Banks, Political Capital, and Growth

Thomas Lambert
Rotterdam School of Management, Erasmus University

Wolf Wagner
Rotterdam School of Management, Erasmus University and CEPR

Eden Quxian Zhang
Monash Business School, Monash University

We show that politically connected banks influence economic activity. We exploit shocks to individual banks' political capital following close U.S. congressional elections. We find that regional output growth increases when banks active in the region experience an average positive shock to their political capital. The effect is economically large, but temporary, and is due to lower restructuring in the economy, not increased productivity. We show that eased lending conditions (especially for riskier firms) can account for the growth effect. Our analysis is a first attempt to directly link the politics and finance literature with the finance and growth literature. (*JEL*, D72, E65, G21, G28, O43, O51).

A large body of evidence belonging to the "finance and growth" literature shows that banking sector development facilitates economic growth, at least in part, by fostering an efficient allocation of capital across

investment opportunities.[1] Recent evidence in the "politics and finance" literature shows that rent-seeking pressures and political power of the banking sector create distortions in the allocation of capital in an economy.[2] Although the politics and finance literature is intimately related to the finance and growth literature, we still have a rather limited understanding of the extent to which these two important literatures interact.[3]

This paper addresses two fundamental questions directly linking both literatures: Does political capital held by banks influence aggregate economic outcomes? And, if so, how? These are difficult questions to answer empirically. Political capital is clearly endogenous since banks choose to invest in it. In particular, banks decide whether or not to seek connections with politicians and, if so, to which ones. Banks' investment in political capital also may be the result of changes in economic conditions, rather than a cause thereof. And even if exogenous changes to political capital are cleanly identified, it is not clear that any resultant change in the behavior of individual banks is sufficiently consequential to produce aggregate effects for the economy.

To assess the relevance of banks' political capital for economic activity, we devise a "micro-to-macro" strategy that starts with isolating exogenous changes in individual banks' political connectedness. We focus on campaign contributions to candidates in close U.S. congressional elections in the 2002–2014 cycles. Specifically, we calculate shocks to net connections, defined as the number of candidates a bank contributes to who win a close election minus the number of supported candidates who lose a close election. The primary identifying assumption is that close election outcomes at the time of banks' donations are plausibly exogenous. We confirm this assumption that banks are not able to predict close-election winners with significant accuracy in the data. Then, to study the extent to which politically connected banks affect economic activity, we exploit the regional distribution of national banks operating across the country; hence, shocks to individual banks translate into

---

[1] Efficiently allocating scarce resources to their greatest value use has been associated with economic growth at least since Bagehot (1873), who argued that the successful allocation of capital to "immense works" during the Industrial Revolution in England contributed to the country's rapid economic expansion. Schumpeter (1912) linked economic growth to the ability of banks to identify and fund the entrepreneurs with the greatest chances of success.

[2] Several studies show how the relative political strength of interest groups—emerging as a result of the distribution of resources in an economy—can shape banking sector development and access to credit in the United States (Benmelech and Moskowitz 2010; Rajan and Ramcharan 2011). These studies are consistent with the idea that underlies the private interest group theory of regulation and that is associated with work of Olson (1965), Stigler (1971), Peltzman (1976), and Becker (1983), namely, that interest groups or constituencies can use their political power to preserve or extract rents at the expense of others.

[3] A cross-country literature argues that the politics of financial development helps explain how financial development leads to long-term growth (Rajan and Zingales 2003; Pagano and Volpin 2005; Perotti and von Thadden 2006; Degryse, Lambert, and Schwienbacher 2018).

regional shocks. That is, we develop an indicator of net connections based on the predetermined market shares of banks in each region for each election cycle. Using a difference-in-differences framework, we can compare regions that experience a positive shock to their banks' political capital with regions that experience a negative shock, and causally estimate the effect on regional economic activity.

We begin by studying output growth in metropolitan statistical areas (MSAs). Our findings show that "positive" shocks to banks' political capital are associated with higher subsequent regional output growth. Our estimates imply large consequences for the U.S. economy: a one-standard-deviation increase in political capital leads to 0.12-standard-deviation increase in the annual growth rate. This effect is highly statistically significant and holds after performing a battery of robustness and placebo tests. Importantly, net connections to politicians who serve on powerful congressional committees (responsible for banking and finance matters) following the election drive a significant part of the growth effect.[4] However, we also find that the growth effect is not a permanent one: it rapidly vanishes following the close election.

We thus examine the source of the temporary growth effect. We observe that MSAs where banks experience positive political capital shocks see fewer establishments exiting the market, whereas the effect on entry (through forming new establishments) is limited. Our findings on employment accordingly show that positive shocks to banks' political capital are associated with more (less) job creation (destruction) by incumbents. However, we do not observe effects on job creation by new entrants, nor on job reallocation. Consistent with the latter evidence, we also do not find significant effects on wage and patent growth in MSAs where banks receive a positive shock to their political capital. Together, these findings indicate that the (temporary) output growth is caused by less restructuring in the real economy, rather than by productivity improvements. This interpretation is consistent with the notion that political connections are used to support incumbent firms, rather than encouraging the process of creative destruction (Akcigit, Baslandze, and Lotti 2018; Faccio and McConnell 2020).

We then explore the channel through which banks' investment in political capital may affect economic activity. The finance and growth

---

[4] This is an important result illuminating the mechanism since politicians serving on the relevant congressional committees hold the most power to help banks. Both the Senate and the House of Representatives have committees responsible for banking and finance matters, committees that have a great deal of discretion over the legislative process as banking bills must first be introduced to and then pass these committees before being considered for a general vote. Campaign contributions are especially targeted toward politicians sitting on a committee (see Kroszner and Stratmann [1998], who show how committee-based congressional organization explains campaign contribution patterns in the banking sector). For example, Duchin and Sosyura (2012) find compelling evidence that political ties between banks and the members of the House Committee on Financial Services sway the allocation of capital under the Capital Purchase Program initiated in October 2008 and closed in December 2009.

literature argues that the banking sector facilitates growth by improving the efficiency with which capital is allocated, and also increasing the quantity of capital invested (King and Levine 1993). At the same time, the politics and finance literature shows that politically connected banks take up risky strategies, often associated with short-term benefits but adverse consequences in the longer run (Igan, Mishra, and Tressel 2012; Kostovetsky 2015). One explanation for this behavior is that connected banks can expect favorable treatment that partially insulates them from the negative consequences of their risk-taking (moral hazard).[5] Such behavior is likely to distort the allocation of capital, rather than improving it. While our analysis of regional economic activity is not able to precisely identify this "favorable treatment" channel documented in the literature, the results are consistent with it as we find that (1) output growth is only temporary, (2) incumbent firms mainly gain from it, and (3) it does not translate into higher productivity.

Next, we turn to directly analyzing the behavior of banks. We first evaluate whether banks respond to political capital shocks by increasing new corporate lending at the regional level. We look at the total volume of small business loans originated by the Community Reinvestment Act (CRA) reporting banks. We find that MSAs where banks experience positive shocks to their political capital see a significant increase in the quantity of lending to small businesses. We then examine the behavior of banks using data on individual lending decisions in the syndicated loan market, which is the most important source of corporate financing in the United States (Ivashina 2009). The analysis reveals clear evidence that banks experiencing a positive shock to their political capital ease corporate lending conditions by increasing lending volumes (consistent with the CRA quantities results) as well as lowering interest rates. Exploring heterogeneity in borrower characteristics, we also find that the effect is more pronounced for riskier borrowers. Again, these results on corporate lending provide evidence consistent with politically connected banks taking more risks under the "favorable treatment" channel.

Our paper belongs to the finance and growth literature (for a survey, see Popov 2018). A significant part of this literature has exploited within-country heterogeneity deriving from the implementation of policies that promote banking sector development. In the United States, Jayaratne and Strahan (1996) find that state banking deregulation is associated with a 0.51–1.19 percentage point (pp) increase in real per capita state growth. Huang (2008) examines changes in growth rates for contiguous

---

[5] Examples of favorable treatment for connected banks are preferential bailouts (Duchin and Sosyura 2012; Blau, Brough, and Thomas 2013), beneficial regulation (Igan and Mishra 2014), regulatory forbearance (Kang, Lowery, and Wardlaw 2015; Heng, Zhang, and Zhong 2021), fewer supervisory sanctions (Adams 2017; Lambert 2019), and reduced supervisory effectiveness (Lim, Hagendorff, and Seth 2019).

counties across state borders and finds a growth effect in only a smaller subset of deregulations. Dehejia and Lleras-Muney (2007) show that the expansion of bank branching in the early 20th century United States spurred growth in manufacturing. Further research explores how banking sector development, entrepreneurship, creative destruction, and economic growth all tie together. Black and Strahan (2002) and Cetorelli and Strahan (2006) document that state banking deregulation, by enhancing competition, fosters new business creation. Kerr and Nanda (2009) qualify these findings by looking at the rates of business churning. They show that U.S. banking deregulation brings about more entry by new firms but also higher levels of exit among new entrants. Gropp et al. (2020) find that MSAs where supervisory forbearance on distressed banks was higher during the recent banking crisis experience lower productivity growth after the crisis with less establishment entry and employment. Other studies show that gains in new business creation may come from a reduced cost of credit in the United States (Rice and Strahan 2010; Erel 2011; Keil and Müller 2020). These results complement Bertrand, Schoar, and Thesmar (2004), who show that banking deregulation in France led to more entry in bank-dependent sectors of production, and Guiso, Sapienza, and Zingales (2004), who report that local financial development in Italy promotes entry of new firms, increases competition, and boosts economic growth. We extend this literature by showing that banks' political connectedness creates distortions in corporate lending, and through this affect creative destruction and growth.[6] Our findings thus have some parallels in the literature assessing the consequences of zombie lending for credit (mis)allocation and real economic activity (Caballero, Hoshi, and Kashyap 2008; Giannetti and Simonov 2013; Acharya et al. 2019; Schivardi, Sette, and Tabellini 2020).

Our paper also contributes to the literature on the politics of finance (for an overview of this literature, see Lambert and Volpin 2018). Ample evidence in this literature—predominantly in the context of developing economies—reveals that politically connected firms enjoy preferential access to and better terms of credit (for evidence in Pakistan, see Khwaja and Mian 2005; for evidence in Brazil, see Claessens, Feijen, and Laeven 2006; for evidence in China, see Li et al. 2008; for evidence in Mexico, see Agarwal et al. 2016).[7] In Italy, Sapienza (2004) finds that the stronger the political party in the area where the firm is borrowing, the lower the interest rates charged by state-owned banks. In France, Pérignon and

---

[6] Our empirical methodology also differs from the bulk of the literature: even though we examine aggregate economic outcomes (at the regional level), the variation originates from (exogenous) shocks to individual banks.

[7] These results bear some similarity to the literature on political lending cycles that documents that credit is used politically to secure votes (see Dinç 2005; Cole 2009; Carvalho 2014; Englmaier and Stowasser 2017; Bircan and Saka 2021; Koetter and Popov 2021).

Vallée (2017) show that banks designed financial securities (structured loans) enhancing incumbent politicians' likelihood of reelection. Other studies closer to our paper examine how politics affects loan renegotiations (Agarwal et al. 2018), retail lending (Chavaz and Rose 2019), consumer credit (Akey et al. 2018; Akey, Heimer, and Lewellen 2021), and small business loan subsidies (Raina and Xu 2020) in the United States. We also uncover distortionary effects of politics on corporate lending (syndicated loans). Relative to these papers, our study additionally shows that political connections have direct, non-negligible consequences for aggregate economic outcomes.[8] Our paper thus represents a first attempt (to our knowledge) to bring the "micro" literature on the politics of finance together with the "macro" literature on finance and growth.

1. Identification and Empirical Approach

Estimating the effect of banks' political capital on ex post aggregate economic outcomes is a challenging task. First, banks endogenously determine their political connectedness; that is, they choose (whom) to support (as) politicians running for office. Second, election outcomes are often predictable, making it difficult to isolate the effect of political capital shocks. Third, election outcomes can be driven by changes in economic activity and not the other way around. Besides these identification-related concerns, another challenge in our research question is to map shocks that occur at the level of individual banks to regions.

