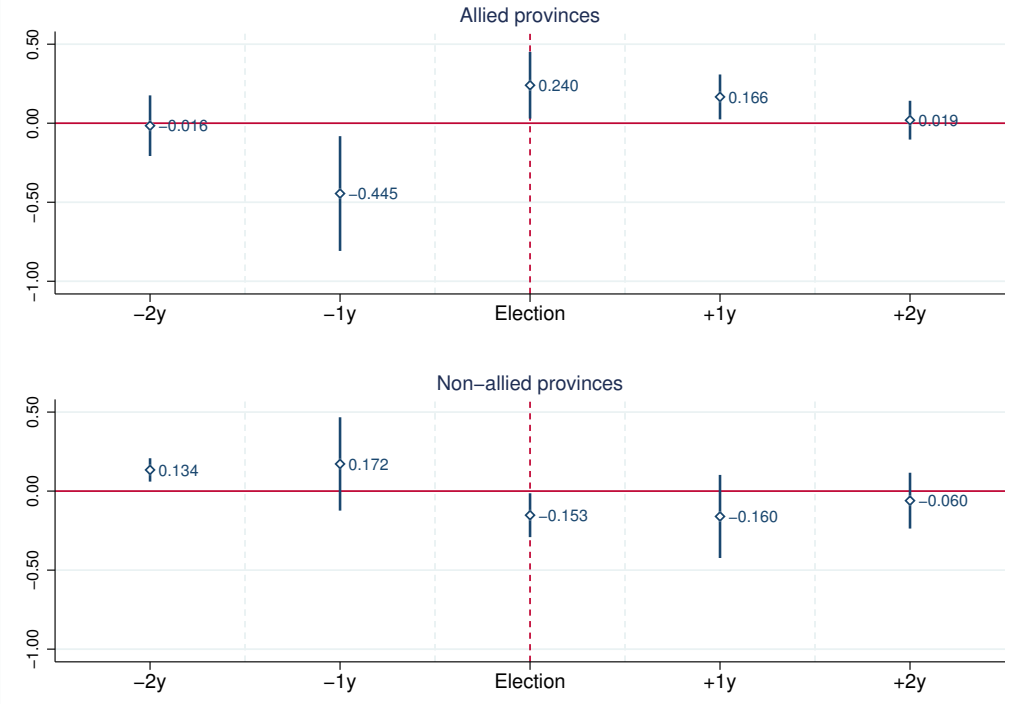


Notes: This figure shows results of Equation (2) estimated on yearly data when τ takes values from -2 to +2, indicating the number of years around elections. Sample includes metropolitan provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

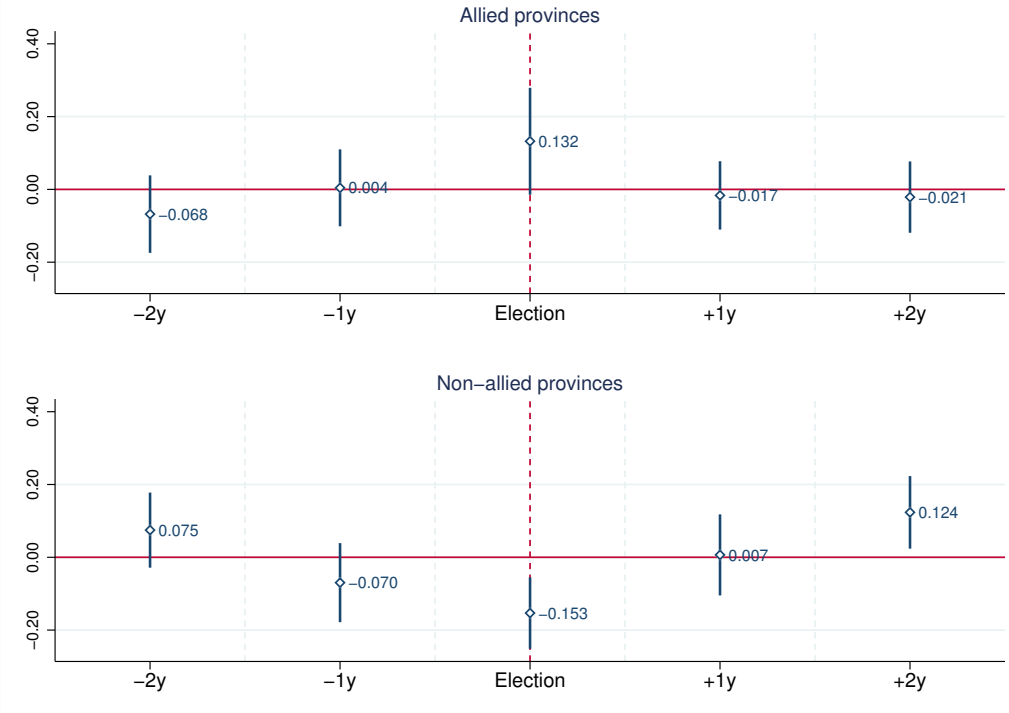
Figure B.15: Tactical redistribution of state-bank credit over the election cycle: Dropping one local election at a time with yearly data



(a) Sample: Excluding 2014 election cycle



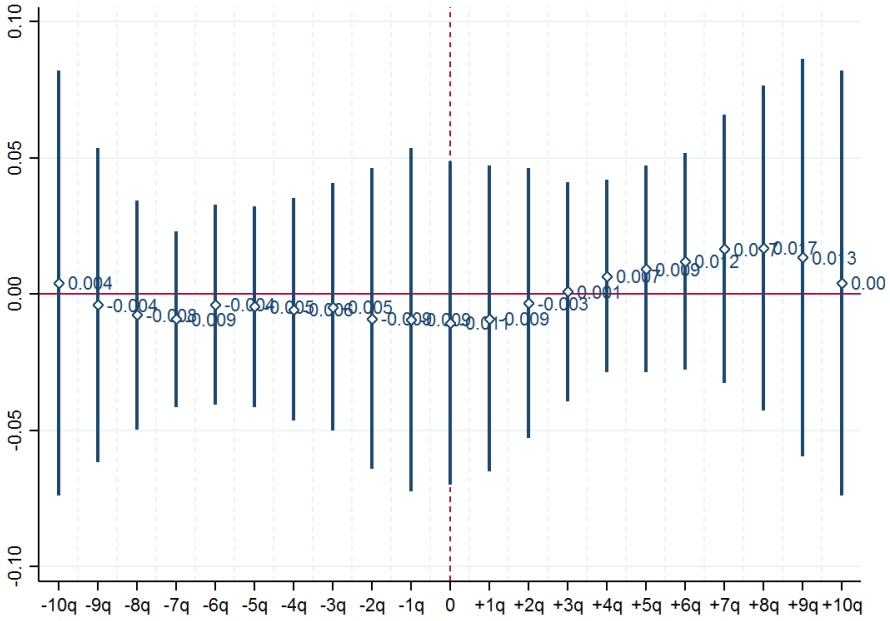
(b) Sample: Excluding 2009 election cycle



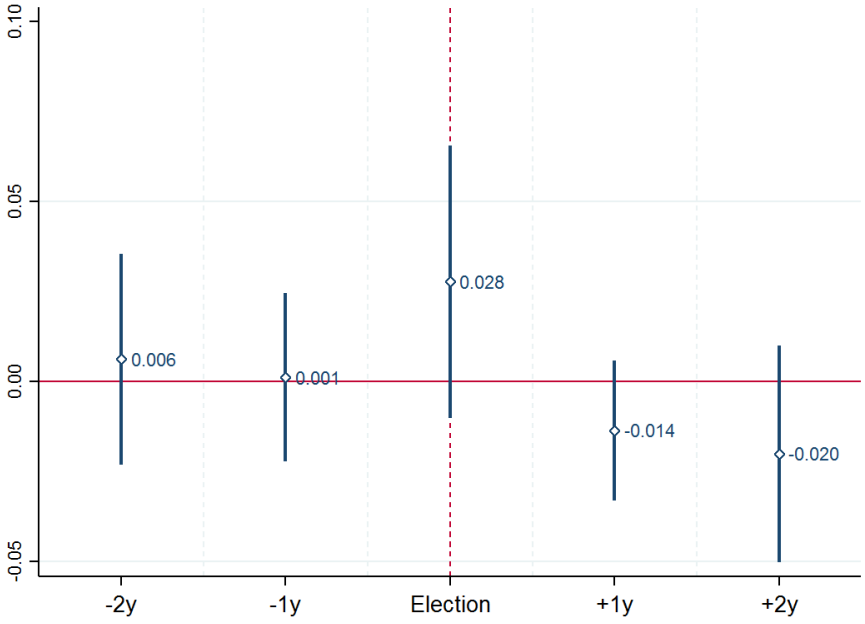
(c) Sample: Excluding 2004 election cycle

Notes: This figure shows results of Equation (2) estimated on yearly data (2003-2017) when τ takes values from -2 to +2, indicating the number of years around elections. Sample includes metropolitan provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression our baseline controls and province time trends.

Figure B.16: Political uncertainty and bank lending



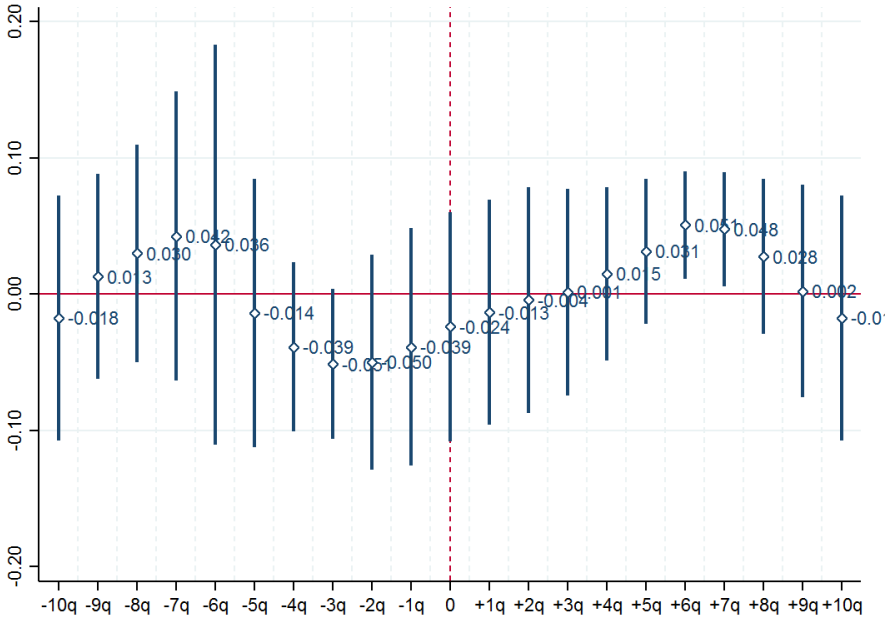
(a) Sample: Quarterly data, 2007q4-2017q4



