Electoral cycles in savings bank lending

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Abstract

We test the hypothesis that incumbent politicians try to boost economic conditions in pre-election periods. We exploit a peculiarity in the German public banking system where local politicians are by law involved in the management of local savings banks. We use the different timing of municipal elections across states and the existence of cooperative banks as a control group to estimate the causal effect of elections on lending policy of German savings banks. We use a novel, largely hand-collected dataset, merging municipal election outcomes for a subset of German states with accounting information of local banks. Econometrically, we conduct difference-in-difference (DD) as well as triple-difference (DDD) estimation embedded in a fixed-effects panel data setup. We find that savings banks systematically extend more credits in pre-election periods. This effect is robust to various specifications. Importantly, the effect is not present with cooperative banks that are very similar to savings banks but that lack the political connectedness of savings banks. We find weak evidence for overly prudent lending policies after elections and that lending cycles are more likely the stronger the dominance of the incumbent party. We also find significant differences in effect sizes across states, suggesting an interaction with other details of the institutional environment that call for further analysis in future research.

Keywords: Bank lending cycles, political business cycles, political connectedness, public banks

JEL classification: G21, D72, D73

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1 Introduction

Politicians regularly rank highly in polls of people's most mistrusted professions, ahead of usual suspects such as car salesmen or estate agents.[1] Arguably, the most important reason for this unflattering ranking is, according to modern political economics, that politicians are not necessarily interested in what is best for their constituency but rather in what they can get out of it for themselves. This may take on drastic forms such as outright corruption and illegal enrichment or more subtle ones like a preference for policies that benefit re-election prospects but are socially suboptimal. A prime example for the latter is the theory of (opportunistic) political business cycles (PBC) pioneered by Nordhaus (1975), and MacRae (1977), which describes politicians' incentives to enact expansionary fiscal policies shortly before elections to boost their own popularity, only to countermand them with contractionary policies afterwards. While this theory has been empirically put to test in numerous studies (e.g. Alesina *et al.*, 1997; Mitchell and Willett, 2006; and Schneider, 2010), the literature so far has concentrated on the behavior of politicians on the federal or state level. As a consequence, only little is known about electoral cycles on the municipal level, even though local office holders should have incentives to strategically affect the regional economy as well.

Though local politicians are not known for having control over macro policies like stimulus packages, the federal tax code, or interest rates, it may not take large-scale measures to have a significant impact on a local economy that is of small scale itself. It probably is enough to invest in long overdue road maintenance, to inaugurate a recreational park, or to vigorously – and, more importantly, visibly – fight for the preservation of jobs at a local business in dire straits, to shift the mood of the electorate in one's favor. Moreover, the literature on political connectedness (PC) makes the point that the ability to manipulate economic outcomes may not depend on statutory authority alone, but also on social relationships that create a more informal opportunity to achieve one's goals. Most contributions to the PC literature such as Fisman (2001), Johnson and Mitton (2003), Ramalho (2004), Sapienza (2004), Faccio (2006), and Faccio *et al.* (2006) focus on the advantages for *firms* from entertaining close ties with politicians. Yet, the reverse direction of these relationships is being documented by academic work as well, as evidenced by Shleifer and Vishny (1994) who

[1] To name just a few of these recurring surveys, consider the "Veracity Index" by *Ipsos*, *YouGov's* "KRC poll" and the "Voice of the People" study conducted by *Gallup International*.

develop the quid-pro-quo argument of favors being traded between (state-owned) corporations and the political sector.

A key takeaway from the PC literature is that the degree to which political connections can affect real outcomes greatly depends on a country's institutional environment: in general, the weaker the institutions, the easier it will be for political and business elites to form networks that allow for rent extraction. On this account, it is not surprising that most of the empirical evidence of harmful political ties comes from developing or emerging countries. A notable exception is the study by Bertrand *et al.* (2007) who find that French firms destroy less jobs in election years, when they are run by CEOs who are politically or socially connected with the head of the respective local government. They argue that the seeds for these networks are planted during higher education, as the vast majority of business and political leaders attend the same very limited set of elite schools, the so-called *Grandes Ecoles*. This example demonstrates that even in highly developed countries, politicians may find ways to extend their influence beyond the statutory powers that come with holding office – as long as the institutional setup gives them the opportunity to do so.

In Germany, local politicians have such an opportunity, opened up by an institutional peculiarity in the German savings bank system: Not only is the board of directors of each regional savings bank staffed with a number of politicians from said region. On top of that, the board's chairman is usually the elected official of that respective community. This, taken together with the vital role savings banks play in local lending to private households and small to medium-sized enterprizes (SMEs)[2], provides an alluring opening for municipal politicians to improve their electoral outlook. The main research hypothesis we put to test is that incumbents may try to boost economic conditions – and voter satisfaction – by pushing for more lavish lending policies within the bank's board of directors, shortly before elections. We also expect negative fallouts of this policy intervention, taking the form of higher credit default rates, as some of these loans should probably not have been granted in the first place. Moreover, in order to be sure to not measure any general booms around elections, we expect neutral or overly prudent lending policies immediately after elections. Our identification strategy relies on the fact that we should only observe politically motivated lending around election years, only in municipalities in which elections are held at this point in time, and – importantly – only for politically connected savings banks. Econometrically, we conduct difference-in-

[2]See Section 2.2.

difference (DD) as well as triple-difference (DDD) estimation embedded in a fixed-effects panel data setup.

To test our hypotheses we use a novel, largely hand-collected dataset that combines detailed information on German municipal elections, macroeconomic data on the district level, and balance-sheet information on bank lending. We find that savings banks systematically extend more credits in pre-election periods. This effect is robust to various specifications. Importantly, the effect is not present with cooperative banks that are very similar to savings banks, except for their lack of political connectedness. Our hypotheses are further strengthened by the fact that lending cycles only occur around municipal but not around state or federal elections. We find weak evidence for overly prudent lending policies after elections, consistent with a binding credit constraint that banks face so that they have to make up for excessive pre-election lending. Our evidence also suggests that the ability to induce political lending cycles depends on the degree of dominance of the incumbent party. Finally, we find significant differences in effect sizes across states, suggesting an interaction with other details of the institutional environment that we intend to further investigate in future research.

Our results complement the finance literature, which has long suspected that the behavior of government-controlled banks is rather different from that of private financial institutions. Caprio Jr. and Martinez Peria (2000), Barth *et al.* (2001), and La Porta *et al.* (2002) find that politicians use public banks to further their own political objectives and that government ownership in banks is associated with increased risk of banking crises, reduced financial development, and subpar economic growth, respectively. In related work, Khwaja and Mian (2005) find that politically connected firms in Pakistan have easier access to credit but that this preferential treatment is only granted by government banks. Once again, however, the evidence so far is limited to case-studies in the developing world. In fact, Dinç (2005) studies in a framework similar to ours, whether the lending behavior of public banks depends on the timing of elections, and finds that – as opposed to emerging markets – no such wrongdoing seems to be occurring in developed economies. One potential reason for the discrepancy between our results and those of Dinç is likely to be found in our decision to concentrate on municipal (instead of general) elections, reflecting that in the German case, political connections are established on the local and not the federal level.

The remainder of this paper is organized as follows: The institutional background that facilitates the creation of connections between local politicians and savings banks is described in section 2 and followed by section 3, in which we specify our research hypotheses and predictions. Section 4 describes the data we use and discusses the problems that arise from having to rely on balance-sheet information. Methodological issues and our identification strategy are presented in section 5. Section 6 contains the empirical results while section 7 is reserved for robustness analysis. Section 8 concludes.

2 Institutional background

In this section we provide the institutional details relevant for understanding the German public banking sector. In doing so, we lay out the case why savings banks are a prime example for politically connected firms, how cooperative banks are an ideal control group and how the German electoral rules allow us to cleanly estimate causal effects of elections on banks' lending behavior.

2.1 Public guarantee obligation

German law installs public guarantee obligation (*Gewährträgerhaftung*) for public institutions. This rule provides that the creditor is going to be reimbursed by the government in case the public institution is not able to live up to its contractual obligations. German savings banks have been founded by the respective municipalities (see below), were considered public institutions, and were covered by a municipal public guarantee obligation.[3] Due to these governmental guarantees, essentially transforming savings-bank debt into Federal German AAA-rated bonds, savings banks had substantially improved access to refinancing in the capital market. Given this important institutional linkage, the municipality holds substantial sway over the savings bank's management, as will be detailed in section 2.2.

2.2 German banking system

Traditionally, the German banking systems relies on three pillars (*Drei-Säulen-Modell*): private banks, savings banks (*Sparkassen*), and cooperative banks (*Genossenschaftsbanken*). Whereas private banks are best described as profit-maximizing firms, savings banks and cooperative banks are legally bound to also pursue welfare enhancing policies, in particular within their region. According to the German Central Bank (*Deutsche Bundesbank*), in

[3] The European Court of Justice deemed this an obstacle to competition in retail banking and savings banks were exempted from public guarantee obligation as of July 19, 2005.

2010 there were roughly 1,100 cooperative banks, 431 savings banks and 218 private banks in Germany.[4] Since savings banks and cooperative banks are the focus of our empirical analysis, we describe these two bank types in more detail.

Savings banks

The first "modern" savings banks in Germany were founded by local governments in the late 18th century in Northern Germany. Initially, the number of savings banks increased from 300 (in 1836) to more than 3,000 (in 1913). Gradually, this number was reduced when for efficiency reasons neighboring local institutions merged. Today there exist 431 savings banks, i.e., roughly in every municipality (electoral district) there is one savings bank.

The German savings-bank sector has a three-level structure: On the local level there are the individual savings banks. On the state level there are associations (*Sparkassen- und Giroverbände*) to realize economies of scale for operative tasks. On the federal level, a further association (*Deutscher Sparkassen- und Giroverband (DSGV)*) is primarily responsible for representing the interests of savings banks towards the federal government and international institutions. All relevant decisions regarding an individuals savings bank's business policy are autonomously made on the local level. Due to their local structure, and imposed by law, the savings banks' operational areas have a strong focus on their respective regions (*Regionalprinzip*). Their main clientele are private customers and local businesses and savings banks hold a large share in the retail banking markets. According to the DSGV, in 2009 they held more than 40% of all private deposits and, important for our paper, more than 40% of all credits to SMEs and more than 70% of all credits to handicraft businesses. The latter two are traditionally considered the backbone of the German economy.

Local politicians have strong influence over the banks' decisions, in particular their lending activities. Since savings banks are founded by the local governments and were covered by a public guarantee obligation, municipalities have the formal right to send representatives into the board of directors (*Sparkassenverwaltungsrat*) and the central credit committee (*Kreditausschuss*) of the local savings bank. As a result, their members are to a large degree composed of local parliament members, roughly reflecting the relative political powers in the electoral district. On top of that, the

[4]Currently, our sample covers four states with a total of 268 savings banks and 722 cooperative banks.

chairmen of both chambers is, as a rule, the executive representative of the respective district. By law, the directors are not bound by an imperative mandate but are supposed to only consider the greater good of the savings bank. Besides general guideline competence, board members also hold substantial sway over credit decisions that exceed the authority of the savings bank's management, as the board of directors or the central credit committee have to vote on those credits (that are either large in size or considered rather risky).

Cooperative banks

The first cooperative banks in Germany were founded by Franz Hermann Schulze-Delitzsch und Friedrich Wilhelm Raiffeisen in the middle of the 19th century. They are organized as cooperatives, i.e. every customer is also a "member". They are locally organized, with basically every municipality being the location of at least one cooperative bank and their main clientele are private customers and local businesses. In 2010 they had a market share of 16% for private deposits, 15% of all credits to SMEs and 28% of all credits to self-employed persons.

