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Elections, political competition and bank failure ☆

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ABSTRACT

We exploit exogenous variation in the scheduling of gubernatorial elections to study the timing of bank failure in the US. Using hazard analysis, we show that bank failure is about 45% less likely in the year leading up to an election. Political control (i.e., lack of competition) can explain all of this average election year fall in the hazard rate. In particular, we show that the reduction in hazard rate doubles in magnitude for banks operating in states where the governor has simultaneous control of the upper and lower houses of the state legislature (i.e., complete control) heading into an election.

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1. Introduction

The relation between banking and politics is an intimate one. Governments control the supply of banks in the economy through chartering restrictions and licensing, they set up institutions that provide depositors with insurance and banks with a lender of last resort, and routinely set rules that attempt to govern the risk taking behavior of banks.

According to the biannual Banking Banana Skins survey by Pricewaterhouse Coopers and the Centre for the Study of Financial Innovation, "political interference" was rated as the number one risk that banks faced in 2010.[1] The result was surprising, given that the international banking system had witnessed possibly the worst crisis on record, which was largely attributed to credit and liquidity risks. And it was ironic, given that the banks were bailed out by politicians using public money.

This active role of government in the banking sector creates an incentive problem. On the one hand, governments play a role in the creation of institutions that make a banking system possible. On the other hand, they often look to the banking system to facilitate their own political survival. Political support can be indirect through, say, subsidized lending to preferred industries or direct in the form of campaign contributions or a share of profits due to ownership. For example, according to the Centre for Responsive Politics, Spencer Bachus, Republican from Alabama, who was the chairman of the House Financial Services Committee in the 112th Congress (2011–2013),

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[1] See http://www.pwc.com.au/media-centre/2010/political-interference-banking-risks-feb10.htm.

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raised more than $2.3 million in campaign funds in 2011–2012. The top five industries, contributing over 40% the funds, were commercial banks, securities and investment, insurance, real estate, and finance or credit companies.[2]

So while a healthy banking system can be huge source of benefit for politicians, bank failure can get politicians into electoral hot water. Politicians therefore have incentives to interfere with bank closure rules, for example, to favor preferred (politically connected) constituents or simply to avoid the political costs associated with failure.

The media have reported on several examples of political interference in the banking system. Probably the most famous case is that of Lincoln Savings and Loans, in which five US senators (known as the Keating Five) were accused of improperly intervening in a regulatory investigation of Charles H. Keating, Jr. (chairman of the Lincoln Savings and Loan Association) by the Federal Home Loan Bank Board (FHLBB) in 1987.[3] Lincoln Savings and Loans eventually collapsed in 1989, at a cost of over $3 billion to the federal government. The substantial political contributions Keating had made to each of the senators, totalling $1.3 million, attracted considerable public and media attention leading to a Senate Ethics Committee investigation in which three of the senators were found to have "substantially and improperly interfered with the FHLBB's investigation" and the other two while being cleared were still criticized for exercising "poor judgment". All five senators served out their terms. However, only two ran for reelection.[4]

A more recent example is that of Cleveland thrift AmTrust, whose failure was delayed by 11 months because Ohio representative Steven C. LaTourette and Cleveland mayor Frank G. Jackson intervened when the Federal Deposit Insurance Corporation (FDIC) tried to seize and sell the institution in January 2009.[5] By the time AmTrust was finally seized by the FDIC on December 4, 2009 its common equity had fallen by $667 million to $276 million from the year before. The failure cost the FDIC insurance fund $2 billion.

Are these incidents isolated cases? Or are they representative of a more systematic phenomenon? A natural place to look for systematic evidence of political interference in banking is around elections, as this is when bank failure can be the most costly to a politician. Bank failure typically leads to costs that are borne by the local voting population, leading the electorate to question the competency of the incumbent in regulating the banking sector.[6] Accordingly, politicians have the incentive to take costly action to delay bank failure during election periods. Further, the economic cost of delay (possibly from larger losses to the insurance fund than would otherwise be the case) is widespread across taxpayers, whereas the benefits are concentrated with interest groups such as bank owners, employees, uninsured depositors and small business borrowers that cannot access alternative sources of financing thus exacerbating the political incentive to delay bank failure in an election year (for more on interest groups, see Stigler, 1971; Peltzman, 1976; Becker, 1983).

Our empirical application tests this conjecture using data from the United States between 1934 and 2012, covering all failed banks (3,995) documented by the FDIC.[7] We use a hazard analysis to exploit the significant cross-state and within-state exogenous variations in gubernatorial electoral timing to explain bank failure. A consistent picture emerges: Bank failure is much less likely to occur in the 12 months leading up to an election than in nonelection periods. Our results are not only statistically significant but also economically meaningful. On average, bank failure is approximately 45% less likely in the year leading up to an election. The results are robust to multiple model specifications and estimation techniques.

We also investigate the role of political control (i.e., lack of political competition) in determining the election year fall in hazard rate, we construct a variable to capture instances in which the incumbent governor's party has control (holds the majority of seats) of both the lower and upper house simultaneously (complete control of the state legislature). We show that years in which the governor's party has complete political control heading into an election can explain all of the average preelection fall in the hazard rate. In particular, our estimates suggest that the magnitude of the election year reduction in failure rate more than doubles for banks in states where the governor has complete control heading into an election.[8]

Our work is related to several streams of literature. First, our work is most related to a paper by Brown and Dinc (2005), who study electoral incentives to delay bank failure for a sample of 164 banks (40 of which failed) in developing countries between 1994 and 2000. They conduct their analysis at the bank level and show that bank failure is much less likely before an election. Our work complements and extends theirs in several ways. First, a key focus of our analysis is on political competition and political control and their impact on bank failure during election years. Second, our study in a US setting provides a much larger sample of banks and a much larger number of

[2] He was quoted in an interview with the Birmingham News on December 8, 2010 saying "in Washington, the view is that the banks are to be regulated, and my view is that Washington and the regulators are there to serve the banks."

[3] Alan Cranston (Democrat of California), Dennis DeConcini (Democrat of Arizona), John Glenn (Democrat of Ohio), John McCain (Republican of Arizona), and Donald W. Riegle, Jr. (Democrat of Michigan)

[4] Glenn and McCain were cleared of the charges and re-ran for office.

[5] AmTrust was issued with a cease and desist order in November 2008, and when it failed to recapitalize by the December 31, 2008 deadline, the FDIC stepped in. The local politicians were able to delay the failure by convincing the Department of Treasury and the White House to stop the FDIC intervention.

[6] For example, due to losses to uninsured depositors, shareholders, bank employees and small business borrowers who cannot switch to other parties.

[7] While our summary statistics include all bank failures between 1934 and 2012, our regression analyses requires accounting data that are available only between 1976 and 2010, covering 1,966 bank failures.

[8] We also investigate the role of electoral competition in contributing to the reduction in the election year hazard rate. One might expect that the benefits associated with delaying bank failure increase with the degree of electoral competition (i.e. bank failure matters more to re-election chances when elections are close). However, our results from these analyses are not statistically significant. We argue that this weak electoral competition result could be due to the fact that while a lack of political competition reduces the benefit of delaying bank failure, it also reduces the costs of delaying bank failure (discussed in Section 5). The net effect, is therefore, an empirical issue.

failed banks. Moreover, the US setting is useful because the gubernatorial election cycle is not only exogenous, but also differs both across and within state.[9] Third, the Brown and Dinc analysis is conducted for banks in developing countries where corruption is arguably more of a problem. In contrast, we study the bank failure in the US, a developed democracy, and show that political incentives to delay bank failure near elections remain strong.

Second, our work relates directly to the early work arguing that politicians have incentives to take actions to induce favorable macroeconomic outcomes before elections (see, for example, McRae, 1977; Nordhaus, 1975; Rogoff and Sibert, 1988). More recent works by Levitt (1997, 2002) use election cycles to instrument for the number of police in his study of the relation between police and crime, arguing that politicians tend to hire more police prior to elections. Election cycles have also been used recently in the analysis of corporate investment decisions. Julio and Yook (2012) find a fall in corporate investment corresponding with timing of national elections around the world.

Third, our paper is related to a broad literature examining various aspects of the political economy of banking and bank regulation. Earlier work examining the role of politics and the incentives for regulators to intervene in failing banks' operations includes Kroszner and Strahan (1996), who show that regulators deferred the realization of costs in failing savings and loan (S&L) associations in the United States. Kroszner and Strahan (1999) also study the political economy factors that determine the timing of state level relaxation of bank branching restrictions in the US and find that private interest (or positive) theory of regulation (Stigler, 1971; Peltzman, 1976) best explains the timing of branching deregulation. Rosenbluth and Schaap (2003) study how electoral rules (centrifugal versus centripetal) shape the way politicians choose to regulate their national banking sectors and the resultant impact on market structure. Most recently, Dam and Koetter (in press) show that political factors determine the likelihood of bank bailout and, therefore, bank risk taking (moral hazard).

Finally, our paper is related to the large and important debate on the role of political competition in determining the degree of corrupt behavior by public officials. Theoretical studies, for example, by Barro (1973), Rose-Ackerman (1978), Ferejohn (1986), Shleifer and Vishny (1993), Aidt (2003), and Alt and Lassen (2003) conclude that political competition tends to ameliorate corrupt behavior. Empirical contributions also find support for the idea that political competition reduces corruption (see, for example, Kunicova and Rose-Ackerman, 2005; Lederman, Loayza and Soares, 2005; Tavits, 2007; Nyblade and Reed, 2008). We also find that political competition tends to discipline politicians, while political control exacerbates the problem.

The next section discusses the nature of bank failure and bank regulation in the US context. Section 3 discusses how the US election cycle works and provides some historical background on political competition and bank failure in the US. Section 4 outlines our empirical approach, presents our main results and robustness test. Section 5 presents the results from additional tests to investigate the role of political competition and control. Finally, Section 6 concludes.

[9] Not all states hold gubernatorial elections in the same year. Moreover, some states change their constitution and, for example, switch from a 2-year election cycle to a 4-year election cycle during our sample period.