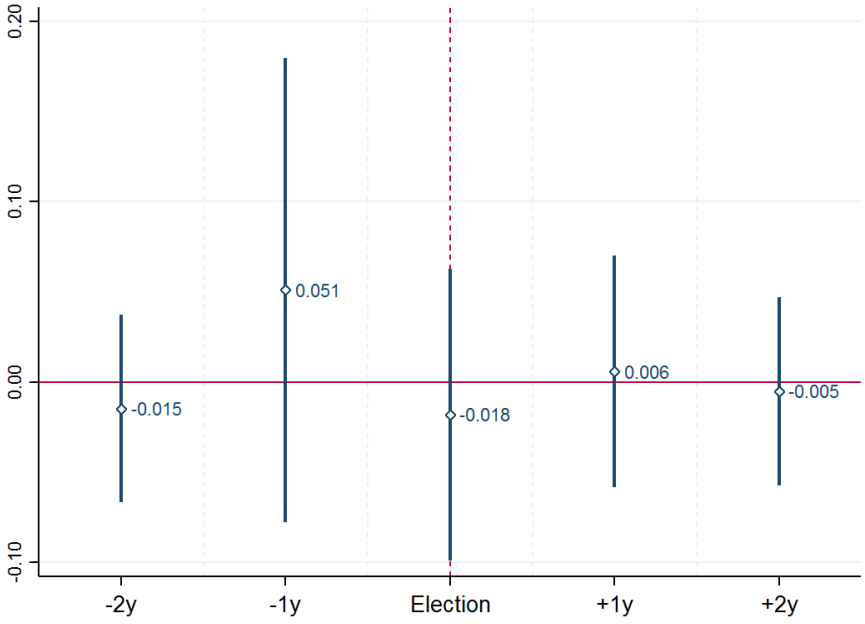
(b) Sample: Yearly data, 2003-2017

Notes: This figure shows results of Equation (14) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10 in panel A, and on yearly data (2003-2017) when τ takes values from -2 to +2 in panel B. Sample includes metropolitan provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

Figure B.17: Political uncertainty and state-bank lending relative to private banks



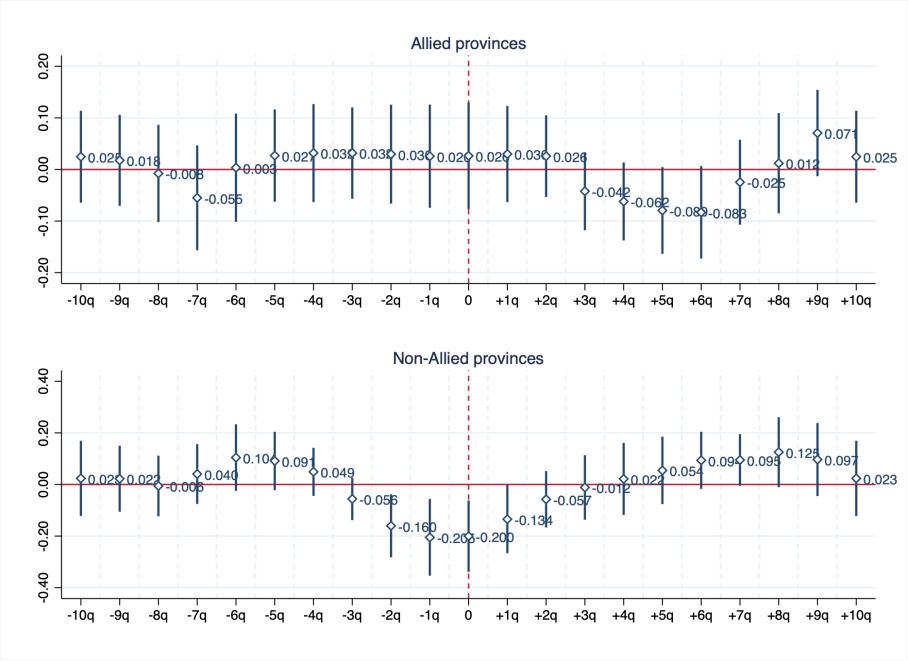
(a) Sample: Quarterly data, 2007q4-2017q4



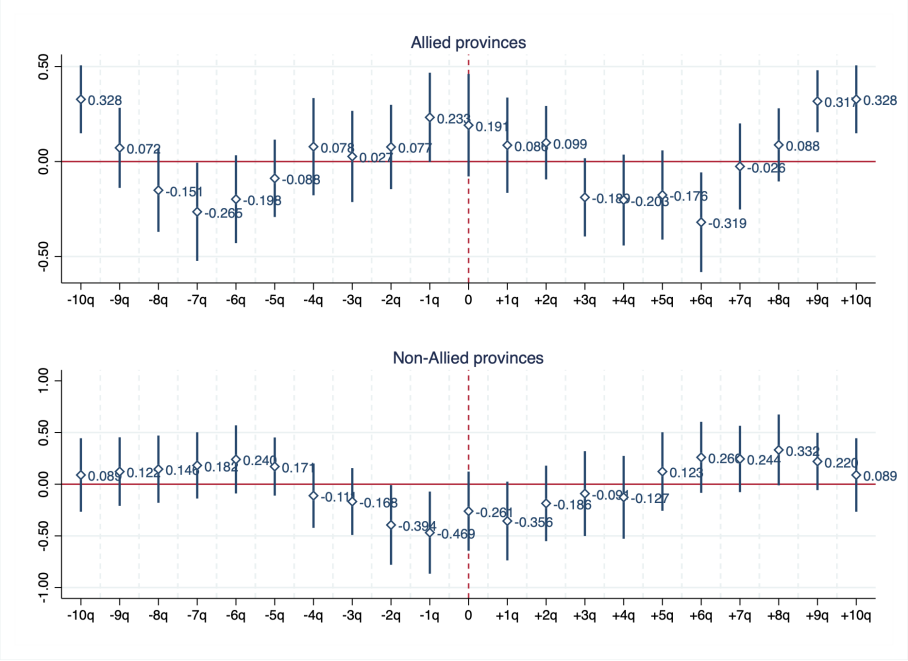
(b) Sample: Yearly data, 2003-2017

Notes: This figure shows results of Equation (15) estimated on quarterly data (2007q4-2017q4) when τ takes values from -10 to +10 in panel A, and on yearly data (2003-2017) when τ takes values from -2 to +2 in panel B. Sample includes metropolitan provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends.

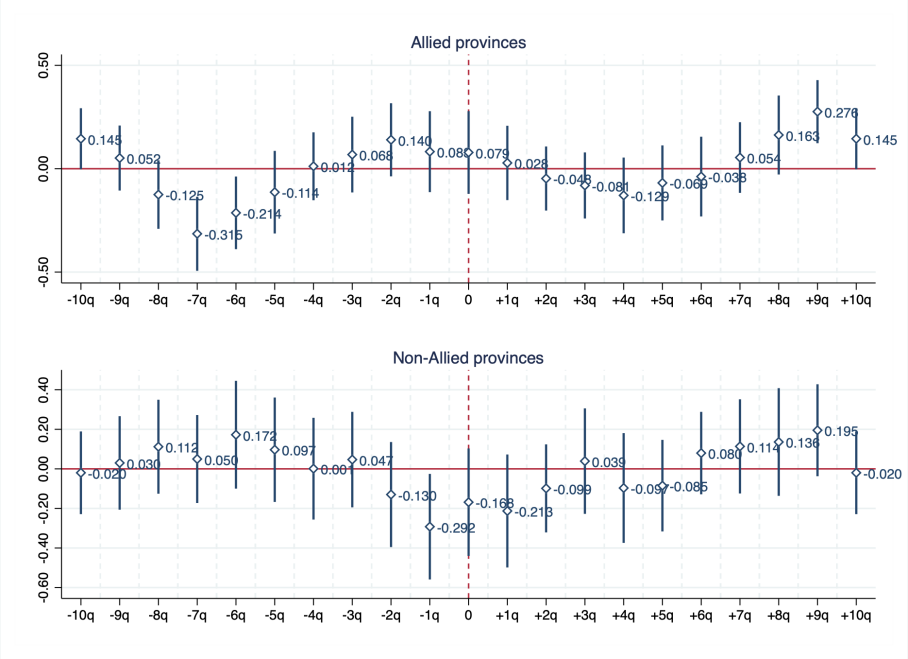
Figure B.18: Is there a political cycle in investment incentive certificates?



(a) Dependent variable: (log) Number of investment incentive certificates awarded



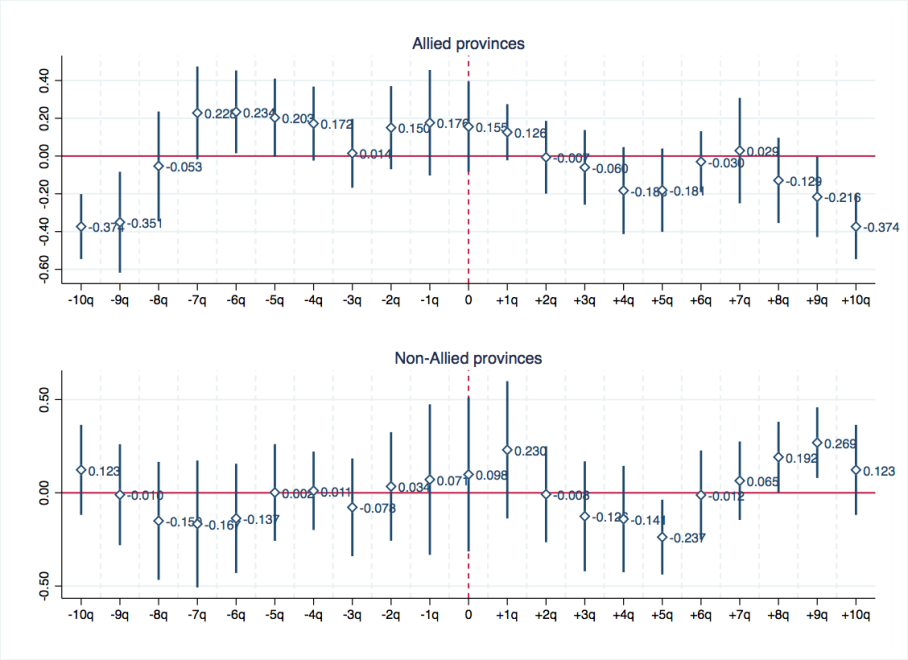
(b) Dependent variable: (log) Number of jobs promised by certificate recipients