Similar to Akey and Lewellen (2017) and Brogaard, Gerasimova, and Rohrer (2021), among others, we address these challenges by exploiting close U.S. congressional elections in order to obtain exogenous variation in a bank's political capital. Specifically, we consider election outcomes for which the ex post margin of victory is less than 5%. The identifying assumption is that close election outcomes are plausibly exogenous at the time banks donate to candidates. Although we cannot directly test this assumption, below we provide supporting evidence that election outcomes are largely unpredictable in our sample of close elections. In addition, we only focus on the subset of banks that contribute to the campaign of candidates in close races. This allows us to effectively control for the selection of politically active banks. Finally, a useful feature of close elections is that they are generally decided on the election day,

---

[8] This is arguably an important result from a policy perspective. Indeed, it is far from obvious whether shocks to individual banks also produce aggregate changes. Even though shocks may matter at individual banks (as shown in previous studies), these effects may cancel out in aggregate (as some banks receive positive and other negative shocks). Shocks to individual banks may also purely cause a reallocation of credit to or from unaffected banks, again without any aggregate effect. And, finally, even if aggregate bank lending responds this may not affect production and output if firms substitute away to other forms of funding or if (marginal) lending is not productive.

which allows us to cleanly examine the timing of potential changes in economic conditions in response to changes in political capital.

Our analysis focuses on Bank Holding Companies (BHCs).[9] The Federal Reserve regulates and supervises BHCs, which are large corporations controlling their subsidiaries operating across several regions in the country. Political connections across regions are also predominantly established at the BHC level. We measure shocks to a BHC's political capital in a specific election cycle as follows:

$$NetCloseWins_{bc} = CloseWins_{bc} - CloseLosses_{bc},$$

where $CloseWins_{bc}$ is the number of winning candidates in close elections that bank b contributed to in election cycle c and $CloseLosses_{bc}$ is the corresponding number of losing candidates.[10] Consider, for instance, CIT Group Inc. (a BHC headquartered in New Jersey) that donated to 4 winners in close elections and 3 losers in close elections during the 2014 election cycle, then $NetCloseWins_{bc}$ is $4 - 3 = 1$, and captures the CIT Group Inc.'s overall political capital gain in close elections during the 2014 cycle.

Table 1 presents summary statistics for the key variables. Panel A reports that the average value of $NetCloseWins_{bc}$ varies widely across election cycles. For example, this variable is negative with -0.524 in 2008, whereas it is as high as 1.505 in 2002. The average value across all election cycles is 0.754, and thus larger than zero. At first sight this may indicate that banks can partially predict close election outcomes. However, Eggers et al. (2015) have shown that imbalances around election thresholds arise by chance and do not necessarily invalidate the identifying assumption.[11] Generally, the observed variations in $NetCloseWins_{bc}$ are consistent with for instance Akey (2015), in that the size and sign of $NetCloseWins_{bc}$ varies by election cycle, reflecting randomness of close elections outcomes. Complete predictive power is also clearly inconsistent with the data as banks would then only donate to winning candidates. Furthermore, in panel B of Table 1, we compare (observable) characteristics of banks that contribute to close-election winners with the ones that contribute to close-election losers. We do not find statistically significant differences between both groups, further validating the identifying assumption.

Panel C of Table 1 provides information about BHCs in our sample. These BHCs tend to be large national banks, running hundreds of

---

[9] We use "BHC" and "bank" interchangeably in the text.

[10] In case a bank contributed to both candidates in a close election, the net shock is set to zero (such cases represent about 6% of contributions in our sample).

[11] Ex post imbalances may also arise when one party performs unexpectedly well in an election cycle and banks have contributed more (or less) to candidates of that party.

Table 1
Statistics for the key variables

A. Bank-level political variables

	N	Mean	SD	p25	p50	p75
Across election cycles						
NetCloseWins	435	0.754	1.953	0.000	1.000	1.000
CloseWins	435	2.970	3.120	1.000	2.000	4.000
CloseLosses	435	2.209	2.482	0.000	1.000	3.000
By election cycles						
NetCloseWins in 2002	97	1.505	2.006	1.000	1.000	2.000
. . . 2004	53	1.132	1.569	0.000	1.000	2.000
. . . 2006	67	0.299	1.596	-1.000	0.000	1.000
. . . 2008	42	−0.524	1.486	-2.000	0.000	1.000
. . . 2010	58	0.828	2.129	-1.000	1.000	2.000
. . . 2012	58	0.983	2.048	0.000	1.000	2.000
. . . 2014	60	0.317	1.953	-1.000	0.000	1.000

B. Tests of differences between close-election winners and close-election losers

	Winners	Losers	Winners − Losers	*t*-statistic
Size	16.70	16.79	−0.08	(−0.59)
ROA	8.73	10.12	−1.39	(−1.42)
Liquidity	34.70	36.21	−1.50	(−1.45)
NPL	3.55	3.58	−0.03	(−0.22)
Tier1	66.53	80.80	−14.27	(−1.15)

C. Characteristics of sample BHCs

	N	Mean	SD	p25	p50	p75
Number of branches	435	573.699	1140.475	2.000	94.000	535.000
Number of states covered	435	6.389	7.497	1.000	4.000	8.000
Number of MSAs covered	435	47.400	83.899	1.000	12.000	51.000
Deposit share in the HQ state	434	0.666	0.334	0.371	0.725	1.000
Deposit share in the HQ MSA	434	0.523	0.383	0.166	0.414	1.000

D. Characteristics of candidates supported by sample BHCs

	N	Mean	SD	p25	p50	p75
Number of candidates supported	435	56.724	64.862	7.000	30.000	89.000
Number of close election candidates supported	435	5.202	5.362	1.000	3.000	8.000
Number of banking committee members supported	435	17.561	19.856	2.000	9.000	28.000
Number of states covered by supported candidates	435	18.287	16.134	3.000	12.000	34.000
%candidates in the HQ state	435	0.297	0.303	0.057	0.167	0.500
%candidates in the states with branches	435	0.516	0.376	0.070	0.584	0.857

E. MSA-level political variables

	N	Mean	SD	p25	p50	p75
Across election cycles						
NetCloseWins	2,631	0.632	0.945	−0.013	0.407	1.143
By election cycles						
NetCloseWins in 2002	376	1.390	1.126	0.488	1.147	2.129
. . . 2004	371	0.515	0.431	0.174	0.428	0.817
. . . 2006	378	0.869	0.648	0.355	0.831	1.325
. . . 2008	375	−0.252	0.334	−0.426	−0.227	0.000

Table 1

Continued

E. MSA-level political variables

	N	Mean	SD	p25	p50	p75
... 2010	378	0.540	0.740	0.010	0.436	1.030
... 2012	376	1.479	0.923	0.754	1.457	2.181
... 2014	377	−0.118	0.391	−0.348	−0.090	0.112

This table presents summary statistics for the key variables. Panel A shows the variables of interest at the bank level over the whole sample period and per election cycle. Panel B tests the difference in means for several bank characteristics between close-election winners and close-election losers. Panel C shows the characteristics of sample BHCs. Panel D shows the characteristics of politicians supported by sample BHCs. Panel E shows the variables of interest at the MSA level over the whole sample period and per election cycle. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.

branches across the country: the average (median) number of branches run by BHCs is 574 (94) across more than 6 (47) states (MSAs) on average. Although the BHCs operate across the country, their activities are concentrated in the region of their headquarter, with 66.6% (52.3%) of deposits held by branches located in their state (MSA).

Panel D of Table 1 gives further information about the candidates to whom BHCs donate. BHCs in our sample donate on average to more than 56 politicians, of which about 5 end up in a close race. Furthermore, almost one-third of the donations by BHCs are targeted toward *influential and relevant* politicians, those who sit in the Senate Committee on Banking, Housing, and Urban Affairs or in the House Committee on Financial Services. Compared to the geographic distribution of their business activities, BHCs spread their donations more widely across the country: they make their donations to politicians running in approximately 18 different states on average, which is three times as many states as compared to where they are in business. Importantly, BHCs support on average only 29.7% of candidates running for office in the same state than their headquarter, while half of the time they support candidates in states where they even do not have bank branches. These statistics indicate that BHCs invest in political capital across the country (at the federal level) and this is what our *NetCloseWins$_{bc}$* indicator reflects.

Next, we address the last challenge. That is, we translate shocks to individual banks into regional shocks. We develop a regional indicator capturing shocks to the political capital of all BHCs operating in a given region, accounting for differences in the importance of BHCs for that region. We measure importance using the predetermined deposit market share of a bank in a region as follows:

$$DepositShare_{bcr} = \frac{Deposits_{bcr}}{Deposits_{cr}},$$

where $Deposits_{bcr}$ is the total deposits held by the BHC's b branches located in region r in the year prior to election cycle c, and $Deposits_{cr}$ is the total deposits of all BHCs' branches in region r in the year prior to election cycle c. Our regional indicator for an election cycle is then obtained by summing the political shocks of all BHCs active in that region, weighted by their market share according to the following formula:

$$NetCloseWins_{cr} = \sum_b DepositShare_{bcr} \times NetCloseWins_{bc}.$$

Higher value of the $NetCloseWins_{cr}$ indicator implies larger overall political capital gain for the banks operating in a given MSA during a congressional election cycle.[12] Panel E of Table 1 reports an average value for $NetCloseWins_{cr}$ of 0.632, with again wide variation across election cycles (see also Figure 1). Importantly, the standard deviations reported in panel E also show significant variation across regions in the $NetCloseWins_{cr}$ indicator for all election cycles. Thus, shocks to individual banks seem to translate into meaningful regional shocks.

We can also split the $NetCloseWins_{cr}$ indicator into two parts, one measuring shocks from close-election winners and one from close-election losers:

$$CloseWins_{cr} = \sum_b DepositShare_{bcr} \times CloseWins_{bc},$$
$$CloseLosses_{cr} = \sum_b DepositShare_{bcr} \times CloseLosses_{bc}.$$

In a same vein, we also construct the $NetCloseWins_{cr}$ indicator split into shocks from influential and relevant elected politicians and from other politicians. To do so, we identify those close-election winners who are assigned to either the Senate Committee on Banking, Housing, and Urban Affairs or the House Committee on Financial Services after the election. We then calculate $Banking\ Cmte\ CloseWins_{cr}$ based on this subset of BHCs' $CloseWins_{bc}$, which is by construction nonnegative. We can then decompose our $NetCloseWins_{cr}$ indicator as follows:

$$Banking\ Cmte\ CloseWins_{cr} = \sum_b DepositShare_{bcr} \times Banking\ Cmte\ CloseWins_{bc},$$
$$Nonbanking\ Cmte\ NetCloseWins_{cr} = NetCloseWins_{cr} - Banking\ Cmte\ CloseWins_{cr}.$$

Next, we turn to our empirical strategy. In the first part of our study, we use a difference-in-differences model to estimate the effects of political

---

[12] The variation in the $NetCloseWins_{cr}$ indicator arises only from banks contributing to close elections; for all other banks the $NetCloseWins_{bc}$ indicator equals zero (we drop MSAs where there is no bank that contributed to a close-election winner or loser). Note also that shocks are defined at the BHC level; therefore, they arise even if there was no close election in a specific region.
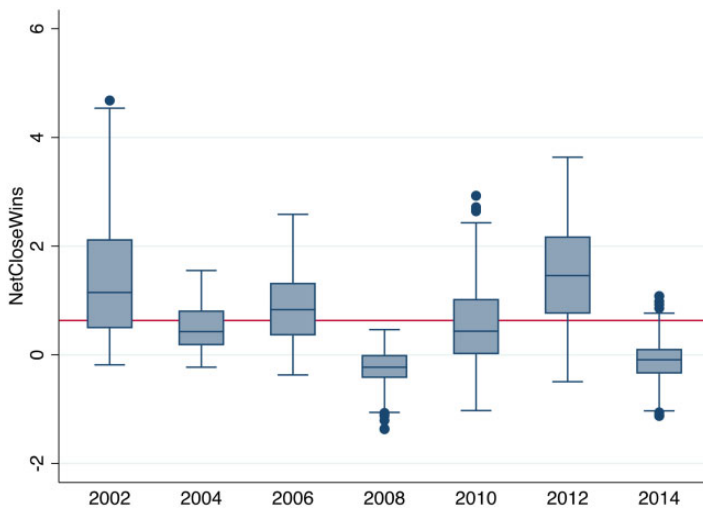
Figure 1
Distribution of MSA-level indicator of net connections per election cycle
This figure shows the cross-sectional distribution of the *NetCloseWins$_{cr}$* indicator for each election cycle between 2002 and 2014. The box plot shows the upper adjacent value, the 75th percentile, median, the 25th percentile, and the lower adjacent value (along the whiskers and the box). A dot represents an outlier. The red horizontal line represents the mean of the *NetCloseWins$_{cr}$* indicator (0.635) across all election cycles. The *NetCloseWins$_{cr}$* indicator (defined in Table A1) is winsorized at the 1st and 99th percentiles.