2. Bank failure

The US banking sector is unique in the sense that there are an incredibly large number of banks, most of which are relatively small. Bank failure is also more frequent relative to other countries, making the US an ideal setting to study bank failure. Data on bank failures and the characteristics of the failing banks at the time of failure are from the FDIC. The FDIC has a number of ways in which it deals with a failing institution, so "failure" does not always imply the bank in question ceases to operate. Broadly, the FDIC categorizes failures into those in which the bank's charter survives ("assistance transactions") and those in which the bank charter is terminated ("outright failure"). In the case of the former, the FDIC either provides direct assistance to the failing bank, known as an open bank assistance (OBA) transaction, or provides assistance to an acquiring institution to purchase the entire failing bank.[10] In the case of the latter, the bank charter is terminated and its assets are auctioned off.[11] In what follows we initially consider both assistance transactions and outright failures as the same. In later analysis, we examine whether the type of failure differs around gubernatorial elections.

Bank regulation in the US is also segmented. While the FDIC insures all deposit-taking institutions, the chartering authority differs depending on whether the institution is a national bank, a state bank or a thrift. The chartering authority for national banks is the Office of the Comptroller of the Currency (OCC), and states banks are chartered by state regulators.[12] Thrifts are chartered by the Office of Thrift Supervision (OTS), which is a federal agency.[13]

Panel A in Table 1 presents a summary of all failed banks. In total, there have been 3995 bank failures in the US between 1934 and 2012. Not surprisingly, these have been concentrated (2,822 failures) in the two major crises since the great depression of the 1930s: the S&L crisis (1986–1992) and the recent Global Financial Crisis (GFC,

[10] OBA transactions were popular leading into and during the S&L crisis but lost their lustre following passage of the Financial Institutions Reform, Recovery and Enforcement Act (FIRREA) of 1989 and the Federal Deposit Insurance Corporation Improvement Act (FDICIA) of 1991. The FDICIA in particular made it more difficult for the FDIC to provide assistance to failing banks unless it could demonstrate that doing so would minimize the cost to the insurance fund, or show that the closure of the failing bank would increase the risk of systematic failure (Mingo, 1994).

[11] For more detail on FDIC transaction types, see http://www2.fdic.gov/hsob/help.asp

[12] Supervision is carried out by the FDIC in the case of state banks while the Federal Reserve supervises national banks as well as state banks electing to be members of the Federal Reserve System.

[13] The OTS was dissolved on July 2011 and its powers transferred to the OCC.

Table 1
Summary statistics.

This table presents summary statistics of bank failures and the key variables used in this study. Panel A presents a summary of the frequency of bank and thrift failures for all bank failures between 1934 and 2012 as well as broken down into subcategories. Crisis denotes failures occurring between 1986 and 1992 (savings and loan crisis) and between 2007 and 2010 (Global Financial Crisis). Federal Deposit Insurance Corporation recorded failure can be either an outright failure in which the institution loses its charter and ceases to operate or an assistance transaction whereby the institution is restructured and allowed to retain its charter. Panel B presents the state-level controls used in the hazard analysis (sample period 1976–2010). Panel C presents the bank-level controls used in the hazard analysis (sample period 1976–2010).

Panel A: Failures

Months around election	All	Crisis	Noncrisis	South	West	Northeast	Midwest
1934–2012							
All banks and thrifts							
[−12,0]	941	705	236	433	161	97	250
[0,+12]	963	577	386	446	151	133	233
Not around election	2,091	1,540	551	1,100	317	195	479
Commercial banks only							
[−12,0]	561	410	151	281	88	42	150
[0,+12]	656	333	323	327	96	61	172
Not around election	1,311	855	456	676	204	111	320
National banks							
[−12,0]	201	165	36	127	33	18	23
[0,+12]	181	125	56	99	27	24	31
Not around election	433	329	104	297	56	36	44
State banks							
[−12,0]	360	245	115	154	55	24	127
[0,+12]	475	208	267	228	69	37	141
Not around election	878	526	352	379	148	75	276
Assistance							
[−12,0]	141	59	82	56	18	21	46
[0,+12]	94	47	47	43	14	18	19
Not around election	358	283	75	205	41	21	91
Outright failure							
[−12,0]	800	646	154	377	143	76	204
[0,+12]	869	530	339	403	137	115	214
Not around election	1,733	1,257	476	895	276	174	388
1976–2012							
All banks and thrifts							
[−12,0]	850	705	145	420	158	68	204
[0,+12]	783	577	206	384	143	110	146
Not around election	1,840	1,540	300	1,021	304	137	378
Commercial banks only							
[−12,0]	470	410	60	268	85	13	104
[0,+12]	476	333	143	265	88	38	85
Not around election	1,062	855	207	597	191	54	220
National banks							
[−12,0]	182	165	17	126	30	4	22
[0,+12]	151	125	26	96	23	14	18
Not around election	386	329	57	283	48	16	39
State banks							
[−12,0]	288	245	43	142	55	9	82
[0,+12]	325	208	117	169	65	24	67
Not around election	676	526	150	314	143	38	181
Assistance							
[−12,0]	141	59	82	56	18	21	46
[0,+12]	93	47	46	43	14	17	19
Not around election	357	283	74	205	41	21	90
Outright failure							
[−12,0]	709	646	63	364	140	47	158
[0,+12]	690	530	160	341	129	93	127
Not around election	1,483	1,257	226	816	263	116	288

Panel B: State-level variables

Variable	All	Crisis	Noncrisis	South	West	Northeast	Midwest
VICTORY MARGIN (party neutral): winner vote percentage – second vote percentage							
Mean	0.17	0.18	0.16	0.18	0.16	0.17	0.16
Standard deviation	0.14	0.16	0.13	0.15	0.13	0.15	0.13
DEMOCRAT VOTE SHARE: Democrat votes/total votes							
Mean	0.49	0.49	0.48	0.52	0.48	0.47	0.46
Standard deviation	0.14	0.17	0.13	0.15	0.13	0.16	0.12
DEMOCRAT GOVERNOR: indicator equals one if the governor is a Democrat							
Mean	0.53	0.56	0.52	0.61	0.58	0.52	0.38

Table 1 (*continued*)

Panel B: State-level variables Variable	All	Crisis	Noncrisis	South	West	Northeast	Midwest
Standard deviation	0.5	0.5	0.5	0.49	0.49	0.5	0.49
% DEMOCRAT IN LOWER HOUSE: Democrat seats/total seats in the lower house of state legislature							
Mean	0.57	0.58	0.56	0.67	0.49	0.6	0.48
Standard deviation	0.18	0.16	0.18	0.17	0.17	0.16	0.11
% DEMOCRAT IN UPPER HOUSE: Democrat seats/total seats in upper house of state legislature							
Mean	0.56	0.57	0.56	0.68	0.49	0.57	0.47
Standard deviation	0.18	0.17	0.18	0.16	0.17	0.17	0.12
GROWTH: state income growth							
Mean	0.07	0.05	0.07	0.07	0.07	0.06	0.06
Standard deviation	0.04	0.04	0.04	0.04	0.04	0.04	0.04
BUDGET DEFICIT: (total taxes-government expenditure)/gross state product							
Mean	−0.02	−0.02	−0.02	−0.02	−0.03	−0.02	−0.01
Standard deviation	0.03	0.03	0.03	0.03	0.04	0.02	0.01
EMPLOYMENT: total employed persons / population							
Mean	0.57	0.58	0.56	0.57	0.57	0.56	0.58
Standard deviation	0.11	0.11	0.11	0.17	0.06	0.05	0.07

Panel C: Bank-level variables Variable	All	Crisis	Noncrisis	Failed banks	Assisted banks	Other banks
TOO BIG TO FAIL: bank assets/total state banking assets						
Mean	0.0048	0.0049	0.0047	0.0026	0.0206	0.0048
Standard deviation	0.0286	0.0295	0.0281	0.0097	0.0675	0.0286
TOO MANY TO FAIL: average capital ratio of other banks in the same state						
Mean	0.0994	0.099	0.0996	0.0904	0.0875	0.0994
Standard deviation	0.0175	0.0211	0.0157	0.02	0.0418	0.0175
INCOME/ASSET RATIO: net income/total assets						
Mean	0.0153	0.0038	0.0217	−0.0449	−0.0231	0.0154
Standard deviation	9.4348	0.0823	11.7264	0.0577	0.0578	9.441
SIZE: log(total deposits)						
Mean	10.7383	10.9572	10.6414	10.7036	11.5951	10.7384
Standard deviation	1.411	1.381	1.4132	1.507	2.2158	1.4108
CAPITAL RATIO: total equity/total assets						
Mean	0.0995	0.0991	0.0997	0.0007	0.0013	0.0996
Standard deviation	0.0665	0.0744	0.0627	0.0616	0.1056	0.0664
NPL: non-performing loans/total loans (percent)						
Mean	1.8864	2.4513	1.4763	13.6842	9.6820	1.869
Standard deviation	2.8619	3.3759	2.3372	9.2565	8.8496	2.8054
CHARTER AUTHORITY: indicator equals one if federal charter						
Mean	0.3019	0.3051	0.3006	0.3539	0.4430	0.3019
Standard deviation	0.4591	0.4604	0.4585	0.4783	0.4984	0.4591

2007–2010). Appropriately controlling for these crises is very important for our analysis. Failures are also concentrated in the Southern states, where almost half (1,979) of the failures were recorded. Of the failures, 2,528 were commercial banks (815 with national charters and 1,713 state chartered) and 1,467 were thrifts. In terms of the two broad categories of FDIC failure transactions, unlike Brown and Dinc (2005), most of the failures in our sample are outright failures whereby the bank's charter is terminated and the bank ceases to operate. Only 593 of the 3,995 failures are assistance transactions in which the bank's charter continues. Because our regression analysis requires bank accounting data that are available only from 1976, we also report in Panel A a summary of the failed bank sample for banks failing from 1976 onwards. The distribution of bank failures is largely the same as described for the full sample. Briefly, 3,473 of the 3,995 failures occurred between 1976 and 2012. Of these failures, 719 are national banks, 1,289 are state banks, 591 are assistance transactions, and 2,882 are outright failures.