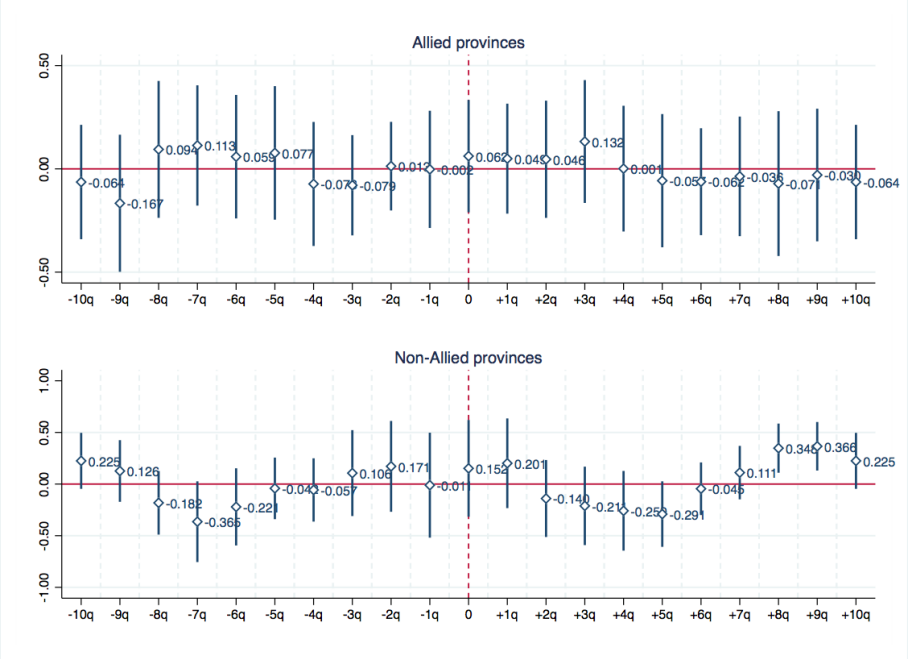
(c) Dependent variable: (log) Capital investment promised by certificate recipients

Notes: This figure shows results of Equation (16) estimated on quarterly data (2003q1-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes all provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for province and time fixed effects.

Figure B.19: Is there a political cycle in new public construction?



(a) Dependent variable: Number of new buildings started by public sector



(b) Dependent variable: Total building area started by public sector

Notes: This figure shows results of Equation (16) estimated on quarterly data (2003q1-2017q4) when τ takes values from -10 to +10, indicating the number of quarters around elections. Sample includes all provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for province and time fixed effects.

Table B.1: Tactical reallocation: Inference with wild-boostrap standard errors

	Aligned provinces			*Non-aligned provinces*		
	(1)	(2)	(3)	(4)	(5)	(6)
Comp x StateBank x Election	0.241***	0.241***	0.191**	-0.184**	-0.183**	-0.175**
	0.010	*0.010*	*0.010*	*0.012*	*0.013*	*0.021*
Local branches	Yes	Yes	Yes	Yes	Yes	Yes
Bank type FE	Yes	Yes		Yes	Yes	
Province FE	Yes			Yes		
Time FE	Yes			Yes		
Province-Time trends	Yes			Yes		
Province x Time FE		Yes	Yes		Yes	Yes
Bank x Time FE			Yes			Yes
N	1,380	1,380	1,380	1,080	1,080	1,080
R^2	0.887	0.829	0.839	0.923	0.888	0.895

Notes: This table shows results of Equation (2) estimated on quarterly data (2007q4-2017q4). Estimates are shown only for the triple interaction term. Standard errors are clustered at the province level and p-values are calculated by wild bootstrap method with the assumption of no null imposed. P-values are reported in italics below each coefficient; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table B.2: State-bank lending in election years: Yearly estimates (2003-2017)

	Metropolitan sample		*Full sample*	
	(1)	(2)	(3)	(4)
State Bank x Election	-0.159***	-0.159***	-0.135***	-0.136***
	[0.022]	[0.030]	[0.019]	[0.026]
Local branches; bank type FE	Yes	Yes	Yes	Yes
Province FE	Yes		Yes	
Time FE	Yes		Yes	
Province-Time trends	Yes		Yes	
Province x Time FE		Yes		Yes
N	900	900	2,430	2,430
R^2	0.924	0.878	0.874	0.799

Notes: This table shows results of Equation (1) estimated on yearly data. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

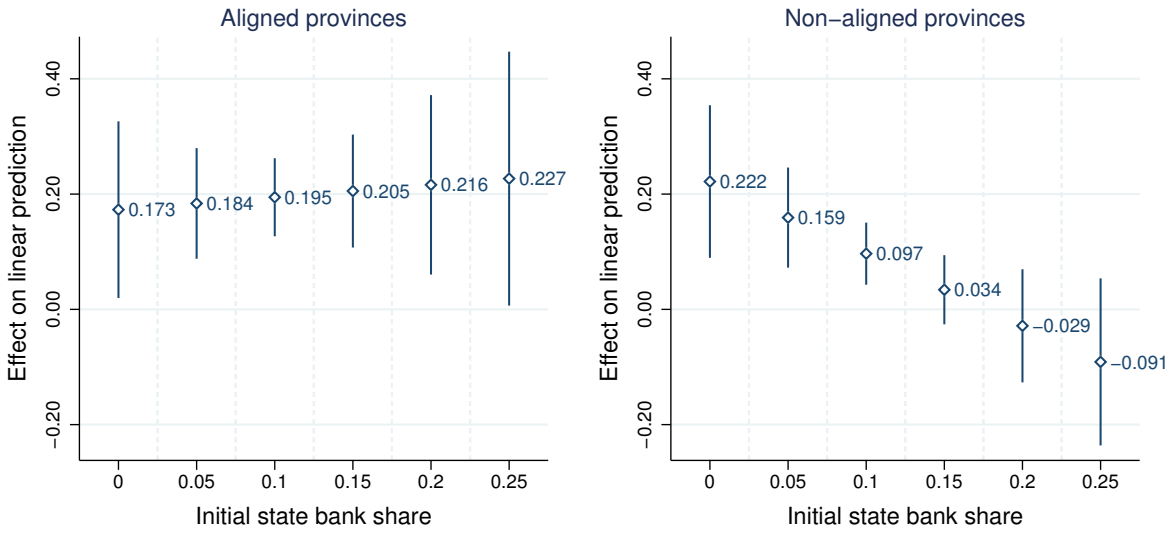
Table B.3: Tactical reallocation in metropolitan provinces: Yearly estimates (2003-2017)

	Aligned provinces				*Non-aligned provinces*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Comp x StateBank x Election	0.208**	0.208**	0.208*	0.198**	-0.167***	-0.167**	-0.167**	-0.127**
	[0.078]	[0.080]	[0.105]	[0.092]	[0.056]	[0.058]	[0.075]	[0.052]
Comp x StateBank	-0.155	-0.155	-0.155	-0.155	0.038	0.036	0.039	-0.041
	[0.151]	[0.154]	[0.203]	[0.235]	[0.137]	[0.138]	[0.177]	[0.169]
StateBank x Election	-0.212***	-0.212***	-0.211**		-0.059*	-0.059	-0.059	
	[0.056]	[0.058]	[0.077]		[0.033]	[0.034]	[0.044]	
Comp x Election	-0.171***	-0.171***			0.157***	0.152***		
	[0.034]	[0.043]			[0.025]	[0.027]		
Comp	0.159*	0.057			0.136	0.072		
	[0.088]	[0.092]			[0.124]	[0.074]		
Local branches	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank type FE	Yes	Yes	Yes		Yes	Yes	Yes	
Province FE	Yes	Yes			Yes	Yes		
Time FE	Yes	Yes			Yes	Yes		
Province-Time trends		Yes				Yes		
Province x Time FE			Yes	Yes			Yes	Yes
Bank x Time FE				Yes				Yes
N	534	534	534	534	366	366	366	366
R^2	0.904	0.908	0.851	0.868	0.936	0.944	0.915	0.939

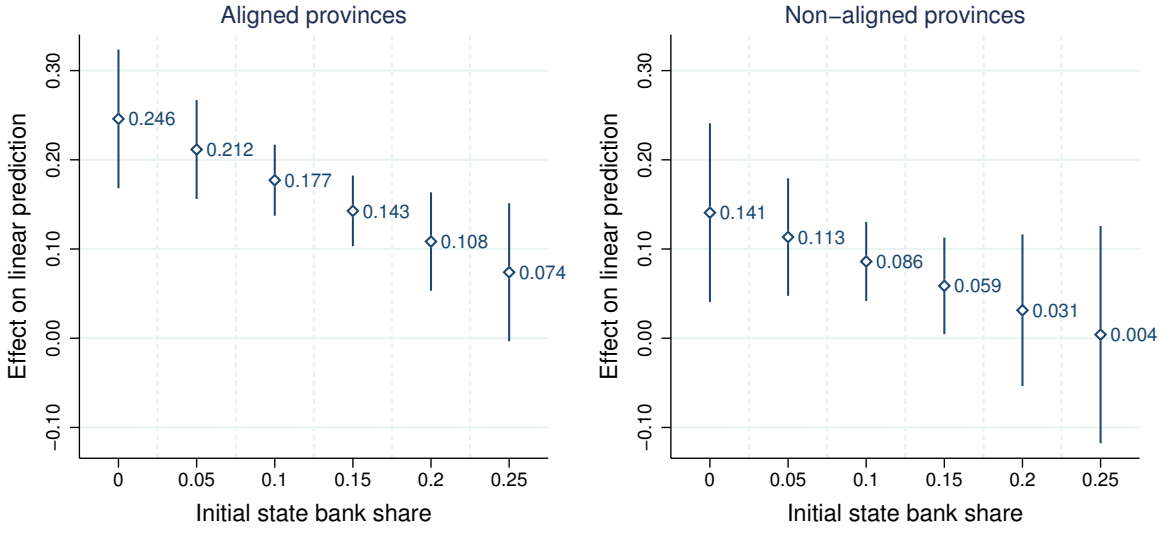
Notes: This table shows results of Equation (2) estimated on yearly data. Columns (1)-(4) include politically aligned provinces and columns (5)-(8) include non-aligned provinces. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Appendix C