capital shocks on aggregate economic outcomes.[13] The specification is given by

$$Y_{rt} = \alpha + \beta NetCloseWins_{cr} \times Postelection_{ct} + \gamma Controls_{rt} + \eta_{cr} + \mu_t + \varepsilon_{crt},$$
(1)

where Y_{rt} is the economic outcome of interest in region r at time t. α is a constant term. *NetCloseWins$_{cr}$* is the regional indicator of shocks to banks' political capital as defined above, and *Postelection$_{ct}$* is a dummy variable that takes the value of one on the 2 years following the election year, and zero in the 2 years preceding election cycle c (see Figure 2). The years ($t = -1$ and $t = 0$) corresponding to election cycle c under consideration are thus excluded in order to cleanly compare output growth before and after the election.[14] η_{cr} denotes election cycle-region fixed effects that absorb the influence of all regional attributes that remain unchanged per election cycle (and thus sweep away the *NetCloseWins$_{cr}$* indicator), and μ_t denotes year fixed effects that account for any

---

[13] Standard difference-in-differences models are characterized by a treatment group and a control group. Here, and similar to Akey and Lewellen (2017), both groups are treated: one group receives a positive shock, while the other group receives a negative shock.

[14] Including the years of the election cycle has no material effect on our results (see Section 3.2).
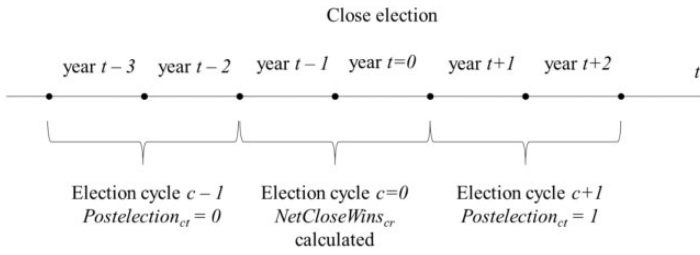
Close election

Figure 2
Illustration of the empirical strategy
This figure shows the construction of the *NetCloseWins$_{cr}$* indicator and the *Postelection$_{ct}$* dummy variable, which are the key variables of the empirical strategy. The *NetCloseWins$_{cr}$* indicator is calculated in the election year $t = 0$. The *Postelection$_{ct}$* dummy variable takes the value of one in the years $t + 1$ and $t + 2$, which follow the election year $t = 0$, and zero in the years $t - 2$ and $t - 3$, which precede the election year $t = 0$. The years $t = 0$ and $t - 1$ correspond to election cycle c under consideration and are excluded. Table A1 defines both variables.

nationwide temporal variation (and thus sweep away the *Postelection$_{ct}$* dummy variable). *Controls$_{rt}$* is a vector of control variables that accounts for demand and supply of credit at the region-year level. Finally, ε_{crt} is the error term. We cluster standard errors at the regional level across all specifications.

The coefficient of interest in Equation (1) is β, which is identified from the within-region, yearly variation in banks' political connectedness in a given congressional election cycle. It measures the marginal effect of an unexpected change to banks' political capital resulting from the outcome of close elections on regional economic activity. Figure 3 examines whether the "parallel trends" assumption holds in our analysis, by comparing output growth in regions that subsequently experience positive and negative shocks to political capital. There are no visible differences between the two groups prior to the election years.[15]

In a second part of our study, we also examine lending decisions by banks. We thus specify a version of Equation (1) at the bank-year level:

$$Y_{bt} = \alpha + \beta NetCloseWins_{bc} \times Postelection_{ct} + \gamma Controls_{bt} + \eta_{bc} + \mu_t + \varepsilon_{bct}.$$

$$(2)$$

Here, Y_{bt} is a measure of the issuance or the pricing of loans; *NetCloseWins$_{bc}$* is the BHC-level shock to political capital; and *Controls$_{bt}$* is a set of bank-level control variables. The bank-election cycle fixed effects, η_{bc}, control for BHC characteristics that remain unchanged per election cycle, while the remaining indexes and parameters are defined as in Equation (1). Standard errors are clustered at the BHC level.

---

[15] In addition, we analyze bank lending using a specification similar to Equation (1). Figures IA1 and IA2 in the Internet Appendix also show no preelection trend for CRA lending at either the regional or bank level.
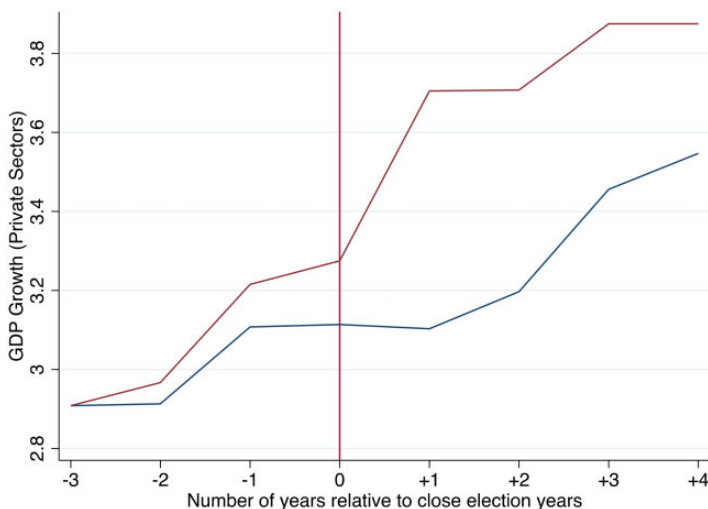
Figure 3
Output around close elections
This figure shows in red (blue) the average output of MSAs where banks experience positive (negative) shocks to their political capital. Positive (negative) shocks indicate that the *NetCloseWins$_{cr}$* indicator is above (below) the median in a given election cycle. The dependent variable reported in y-axis is private sector GDP growth. The average values are fitted value with MSA × Election cycle fixed effects and year fixed effects. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.

2. Data

We employ different data sources to generate our final samples. The first part of our study uses a sample consisting of an annual panel at the regional level, while the second part of our study considers lending by banks. Table A1 of the appendix provides the exact variable definitions, and Table 2 reports the summary statistics for all variables.

2.1 Economic activity

To study regional economic activity, we use data from the U.S. Census Bureau, the Bureau of Economic Analysis (BEA), and the Patent and Trademark Office (PTO). Our analysis mainly focuses on regional output growth, for which we obtain MSA- and county-level data on private and/ or public sector gross domestic product (GDP) from the BEA. Additional analysis focuses on the restructuring of the real economy and on productivity. We collect data on establishments from the Business Dynamics Statistics (BDS) of the Census Bureau. The data include the number of active establishments in each MSA, the number and rate of entries and exists, job creation and job destruction at both the intensive and extensive margins, and, finally, the rate of reallocation (defined as the sum of the job creation rate and the job destruction

Table 2
Summary statistics

A. MSA-level variables

	N	Mean	SD	p25	p50	p75
Economic activity						
GDP growth	9,401	3.851	4.261	−8.360	1.487	3.782
GDP growth (private sectors)	9,401	3.871	4.892	−10.258	1.222	3.780
Per capita GDP growth (private sectors)	9,401	2.917	4.699	−10.885	0.390	2.981
Establishment entry rate	9,770	9.937	2.222	6.027	8.320	9.613
Establishment exit rate	9,770	9.218	1.586	6.122	8.139	9.061
Job creation rate	9,769	13.480	2.823	8.023	11.442	13.161
Job creation rate by births	9,772	4.595	1.558	1.902	3.485	4.380
Job creation rate by continuers	9,769	8.868	1.732	5.295	7.651	8.733
Job destruction rate	9,764	12.956	2.787	7.737	10.955	12.667
Job destruction rate by deaths	9,765	4.022	1.292	1.756	3.122	3.859
Job destruction rate by continuers	9,760	8.911	1.932	5.363	7.514	8.664
Reallocation rate	9,762	23.875	4.397	14.976	20.728	23.551
Wage growth	9,772	2.847	1.715	−1.685	1.776	2.847
Patent growth	9,245	8.253	41.991	−66.667	−15.385	0.000
Population growth	9,772	0.933	1.007	−1.001	0.230	0.796
Total deposits	9,772	15.256	1.282	13.323	14.328	14.928
Number of branches	9,772	4.502	1.014	2.773	3.784	4.263
Corporate lending (CRA)						
Loan growth	8,642	−1.171	20.304	−66.462	−9.841	1.432
Loan value	8,642	402.450	540.007	28.824	105.216	193.577
Political connections						
NetCloseWins	9,772	0.572	0.893	−1.024	−0.034	0.359
CloseWins	9,772	2.522	1.717	0.020	1.181	2.213
CloseLosses	9,772	1.943	1.285	0.000	0.930	1.752
Banking cmte CloseWins	9,772	1.085	0.739	0.000	0.491	0.969
Nonbanking cmte NetCloseWins	9,772	−0.510	0.634	−2.236	−0.901	−0.454

B. Bank- and loan-level variables

	N	Mean	SD	p25	p50	p75
Corporate lending (DealScan)						
Number of loans	1,013	99.810	226.620	0.000	0.000	5.000
Facility amount	1,013	34.514	101.919	0.000	0.000	0.360
Size	1,013	17.962	1.789	11.693	16.778	18.011
ROA	1,013	3.484	3.777	−11.893	2.104	3.852
Liquidity	1,013	27.586	15.200	3.509	17.731	23.366
NPL	1,013	2.934	2.841	0.000	1.101	1.881
Tier1	1,013	10.526	4.564	1.462	8.199	9.664
Interest rate spread	71,730	250.336	146.200	20.000	150.000	225.000
Junk borrower	14,478	0.547	0.498	0.000	0.000	1.000
Political connections						
NetCloseWins	1,013	0.731	1.980	−4.000	−1.000	1.000
Borrower NetCloseWins	4,034	0.516	2.263	−4.000	−1.000	0.000

This table presents summary statistics for all the variables. Panel A shows the variables at the MSA level over the whole sample period. Panel B shows the variables at the bank and loan levels over the whole sample period. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.

rate). The BDS data are commonly used to proxy for the intensity of creative destruction and the definitions of our proxy variables follow the seminal work of Davis, Haltiwanger, and Schuh (1996). We also obtain data on wage from the BEA and on patent grants from the PTO. All these data are available at the MSA level for different time periods. However, we keep data between 2000 and 2016 throughout our analyses since GDP data (our main focus) are only available from 2001 to 2017.[16] Our analysis primarily focuses on MSAs because, as economically integrated areas, they are likely to be affected by the same regional shocks.[17] Our final main sample consists of 378 unique MSAs (in Section 3.2 we also consider county-level data).

2.2 Corporate lending

In the second part of our study, we use data on small business loan originations collected by the Federal Financial Institutions Examination Council (FFIEC) under the auspices of the CRA (for a more comprehensive description of CRA data, see, e.g., Bord, Ivashina, and Taliaferro 2021). The CRA focuses on loans with commitment amounts below $1 million originated by banks with more than $1 billion in assets, which we interpret as loans to small business. The purpose of the CRA is to encourage banks to extend credit in the regions where they are chartered. The CRA data are disaggregated by size but also by geographical location. Consequently, these data provide us with a complete record of new lending quantities by the subsidiaries of BHCs in each region.