3. Elections in the US

Election timing in the US is exogenously determined by law. Since 1845, election day takes place on the Tuesday in November after the first Monday, so, election day must fall somewhere between November 2 and November 8 (inclusive). Presidential elections follow a four-year cycle in even-numbered years. Other federal offices (House of Representatives and Senate) run on a two-year cycle in even-numbered years (i.e. in presidential election years as well as midterm elections).

At the state level, most states choose to run their gubernatorial elections in the same years as the federal elections (gubernatorial elections coincide with either presidential or midterm elections). Only five states run their gubernatorial elections in off-years or odd-numbered years.[14] In all but the

[14] These are Kentucky, Louisiana, Mississippi, New Jersey, and Virginia

states of New Hampshire and Vermont, gubernatorial elections currently follow a four-year cycle.[15]

For example, consider the Ohio General Assembly which is the state legislature of the US state of Ohio.[16] State election years coincide with federal midterm elections (e.g., 2010, 2014, 2018, 2022, etc.) and election day involves electing the governor, lieutenant governor, secretary of state, treasurer of state, auditor of state, attorney general, state senators (odd-numbered districts), state representatives, state board of education (one-third of members), Supreme Court justices (two or three), and some county officials.[17] In some cases, states have changed the length of their gubernatorial election cycle. For example, in 1986 Arizona changed from holding gubernatorial elections every two years to every four years.

Accordingly, unlike presidential elections, substantial across- and within-state variations exist in the timing of gubernatorial elections. We exploit this exogenous variation in gubernatorial election timing to study whether bank failure can be explained by electoral concerns. Because we are focusing on gubernatorial elections, the financial institutions of interest are state banks whose charters are controlled by state authorities. While thrifts and nationally chartered banks are not the focus of the main analysis, they are included in our summary statistics for a fuller picture.

Let the election date be day 0. We report in Table 1 the frequency of bank failure for a 24-month period around day 0 (i.e., 12 months prior to and 12 months after an election) as well as the failures that fall outside this 24-month window. Because a number of states either currently or historically hold gubernatorial elections every two years, we choose the [12, +12] window as this is the longest possible window around a gubernatorial election that does not crossover into elections preceding and following it.

If we compare the number of failures that occur in the 12 months prior to an election date to the number that occur in the 12 months following an election date, we find (for the full sample) 941 failures occur in the 12 months leading up to a gubernatorial election and 963 failures occur in the 12 months following. The remaining 2,091 failures fall outside our [−12, +12] window.[18]

While these data show fewer bank failures in the 12 months prior to an election compared with the 12 months after, the difference is not meaningful. Further investigation shows that failures clustered in crises periods tend to coincide with the preelection period. To see this, we subdivide the sample into crisis periods (the S&L crisis, 1986–1992, and the Global Financial Crisis, 2007–2010) versus non-crisis periods (all other years). The second column of Panel A in Table 1 shows that, in crisis years, 705 failures occur in the 12 months leading up to an election and only 577 occur in the 12 months following. We argue that there are several reasons that we should concern ourselves predominantly with investigating political incentives to delay failure in noncrisis years. First, during a crisis, the political cost to a local politician associated with a bank failure is lower because he can, in part or in full, deflect the cause of the failure away from his potential mismanagement of the economy and bank regulation. Second, bank failure tends to be more severe during a crisis, which accordingly makes it more difficult for politicians to delay regulatory intervention, other things equal. These differing incentives during a crisis imply that we are much less likely to observe political factors determining bank failure.

As reported in Table 1, Panel A, in non-crisis periods, 236 failures occur in the 12 months prior to an election and 386 fail in the 12 months after. These data imply that the frequency of bank failure is almost 40% lower in the 12 months before an election compared with the 12 months after. These findings are best illustrated in Fig. 1. Here, the light gray bars plot the number of bank failures in three-month blocks leading up to an election, and the dark gray bars plot the number of bank failures in three-month blocks in the months after state elections. The light gray dashed and dark gray dotted horizontal lines represent the average number of bank failures in a three-month period before and after elections respectively. Panel A of Fig. 1 plots bank failure for all years between 1934 and 2012, and Panel B plots bank failure around elections only for noncrisis years. That is, failures that occur during the S&L crisis (1986–1992) and the Global Financial Crisis (2007–2010) are excluded.

While Panel A of Fig. 1 shows no discernible difference between the pre- and post-election failure rates (235 versus 240 failures per three-months respectively), a striking picture emerges when we control for the clustering of bank failure around crises: Bank failure is much less likely in the months leading up to an election than in the months after. For the noncrisis period, the pre- and post-election average numbers of bank failures are 59 and 97 failures per three months respectively. Therefore, based on raw numbers alone, bank failure is about 40% less likely in the months leading up to an election compared with the months following an election. This finding highlights the importance of properly controlling for the impact of a financial crisis. The pattern described above is largely consistent for our subsamples. Comparing failures in the pre-election period with the post-election reported in Panel A of Table 1, failure is 53% less frequent for all commercial banks (151 failures pre- versus 323 failures post-election), 57% less frequent for state-charted banks (115 failures pre- versus 267 failures post-election), 30% less frequent for all banks and thrifts in the 1976–2012

[15] These two states hold gubernatorial elections every two years.

[16] It consists of the 99-member Ohio House of Representatives and the 33-member Ohio Senate. Both houses of the General Assembly meet at the Ohio Statehouse in Columbus.

[17] Other state races such as those for state senators (even-numbered districts), state representatives, state board of education (one-third of members), Supreme Court justices (two or three) and remaining county officials are held on presidential election years (2012, 2016, 2020, etc.). State election days also involve electing the federal offices of president of the United States, US senators (if term expires), and US representatives.

[18] The failures occurring outside our [−12, +12] window (i.e., "Not around election" rows) are not directly comparable with the [−12, 0] and [0, +12] failure counts. For states with a two-year gubernatorial election cycle, by definition, banks fail either before or after an election. However, for states with a four-year election cycle, the "Not around election" period represents a 24-month period. So, the failure count would have to be divided by two to be comparable to our pre- and post-election windows.

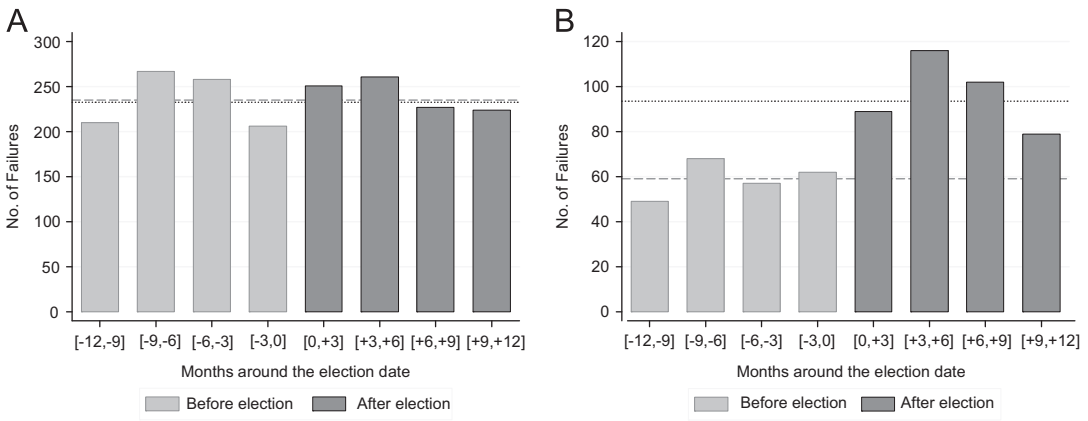

Fig. 1. Election cycles and bank failure. The figure plots the frequency of bank failures around gubernatorial elections for all recorded failures (3,995) of Federal Deposit Insurance Corporation (FDIC)–insured financial institutions (commercial banks and thrifts) between 1934 and 2012. Panel A plots bank failures in all periods. Panel B plots banks fails in noncrisis periods only; that is, failures that occur during the savings and loan crisis (1986–1992) and the Global Financial Crisis (2007–2010) are excluded. The bars in light gray plot the number of failures in three-month blocks leading up to an election, and the bars in dark-gray plot the number of failures in three-month blocks in the months after state elections. The horizontal dotted and dashed lines represent the average number of failures for a three-month period before and after elections, respectively.

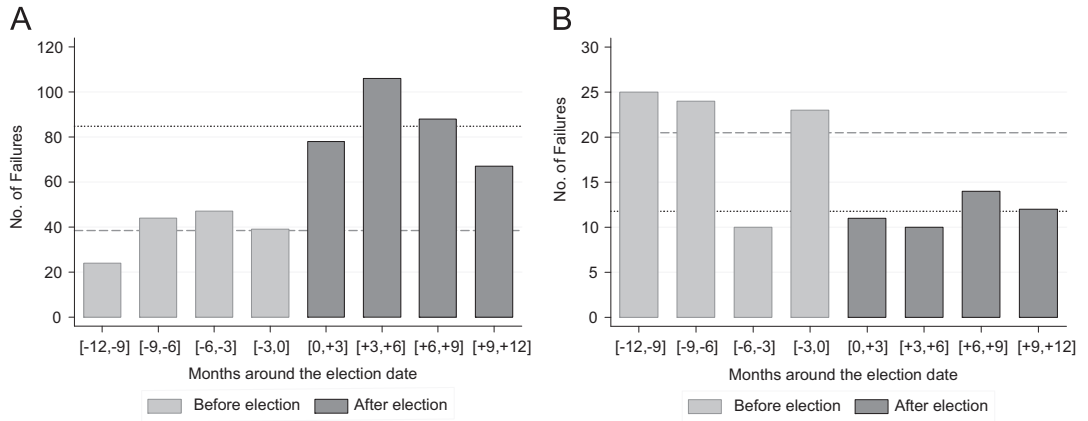

Fig. 2. Election cycles and bank failure, by type of failure. The figure plots the frequency of bank failures around gubernatorial elections for all recorded failures (3,995) of Federal Deposit Insurance Corporation (FDIC)–insured financial institutions (commercial banks and thrifts) between 1934 and 2012 in noncrisis periods only; that is, failures occurring during the Savings and loan crisis (1986–1992) and the Global Financial Crisis (2007–2010) are excluded. Panel A plots the frequency of outright bank failures, when a failing bank loses its charter and ceases to operate. Panel B plots bank failures that are classified as assistance transactions, when the failing bank is restructured with FDIC assistance and then allowed to continue to operate under its existing charter. The bars in light gray plot the number of failures in 3-month blocks leading up to an election, and the bars in dark gray plot the number of failures in three-month blocks in the months after state elections. The horizontal dotted and dashed lines represent the average number of failures for a three-month period before and after elections, respectively.

period (145 failures pre- versus 206 failures post-election), 58% less frequent for commercial banks in the 1976–2012 period (60 failures pre- versus 143 failures post-election), and 63% less frequent for state-chartered banks in the 1976–2012 period (43failures pre- versus 117 failures post-election).

