

# Political Lending Cycles and Real Outcomes: Evidence from Turkey\*

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## Abstract

We use data on the universe of credit extended over a 14-year period in Turkey to document a strong political lending cycle. State-owned banks systematically adjust their provincial lending around local elections compared with private banks in the same province. There is considerable tactical redistribution: state-owned banks increase loans in politically competitive provinces with a current mayor aligned with the ruling party but reduce it in similar provinces with a current mayor from opposition. This effect only exists in corporate lending as opposed to consumer loans, suggesting that tactical redistribution targets job creation to increase electoral success. Such political lending seems to influence real outcomes as the credit-constrained opposition areas suffer a drop in economic output as measured by local construction activity.

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\*The opinions expressed in this paper are those of the authors only and do not necessarily represent the views of the European Bank for Reconstruction and Development (EBRD).

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# 1. Introduction

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 significantly reallocated around election years and such targeted redistribution would often be aimed to shift local election outcomes in favour of the ruling party, or coalition parties in control of central government. Is it then possible to see that some regions would be favoured and others get punished on the basis of their attractiveness to politicians? More importantly, does this reallocation have real effects on the local economy?

We test theories of political cycles in Turkey for the period from 2003 to 2016 using the universe of bank credit for the country. We collect detailed data on election outcomes, banking activity for different bank types, and indicators of economic activity all observed at the province level. Unlike previous literature, we can draw on quarterly data to identify the exact timing of politically induced lending. Our data also allow us to differentiate between the effects of politically driven lending on firms and consumers separately.

We document two main sets of findings. First, we show that state-owned banks engage in strategic lending around election years when compared with private banks. In contrast to earlier findings, state-owned banks curb aggregate credit prior to local elections and increase lending immediately afterward. However, 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 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. We interpret this vastly different behaviour of state banks around elections as strong evidence for the existence of a political lending cycle. It appears that the central government 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.

Election cycles and close election outcomes provide a quasi-exogenous variation in how aggregate credit is allocated across the country. In our second set of findings, we present evidence that local economic activity is influenced by this reallocation. In particular, economic output – as measured by private sector building activity – suffers in provinces with an opposition mayor and close electoral competition when compared against provinces with aligned mayors. In line with the interpretation that this reallocation of economic activity is driven by the political lending cycle, we find that credit extended to the corporate sector follows the same pattern.

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 use cross-sectional variation in electoral competition and political alignment across localities to identify elements of tactical redistribution and rule out alternative explanations. On the one hand, this helps us eliminate demand-driven explanations of the lending cycle, since local economic shocks that may be correlated with the election cycle should affect private banks equally. On the other hand, private banks may also be subject political influence, and they may respond to competition from state banks. In that case, our estimations constitute a lower bound for the true size of the political cycle.

Our setting also allows us to differentiate between some of the main mechanisms of political resource reallocation suggested in earlier work. 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 might enjoy strong support. Such constituencies would absorb a disproportionate amount of resources regardless of the electoral cycle. “Tactical redistribution” predicts that resources will be directed towards ‘swing’ districts either to change the election outcome, in which case we are more likely to see an impact prior to the election, or rewarding the party’s strongholds, where one would expect to see a post-election impact. Our results provide strong evidence consistent with tactical redistribution, while we also find some evidence supporting the constant patronage argument.

We contribute to two strands of the literature. First, we provide new evidence on political cycles and mechanisms underlying tactical redistribution. Inspired by theories of opportunistic political cycles,<sup>1</sup> earlier studies investigate the effect of elections on governments’ tax revenues and budget deficits.<sup>2</sup> Evidence shows that such political budget cycles are prevalent across the world, especially in developing countries and young democracies (Akhmedov and Zhuravskaya, 2004; Shi and Svensson, 2006; Brender and Drazen, 2008). A more recent set of papers asks whether lending by state-owned banks follows a political cycle. Dinç (2005) finds cross-country evidence that government-owned banks raise lending in national election years compared with private banks. Cole (2009) finds that state banks in India extend more agricultural credit during election years, but with no tangible effect on agricultural output,

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<sup>1</sup>See Nordhaus (1975), MacRae (1977) and Rogoff and Sibert (1988).

<sup>2</sup>These studies explore the possibility that politicians in power may use the central government’s fiscal muscles to boost the economy and improve their own reelection prospects. However, there is a chance that sophisticated voters might punish opportunistic governments as in Peltzman (1992), although this would require fully-informed voters with plenty of democratic experience (Brender and Drazen, 2005).

especially in ‘swing’ regions.<sup>3</sup> Similarly, [Carvalho \(2014\)](#) shows that Brazilian firms eligible for state-bank lending employ more people in politically attractive regions near elections and in return, these expansions are likely to be financed by state-bank loans. Most recently, [Englmaier and Stowasser \(2017\)](#) find that German savings banks, which are subject to political influence, change their lending behaviour in the run up to local elections.

Our work complements these studies. We take advantage of the Turkish electoral system, which differentiates between the election of district and metropolitan mayors, to create an exact match between political, credit, and real outcomes at the province level. Our identification is strongest in metropolitan provinces where a single mayor is elected by the majority of votes coming from all voters located in that province. This helps us derive more precise estimates for political competition and avoid vote aggregation issues encountered by earlier studies. Furthermore, we draw on a newly available quarterly dataset of bank loans to explore the lending cycle in a higher frequency and differentiate between pre- and post-election behaviour. This is a considerable improvement over previous studies, which analyse lending cycles using yearly observations that do not always correspond to exact election timing.

In terms of mechanisms, our setting is similar to the political capture mechanism described by [Brollo and Nannicini \(2012\)](#) and [Carvalho \(2014\)](#) for Brazil, where state-bank lending is controlled by the central government and reallocated among regions depending on their political attractiveness. We provide evidence that state-bank loans are reallocated towards politically competitive provinces when the incumbent mayor is allied with the ruling party in central government. However, in opposition provinces, this reallocation takes the form of punishment, as credit is withdrawn especially from competitive regions. Our findings suggest that the latter mechanism outweighs the former. Our evidence is therefore consistent with the incentives of “tying your enemy’s hands in close races” ([Brollo and Nannicini, 2012](#)).

Our second contribution to the literature is on potential benefits and harms of state-owned banks. 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 the politicians ([Shleifer and Vishny, 1994](#); [Shleifer, 1998](#)). In a seminal paper, [La Porta, Lopez-de Silanes, and Shleifer \(2002\)](#) show that state ownership of the banking sector across countries is associated with lower levels of growth, financial development, and government efficiency. [Sapienza \(2004\)](#) uses loan-level data to find that Italian state banks charge lower interest rates to similar firms. This tendency

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<sup>3</sup>[Cole \(2009\)](#) also finds that loan defaults increase after directed lending with no concurrent rise in output, which implies that election-induced loans are not used efficiently.

strengthens as the political party associated with the state bank has more support in the region, implying financial favours for its supporters. Similarly, [Khwaja and Mian \(2005\)](#) present evidence that firms in Pakistan with a politician on their board benefit from lower rates and default more often when they borrow from government banks, but not from private ones.<sup>4</sup>

Our paper contributes to this literature by showing that the political lending cycle in Turkey is driven mainly by corporate sector loans, implying that the government prefers enriching the (potentially connected) firms operating in allied regions while impoverishing the ones located on the opposition side. Since such reallocation has real economic effects in the same locality, this could lead to an increase in inequality among provinces of different political affiliations.

The rest of the paper is organised as follows. The next section briefly outlines the Turkish banking industry and gives the institutional background for local elections in Turkey. Section 3 describes the data. Our empirical methodology and results are presented in section 4. Section 5 concludes.

## 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 as in other emerging markets. 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 fiscal repression and funding government deficits of the 1980's and 1990's.

The shift in Turkish banking activity toward private sector financing followed an intensive restructuring phase, which was instigated by the twin currency and banking crises that struck the country between 1999 and 2001.<sup>5</sup> More than 15 banks failed during the episode and many were taken over by the country's Savings Deposit Insurance Fund (SDIF). An extensive reform package was initiated under the guidance of the International Monetary Fund (IMF) to strengthen the operational efficiency and financial stability of the banking

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<sup>4</sup>See also [Leuz and Oberholzer-Gee \(2006\)](#), who detect a negative relationship between political connectedness of Indonesian firms and their foreign financing; this is consistent with the view that connected firms can obtain cheap financing from government banks and do not benefit from foreign financing.

<sup>5</sup>One of the root causes of these crises was the heavy involvement of the banks in the domestic government debt market, which has since receded. For a detailed discussion, see [Akyüz and Boratav \(2003\)](#).

sector. The central bank gained its institutional independence by law, while an independent Banking Regulation and Supervision Agency (BRSA) was established to solve the conflict of interest problem in bank supervision.<sup>6</sup> The BRSA was also given the sole right to issue new banking permits, which had been at the hands of the central government's Council of Ministers and therefore heavily politicised. In early 2003, BRSA pushed through the early adoption of Basel II capital adequacy standards. In 2004, a limited deposit insurance scheme was introduced and replaced the previously unlimited coverage for all financial institutions.

These reforms have 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* –, while they have no direct influence over private banks. Therefore, our sample period, which starts around the time that these reforms took effect, constitutes an ideal period to investigate the influence of the central government on state-owned banks. Even though such influence has always existed in the Turkish political sphere, we expect the ownership to be the only channel through which government may exert pressure on the banking system during the period under study.<sup>7</sup>

[Insert Table 1 near here]

Table 1 shows how deposit-taking banks in Turkey have evolved over the past two decades.<sup>8</sup> Panel A indicates that the sector has shrunk in size considerably between 1999 and 2004 following the financial stability programme. In total 20 banks were closed down, while state banks became much leaner by shedding branches and personnel. However, both state and private banks flourished since then, expanding their branch network and employees considerably. The sector consolidated on the private side through entry or mergers involving new and foreign banks. The three state banks were initially aimed to be privatised as part of the post-crisis restructuring programme, but these plans were never put into practice by the government.

Panel B shows that the formation of a uniform supervisory and regulatory system leveled the playing field for private and state banks. State banks have substantially improved their

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<sup>6</sup>Up until 2000, the Treasury and the Central Bank shared the responsibility for bank supervision. These institutions were not able to step in to prevent the excessive carry-trade tendency when weakly-capitalised banks started financing Turkish government debt with cheap borrowing from abroad and exposed themselves to massive currency risks (see Baum, Caglayan, and Talavera, 2010).

<sup>7</sup>In the coalition governments of 1990s, for instance, it was common practice to share control of state banks among coalition parties based on their vote shares (Önder and Özyıldırım, 2013).