Figure C.1: Average marginal effects on corporate borrowing



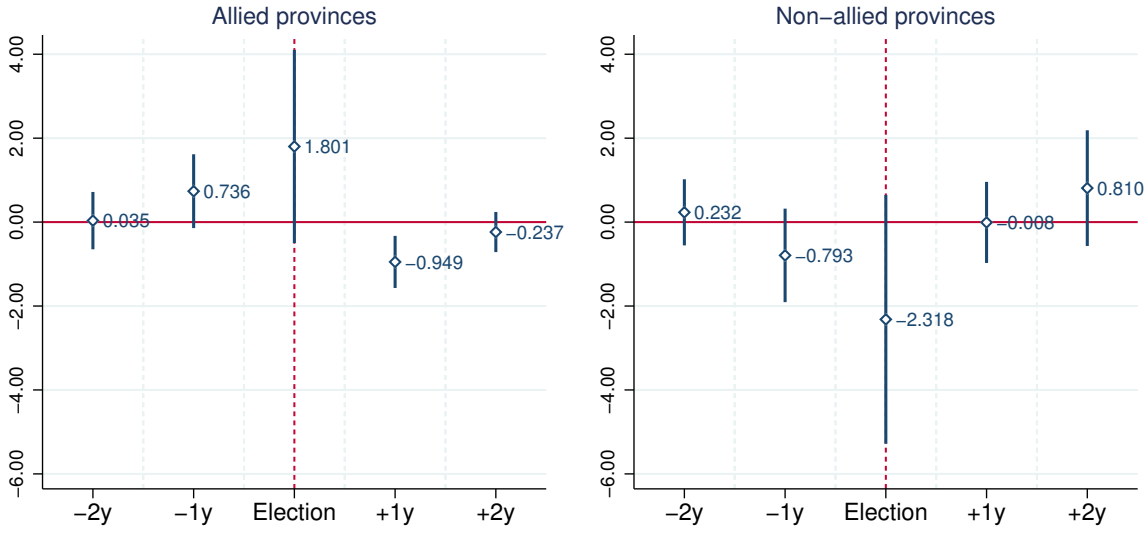
(a) Average marginal effect in contested provinces



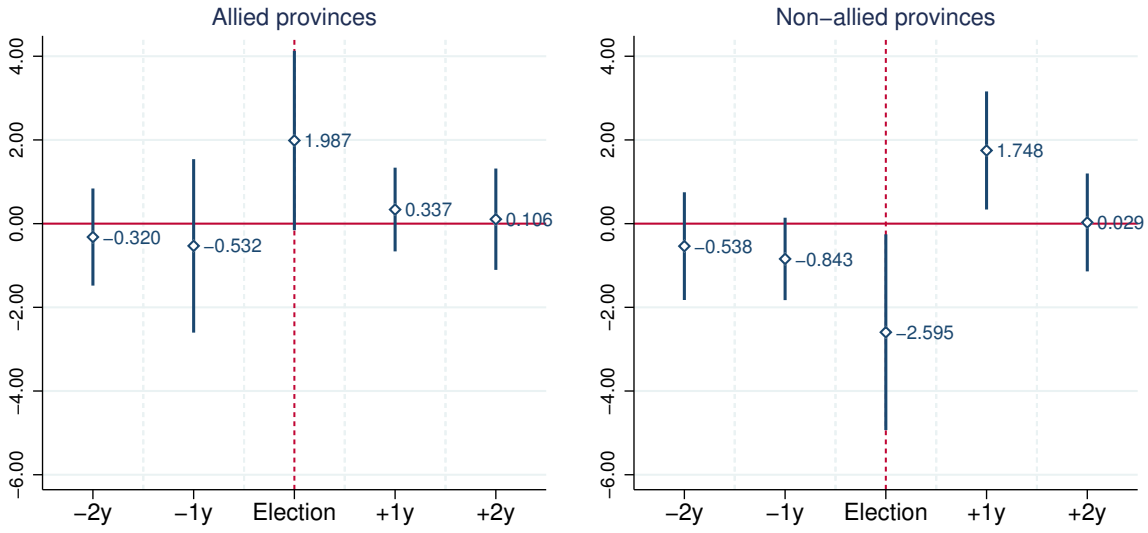
(b) Average total effect by alignment

Notes: This figure shows the average marginal effect of $Election_t$ calculated from Equation (3) when $Election_t$ is included and time FE are dropped. The dependent variable is (log) bank debt. See columns (3) and (8) of Table C.2 for the underlying coefficient estimates. Panel A shows the average marginal effect in politically contested and Panel B shows the average total effect across both contested and uncontested provinces. Marginal effects are shown for different values of state banks' initial market shares.

Figure C.2: Effects of political lending on short-term and long-term borrowing



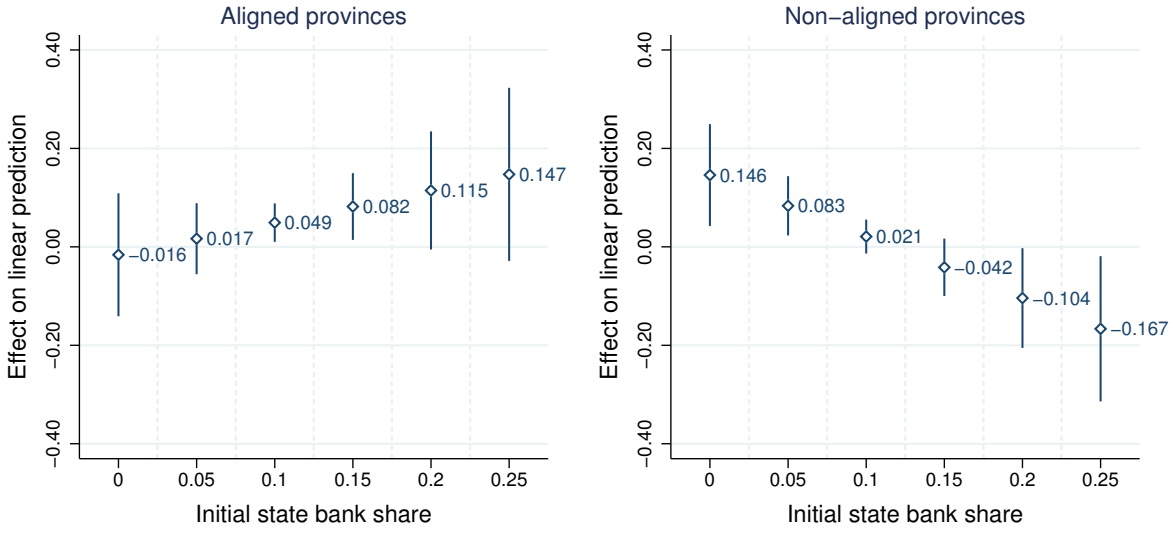
(a) Dependent variable: Short-term bank debt



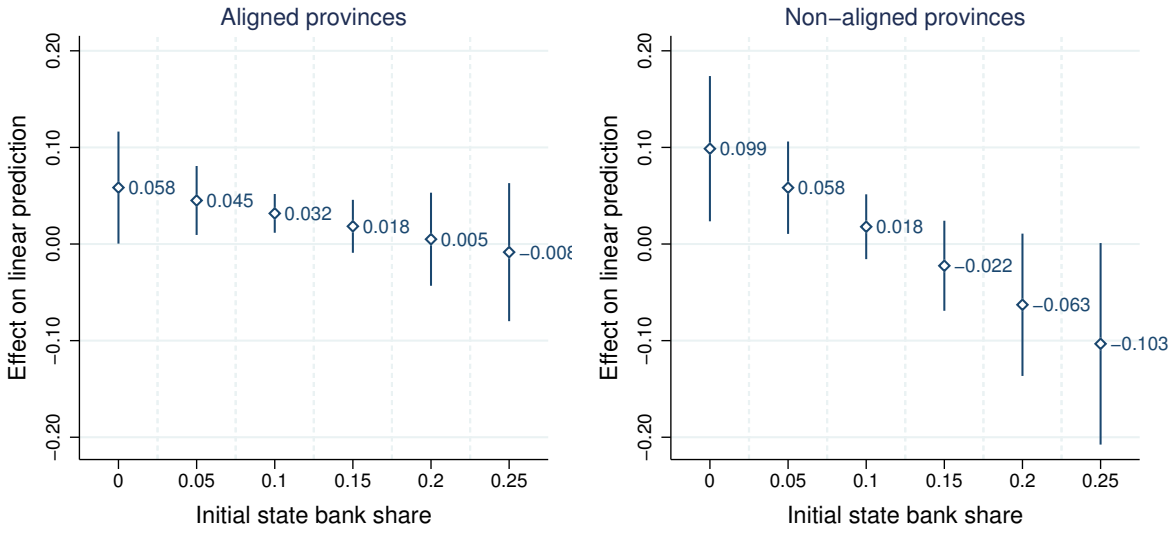
(b) Dependent variable: Long-term bank debt

Notes: This figure shows results of Equation (3) estimated on yearly data (2006-2016) when τ takes values from -2 to +2, indicating the number of years around elections, for manufacturing industries in metropolitan provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for a set of fixed effects and province time trends.

Figure C.3: Total effects on assets by political alignment



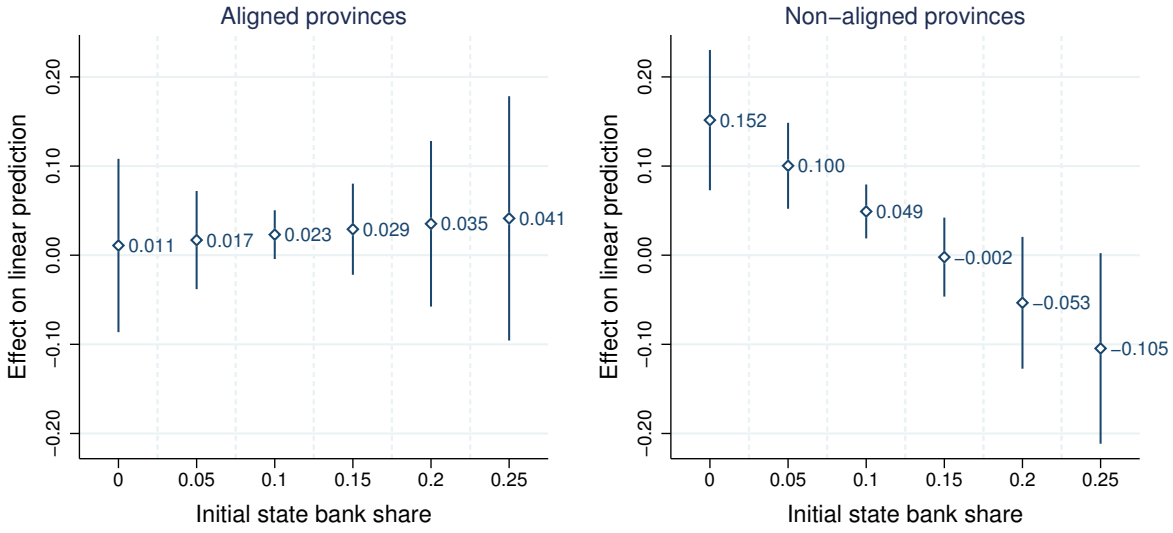
(a) Average marginal effect in contested provinces