Up to this point, we use the term "bank failure" to mean outright failures where banks lose their charter and cease to operate as well as FDIC assistance transactions in which a failing institution is restructured with FDIC assistance and allowed to continue to operate under its existing charter. However, if we split all failures into two subsamples depending on the failure type, we find that, for the 1934–2012 non-crisis sample, outright failures are about 55% less frequent in the months leading up to an election compared with the months following (154 failures pre- versus 339 failures post-election). Yet, we find that FDIC assistance transactions are much more likely in the months before an election. Based on our raw data, assistance is almost 75% more likely to occur in the 12 months leading up to an election (82 failures pre- versus 47 failures post-election). It appears as though politicians, as an alternative to delaying outright bank failure, can also opt to provide assistance to failing banks so that they can continue to operate in the year leading up to an election. In Fig. 2 we plot the frequency of bank failures around elections separately for outright failures (Panel A) and assistance transactions (Panel B). Like Fig. 1 the light gray bars plot the number of failures in three-month blocks leading up to an election while the dark gray bars plot the number of failures in three-month blocks in the months after elections. The light gray dashed and dark gray dotted

horizontal lines represent the average number of bank failures in a three-month period before and after elections respectively. A remarkable difference exists between the two figures. Outright failures are clearly less frequent prior to elections whereas assistance is much more frequent.

4. Empirical strategy and results

This section describes our empirical approach and presents the results from our analysis. We obtain quarterly accounting data for all (failed and surviving) commercial banks operating in the US from bank call reports filed with regulators. These data are available from March 31, 1976 until December 31, 2010. We hand-collect political data on the election dates and outcomes, the composition of state legislatures, party affiliation of governor and so on from the US Bureau of the Census Statistical Abstracts.[19] State macroeconomic data are sourced from the US Bureau of Economic Analysis and US Bureau of Labor Statistics. Our final sample of banks for which accounting data are available is an unbalanced panel of 22,230 banks, of which 1,966 fail.

Let the election date for state j be day 0. We construct *PREELECTION*, a variable that equals one if accounting quarter t of bank i from state j falls in the $[-12, 0]$ month window and zero otherwise. We test whether bank failures, defined as an outright failure or assistance transaction, do not depend on the gubernatorial election cycle in a Cox proportional hazard model given by

$$h(t) = \exp(\boldsymbol{\beta}'\mathbf{X}_{it-1} + \gamma_1 PREELECTION_{it} + \theta_t + \theta_j) \quad (1)$$

for $t = t_i, \ldots, T_i$, where t_i and T_i represent bank i's entry and exit dates (quarter) respectively.[20] In particular, the following entry and exit dates are used for the analysis. Bank i enters the study in quarter t_i, which is the later of two possible dates: March 31, 1976 (the start of the sample period), or the date bank i files its first call report after receiving its charter. Bank i exits the study in quarter T_i, which is the earliest of four possible events: the banks fails (outright) and its charter is terminated; the failing bank receives FDIC assistance, is restructured and allowed to continue to operate under its existing charter; the bank is acquired by another bank so balance sheet data are no longer available for that bank as a separate entity; or the bank survives until December 31, 2010 (the end of the sample period). In what follows, the first and second exit scenarios are both considered bank failures unless we explicitly distinguish between the two in our discussion.

Here $\mathbf{X}_{it-1}$ is a vector of lagged bank level and state-level controls, θ_t is a year fixed effect to control for common time effects such as crises, and θ_j is a state fixed effect. State macroeconomic controls (important for appropriately controlling for crises) include *GROWTH* which is annual state personal income growth; *EMPLOYMENT*, defined as the ratio of total employed persons to total state population; and *BUDGET DEFICIT*, defined as the ratio of total state taxes less total state government expenditure to gross state product. Bank-level controls include standard predictors of bankruptcy: *SIZE*, defined as the natural log of total deposits; *INCOME/ASSETS RATIO*, defined as net income to total assets (i.e., return on assets); *CAPITAL RATIO*, defined as total equity capital to total assets; and *NPL*, defined as non-performing loans (+90 days past due) as a percentage of total loans. Recent policy discussions have emphasized the importance of "too big to fail" in determining bank failure and risk taking. Accordingly, we also include the variable *TOO BIG TO FAIL*, defined as a bank's assets at quarter t as a percentage of total banking assets in state j at quarter t. Finally, Brown and Dinc (2011) provide evidence from developing countries that a government is less likely to take over or close a failing bank if the banking system is weak. To capture the possibility of a "too many to fail" effect, we include in our regressions the variable *TOO MANY TO FAIL* defined as the average capital ratio of all other banks in state j.[21]

Panel B in Table 1 presents summary statistics for state controls.[22] State income growth averaged 7% for the entire sample and is lower (6%) for states in the Northeast and Midwest. Employment is around 57% across all states, with the Midwest having the highest employment at 58% and the Northeast having the lowest at 56%. States run persistent government deficits, averaging 2%, with states in the West having the largest deficits of 3% on average. Panel C in Table 1 presents summary statistics for bank-level controls for the full sample as well as for the subsamples of failed banks (outright failure and assistance transactions separately) and all other banks. As expected, failed banks are routinely less profitable, have higher nonperforming loans, and have lower capital ratios than banks that do not fail. Of the failed banks, those receiving FDIC assistance tend to be slightly better performing on these three measures compared with banks failing outright. Banks failing outright tend to be slightly smaller than nonfailing banks ($45 million versus $46 million total deposits). However, banks receiving assistance are significantly larger than both banks failing outright and surviving banks with an average deposit base of approximately $109 million. If we examine state market shares—our proxy for "too big to fail"—we see that banks failing outright control less than 0.3% of state banking assets (almost 0.5% for surviving banks) compared with 2.1% for banks receiving FDIC assistance. This evidence hints at the possibility of a "too big to fail" effect in US bank closure policy; that is, larger banks controlling a larger fraction of banking assets tend to receive assistance rather than having their charters terminated by regulators. Finally, comparing our *TOO MANY TO FAIL* variable across our three categories of banks, the average capital ratio of other banks is slightly lower for failing banks than surviving banks (9.0% versus 9.9%) which is inconsistent with the conjecture that

[19] See: http://www.census.gov/compendia/statab/cats/elections.html. These data are also verified using internet sources such as http://www.ourcampaigns.com/.

[20] See Shumway (2001) for a discussion of forecasting bankruptcy using hazard models.

[21] We also use alternative proxies for TOO BIG TO FAIL, namely, the average percentage of NPL of all other banks in state j and the average income-to-assets ratio of all other banks in state j, and we find similar results (not reported).

[22] The regression sample period is 1976–2010 (inclusive).

Table 2
Election cycles and bank failure.