Most local cooperative banks are organized in a federal association of cooperative banks (*Bundesverband der Deutschen Volksbanken und Raiffeisenbanken*). Cooperative banks are not covered by the public guarantee obligation but their federal association provides an insurance fund to provide deposit guarantees. Since cooperative banks are independent from governmental institutions and are not protected by public guarantees, politicians have no formal way to influence cooperative banks' business policies.

Hence, cooperative banks are an ideal control group for our purposes: They share the regional structure and their clientele with savings banks[5], but they are exempted from the direct control local politicians hold over savings banks' business policies.[6]

[5]Comparing the regulating laws (our translation) describing the purposes of cooperative banks (here for *Volksbanken*) and savings banks (here for Baden-Württemberg) highlights that they share basically the same objectives:
§1(1) Genossenschaftsgesetz: "[...] to foster the income or the enterprise of the members [...]"
§6(1) Sparkassengesetz Baden-Württemberg: "[...] to ensure the provision with money and credit in their region in particular for SMEs [...]"

[6]In contrast to this, private banks differ greatly from savings banks: First, their business model solely focusses on profit-maximization and is unrestricted by welfare considerations. Second, their outreach is usually not confined to a specific region. Third, and most importantly, their regional representation does not consist of independent regional units but of

2.3 German electoral system

Germany has a federal system with three layers of government: the federal state, the 16 states (*Bundesländer*), and 399 municipalities (consisting of 292 counties (*Landkreise*) and 107 urban municipalities (*Kreisfreie Städte*)).[7] Each layer has specific powers and responsibilities as well as separate parliaments and elected executives. These are elected in regular intervals: every 4 years on the federal level, every 4-5 years on the state level and every 4-9 years in the respective municipalities (depending on the state and the form of the municipalities). We focus on the latter class of elections, as it is the *local* ruling politicians that have direct influence on the policies of their local savings bank.

There exist two types of municipal elections: elections of legislative bodies (*Kreistag* in case of counties and *Stadtrat* in case of urban districts) and of executive representatives (*Landrat* in case of counties and *Bürgermeister* in case of urban districts). The latter may occur in direct elections or indirect elections (via the aforementioned legislative bodies).

Elections of legislative bodies

Each municipality has its own parliament. The elections of these legislative bodies are coordinated on the state level, i.e., within a state they all take place on the same election day. These dates, however, generally differ from election dates of federal or state parliaments (*Bundestagswahlen* and *Landtagswahlen*, respectively); i.e., as a rule they are not held on the same day. Furthermore, municipal election dates differ across states with intervals between elections varying between 4 and 6 years. For the states used in our analysis the elections are held every 5 years in Baden-Württemberg, Hesse and Rhineland-Palatinate and every 6 years in Bavaria. The electoral system is one of proportional representation with a minimum vote share requirement.

Elections of executive representatives

Executive representatives, on the other hand, are elected with majority rule and runoffs. There are substantial cross-state differences in electoral laws and various changes across time. Some states have direct elections of executive representatives, others indirect elections via municipal parliaments. In the last decades there has been a strong trend towards more direct repre-

mere branches that are legally part of operational headquarters. For these reasons, private banks are not suitable as a control group for our purposes.

[7] Currently, our sample covers four states with a total of 202 electoral districts.

sentation, whereas up to the early 1990s indirect elections were the norm. States also differ with respect to whether they hold the municipal legislative and executive elections on the same time (minority of states) or on differing days (majority of states). In fact, executive elections are usually not held at the state level but dispersed across time. Finally, there are substantial differences regarding the intervals, from 4 to 9 years, between these elections. The details for the states used in our analysis are as follows.

Baden-Württemberg: Elections are held every 8 years and on separate dates from elections of legislative bodies. In counties the executive representatives are determined in indirect elections via municipal parliaments, whereas urban municipalities choose their mayors in direct elections.

Bavaria: All elections are direct and are held every 6 years. Election dates coincide with those of legislative bodies.

Hesse: Since 1993 all elections are direct (before 1993 all were indirect elections via municipal parliaments) and are held every 6 years. Election dates do not coincide with those of legislative bodies.

Rhineland-Palatinate: Since 1994 all elections are direct (before 1994 all were indirect elections via municipal parliaments) and are held every 6 years. Election dates do not coincide with those of legislative bodies.

3 Main hypothesis and testable predictions

The main hypothesis this paper seeks to test is whether local politicians take advantage of the institutional environment described in section 2 and artificially expand lending in their respective districts in the wake of elections, in hopes of swaying their prospects at the ballot box. The opportunities and incentives for doing so certainly exist. To begin with, there should not be any real doubt about the connectedness between local politicians and savings bank, as their relation is legally manifest in board-of-directors membership or even chairmanship. Moreover, the board of directors does not have mere representational functions that consist of rubber-stamping the decisions made by the bank's management. On the contrary, the bank's large-scale lending activities are one operational area where directors can directly influence decisions and do not have to rely on taking indirect influence.

Our design that uses cooperative banks as control group allows us to distinguish the increased lending from a mere increase in demand for credit in response to real economic growth around election years, caused e.g. by traditional political spending cycles, as the latter would arguably affect co-operative bank lending equivalently.

The question remains whether increased lending will help politicians to tip the electoral scales in their favor. We argue that there is a number of reasons why this may indeed be the case. First, given the legally mandated local focus of savings banks, borrowers will almost certainly live – and vote – in the region represented by the incumbent, and it is save to assume that constituents will be more satisfied when they are not troubled by credit rationing. This argument becomes even more powerful when loans to SMEs are under consideration, as these may be paramount for the creation or preservation of employment in the politician's district. Second, in small municipalities where little goes unnoticed, the politician's role during credit negotiations may well become common knowledge sooner or later. In that case, hearsay about the mayor relentlessly fighting for her constituency will even send a signal to voters that are not directly affected by the approval of loans. Third, the option of affecting economic outcomes through lending may be attractive inasmuch as the potential costs of this intervention (e.g., higher default rates on the marginally granted credits) are deferred until the loans in question mature. As a consequence, the negative fallout is not instantly visible and may in fact never be traced back to the responsible politician.

The final argument in favor of our main hypothesis concerns the timing of bank lending distortions. If politicians truly exploit their ability to sway the credit operations of their local savings bank, we should expect a concentration of such behavior in times when it helps them most. Assuming that voters are myopic, political gain is maximal when the incumbent's fate is on the line: in the wake of elections. Hence, it seems fair to assume that politically motivated lending be focussed on election seasons rather than equally distributed throughout the legislative period.

As we do not want to rely on these theoretical arguments alone, we formulate five predictions we expect to survive empirical testing, if politicians truly behave as suggested above.

Prediction 1: Election effect. Before municipal elections, local savings-bank lending should increase, compared to a hypothetical situation without elections. Importantly, and facilitating identification, there should be no increased pre-election lending for cooperative banks that are similar in local organization and clientele but are not formally politically influenced.

Prediction 2: Election kind. Lending should react to *municipal* elections. Elections on the state or federal level should have no (or at least a much weaker) systematic impact, since politicians from these levels of government are not institutionally connected with local savings banks.

Prediction 3: Pre- and post-election lending. Politically motivated lending increases should exclusively occur in the ultimate wake of elections. Given voter myopia, they should not happen too far in advance as this would represent premature flexing of political muscle. For similar reasons, the election effect should not be permanent, since incentives to allure voters will instantly vanish once the polls are closed. Whether post-election lending should return to its steady-state level or whether the election increase should be compensated by overly prudent post-election lending is not entirely clear, a priori. Which of the two options is more plausible depends on the credit constraints savings banks face. In addition, confirmation of prediction 3 would reduce the risk of confounding *political* motives behind a surge in lending with the effect of any general booms around elections, as the latter would arguably create less abrupt patterns in the extension of credit.

Prediction 4: Electoral competition. Does politically induced lending depend on the contestedness of elections? Predicting the answer to this question on theoretical grounds alone is not trivial, as electoral competition may exert two offsetting effects. The first is a matter of efficiency considerations, according to which election effects should be more pronounced in districts with fierce *current* electoral competition. The assumption underlying this hypothesis is that – given prediction 5 below holds – politically motivated lending will be costly for the savings bank and ultimately harm the politician's reputation. As a consequence, incumbents may not make much use of this distorting instrument unless they genuinely fear for their re-election. By contrast, the second effect of electoral competition may curb the politician's ability to influence savings-bank lending if the political process is *generally* contested and has led to close election results in the past. The rationale for this argument is one of dominance: A competitive electoral environment will be reflected in the partisan composition of the bank's board of directors, reducing the likelihood of collusion among board members who represent rivalling political parties. Similarly, the degree of sway over lending decisions may well depend on informal ties between politicians and the bank's management, which are likely to be stronger, the longer the incumbent has been in office. As a result, regular changes in power and slim majorities would limit the scope of electoral lending cycles.

Prediction 5: Increased default risks. Loans granted during the height of an election season should have a higher tendency to default, since some marginal loans would not have been been approved, if financial fundamentals alone had played a role in determining the credit rating.

Whether our hypothesis is verified by empirical testing, is investigated in section 6. Before turning to this analysis, however, we continue with the description of our data and discuss which of the above predictions are ultimately testable with the information at hand.

4 Data

We use a novel, in large parts hand-collected, dataset that combines information from multiple sources. The observational units are savings and cooperative banks in Southern Germany. This bank data is merged with information on municipal, state-level and federal elections as well as with macro-economic and demographic data on the district level.

4.1 Bank data

The source of our bank data is *Hoppenstedt*, a business data provider that hosts the largest commercial database for balance sheets and annual reports in Germany. The main advantage of *Hoppenstedt*, compared to other commercial databases such as *Bankscope*, are the ample N and T dimensions their sample provides: It covers virtually all savings banks and a very large fraction of cooperative banks that operated in Germany between 1987 and 2009.[8] For the four Southern German states we have initially picked for our analysis (see section 4.2), this amounts to a total of 268 savings banks (4,568 bank-year observations) and 722 cooperative banks (6,181 bank-year observations). Note that these numbers include a sizeable amount of banks that exited or entered the sample due to bank mergers. The average time, savings banks remain in the sample is 17 years, whereas the average cooperative bank is only observable for roughly 9 consecutive years. This reflects that our panel is considerably less balanced for cooperative banks, as a large fraction is only covered by the sample since the early 2000s. To ensure that our results are not driven by these sample characteristics, we

[8]We ran several internal consistency checks to ensure that the *Hoppenstedt* data be of comparable quality to that of *Bankscope*.

perform robustness checks by varying the degree of panel balancedness in section 7.2.

All information is taken from official balance sheets. The key variables are the bank's overall lending position, the amount of non-performing loans, total assets and the capital ratio. All monetary positions are deflated and measured in 1995 EUR. A look at the panel characteristics reveals that for all items between-variation is substantially greater than within-variation.

Note that the use of balance-sheet data has the disadvantage of rendering prediction 5 untestable within the scope of this paper, as we are unable to determine the time period in which non-performing loans were granted. If marginal credits from pre-election periods were truly more default-prone, this could still take its toll 5, 10, or 15 years after the loan was granted, making it empirically impossible to causally tie the eventual default to the election in question.[9]

4.2 Election data

A database that combines information on German municipal elections in any comprehensive way does not exist. Even on the state-level, the collection of local electoral data is the clear exception. For this reason, we have begun to create our own unique dataset by gathering all relevant information ourselves. To this end, we have contacted regional statistical offices, the respective communities, and historical archives all over Germany. Given the enormous labor intensity of this project, data collection is still ongoing. So far, we were able to complete our search for the following Southern German states: Baden-Württemberg, Bavaria, Hesse, and Rhineland-Palatinate. By focussing on neighboring states, we are confident that regional differences be kept to a minimum, improving the comparability of cross-state observations. Together, these four states account for more than half of all German counties and urban districts (202 out of 399). As data collection progresses, we will gradually extend our sample until all of Germany is covered.