<sup>8</sup>Note that information in Table 1 does not include investment banks, development banks, or banks under the management of SDIF.

loan quality and capital buffers since 2004. More importantly, private and state banks have converged to a similar level of financial performance over time. This ensures that our identification strategy is immune to operational differences or balance sheet effects between these two sets of banks. State and private banks have typically controlled 30% and 60% of total banking assets, respectively. Their shares in total deposits and lending have been similar. Banking sector in general has experienced a strong growth, nearly doubling its size with respect to country's GDP since 2004.<sup>9</sup>

## 2.2. *Politics and local elections in Turkey*

Turkey is a parliamentary democracy with a multi-party political system. The Prime Minister, typically the leader of a political coalition, serves as the head of government and exercises executive powers with the Council of Ministers. The current ruling party, AKP (*Adalet ve Kalkinma Partisi*), has been in power since 2002 and retained its majority of seats in parliament through several general elections. The AKP inherited the IMF-led reforms of 1999-2001 and successfully implemented them, bringing public expenditures under control, strengthening the overall quality of institutions, and starting accession negotiations with the European Union in 2005.<sup>10</sup>

Turkey is divided into 81 provinces (or cities) for administrative purposes, which are further divided into 923 districts. Each district corresponds to a constituency in a local election. Out of the 81 provinces, 30 are designated as metropolitan municipalities. A metropolitan municipality consists of all districts within the borders of that province, and a metropolitan mayor is elected by the majority of votes cast in that province.<sup>11</sup> The electorate in metropolitan areas also votes for district mayors on the same election day. Voters in non-metropolitan areas only vote for mayoral candidates of the district they live in. 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 in the remaining 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). Although early local elections are possible *de jure* in Turkey, *de facto* they

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<sup>9</sup>See Table A1 for the growth in assets, loans and deposit activity separately for state and private banks since 1999.

<sup>10</sup>See Acemoglu and Ucer (2015) for a discussion of Turkish politics and institutions under the AKP rule.

<sup>11</sup>As discussed later, this helps us have a better correspondence between election and credit data in metropolitan provinces.

do not exist in the country’s political culture.<sup>12</sup> We focus on political cycles based on local, as opposed to general, elections to identify possible effects on bank lending and economic outcomes.<sup>13</sup> The reasons for this are twofold.

First, as Turkey gradually shifted from coalition governments to single-party governments 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 municipalities have commanded substantial political clout.<sup>14</sup> These developments are consistent with the political model of [Brollo and Nannicini \(2012\)](#), in which voters are unable to distinguish the sources of government transfers and political credit spillovers occur in favour of municipal governments. The central government may then use transfers to favour political friends or to punish political enemies at the local level, since mayoral candidates can be important allies for the central government once elected ([Brollo and Nannicini, 2012](#)). In addition, the single-party AKP government has rarely faced any competition at national elections during our sample period. Thus, it is reasonable to expect that any potential reallocation of resources should follow local elections, especially where the ruling party in central government faces real competition to ”win” or ”lose” certain provinces.

Second, province-level vote shares of political parties at national elections do not translate directly into the number of seats gained in parliament, and thereby into political influence over resource transfers. This is due to the presence of a relatively high election threshold, which requires each political party to receive at least 10% of the national vote to enter the parliament. This makes it impossible to have a clear measure of the actual province-level electoral contest, since votes for parties that fail to clear the national threshold are redistributed among remaining parties in each province. The number of legislators that go to parties with at least 10% of the national vote are artificially increased as a result. We believe that such uncertainty regarding the number of legislative seats that can be won at the province level deters the central government from pursuing a regional targeting policy.<sup>15</sup> In contrast, competition in a local election is straightforward to quantify and more visible

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<sup>12</sup>There has never been an early local election in Turkey since 1982.

<sup>13</sup>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: 2007, 2011, 2015 (June), and 2015 (November).

<sup>14</sup>Indeed, current President Recep T. Erdogan served as mayor of Istanbul between 1994 and 1998, before he set up the AKP that has ruled the country since 2002. See [İncioğlu \(2002\)](#) and [Sayarı \(2014\)](#) for the rising importance of local elections in Turkey.

<sup>15</sup>[Baum et al., 2010](#) check for parliamentary election cycles in the Turkish banking sector from 1963 to 2007 and find no evidence of a meaningful difference between state and private banks. This could be due to two possible reasons. Either governments do not resort to such tactics for general elections, or political influence also affects private banks, as it used to be the case before 2001.



as it resembles to a single-winner voting system, in which the party that gets the most votes wins the constituency. Therefore, our focus on local elections helps us understand tactical reallocation by the central government when it faces a clear competitive threat to win or lose a region.

### 3. Data

There are three main data sets that we exploit in our analysis. Our first dataset combines various sources with detailed banking information. We use annual bank credit data provided by the Central Bank of the Republic of Turkey (CBRT) and the Banks Association of Turkey (BAT). We combine these two datasets and eliminate the pre-crisis era, focusing instead on the period characterised by the single-party government. This gives us the year-end total cash loan exposure of each bank type (state or private) in each of Turkey’s 81 provinces from 2003 to 2016. Additionally, we benefit from the FinTürk database maintained by the Banking Regulatory and Supervisory Agency (BRSA). BRSA provides quarterly province-level data on credit extended by state and private banks since the fourth quarter of 2007. These data constitute the universe of bank cash and non-cash loans in the country, and they are further broken down by credit extended to different sectors (e.g. corporate vs consumer). They cover 81 provinces over 37 quarters for different bank types, which gives us the opportunity to employ higher frequency data around elections and differentiate between pre- and post-election effects. In addition, we collect quarterly data on bank branches from FinTürk, again at the level of province and bank type.

Our second dataset contains measures of real economic outcomes. Since Turkey provides economic indicators typically at a more aggregate subregional level, we resort to a different proxy for economic activity at the province level.<sup>16</sup> In particular, we obtain records of construction permits issued by local municipalities from the Turkish Statistical Institute (TurkStat). These permits are a standard requirement for any entity to start a construction project. We believe that new construction activity in a province provides a good proxy for local economic activity. We obtain information on all buildings constructed in each province between 2003 and 2016, including total number of flats and houses, square-meters covered, and monetary value (in Turkish Liras). These data are also broken down by ownership (private vs public sector); we only keep private sector construction in our sample to avoid the possibility that state-funded projects might be targeted independent of credit conditions.<sup>17</sup>

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<sup>16</sup>Turkey follows EuroStat’s NUTS (Nomenclature of Territorial Units for Statistics) designation for regions. There are 81 provinces at the NUTS-3 level, 26 subregions at the NUTS-2 level, and 12 regions at the NUTS-1 level.

<sup>17</sup>Marschall, Aydogan, and Bulut (2016) provide evidence consistent with the view that government-

Our third data set consists of 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 competition (or contestedness) that captures the margin of victory/loss by the ruling-party (AKP) candidate against the most popular opposition (non-AKP) candidate. Formally, we construct the following *Competition* variable:

$$Competition_{p,t} = 1 - |WinMargin_{p,t}|$$

where  $p$  stands for province,  $t$  indicates the particular election and *WinMargin* 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. For instance, in the extreme case that the top two candidates get the exact same share of votes (which is never observed in our sample), *Competition* would equal 1. To capture province-level competition, we work with the win 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.

Our second political variable is a dummy for political alignment (or incumbency), which indicates whether the ruling-party (AKP) candidate wins (i.e., gets the highest number of votes) in that province or not. Recall that voters elect both district and metropolitan mayors in metropolitan provinces, while they elect only a district mayor in non-metropolitan provinces.<sup>18</sup> However, our credit data are only available at the province level, which means we need to aggregate voting outcomes to define a province-level measure of alignment. Previous literature deals with this problem by averaging voting outcomes across constituencies of a region (see, for instance, [Cole, 2009](#)). However, this approach may be inappropriate in our setting. Unlike most 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. Thus, tactical reallocation not only depends on electoral competition in a province, but also crucially on whether the province is currently aligned or not.<sup>19</sup> This forces us to have a cleaner measure of alliance than averaging across districts.

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

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funded building projects in Turkey might be politically motivated.

<sup>18</sup>Note also that the main contest between political parties is over the election of metropolitan mayors in metropolitan provinces and mayor of the central district in non-metropolitan provinces.

<sup>19</sup>Alliance with the central or federal government does not matter in the political settings of [Sapienza \(2004\)](#), [Cole \(2009\)](#) or [Englmaier and Stowasser \(2017\)](#), where locally elected governments have a direct influence on state banks that operate locally. [Carvalho \(2014\)](#) has a setting similar to ours, in which the central government in Brazil manipulates state-bank lending to help reelect allied state governors.

alliance for each province. However, this matching is still not ideal for non-metropolitan provinces, since some central districts – even though they are the largest by population within a province – do not always represent the political dynamics of the whole province. This can be seen in Figure 2, which shows the alliance of elected district mayors in two non-metropolitan provinces during 2004 elections. Panel A shows that in Muş, the only aligned district was the central district, where the electorate represented less than half of all voters (48.3%) in that province. In contrast, the central district in Kastamonu (Panel B) was not aligned with the ruling party; however, a large portion of the province (43.9% by votes) was still governed by an aligned mayor. If politically induced lending occurs at the level of districts, this may create some measurement error and lead to attenuation bias in our estimates. We therefore base our main findings on results from metropolitan provinces, where the elected mayor represents the whole electorate and acts as the main political figure in the province.<sup>20</sup> Our estimates from the metropolitan sample should thus be free of measurement error. Nevertheless, we will also report our findings from a full sample that also includes non-metropolitan provinces.

[Insert Figure 2 near here]

Table 2 presents summary statistics for the main variables in our analysis. During our sample period, 60% of provinces on average 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.

[Insert Table 2 near here]

## 4. Methodology & Results

### 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 that operate in the same provinces. If there is politically induced lending, then political pressure on state-owned banks should *intensify*

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<sup>20</sup>Given the rising importance of metropolitan mayors in the Turkish political sphere and their importance in the overall economy, we also believe that the central government is more likely to strategically target metropolitan provinces.

around election years. We therefore expect state banks to alter their lending behaviour closer to elections compared with private banks. To the extent that the effect of politicians on lending decisions by state banks is stable over time, or that politicians might also affect private banks around elections, our DD estimates provide a lower bound for the true size of politically induced lending.

The essence of DD relies on the premise that treated and untreated groups share a parallel trend in the absence of treatment (Angrist and Pischke, 2009). Figure 1 shows the evolution of total cash loans extended by state and private banks since 2003 (in levels on the left panel and in logs on the right panel). Aggregate credit has been on a stable trajectory for both state and private banks throughout this period. Two exceptions to these trends appear in 2009 and 2016, when lending by private banks have actually contracted due to significant slowdown in the Turkish economy.<sup>21</sup> Our DD strategy should be immune to year-specific shocks to the extent that economic slowdowns affect all provinces or bank types similarly. Nevertheless, we carry out extensive checks to ensure that no single election or unobserved province- or bank type-specific shocks drive our results. Moreover, we include the number of local branches by bank type in each of our regressions. This should help us control for any long-term credit demand and supply conditions in each province by bank type, and potential sorting of banks that may be linked to regional unobservables.