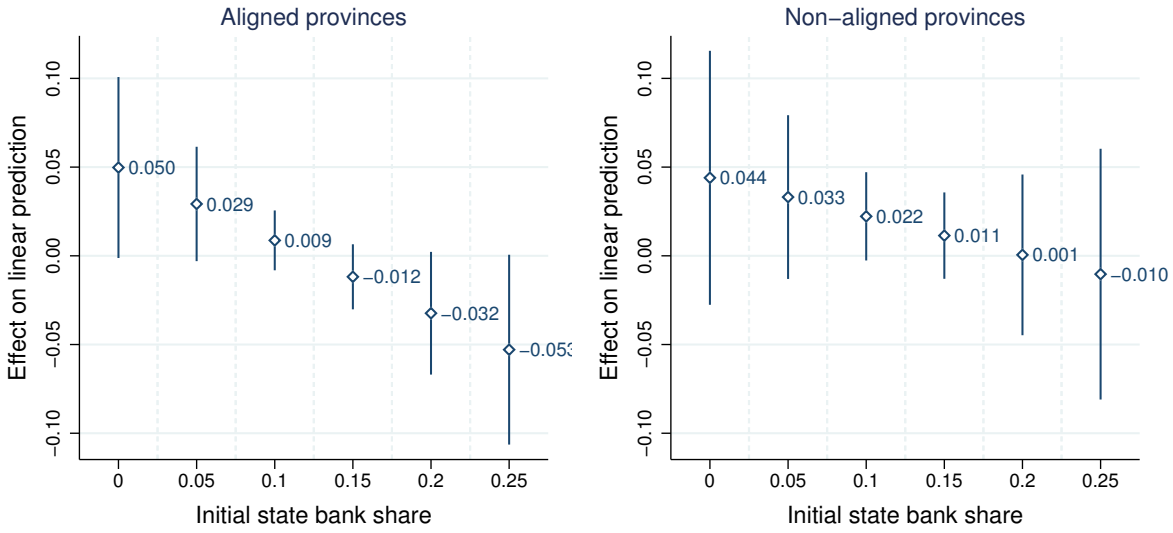
(b) Average total effect by alignment

Notes: This figure shows the average marginal effect of $Election_t$ calculated from Equation (3) when $Election_t$ is included and time FE are dropped. The dependent variable is (log) total assets. See columns (3) and (8) of Table C.4 for the underlying coefficient estimates. Panel A shows the average marginal effect in politically contested and Panel B shows the average total effect across both contested and uncontested provinces. Marginal effects are shown for different values of state banks' initial market shares.

Figure C.4: Total effects on employment by political alignment



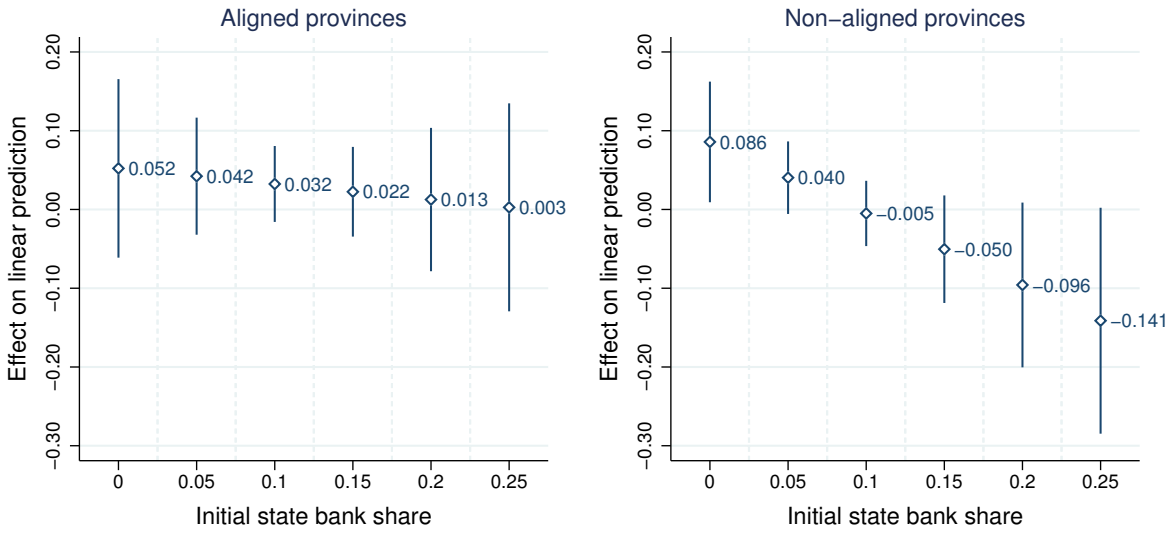
(a) Average marginal effect in contested provinces



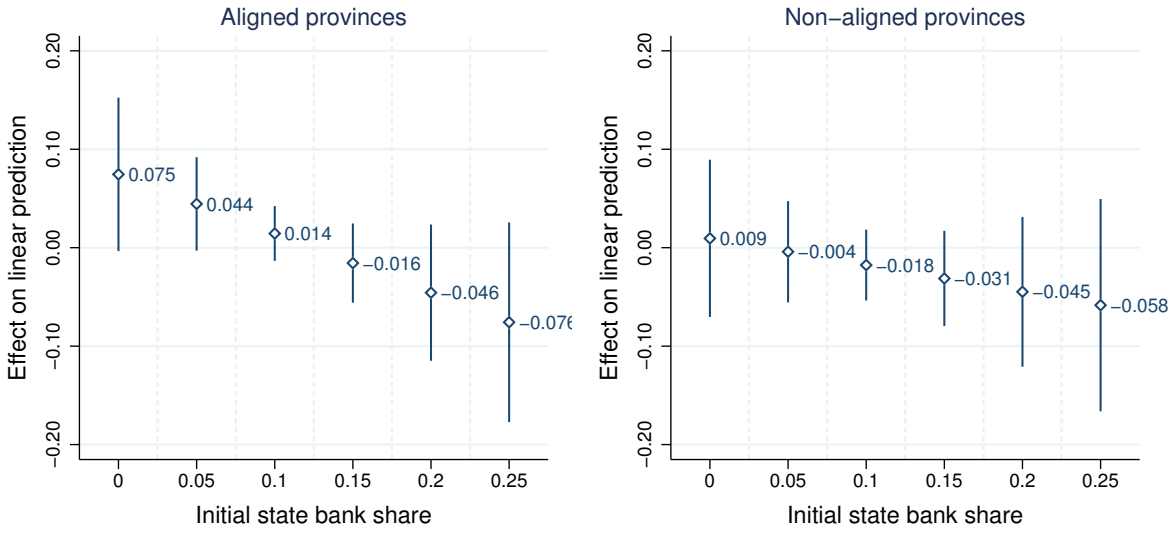
(b) Average total effect by alignment

Notes: This figure shows the average marginal effect of $Election_t$ calculated from Equation (3) when $Election_t$ is included and time FE are dropped. The dependent variable is (log) employment. See columns (3) and (8) of Table C.5 for the underlying coefficient estimates. Panel A shows the average marginal effect in politically contested and Panel B shows the average total effect across both contested and uncontested provinces. Marginal effects are shown for different values of state banks' initial market shares.

Figure C.5: Total effects on net sales by political alignment



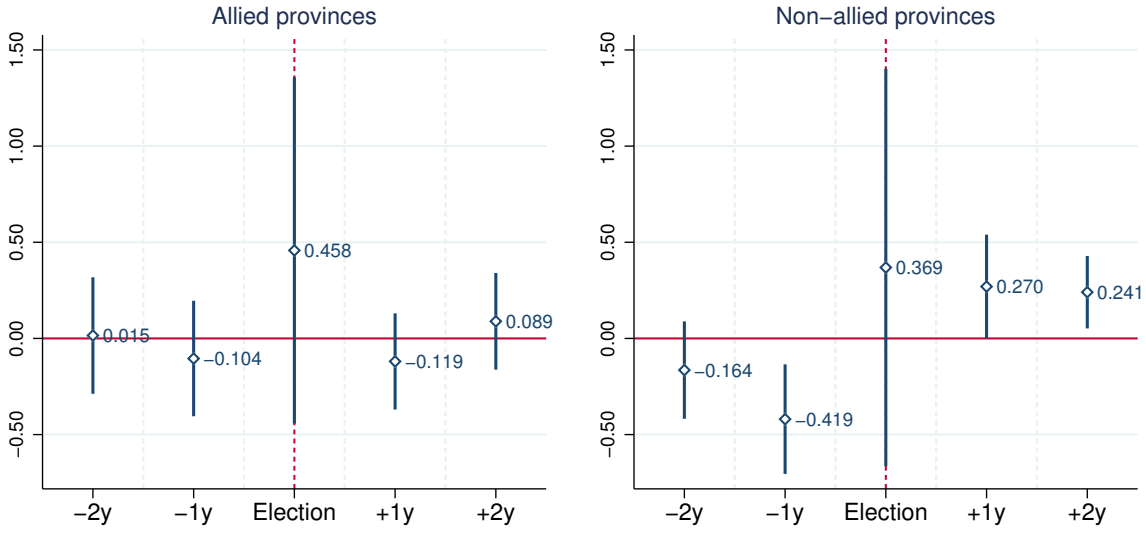
(a) Average marginal effect in contested provinces



(b) Average total effect by alignment

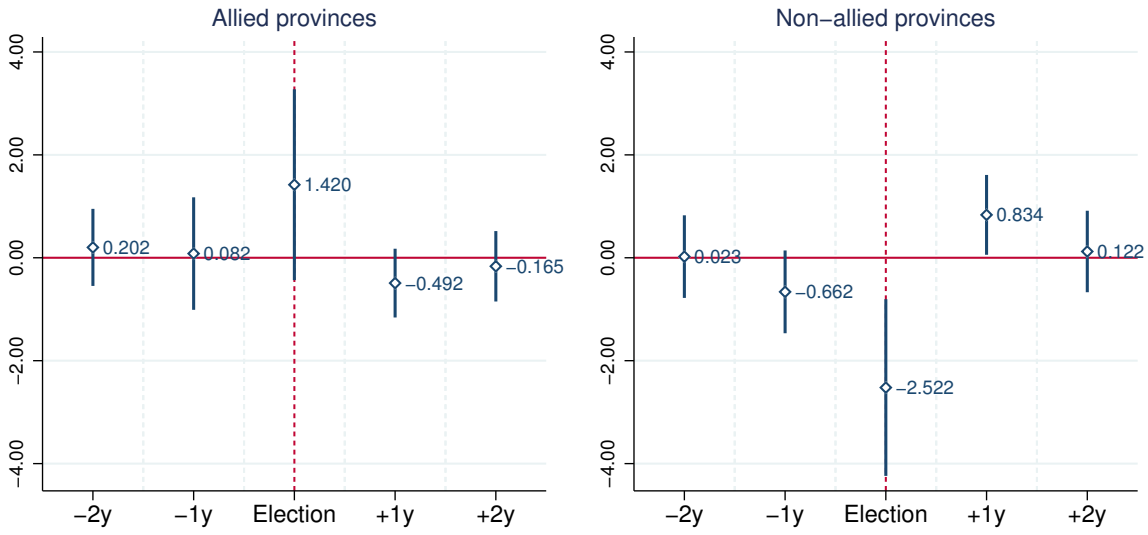
Notes: This figure shows the average marginal effect of $Election_t$ calculated from Equation (3) when $Election_t$ is included and time FE are dropped. The dependent variable is (log) net sales. See columns (3) and (8) of Table C.6 for the underlying coefficient estimates. Panel A shows the average marginal effect in politically contested and Panel B shows the average total effect across both contested and uncontested provinces. Marginal effects are shown for different values of state banks' initial market shares.