This table presents the regression estimates of Eq. (1) using the Cox proportional hazard model. The independent variable of interest is *PREELECTION*, which is an indicator variable equal to one for all quarters in the 12 months prior to an election date. Control variables are (1) *INCOME/ASSET RATIO*, which is the ratio of net income to total assets; (2) *SIZE*, which is the natural log of total deposits; (3) *CAPITAL RATIO*, which is the ratio of total equity to total assets; (4) *NPL*, which is nonperforming loans (+90 days past due) as a percentage of total loans; (5) *GROWTH*, which is personal income growth in state *j*; (6) *EMPLOYMENT*, which is the ratio of total employed persons to the population in state *j*; (7) *BUDGET DEFICIT*, which is the ratio of total taxes less government expenditure to gross domestic product in state *j*; (8) *TOO BIG TO FAIL*, which is the ratio of bank *i*'s assets to total banking assets in the state bank *i* is headquartered; and (9) *TOO MANY TO FAIL*, which is the average capital ratio of all other banks in the state bank *i* is headquartered in. Panel A presents results when we classify failure to mean either outright failure or a Federal Deposit Insurance Corporation assistance transaction. Panel B presents results when we classify failure as outright failures only. The reported coefficients are marginal effects. Robust *z*-statistics (clustered standard errors by state) are in parentheses. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: All failures									
PREELECTION	−0.113	−0.468**	−0.448**	−0.151	−0.559*	−0.561*	−0.0751	−0.548	−0.480
	(−1.121)	(−2.376)	(−2.281)	(−1.092)	(−1.916)	(−1.916)	(−0.542)	(−1.519)	(−1.366)
INCOME/ASSET RATIO	−3.080***	−1.594	−1.752	−6.861***	−4.041	−4.056	−2.356*	−1.433	−1.791
	(−3.870)	(−0.486)	(−0.514)	(−5.085)	(−0.827)	(−0.818)	(−1.819)	(−0.186)	(−0.222)
SIZE	−0.249***	−0.114	−0.158***	−0.331***	−0.126	−0.168**	−0.178**	−0.139	−0.212**
	(−4.222)	(−1.634)	(−2.662)	(−5.411)	(−1.459)	(−2.255)	(−2.242)	(−1.359)	(−2.239)
CAPITAL RATIO	−11.30***	−44.93***	−44.88***	−13.46***	−45.25***	−45.29***	−9.699***	−47.76***	−48.13***
	(−5.857)	(−13.37)	(−13.34)	(−4.740)	(−8.297)	(−8.433)	(−6.439)	(−7.488)	(−7.795)
NPL	0.0943***	0.0545***	0.0561***	0.0922***	0.0453***	0.0458***	0.0970***	0.104***	0.104***
	(14.30)	(7.189)	(7.355)	(8.614)	(3.041)	(3.152)	(11.23)	(5.732)	(5.810)
GROWTH		−5.906	−6.562		−19.04**	−19.06**		13.13	13.73
		(−0.922)	(−0.999)		(−2.499)	(−2.525)		(1.442)	(1.599)
EMPLOYMENT		−25.48***	−24.15***		−23.46***	−23.81**		−32.53***	−28.87***
		(−3.445)	(−3.187)		(−2.598)	(−2.560)		(−3.081)	(−2.665)
BUDGET DEFICIT		14.56	10.89		12.99	13.42		46.98*	51.36**
		(0.981)	(0.731)		(0.668)	(0.728)		(1.884)	(2.321)
TOO BIG TO FAIL			3.699***			5.634***			2.858*
			(3.529)			(3.991)			(1.840)
TOO MANY TO FAIL			5.488			−1.833			12.97*
			(0.937)			(−0.259)			(1.720)
Sample	All banks	All banks	All banks	State banks	State banks	State banks	Federal banks	Federal banks	Federal banks
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,165,698	654,553	654,553	819,884	474,540	474,540	345,814	180,013	180,013
Wald *p*-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel B: Outright failures									
PREELECTION	−0.0796	−0.411**	−0.407**	−0.121	−0.532*	−0.536*	−0.0607	−0.416	−0.398
	(−0.721)	(−2.075)	(−2.060)	(−0.797)	(−1.750)	(−1.753)	(−0.408)	(−1.240)	(−1.166)
INCOME/ASSET RATIO	−3.187***	−3.747	−3.838	−7.573***	−5.069	−5.043	0.328	−7.694	−8.224*
	(−5.643)	(−1.068)	(−1.047)	(−4.874)	(−0.958)	(−0.944)	(0.588)	(−1.616)	(−1.710)
SIZE	−0.282***	−0.0494	−0.0102	−0.349***	−0.100	−0.106	−0.251**	0.0260	0.181
	(−5.110)	(−0.577)	(−0.101)	(−5.450)	(−1.183)	(−1.247)	(−2.377)	(0.205)	(0.981)
CAPITAL RATIO	−11.60***	−44.35***	−44.30***	−13.27***	−44.19***	−44.20***	−11.53***	−51.05***	−51.48***
	(−5.676)	(−13.22)	(−13.13)	(−4.610)	(−7.599)	(−7.617)	(−5.744)	(−12.29)	(−12.54)
NPL	0.0955***	0.0551***	0.0550***	0.0935***	0.0466***	0.0465***	0.101***	0.0970***	0.0973***
	(15.23)	(6.711)	(6.833)	(9.502)	(3.135)	(3.199)	(11.10)	(4.321)	(4.580)
GROWTH		−7.675	−7.426		−17.55**	−17.40**		13.14*	13.97
		(−1.217)	(−1.185)		(−2.281)	(−2.293)		(1.663)	(1.638)
EMPLOYMENT		−24.42***	−24.27***		−24.14***	−24.32***		−21.39	−19.23
		(−3.263)	(−3.073)		(−2.598)	(−2.579)		(−1.589)	(−1.297)
BUDGET DEFICIT		10.16	9.870		10.93	11.71		68.37***	63.12**

		(0.618)	(0.622)		(0.583)	(0.660)		(2.765)	(2.473)
TOO BIG TO FAIL			−6.386*			1.106			−21.33
			(−1.904)			(0.498)			(−1.435)
TOO MANY TO FAIL			2.186			−1.953			6.661
			(0.323)			(−0.231)			(1.179)
Sample	All banks	All banks	All banks	State banks	State banks	State banks	Federal banks	Federal banks	Federal banks
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,168,037	655,883	655,883	821,176	475,340	475,340	346,861	180,543	180,543
Wald *p*-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

regulators tend to close banks when the banking system is stronger. For banks receiving assistance, the average capital ratio of other banks is 8.8%, which is lower than the case for outright failure. Consistent with the “too many to fail” hypothesis, when the banking sector is weak, regulators prefer to provide assistance as opposed to closing a bank. So while the evidence on “too many to fail” is somewhat mixed, our summary statistics do suggest the possibility of regulatory forbearance when the banking sector is weak, where forbearance comes in the form of providing assistance to larger and more systematically important institutions.

4.1. Elections and bank failure

The main regression results are reported in Table 2, Panel A. Unless explicitly stated in the tables, all regressions include state fixed effects as well as year fixed effects to control for common state and time factors. Standard errors are robust to heteroskedasticity and clustering at the state level. Regression Models 1–3 are performed on the full sample of banks (i.e., federally chartered and state chartered banks), and Models 4–6 use only state-chartered banks.

A consistent picture emerges: In all specifications, the coefficient on *PREELECTION* is negative, suggesting that bank failure is less likely in the year leading up to a gubernatorial election. As discussed earlier (and illustrated in Fig. 2) controlling for the impact of financial crisis is crucial to our analysis because failures tend to cluster around crises. While some of the impact of crises is accounted for in our time and state fixed effects, it is clear that our results are much stronger when time-varying macroeconomic controls are added into our specification. The preelection effect is significant in all models (2, 3, 5 and 6) where we include macroeconomic controls. The result is also stronger for our sample consisting only of state-chartered banks, which makes sense given that we are studying the impact of gubernatorial elections, so the relevant regulatory jurisdiction is at the state level. To give an indication of the economic significance of this preelection effect, the coefficient for state-chartered banks is approximately −0.56, which translates to a reduction in the probability of failure by about 45% in the year leading up to an election. For the full sample, the *PREELECTION* coefficient of -0.45 translates into a 35% reduction in the probability of failure.[23]

Across all specifications, *INCOME/ASSETS RATIO*, *SIZE*, and *CAPITAL RATIO* are negatively related to bank failure and higher *NPL* increases the likelihood of failure. These results are expected and are in line with previous studies. Our macroeconomic controls also provide results consistent with expectations. Higher state income growth and employment reduces the likelihood of bank failure, with the effect being stronger for and significant for the regression models using the state chartered banks only

[23] In comparison, Brown and Dinc (2005) show for their sample of developing country banks a decrease in the hazard rate by about 70% in the year leading up to an election.

Table 3
Election cycles and bank failure: alternative estimation techniques.

This table presents the regression estimates of Eq. (1) using three alternative estimation techniques: (1) linear probability (Models 1–2); (2) dynamic logit (Models 3–4); and (3) exponential proportional hazard (Models 5–6). The independent variable of interest is *PREELECTION*, which is an indicator variable equal to one for all quarters in the 12 months prior to an election date. Control variables are (1) *INCOME/ASSET RATIO*, which is the ratio of net income to total assets; (2) *SIZE*, which is the natural log of total deposits; (3) *CAPITAL RATIO*, which is the ratio of total equity to total assets; (4) *NPL*, which is nonperforming loans (+90 days past due) as a percentage of total loans; (5) *GROWTH*, which is personal income growth in state *j*; (6) *EMPLOYMENT*, which is the ratio of total employed persons to the population in state *j*; (7) *BUDGET DEFICIT*, which is the ratio of total taxes less government expenditure to gross domestic product in state *j*; (8) *TOO BIG TO FAIL*, which is the ratio of bank *i*'s assets to total banking assets in the state bank *i* is headquartered; and (9) *TOO MANY TO FAIL*, which is the average capital ratio of all other banks in the state bank *i* is headquartered in. Panel A presents results when we classify failure to mean either outright failure or a Federal Deposit Insurance Corporation assistance transaction. Panel B presents results when we classify failure as outright failures only. The reported coefficients are marginal effects. Robust *z*/*t*-statistics (clustered standard errors by state) are in parentheses. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

	Linear probability		Logit		Exponential	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All failures						
PREELECTION	−0.000218**	−0.000201*	−0.754***	−0.916***	−0.501***	−0.581***
	(−2.373)	(−1.842)	(−4.266)	(−4.485)	(−3.057)	(−2.999)
Sample	All banks	State banks	All banks	State banks	All banks	State banks
Control variables 1–9	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Number of observations	658,485	477,425	569,712	388,898	654,553	474,540
F/Wald *p*-value	0.00	0.00	0.00	0.00	0.00	0.00
Panel B: Outright failures						
PREELECTION	−0.000204**	−0.000189*	−0.863***	−1.050***	−0.584***	−0.683***
	(−2.132)	(−1.930)	(−4.269)	(−5.129)	(−3.272)	(−3.326)
Sample	All banks	State banks	All banks	State banks	All banks	State banks
Control variables 1–9	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	658,485	477,425	569,712	388,898	654,553	474,540
F/Wald *p*-value	0.00	0.00	0.00	0.00	0.00	0.00

(Models 5 and 6). This is not surprising given that federally chartered banks are usually larger and more diversified across state boarders making them less sensitive to changes in local economic conditions. To the extent that delaying bank failure involves some fiscal costs, one might expect a positive relation between *BUDGET DEFICIT* and bank failure since states with larger budget deficits are less able to influence the timing of bank failure. Consistent with our conjecture, the variable *BUDGET DEFICIT* is positively related to bank failure, however insignificant. Unlike Brown and Dinc (2011), we find no statistically significant evidence of a "too many to fail" effect in our sample of US banks once we control for the macroeconomic environment and the "too big to fail" effect.

Finally, our *TOO BIG TO FAIL* variable is positive and significant across all specifications. This might at first seem counterintuitive. However, recall for our hazard analysis, we define failure to be either outright failure or FDIC assistance and from our summary statistics, larger banks with a bigger market share are more likely to receive FDIC assistance, which could lead to a positive coefficient on *TOO BIG TO FAIL*. We investigate this explanation for the unintuitive coefficient sign on *TOO BIG TO FAIL* explicitly in additional tests below.

For the remainder of the paper, we indicate which control variables we use in our specifications but do not report the coefficient estimates to preserve space. Moreover, the coefficient estimates for our control variables remain largely unchanged for our various specifications.