[Insert Figure 1 near here]

As discussed before, we mainly search for tactical redistribution prior to elections in our context while still being open to the possibility of patronage in non-election years. To test this idea, we make use of the full 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. The DDD model allows us to control for a full set of province-by-year or bank type-by-year fixed effects. This helps us eliminate any unobserved province- or bank-specific shocks that may be correlated with election cycles.

Indeed, a key feature of our identification comes from the fact that we test the differential allocation of state-bank credit towards ‘swing’ provinces over the entire election cycle instead of only comparing election versus non-election years. This gives us a full picture of the evolution of political pressure on state-banks, and provides a much more powerful test of election-induced lending. In fact, bank credit cycles over time could be explained by reasons unrelated to politics (such as banks’ different sensitivities to political uncertainty). Cross-

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<sup>21</sup>Turkey experienced a recession in 2009 due to the global financial crisis, while growth slowed down in 2016 due to increased uncertainty, heightened by a failed coup attempt in July.

sectional allocation of credit towards certain provinces could be related to province-specific factors (such as concentration of certain sectors in certain provinces). However, it is almost impossible to explain why such cross-sectional relationships would vary over time specifically around elections without resorting to an explanation based on political incentives (Cole, 2009).

#### 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 using a standard DD model. Consider:

$$\text{LogCredit}_{b,p,t} = \beta_{\tau} \text{StateBank}_b \times \text{Election}_{t+\tau} + \delta X_{b,p,t-1} + \theta_b + \gamma_p + \lambda_t + \varepsilon_{b,p,t} \quad (1)$$

where  $b$  is an index for bank type (state or private),  $p$  stands for province, and  $t$  denotes years in the yearly data (CBRT) and year-quarters in the quarterly data (FinTürk).  $\text{StateBank}_b$  is a dummy variable indicating state-owned banks. Importantly,  $\text{Election}_t$  equals one in the year before a local election and zero otherwise.<sup>22</sup> To document the full election cycle, we generalise the definition of  $\text{Election}_t$  to  $\text{Election}_{t+\tau}$  and re-run regressions where  $\tau$  takes values from -2 to +2 indicating the number of years around elections. For instance, we have  $\tau = 1$  to indicate the first year-end after an election (corresponding to 2004, 2009 and 2014).

Our coefficient of interest in equation 1 is  $\beta_{\tau}$  and captures the behaviour of state banks compared with private banks at each point over the election cycle. We include fixed effects at the levels of bank type, province, and time in our baseline. Lastly,  $X_{b,p,t-1}$  includes lagged number of bank branches, which control for local market shares separately for each bank-type. We cluster standard errors in all of our regressions at the province level, since local credit outcomes are likely to be correlated across time within localities.

Table 3 presents results on the election year (i.e.,  $\tau = 0$ ) for the full sample and the subsample of metropolitan provinces. In both samples and across different sets of controls, state banks decrease credit supply with respect to private banks in the run up to local elections.<sup>23</sup> This is the case even when all province-specific and time-varying factors are non-parametrically controlled (Columns IV and VIII), where all relevant local shocks to credit demand such as unemployment or growth are absorbed. State bank lending is between 10.3% and 14.2% lower compared with private-bank lending in election years.

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<sup>22</sup>Since all three elections are held in March, this strategy ensures that we capture a pre-election rather than a post-election effect in our yearly regressions. Note that this approach is also in line with previous literature (Englmaier and Stowasser, 2017).

<sup>23</sup>Table A2 shows that this result is not driven by a particular local election in our sample period.

Figure 3 shows results for the whole election cycle from regressions that control for local branches, baseline fixed effects, and province time trends. Each plotted coefficient corresponds to a single regression with an estimate of  $\beta_\tau$  when  $\tau$  is equal to -2, -1, 0, +1 or +2. Hence, coefficient estimates for  $\tau = 0$  in Panels A and B equal estimates reported in Columns III and VII, respectively, of Table 3. The figure shows that state banks start curbing credit with respect to private banks one year before an election, and they further reduce lending in an election year. However, they increase lending on a larger scale than private banks directly afterwards. This cycle seems slightly stronger in metropolitan provinces than in our full sample.

[Insert Table 3 near here]

[Insert Figure 3 near here]

This finding may at first seem counter-intuitive, since most earlier studies document a rise in state-bank lending in the run up to elections. There are two reasons why earlier findings and ours actually complement, rather than contradict, each other. First, our focus is on local election cycles rather than general elections that have been studied by previous literature (Dinç, 2005). In local elections, a central government’s control over state banks leads to different incentives across provinces depending on their political attractiveness (Brollo and Nannicini, 2012; Carvalho, 2014). Therefore, local elections do not necessarily imply an overall pre-election credit boom in the country. Second, earlier studies that investigate local elections and bank credit typically have political settings in which local governments are in direct control of local state banks (Cole, 2009; Englmaier and Stowasser, 2017). In that case, each local government would have an incentive to encourage pre-election lending to increase their reelection prospects, and thus there would be an overall credit boom in the country before elections. However, our political pressure channel goes from central government to state banks, which predicts a reallocation of credit across provinces but does not necessitate a rise in aggregate lending.

We next zoom in on the whole election cycle and check how lending by state banks evolves in the quarters immediately up to and after local elections. For this purpose, we estimate Equation 1 with the quarterly data provided by FinTürk over the period between 2007 and 2016, which covers two local elections.  $Election_t$  now takes the value of 1 in the first quarters of 2009 and 2014 as well as in the preceding three quarters (and 0 otherwise).<sup>24</sup> Thus, we can differentiate exactly between pre- and post-election outcomes since the  $Election_t$  dummy covers the four quarters immediately before the election takes place.

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<sup>24</sup>Exact election dates are 29 March in 2009 and 30 March in 2014.

Table 4 presents the results. In line with our earlier findings from yearly data, state banks reduce their lending in the four quarters up to and including elections when compared with private banks. Point estimates range from 6.4% in the full sample to 11.2% in the metropolitan sample; all coefficients are estimated with a high level of statistical significance across different sets of controls.

The main advantage of working with quarterly data is that we can pinpoint exactly when state banks alter their lending behaviour. We therefore extend our definition of the election variable to the whole cycle by employing a rolling definition of  $Election_{t+\tau}$ , where  $\tau$  corresponds to the quarters before and after elections. For instance,  $Election_{t-2}$  equals 1 for two to six quarters prior to the election and 0 otherwise.

Figure 4 plots coefficients for the entire credit cycle. 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 -11.4% two quarters before local elections, while it hits a trough at -6.7% in the election quarter in the full sample. This negative effect is estimated with precision in the five quarters leading up to the election and persists for another two to three quarters following it. These findings clearly illustrate that state bank credit is subject to a cycle around local elections. State banks reduce their lending prior to local elections and boost it afterwards compared with private banks, especially in metropolitan provinces.

[Insert Table 4 near here]

[Insert Figure 4 near here]

An important implication of these findings is that low frequency data may not be optimal to explore electoral cycles in bank lending. This point was first made by Akhmedov and Zhuravskaya (2004) in the context of political budget cycles. As the use of annual data do not allow a clear differentiation between pre- and post-election outcomes, studies may misinterpret the post-election rise in credit as direct evidence of political incentives. For instance, if  $Election_t$  dummy in Equation 1 was defined as the actual election year instead of the year before, our estimates in Table 3 would come out as significantly positive.<sup>25</sup> However, as can be seen from Figure 3/ 4, this would only be a post-election effect, which may not be directly driven by political motives.<sup>26</sup>

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<sup>25</sup>Notice that estimates for  $\beta_\tau$  would then be the same as current estimates for  $\beta_{\tau+1}$  in Figure 3.

<sup>26</sup>In fact, Önder and Özyıldırım (2013) find that state banks in Turkey increase their share in the credit market during local elections; the authors use the same yearly dataset as we do but with the definition of ‘actual election year’ and interpret their findings as a sign of political manipulation. As obvious from the discussion above, such a conclusion might be biased.

Although we find evidence that state banks' lending behaviour changes around elections, it is important to note that such intertemporal reallocation does not strictly imply political manipulation. It is possible that state banks are more sensitive than private banks to overall political uncertainty induced by local elections. As a result they may choose to postpone lending decisions until after elections take place. Since we document a recovery in state-bank lending a few quarters after elections, we do not yet rule out this possibility.

### 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 local 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 where a small number of votes can determine the outcome; that is, in closely contested elections. We should therefore find stronger reallocation of credit in provinces with high electoral competition if the election-induced cycle is driven (at least partly) by political goals. To test this idea, we extend Equation 1 to a *triple difference-in-differences* model as in the following:

$$\begin{aligned} \text{LogCredit}_{b,p,t} = & \beta_{\tau} \text{Comp}_{p,t} \times \text{StateBank}_b \times \text{Election}_{t+\tau} + \alpha_1 \text{Comp}_{p,t} \times \text{StateBank}_b \\ & + \alpha_2 \text{StateBank}_b \times \text{Election}_{t+\tau} + \alpha_3 \text{Comp}_{p,t} \times \text{Election}_{t+\tau} \\ & + \alpha_4 \text{Comp}_{p,t} + \delta X_{b,p,t-1} + \theta_b + \gamma_p + \lambda_t + \varepsilon_{b,p,t} \quad (2) \end{aligned}$$

where  $\text{Comp}_{p,t}$  represents the *Competition* variable created in Section 3. Notice that  $\text{Comp}_{p,t}$  is time-varying and we need to make an assumption on political contestedness for non-election years. We follow the literature in assuming that competition for the next two years after an election is captured by the previous election outcome, while it is captured by an upcoming election outcome for the two years before an election in that constituency (Cole, 2009; Englmaier and Stowasser, 2017). Despite the obvious endogeneity concern between credit as a dependent variable and competition as an independent variable in Equation 2, we believe 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.<sup>27</sup>

Our main coefficient of interest in Equation 2 is the triple-interaction effect denoted by  $\beta_{\tau}$ . It captures the impact of rising political competition in a province on the difference between

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<sup>27</sup>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.



state-bank and private-bank lending during an election year (i.e., when  $\tau = 0$ ). The two-way interactions underlying the triple effect absorb economically important effects and are also of interest. Based on the discussion in Section 4.1,  $\alpha_1$  accounts for the possibility that state banks may differ in their local lending behaviour depending on the political attractiveness of a province independent of an election cycle. Similarly,  $\alpha_2$  captures any election-induced effects that may differ between the two types of banks, while  $\alpha_3$  accounts for any responses to elections that may vary across provinces but not bank types. Hence, the model captures any shocks to banks or provinces that may be correlated with either the electoral cycle or the degree of contestedness in an election.