Figure C.6: Effects of political lending on number of enterprises

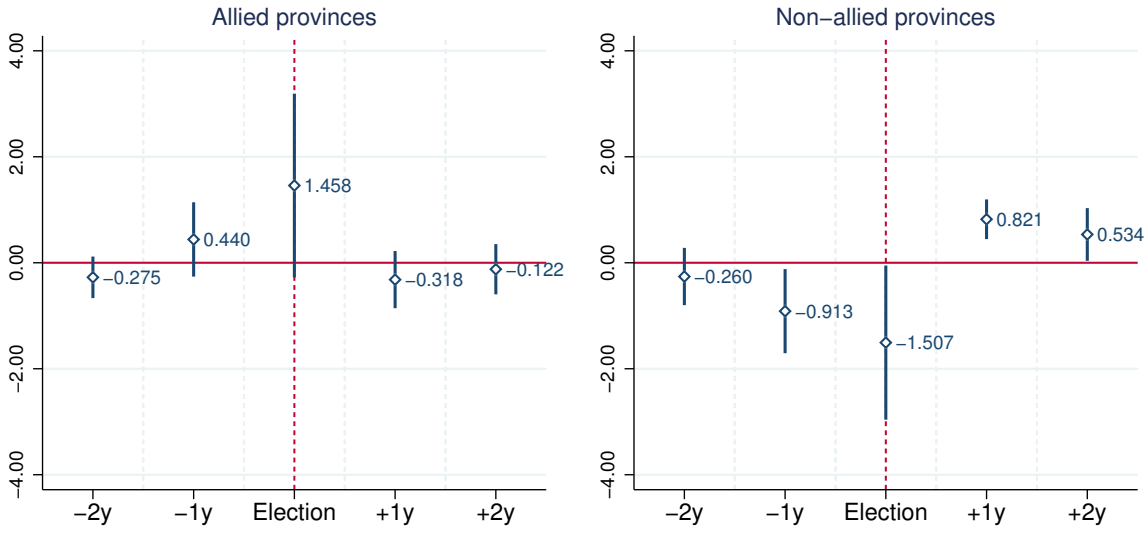


Notes: This figure shows results of Equation (3) estimated on yearly data (2006-2016) when τ takes values from -2 to +2, indicating the number of years around elections, for manufacturing industries in metropolitan provinces. The dependent variable is (log) number of enterprises in operation. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for a set of fixed effects and province time trends.

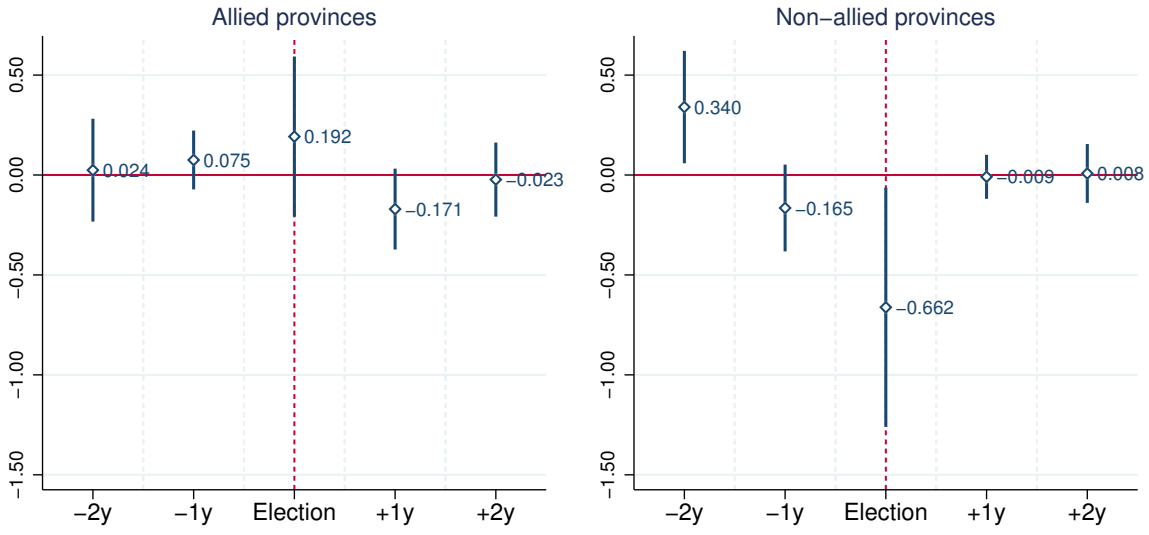
Figure C.7: Effects of political lending on corporate activity in all sectors



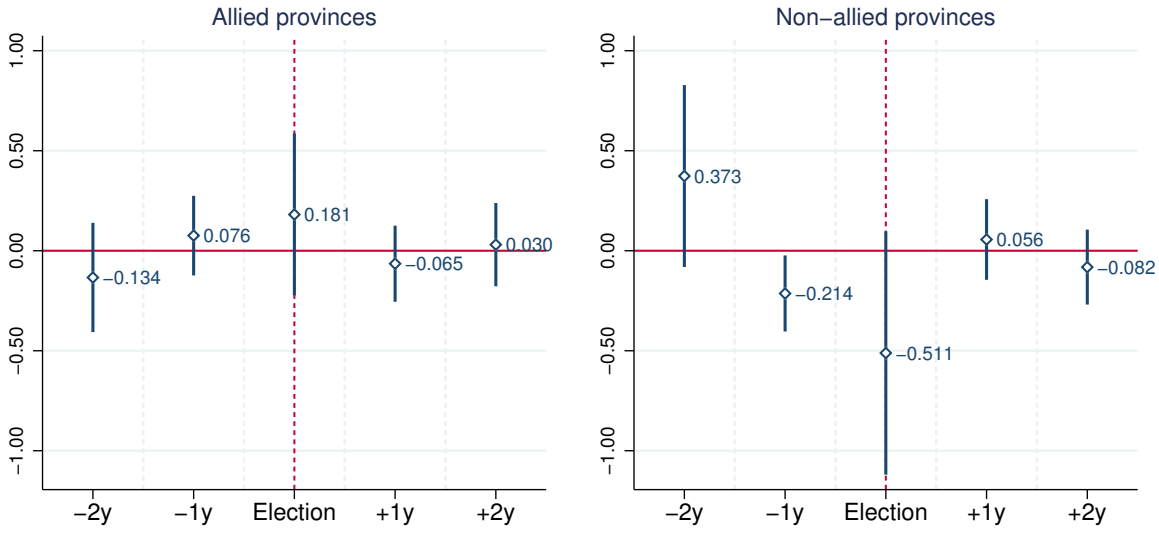
(a) Dependent variable: Bank debt



(b) Dependent variable: Total assets



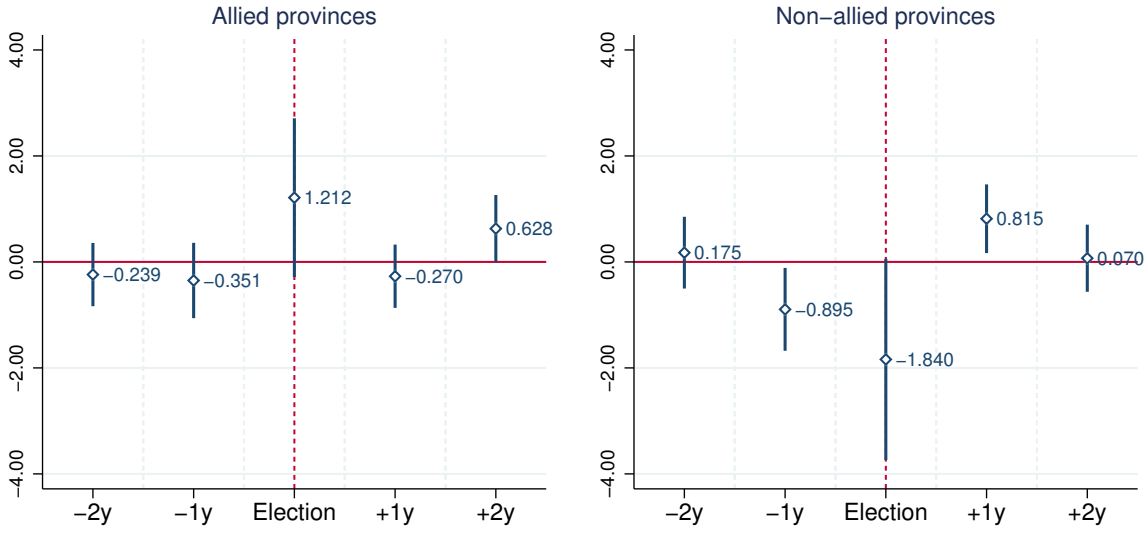
(c) Dependent variable: Employment



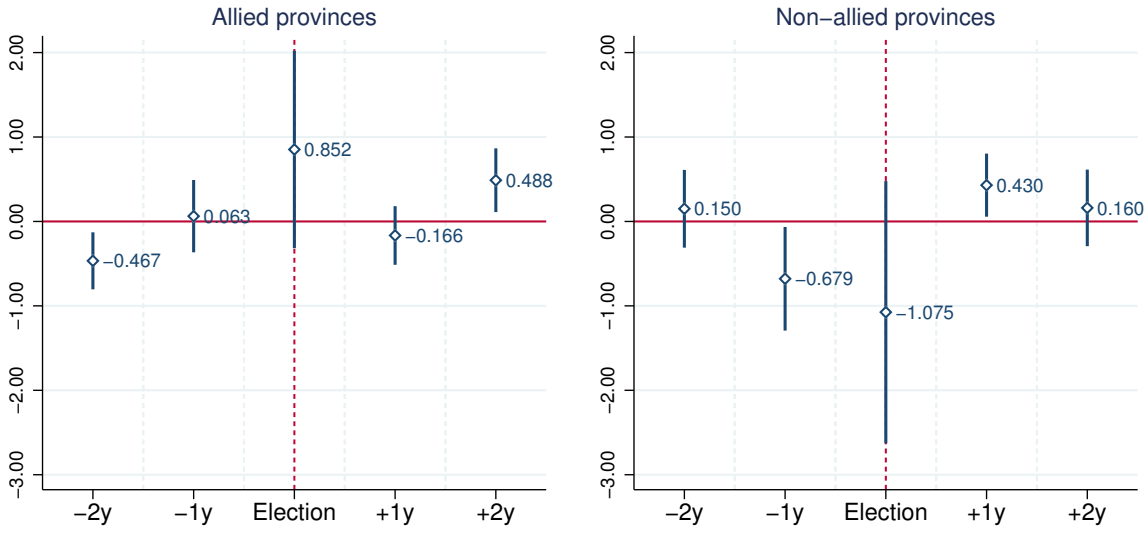
(d) Dependent variable: Net sales

Notes: This figure shows results of Equation (3) estimated on yearly data (2006-2015) when τ takes values from -2 to +2, indicating the number of years around elections, in metropolitan provinces and including all sectors of the economy. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for a set of fixed effects and province time trends.