We use CRA to build two key dependent variables at the MSA level. We define loan growth as the annual growth rate of new loan originations under $1 million in a given MSA. To mitigate the effect of outliers, we normalize the year-to-year change in lending volume by the midpoint of originations between the 2 years, as in Cortés et al. (2020). We also use loan value that we define as the total dollar amount of new loans originated per year in each MSA by BHCs. The resultant sample covers 378 MSAs over the period 2000–2016.

---

[16] We do not use the year 2017 because our empirical strategy discussed previously requires 2 years of data after the 2014 election cycle. For the same reason, our analysis of output growth does not include the 2002 election cycle. Patent data from PTO are not available after 2015.

[17] A metropolitan statistical area has at least one urbanized area of 50,000 or more inhabitants, while a micropolitan statistical area counts at least one urban cluster of at least 10,000 but fewer than 50,000 inhabitants. Both statistical areas include one or more counties, and some contain counties from several states (e.g., the New York MSA includes counties from New York, New Jersey, Connecticut, and Pennsylvania). Metropolitan and micropolitan statistical areas are defined by the Office of Management and Budget as core-based statistical areas (CBSAs). Our sample excludes Alaska, Hawaii, and U.S. territories and only contains MSAs because GDP data are not available for micropolitan statistical areas.

Next to small business loans, we also examine the issuance of syndicated loans. Syndicated loans are large and important source of corporate finance in the United States.[18] We use detailed information on syndicated loans from the Thomson Reuters LPC DealScan database. We retrieve data on loan contract facilities, where multiple facilities may be included in a deal package, and construct variables on loan issuance and pricing. Our key dependent variables are total loan facilities extended by banks as well as the interest rate spread on drawn funds (usually over LIBOR).

We then manually match our DealScan data with BHC-level data from the Federal Reserve FR Y-9C filings based on names.[19] From the latter source, we extract data on total assets, return on assets, liquidity ratio, nonperforming loans, and Tier 1 capital ratio for BHCs in our sample. We focus on the lead arrangers of syndicated loans. If there are multiple lead arrangers, we keep the bank with the highest capital allocation (we drop the observation in case multiple banks have the same highest or missing capital allocation). We only use syndicated loans to firms in the United States. The matching process reveals that more than half of the BHCs that support candidates in close elections are also in the universe of syndicated lenders (298 of 499 BHCs). For some specifications in our analysis of interest rate spreads, we also match these DealScan data with borrower-level data from Compustat, using the linking file from Chava and Roberts (2008). As is customary, we exclude financial firms and regulated utilities, as well as firms with negative assets.

2.3 Political connections

To construct our sample of politically connected banks in close elections, we start by consolidating bank data at the BHC level. We retrieve data from the Federal Reserve FR Y-9C Consolidated Financial Statements for BHCs,[20] and complement them with individual bank data from the FDIC's Statistics on Depository Institutions (SDI) database for balance sheet information and the identity of the parent's BHC for each insured

---

[18] Syndicated loans are at the center of an active body of empirical research. Important contributions include Dennis and Mullineaux (2000), Sufi (2007), Ivashina (2009), Gopalan, Nanda, and Yerramilli (2011), Ferreira and Matos (2012), Chodorow-Reich (2014), Lim, Minton, and Weisbach, (2014), Berg, Saunders, and Steffen (2016), Falato and Liang (2016), Amiram et al. (2017), and Keil and Müller (2020).

[19] We require the DealScan lender name matched to a BHC or its subsidiary at the time of the facility starting date.

[20] We require the entity to (a) have positive values for total assets; (b) be either a BHC or a thrift holding company; (c) be a corporation as legal structure; (d) have as charter type either a holding company or a securities broker/dealer (except for Goldman Sachs, Morgan Stanley, Ally Financial, American Express); (e) not be a grandfathered savings and loan holding company; and (f) not be a lower-tier holding company whose parent also files FR Y-9C.

deposit institutions in SDI. For banks without a BHC, we treat them as individual banks.[21]

Then we measure banks' political connectedness by focusing on contributions to politicians running for office in the U.S. House of Representatives or the U.S. Senate. These elections typically occur on the first Tuesday of November in even-numbered years. In each election cycle, banks can contribute to support candidates' campaign through legal entities known as Political Action Committees (PACs). In particular, a bank sets up a PAC (a "firm PAC") that contributes to a candidate's election PAC ("election PAC"), which distributes the contributions to the candidate's campaign rather than to the candidate's personal account (which is illegal in the US). Under the Bipartisan Campaign Reform Act of 2002, the maximum amount that a firm PAC can contribute to an election PAC is capped at $10,000 per election cycle. As is standard in the literature, we use a firm's PAC contributions to election PACs our measure of a bank's political connectedness.

We obtain election outcome data from the Federal Election Committee (FEC) for all federal elections in the 2002–2014 cycles, which correspond to the cycles covered by GDP data from the BEA.[22] Our approach to identify close-election candidates is similar to those of Akey (2015), Akey and Lewellen (2017), Do et al. (2020), and Heitz, Wang, and Wang (2021). We calculate the margin of votes between the winning and runner-up candidates for each election, and restrict the sample to elections in which the margin is below 5%, meaning that the winning candidate receives less than 52.5% of the vote and the losing candidate more than 47.5% in elections with two candidates. Our sample contains 191 close elections.

Next, we construct the *NetCloseWins$_{bc}$* and *NetCloseWins$_{cr}$* variables described previously. We collect PAC contributions data (also from the FEC), trace each close-election candidate's election PACs and match then with firm PACs. Then, we manually match the firm PACs with the names of BHCs or their subsidiaries.[23] This matching process leads to 499 matches between BHCs and election cycles. We then match the sample to the Summary of Deposits (SOD) database provided by the Federal Deposit Insurance Corporation (FDIC) in order to calculate shocks to political capital at the regional level based on deposit share;

---

[21] We require the entity to (a) have positive values for total assets; (b) have nonmissing RSSD ID (a unique identifier assigned to financial institutions by the Federal Reserve); and (c) be not covered by FR Y-9C.

[22] We also get for all winning campaigns data on politicians' committee assignments on serving committee in the upcoming congressional session. These data are from Charles Stewart's Congressional Data (CSCD) page. We thank Charles Stewart III for generously providing this data on his website http://web.mit.edu/17.251/www/data_page.html (last accessed: June 30, 2021).

[23] If the firm PAC name matches a nonbank institution, we use the National Information Center's organization hierarchy data to identify the BHC of the institution at the time of the contribution.

this results in 435 BHC-election cycle pairs (see panel A of Table 1 for a breakdown per election cycle). The BHCs in the sample contributed a total of $10.7 million to election PACs of close-election candidates in the 2002 cycle. Total annual contributions then remained in this same range for all election cycles in the sample period.

3. Results: Political Connectedness and Economic Activity

In this section, we provide our results on the effect of political capital on economic activity.

3.1 Output growth

Table 3 reports the coefficients of regression models derived from Equation (1) using GDP growth as the dependent variable in columns 1–4 and private sector GDP growth in columns 5–8. The findings in this table show that positive shocks to banks' political capital lead to higher output growth. In column 1, we do not include any control variables but the MSA-election cycle and year fixed effects. The coefficient of interest, β, is positive and statistically different from zero at the 1% significance level. In column 2, we add the set of MSA-level control variables (population growth, total deposits, and number of branches) to the previous specification, and still find that β is positive and statistically different from zero at the 1% level. The economic magnitude is meaningful in both columns. A one-unit increase in our MSA-level indicator of shock to banks' political capital implies a 0.56–0.58 pp increase in annual GDP growth. To put this economic effect into perspective, recall that a one-unit increase in the $NetCloseWins_{cr}$ indicator is equivalent to all banks operating in a given MSA receive one extra winning candidate in a close election. In terms of standard deviation changes, our estimates imply that a one-standard-deviation change in $NetCloseWins_{cr}$ increases annual GDP growth by 0.12 standard deviations.[24]

In column 3, we decompose our $NetCloseWins_{cr}$ indicator into close wins and close losses indicators (as defined in Section 1). Both indicators are statistically different from zero and with the predicted signs (positive for $CloseWins_{cr}$ and negative for $CloseLosses_{cr}$). Interestingly, the (absolute) size of the coefficients is very similar, indicating that positive and negative shocks to political capital have almost the same (but diametrical) effects. The fact that we find symmetric results for close wins and close losses indicators suggests that our indicator is capturing meaningful

---

[24] A one-standard-deviation change is 0.893 for the $NetCloseWins_{cr}$ indicator and 4.261 for the *GDP growth* variable (see Table 2): $0.893 \times 0.5785/4.261 = 0.12$. For ease of interpretation, we consider a one-unit change in $NetCloseWins_{cr}$ throughout the text as we note that this is close to its standard deviation of 0.893.

Table 3.
Output growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	GDP growth				GDP growth (private sectors)			
NetCloseWins × Postelection	0.5624***	0.5785***			0.5279***	0.5564***		
	(3.6608)	(3.7945)			(3.0084)	(3.1399)		
CloseWins × Postelection			0.5082***				0.4743**	
			(3.1594)				(2.5288)	
CloseLosses × Postelection			−0.4953**				−0.4577**	
			(−2.5007)				(−1.9805)	
Banking cmte CloseWins × Postelection				0.6131***				0.6101***
				(4.0753)				(3.5029)
Nonbanking cmte NetCloseWins × Postelection				0.5526***				0.4963**
				(2.7317)				(2.1121)
Population growth		1.1035***	1.1061***	1.1027***		1.1575***	1.1604***	1.1568***
		(7.6455)	(7.6551)	(7.6364)		(7.1350)	(7.1440)	(7.1256)
Total deposits		−1.6376***	−1.6102***	−1.6664***		−2.0815***	−2.0542***	−2.1211***
		(−3.0434)	(−2.9553)	(−3.0574)		(−3.3184)	(−3.2334)	(−3.3410)
Number of branches		−0.6164	−0.6311	−0.6108		−0.1993	−0.2133	−0.1968
		(−0.6899)	(−0.7065)	(−0.6830)		(−0.1918)	(−0.2051)	(−0.1892)
Adj. R^2	.243	.268	.268	.268	.212	.233	.233	.233
N	9,401	9,401	9,401	9,401	9,401	9,401	9,401	9,401
MSA × Election cycle FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table documents the effects of political capital shocks on regional output growth. Columns 1–4 present the difference-in-differences model (illustrated in Figure 2) using *GDP growth* as dependent variable. Columns 5–8 present the difference-in-differences model (illustrated in Figure 2) using *GDP growth (private sectors)* as dependent variable. Observations are at the MSA-year level. *t*-statistics are in the parentheses. Standard errors are clustered at the MSA level. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.
*$p < .1$; **$p < .05$; ***$p < .01$.

variation in regional exposure to both close election outcomes. In column 4, we isolate positive shocks to political capital associated with having a politician serving on powerful congressional committees after the election (*Banking Cmte CloseWins$_{cr}$* indicator) from "residual shocks" to political capital (*NonBanking Cmte CloseWins$_{cr}$* indicator). Both indicators are positive and statistically different from zero. However, the effect is statistically and economically higher for *Banking Cmte CloseWins$_{cr}$*, which indicates that gaining (or losing) net connections to powerful politicians who obtain key responsibilities in banking and finance as a result of the election is of high relevance for the growth effect we identify.

We now exclusively focus on the private sector to investigate whether our effects are driven by public sector spending by incumbent politicians. In columns 5–8, we show that shocks to banks' political capital rather affects private sector GDP growth. As can be seen, the results are virtually unchanged from columns 1–4: the coefficient β is positive and statistically different from zero across specifications. From columns 5 and 6, the effect is economically similar, indicating that annual private sector GDP growth increases by 0.53–0.56 pp following positive regional political capital shocks. From column 7, we still find that the loadings on the close wins and close losses indicators are roughly symmetric in size and opposite in sign. And, from column 8, we continue to observe that the growth effect is statistically and economically driven by politicians sitting in congressional committees that have most power to help banks. In the remainder of the paper, we will then focus on private sector GDP growth.