A central government’s incentives to redistribute resources across provinces depends not only on political attractiveness, but also on whether the incumbent mayor is a political ally or not. In particular, if a province is currently ruled by a mayor from the ruling party, then the central government has an interest in increasing voter appreciation and the re-election chances of the incumbent mayor. However, the opposite would be true if a mayor from opposition is currently in charge. It is thus optimal from the central government’s perspective to increase credit and positively influence economic conditions in politically aligned provinces, and to decrease credit and reduce economic activity in non-aligned provinces. Therefore, we divide our sample into two subsamples based on current mayoral incumbency and condition our expectations of  $\beta_\tau$  on political alliance. If *tactical redistribution* exists, we expect  $\beta_\tau > 0$  in aligned provinces and  $\beta_\tau < 0$  in non-aligned provinces just prior to the elections ( $\tau = 0$ ). As for the *constant patronage* argument: central government would ‘normally’ (i.e., in non-election periods) be expected to favor its strong supporters (less competitive areas) in allied provinces and more competitive areas in non-allied provinces. Hence, we would expect  $\beta_\tau$  to switch its sign further away from elections (for very low or high values of  $\tau$ ).

We estimate Equation 2 on both our yearly and quarterly data. Table 5 shows the yearly results for metropolitan provinces, where we expect our identification to be strongest (see Section 3). In line with a tactical redistribution mechanism, there is evidence that state banks lend more in provinces with higher political contestedness and an aligned incumbent mayor (i.e.,  $\beta_\tau > 0$  in columns I-V), while they significantly cut credit in provinces with higher political contestedness but ruled by an opposition mayor (i.e.,  $\beta_\tau < 0$  in columns VI-X). We report estimates in each sub-sample with varying degrees of saturation in our fixed effects and find especially strong results in non-aligned provinces.

Figure 5 reports our yearly results for the whole election cycle by plotting the coefficient estimates of the triple-interaction term ( $\beta_\tau$ ) for different values of  $\tau$ .<sup>28</sup> Panel A shows

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<sup>28</sup>The exact model used for the estimates shown in the figure includes our baseline controls and *Province time trends* as in Columns III and VIII in Table 5.

estimates from metropolitan provinces. In politically aligned provinces, state banks lend more than private banks in the election year especially when political competition is high, and this effect persists in the post-election period. In non-aligned provinces, the drop in state-bank lending in the election year similarly persists one year after the election before recovering. These findings suggest that the central government may continue its tactical redistribution even after elections by rewarding constituencies in which it narrowly won, and punishing regions in which it narrowly lost elections. There is also some support for constant patronage hypothesis as  $\beta_\tau$  switches signs when the central government does not have electoral concerns but would rather favor areas where it faces stronger support in general ( $\tau = -2$  and  $+2$ ). The same patterns are also observed in Panel B, which shows estimates from the full sample of provinces, although coefficients have less precision.

[Insert Table 5 near here]

[Insert Figure 5 near here]

It is crucial to differentiate between pre- and post-election effects to understand the exact nature of the lending cycle. We re-estimate Equation 2 with the quarterly data to see the effects of tactical redistribution in a more granular timeline.<sup>29</sup> Table 6 shows the corresponding estimates. In line with our yearly results, politically aligned provinces benefit from a relative rise in credit supply by state banks when elections are closely contested, while non-aligned provinces suffer from a relative reduction. Estimates are statistically significant for non-aligned provinces and comparable to those reported in Table 5. A one standard-deviation rise in the competitiveness of an opposition province leads to a decline of almost 6% in state-bank loans on the election year compared with private banks. This effect is quite sizable given that our credit measure covers the entire state-bank lending in a province. It is also comparable to results by Cole (2009), who finds that state banks increase agricultural lending by 5-10 percentage points in an election year.

[Insert Table 6 near here]

[Insert Figure 6 near here]

Figure 6 illustrates the presence of tactical reallocation over the full election cycle, which covers ten quarters before and after an election. It is clear from Panel A that targeted

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<sup>29</sup>As noted earlier, our quarterly observations start from the end of 2007 and hence do not cover the first local election in 2004. However, we do not expect this to be driving our previous results. Indeed, one could predict a more intense pre-election manipulation in the last two local elections since they correspond to a later period in which the ruling party has consolidated its control over government institutions.

redistribution starts at least four quarters before an election. It is strongest in two to three quarters prior to an election, but it quickly disappears following an election. In both the metropolitan and full samples, politically non-aligned provinces suffer from a relative reduction in lending by state banks for multiple quarters in the run up to closely contested elections. Again, for constant patronage argument, it is clear that  $\beta_\tau$  switches signs further away from elections (though not always statistically significant) consistent with the view that without election concerns, central government would favour its strongholds.

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. There could be alternative explanations for why state banks in general would behave differently around elections (e.g. flight to safety amongst depositors induced by political uncertainty). There could also be reasons why certain provinces get a higher share of state-bank loans than others (e.g. banks may specialise in lending to certain industries, which agglomerate in certain provinces). However, without resorting to the argument of political incentives, it is very difficult to explain why such cross-sectional relationships would vary in different directions based on local political alignment and exactly prior to local elections.

In order to shed more light on political incentives, we explore the channels through which the central government engages in tactical redistribution. Our aggregate credit data can be broken down by lending to different segments of the economy. This allows us to test whether targeted lending occurs in certain segments but not others, which helps us understand what voters respond to. On the one hand, politicians may try to induce a quick and direct impact on voters by raising their instant consumption. [Healy and Lenz \(2014\)](#) find that voters judge U.S. presidential candidates on the election-year economy because this is the most immediately available metric to them for judging a president's performance. However, given that province mayors have no direct control over bank credit supply in Turkey which is widely known by the public, it is difficult to argue that a change in consumer loans would have a direct impact on consumers' perception about the incumbent mayor.

On the other hand, politicians may be tempted to use bank credit to boost or contain corporate activity in a region. This would be more likely to influence voting patterns if corporates have a say in local politics and voters – at least partly – attribute corporates' economic outcomes to local politicians. For instance, [Carvalho \(2014\)](#) finds evidence in line with this view and shows that the central government in Brazil provides favourable credit to firms in aligned regions, who in turn expand employment to increase the re-election chances of incumbents. Although the consumer and corporate channels are not mutually exclusive, we expect the latter to be dominant in the Turkish political setting given its similarity to that of Brazil.

[Insert Figure 7 near here]

Figure 7 plots quarterly estimates from Equation 2 separately for corporate and consumer loans for different values of  $\tau$ . A simple comparison between Panels A and B confirms our expectation that tactical redistribution is mainly targeted at corporate loans. The coefficient estimates are sizable and statistically significant for both aligned and non-aligned provinces in the case of corporate loans. On average, a one standard deviation change in competition leads state banks to increase corporate loans by 9.8% in aligned municipalities and reduce it by 15.7% in non-aligned ones in the election year when compared with private banks. The positive impact in aligned provinces peaks precisely on the election quarter, while the negative impact in non-aligned provinces hits the bottom two quarters prior to the election. There is also statistical evidence that these patterns reverse in periods away from elections, supporting the notion that government might be pursuing patronage in those quarters.

In contrast, estimates for consumer loans are all insignificant and show no visible pattern around elections. We therefore conclude that the central government’s reallocation of state-bank credit targets firms’ credit access and aims to influence local economic and voting outcomes through the corporate channel. This leads us to investigate the effects of such redistribution on economic activity in the next section.

#### 4.4. *How does political lending affect economic outcomes?*

We have so far established that there is an election-induced cycle in state-bank loans, which affects especially the corporate sector. Such lending is targeted at politically competitive provinces based on their political alignment. In ruling-party constituencies, it takes the form of rewarding the competitive region by increasing credit supply, and in opposition regions, it takes the form of punishment by lowering state-bank lending. This gives us a quasi-exogenous source of variation in the amount of total bank credit around local election times that provinces receive depending on their alignment. We now ask whether this variation in credit translates into real outcomes. If it does, then ‘swing’ provinces ruled by an opposition mayor are expected to suffer from lower economic activity around elections compared with provinces governed by a politically aligned mayor.

Since there is no data currently available on province-level GDP, we draw on a new dataset that contains all construction permits issued over the sample period in Turkey as a proxy for local economic activity. To abstract from the possibility that central government may directly interfere in the construction industry via state-funded institutions,<sup>30</sup> we only keep private sector activity in our sample. Construction by private entities is likely to be a

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<sup>30</sup>See Marschall et al. (2016).

good proxy for overall economic activity, since it tends to have a high correlation with an economy’s growth rate.

Formally, we estimate the following model:

$$\begin{aligned} \text{LogActivity}_{p,t} = & \beta_{\tau} \text{Opposition}_{p,t} \times \text{Comp}_{p,t} \times \text{Election}_{t+\tau} + \alpha_1 \text{Comp}_{p,t} \times \text{Election}_{t+\tau} \\ & + \alpha_2 \text{Comp}_{p,t} \times \text{Opposition}_{p,t} + \alpha_3 \text{Opposition}_{p,t} \times \text{Election}_{t+\tau} \\ & + \alpha_4 \text{Comp}_{p,t} + \alpha_5 \text{Opposition}_{p,t} + \delta X_{b,p,t-1} + \theta_b + \gamma_p + \lambda_t + \varepsilon_{b,p,t} \quad (3) \end{aligned}$$

where  $\text{LogActivity}_{p,t}$  is computed in three different ways. First, *Log Flats* measures (in logs) the total number of flats constructed by the private sector in province  $p$  in year  $t$ . Second, *Log SqMtr* measures the total square meter area covered by new construction. Third, *Log Value* measures the total monetary value of new construction. We use all three measures as alternative dependent variables. In order to control for potential drivers of construction activity, we include the lagged population size of each province over time (in logs).  $\text{Opposition}_{p,t}$  indicates whether a province is governed by a mayor affiliated with an opposition party or not. The main coefficient of interest is  $\beta_{\tau}$ , which measures the economic impact of being in an opposition province with high political contestedness around election times compared with being in an aligned province. If opposition regions suffer from a credit squeeze as we have shown previously, then one would expect to find a negative impact on local economic activity as captured by  $\beta_{\tau} < 0$  around elections.<sup>31</sup>

Table 7 presents estimates of this regression for the election year (i.e.,  $\tau = 0$ ). For all three of our dependent variables and across varying sets of controls, the triple-interaction term carries a significantly negative value. The estimated effects are economically substantial. *Ceteris paribus*, a one standard deviation increase in electoral competition would decrease the number of flats constructed in an opposition province by almost 10% in an election year. Given that new construction activity accounts directly for around 8-9% of GDP in Turkey, only the effect of credit on construction itself would translate into almost a 1% reduction in total economic output.