The Southern German municipal election data already gathered, covers the years between 1970 and 2009. Yet, since this political data is merged with the aforementioned bank data, the maximum interval for our anal-

[9]To test prediction 5, we additionally applied for access to officially kept loan-level bank data at the Federal Savings Banks Association and at the German *Bundesbank* that would have provided information on the exact timing and the eventual (non-)performance of credit contracts. While these data have been made available for academic research in the past, neither institution has granted us access, once we described our research project.

ysis is effectively reduced to 1987–2009 as well. During this time span, Bavaria held 4, and the other states 5 elections of legislative bodies. The pattern for executive elections is a lot more irregular: Districts in Bavaria and Hesse experienced an average of 4 elections, Rhineland-Palatinate and Baden-Württemberg's urban municipalities just 2 to 3, and finally counties in Baden-Württemberg – qua institution – none.

Our dataset contains information on election dates, election results (measured in vote shares), names and party affiliations of incumbents and winners, election types (direct vs. indirect), whether runoffs were necessary and whether there was a change in executive power. Overall, data quality is better for legislative elections than for executive elections if the latter were held in indirect fashion through municipal parliaments, since for these votes information is unavailable. In fact, we do not even know *when* such indirect elections of regional executives occur. We only observe if the identity of the person in power changes. Whether this is due to electoral defeat, retirement, or death, however, is unknown. For this reason, our main analysis focusses on *legislative* elections and their effect on savings bank lending. While there are good reasons to believe that this election type is in fact the more relevant one for the mechanism under study, this is ultimately an empirical question that we defer to section 7.1.

To enable empirical testing of prediction 3, we have also added dates and outcomes of state and federal elections.

4.3 District data

Finally, to warrant better control for confounding factors and to increase statistical precision, we augment our sample with macro-economic and demographic information at the district level, which are available at the German Federal Statistic Office (*Statistisches Bundesamt Deutschland*). These include population size, GDP, unemployment, public spending and expenditure, public debt, as well as firm creation, closures and bankruptcies. Once again, all monetary values are converted to 1995 EUR. Available time spans vary significantly among these variables so that the addition of certain control variables would result in significant loss of sample size. The longest time series are available for GDP, population size and unemployment, spanning from the early 1990s to 2009. The collection of the other variables by the Statistic Office sets in considerably later. As a result, the effective time-span covered by our preferred econometric specification presented in section 5 covers the years 1993 to 2009, whereas longer time spans are analyzed for robustness in section 7.

Table 1. *Variables used for analysis*
Summary statistics

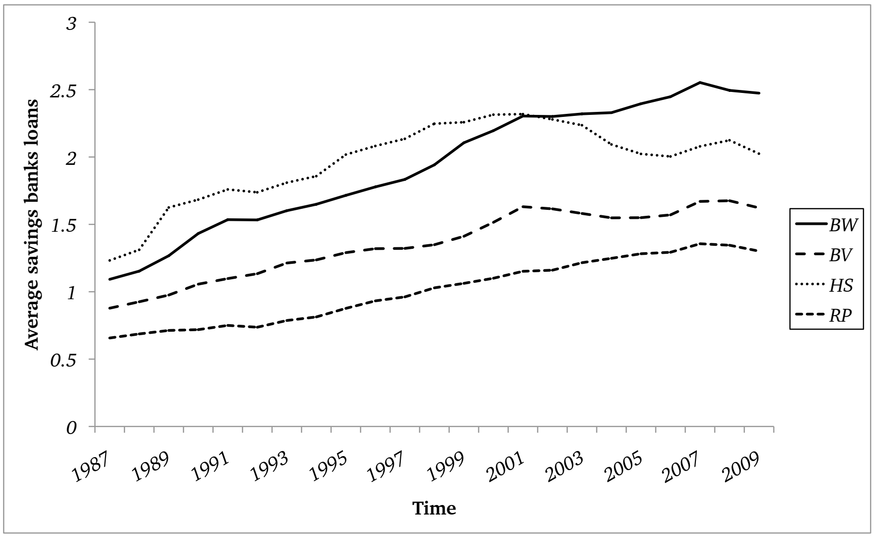
Variables	Total	BW	BV	HS	RP
	N=10,738	N=3,624	N=4,229	N=1,639	N=1,246
			Panel A: Banks		
Savings banks					
- No. of banks	268	73	103	52	40
- Total assets	1.936 (1.950)	2.295 (1.920)	1.658 (1.579)	2.485 (3.095)	1.464 (0.790)
- Loans	1.346 (1.357)	1.588 (1.332)	1.148 (1.078)	1.749 (2.176)	1.023 (0.599)
- Capital ratio	0.046 (0.010)	0.043 (0.008)	0.048 (0.012)	0.044 (0.009)	0.046 (0.010)
Cooperative banks					
- No. of banks	722	241	316	95	70
- Total assets	0.651 (1.581)	0.655 (0.687)	0.623 (2.408)	0.758 (0.881)	0.575 (0.641)
- Loans	0.488 (1.230)	0.484 (0.467)	0.468 (1.892)	0.565 (0.665)	0.451 (0.556)
- Capital ratio	0.057 (0.017)	0.056 (0.012)	0.058 (0.017)	0.058 (0.028)	0.056 (0.014)
			Panel B: Municipal elections		
No. of elections	19	5	4	5	5
Vote share *CDU/CSU*	38.97 (7.39)	36.83 (7.07)	42.45 (6.00)	35.35 (7.02)	40.64 (7.75)
Vote share *SPD*	26.33 (9.80)	21.06 (5.09)	23.42 (8.27)	38.55 (7.76)	34.46 (8.18)
Vote share swing	9.30 (2.56)	8.21 (2.30)	8.99 (2.42)	10.25 (1.67)	12.19 (1.85)
Party change	0.105 (0.160)	0.059 (0.115)	0.065 (0.158)	0.245 (0.153)	0.171 (0.172)
			Panel C: Municipal districts		
No. of districts	199	44	94	26	35
Population	33,364	10.754	12.539	6.067	4.004
Real GDP	6.644 (8.646)	8.339 (5.134)	5.731 (11.369)	7.901 (8.305)	3.226 (2.089)
Unemployment rate	7.01 (2.56)	6.11 (1.84)	6.81 (2.58)	8.35 (2.74)	8.48 (2.72)

Notes: Reported are total numbers (for the state level) and means (for the district level) respectively. For the latter, standard deviations are in brackets. BW, BV, HS, and RP denote the states of Baden-Württemberg, Bavaria, Hesse, and Rhineland-Palatinate, respectively. N stands for the number of available bank-year observations. Election data refers to municipal elections of legislative bodies. *CDU/CSU* are the conservative parties and *SPD* the social-democratic party of Germany. "Vote share swing" denotes the average swing in vote shares (cumulated over all parties) that results from a given election. "Party change" indicates the share of elections that result in a change of the winning party. State population is measured in million habitants (as of 2010). All monetary values are measured in 1995 EUR billion.

4.4 Descriptive statistics

Summary statistics of variables used in our analysis are presented in table 1. Overall, our data is substantially right-skewed, which is why our preferred empirical specification presented below makes use of log-transformed data. As is evident from panel A, savings banks are on average larger than their

Figure 1. *Time trends in bank lending 1*
Savings bank lending across states



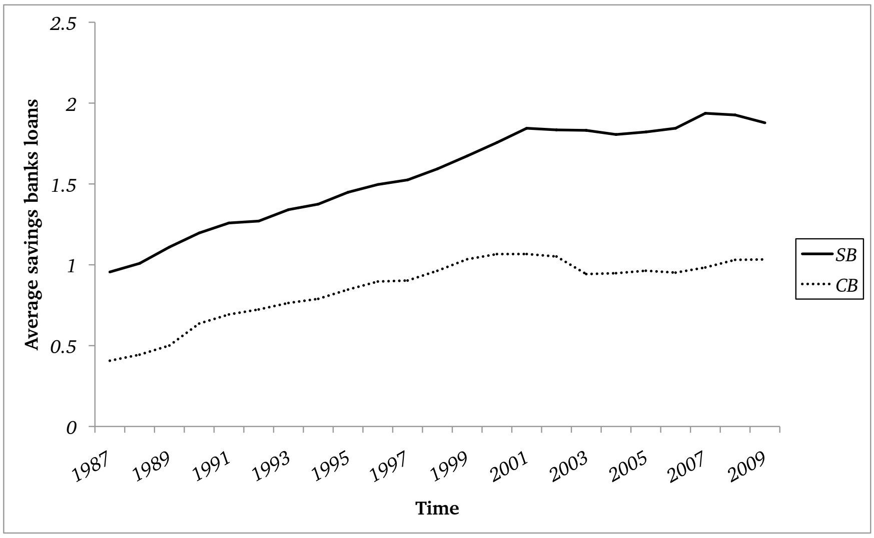
Notes: Depicted are time series from a balanced panel of average savings bank lending for Baden-Württemberg (BW), Bavaria (BV), Hesse (HS) and Rhineland-Palatinate (RP). Loans are measured in 1995 EUR billion.

cooperative counterparts, which reflects the wave of mergers that took place among German savings banks throughout the years. Judging from the ratio of loans and total assets, both bank types clearly set their business focus on lending operations: The average loan position of savings banks makes up 70% of the entire balance sheet, while that number is even slightly higher for cooperative banks, which devote 75% of their operations to providing credit. Furthermore, the capital ratio seems to be mildly, but systematically, larger for cooperative banks.

A look at panel B reveals that municipalities in Baden-Württemberg and Bavaria are clearly dominated by conservative parties – Bavaria's *Christlich-Soziale Union (CSU)* and its sister party, *Christlich Demokratische Union (CDU)*, which competes in the rest of Germany – whereas the other two states see a closer gap between the main political rivals: For one, Germany's largest left-of-center party, *Sozialdemokratische Partei Deutschlands (SPD)*, generally fares very poorly in the two former states. In addition, incumbent dominance appears to be much stronger, suggesting a rather static political environment. As an illustration, consider that only about 6% of all municipal elections in Bavaria and Baden-Württemberg result in a change of the winning party, whereas Hesse and Rhineland-Palatinate experience such changes in power after 25% and 17% of all elections, respectively.

Note that these summary statistics are for pooled data and represent an average over time. To better assess the dynamics of German bank lending,

Figure 2. *Time trends in bank lending 2*
Savings bank versus cooperative bank lending



Notes: Depicted are time series from a balanced panel of savings bank (SB) and cooperative bank (CB) lending, averaged over all four states in our sample. Loans are measured in 1995 EUR billion.

figure 1 plots the time series of average savings bank lending, stratified by state. Clearly, our loan data is subject to an upward trend. For this reason, section 5 proposes two alternative approaches to account for the effect of time: a year dummies specification and quadratic time trends. Overall, savings banks in all four states appear to be on similar time trends which provides good news for a DD identification strategy such as ours (see Angrist and Pischke, 2009). If anything, Hesse's trend becomes a bit idiosyncratic after the turn of the century, which is why results that seem exclusively driven by Hesse may be taken with a grain of salt. Finally, figure 2 shows that time trends are also comparable for both bank types (averaged over all states in our sample), which provides further evidence that cooperative banks are indeed a valid control group for savings banks.