In Table 4, we investigate the dynamics of the growth effect. We replace in Equation (1) the single *Postelection$_{ct}$* dummy variable with five dummy variables, *Year$_{ct^{-(+)n}}$*, taking the value of one on the nth year before (after) the year t of election cycle c, and zero otherwise. The *Year$_{ct^{-n}}$* dummy variable allows us to assess whether any growth effect can be found prior to the election. Finding such a growth effect before election years could be symptomatic of reverse causality. In particular, one could argue that since elections are won or lost as a result of economic conditions, incumbent politicians have incentives to create desirable economic conditions immediately before the election ("political business cycles"; see Nordhaus 1975). Consistent with a causal interpretation of our basic result, the estimated coefficient for the *Year$_{ct^{-n}}$* dummy variable is indistinguishable from zero. In fact, the increase in GDP growth is concentrated in the year right after the election, that is, when banks received the shock to their political capital. Thus, the growth effect appears to be a temporary one as it vanishes in the years thereafter. Figure 4 illustrates this temporary effect; it compares output levels (log) in MSAs with positive and negative shocks. We can see that following year the output gap diminishes and becomes zero around years 5 to 6.

Table 4.
Output growth dynamics

|	GDP growth (private sectors)
NetCloseWins × Year (t-2)	−0.0896
	(−1.1429)
NetCloseWins × Year (t+1)	0.2715***
	(3.5708)
NetCloseWins × Year (t+2)	0.0600
	(0.7629)
NetCloseWins × Year (t+3)	0.1036
	(1.0533)
NetCloseWins × Year (t≥+4)	−0.0248
	(−0.2989)
Year (t-2)	0.0284
	(0.7074)
Year (t+1)	−0.1734***
	(−3.8268)
Year (t+2)	−0.0421
	(−0.9427)
Year (t+3)	−0.0856
	(−1.3157)
Year (t≥+4)	0.0017
	(0.0399)
Population growth	1.2987***
	(9.6539)
Total deposits	−1.3615***
	(−2.6851)
Number of branches	−1.7230*
	(−1.9557)
Adj. R^2	.210
N	30,826
MSA × Election cycle FE	Yes
Year FE	Yes

This table documents the effects of political capital shocks on regional output growth dynamics. The difference-in-differences model reported uses *GDP growth (private sectors)* as dependent variable. The *Year (t)* dummy variables equal one for observations in year t relative to the close election year $t = 0$, and zero otherwise. As in other tables, observations in election cycle c under study ($t − 1$ and $t = 0$) are dropped. The interaction term including *Year* (year $t≤-3$) as well as the *Year* (year $t ≤ −3$) dummy variable are absorbed by the fixed effects. Observations are at the MSA-year level. t-statistics are in the parentheses. Standard errors are clustered at the MSA level. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.
*$p < .1$; **$p < .05$; ***$p < .01$.

3.2 Robustness and placebo tests

Table 5 probes the robustness of our main results to alternative sample choices and variable definitions. For this we focus on the specification of column 6 of Table 3 (the estimates of the coefficients for the other specifications of Table 3 are in line with the ones reported in Table 5).

In column 1, we drop the 2008 and 2010 election cycles to avoid our analysis being contaminated by the recent banking crisis. With banks incentivized to increase their investment in political capital, the crisis period has arguably led to excessive supervisory forbearance (Kang, Lowery, and Wardlaw 2015; Heng, Zhang, and Zhong 2021) and preferential bailouts (Duchin and Sosyura 2012), subsequently affecting aggregate output (Gropp et al. 2020). Excluding election cycles overlapping
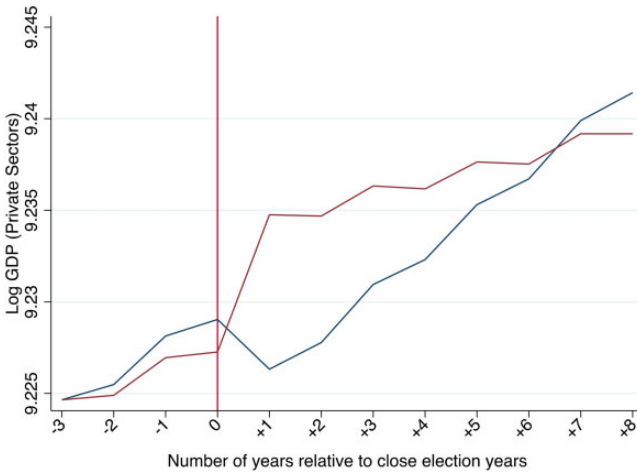
Figure 4
Long-run output after close elections
This figure shows in red (blue) the average output of MSAs where banks experience positive (negative) shocks to their political capital. Positive (negative) shocks indicate that the $NetCloseWins_{cr}$ indicator is above (below) the median in a given election cycle. The dependent variable reported in y-axis is private sector GDP in level (log). The average values are fitted value with MSA × Election cycle fixed effects and year fixed effects. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.

with the banking crisis yields even stronger results to our baseline results.[25]

In column 2, we exclude from the calculation of the $NetCloseWins_{cr}$ indicator the close elections happening in the state where the MSA is located. That is, variations in political capital shocks are now exclusively driven by close election outcomes outside the MSA itself. Excluding close elections occurring in the state where the MSAs belong provides for an even cleaner identification setting. Indeed, a concern about our empirical strategy is that our results may be driven by local election outcomes affecting local MSA growth independently of banks. Such a relationship may arise if banks tend to support candidates that are expected to be good for the local economy (and such candidates then indeed stimulate the economy when elected), or if the political capital of local firms correlates with the one of banks.[26] We observe in column 2

---

[25] Recent papers raise concerns about inferences in difference-in-differences models where treatments occur at different times, for example, staggered rollout (Borusyak and Jaraval 2018; Goodman-Bacon 2021; Sun and Abraham 2021). Our empirical setting does not suffer from this problem as we do not have staggered treatments. Rather, in each election cycle, MSAs receive political shocks at the same time. We also provide subsample regression results for the main specification for each election cycle (see Table IA1 of the Internet Appendix). The treatment effect is consistently estimated to be positive across election cycles (but not always significant in each subsample), and its average is close to the one obtained for the entire sample.

[26] In Section 4.2, we also carry out a loan-level analysis, which specifically allows us to control for shocks to the political capital of firms.

Table 5.
Robustness tests

	(1)	(2)	(3) GDP growth (private sectors)	(4)	(5)	(6) Per capita GDP growth (private sectors)
NetCloseWins × Postelection	0.7533***	0.4413***	0.7988*	0.4851***	0.3388***	0.5597***
	(3.6613)	(2.7891)	(1.9062)	(4.5407)	(3.4565)	(3.1779)
Postelection					−0.2507***	
					(−3.2845)	
Population growth	1.2088***	1.1591***	1.2337***	0.6690***	1.0396***	0.0509
	(6.4754)	(7.1415)	(7.9187)	(8.3530)	(6.5516)	(0.2898)
Total deposits	−1.9830***	−2.0062***	−1.9971***	−4.1878***	−1.8517***	−2.1262***
	(−3.0303)	(−3.2490)	(−3.1925)	(−7.9080)	(−3.0205)	(−3.3893)
Number of branches	0.9456	−0.2466	−0.2273	0.6822	0.9338	−0.1669
	(0.7970)	(−0.2358)	(−0.2114)	(1.0311)	(0.8895)	(−0.1615)
Description	Exclude 2008–2010 election cycles	Nonlocal MSA NetCloseWins	Only election outcomes ≤ 1% margins	County-level specification	Include electoral cycle c = 0	Specification as in column 6 of Table 3
Adj. R^2	.230	.234	.235	.134	.252	.184
N	6,389	9,356	9,055	50,615	14,287	9,401
MSA × Election cycle FE	Yes	Yes	Yes	—	Yes	Yes
County × Election cycle FE	—	—	—	Yes	—	—
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table documents the effects of political capital shocks on regional output growth focusing on alternative sample choices and variable definitions. Columns 1–4 present the difference-in-differences model (illustrated in Figure 2) using *GDP growth (private sectors)* as dependent variable. Column 5 presents the difference-in-differences model (illustrated in Figure 2) using *Per capita GDP growth (private sectors)* as dependent variable. In column 1, the 2008 and 2010 election cycles are excluded. In column 2, the calculation of the *NetCloseWins$_{cr}$* indicator excludes close elections occurring in the states where the MSA is located. In column 3, the calculation of the *NetCloseWins$_{cr}$* indicator only considers election outcomes for which the ex post margin of victory is less than 1%. In column 4, a version of Equation (1) specified at the county level is reported. In column 5, the specification further includes the electoral cycle c = 0 (i.e., years t and t − 1). In column 6, *Per capita GDP growth (private sectors)* is used as dependent variable. Observations are at the MSA-year level, except in column 4, where observations are at the county-year level. t-statistics are in the parentheses. Standard errors are clustered at the MSA level, except in column 4, where standard errors are clustered at the county level. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.
*p < .1; **p < .05; ***p < .01.

that our results do not change materially, although the size of the coefficient β is slightly smaller than in the baseline. This shows that our results are not driven by the effect of local election outcomes. Rather, the results point to the importance of political capital at the level of the BHC. This is consistent with summary statistics discussed in Section 1 showing that BHCs support candidates in several states, and even in states where they do not have operations.

In column 3, we construct the $NetCloseWins_{cr}$ indicator using only those election outcomes when the ex post margin of victory is less than 1%. We find that our results are similar when we only consider those elections that are the most likely to be randomly determined. In column 4, we verify the robustness of our results to a different level of regional aggregation (counties instead of MSAs). Consistent with our MSA-level results, we find that a one-unit increase in our county-level indicator of shocks to banks' political capital leads to a 0.49-pp increase in annual, county-level GDP growth. In column 5, we include the years of the current election cycle c (i.e., years t and $t-1$) to the baseline regression. The sample size thus increases by a third and the election cycle dummy variable can now be estimated due to increased degrees of freedom. The coefficient β is smaller but still positive and statistically different from zero at the 1% level. The negative estimated coefficient for the $Postelection_{ct}$ dummy variable is consistent with the observation of the diminishing trend in output growth over the years. In column 6, we examine whether our results are driven by demographic changes at the MSA level and use per capita GDP growth as dependent variable. Again, our results remain unchanged.

Another concern might be that our $NetCloseWins_{cr}$ indicator is not picking up the treatment, but instead a "general election" effect or any other regional factors. We now incorporate a series of placebo tests into our analysis to ensure that the estimated treatment effect is not either a random effect or capturing some spurious correlation(s) with omitted factors. If this is the case, we should obtain the same results independent of the assignment of treatment observations. In panels A and B of Figure 5, we present placebo tests that randomly perturb components of our indicator of interest. We construct 1,000 placebo samples that randomize close election outcomes and rerun the same specification as in column 6 of Table 3 on these placebo samples. In each placebo sample in panel A, we randomly assign the $NetCloseWins_{cr}$ indicator within each state to construct the "pseudo"-$NetCloseWins_{cr}$ indicator. That is, we assign an MSA a random $NetCloseWins_{cr}$ from another MSA of that state and across election cycles. In panel B, we take instead random permutations of the $NetCloseWins_{bc}$ indicator to calculate the pseudo-$NetCloseWins_{cr}$ indicator (recall that the BHC-level $NetCloseWins_{bc}$ indicator is the input into the MSA-level $NetCloseWins_{cr}$ indicator). That is, we replace the
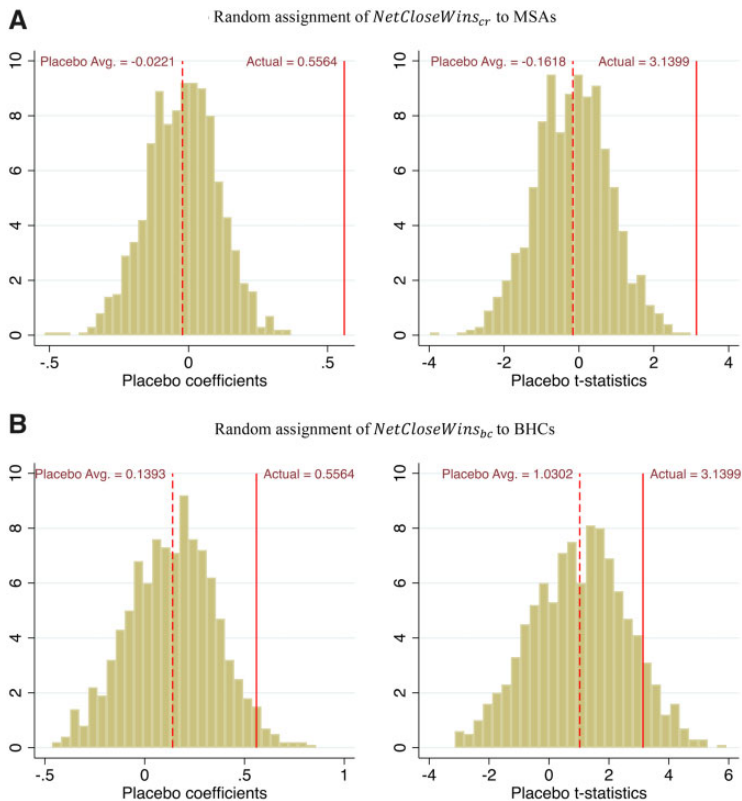
Figure 5
Distribution of estimated coefficients and *t*-statistics across placebo samples
This figure shows the distribution of estimated coefficients (left) and *t*-statistics (right) when we run the specification in column 6 of Table 3 for 1,000 placebo samples that randomize close election outcomes. In panel A, in each placebo sample, we take random permutations of the *NetCloseWins*$_{cr}$ indicator to each MSA in the same state to calculate a pseudo-*NetCloseWins*$_{cr}$ indicator. Multiple-state MSAs are therefore dropped. In this way, we preserve the overall distribution of MSAs across states. In panel B, in each placebo sample, we take random permutations of the *NetCloseWins*$_{bc}$ indicator to calculate the pseudo-*NetCloseWins*$_{cr}$ indicator. In this way, we preserve the overall distribution of BHCs across election cycles. Each panel reports the distribution of estimated coefficients and *t*-statistics for regression coefficients of the interaction term, *NetCloseWins*$_{cr}$ × *PostElection*$_{ct}$. Each panel also reports the average estimated coefficients and *t*-statistics across all placebo simulations (dotted line), as well as the estimated *t*-statistics from the specification in column 6 of Table 3 using actual close election outcomes (solid line). Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.