To observe the full election cycle in local economic activity, Figure 8 plots estimates of  $\beta_{\tau}$  for  $\tau = -2, -1, 0, 1, 2$ . Panels A-C report results on *Log Flats*, *Log SqMtr*, and *Log Value*, respectively. For all three measures of activity, there is a visible downward trend in opposition areas with high political competition as elections get closer. In line with the persistence of the lending cycle beyond elections documented earlier, we find that construction activity lags in opposition provinces one year after elections take place. Hence, withdrawal of credit

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<sup>31</sup>Since construction sector usually responds to local economic factors with a lag, we define  $\text{Election}_t$  dummy according to ‘actual election’ years.

by state banks in politically competitive provinces under an opposition mayor leads to a significant distortion of economic activity.

[Insert Table 7 near here]

[Insert Figure 8 near here]

## 5. Conclusion

In this paper, we test for the presence of politically motivated distribution of resources in Turkey using a dataset with detailed information on banking activity and local economic outcomes. Our dataset is novel along several dimensions and helps us achieve stronger identification than earlier studies, while shedding light on some of the theoretical arguments voiced in the literature.

Our main findings are two-fold. First, we show that state banks in Turkey engage in politically motivated lending around local elections when compared with private banks. In particular, they increase lending to the corporate sector in politically attractive provinces when an incumbent mayor is aligned with the ruling party, while they reduce it if the incumbent mayor is from an opposition party. Second, this redistribution of credit has real consequences. Specifically, it leads to a significant reduction in local economic activity in opposition provinces that are politically contested.

Our findings around elections support the idea of tactical redistribution. Rolling estimations in non-election years show some evidence that central government may have resorted to patronage when it did not have election concerns. In ongoing work, we ask whether re-allocation of bank credit helps the central government increase the electoral success of its allied mayoral candidates. To the extent that it does, it may provide one of the first pieces of evidence on how voters can be manipulated via financial intermediaries.

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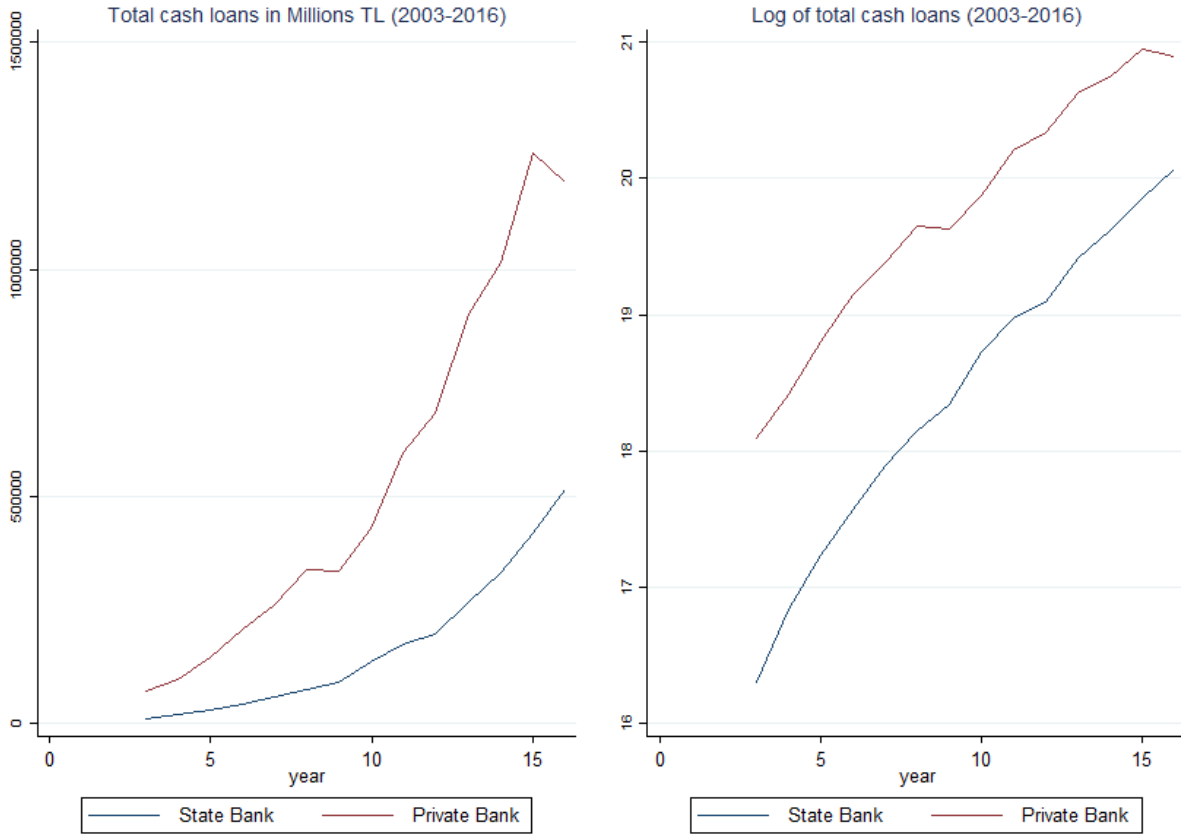
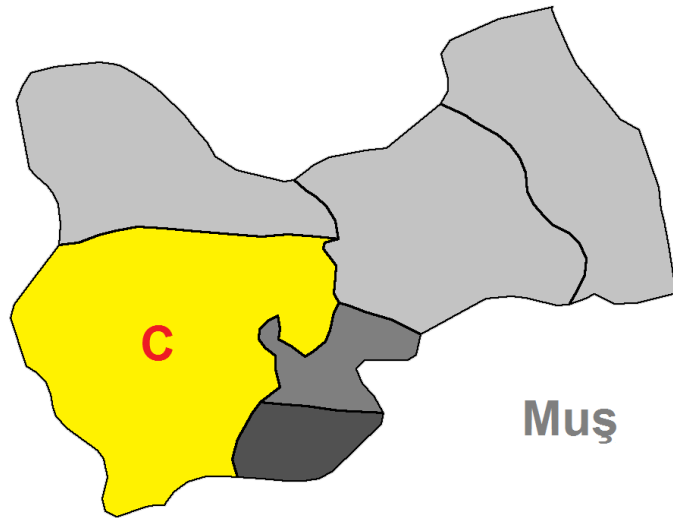
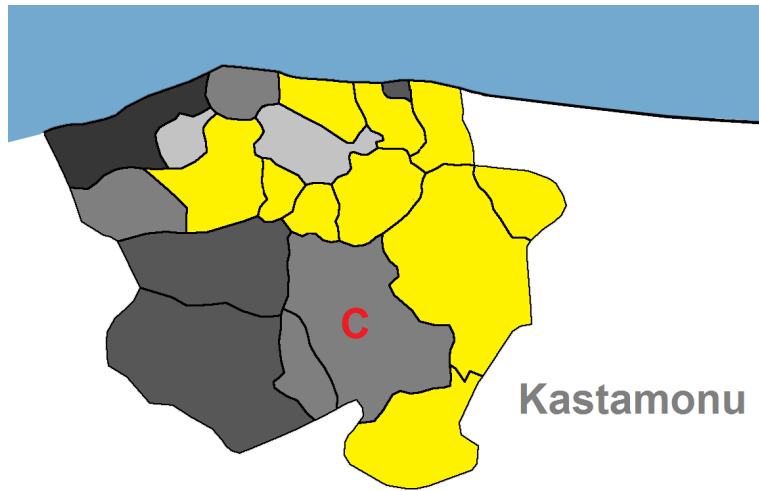


Fig. 1. **Evolution of aggregate credit by bank type, 2003-2016.** This figure shows the evolution of the stock of all cash loans extended by state-owned and private banks during the period 2003-2016.

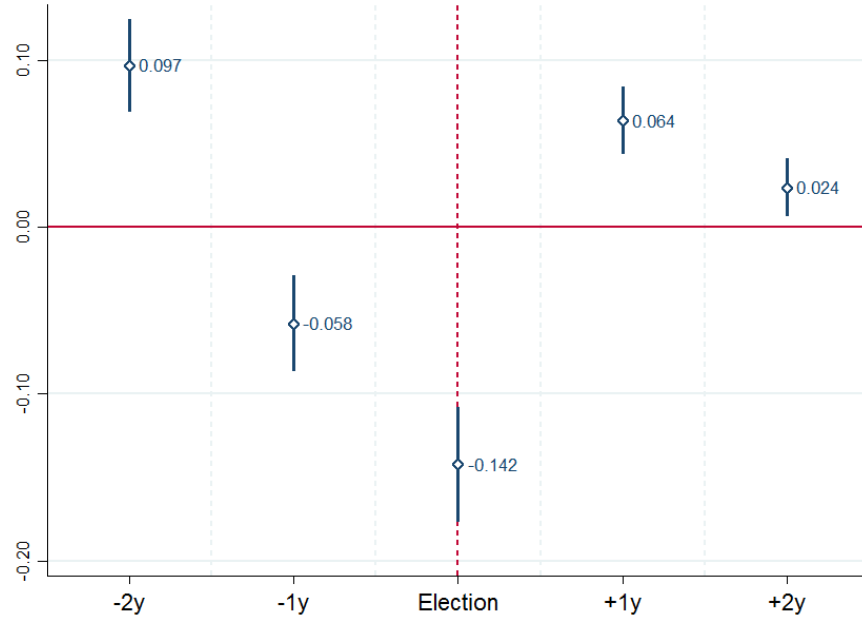


(a) An allied province in 2004 elections

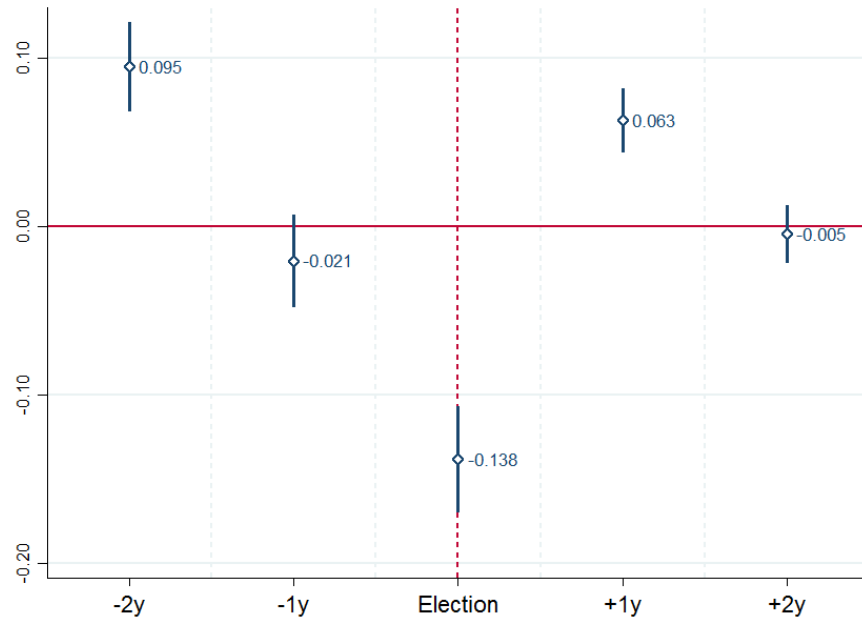


(b) A non-allied province in 2004 elections

Fig. 2. **District-level alliances in two non-metropolitan provinces.** Panel A shows a province in which the elected central district mayor is allied with the central government and Panel B shows a province in which the elected central district mayor is non-allied. ‘C’ in red colour stands for the central district. Allied districts are given in yellow and non-allied districts are given in varying shades of gray corresponding to different opposition parties.