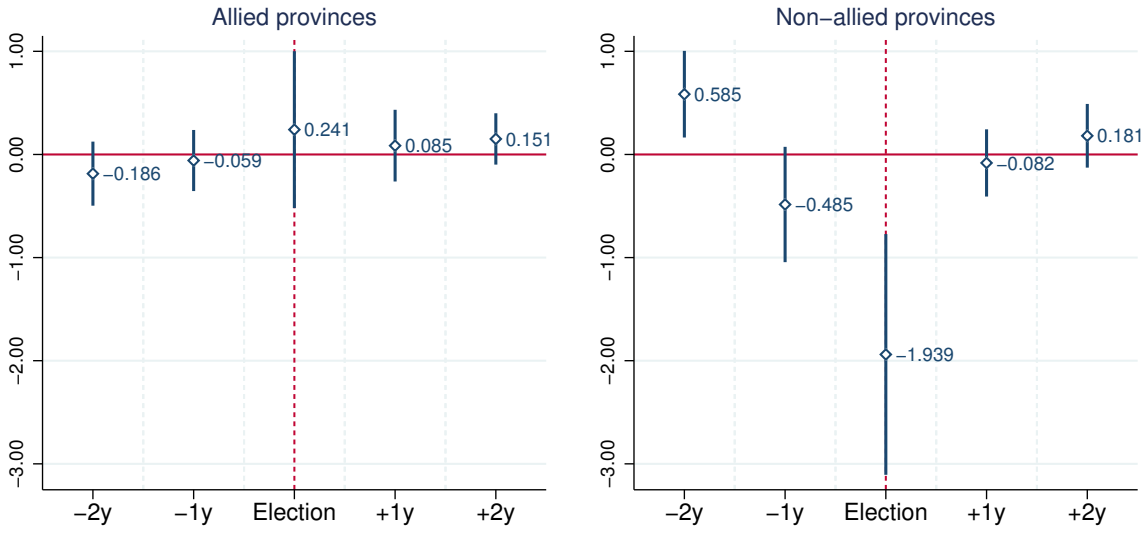
Figure C.8: Effects of political lending on corporate activity across all provinces



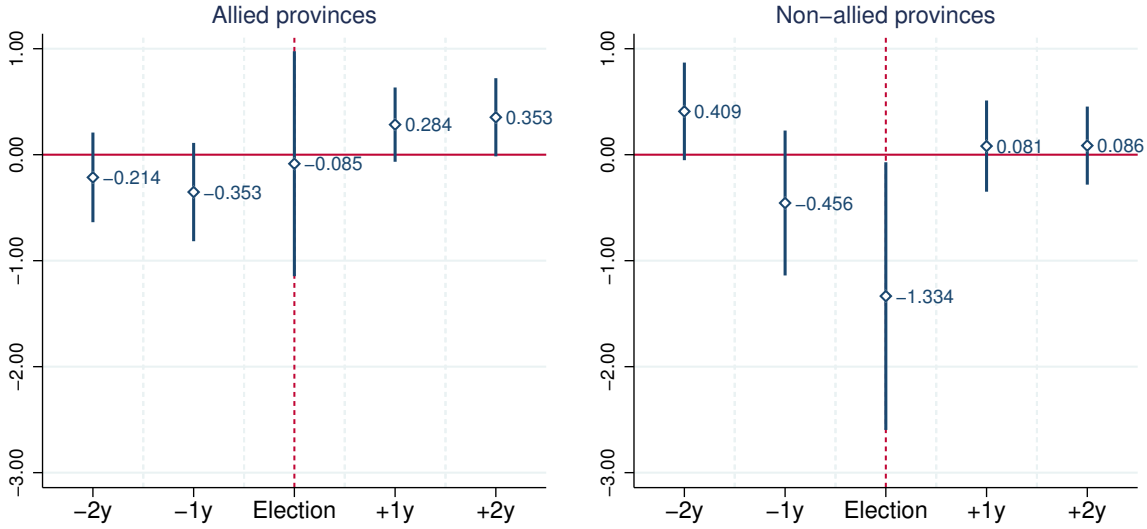
(a) Dependent variable: Bank debt



(b) Dependent variable: Total assets



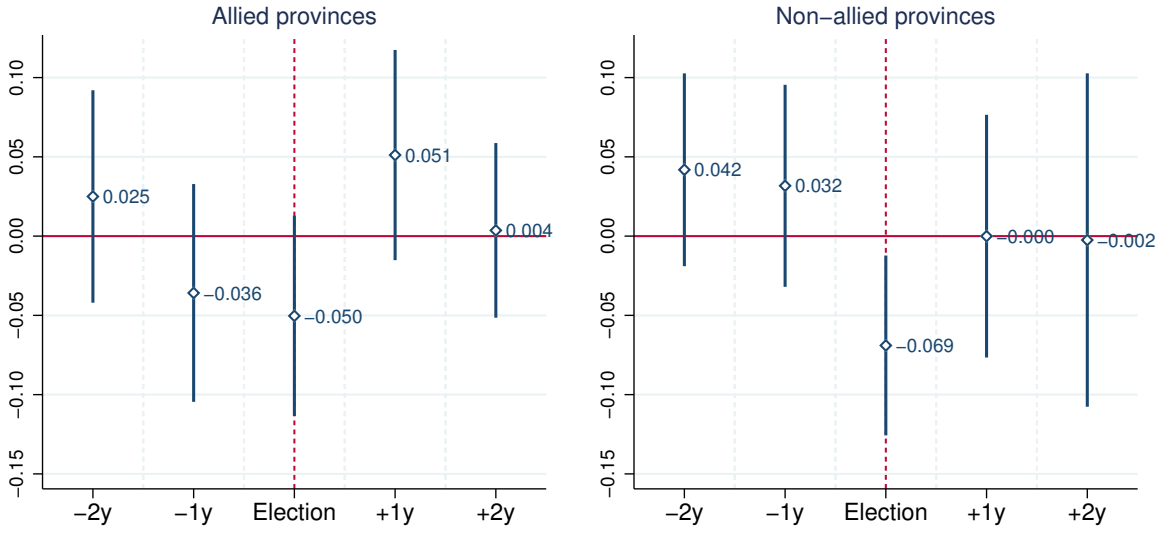
(c) Dependent variable: Employment



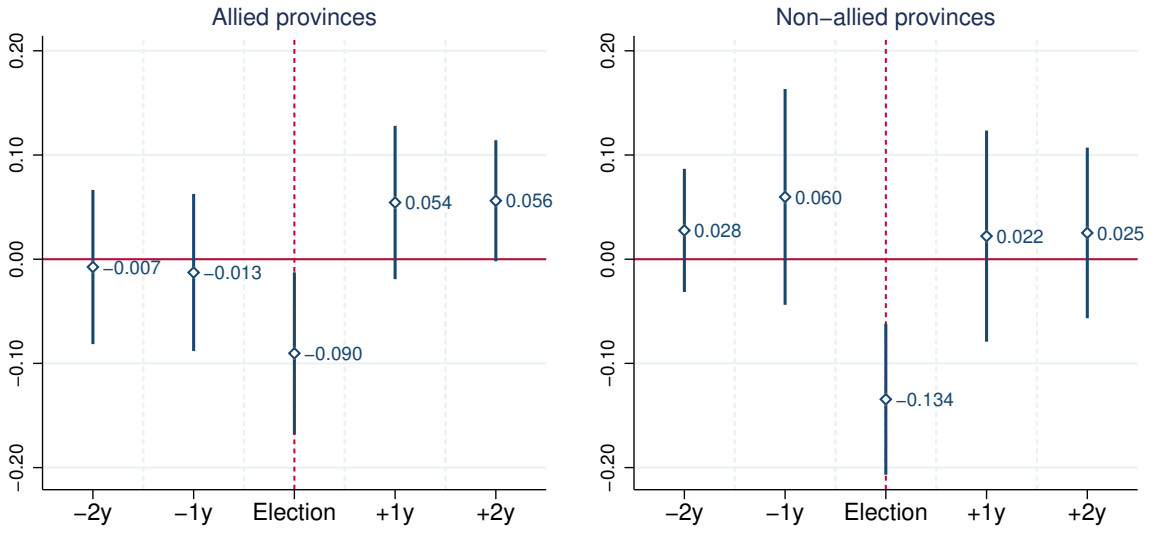
(d) Dependent variable: Net sales

Notes: This figure shows results of Equation (3) estimated on yearly data (2006-2016) when τ takes values from -2 to +2, indicating the number of years around elections, for manufacturing industries in all provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for a set of fixed effects and province time trends.

Figure C.9: Electoral cycle in the allocation of bank credit by initial efficiency



(a) Initial efficiency measured by asset turnover



(b) Initial efficiency measured by MRPK

Notes: This figure shows results of Equation (4) estimated on yearly data when τ takes values from -2 to +2, indicating the number of years around elections, for all provinces. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for industry and province-by-time fixed effects.