NetCloseWins$_{bc}$ for a bank in a given election cycle with *NetCloseWins*$_{bc}$ of the same bank from another election cycle. In this way, we preserve the overall distribution of BHCs across election cycles. Both panels of Figure 5 show that the coefficients (histogram on the left) and *t*-statistics (histogram on the right) on our placebo versions of the interaction term, *NetCloseWins*$_{cr}$ × *Postelection*$_{ct}$, are centred around zero. The fact that our results (correctly) disappear when we perform these placebo tests provides

us with some confidence that the observed growth effects are due to the treatment, as opposed to some other forces.[27]

3.3 Allocative efficiency and productivity

What is the source of the temporary increase in output growth? We now turn to answer this question by investigating whether the growth effect we document previously is symptomatic of an increase in allocative efficiency. Higher pace of reallocation of resources is often interpreted as a sign of a more competitive and efficient business environment. This view goes back to Schumpeter's (1912) process of creative destruction. However, higher turnover rate of firms does not necessarily imply enhanced efficiency if firms are wrongly forced to exit. Evidence also shows that higher reallocation is closely linked to productivity: resources are shifting away from low-productivity firms toward high-productivity firms (Foster, Grim, and Haltiwanger 2016). In this subsection, we utilize our MSA-level setting to study whether banks' political connectedness affects the productivity-enhancing reallocation of resources at both establishment and employment levels.

Table 6 presents the results. This table uses the specification of column 6 of Table 3 first replacing GDP growth with proxy variables for allocative efficiency. We find that political capital shocks lead to a large reduction in the number of exits, whereas they increase entry of new establishments to the market only at a much smaller margin. In a similar vein, our findings on employment show that positive political capital shocks are associated with more (less) job creation (destruction) by incumbents. However, we do not find an effect (and, if anything, a negative one) on job creation by new entrants, nor on job reallocation. Taken together, these findings are in line with Garcia-Macia, Hsieh, and Klenow (2019) who find that most U.S. output growth appears to come from incumbents since they comprise a larger share of employment.

In column 1 of Table 6, we report a regression model derived from Equation (1) using establishment entry rate as dependent variable. We find that the coefficient of interest, β, is small, positive and statistically different from zero at the 5% level. In column 2, we use the same specification as in column 1 but with establishment exit rate as dependent variable. We obtain a coefficient β positive and statistically different from zero at the 1% level. The effect is economically meaningful as a one-unit increase in our MSA-level indicator of shock to banks' political capital

---

[27] As another type of placebo test, we use a different cutoff than 50% for winning an election. We set 60% as the cutoff by taking the difference between candidates with vote shares between 50% and 60%, and 60% and 70% (we do not go higher in terms of vote shares as the groups of unexpected and sure winners would become very unbalanced). The results are contained in Table IA3 of the Internet Appendix. Again, they (correctly) disappear. We thank the referee for suggesting this additional placebo test.

Table 6
Allocative efficiency and productivity

	(1) Establishment entry rate	(2) Establishment exit rate	(3) Job creation rate	(4) Job creation rate by births	(5) Job creation rate by continuers	(6) Job destruction rate	(7) Job destruction rate by deaths	(8) Job destruction rate by continuers	(9) Reallocation rate	(10) Wage growth	(11) Patent growth
NetCloseWins × Postelection	0.0515**	−0.1598***	0.0632	−0.0040	0.0738*	−0.2105***	−0.1075***	−0.1076**	−0.1318	0.0749	0.7211
	(2.0244)	(−5.5582)	(1.2075)	(−0.1334)	(1.7803)	(−3.1132)	(−3.3424)	(−2.0086)	(−1.5127)	(1.3595)	(0.6329)
Population growth	0.2828***	−0.3992***	0.6003***	0.1184***	0.4539***	−0.5455***	−0.1856***	−0.3605***	−0.0596	0.1666***	1.6713
	(10.1860)	(−10.9490)	(9.1577)	(3.8721)	(9.9075)	(−9.8250)	(−6.7488)	(−8.5977)	(−0.8151)	(3.4434)	(1.1171)
Total deposits	0.4432***	0.0336	−0.1607	0.0290	−0.2201*	0.1390	−0.0791	0.1845	−0.1116	−0.3855*	−0.1095
	(3.3812)	(0.3556)	(−0.8581)	(0.2427)	(−1.7621)	(0.6132)	(−0.9844)	(1.0374)	(−0.3088)	(−1.8788)	(−0.0287)
Number of branches	−0.6320***	0.2543	−0.7549*	−0.3661	−0.4472	−0.1198	0.0127	−0.0454	−0.1054	−0.2165	−5.9842
	(−3.0994)	(1.4323)	(−1.8249)	(−1.5426)	(−1.6173)	(−0.2958)	(0.0595)	(−0.1441)	(−0.1728)	(−0.6844)	(−0.6959)
Adj. R^2	.908	.811	.706	.629	.563	.659	.511	.571	.714	.385	−.053
N	9,770	9,770	9,769	9,772	9,769	9,764	9,765	9,760	9,762	9,772	9,245
MSA × Election cycle FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table documents the effects of political capital shocks on the productivity-enhancing reallocation of resources at both establishment and employment levels. Columns 1–11 present the difference-in-differences model (illustrated in Figure 2) using the variable specified in the column label as dependent variable. Observations are at the MSA-year level. *t*-statistics are in the parentheses. Standard errors are clustered at the MSA level. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.
*$p < .1$; **$p < .05$; ***$p < .01$.

27

leads to a 0.16-pp decrease in establishment exit rate. This corresponds to a decrease in exit rate of about 1.2% relative to the sample mean reported in panel A of Table 2.

In columns 3 to 9, we use the same specification as previously to analyze employment. Specifically, we consider job creation and job destruction at both intensive and extensive margins, as well as the job reallocation rate. In column 3, we can observe that job creation in aggregate is hardly affected (β is indistinguishable from zero). However, looking at job creation at both the extensive and intensive margins qualifies this finding. As can be seen in column 4, the coefficient of interest, β, is indistinguishable from zero, while in column 5, β is positive and statistically different from zero at the 10% level. In economic terms, a one-unit increase in our MSA-level indicator of shock to banks' political capital is associated with a 0.07-pp increase in job creation by incumbents (intensive margin), while job creation by new entrants (extensive margin) is not affected. In column 6, we also find that MSAs experiencing positive political capital shocks observe a reduction in job destruction (layoffs) in aggregate. In economic terms, a one-unit increase in our MSA-level indicator of shock to banks' political capital implies a 0.21-pp decrease in job destruction rate, corresponding to a decrease in job destruction of more than 2%, on average. In columns 7 and 8, we show that this effect on job destruction shows up at both the extensive and intensive margins. In column 9, we complement these results by looking at another measure of efficiency. We examine whether political capital shocks affect the job reallocation rate (i.e., a measure of employment turnover). Consistent with our previous results on job creation and destruction, we fail to find evidence of a significant effect on reallocation rate.

The next question is whether this restructuring pattern in the real sector, which seems to benefit to incumbent firms, translates into higher productivity. The evidence provided in the remaining columns of Table 6 does not suggest so. We do not find that wage growth (a measure of enhancement in labour productivity) is affected by shocks to banks' political capital. We get similar results when we focus on the number of patents granted, which can proxy for potential productivity growth. The regression results are displayed in columns 10 and 11, respectively.

Collectively, the findings in this subsection suggest that the temporary boost in output growth is due to less restructuring in the real economy and is not accompanied by an increase in productivity. This is consistent with the notion that banks' investment in political capital spurs growth by fostering incumbents (i.e., discouraging destruction) instead of new entrants (i.e., fueling creative destruction).

4. Results: Political Connectedness and the Finance-Growth Nexus

In this section, we assess the channel through which banks' political capital can influence temporarily output growth. We examine the issuance and pricing of loans.

4.1 Loan issuance

Banks are generally viewed as an engine of economic growth because one of their key functions is to extend credit to the most productive businesses. If the growth effect we identified in regions experiencing positive political capital shocks goes through this function of banks, we should observe increased availability of credit in these regions. To test this prediction, we analyze loan issuance volume directed toward both small and large businesses. Our analysis of small loans uses MSA-level data, while our analysis of larger (syndicated) loans uses loan-level information. Together, our analyses speak to a very significant fraction of the total corporate loan issuance in the U.S. market. In 2016 (the last sample year), the total issuance of small business loans and syndicated loans amounted to more than \$600 billion and \$2 trillion, respectively.

Table 7 presents the MSA-level results on bank originations of small business loans. We use CRA data to build the dependent variables at the MSA level and estimate Equation (1). Consistent with our prediction, we find an increase in supplied loan quantities in MSAs receiving positive political capital shocks. Column 1 uses loan growth as dependent variable. The coefficient β appears positive and statistically different from zero at the 5% level, and indicates that the growth in small business loan originations increases by 1.16 pp in MSAs where banks experience a positive shock to their political capital. This is a sizable magnitude relative to the unconditional mean of -1.17% and the standard deviation of 20.30% reported in panel A of Table 2. We obtain very consistent results in column 2, where we use loan value as dependent variable. These results are based on variations in CRA lending at the MSA level. However, we obtain consistent results when we exploit variations in political capital at the bank level within MSAs (see Table IA2 in the Internet Appendix). In other words, banks receiving positive shocks due to close elections expand their (CRA) lending relative to other banks in the same MSA.

Table 8 turns to the results on syndicated loans. We regress loan issuance by a given BHC in a given year on the *NetCloseWins*$_{bc}$ indicator interacted with the *Postelection*$_{ct}$ dummy variable, as specified in Equation (2). In the regressions, we also control for a host of bank-level characteristics (namely, size, earnings, liquidity, nonperforming loans, capital adequacy) and fixed effects (namely, BHC election cycle

Table 7
Loan issuance: CRA data at the MSA level

	(1) Loan growth	(2) Loan value
NetCloseWins × *Postelection*	1.1579**	6.3078*
	(2.1413)	(1.6616)
Population growth	1.2003**	2.2034
	(2.5835)	(0.5045)
Total deposits	−0.1671	27.4629*
	(-0.1020)	(1.7534)
Number of branches	−10.1348***	10.5155
	(-2.8451)	(0.3956)
Adj. R^2	.447	.968
N	8,567	8,567
MSA × Election cycle FE	Yes	Yes
Year FE	Yes	Yes

This table documents the effects of political capital shocks on bank originations of small business loans using CRA data. Column 1 presents the difference-in-differences model (illustrated in Figure 2) using *Loan growth* as dependent variable. Column 2 presents the difference-in-differences model (illustrated in Figure 2) using *Loan value* as dependent variable. Observations are at the MSA-year level. *t*-statistics are in the parentheses. Standard errors are clustered at the MSA level. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.
*p < .1; **p < .05; ***p < .01.