5 Methodology

Our strategy to identify any causal effect of elections on savings-bank lending, relies on the fact that we should only observe politically motivated lending before election years, only in municipalities in which elections are held at this point in time, and – importantly – only for politically connected savings banks. Identification is facilitated by the different timing of elections across states and the existence of a control group of cooperative banks that operate in the same electoral districts as savings banks. Furthermore, given the statutory nature of legislative elections at the municipal level, for which early elections are largely non-existent, we certainly need not worry about any endogeneity in (the timing) of our key regressor. Econometrically, we conduct difference-in-difference (DD) as well as triple-difference (DDD) estimation embedded in a fixed-effects panel data setup.

Testing prediction 1: Election effect

A natural starting point for testing prediction 1 that savings-bank lending increases in the wake of elections, is the following empirical specification:

$$Y_{ist} = \mathbf{X}'_{\mathbf{ist}}\boldsymbol{\beta}_{\mathbf{1}} + \mathbf{S}'_{\mathbf{s}}\gamma_{\mathbf{1}} + \mathbf{T}'_{\mathbf{t}}\boldsymbol{\lambda}_{\mathbf{1}} + \delta_1 ELEC^M_{st} + \epsilon_{ist}, \quad (1)$$

where Y_{ist} is a measure for loans from savings bank i, operating in state s at time t. The parameter of interest, δ_1, estimates the causal effect of municipal election seasons, which are indicated by the dummy variable $ELEC^M_{st}$.[10] To ensure identification of δ_1, we control for the following covariates and fixed effects: $\mathbf{S_s}$ donates a full vector of state effects to control for secular lending differences across states. Similarly, time effects, $\mathbf{T_t}$, are included to capture any national trends or year shocks. Note that we use two alternative specifications to capture the effect of time: The first consists of introducing a full set of year dummies, whereas the second – with the purpose of reducing the number of parameters to be estimated – controls for quadratic time trends instead. Finally, $\mathbf{X_{ist}}$ is a vector of bank- and district-specific variables that may directly influence the outcome variable. The inclusion of these covariates should considerably improve the predictability of Y_{ist}, which will in turn reduce the sample variance of our estimates.

Estimation of model 1 by OLS ensures that both T- and N-variation are exploited. The former compares the same banks across time, as each bank will be subject to recurring election "treatments". The latter contrasts different banks at a given time, as municipal elections dates vary across states. Yet, while a positive estimate of δ_1 would speak in favor of prediction 1 and suggest that savings banks are indeed systematically easing credit before elections, we cannot yet say anything about the reasons for this behavior. For instance, it could well be that banks simply react to increased demand for credit if – for some reason – local GDP were positively correlated with the timing of elections. Even though we are able to shield ourselves from

[10] Note that the way model 1 is written down, corresponds to the case of legislative elections, $ELEC^M_{st}$, as these vary at the state level. For election of executive representatives (dealt with in chapter 7.1), $ELEC^M_{ist}$ would be permitted to also vary at the district (and, hence, individual) level.

such confounds by adding macro-economic district variables to the list of controls $\mathbf{X_{ist}}$, a fundamental problem remains: So far, we have no way of gauging whether the responsibility for the credit boost lies with local politicians and their involvement with the savings banks' board of directors. The latter hypothesis, however, would be substantially strengthened if the detected pattern were to apply to savings banks alone.

The control group of cooperative banks allows us to identify this: If our hypothesis is true, we should not observe increased cooperative-bank lending before elections, as politicians have no institutional sway over their credit policies.[11] Therefore, a more convincing way of testing prediction 1 consists of applying the following DD model to a sample that includes both types of banks:

$$Y_{isbt} = \mathbf{X}'_{\mathbf{isbt}}\boldsymbol{\beta}_\mathbf{2} + \mathbf{S}'_\mathbf{s}\boldsymbol{\gamma}_\mathbf{2} + \mathbf{T}'_\mathbf{t}\boldsymbol{\lambda}_\mathbf{2} + \mu_2 B_b + \theta_2 ELEC^M_{st} + \delta_2 ELEC^M_{st} * B_b + \epsilon_{isbt}. \quad (2)$$

Model 2, which is also estimated by OLS, differs from specification 1 inasmuch as bank-type effects, B_b, are needed to control for perpetual differences between savings and cooperative banks. B_b is defined as a dummy variable that takes on the value of 1 if the individual unit is a savings bank. In addition, we interact the election dummy with the bank-type indicator such that $ELEC^M_{st} * B_b$ switches on if and only if Y_{isbt} measures lending activity of a savings bank during election season.

The parameter of interest is denoted δ_2 and arguably provides improved identification over δ_1, since the representation of counterfactual lending in the absence of election is more encompassing. The estimate from model 1 measures the difference in lending behavior between savings banks in election periods and savings banks in non-election periods. The DD estimate from model 2, on the other hand, captures the difference between election-induced increase in savings-bank lending (which is expected to be positive after controlling for time trends) and election-induced increase in cooperative-bank lending (which is expected to be zero after controlling for time trends).

Testing prediction 2: Election kind

Our main hypothesis would be further strengthened if prediction 2 – that only municipal elections have a systematic impact on savings-bank lending – were to survive empirical testing as well. Recall that it is local politicians

[11] This, of course, can – and will – be individually tested by applying specification 1 to the sample of cooperative banks. This time, we would expect δ_1 to *not* be significantly different from zero for our prediction to hold.

who are granted membership in the bank's board of directors. While a few exceptions from this rule (with members of state parliaments being granted access as well) certainly exist, any potential effect should at least be considerably weaker than that of municipal elections.

Empirical testing of prediction 2 is straightforward if state elections are concerned, as model 2 can be applied almost verbatim since both, legislative municipal elections and state elections (which are always for the state *legislature*) vary at the state level. The only difference to the specification used for prediction 1 is that $ELEC^M_{st}$ is replaced with an indicator for state election seasons, $ELEC^S_{st}$. The case of federal elections (which are also parliamentary), however, requires a slightly altered specification, as there is no more cross-state variation in treatment:

$$Y_{isbt} = \mathbf{X}'_{\mathbf{isbt}}\boldsymbol{\beta}_\mathbf{3} + \mathbf{S}'_\mathbf{s}\gamma_\mathbf{3} + \mathbf{T}'_\mathbf{t}\boldsymbol{\lambda}_\mathbf{3} + \mu_3 B_b + \theta_3 ELEC^F_t + \delta_3 ELEC^F_t * B_b + \epsilon_{isbt}. \quad (3)$$

Note that $ELEC^F_t$, which indicates federal election periods, only varies in the time dimension, taking the value of 1 every 4 years. As a consequence, the effect of federal elections will not be identified in case year dummies are used to control for time effects. For this reason, $\mathbf{T_t}$ will have to be represented by quadratic time trends when specification 3 is used.

Testing prediction 3: Pre- and post-election lending

Another way of solidifying support for our hypothesis is to look at pre- and post-election periods, as the increase in lending should be confined to the immediate election season. Particularly, we expect lending policies to immediately return to their steady-state level once ballots are cast. In case higher default rates on the marginally granted credits need to be compensated, we may even expect overly prudent lending behavior in the aftermath of elections. Prediction 3 can be tested with the following specification to be estimated with OLS:

$$Y_{isbt} = \mathbf{X}'_{\mathbf{isbt}}\boldsymbol{\beta}_\mathbf{4} + \mathbf{S}'_\mathbf{s}\gamma_\mathbf{4} + \mathbf{T}'_\mathbf{t}\boldsymbol{\lambda}_\mathbf{4} + \mu_4 B_b + \theta_4 ELEC^M_{st-\tau} + \delta_4 ELEC^M_{st-\tau} * B_b + \epsilon_{isbt}, \quad (4)$$

and $\tau = (1,2,3,4)$.[12] To study post-election periods, the dummy variable $ELEC^M_{st-\tau}$ indicates whether there was an election in state s, τ years ago. We expect the estimate of δ_4 to be either close to zero or, in case of binding credit constraints, negative. For the analysis of pre-election periods, $ELEC^M_{st-\tau}$ will be replaced with a corresponding $ELEC^M_{st+\tau}$ indicator variable.

[12]Higher-order lags exceeding 4 years should not be used, since this would blur the line between post-election periods of the past and pre-election periods of the next campaign.

Once again, we would interpret a zero estimate of δ_4 as supportive evidence for prediction 3.

Testing prediction 4: Electoral competition

The test for prediction 4 can be implemented with the following DDD model, estimated with OLS:

$$\begin{aligned} Y_{isbt} = & \mathbf{X}'_{\mathbf{isbt}}\boldsymbol{\beta}_{\mathbf{5}} + \mathbf{S}'_{\mathbf{s}}\boldsymbol{\gamma}_{\mathbf{5}} + \mathbf{T}'_{\mathbf{t}}\boldsymbol{\lambda}_{\mathbf{5}} + \mu_5 B_b + \psi_5 I_{it} + \theta_5 ELEC^M_{st} + \dots \\ & + \phi^1_5 B_b * I_{it} + \phi^2_5 B_b * ELEC^M_{st} + \phi^3_5 I_{it} * ELEC^M_{st} + \dots \\ & + \delta_5 ELEC^M_{st} * B_b * I_{it} + \epsilon_{isbt}, \end{aligned} \tag{5}$$

where I_{it} is the respective indicator variable of interest: In case *current* electoral competition is investigated, $I_{it} = C_{it}$ is an indicator for whether the present election is contested. The ruling party's dominance (or alternatively: the lack of electoral competition *in general*) is measured with $I_{it} = D_{it}$.[13] In line with our predictions in section 3, the former indicator switches on if the election is competed, while the latter takes the value of one in case the political process is *not* contested. The first line of model 5 contains the usual controls as well as all main fixed effects. Line 2 contains the full set of first-order interactions which are necessary to identify the causal effect of interest, captured by the DDD estimate of δ_5 in line 3 (see Gruber, 1994).

Testing prediction 5: Increased default rates

As previewed in section 4.1, prediction 5 is untestable with the balance-sheet data at hand, as it shows the entire *stock* of non-performing loans at year's end that also reflects lending activities outside of election seasons. Since we do not know when a given non-performing loan was granted, we will, for the scope of this paper, have to leave the question about the welfare cost of politically induced lending unanswered and adjourn this analysis to future research, in hope that access to loan-level data will eventually be granted.

[13]Note that we use several alternative measures for electoral contestedness and party dominance (see section 6). While in general D_{it} may change its value depending on the competitiveness of the election under consideration, D_i^G is defined to be time-constant in case we want to capture the *general* electoral competition of the district, bank i operates in.

6 Results

All results presented here are estimates from an unbalanced panel to which we apply our preferred empirical specification with the following properties: The dependent variable, Y_{it}, is defined as the natural logarithm of the loan sum of bank i as reported in the balance sheet for year t. This facilitates interpretation of coefficients – which represent (semi-)elasticities – and accounts for the right-skewedness of our data. The pre-election indicator, $ELEC_{st}$, is defined as follows: It takes on the value of 1 if there is an election in either the final two quarters of the same year, or the first two quarters of the following year.[14] The vector of control variables, $\mathbf{X_{ist}}$, includes bank-specific (total assets and capital ratio, to account for bank size and degree of capitalization, respectively) and district-specific (population size and real GDP) covariates. To account for the possibility that the bank variables are only sequentially exogenous, we use their lagged values instead (see Dinç, 2005). All elements of $\mathbf{X_{ist}}$ are log-transformed. Finally, standard errors are clustered on the bank level (as opposed to the bank-year level), to correct for substantial serial correlation. Note that our results are not driven by these modeling choices. As section 7 demonstrates, the main conclusions are insensitive to varying definitions of key variables, sets of controls, sample compositions, and assumptions regarding the error-term structure.