Table C.1: Manufacturing employment and incumbent party's election performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:	*Metropolitan sample*							
Employment growth,	0.377**	0.506**	0.508**	0.508*				
past 1 year	[0.149]	[0.209]	[0.213]	[0.274]				
Employment growth,					0.099	0.253**	0.271**	0.271*
past 2 years					[0.084]	[0.116]	[0.121]	[0.146]
N	60	60	60	60	60	60	60	60
R^2	0.102	0.169	0.170	0.170	0.024	0.142	0.151	0.151
Panel B:	*Full sample*							
Employment growth,	0.064*	0.094*	0.098	0.098**				
past 1 year	[0.035]	[0.056]	[0.062]	[0.043]				
Employment growth,					0.033	0.054	0.053	0.053**
past 2 years					[0.026]	[0.036]	[0.037]	[0.024]
N	162	162	162	162	162	162	162	162
R^2	0.022	0.033	0.034	0.034	0.012	0.028	0.028	0.028
Election FE	Yes		Yes	Yes	Yes		Yes	Yes
Province FE		Yes	Yes	Yes		Yes	Yes	Yes
S.E. Clustering				Province				Province

Notes: This table shows regression results of election performance on employment growth estimated with province level data. The dependent variable is the change in the vote share won by the incumbent party between the current and previous local elections for metropolitan provinces, and by the incumbent ideology (right vs. left) in other provinces. Standard errors are clustered at the province level in columns (5) and (10) and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table C.2: Effects of tactical redistribution on bank debt

	Aligned provinces				*Non-aligned provinces*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Comp x State Bank Share x Election	1.420	1.430	1.272	1.324	-2.522**	-2.469**	-2.405**	-2.058**
	[1.080]	[1.081]	[1.096]	[1.094]	[0.983]	(0.991)	[1.002]	[0.937]
Comp x State Bank Share	-1.380	-1.386	-1.355	-1.290	-0.046	-0.089	-0.016	-0.512
	[2.641]	[2.637]	[2.729]	[2.942]	[1.657]	[1.636]	[1.722]	[2.019]
State Bank Share x Election	-1.218***	-1.215**	-1.208**		1.252	1.217	1.116	
	[0.431]	[0.433]	[0.460]		[0.753]	[0.759]	[0.748]	
Comp x Election	-0.115	-0.115			0.298**	0.284**		
	[0.123]	[0.126]			[0.102]	[0.116]		
Comp	0.059	0.075			0.050	0.057		
	[0.294]	[0.290]			[0.172]	[0.164]		
Election		0.288***				-0.062		
		[0.066]				[0.080]		
Industry FE	Yes	Yes	Yes		Yes	Yes	Yes	
Province FE	Yes	Yes			Yes	Yes		
Time FE	Yes				Yes			
Province-Time trends	Yes	Yes			Yes	Yes		
Province x Time FE			Yes	Yes			Yes	Yes
Industry x Time FE				Yes				Yes
N	2,330	2,330	2,330	2,330	1,628	1,628	1,628	1,628
R^2	0.516	0.514	0.521	0.534	0.513	0.511	0.518	0.541

Notes: This table shows results of Equation (3) for the sample of metropolitan provinces and manufacturing industries. The dependent variable is (log) total bank debt, defined as the sum of short-term and long-term bank debt on corporate balance sheets. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table C.3: Effects of tactical redistribution on corporates' liabilities excluding bank debt

	Aligned provinces				*Non-aligned provinces*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Comp x State Bank Share x Election	1.456*	1.459*	1.447	1.307	-0.643	-0.627	-0.629	-0.672
	[0.842]	[0.842]	[0.871]	[0.879]	[0.775]	[0.779]	[0.830]	[0.893]
Comp x State Bank Share	-0.108	-0.108	-0.100	0.026	-1.205	-1.231	-1.214	-1.529
	[2.235]	[2.230]	[2.309]	[2.459]	[1.438]	[1.427]	[1.477]	[1.672]
State Bank Share x Election	-0.734*	-0.735*	-0.747*		-0.302	-0.315	-0.337	
	[0.403]	[0.402]	[0.422]		[0.498]	[0.511]	[0.540]	
Comp x Election	-0.141	-0.131			0.119	0.107		
	[0.103]	[0.105]			[0.073]	[0.075]		
Comp	0.048	0.024			0.187	0.186		
	[0.239]	[0.241]			[0.154]	[0.144]		
Election		0.111**				0.017		
		[0.053]				[0.025]		
Industry FE	Yes	Yes	Yes		Yes	Yes	Yes	
Province FE	Yes	Yes			Yes	Yes		
Time FE	Yes				Yes			
Province-Time trends	Yes	Yes			Yes	Yes		
Province x Time FE			Yes	Yes			Yes	Yes
Industry x Time FE				Yes				Yes
N	2,330	2,330	2,330	2,330	1,628	1,628	1,628	1,628
R^2	0.517	0.516	0.520	0.534	0.523	0.522	0.527	0.543

Notes: This table shows results of Equation (3) for the sample of metropolitan provinces and manufacturing industries. The dependent variable is (log) total liabilities minus total bank debt on corporate balance sheets. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table C.4: Effects of tactical redistribution on total assets

	Aligned provinces				*Non-aligned provinces*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Comp x State Bank Share x Election	1.458	1.460	1.442	1.018	-1.507*	-1.500*	-1.451	-1.571*
	[1.009]	[1.008]	[1.038]	[1.014]	[0.833]	[0.836]	[0.864]	[0.859]
Comp x State Bank Share	-0.986	-0.988	-0.976	-0.981	-1.261	-1.270	-1.262	-1.600
	[2.103]	[2.101]	[2.170]	[2.307]	[1.615]	[1.609]	[1.665]	[1.935]
State Bank Share x Election	-0.805	-0.806	-0.798		0.253	0.251	0.182	
	[0.476]	[0.476]	[0.499]		[0.468]	[0.472]	[0.479]	
Comp x Election	-0.114	-0.118			0.168*	0.160*		
	[0.110]	[0.112]			[0.085]	[0.085]		
Comp	0.121	0.129			0.186	0.184		
	[0.215]	[0.225]			[0.172]	[0.164]		
		0.102*				-0.014		
		[0.057]				[0.053]		
Industry FE	Yes	Yes	Yes		Yes	Yes	Yes	
Province FE	Yes	Yes			Yes	Yes		
Time FE	Yes				Yes			
Province-Time trends	Yes	Yes			Yes	Yes		
Province x Time FE			Yes	Yes			Yes	Yes
Industry x Time FE				Yes				Yes
N	2,378	2,378	2,378	2,378	1,662	1,662	1,662	1,662
R^2	0.535	0.535	0.538	0.549	0.546	0.546	0.550	0.564

Notes: This table shows results of Equation (3) for the sample of metropolitan provinces and manufacturing industries. The dependent variable is (log) total assets. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table C.5: Effects of tactical redistribution on employment

	Aligned provinces				*Non-aligned provinces*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Comp x State Bank Share x Election	0.835	0.837	0.877	0.495	-2.794**	-2.746**	-2.883**	-2.786**
	[0.802]	[0.803]	[0.830]	[0.754]	[1.107]	[1.113]	[1.112]	[1.058]
Comp x State Bank Share	-1.356	-1.356	-1.367	-1.289	-0.220	-0.231	-0.193	-0.521
	[1.573]	[1.572]	[1.618]	[1.700]	[1.086]	[1.082]	[1.140]	[1.250]
State Bank Share x Election	-0.717*	-0.715*	-0.744*		1.756*	1.722*	1.876*	
	[0.390]	[0.391]	[0.413]		[0.894]	[0.901]	[0.899]	
Comp x Election	-0.059	-0.061			0.371**	0.366**		
	[0.086]	[0.086]			[0.139]	[0.145]		
Comp	0.152	0.147			0.000	0.008		
	[0.163]	[0.167]			[0.104]	[0.096]		
Election		0.072				-0.214		
		[0.047]				[0.126]		
Industry FE	Yes	Yes	Yes		Yes	Yes	Yes	
Province FE	Yes	Yes			Yes	Yes		
Time FE	Yes				Yes			
Province-Time trends	Yes	Yes			Yes	Yes		
Province x Time FE			Yes	Yes			Yes	Yes
Industry x Time FE				Yes				Yes
N	2,512	2,512	2,512	2,512	1,789	1,789	1,789	1,789
R^2	0.627	0.627	0.630	0.639	0.619	0.619	0.623	0.642

Notes: This table shows results of Equation (3) for the sample of metropolitan provinces and manufacturing industries. The dependent variable is (log) employment. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table C.6: Effects of tactical redistribution on net sales

	Aligned provinces				*Non-aligned provinces*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Comp x State Bank Share x Election	0.644	0.633	0.636	0.336	-2.181**	-2.144**	-2.251**	-2.111**
	[0.885]	[0.883]	[0.904]	[0.845]	[0.860]	[0.863]	[0.911]	[0.878]
Comp x State Bank Share	-1.600	-1.591	-1.590	-1.307	-1.634	-1.654	-1.594	-1.962
	[2.265]	[2.260]	[2.329]	[2.442]	[1.658]	[1.656]	[1.710]	[1.825]
State Bank Share x Election	-0.837	-0.831	-0.818		1.260	1.236	1.320	
	[0.618]	[0.617]	[0.631]		[0.728]	[0.731]	[0.778]	
Comp x Election	-0.017	-0.035			0.274**	0.257**		
	[0.088]	[0.092]			[0.107]	[0.106]		
Comp	0.196	0.226			0.172	0.183		
	[0.238]	[0.247]			[0.156]	[0.150]		
Election		0.087				-0.171		
		[0.062]				[0.103]		
Industry FE	Yes	Yes	Yes		Yes	Yes	Yes	
Province FE	Yes	Yes			Yes	Yes		
Time FE	Yes				Yes			
Province-Time trends	Yes	Yes			Yes	Yes		
Province x Time FE			Yes	Yes			Yes	Yes
Industry x Time FE				Yes				Yes
N	2,514	2,514	2,514	2,514	1,798	1,798	1,798	1,798
R^2	0.538	0.536	0.540	0.549	0.547	0.545	0.550	0.566

Notes: This table shows results of Equation (3) for the sample of metropolitan provinces and manufacturing industries. The dependent variable is (log) net sales. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Table C.7: Effects of tactical redistribution on the share of firms investing

	Aligned provinces				*Non-aligned provinces*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Comp x State Bank Share x Election	-0.018	-0.016	-0.058	-0.061	0.024	0.034	-0.009	-0.061
	[0.104]	[0.105]	[0.105]	[0.112]	[0.092]	[0.089]	[0.092]	[0.103]
Comp x State Bank Share	0.031	0.021	0.026	0.023	0.166***	0.161***	0.170***	0.152**
	[0.053]	[0.052]	[0.053]	[0.053]	[0.053]	[0.053]	[0.051]	[0.065]
State Bank Share x Election	-0.143*	-0.141*	-0.113*		-0.068	-0.069	-0.030	
	[0.070]	[0.068]	[0.063]		[0.092]	[0.091]	[0.106]	
Comp x Election	-0.011	-0.020			0.029	0.011		
	[0.033]	[0.034]			[0.041]	[0.040]		
Comp	-0.001	-0.052*			-0.033	-0.029		
	[0.024]	[0.027]			[0.020]	[0.028]		
Industry FE	Yes	Yes	Yes		Yes	Yes	Yes	
Province FE	Yes	Yes			Yes	Yes		
Time FE	Yes	Yes			Yes	Yes		
Province-Time trends		Yes				Yes		
Province x Time FE			Yes	Yes			Yes	Yes
Industry x Time FE				Yes				Yes
N	3,595	3,595	3,595	3,595	2,440	2,440	2,440	2,440
R^2	0.214	0.272	0.388	0.444	0.234	0.245	0.264	0.339

Notes: This table shows results of Equation (3) for the sample of metropolitan provinces and all industries. The dependent variable is the share of firms that carry out physical investments in each province-industry pair by year. Standard errors are clustered at the province level and provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

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