4.2. Robustness of the main result

Before moving on to additional analyses, we perform a series of robustness tests on our main finding that bank failure is much less likely in the 12 months prior to an election.

4.2.1. Federally chartered banks and gubernatorial elections

As an initial robustness test, we repeat our analysis for banks that are always chartered federally. This subsample acts as a placebo because the banks' closures should not be affected by statewide gubernatorial elections. The results presented in Models 7–9 of Table 2 show that while the *PREELECTION* variable is negative, it is not significant as expected. In additional tests (not reported), we examine the impact of the presidential election cycle on the likelihood of failure for federally charted banks and do not find a statistically significant result (though the *PREELECTION* coefficient is again negative). This weak result could reflect the fact that voters may attribute less responsibility to the president for failures of smaller banks (which make up the bulk of our failures) because their performance more correlated with local conditions implying the cost of failure remains locally concentrated despite being federally regulated, or could indicate a lack of power in our tests because no variation exists across banks in the timing of presidential elections, or both.

4.2.2. Defining failure as outright failure only

Up to this point, we define bank failure to mean either outright failure, where the failing bank loses its charter and ceases to operate, or assistance transactions, in which the failing bank is given FDIC assistance, restructured, and allowed to continue operating under its original charter. While assistance transactions make up only a relatively small fraction of the recorded failures (593 out of 3,995 recorded failures between 1934 and 2012), to rule out the possibility that our result is driven by our classification, we redefine failure to be only cases in which a bank's charter is revoked and repeat the analysis presented in Table 2, Panel A. The results of this analysis are presented in Table 2, Panel B. Our results remain virtually unchanged. The *PREELECTION* variable is of the same sign and order of magnitude and significance as our previous analysis. All control variables are also very similar to those obtained previously with the exception of the coefficient estimate for our *TOO BIG TO FAIL* variable. We previously found a positive coefficient on *TOO BIG TO FAIL*, implying that banks with a larger market share are more likely to fail, and we argued that this unintuitive result is driven by our classification of assistance transactions as failures (because larger and more prominent banks are more likely to receive assistance). When we redefine failure to mean outright failure only, we find that the coefficient on *TOO BIG TO FAIL* is negative and significant for our full sample of banks, consistent with the view that regulators are less willing to close banks that are systematically important. The *TOO BIG TO FAIL* coefficient is insignificant for our sample containing only state banks, which reflects that fact that state-chartered banks are smaller and have a smaller market share in comparison with federally chartered banks.

4.2.3. Alternative estimation techniques

We reestimate our baseline regressions presented in Table 2 using alternative methods to ensure our results are robust to estimation technique. We employ three alternative estimation techniques: (1) linear probability model, (2) dynamic logit model, and (3) the exponential proportional hazard model used in Brown and Dinc (2005). The linear probability model and logit model are discrete models in which the dependent variable equals one in the quarter a bank fails and zero otherwise. The difference between the Cox and exponential proportional hazard models is that the Cox model leaves the unconditional survival function unspecified, and survival is assumed to follow an exponential distribution in the case of the latter. The results are presented in Table 3. Panel A reestimates our baseline regressions with our original definition of

Table 4

Election cycles and bank failure: crisis versus noncrisis periods.

This table presents the regression estimates of Eq. (1) using the Cox proportional hazard model for crisis periods (Models 1–4) and noncrisis periods (Models 5–6) separately. Crisis periods are all years between 1986 and 1992 (savings and loan crisis) and 2007 and 2010 (Global Financial Crisis). The independent variable of interest is *PREELECTION*, which is an indicator variable equal to one for all quarters in the 12 months prior to an election date. Control variables are (1) *INCOME/ASSET RATIO*, which is the ratio of net income to total assets; (2) *SIZE*, which is the natural log of total deposits; (3) *CAPITAL RATIO*, which is the ratio of total equity to total assets; (4) *NPL*, which is nonperforming loans (+90 days past due) as a percentage of total loans; (5) *GROWTH*, which is personal income growth in state *j*; (6) *EMPLOYMENT*, which is the ratio of total employed persons to the population in state *j*; (7) *BUDGET DEFICIT*, which is the ratio of total taxes less government expenditure to gross domestic product in state *j*; (8) *TOO BIG TO FAIL*, which is the ratio of bank *i*'s assets to total banking assets in the state bank *i* is headquartered; and (9) *TOO MANY TO FAIL*, which is the average capital ratio of all other banks in the state bank *i* is headquartered in. Panel A presents results when we classify failure to mean either outright failure or a Federal Deposit Insurance Corporation assistance transaction. Panel B presents results when we classify failure as outright failures only. The reported coefficients are marginal effects. The reported coefficients are marginal effects. Robust *z*-statistics (clustered standard errors by state) are in parentheses. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

	Crisis periods				Noncrisis periods	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All failures						
PREELECTION	0.167	0.0736	−0.864***	−1.150**	−0.466*	−0.192
	(1.241)	(0.386)	(−3.057)	(−2.490)	(−1.669)	(−0.577)
Sample	All banks	State banks	All banks	State banks	All banks	State banks
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	No	No
Controls 1–4	Yes	Yes	Yes	Yes	Yes	Yes
Controls 5–9	No	No	Yes	Yes	No	No
Number of observations	440,588	307,328	137,792	101,157	725,110	512,556
Wald *p*-value	0.00	0.00	0.00	0.00	0.00	0.00
Panel B: Outright failures						
PREELECTION	0.258*	0.121	−0.733**	−1.085**	−0.459*	−0.189
	(1.724)	(0.579)	(−2.518)	(−2.237)	(−1.652)	(−0.569)
Sample	All banks	State banks	All banks	State banks	All banks	State banks
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	No	No
Controls variables 1–4	Yes	Yes	Yes	Yes	Yes	Yes
Controls variables 5–9	No	No	Yes	Yes	No	No
Number of observations	441,679	307,878	138,121	101,326	726,358	513,298
Wald *p*-value	0.00	0.00	0.00	0.00	0.00	0.00

failure (failure is either outright failure or an assistance transaction), and Panel B reestimates our baseline regressions when failure is defined as outright failure only. In both panels, regression Models 1–2 represent estimates from the linear probability regression, Models 3–4 are obtained from the logit regression, and Models 5–6 are obtained from the exponential hazard. We find consistent results from all three estimation techniques irrespective of how we define failure. First, the estimates from the exponential hazard model are very similar to those obtained from our Cox regression save that the coefficients are slightly larger in magnitude and statistically more significant. Second, the results from the logit model are again similar to those obtained from the Cox regressions. However, the magnitudes of the coefficient estimates are larger and more significant for state banks, implying a reduction in the hazard rate by about 65% in the year leading up to an election. Finally, our linear probability estimates also confirm the reduction in hazard rate in the year before an election. The point estimates are about −0.0002. When compared with the unconditional failure rate of 0.001, these estimates imply a reduction in failure rate by about 20%. While the magnitude of the election year reduction in hazard rate differs across estimation techniques, these results confirm our main result.

4.2.4. Crisis versus noncrisis periods

We previously argued that controlling for financial crisis appropriately is of significant importance for our result because failures tend to be concentrated in crisis and because differing political incentives during crises imply we are much less likely to observe political factors determining bank failure. Differing political incentives during crises arise from lower private costs to a local politician associated with a bank failure, as he can at least in part attribute the cause of the failure to a nationwide problem. Moreover, because bank failure tends to be more severe during a crisis, the scope for politicians to delay regulatory intervention is relatively limited. This discussion implies that our *PREELECTION* indicator should be insignificant for crisis periods when we do not control for variables correlated with financial crises. To investigate this further we split the sample into crisis periods and noncrisis periods and re-estimate Eq. (1) excluding year fixed effects and macroeconomic controls. The results are presented in Table 4. Panel A defines bank failures as either outright failure or assistance transactions, and Panel B defines failure and outright failure only. Models 1–4 represent estimates for the crisis periods and Models 5–6 present estimates for noncrisis periods. Odd-numbered models are for all banks, and even-numbered models use

Table 5
Election cycles and bank failure: robustness.

This table presents the regression estimates of Eq. (1) using the Cox proportional hazard model for various subsamples. The independent variable of interest is *PREELECTION*, which is an indicator variable equal to one for all quarters in the 12 months prior to an election date. Control variables are (1) *INCOME/ASSET RATIO*, which is the ratio of net income to total assets; (2) *SIZE*, which is the natural log of total deposits; (3) *CAPITAL RATIO*, which is the ratio of total equity to total assets; (4) *NPL*, which is nonperforming loans (+90 days past due) as a percentage of total loans; (5) *GROWTH*, which is personal income growth in state *j*; (6) *EMPLOYMENT*, which is the ratio of total employed persons to the population in state *j*; (7) *BUDGET DEFICIT*, which is the ratio of total taxes less government expenditure to gross domestic product in state *j*; (8) *TOO BIG TO FAIL*, which is the ratio of bank *i*'s assets to total banking assets in the state bank *i* is headquartered; (9) *TOO MANY TO FAIL*, which is the average capital ratio of all other banks in the state bank *i* is headquartered in; and (10) *POST-FDICIA*, which is an indicator equal to one for all years after the introduction of the Federal Deposit Insurance Corporation Improvement (FDIC) Act of 1991. We classify failure to mean either outright failure or a Federal Deposit Insurance Corporation assistance transaction. The reported coefficients are marginal effects. Panel A presents results for the full sample of banks. Panel B presents results for state banks only. Robust *z*-statistics (clustered standard errors by state) are in parentheses. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