In a nutshell, all of our testable predictions withstand empirical scrutiny, which corroborates our hypothesis that local politicians exploit their membership in savings banks' directing boards to sway their electoral fortunes. Not only do all estimated effects have the correct sign, they are also statistically significant at the 5% level and, in the majority of cases, even at the 1% level. Effect sizes are estimated to be in the range of 1%–3% election-induced increase in the stock of lending. Note that this increase is relative to the *total* stock in bank lending. If we were to model the extension of *new* credit contracts alone, relative effect sizes would certainly be larger. To provide a better sense for the actual magnitude of the effect, consider

[14]This definition ensures that election-induced lending is reflected in the balance sheet of the actually relevant year: If an election takes place in, say, January, pre-election lending will arguably leave its mark in the balance sheet of the previous year, which is why the latter will switch on $ELEC_{st}$, whereas $ELEC_{st} = 0$ for the actual election year. By contrast, if the election is held around year's end, the balance sheet of the preceding year is probably less informative than that of the election year, for which reason the pre-election indicator would then coincide with the year of the election. Note that results, not reported here, based on alternative definitions of $ELEC_{st}$, are comparable to those presented in this paper.

that its absolute size amounts to an average of EUR 30.6 million extra stock in lending per bank, when our preferred empirical specification is used.

Prediction 1: Do savings banks expand lending prior to elections?

The empirical tests of prediction 1 are summarized in table 2, which contains OLS estimates of the key parameters from models 1 and 2, as well as regression coefficients of control variables. The first four columns display results from variants of model 1 being applied to savings banks. Estimates of the key parameter, δ_1, can be found in the first row. Results in column (A) suggest that savings banks increase lending by about 1.6% in the wake of municipal elections. This result is largely unchanged by the inclusion of state fixed effects in column (B). As the comparison of results in columns (C) and (D) shows, estimates are somewhat sensitive to the way, time effects are accounted for: If quadratic time trends are used, effect sizes are roughly twice as large as those coming from a year-dummy specification.

While these results are certainly encouraging, our hypothesis would be greatly invigorated if the election effect were exclusively present for savings banks. This enhanced prediction is under study in columns (E) through (H) of table 2. The first two of those repeat the analysis of columns (C) and (D) for the subsample of cooperative banks. As predicted, there is no evidence for these politically unconnected banks adjusting their lending in response to elections. The last two columns contain the DD results of model 2 being applied to a sample that includes both bank types. Apparently, the presence of a control group strengthens results considerably, suggesting that savings banks' lending increases by 2% to 3% in the wake of elections. These δ_2 estimates are statistically significant at the 1% level. Moreover, the sensitivity of results to the way time effects are incorporated is no longer present, as both controlling for time trends and including year dummies, yield almost identical results.

Since we believe that DD model 2 provides improved identification of the causal election effect over model 1, the remainder of this section describes results that only make use of the former specification. Yet, since the superiority of the DD model critically depends on the assumption that cooperative banks are indeed an appropriate control group, for robustness we also tested predictions 2, through 4 with model 1 that only uses differences between savings banks. As results in appendix A demonstrate, our main conclusions are largely insensitive to which of the two specifications we use.

Table 2. *Results for prediction 1*
Dependent variable: Log loans

Explanatory variables	**OLS regression coefficients** (Empirical p-values in brackets)							
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)
Key regressors								
- $ELEC_{st}^{M}$	**0.016** **(0.001)**	**0.017** **(0.000)**	**0.010** **(0.032)**	**0.018** **(0.000)**	-0.001 (0.795)	0.006 (0.310)	-0.008 (0.137)	0.001 (0.913)
- $ELEC_{st}^{M} * B_b$	–	–	–	–	–	–	**0.027** **(0.001)**	**0.024** **(0.004)**
Bank controls								
- Total assets	**0.988** **(0.000)**	**0.988** **(0.000)**	**0.999** **(0.000)**	**0.991** **(0.000)**	**0.959** **(0.000)**	**0.959** **(0.000)**	**0.977** **(0.000)**	**0.976** **(0.000)**
- Capital ratio	-0.015 (0.599)	-0.024 (0.389)	0.011 (0.758)	0.013 (0.719)	0.020 (0.512)	0.023 (0.466)	0.037 (0.102)	0.039 (0.083)
District controls								
- Population	-0.003 (0.890)	0.005 (0.825)	0.004 (0.865)	0.004 (0.858)	-0.028 (0.504)	-0.031 (0.452)	-0.016 (0.548)	-0.018 (0.502)
- Real GDP	0.025 (0.195)	0.025 (0.203)	0.024 (0.222)	0.024 (0.219)	0.053 (0.145)	0.056 (0.122)	0.043 (0.080)	0.045 (0.067)
State FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE			Yes		Yes		Yes	
Time trends				Yes		Yes		Yes
Bank type FE							Yes	Yes
Banks in sample	SavB	SavB	SavB	SavB	CoopB	CoopB	All	All
N	3,083	3,083	3,083	3,083	4,033	4,033	7,116	7,116
R^2	0.969	0.969	0.970	0.969	0.966	0.966	0.978	0.978

Notes: Results are for our preferred empirical specification (see text). Key regressors are $ELEC_{st}^{M}$ for columns (A) to (F), and $ELEC_{st}^{M} * B_b$ for columns (G) and (H), respectively. The index M denotes a municipal election. Boldfaced numbers indicate statistical significance at the 5% level.

Besides these causal effects of interest, there is only one additional covariate with a statistically significant impact on lending: a bank's (lagged) total assets. In fact, the relationship between these two variables proves to be extremely strong. Not only are estimated effects very sizeable, the inclusion of total assets to the set of regressors leaves barely any variation in the data unexplained, as is evidenced by R^2 exceeding 0.95. Note that this is not an indication for overfitting. Much rather, this tight connection is not surprising since German financial regulation *mandates* that a bank's lending position be backed by equivalent net equity. Given this quasi-mechanical relationship between these variables, we repeat our analysis for robustness without total assets in section 7.1.

Prediction 2: Does lending react to other elections?

To further back up our theory of *local* politicians being the driving force behind electoral cycles in bank lending, we now turn to the second prediction that credit policy should react only to municipal elections. A look at table 3 suggests that this seems to indeed be the case. This table con-

Table 3. *Results for prediction 2*
Dependent variable: Log loans

Explanatory variables	**OLS regression coefficients** (Empirical p-values in brackets)			
	(A)	(B)	(C)	(D)
Key regressors				
- $ELEC^{M}_{st}$	–	–	–	0.002 (0.836)
- $ELEC^{M}_{st} * B_b$	–	–	–	**0.027** **(0.006)**
- $ELEC^{S}_{st}$	-0.005 (0.358)	0.002 (0.643)	–	0.004 (0.491)
- $ELEC^{S}_{st} * B_b$	0.007 (0.245)	0.003 (0.670)	–	0.011 (0.142)
- $ELEC^{F}_{t}$	–	–	-0.005 (0.344)	-0.006 (0.285)
- $ELEC^{F}_{t} * B_b$	–	–	-0.005 (0.357)	-0.004 (0.486)
Bank controls	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Time FE	Yes			
Time trends		Yes	Yes	Yes
Bank type FE	Yes	Yes	Yes	Yes
Banks in sample	All	All	All	All
N	7,116	7,116	7,116	7,116
R^2	0.977	0.977	0.977	0.977

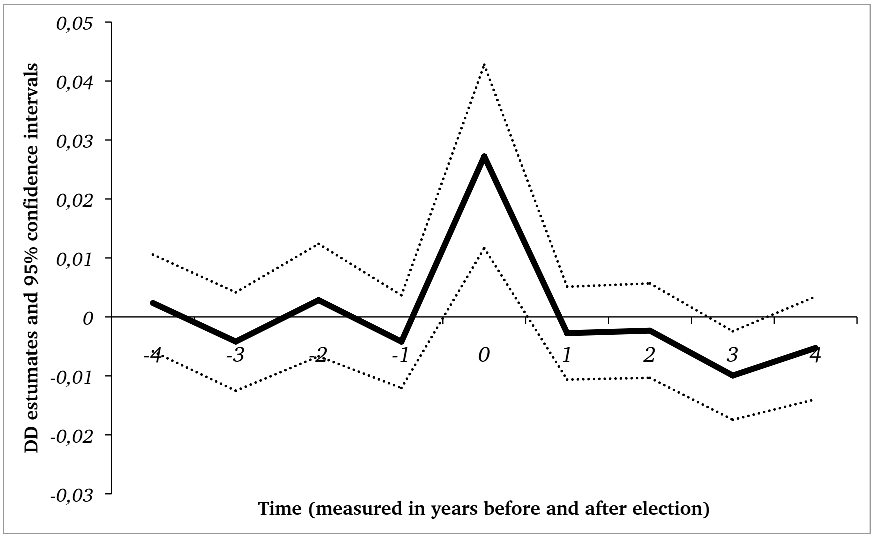
Notes: Results are for our preferred empirical specification (see text). Key regressors are $ELEC^{M}_{st} * B_b$, $ELEC^{S}_{st} * B_b$, and $ELEC^{F}_{t} * B_b$, respectively. The indexes M, S, and F denote municipal, state, and federal elections, respectively. Boldfaced numbers indicate statistical significance at the 5% level.

tains estimates for δ_2 and δ_3 when DD models 2 and 3, respectively, are applied to our full sample. In line with our premise, we find no evidence that lending reacts in any systematic way to either state (see columns (A) and (B)) or general (see column (C)) elections. These findings are corroborated by an additional specification which jointly regresses on all three election types. As results in column (D) show, savings bank lending is in fact only responsive to elections at the municipal level, whereas elections at higher government levels have practically no influence.

Prediction 3: What happens to lending before and after election seasons?

Prediction 3 suggests that the increase in lending should be limited to the immediate election season. Particularly, the positive effect should instantly disappear, or even become negative, once the election was held. We have tested this hypothesis by estimating the effect of municipal elections on savings-bank lending in the four years preceding and following a munici-

Figure 3. *Results for prediction 3*
Visualization of the lending cycle



Notes: Results are for our preferred empirical specification (see text). The solid line depicts DD estimates of δ_4 coming from model 4 with year fixed effects. Dottet lines indicate the corresponding 95% confidence intervals. Time is measured on the abscissa: A value of zero denotes an election season. Negative and positive values stand for years before and after an election, respectively.

pal election. The results are depicted in figure 3, which provides a visual representation of the electoral lending cycle. The solid line represents DD estimates of the effect elections have on lending in the eight years surrounding said election. The dotted lines indicate 95% confidence intervals. Note that these δ_4 estimates come from 9 distinct regressions, for which τ is accordingly varied in model 4.[15]

As is evident from the graph, lending stays relatively flat before election season, only to spike upwards in the immediate wake of an election year. This increase is statistically significant at the 1% level (and identical to the estimate in column (G) of table 2). As expected, the effect quickly dissipates after the election, returning to its steady state level or even slightly below. Three years after the election, the election effect dips into statistically significant negative territory (at the 5% level). We take this as weak evidence for overly prudent lending policies after elections, consistent with a binding credit constraint that banks face as they have to make up for excessive pre-election lending. Importantly, the spike in lending appears to be too abrupt and short-lived to reflect an increase in demand for credit in re-

[15]The empirial specification used for this graph controls for time via year fixed effects. Notably, the depicted pattern also holds if DD model 4 is combined with quadratic time trends instead.