Table 8
Loan issuance: DealScan data

	(1) Number of loans	(2) Facility amount
NetCloseWins × *Postelection*	10.1135**	4.9771**
	(2.2798)	(2.0878)
Size	0.2539	0.1069
	(0.0289)	(0.0281)
ROA	−1.1730	−0.6641
	(-0.7220)	(-0.8876)
Liquidity	0.6701*	0.4958*
	(1.6916)	(1.7768)
NPL	1.3307	0.7363
	(0.6200)	(0.4988)
Tier1	1.7197	1.1776*
	(1.3368)	(1.7915)
Adj. R^2	.915	.889
N	1,013	1,013
BHC × Election cycle FE	Yes	Yes
Year FE	Yes	Yes

This table documents the effects of political capital shocks on syndicated loans using DealScan data. Column 1 presents the difference-in-differences model (illustrated in Figure 2) using *Number of loans* as dependent variable. Column 2 presents the difference-in-differences model (illustrated in Figure 2) using *Facility amount* as dependent variable. Both columns control for bank-level characteristics that are lagged by 1 year. Observations are at the BHC-year level. *t*-statistics are in the parentheses. Standard errors are clustered at the BHC level. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.
*p < .1; **p < .05; ***p < .01.

30

and year).[28] The results illustrate that, consistent with our prediction, syndicated loan issuance expanded for banks receiving a positive political capital shock following close elections. In column 1, the dependent variable is the number of loans, whereas in column 2 it is the amount of loan facilities. In both columns, the coefficient of interest, β, is positive and statistically different from zero at the 5% level. The magnitude of the effect is also sizable as the number of facilities increases by 10.13% (from column 1) for each unit increase in our bank-level indicator of shock to political capital, while the total facility amount increases by 14.42% (column 2).[29]

4.2 Loan pricing

The evidence thus far paints a positive role of banks' political connectedness in boosting short-term growth through facilitating loan originations to businesses. A question naturally arises: why do politically connected banks extend relatively more business loans? Prior literature has shown that political connections help banks obtain favorable treatments (Duchin and Sosyura 2012; Blau, Brough, and Thomas 2013; Igan and Mishra 2014; Kang, Lowery, and Wardlaw 2015; Lambert 2019; Heng, Zhang, and Zhong 2021). Igan, Mishra, and Tressel (2012) and Kostovetsky (2015) also show evidence that favorable treatment causes banks take on more risk (originate riskier loans), as they are more isolated from the negative consequences of risk-taking. In this subsection, we examine loan pricing and borrower attributes to study whether they are consistent with this "favorable treatment" channel.

Equipped with our empirical strategy, we run a version of Equation (2) at the level of the individual loan facility. Specifically, we regress the loan spread (the interest on a loan facility) on the $NetCloseWins_{bc}$ indicator interacted with the $Postelection_{ct}$ dummy variable, controlling for loan-level variables and BHC-election cycle fixed effects. The set of control variables accounts for facility size, maturity, purpose, number of participants in the syndicate, and other loan contract characteristics (such as whether the loan is a term or revolver loan and whether the loan is secured), as defined in Table A1 of the appendix. We cluster standard errors at the BHC level.

The estimate of the coefficient for the interaction term (i.e., β) measures the effect of a BHC's shock to its political capital following close elections on the spreads of syndicated loans it issues. Table 9 presents the results. We find that politically connected banks tend to relax corporate

---

[28] Table A1 of the appendix defines the control variables.

[29] We obtain the 10.13% increase using the coefficient of column 1 and the mean reported in panel B of Table 2 (10.11/99.81 = 10.13%), and the 14.42% increase from the coefficient reported in column 2 and the mean in panel B of Table 2 (4.98/34.51 = 14.42%).

Table 9
Loan pricing

	(1)	(2)	(3)	(4)
			Interest rate spread	
NetCloseWins × *Postelection*	−7.9756**	−5.7144**	−4.1664*	−6.2836**
	(-2.1193)	(-2.1256)	(-1.8225)	(-2.4088)
Postelection	23.3604***	21.1713***	20.8317***	20.9204***
	(5.6412)	(3.1209)	(6.7848)	(4.4222)
Borrower NetCloseWins		0.1031		
		(0.2115)		
Borrower NetCloseWins × *Postelection*		0.0654		
		(0.1755)		
Junk borrower			68.6139***	
			(12.8728)	
Junk borrower × *Postelection*			−3.9532	
			(-0.7135)	
NetCloseWins × *Junk borrower*			3.3931***	
			(2.9731)	
NetCloseWins × *Junk borrower* × *Postelection*			−4.5554**	
			(-2.1811)	
Small borrower				5.5486
				(1.4150)
Small borrower × *Postelection*				−1.3120
				(-0.4297)
NetCloseWins × *Small borrower*				2.6216**
				(2.5023)
NetCloseWins × *Small borrower* × *Postelection*				−2.1474**
				(-2.1869)
Description	Baseline	Borrower net connections	Borrower risk	Borrower size
Adj. R^2	.372	.492	.485	.390
N	71,706	4,011	14,466	30,293
Loan-level control variables	Yes	Yes	Yes	Yes
BHC × Election cycle FE	Yes	Yes	Yes	Yes

This table documents the effects of political capital shocks on interest rate spread using syndicated loan data from DealScan. Column 1 presents the difference-in-differences model (illustrated in Figure 2) using *Interest rate spread* as dependent variable. Column 2 presents the difference-in-differences model (illustrated in Figure 2) using *Interest rate spread* as dependent variable and further adding the interaction between the *Borrower NetCloseWins* indicator and the *Postelection*$_{ct}$ dummy variable. Columns 3 and 4 present the triple-difference model using *Interest rate spread* as dependent variable and further condition the effect of the interaction between the *NetCloseWins*$_{bc}$ indicator and the *Postelection*$_{ct}$ dummy variable on borrower characteristics. All columns control for loan-level characteristics, including *Facility size*, *Maturity*, *Revolver*, *Term loan*, *Secured*, *Loan purpose* (vector of dummy variables), and *Number of lenders*. Observations are at the loan level. *t*-statistics are in the parentheses. Standard errors are clustered at the BHC level. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.
*$p < .1$; **$p < .05$; ***$p < .01$.

lending conditions. In particular, syndicated loan spreads decrease when BHCs experience a positive shock to their political capital. Furthermore, the decline in interest rates is more pronounced for riskier borrower firms.

Column 1 reports the baseline pricing results. First, the coefficient for the $Postelection_{ct}$ dummy variable indicates that loans are 23.36 basis points (bps) more expensive following close elections regardless of the political capital shock received by BHCs.[30] This result is consistent with the overall trend of increasing loan spread in our sample period. Then, the coefficient for the interaction term, β, is negative and statistically different from zero, with a magnitude of -7.98. This implies that a one-unit increase in our bank-level indicator of shocks to political capital leads to a reduction in loan spreads of 7.98 bps. This reduction corresponds to a drop of more than 34% relative to the general increase in loan spreads after close elections of 23.36 bps.

A possible reason for this decline in interest rates is that borrowers themselves have contributed to the campaign of close-election candidates. The interaction term may then appear significant in our regression if banks' campaign contributions correlate with borrowers' campaign contributions. The loan-level setup allows to control for this possibility. Analogous to banks, we now calculate shocks to borrowers' political capital after close elections. We can only calculate these shocks for publicly listed borrowers due to data availability, which considerably reduces sample size. In column 2, we add the *Borrower NetCloseWins* indicator with the $Postelection_{ct}$ dummy variable to the previous specification. We obtain very similar results. Our coefficient β is, if anything, slightly smaller in column 2 relative to column 1. The interaction term between the *Borrower NetCloseWins* indicator with the $Postelection_{ct}$ dummy variable is indistinguishable from zero.

In the remaining columns, we exploit borrower heterogeneity to test whether cheaper lending conditions are directed toward riskier borrowers. Doing so may tell us more about the channel through which political capital affects bank incentives. As noted above, the "favorable treatment" channel predicts risk-shifting behavior at banks. It is important to look at risk-taking because favorable policies may also arise more broadly, without changing banks' risk-taking attitude, that is, policies aligned with the preferences of banks (e.g., government subsidies, protection against new competition). To examine the influence of risk-taking, we use a triple-difference strategy. We run similar regressions than in column 1, but we additionally condition the effect of the interaction between the

---

[30] The specification in column 1 is at the (cross-sectional) loan level and does not include year fixed effects, which explains why the $Postelection_{ct}$ dummy variable is not absorbed here. We obtain qualitatively similar results if we include year fixed effects.

NetCloseWins$_{bc}$ indicator and the *Postelection$_{ct}$* dummy variable on borrower characteristics. From columns 3 and 4, we uncover that interest rate decreases are indeed concentrated in riskier firms. In column 3, we run the triple-difference regression using credit ratings to proxy firm risk. For risky borrower, spreads clearly increase (see coefficient for the *Junk borrower* variable). However, the coefficient β (negative and statistically different from zero at the 1% level) implies that BHCs with positive shock to their political capital charge lower interest rates to firms with inferior credit ratings. Having a BB+ or lower rating ("junk") implies a sizable 4.56 bps further decrease in loan spreads. In column 4, we use the same triple-difference setup and introduce the interaction terms with borrower firm size (as another proxy for riskiness). The *Small borrower* dummy variable is positive though it just fails to be statistically different from zero at conventional significance levels, suggesting that smaller (and thus typically riskier) borrowers pay higher interest rates. Consistent with the results in the previous column, the triple-interaction term is negative and statistically different from zero. The estimate of the coefficient suggests an important heterogeneity for the impact of banks' political capital shock within our sample: a firm below the median of the size distribution sees a 2.15 bps further decrease in spreads.

Overall, the effects documented in this subsection—though less precisely estimated—suggest that BHCs receiving a shock to their political capital after close election outcomes charge lower interest rates likely. The fact that the results are also stronger for riskier borrowers is consistent with the idea that banks take more risks due to moral hazard under the "favorable treatment" channel. However, these findings are rather indirect, and our analysis does not allow us to fully rule out other channels. One of such channels is that politically connected banks do well because "their" candidates who won the election implement policies aligned with their broad interests.

5. Conclusion

In this paper, we study the consequences of banks' political connectedness for economic activity. We focus on the subset of banks that donate to candidates in U.S. congressional elections and exploit close election outcomes as plausible exogenous changes in banks' political capital.

We first document that aggregate shocks to banks' political capital produce larger subsequent changes in output growth in the regions where these banks operate. A region's output growth increases by 0.12 standard deviations when the banks active in the region experience a one-standard-deviation shock to their political capital due to close election outcomes. Political capital associated with powerful congressional

committee members drives a significant part of this growth effect. However, we also find that it is temporary, vanishing rapidly after the election.

We then show that this growth effect is primarily due to relative sclerosis. There is less restructuring in the real economy, and this is not accompanied by higher productivity. Regions experiencing positive shocks to their banks' political capital have lower establishment exits and, similarly, fewer job losses in their real sector. However, we do not find that positive political capital shocks result in many more establishment entries as well as in more job creation and reallocation. Studying wages and patents also does not provide any evidence of productivity enhancement. Taken together, these findings suggest that banks' investment in political capital produces short-term improvement in the real economic activity, mostly by supporting incumbent firms rather than by fostering a Schumpeterian process of creative destruction.

Finally, we present some evidence indicating that political connections incentivize banks to ease lending conditions for firms. Banks experiencing a positive shock to their political capital issue more loans and reduce interest rates, particularly so for riskier borrowers. These results are consistent with the idea that political connectedness magnifies the moral hazard problem in banking; that is, politically connected banks take on extra risks because their ties to elected politicians may protect them (especially when conditions worsen).