Variable	Pre-FDICIA (1)	Post-FDICIA (2)	Linear probability (3)	Small banks (4)	Medium banks (5)	Large banks (6)	Excluding Louisiana (7)
Panel A: All banks							
PREELECTION	−0.685**	−0.635**	−0.000565**	−1.451	0.0120	−0.569***	−0.480**
	(−2.517)	(−2.431)	(−2.042)	(−1.322)	(0.0225)	(−2.615)	(−2.404)
PREELECTION × POST-FDICIA			−0.00005				
			(−0.217)				
POST-FDICIA			0.000503*				
			(1.825)				
State fixed effects	Y	Y	Y	Yes	Yes	Yes	Yes
Control variables 1–9	Y	Y	Y	Yes	Yes	Yes	Yes
Year fixed effects	N	N	N	Yes	Yes	Yes	Yes
Number of observations	181,322	473,231	658,485	106,241	226,490	321,822	642,887
Wald *p*-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel B: State banks							
PREELECTION	−0.671**	−0.827***	−0.000516*	−0.648	−0.259	−0.916***	−0.589*
	(−2.090)	(−2.795)	(−1.890)	(−1.165)	(−0.374)	(−2.641)	(−1.936)
PREELECTION × POST-FDICA			−0.00007				
			(−0.321)				
POST-FDICIA			0.000450				
			(1.674)				
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables 1–9	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	No	Yes	Yes	Yes	Yes
Number of observations	126,685	347,855	477,425	86,318	168,260	219,962	464,606
Wald *p*-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00

only state banks. Comparing Models 1 and 2 (crisis period) with models 5 and 6 (noncrisis period) we can see that the coefficient on *PREELECTION* is positive and, in most cases, insignificant for the crisis period, and it remains negative for the noncrisis period (and also significant for the sample of all banks). While the results for the noncrisis period remain negative, they are weaker than previously reported. This is due to the fact that bank failures are clustered in crisis periods, meaning that we lose power in our tests because there is much less variation in our dependent variable in the noncrisis subsample. That is, identification for our results relies on observing bank failure, which is concentrated during crisis periods. To show this is the case, we reintroduce year fixed effects and macroeconomic controls and reestimate Eq. (1) for the crisis-period subsample. Models 3 and 4 show that once we control for common time and time-varying determinants of crises, the coefficient on PREELECTION becomes negative and significant even in crisis periods. The magnitudes of the coefficient estimates are also much larger than those obtained for the full sample reported in Table 2.

4.2.5. Pre- and post-FDICIA

The S&L crisis was in part attributed to regulatory forbearance. That is, regulators allowed banks to continue operating with low or no net worth, which led to excessive risk taking. In the wake of the S&L crisis, two major reforms—the Financial Institutions Reform, Recovery and Enforcement Act (FIRREA) of 1989 and the Federal Insurance Corporation Improvement Act (FDICIA) of 1991—were introduced to reduce the degree of regulatory discretion in bank closure rules. The FDICIA in particular made it more difficult for the FDIC to use its discretion in closing a bank with the introduction of prompt corrective action which mandates progressive penalties against banks as their capital ratios deteriorate. At the extreme, the FDIC is required to put a bank into receivership if it is deemed to be critically undercapitalized (capital ratio less than 2%). To investigate whether the introduction of these major reforms changed political incentives to delay bank failure, we split that sample into pre- and post-FDICIA and reestimate our main regression excluding year fixed effects. The results are presented in Table 5. Panel A covers all banks; Panel B state banks only. Models 1 and 2 show that, for both pre- and post-FDICIA subsamples, the coefficient on *PREELECTION* is negative and significant implying that the introduction of the FDICIA did not eliminate political incentives to delay bank failure. Moreover, the magnitudes of coefficient estimates are also quite similar across the two sub-samples. To see if political incentives to delay bank failure increased or decreased post-FDICIA, we create a post-FDICIA indicator (*POST-FDICIA*) equal one for all years after 1992, interact it with our *PREELECTION* dummy and introduce this interaction term into our regression model. We estimate the model using least squares because interaction terms in a non-linear model such as the Cox model could be biased (Ai and Norton, 2003). The results presented in Model 3 of Table 5 show an insignificant coefficient estimate on *PREELECTION* × *POST-FDICIA*, implying that the introduction of the FDICIA did not change political incentives to delay failure at all. This result highlights the fact that the introduction of new rules designed to govern regulators' discretion cannot work effectively without the support of the politicians. Our example of the Cleveland thrift AmTrust, discussed in the Introduction, is a case in point. Even though prompt corrective action mandated the FDIC to step in and seize the assets of AmTrust when it became critically undercapitalized, political interference meant that the FDIC could not carry out this mandate.

4.2.6. Bank size

To investigate whether politicians treat banks of differing sizes differently, we split our sample into size terciles and reestimate our main regression to see if our *PREELECTION* coefficient varies across bank size. The results from this analysis are presented in Models 4–6 in Table 5. While the coefficient estimates are negative across the bank size terciles, the election year reduction in hazard rate is strongest and significant for large banks. These findings suggest that the political gain from delaying the failure of a large bank is greatest, which makes sense because the failure of a relatively large bank leads to larger losses to the voting public and, therefore, has a greater bearing on an election outcome.

4.2.7. Excluding Louisiana

One of the benefits of studying the impact of the electoral cycle on bank failure in a US setting is that election timing is exogenous. This is true for all states except for Louisiana, where elections do not take place in the first week of November, but vary from one election to the next. To ensure our results are robust, we exclude Louisiana from our sample and reestimate our main regressions. The results presented in Model 7 of Table 5 show that our finding remains unchanged when we exclude Louisiana from our sample.

In sum, the results in this section provide evidence supporting the view that electoral concerns drive politicians to take costly action to delay bank failure in an election year.

5. Electoral competition, political control and bank failure

In this section we investigate whether electoral competition or political control of the state legislature, or both, attenuates or exacerbates the election year reduction in hazard rate.

The costs associated with bank failure, such as losses to uninsured depositors and shareholders, bank job losses, small borrowers with no alternative financing options and the potential reductions in local economic activity, are likely to be concentrated in the state where the bank operates. Accordingly, political support for the incumbent party in that state could decrease because of these costs. Moreover, if the incumbent and opposition parties have similar levels of voter support, the impact of the voter backlash is likely to be stronger. Political (electoral) competition, therefore, increases the benefit to politicians from delaying bank failure, and we might expect that elections which are closely contested exacerbate the preelection reduction in the hazard rate, ceteris paribus. To measure

Table 6
Electoral competition, political control, and electoral incentives to delay failure.

This table presents the regression estimates of Eq. (1) using the linear probability model. The independent variable of interest is *PREELECTION*, which is an indicator variable equal to one for all quarters in the 12 months prior to an election date. This table includes additional variables to examine the impact of electoral competition and political control. The measure of electoral competition used in this table is a party neutral measure defined as the difference between the winner's percentage vote share and the runner-up's percentage vote share. We construct an indicator variable *HIGH VICTORY MARGIN* for above median values of victory margin (i.e., least competitive elections) interacted with the *PREELECTION* indicator. We construct a variable *CONTROL OF BOTH HOUSES*, which is an indicator equal to one if the governor's party has control (i.e. holds the majority of seats) of both the lower and upper house simultaneously. Of interest is the interaction between this variable with the *PREELECTION* indicator. Control variables are (1) *INCOME/ASSET RATIO*, which is the ratio of net income to total assets; (2) *SIZE*, which is the natural log of total deposits; (3) *CAPITAL RATIO*, which is the ratio of total equity to total assets; (4) *NPL*, which is nonperforming loans (+90 days past due) as a percentage of total loans; (5) *GROWTH*, which is personal income growth in state *j*; (6) *EMPLOYMENT*, which is the ratio of total employed persons to the population in state *j*; (7) *BUDGET DEFICIT*, which is the ratio of total taxes less government expenditure to gross domestic product in state *j*; (8) *TOO BIG TO FAIL*, which is the ratio of bank *i*'s assets to total banking assets in the state bank *i* is headquartered; (9) *TOO MANY TO FAIL*, which is the average capital ratio of all other banks in the state bank *i* is headquartered in; and (10) *DEMOCRAT GOVERNOR*, which is an indicator equal to one if the governor is from the Democratic party. We classify failure to mean either outright failure or a Federal Deposit Insurance Corporation assistance transaction. The reported coefficients are marginal effects. Robust *z*-statistics (clustered standard errors by state) are in parentheses. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PREELECTION	−0.000294***	−0.000267***	−0.000186*	−0.000247	0.0000584	0.000104	−0.0000461	0.0000010	0.0000601	0.0000410
	(−3.166)	(−2.703)	(−1.929)	(−1.645)	(0.522)	(0.839)	(−0.389)	(0.00751)	(0.461)	(0.265)
PREELECTION × *HIGH VICTORY MARGIN*	0.000173	0.000153	0.000102	0.000136			0.000197*	0.000203	0.000114	0.000147
	(1.570)	(1.057)	(0.985)	(1.054)			(1.833)	(1.488)	(1.069)	(1.129)
PREELECTION × *CONTROL OF BOTH HOUSES*					−0.000389*	−0.000425**	−0.000388*	−0.000429**	−0.000373*	−0.000427**
					(−1.876)	(−2.138)	(−1.917)	(−2.205)	(−1.839)	(−2.133)
CONTROL OF BOTH HOUSES					0.000397***	0.000417***	0.000400***	0.000421***	0.000404***	0.000427***
					(2.826)	(2.807)	(2.836)	(2.816)	(3.003)	(2.974)
DEMOCRAT GOVERNOR			−0.000099	−0.000108					−0.000130	−0.000142
			(−0.485)	(−0.552)					(−0.661)	(−0.751)
PREELECTION × *DEMOCRAT GOVERNOR*			−0.000168	−0.000031					−0.000136	−0.0000003
			(−0.952)	(−0.158)					(−0.818)	(−0.00165)
Sample	All banks	State banks	All banks	State banks	All banks	State banks	All banks	State banks	All banks	State banks
Control variables 1–9	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	658,485	477,425	646,971	468,661	658,485	477,425	646,971	468,661	646,971	468,661
F-statistic *p*-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

the extent of political competition, we construct a variable similar to Dinc and Gupta (2011). In particular, our party neutral measure, *VICTORY MARGIN*, is defined as the difference between the winning candidate's vote share and the second place candidate's vote share (smaller values are associated with stronger political competition).[24] The summary statistics in Panel B of Table 1 show that gubernatorial elections post-1976 are on average not very competitive, with the average victory margin of 17%. However, significant variation exists in this measure, with a standard deviation 14%. Southern states are marginally less competitive with an average victory margin of 18%.