Table 4. *Results for prediction 4*
Dependent variable: Log loans

Explanatory variables	**OLS regression coefficients** (Empirical p-values in brackets)				
	(A)	(B)	(C)	(D)	(E)
DDD Interaction					
- $ELEC^M_{st} * B_b * D^G_i$	0.026 (0.082)	–	–	**0.026** **(0.049)**	–
- $ELEC^M_{st} * B_b * D_{it-1}$	–	0.028 (0.088)	–	–	**0.035** **(0.048)**
- $ELEC^M_{st} * B_b * C_{it}$	–	–	0.007 (0.583)	0.016 (0.234)	0.015 (0.318)
DD Main effect					
- $ELEC^M_{st} * B_b$	0.013 (0.385)	**0.023** **(0.010)**	**0.024** **(0.004)**	0.003 (0.831)	0.015 (0.190)
Bank controls	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Bank type FE	Yes	Yes	Yes	Yes	Yes
First-order interactions	Yes	Yes	Yes	Yes	Yes
Banks in sample	All	All	All	All	All
N	7,116	7,084	7,116	7,116	7,084
R^2	0.977	0.977	0.977	0.977	0.977

Notes: Results are for our preferred empirical specification (see text). D^G_i indicates whether political contestedness is *generally* low in the district, bank i operates in. D_{it-1} indicates whether the *preceding* election was close. C_{it} measures the contestedness of the *current* election. Boldfaced numbers indicate statistical significance at the 5% level.

sponse to real economic growth around election years, as the latter would arguably result in a much smoother pattern. Thus, figure 3 provides strong evidence for the political nature of the increase in loan extensions.

Prediction 4: What is the role of electoral competition?

Finally, prediction 4 suggests that electoral competition may have two partly offsetting effects. For one, the increase in lending may depend on the *ability* to manipulate the bank's policies and hence be more pronounced, the clearer the general (or historical) dominance of the incumbent party. This hypothesis is under consideration in table 4. We use two alternative measures to capture the strength of the ruling party. The first, D^G_i, is an indicator variable that switches on if the electoral district, bank i operates in, experiences relatively few changes in party power.[16] As column (A) of table 4

[16]To construct this measure, we create a normalized index that counts the number of times the strongest party has changed within a district. D^G_i indicates whether the electoral district under consideration ranks in the bottom quartile of the distribution of said index. Note that a change in relative party strength may not necessarily translate into a change in power, as the party with the plurality of votes may fail to reach an outright majority,

indicates, there is some evidence that stability of incumbency may in fact be a precondition for electoral cycles in lending: Despite being barely imprecisely estimated, savings banks in politically stable areas increase lending by 2.6% in the wake of elections relative to districts that see more frequent changes in power. For the latter, this election effect is – albeit positive and amounting to 1.3% – statistically insignificantly different from zero.

A similar picture emerges for our second measure of incumbent dominance, D_{it-1}, that reflects results of the *preceding* election, hence capturing the relative strength of the sitting government during its expiring term.[17] Results in column (B) indicate that lending increases by – again, barely imprecise – 2.8% in districts with clear majorities relative to all other districts, for which the electoral effect already amounts to 2.3%.

The converse effect of current electoral competition, which may increase *incentives* to induce a lending cycle, is under study in the last three columns of table 4. Since pre-election polling is generally unavailable for municipal elections, we have to rely on an ex-post measure when assessing the contestedness of the electoral campaign: the actual election outcome. Given rational expectations and a reasonable feeling of local politicians for the mood of their electorate, we argue that the closeness of the final result should provide a reasonable proxy for the perceived closeness of the contest itself. On this account, our indicator of current electoral competition, C_{it}, takes on the value of 1 if the winner's final vote share is either below 40% or if the winning margin is less than 5%.[18]

According to estimates in column (C), there is no compelling evidence of current electoral competition exerting any systematic influence on the strength of the election effect, which is estimated to be 2.4%. Yet, given that our measure of present contestedness may in part capture the diametric effect of party dominance as well, we refine our empirical specification by additionally controlling for general and historical contestedness, respectively. Results are displayed in columns (D) and (E) and they suggest that,

in which case it may have to accept opposition status if the other parties agree to form a coalition government. This notwithstanding, D_i^G should provide a reasonable approximation to the general stability of incumbency, we are ultimately interested in.

[17] Similar to the first measure, D_{it-1} is a bottom-quartile indicator for a normalized index that measures both, the absolute vote share of the winning party, as well as its margin of victory.

[18] Note that we have also experimented with alternative combinations of cutoff points – namely vote shares of 37% and 43% as well as winning margins of 3%, 7% and 10% – but that results are largely insensitive to these variations. The same holds true when instead applying a quartile indicator that follows the same logic as the index-based measure of past incumbent dominance, D_{it-1}.

given overall party dominance, there is at least some indication that lending cycles may indeed be more likely if the upcoming election promises to be close. According to our point estimates, savings banks in districts that fall into this category, increase their lending by roughly 1.5% relative to a situation with little competition. However, given the apparent lack of precision of these estimates, we refrain from interpreting these results as anything more than suggestive evidence. Notably, earlier results that incumbent dominance can be viewed as a precondition for politically induced lending, are soundly reconfirmed, as statistically significant election effects appear to be exclusively present in districts with high degrees of political stability and current incumbent strength, respectively.

7 Robustness

As mentioned above, results presented in section 6 are based on our preferred empirical specification being applied to the full data sample. To ensure that conclusions are not driven by these choices, we present a number of robustness checks that demonstrate that the election effect, as measured by δ_2, is immune to varying definitions of key variables, sets of controls, sample compositions, and assumptions regarding the error-term structure. Note that the following results should be compared to the baseline estimates in columns (G) and (H) of table 2, which suggest politically induced lending increases of 2.7% and 2.4% (relative to the bank's total loan sum), respectively.

7.1 Alternative choices of variables

Alternative dependent variables

We start by gauging the robustness of the election effect to the choice of the dependent variable. Columns (A) and (B) of table 5 contain estimates of model 2, when applied to loan data in real values instead of log-transformed data. Apparently, our results in section 6 are not driven by the log-transformation of variables, as sign and significance of estimates are very similar to those in corresponding columns (G) and (H) of table 2. As a side effect, these estimates provide a better feel for the average size (in absolute terms) of the election effect, which is estimated to lie in the range of EUR 26.0 million and 30.6 million per bank, depending on the way time effects are controlled for.

Table 5. *Alternative dependent variables*
Dependent variables: See table notes

Explanatory variables	**OLS regression coefficients** (Empirical p-values in brackets)			
	(A)	(B)	(C)	(D)
Key regressor				
- $ELEC^M_{st} * B_b$	**30.593** **(0.001)**	**26.057** **(0.006)**	**0.019** **(0.000)**	**0.016** **(0.000)**
Bank controls	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Time FE	Yes		Yes	
Time trends		Yes		Yes
Bank type FE	Yes	Yes	Yes	Yes
N	7,116	7,116	7,116	7,116
R^2	0.977	0.977	0.135	0.131

Notes: Dependent variables for columns (A) and (B) are bank's loans in 1995 EUR and logs of (loans/total assets) for columns (C) and (D). Boldfaced numbers indicate statistical significance at the 5% level.

Yet another specification can be found in tables (C) and (D) of table 5. Here, loans are normalized by total assets before being transformed into logs, so that loan sum sizes are put into perspective with the size of the respective bank. Once again, the election effect is positive and highly significant at the 0.1% level.

Alternative control variables

The following two tables provide evidence that the election effect is also robust to variations in the set of covariates used for the analysis. Table 6 displays results for specifications that drop certain variables from the list of regressors, whereas specifications in table 7 are augmented with additional control variables, not used in our preferred empirical model.

As is evident from columns (A) through (D) of table 6, the election effect remains significant at the 1% level if any of the four control variables is individually excluded from the set of regressors. In fact, this robustness check suggests that results presented in section 6 represent rather conservative estimates of the true effect. As previewed above, once we exclude a bank's total assets – which have a quasi-mechanical relationship with loans and leave hardly any variation in the data unexplained – R^2 drops significantly to 0.714 and the estimate of the election effect increases to 18.7%. A similar picture emerges if all bank controls are excluded from our regression (column (E)) and if model 2 is implemented as pure DD without any additional controls (column (G)), with estimates for δ_2 ranging within a

Table 6. *Alternative control variables 1: Fewer covariates*
Dependent variable: Log loans

Explanatory variables	**OLS regression coefficients** (Empirical p-values in brackets)						
	(A)	(B)	(C)	(D)	(E)	(F)	(G)
Key regressor							
- $ELEC^{M}_{st} * B_b$	**0.187**	**0.027**	**0.027**	**0.027**	**0.116**	**0.015**	**0.069**
	(0.000)	**(0.001)**	**(0.001)**	**(0.001)**	**(0.000)**	**(0.012)**	**(0.000)**
Bank controls							
- Total assets	–	**0.974**	**0.978**	**0.983**	–	**0.990**	–
		(0.000)	**(0.000)**	**(0.000)**		**(0.000)**	
- Capital ratio	**-1.023**	–	0.038	0.042	–	**0.048**	–
	(0.000)		(0.089)	(0.062)		**(0.036)**	
District controls							
- Population	**-0.691**	-0.017	–	**0.028**	**-0.751**	–	–
	(0.000)	(0.511)		**(0.000)**	**(0.000)**		
- Real GDP	**0.936**	0.045	**0.031**	–	**1.002**	–	–
	(0.000)	(0.070)	**(0.000)**		**(0.000)**		
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	7,116	7,116	7,116	7,116	7,714	8,880	9,934
R^2	0.714	0.977	0.977	0.977	0.524	0.977	0.411

Notes: Combinations of explanatory variables are excluded from the vector of controls as indicated by "–". Boldfaced numbers indicate statistical significance at the 5% level.

7% and 12% increase in lending. On the other hand, results are slightly weakened if we fail to control for district controls (column (F)), as the election effect drops to 1.5%, which still represents an increase that is statistically significant at the 5% level. Given that our analysis certainly benefits from controlling for variables that may impact lending decisions irrespective of electoral timing, we attach higher credibility to specifications that account for both bank-specific factors and district-level macroeconomic factors. We are nonetheless pleased that the election effect is found in all of the aforementioned specifications and apparently not just the artificial result of omitted-variable bias or bad control.