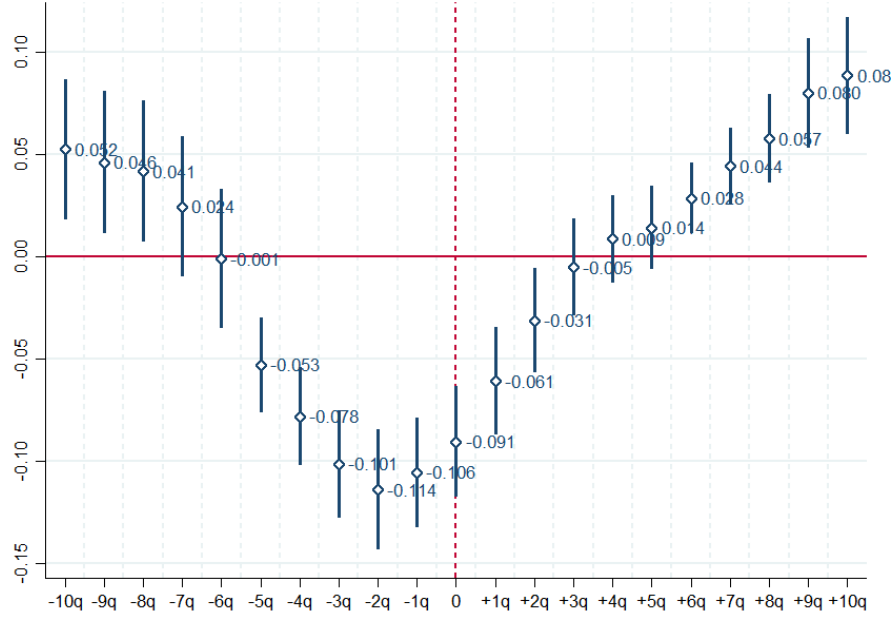


(a) Metropolitan provinces

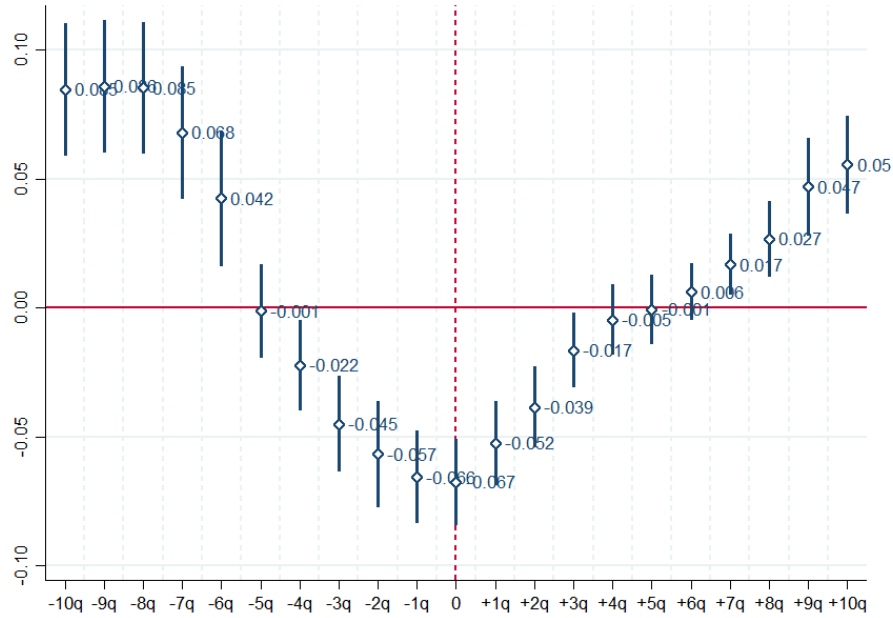


(b) All provinces

Fig. 3. **State vs private bank behaviour over the full election cycle: Yearly estimates (2003-2016)**. This figure shows results of equation (1) estimated on yearly data when  $\tau$  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.

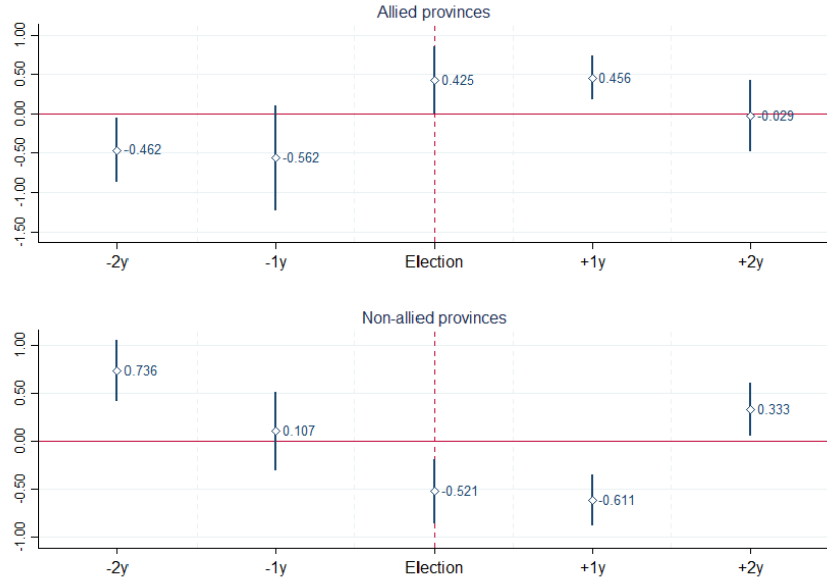


(a) Metropolitan provinces

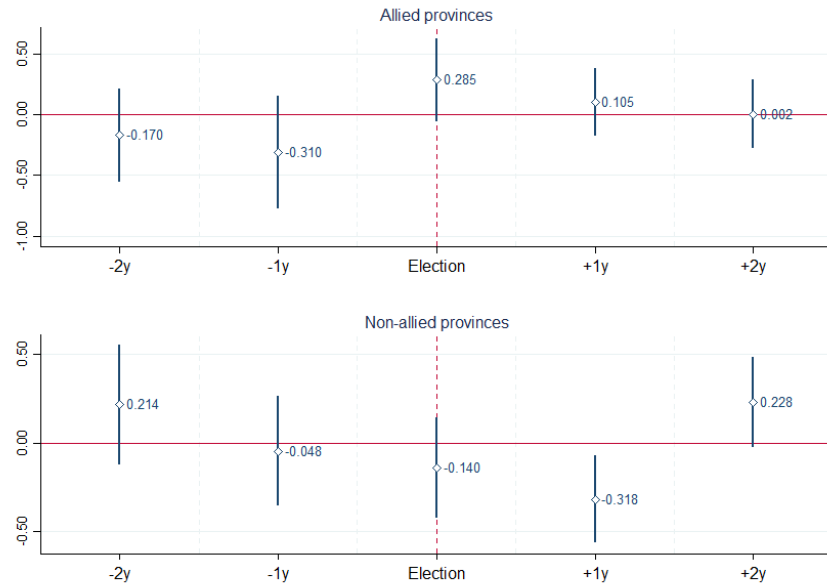


(b) All provinces

Fig. 4. **State vs private bank behaviour over the full election cycle: Quarterly estimates (2007q4-2016q4)**. This figure shows results of equation (1) estimated on quarterly data when  $\tau$  takes values from -10 to +10, indicating the number of quarters 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.

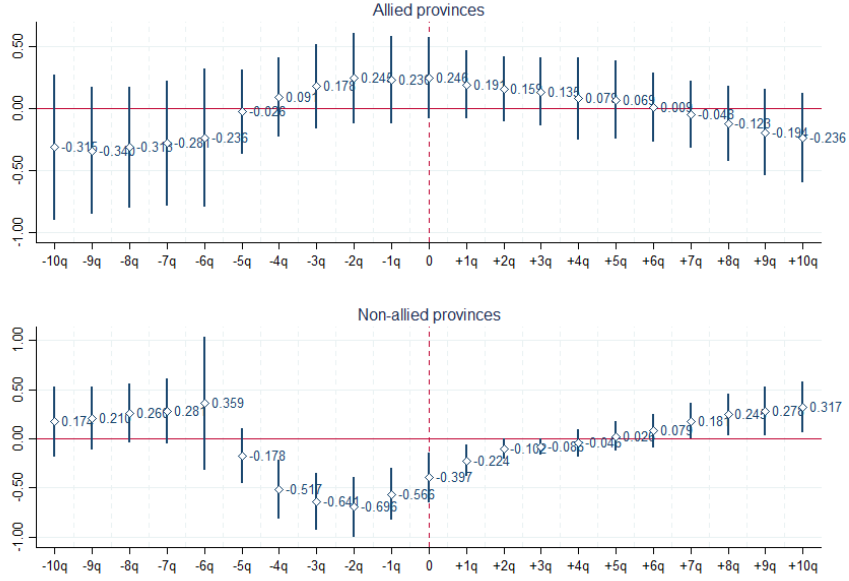


(a) Metropolitan provinces

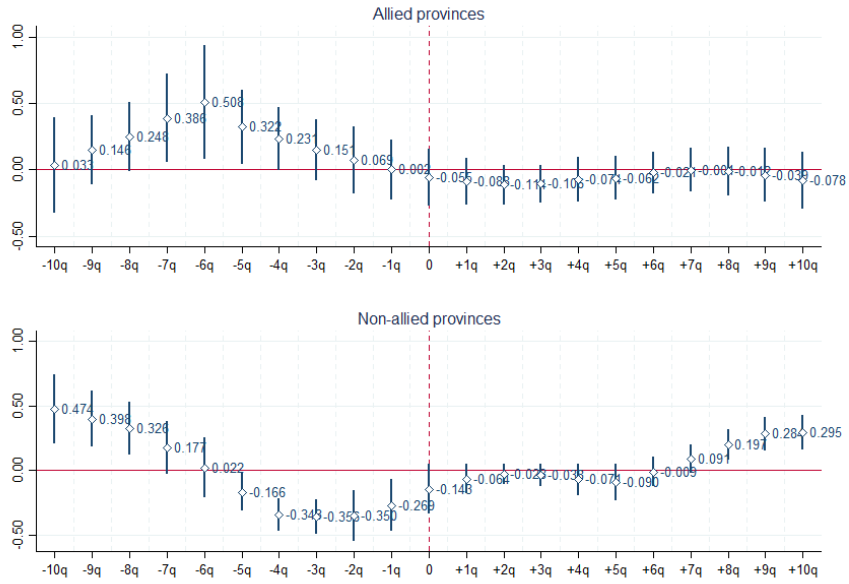


(b) All provinces

Fig. 5. **Tactical redistribution of state-bank credit over the full election cycle: Yearly estimates (2003-2016).** This figure shows results of equation (2) estimated on yearly data when  $\tau$  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; estimates are reported separately for aligned and non-aligned provinces in each panel.