In our empirical application, we construct a dummy variable: *HIGH VICTORY MARGIN* (i.e., low competition) equal to one for elections with above median victory margins.[25] We are interested in the coefficient estimate on the interaction term between this dummy variable and our *PREELECTION* variable. We do not include the level effect of *HIGH VICTORY MARGIN* because we observe electoral outcomes only in election years. This approach has been used by other researchers studying electoral competition (e.g., Julio and Yook, 2012). From our previous discussion, we expect that if higher competition exacerbates the political incentive to delay bank failure then the coefficient on the interaction between *PREELECTION* and *HIGH VICTORY MARGIN* will be positive. Again, because interaction terms in nonlinear models could be biased (Ai and Norton, 2003) we estimate our models for this analysis using least squares. The results are presented in Models 1–4 of Table 6. Regression Models 3 and 4 also include the control variable *DEMOCRAT GOVERNOR*, which is equal to one when the governor is a Democrat, as well as the interaction between *DEMOCRAT GOVERNOR* and *PREELECTION*.

This analysis shows that with or without the inclusion of our *DEMOCRAT GOVERNOR* control the coefficient estimate on *PREELECTION* × *HIGH VICTORY MARGIN* is positive as expected. However, the relation is statistically insignificant.

Our insignificant electoral competition results are, on face value, surprising. However, while we argued earlier that the benefit of delay to the politician is increasing with political competition, at least two reasons can be cited as to why we might observe a larger reduction in the preelection hazard rate for less competitive elections, meaning the net effect is an empirical matter.

First, the costs of delay are also likely increasing with political competition. Private costs associated with delay are those incurred in the event such corrupt behavior is detected. Numerous studies look at the cost of corruption charges on politicians' subsequent electoral performance (e.g., Alford, Teeters, Ward, and Wilson, 1994; Jacobsen and Dimock, 1994; Peters and Welch, 1980) and politicians' decisions to retire (e.g., Groseclose and Krehbiel, 1994; Hall and Van Houweling, 1995).[26] These costs can be increasing with competition for several reasons. First, political competition increases the likelihood that any corrupt behavior by incumbent politicians is detected because opposition parties are either more numbered or more incentivized, or both, to monitor the actions of the incumbent leading up to an election. Rising private costs to the politician reduces their incentive to delay bank failure. More broadly, this idea is related to the large debate on whether political competition reduces corruption. In general, this literature suggests that competitive elections serve as a disciplining role against corruption.[27]

Second, the transactions costs associated with decision making influence the costs of delaying bank failure. As political competition increases, more actors are involved in the decision-making process and, as more decision points must be crossed, transactions costs increase. These kinds of transactions costs are often referred to as veto points (Tsebelis, 1995, 2002). More veto points make policy commitments more credible (i.e., irreversible), but they also make them more costly and time-consuming to implement and change. To the extent that bank closure policy is more credible in states-years with more veto players, we expect political competition to reduce the incumbent politician's ability to make discretionary policy decisions such as delaying bank failure. Evidence of the important role of veto players in economic policy making can be found in Keefer and Stasavage (2003), who study the role of veto players on the degree of central bank independence and subsequent credibility (i.e., effectiveness) of monetary policy. The authors show that rising veto players enhances central bank independence by reducing the time inconsistency of monetary policy and also reduces central bank governor turnover.[28]

To investigate the role of political control, we construct a variable *CONTROL OF BOTH HOUSES*, which is an indicator that equals one if the governor's party has control (holds the majority of seats) of both the lower and upper house

[24] In an earlier version of this paper, we used the party neutral measure of political competition developed in Besley, Persson, and Sturm (2010). Their measure uses data originating from the work of Ansolabehere and Snyder (2002), who collected election results for a broad set of directly elected state executive offices. These elections range from US representatives, over the governorship, to down-ballot officers, such as Lieutenant Governor, Secretary of State, Attorney General, and so on. We thank James Snyder for generously providing us with an updated version of this data which was used in our earlier work.

[25] The average victory margin for elections with above (below) median victory margins is 24.91% (2.05%) with a minimum value of 11.36% (21.1%) and a maximum of 5.2% (0%).

[26] Three of the five Keating Five senators retired following an investigation into their interference in the FDIC investigation of Charles Keating, the chairman of Lincoln S&L: Cranston, DeConcini, Riegle Jr.

[27] See studies by Barro (1973), Rose-Ackerman (1978), Ferejohn (1986), Shleifer and Vishny (1993), Aidt (2003), and Alt and Lassen (2003) for theoretical contributions. Empirical contributions also support the idea that political competition reduces corruption. See for example, Kunicova and Rose-Ackerman (2005), Lederman, Loayza, and Soares (2005), Tavits (2007), and Nyblade and Reed (2008).

[28] Their measure of veto players is based on whether the executive and legislative chamber(s) are controlled by different parties in presidential systems and on the number of parties in the government coalition for parliamentary systems. The indicator rises with the number of veto players (depending upon the number of legislative chambers) and falls when the veto points are occupied by the same political party (depending on whether majorities are multiparty coalitions). The index is then modified to take account of the fact that certain electoral rules (closed list versus open list) affect the cohesiveness of governing coalitions.

simultaneously (i.e. complete control of the state legislature) and interact it with our *PRE-ELECTION* variable.[29] The results are presented in Models 5–10 of Table 6. Models 5 and 6 do not control for any additional political factors, Models 7 and 8 include our proxy for electoral competition, and Models 9 and 10 include additional variables to capture the party affiliation of the governor. Across all specifications, the coefficient estimate for *PREELECTION* × *CONTROL OF BOTH HOUSES* is negative and significant. In addition, the coefficient estimates on our *PREELECTION* variable not only becomes insignificant but also reverses in sign. These results imply that all of the election year fall in hazard rate can be explained by states where the incumbent governor has complete control of the state legislature. Moreover, the coefficient on the level effect of *CONTROL OF BOTH HOUSES* is positive, indicating that political control seems to allow politicians to substitute lower bank failure in the 12 months prior to an election for higher bank failure in other periods. The economic significance of political control is large. For our most complete regression model (Model 10) the coefficient on *PREELECTION* × *CONTROL OF BOTH HOUSES* is −0.00043. When compared with the unconditional failure rate of 0.001, this number implies that in election years when a governor has control of both the upper and lower houses, there is a reduction in the bank hazard rate by about 43%, which is more than double the reduction the election year hazard rate previously estimated using a linear probability model reported in Table 3. All other political variables remain insignificant regardless of specification.

These results are consistent with the view that political control tends to lead to more corrupt behavior. The mechanism through which this occurs is less clear, however. The disciplining role of political competition can come from rising private costs to the politician in the event corrupt behavior is detected, rising costs associated with discretionary policy changes with more veto players when there is a balance of power, or some combination of the two. These results are also consistent with recent studies showing that political competition improves economic outcomes and is therefore welfare enhancing (see, for example, Polo, 1998; Svensson, 1998; Besley, Persson and Sturm, 2010). To the extent that political competition enhances competition in the banking industry whereby bank failure is an efficient mechanism to ensure that poor performing banks exit, thus increasing the overall health of the local bank industry, one might expect that political control is negatively correlated with bank failure. Our finding is also in line with arguments made by Haber (2004, 2008), who demonstrates that political competition led to the breakdown of segmented banking monopolies and increased bank competition in the US over the last century.[30]

6. Conclusion

We exploit exogenous variation in the timing of gubernatorial elections to study political incentives to delay bank failure around elections. In particular, we examine whether bank failure is less likely in the 12 months leading up to an election. Using a hazard analysis, our results show that bank failure is about 45% less likely in the year leading up to an election.

We also investigate if political control exacerbates or attenuates the election year fall in the hazard rate and find strong support that political control can explain the election year fall in the hazard rate. That is, we show that election years in which the governor's party has control of both the upper and lower house of the state legislature (i.e., complete political control) can explain all of the average election year fall in hazard rate. In particular, our estimates suggest that the election year reduction in hazard rate more than doubles for banks in states where the governor has complete control heading into an election.

Our results demonstrate that even developed democracies such as the US are not immune from the incentive problems faced by politicians. The implications for public policy are twofold. First, bank regulatory and closure rules need to account for the perverse incentives of politicians. Similar to central bank independence, the results here suggest that bank regulators could also require the same type of independence to effectively carry out their role. Second, political competition appears to discipline politicians by increasing the costs associated with interfering in bank closure policy. This finding illustrates the importance of political institutions that foster political competition

[29] We also examine situations in which the governor controls the lower house but not the upper house, and the upper house but not the lower house. In these analyses, which are not reported, we do not find any evidence to suggest that partial control of the state legislature has an impact on the election year fall in bank hazard rate. In alternative specifications, we show that an increasing margin of the governor's party in the lower or upper house increases the magnitude of the election year fall in hazard rate, however, the results are not significant in most cases so they are not reported.

[30] Another possible explanation for a negative relation between political competition and incentives delay could exist due to political patronage. That rent-seeking politicians tend to make decisions to reward supporters (see Cox and McCubbins, 1986; Persson and Tabbellini, 2002). Ansolabehere and Snyder (2007) argue that politicians could target both areas that support them and politically competitive areas. Previous evidence of political patronage include Ansolabehere and Snyder (2007), who show governing parties provide more public funds to regions that support them, and Dinc and Gupta (2011), who show that politicians do not privatize firms located in the state from which a minister with jurisdiction over that firm is elected. In an earlier version of the paper, we also calculate a measure of electoral competition to capture the partisan support for the incumbent to investigate the role of political patronage. In particular, we calculate the Democrat vote share across all gubernatorial candidates in any given election and interact it with the party affiliation of the incumbent governor in an election year. We find some weak evidence that patronage could play a role. Democrat (Republican) states with Democrat (Republican) governors tend to have an even larger reduction in the election year hazard rate. However, because the exact role of political patronage is difficult to disentangle from political control [i.e. states which are heavily Democrat (Republican) with a Democrat (Republican) governor tend also to be the ones in which the governor's party has more control], and because the analysis relies on using triple interactions that could be biased, we do not report the results in this version of the paper. We can provide these results upon request.

and reduce the degree of political control any single individual (or group) has in an economy.

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