To examine whether the further addition of covariates has a dampening effect on our δ_2 estimates, we include a multitude of district-level control variables to the set of regressors. As results in table 7 demonstrate, neither information on local public debt, (un)employment, real earnings, real wages, nor firm creation have a notable impact on the election effect, with the latter remaining in a narrow interval of 2.5% to 2.7%. Note that, while firm creation seems to have a statistically significant effect on banks' lending behavior, we decided to exclude this variable from our preferred specification, since it is unavailable for the time before 1998, which would

Table 7. *Alternative control variables 2: Additional covariates*
Dependent variable: Log loans

Explanatory variables	**OLS regression coefficients** (Empirical p-values in brackets)						
	(A)	(B)	(C)	(D)	(E)	(F)	(G)
Key regressor							
- $ELEC^M_{st} * B_b$	**0.027**	**0.027**	**0.027**	**0.027**	**0.027**	**0.025**	**0.025**
	(0.001)	**(0.001)**	**(0.001)**	**(0.001)**	**(0.003)**	**(0.006)**	**(0.007)**
Bank controls							
- Total assets	**0.976**	**0.977**	**0.977**	**0.974**	**0.974**	**0.970**	**0.968**
	(0.000)	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**
- Capital ratio	0.034	0.037	0.036	0.030	0.033	0.022	0.018
	(0.134)	(0.101)	(0.111)	(0.184)	(0.154)	(0.352)	(0.435)
District controls							
- Population	-0.001	-0.016	-0.010	**-0.161**	-0.015	**-0.138**	-0.236
	(0.773)	(0.584)	(0.773)	**(0.003)**	(0.599)	**(0.003)**	(0.072)
- Real GDP	**0.049**	0.040	**0.047**	0.039	0.021	0.028	-0.059
	(0.037)	(0.267)	**(0.030)**	(0.135)	(0.663)	(0.328)	(0.252)
- Public debt	-0.012	–	–	–	–	–	-0.014
	(0.356)						(0.258)
- Employment	–	0.003	–	–	–	–	0.042
		(0.948)					(0.680)
- Unemployment	–	–	-0.011	–	–	–	0.020
			(0.567)				(0.464)
- Real earnings	–	–	–	**0.137**	–	–	0.122
				(0.000)			(0.144)
- Real wages	–	–	–	–	0.023	–	0.047
					(0.585)		(0.501)
- Firm creation	–	–	–	–	–	**0.126**	**0.092**
						(0.000)	**(0.006)**
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	6,435	7,116	7,116	6,435	6,087	5,394	5,394
R^2	0.978	0.977	0.977	0.978	0.978	0.977	0.978

Notes: Combinations of explanatory variables are excluded from the vector of controls as indicated by "–". Boldfaced numbers indicate statistical significance at the 5% level.

needlessly reduce sample size and preclude the analysis of bank lending for most of the 1990s.

Alternative municipal election types

As argued in section 4.2, our main analysis concentrates on municipal elections of *legislative* bodies since our data provides more informational detail for this type of election than for municipal elections of *executive* representatives. This notwithstanding, we additionally test whether savings bank lending also reacts to direct elections of mayors and county representatives, which, as a rule, do not coincide with the election of municipal par-

Table 8. *Alternative municipal election types*
Dependent variables: Log loans

Explanatory variables	OLS regression coefficients (Empirical p-values in brackets)			
	(A)	(B)	(C)	(D)
Key regressor				
- $ELEC_{ist}^{M_X} * B_b$	**0.022** **(0.014)**	**0.024** **(0.010)**	–	–
- $ELEC_{ist}^{M_{XP}} * B_b$	–	–	**0.020** **(0.011)**	**0.021** **(0.017)**
Bank controls	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Time FE	Yes		Yes	
Time trends		Yes		Yes
Bank type FE	Yes	Yes	Yes	Yes
N	7,116	7,116	7,116	7,116
R^2	0.977	0.977	0.977	0.976

Notes: The index M_X stands for direct municipal elections of the executive, and M_{XP} denotes politically relevant elections for executive representatives. Boldfaced numbers indicate statistical significance at the 5% level.

liaments.[19] As results in columns (A) and (B) of table 8 show, this is indeed the case, even though the effect is a bit smaller – ranging between 2% and 2.5% – than that generated by legislative elections.

Since direct municipal elections were not the norm until the mid-1990s and are still not implemented in Baden-Württemberg's rural counties, these results are based on a lower number of electoral events than was the case in section 6. Moreover, they may not be based on the most useful measure of elections of executive representatives because a substantial fraction of the latter is indirectly determined by the regional parliament. As a consequence, the fate of this kind of executive depends on legislative elections, as well. For this reason, we create an alternative variable, $ELEC_{ist}^{M_{XP}}$, that indicates both the occurrence of a legislative election if the executive is appointed by the legislative body, and the occurrence of a direct executive election in case the respective district stipulates this electoral rule. We expect this indicator to be a more appropriate measure of *politically relevant* elections of local executives. According to results displayed in the two final columns of table 8, our earlier conclusion that executive politicians also

[19]Note that, since executive elections are usually not held at the state level, the respective indicator variable, $ELEC_{ist}^{M_X}$, is allowed to vary at the district level, which was not the case for the legislative election dummy, $ELEC_{st}^{M}$.

Table 9. *Alternative sample compositions*
Dependent variable: Log loans

Explanatory variables	**OLS regression coefficients** (Empirical p-values in brackets)						
	(A)	(B)	(C)	(D)	(E)	(F)	(G)
Key regressor							
- $ELEC^{M}_{st} * B_b$	**0.030**	**0.024**	**0.027**	**0.020**	0.014	**0.044**	**0.028**
	(0.001)	**(0.036)**	**(0.009)**	**(0.033)**	(0.079)	**(0.000)**	**(0.001)**
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	No	No	No	No	No	No
Year range	93-09	93-01	01-09	93-09	93-09	93-09	93-09
Dropped state	None	None	None	BW	BY	HS	RP
N	3,945	2,767	4,353	4,730	4,077	6,002	6,304
R^2	0.970	0.969	0.978	0.975	0.984	0.975	0.977

Notes: Abbreviations are as follows: Years: 93=1993; 01=2001; 09=2009. States: BW=Baden-Württemberg; BY=Bavaria; HS=Hesse; RP=Rhineland-Palatinate. Boldfaced numbers indicate statistical significance at the 5% level.

induce electoral lending cycles (that may be mildly less pronounced than those by their legislative counterparts) is confirmed.

7.2 Alternative sample compositions

Alternative panel balancedness

Our main results from section 6 come from the entire available data sample. As mentioned in section 4.1, however, our bank data is quite unbalanced since many banks entered the sample some time after 1993. At the same time, the sample is subject to mild attrition that is mainly due to mergers of savings banks. To ensure that our results are not driven by these data characteristics, we re-estimate DD model 2 on a completely balanced panel. This alternative sample consists of 159 savings banks (2,703 bank years) and 104 cooperative banks (1,678 bank years) and represents roughly 25% of the original sample.[20]

As is evident from column (A) of table 9, the election effect proves to be immune to even such extreme reductions in sample size: Based on banks that remained in the sample from 1993 to 2009, the estimated increase in savings bank lending amounts to 3% and is statistically significant at the 1% level.

[20]Note that in additional robustness checks not reported here, we have also experimented with earlier and later cut-off points than 1993 to create balanced panels. Estimations based on these, however, have not yielded any different conclusions.

Alternative time intervals

To investigate the stability of the election effect across time, we divide the whole sample into two panel sets of equal length, with the first covering the years between 1993 and 2001 and the second covering the time between 2001 and 2009. As can be seen in columns (B) and (C) of table 9, lending increases of roughly 2.5% occur in both the 1990s and the 2000s and are, hence, unlikely to be driven by any temporal anomalies not captured by our set of covariates and time dummies. The fact that the election effect for the later time interval is estimated with increased precision, is most likely attributable to the higher number of bank years: Recall that the representativeness of Hoppenstedt's cooperative bank data greatly improves during the early 2000s, as many smaller banks whose balance sheets were not collected before, are added to the sample around this time. On this account, it is encouraging that the election effect is robust to this kind of sample selectivity as well.

Alternative regional compositions

Finally, to assess the generality of results, we apply model 2 to four subsamples that individually exclude one of the four states our main sample consists of. As columns (D) through (G) of table 9 show, the strength of the election effect appears to be sensitive to the choice of states used for analysis. While results are broadly unchanged by the exclusion of Rhineland-Palatinate, the election effect is weakened if either Baden-Württemberg or Bavaria are dropped from our sample, suggesting that politically induced lending is especially pronounced in the two latter states. On the other hand, the estimate of δ_2 is considerably higher if Hesse is omitted from the working sample.[21]

Finding an explanation for these cross-state differences is certainly desirable, as this may further our understanding of the interplay of institutional features in limiting the extent of politically induced lending. One potential reason for the observed pattern could be found in the role of incumbent dominance, which – as argued in section 6 – may be a precondition for the ability of politicians to manipulate bank policies. Considering that electoral contests in Bavaria and Baden-Württemberg are historically much less contested than those in Hesse, the differences among states may well

[21] While this may reflect the absence of electoral lending cycles in Hesse, it is also possible that this observation is simply an artefact of Hesse's idiosyncratic time trends mentioned in section 4.4, which may disqualify Hesse as a viable control state.

be driven by this feature.[22] Other possible explanations for this regional heterogeneity may, for instance, be found in the aforementioned differences in electoral laws, minor variations in states' savings bank regulation, partisan effects, or – as is the case for Hesse – the concentrated presence of private banks that changes the operational environment for savings and cooperative banks.

A meaningful cross-state analysis that discriminates among such hypotheses, however, proves to be infeasible with just 4 states at our disposal. It is not only the low number of states that poses problems, but also the limited cross-state variability of certain features that is cause for concern: Recall that the states chosen for analysis are all from the southern part of Germany. While this regional proximity improves the identification of the election effect since DD relies on states being on reasonably similar time trends, it is unlikely to provide sufficient variation in institutional and other state-specific characteristics. For example, it would be impossible to tell whether the differences across states are driven by electoral competition or, say, partisan effects because the two states with the highest incumbent dominance – Bavaria and Baden-Württemberg – are also historically dominated by the same party.[23] Hence, distinguishing between these alternative hypotheses would require the addition of states which are dominated by parties that represent the opposite of the political spectrum.[24] For these reasons, we are currently augmenting our dataset with several states from other parts of Germany to obtain a more representative sample than currently available. Besides enabling us to study cross-state heterogeneity, this step will also determine whether electoral lending cycles are a general phenomenon or if our results are instead driven by regional outliers.

7.3 Alternative assumptions on the error structure

The final robustness analysis presented here, assesses the stability of results to varying modes of statistical inference. The growing literature on cluster-

[22]This pattern holds for all indicators we constructed to capture the degree of electoral competition in a district. To give one example, 19% of all municipal elections of legislative bodies in Hesse are lost by the incumbent party. While this number is only a bit lower for Rhineland-Palatinate (17%), Baden-Württemberg (7%) and Bavaria (4%) are characterized by considerably higher party dominance.

[23]Recall from section 4.4 that, while Bavaria's ruling party, *CSU*, is legally independent from its sister party, *CDU*, which dominates politics in Baden-Württemberg, they are generally considered the same organizational entity, covering the conservative spectrum of German politics.

[24]An obvious candidate appears to be the state of *North Rhine-Westphalia* whose political system is largely dominated by left-of-center *SPD*.

robust inference (see Bertrand *et al.*, 2004; Angrist and Pischke, 2009; and Cameron and Miller, 2010 for an overview) highlights the importance of accounting for potential serial correlation and regional clustering in panel data. Both phenomena implicate a violation of one of the main assumptions traditionally imposed when working with cross-sectional data: the independence of observations. While OLS will still be consistent, precision is likely overestimated if these issues are ignored.

Serial correlation

Serially correlated errors, ϵ_{ist}, are a typical problem of panel data applications and there is little reason to believe that the present study is an exception. Formally, $\text{Cor}(\epsilon_{ist}, \epsilon_{isu}) = \rho_\epsilon \neq 0$, for $t \neq u$, where ρ_ϵ denotes the intraclass correlation of the error. That is, the individual (here: bank i) is thought of as a cluster whose observations over time are not independent of one another. A rough estimate of ρ_ϵ – the average autocorrelation over 5 lags of OLS residuals coming from model 2 – equals 0.412 and suggests that our data is indeed subject to substantial serial correlation. For this reason, and in line with Bertrand *et al.* (2004), Khwaja and Mian (2005), and Cameron and Trivedi (2010), our preferred empirical specification already corrects for serial correlation by clustering standard errors on the bank level instead of the bank-year level.