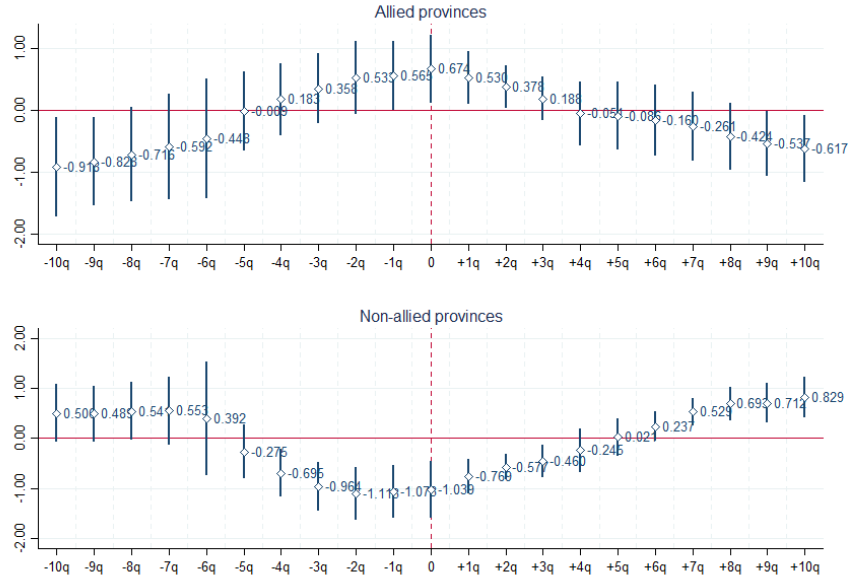


(a) Metropolitan provinces

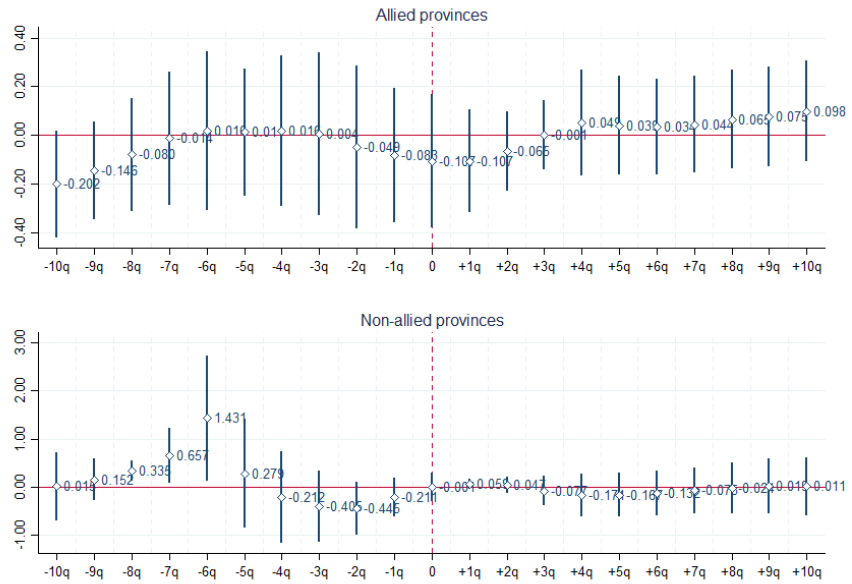


(b) All provinces

Fig. 6. **Tactical redistribution of state-bank credit over the full election cycle: Quarterly estimates (2007q4-2016q4).** This figure shows results of equation (2) estimated on quarterly data when  $\tau$  takes values from -10 to +10, indicating the number of quarters 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; estimates are reported separately for aligned and non-aligned provinces in each panel.



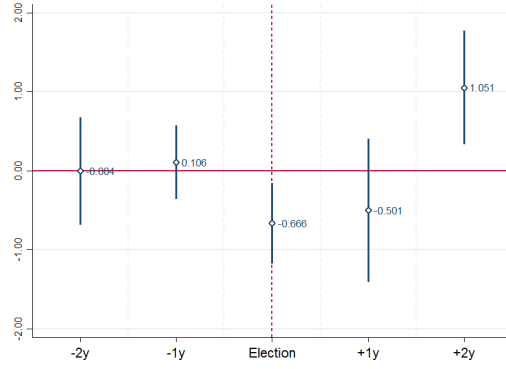
(a) Corporate loans



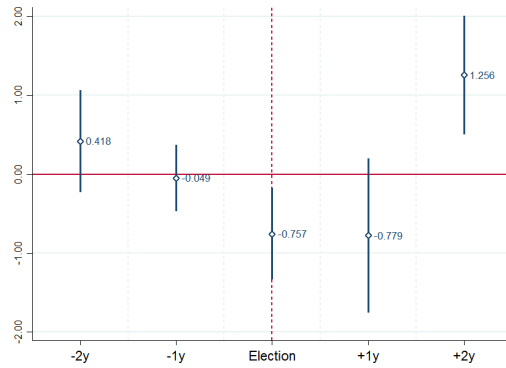
(b) Consumer loans

Fig. 7. **Corporate vs consumer loans: Tactical redistribution of state-bank credit over the full election cycle (2007q4-2016q4).** This figure shows results of equation (2) estimated on quarterly data when  $\tau$  takes values from -10 to +10, indicating the number of quarters 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 shows estimates for corporate loans and panel B shows estimates for consumer loans; estimates are reported separately for aligned and non-aligned provinces in each panel.

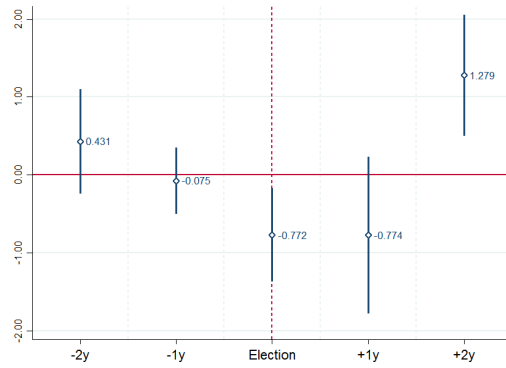




(a) Dependent variable: LogFlats



(b) Dependent variable: LogSqMtr



(c) Dependent variable: LogValue

**Fig. 8. Effect of competition and alliance on local economic activity (2003-2016).** This figure shows results of equation (3) estimated on yearly data when  $\tau$  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 our baseline set of fixed effects. Panel A shows estimates for the dependent variable total number of flats, panel B shows estimates for total square meter area and panel C shows estimates for total value in Turkish liras.

<i>Panel A</i>		<i>Years</i>		
		<u>1999</u>	<u>2004</u>	<u>2015</u>
<b>Number of banks</b>		<b>54</b>	<b>34</b>	<b>33</b>
	State	4	3	3
	Private	50	31	30
<b>Number of branches</b>		<b>6,946</b>	<b>6,087</b>	<b>11,150</b>
	State	2,865	2,149	3,681
	Private	4,081	3,938	7,469
<b>Number of employees</b>		<b>152,578</b>	<b>122,227</b>	<b>195,613</b>
	State	72,007	39,467	58,211
	Private	80,571	82,760	137,402

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<i>Panel B</i>				
<b>NPL/Loans</b>	State	10.0%	11.1%	2.7%
	Private	3.6%	4.9%	3.3%
<b>ROA</b>	State	1.1%	2.5%	1.4%
	Private	4.5%	1.6%	1.0%
<b>Equity/Assets</b>	State	4.1%	9.4%	10.1%
	Private	12.9%	15.8%	11.0%

Table 1: **Composition and performance of Turkish banking sector over time.** This table summarizes the composition and financial performance of the banking sector in Turkey. State banks are defined as banks in which the central government has a controlling stake. Private banks are defined as all other banks. We exclude investment banks, development banks, and participation banks. NPL denotes non-performing loans. ROA denotes Return on Assets. Source: Banks Association of Turkey (BAT) & authors' calculations.

<i>Variables</i>	<i>Mean</i>	<i>Median</i>	<i>Std. Deviation</i>	<i>Min</i>	<i>Max</i>	<i>Observations</i>	<i>Source</i>
<i>LogCredit</i>	13.28	13.29	1.77	8.05	20.36	2,268	CBRT, BAT
<i>LogCredit (state-bank)</i>	13.14	13.17	1.59	8.51	18.83	1,134	CBRT, BAT
<i>LogCredit (private-bank)</i>	13.42	13.44	1.92	8.05	20.36	1,134	CBRT, BAT
<i>LogCredit</i>	13.95	13.88	1.47	9.41	20.15	5,994	FinTürk
<i>LogCredit (state-bank)</i>	13.85	13.77	1.27	10.80	19.07	2,997	FinTürk
<i>LogCredit (private-bank)</i>	14.05	14.05	1.65	9.41	20.15	2,997	FinTürk
<i>Alliance (dummy)</i>	0.60	1.00	0.49	0.00	1.00	243	TurkStat
<i>Competition</i>	0.8231	0.8568	0.1459	0.2391	0.9996	243	TurkStat
<i>LogFlats</i>	7.90	7.89	1.40	2.56	12.23	1,133	TurkStat
<i>LogSqMtr</i>	13.20	13.19	1.38	7.86	17.32	1,133	TurkStat
<i>LogValue</i>	19.49	19.53	1.53	13.82	24.19	1,133	TurkStat

**Table 2: Summary statistics.** This table presents summary statistics for the main variables in our analysis. Credit data from CBRT/BAT are annual, while credit data from FinTürk are quarterly. Alliance indicates whether a province is ruled at the time by a mayor from the ruling party or not. Competition is defined as 1 minus the win margin. Flats, SqMtr, and Value refer to the number of flats, total area measured in m<sup>2</sup>, and value in Turkish Liras, respectively, of newly constructed buildings.