An alternative way of dealing with autocorrelated errors consists of estimating model 2 with a random-effects (RE) specification (see Cameron and Trivedi, 2010). The individual-effects model provides the following rationale for serial correlation: If the error $\epsilon_{ist} = \alpha_i + r_{ist}$, then the presence of a bank-specific effect, α_i, induces correlation over time, even if the idiosyncratic, r_{ist}, is iid. If these assumption on the error structure are correct and as long as α_i is truly random, RE is more efficient than OLS, which is why we present results for regression 2, fitted by FGLS in column (B) of table 10. When compared to our baseline specification in column (A), the estimated election effect is virtually unchanged by this alternative approach of correcting for serial correlation. The estimated standard deviation of the individual effect, $\hat{\sigma}_\alpha$, equals 0.104 and is roughly as large as that of the idiosyncratic error, $\hat{\sigma}_r = 0.120$. Furthermore, intraclass correlation is estimated to equal 0.427, which is in line with our ad-hoc estimate, mentioned above.

Of course, the RE estimator is only consistent if α_i is uncorrelated with regressors. If we wish to relax this assumption, the individual effect needs to be eliminated with a fixed-effects (FE) specification that only relies on

Table 10. *Alternative error assumptions*
Dependent variable: Log loans

Explanatory variables	**Regression coefficients** (Empirical p-values in brackets)			
	(A)	(B)	(C)	(D)
Key regressor				
- $ELEC^M_{st} * B_b$	**0.027** **(0.001)**	**0.028** **(0.000)**	**0.022** **(0.000)**	**0.027** **(0.002)**
Bank controls	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	n.a.	Yes
Time FE	Yes	Yes	Yes	Yes
Bank type FE	Yes	Yes	n.a.	Yes
Estimator	OLS	RE	FE	OLS
Cluster	Bank	Bank	Bank	District
N	7,116	7,116	7,116	7,116
R^2	0.978	0.976	0.954	0.978

Notes: Abbreviations are as follows: OLS=Ordinary Least Square; RE=Random Effects; FE=Fixed Effects (Within estimator). "Cluster" indicates whether standard errors are clustered on the bank or the district level. "n.a." denotes that time-constant variables are omitted. Boldfaced numbers indicate statistical significance at the 5% level.

variation over time. Even though we are not particularly worried about correlated effects, results for model 2 when fitted by a within estimator are presented in column (C) of table 10. While the election effect appears to be somewhat smaller than before (amounting to 2.2%), it is still very precisely estimated. We take this as encouraging evidence that our results survive, even when identification is based on within-variation alone.

Regional clustering

If data has a group structure, independence may not only be violated for observations of one individual bank over time, but also across banks that are part of the same regional cluster. In this case, ϵ_{ist} will contain some variation that is likely to be common to banks in the same geographical area and year, for instance, a regional business cycle. An obvious solution to this problem is to correct standard errors for clustering on the geographical level, these region-year shocks are most likely to occur.

In our context, there are two candidates for such regional clusters: the municipal district (which typically contains one savings bank and one to four cooperative banks) and the state. While clustering standard errors on the district level is straightforward, this methodological fix is infeasible for the state level, since robust inference requires a larger number of clusters than is available in our data.[25] As the non-trivial issue of inference with few clusters is still under study in the literature, we content ourselves with implementing what is methodologically feasible at present and provide estimates of model 2 with standard errors clustered at the district level in column (D) of table 10. Evidently, precision is only marginally reduced – with standard errors rising from 0.0079 to 0.0087 – suggesting that district-level clustering is not much reason for concern.

8 Conclusion

We exploit a particularity in the German public banking system where local politicians are by law actively involved in the management of savings banks' lending decisions to test the hypothesis that incumbent politicians pursue policies that benefit their re-election probability but might be socially suboptimal. Our identification strategy relies on the fact that we should only observe politically motivated lending around election years, only in municipalities in which elections are held at this point in time, and – importantly – only for savings banks that are by law politically connected. Econometrically, we conduct difference-in-difference (DD) as well as triple-difference (DDD) estimation embedded in a fixed-effects panel data setup.

We use a unique, largely hand-collected dataset that combines detailed information on German municipal elections, macro-economic data on the district level, and balance-sheet information on bank lending. We find that savings banks systematically extend more credits in pre-election periods. This effect is not only statistically significant but also economically relevant. Given that savings banks are holding the largest market share in the private customer deposit market and they are the most important lender to the *Mittelstand* (SMEs) that is considered the backbone of the German economy, it is potentially worrisome to find their policies distorted. The pre-election excess-lending effect is robust to various specifications. Importantly, this effect is not present with cooperative banks that are very similar to savings banks but that lack the political connectedness of savings banks. Furthermore, and in line with our hypothesis, lending cycles only occur with *municipal* but not with state or federal elections. In addition, we find weak evidence for overly prudent lending policies after elections, consis-

[25]Note that this problem cannot by solved by simply increasing the number of states in our dataset, since the natural limit will be the total number of 16 German states, whereas the literature suggests a minimum number of around 40 to 50 groups (see Angrist and Pischke, 2009 and Cameron and Miller, 2010).

tent with a binding credit constraint that banks face as they have to make up for excessive pre-election lending.

Our evidence suggests that the *ability* of politicians to influence the lending policies of their respective savings bank depends on the dominance of the incumbent party, since electoral cycles are less common in districts with frequent changes in power and historically close elections. Whether *incentives* to induce socially inefficient lending increases are influenced by the contestedness of the current electoral campaign, is inconclusive and may become answerable once test power increases in the course of gradually extending our dataset for further research: On the one hand, we are currently adding municipal election results for additional German states to better understand the interplay of various institutional features in limiting the extent of politically induced lending cycles. On the other hand, we are hopeful to eventually be granted access to more detailed credit-contract data that will allow us to test more nuanced hypotheses such as prediction 5, as this will shed more light on the social costs of interfering with bank policies for political gain.

A Appendix

As argued in section 6, we believe DD model 2 to provide improved identification of the causal election effect over model 1. However, for robustness, this appendix also tests predictions 2 through 4 with model 1 that only exploits differences between savings banks. Overall, our main results are confirmed, even though effect sizes appear to be a bit smaller.

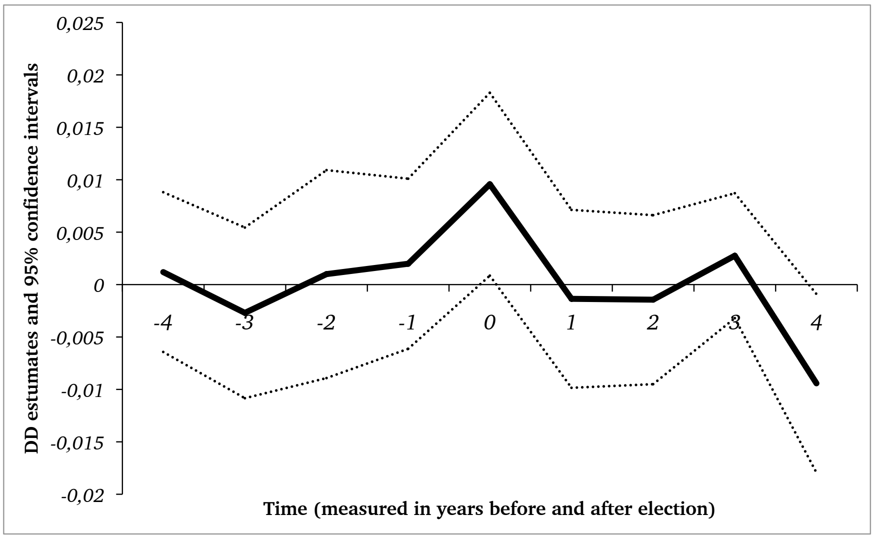
As is evident from table 11, municipal elections are once again the only type of election with a statistically significant impact on savings bank lending, which confirms prediction 2, that state-level and federal elections should play less of a role. The electoral lending cycle, as estimated without a control group of politically unconnected banks, is visualized in figure 4 and roughly follows the pattern depicted in figure 3, with a significant spike just before an election and weak evidence for credit crunching in the years thereafter. The evidence for the effects of electoral competition is a little more mixed (see table 12). As before, current contestedness does not seem to systematically influence politically induced lending. Now, this result even holds if incumbent dominance is controlled for. As far as the latter is concerned, there is still evidence that the ability to induce lending cycles increases with the decisiveness of the past electoral victory, even though the significance level has increased to roughly 10%. Note that, while results in columns (A) and (D) of table 12 do no longer suggest that *general* incumbent stability facilitates electoral cycles, this result does indeed survive if we run separate regressions – not presented here – on two subsamples that are stratified by overall competitiveness of the electoral district.

Table 11. *Results for prediction 2 (Model 1)*
Dependent variable: Log loans

Explanatory variables	**OLS regression coefficients** (Empirical p-values in brackets)			
	(A)	(B)	(C)	(D)
Key regressors				
- $ELEC_{st}^{M}$	–	–	–	**0.021** **(0.000)**
- $ELEC_{st}^{S}$	-0.000 (0.942)	0.003 (0.362)	–	0.010 (0.102)
- $ELEC_{t}^{F}$	–	–	-0.003 (0.294)	-0.003 (0.350)
Bank controls	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Time FE	Yes			
Time trends		Yes	Yes	Yes
Banks in sample	SavB	SavB	SavB	SavB
N	3,083	3,083	3,083	3,083
R^2	0.970	0.969	0.969	0.969

Notes: Results are for our preferred empirical specification (see text). Key regressors are $ELEC_{st}^{M} * B_b$, $ELEC_{st}^{S} * B_b$, and $ELEC_{t}^{F} * B_b$, respectively. The indexes M, S, and F denote municipal, state, and federal elections, respectively. Boldfaced numbers indicate statistical significance at the 5% level.

Figure 4. *Results for prediction 3 (Model 1)*
Visualization of the lending cycle



Notes: Results are for our preferred empirical specification (see text). The solid line depicts estimates of δ_1 coming from model 1 with year fixed effects. Dottet lines indicate the corresponding 95% confidence intervals. Time is measured on the abscissa: A value of zero denotes an election season. Negative and positive values stand for years before and after an election, respectively.

Table 12. *Results for prediction 4 (Model 1)*
Dependent variable: Log loans

Explanatory variables	**OLS regression coefficients** (Empirical p-values in brackets)				
	(A)	(B)	(C)	(D)	(E)
DD Interaction					
- $ELEC^M_{st} * D^G_i$	0.009 (0.376)	–	–	0.008 (0.434)	–
- $ELEC^M_{st} * D_{it-1}$	–	0.022 (0.093)	–	–	0.022 (0.108)
- $ELEC^M_{st} * C_{it}$	–	–	-0.003 (0.767)	0.000 (0.969)	0.001 (0.963)
Main effect					
- $ELEC^M_{st}$	0.004 (0.605)	0.007 (0.196)	0.011 (0.071)	0.004 (0.674)	0.006 (0.375)
Bank controls	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Banks in sample	SavB	SavB	SavB	SavB	SavB
N	3,067	3,059	3,067	3,067	3,059
R^2	0.970	0.970	0.970	0.970	0.970

Notes: Results are for our preferred empirical specification (see text). D^G_i indicates whether political contestedness is *generally* low in the district, bank i operates in. D_{it-1} indicates whether the *preceding* election was close. C_{it} measures the contestedness of the *current* election. Boldfaced numbers indicate statistical significance at the 5% level.

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