	Metropolitan Sample				Full Sample			
	I	II	III	IV	V	VI	VII	VIII
<i>StateBank x Election</i>	-0.128*** [0.019]	-0.142*** [0.020]	-0.142*** [0.020]	-0.142*** [0.027]	-0.103*** [0.018]	-0.138*** [0.019]	-0.138*** [0.019]	-0.139*** [0.026]
<b>Controls</b>								
<i>Local branches</i>		Yes	Yes	Yes		Yes	Yes	Yes
<i>Bank type FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Province FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Time FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>ProvinceTime trends</i>			Yes			Yes		
<i>Province x Time FE</i>				Yes				Yes
<b>Clustering</b>	Province	Province	Province	Province	Province	Province	Province	Province
N	840	840	840	840	2,268	2,268	2,268	2,268
Adj-R-sq	0.947	0.963	0.967	0.950	0.898	0.945	0.951	0.928

Table 3: **State-bank behaviour in election years: Yearly estimates (2003-2016)**. This table shows results of equation (1) estimated on yearly data. Columns I-IV include metropolitan provinces and columns V-VIII include the full sample. Standard errors are provided in brackets; \*, \*\*, \*\*\* indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

	Metropolitan Sample				Full Sample			
	I	II	III	IV	V	VI	VII	VIII
<i>StateBank x Election</i>	-0.112*** [0.017]	-0.090*** [0.016]	-0.091*** [0.016]	-0.090*** [0.022]	-0.064*** [0.011]	-0.068*** [0.010]	-0.067*** [0.010]	-0.068*** [0.014]
<b>Controls</b>								
<i>Local branches</i>		Yes	Yes	Yes		Yes	Yes	Yes
<i>Bank type FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Province FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Time FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>ProvinceTime trends</i>			Yes				Yes	
<i>Province x Time FE</i>				Yes				Yes
<b>Clustering</b>	Province	Province	Province	Province	Province	Province	Province	Province
N	2,220	2,220	2,220	2,220	5,994	5,994	5,994	5,994
Adj-R-sq	0.932	0.967	0.975	0.965	0.833	0.944	0.956	0.932

Table 4: **State-bank behaviour in election years: Quarterly estimates (2007q4-2016q4)**. This table shows results of equation (1) estimated on quarterly data. Columns I-IV include metropolitan provinces and columns V-VIII include the full sample. Standard errors are provided in brackets; \*, \*\*, \*\*\* indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

	Allied Provinces					Non-allied Provinces				
	I	II	III	IV	V	VI	VII	VIII	IX	X
<i>Comp x StateBank x Election</i>	0.399* [0.228]	0.424* [0.240]	0.425* [0.245]	0.425 [0.322]	0.448 [0.321]	-0.571*** [0.146]	-0.517** [0.191]	-0.521** [0.192]	-0.520* [0.249]	-0.482** [0.210]
<i>Comp x StateBank</i>	-0.547 [0.585]	0.138 [0.369]	0.143 [0.381]	0.152 [0.498]	0.112 [0.494]	-0.971* [0.540]	-0.439 [0.526]	-0.480 [0.542]	-0.473 [0.705]	-0.575 [0.510]
<i>StateBank x Election</i>	-0.422** [0.188]	-0.452** [0.197]	-0.452** [0.202]	-0.452 [0.264]		0.324** [0.115]	0.269 [0.157]	0.273 [0.158]	0.272 [0.205]	
<i>Comp x Election</i>	-0.269** [0.104]	-0.314*** [0.104]	-0.306*** [0.104]			0.555*** [0.081]	0.502*** [0.098]	0.374** [0.129]		
<b>Controls</b>										
<i>Local branches</i>		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
<i>Bank type FE</i>	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	
<i>Province FE</i>	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	
<i>Time FE</i>	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	
<i>ProvinceTime trends</i>			Yes					Yes	Yes	
<i>Province x Time FE</i>					Yes				Yes	Yes
<i>Bank x Time FE</i>										Yes
<b>Clustering</b>	Province	Province	Province	Province	Province	Province	Province	Province	Province	Province
N	502	502	502	502	502	338	338	338	338	338
Adj-R-sq	0.937	0.961	0.964	0.945	0.964	0.959	0.965	0.969	0.955	0.977

Table 5: **Tactical reallocation in metropolitan provinces: Yearly estimates (2003-2016)**. This table shows results of equation (2) estimated on yearly data. Columns I-V include allied provinces and columns VI-X include non-allied provinces. Standard errors are provided in brackets; \*, \*\*, \*\*\* indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

	Allied Provinces					Non-allied Provinces				
	I	II	III	IV	V	VI	VII	VIII	IX	X
<i>Comp x StateBank x Election</i>	0.296 [0.203]	0.244 [0.191]	0.246 [0.192]	0.245 [0.261]	0.266 [0.249]	-0.501*** [0.167]	-0.390** [0.146]	-0.397** [0.145]	-0.395* [0.198]	-0.492* [0.261]
<i>Comp x StateBank</i>	-0.898** [0.433]	-0.220 [0.170]	-0.239 [0.170]	-0.231 [0.232]	-0.206 [0.235]	-0.550 [0.372]	0.263 [0.360]	0.217 [0.370]	0.230 [0.507]	0.159 [0.487]
<i>StateBank x Election</i>	-0.326** [0.146]	-0.254* [0.138]	-0.256* [0.139]	-0.255 [0.189]		0.299** [0.130]	0.221* [0.109]	0.225* [0.108]	0.224 [0.147]	
<i>Comp x Election</i>	-0.186 [0.126]	-0.179* [0.101]	-0.286** [0.119]			0.326 [0.198]	0.248 [0.156]	0.240*** [0.070]		
<b>Controls</b>										
<i>Local branches</i>		Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
<i>Bank type FE</i>		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Province FE</i>		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Time FE</i>		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>ProvinceTime trends</i>			Yes				Yes	Yes		
<i>Province x Time FE</i>				Yes	Yes		Province	Province	Province	Province
<i>Bank x Time FE</i>					Yes		Province	Province	Province	Province
<b>Clustering</b>	Province	Province	Province	Province	Province	Province	Province	Province	Province	Province
N	1,236	1,236	1,236	1,236	1,236	984	984	984	984	984
Adj-R-sq	0.929	0.969	0.974	0.964	0.966	0.948	0.971	0.978	0.966	0.967

Table 6: **Tactical reallocation in metropolitan provinces: Quarterly estimates (2007q4-2016q4)**. This table shows results of equation (2) estimated on quarterly data. Columns I-V include allied provinces and columns VI-X include non-allied provinces. Standard errors are provided in brackets; \*, \*\*, \*\*\* indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

<i>Dependent variable:</i>	<i>LogFlats</i>			<i>LogSqMtr</i>			<i>LogValue</i>		
	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>	<i>V</i>	<i>VI</i>	<i>VII</i>	<i>VIII</i>	<i>IX</i>
<i>Opposition x Comp x Election</i>	-0.680** [0.307]	-0.666** [0.307]	-0.519* [0.305]	-0.764** [0.349]	-0.757** [0.349]	-0.605* [0.351]	-0.780** [0.361]	-0.772** [0.360]	-0.628* [0.361]
<i>Comp x Election</i>	0.225 [0.216]	0.209 [0.218]	0.091 [0.221]	0.183 [0.213]	0.175 [0.214]	0.061 [0.219]	0.183 [0.212]	0.175 [0.214]	0.058 [0.220]
<i>Comp x Opposition</i>	0.271 [0.468]	0.257 [0.466]	0.009 [0.491]	0.264 [0.387]	0.257 [0.385]	-0.113 [0.397]	0.132 [0.406]	0.125 [0.403]	-0.206 [0.411]
<i>Opposition x Election</i>	0.502** [0.246]	0.489* [0.246]	0.385 [0.244]	0.596** [0.279]	0.590** [0.279]	0.481* [0.282]	0.604** [0.288]	0.597** [0.288]	0.495* [0.289]
<b>Controls</b>									
<i>Local population</i>		Yes			Yes			Yes	
<i>Province FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Time FE</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Province Time trends</i>			Yes			Yes		Yes	Yes
<b>Clustering</b>	Province	Province	Province	Province	Province	Province	Province	Province	Province
<i>N</i>	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133
<i>Adj-R-sq</i>	0.607	0.611	0.689	0.628	0.629	0.686	0.807	0.807	0.837

Table 7: **Political competition, alliance, and local economic activity (2003-2016)**. This table shows results of equation (3). Standard errors are provided in brackets; \*, \*\*, \*\*\*, \*\*\* indicate statistical significance at the level of 10%, 5%, and 1%, respectively.



Years	Assets/GDP			Loans/GDP			Deposit/GDP		
	State	Private	Total	State	Private	Total	State	Private	Total
1999	0.23	0.37	0.60	0.06	0.12	0.17	0.18	0.22	0.40
2000	0.21	0.32	0.53	0.05	0.11	0.17	0.16	0.19	0.35
2001	0.22	0.40	0.62	0.04	0.11	0.14	0.15	0.30	0.46
2002	0.19	0.35	0.54	0.03	0.11	0.13	0.14	0.24	0.38
2003	0.18	0.32	0.50	0.03	0.11	0.13	0.13	0.20	0.33
2004	0.19	0.32	0.51	0.04	0.13	0.17	0.14	0.20	0.34
2005	0.18	0.38	0.57	0.05	0.17	0.22	0.14	0.23	0.38
2006	0.18	0.41	0.59	0.06	0.20	0.26	0.14	0.25	0.40
2007	0.19	0.43	0.61	0.07	0.23	0.31	0.15	0.26	0.41
2008	0.21	0.48	0.69	0.09	0.27	0.35	0.16	0.29	0.46
2009	0.25	0.52	0.77	0.10	0.26	0.36	0.19	0.32	0.51
2010	0.26	0.54	0.80	0.13	0.30	0.42	0.20	0.33	0.53
2011	0.24	0.56	0.80	0.13	0.32	0.46	0.17	0.33	0.50
2012	0.24	0.55	0.79	0.13	0.34	0.47	0.17	0.32	0.49
2013	0.27	0.60	0.86	0.16	0.38	0.53	0.18	0.34	0.52
2014	0.27	0.61	0.88	0.17	0.39	0.56	0.17	0.35	0.52
2015	0.29	0.62	0.91	0.19	0.40	0.59	0.18	0.36	0.53

Table A1: **Growth in Turkish banking sector (1999-2015)**. This table shows the relative size of the banking activities in Turkey with respect to country's GDP in each year between 1999 and 2015.