Collectively, our findings reveal that, although the interference between banks and (powerful) politicians appears beneficial for the U.S. economy at first sight, these benefits are short lived and directed toward existing firms. Banks' political connectedness may thus create barriers to entry for firms, instead of fostering a productivity-enhancing reallocation of resources that would be the sign of a well-functioning banking sector.

Appendix

Table A1.
Variable definitions and sources

A. MSA-level variables

Variable	Definition	Sources
Economic activity		
GDP growth	The year-on-year growth in real MSA-level GDP	BEA
GDP growth (private sectors)	The year-on-year growth in real MSA-level GDP, only comprising all private sectors	BEA
Per capita GDP growth (private sectors)	The year-on-year growth in real MSA-level private sector GDP per capita	BEA
Establishment entry rate	The count of establishment entrants in year t divided by the average count of employment active establishments in year t and year $t-1$	BDS
Establishment exit rate	The count of establishment exits in year t divided by the average count of employment active establishments in year t and year $t-1$	BDS
Job creation rate	The count of all employment gains from expanding establishments from year $t-1$ to year t, including establishment startups divided by the average of employment in year t and year $t-1$	BDS
Job creation rate by births	The count of all employment gains from establishment openings (births) between year $t-1$ and year t divided by the average of employment in year t and year $t-1$	BDS
Job creation rate by continuers	The count of all employment gains from continuing establishments between year $t-1$ and year t divided by the average of employment in year t and year $t-1$	BDS
Job destruction rate	The count of all employment losses from contracting establishments from year $t-1$ to year t, including establishments shutting down divided by the average of employment in year t and year $t-1$	BDS
Job destruction rate by deaths	The count of all employment losses from establishment closings (deaths) between year $t-1$ and year t divided by the average of employment in year t and year $t-1$	BDS
Job destruction rate by continuers	The count of all employment losses from continuing establishments between year $t-1$ and year t divided by the average of employment in year t and year $t-1$	BDS
Reallocation rate	The sum of *Job creation rate* and *Job destruction rate* minus the absolute value of the difference between *Job creation rate* and *Job destruction rate*. This is often	BDS

(continued)

Table A1.
Continued

A. MSA-level variables

Variable	Definition	Sources
	referred to as an "excess" reallocation rate since it measures the rate of job reallocation over and above that needed to accommodate the net job creation	
Wage growth	The year-on-year growth in wage at the MSA level	BEA
Patent growth	The year-on-year growth in the number of utility patents plus one (i.e., patents for inventions)	PTO
Population growth	The year-on-year growth in total population	BEA
Total deposits	The log of total deposits held by bank branches in a given MSA in year $t-1$	FDIC
Number of branches	The log of the total number of bank branches in a given MSA in year $t-1$	FDIC
Corporate lending (CRA)		
Loan growth	The difference in the total amount of new loan originations under $1 million between year $t-1$ and year t divided by the average of the total amount of loan originations in year $t-1$ and year $t-1$	FFIEC
Loan value	Total amount (in millions of dollars) of new loan originations under $1 million in a given MSA in year t	FFIEC
Political connections		
NetCloseWins	An indicator variable measuring the shocks at the MSA level to BHCs' political capital during election cycle c (see Section 1)	FEC, FDIC
CloseWins	An indicator variable measuring the number of close-election winners that BHCs contributed to during election cycle c, weighted by their respective predetermined market share in an MSA (see Section 1)	FEC, FDIC
CloseLosses	An indicator variable measuring the number of close-election losers that BHCs contributed to during election cycle c, weighted by their respective predetermined market share in an MSA (see Section 1)	FEC, FDIC
Postelection	A dummy variable equal to one for the 2 years following the election year, and zero for the 2 years preceding the election year (see Figure 2)	Own calculation

B. Bank- and loan-level variables

Variable	Definition	Sources
Corporate lending (DealScan)		
Number of loans	The total number of loan facilities extended by BHC *b* in year *t*	DealScan
Facility amount	Aggregate amount in billions of dollars of loan facilities lent by BHC *b* in year *t*	DealScan
Size	The logarithm of total assets	Fed, FDIC
ROA	The ratio of net income over total assets	Fed, FDIC
Liquidity	The ratio of the sum of cash and balances due from depository institutions, interest-bearing balances, federal funds sold and reverse repurchase, federal funds purchased and repurchase agreements, held to maturity securities, and available-for-sale securities over total assets	Fed, FDIC
NPL	The ratio of the sum of assets past due 90+ days, assets in nonaccrual status, and total charge-offs over total assets	Fed, FDIC
Tier1	Tier 1 risk-based capital ratio	Fed, FDIC
Interest rate spread	The annual spread in basis points over LIBOR for each dollar drawn down from the loan	DealScan
Junk borrower	A dummy variable equal to one if the borrower's S&P Long-Term Issuer Credit Rating is a noninvestment grade (BB+ to D), and zero if the rating is an investment grade (AAA to BBB−). The variable is only available for publicly listed borrower firm	Compustat
Small borrower	A dummy variable equal to one if the borrower's book assets is below sample median, and zero if above median. The variable is only available for publicly listed borrower firm	Compustat
Facility size	The log of one plus facility amount in millions of dollars	DealScan
Maturity	The log of the loan maturity in years	DealScan
Revolver	A dummy variable equal to one for revolving line facilities, and zero otherwise	DealScan
Term loan	A dummy variable equal to one for term loan (including A/B/F), and zero otherwise	DealScan
Secured	A dummy variable equal to one if the loan is backed by collateral, and zero otherwise	DealScan
Loan purpose	A vector of dummy variables for the different categories of loan purposes (M&A, Net Working Capital, Corporate Purposes, and Repayment)	DealScan
Number of lenders	The count of all participants in the loan syndicate	DealScan
Number of branches	Number of branches under the BHC *b* recorded in FDIC's summary of deposits (SOD)	FDIC
Number of states covered	Number of states covered by the BHC's *b* branches	FDIC
Number of MSAs covered	Number of MSAs covered by the BHC's *b* branches	FDIC
Deposit share in the HQ state	Percentage of deposits in the branches located in the BHC's *b* headquarters state	FDIC
Deposit share in the HQ MSA	Percentage of deposits in the branches located in the BHC's *b* headquarters MSA	FDIC

(continued)

Table A1.
Continued

B. Bank- and loan-level variables

Variable	Definition	Sources
Political connections		
NetCloseWins	An indicator variable measuring the shock to a BHC's *b* political capital during election cycle *c* (see Section 2)	FEC
Banking cmte CloseWins	An indicator variable measuring the number of close-election winners that BHC *b* contributed to during election cycle *c* (see Section 2) who are assigned to a key congressional committee after being elected. The congressional committees are the Senate Committee on Banking, Housing, and Urban Affairs and the House Committee on Financial Services	FEC, CSCD
Nonbanking cmte NetCloseWins	The difference between *NetCloseWins* and *Banking Cmte CloseWins* for a BHC *b*	
Borrower NetCloseWins	An indicator variable measuring the shock to a firm borrower's political capital during election cycle *c* (constructed in a similar way than the *NetCloseWins* indicator, see Section 2). The variable is only available for publicly listed borrower firm	FEC
Number of candidates supported	Number of congressional election candidates supported by the BHC *b* during election cycle *c*	FEC
Number of close election candidates supported	Number of candidates in close elections supported by the BHC *b* during election cycle *c*	FEC
Number of banking committee members supported	Number of candidates assigned to financial committees and supported by the BHC *b* during election cycle *c*	FEC, CSCD
Number of states covered by supported candidates	Number of states from where the supported candidates are elected	FEC
%candidates in the HQ state	Percentage of candidates supported by the BHC *b* from the headquarter state of the BHC *b*	FEC, FDIC
%candidates in the state with branches	Percentage of candidates supported by the BHC *b* from the states where the BHC's *b* branches are located	FEC, FDIC

References

Acharya, V., T. Eisert, C. Eufinger, and C. Hirsch. 2019. Whatever it takes: The real effects of unconventional monetary policy. *Review of Financial Studies* 32:3366–411.

Adams, R. 2017. Good news for some banks. Working Paper, University of Oxford.

Agarwal, S., G. Amromin, I. Ben-David, and S. Dinç. 2018. The politics of foreclosures. *Journal of Finance* 73:2677–717.

Agarwal, S., B. Morais, C. Ruiz, and J. Zhang. 2016. The political economy of bank lending: Evidence from an emerging market. Working Paper, World Bank.

Akcigit, U., S. Baslandze, and F. Lotti. 2018. Connecting to power: Political connections, innovation, and firm dynamics. Working Paper, University of Chicago.

Akey, P. 2015. Valuing changes in political networks: Evidence from campaign contributions to close congressional elections. *Review of Financial Studies* 28:3188–223.

Akey, P., C. Dobridge, R. Heimer, and S. Lewellen. 2018. Pushing boundaries: political redistricting and consumer credit. Working Paper, University of Toronto.

Akey, P., R. Heimer, and S. Lewellen. 2021. Politicizing consumer credit. *Journal of Financial Economics* 139:627–55.

Akey, P., and S. Lewellen. 2017. Policy uncertainty, political capital, and firm risk-taking, Working Paper, University of Toronto.

Amiram, D., W. Beaver, W. Landsman, and J. Zhao. 2017. The effects of CDS trading on information asymmetry in syndicated loans. *Journal of Financial Economics* 126:364–82.

Bagehot, W. 1873. *Lombard street: A description of the money market*, 1962 ed. Homewood, IL: Irvin.

Becker, G. 1983. A theory of competition among pressure groups for political influence. *Quarterly Journal of Economics* 98:371–400.

Benmelech, E., and T. Moskowitz. 2010. The political economy of financial regulation: Evidence from U.S. state usury laws in the 19th century. *Journal of Finance* 65:1029–73.

Berg, T., A. Saunders, and S. Steffen. 2016. The total cost of corporate borrowing in the loan market: Don't ignore the fees. *Journal of Finance* 71:1357–92.

Bertrand, M., A. Schoar, and D. Thesmar. 2007. Banking deregulation and industry structure: Evidence from the French banking reforms of 1985. *Journal of Finance* 62:597–628.

Bircan, C., and O. Saka. 2021. Lending cycles and real outcomes: Costs of political misalignment. *Economic Journal* 131:2763–96.

Black, S., and P. Strahan. 2002. Entrepreneurship and bank credit availability. *Journal of Finance* 57:2807–33.

Blau, B., T. Brough, and D. Thomas. 2013. Corporate lobbying, political connections, and the bailout of banks. *Journal of Banking and Finance* 37:3007–17.

Bord, V., V. Ivashina, and R. Taliaferro. 2021. Large banks and small firm lending. *Journal of Financial Intermediation* 48:10.1016/j.jfi.2021.100924.

Borusyak, K., and X. Jaravel. 2018. Revisiting event study designs. Working Paper, Harvard University.

Brogaard, J., N. Gerasimova, and M. Rohrer. 2021. The effect of female leadership on contracting from Capitol Hill to Main Street. Working Paper, University of Utah.

Caballero, R., T. Hoshi, and A. Kashyap. 2008. Zombie lending and depressed restructuring in Japan. *American Economic Review* 98:1943–77.

40

Carvalho, D. 2014. The real effects of government-owned banks: Evidence from an emerging market. *Journal of Finance* 69:577–609.

Cetorelli, N., and P. Strahan. 2006. Finance as a barrier to entry: Bank competition and industry structure in local U.S. markets. *Journal of Finance* 61:437–61.

Chava, S., and M. Roberts. 2008. How does financing impact investment? The role of debt covenants. *Journal of Finance* 63:2085–121.

Chavaz, M., and A. Rose. 2019. Political borders and bank lending in post-crisis America. *Review of Finance* 23:935–59.

Chodorow-Reich, G. 2014. The employment effects of credit market disruptions: Firm-level evidence from the 2008-9 financial crisis. *Quarterly Journal of Economics* 129:1–59.

Claessens, S., E. Feijen, and L. Laeven. 2008. Political connections and preferential access to finance: The role of campaign contributions. *Journal of Financial Economics* 88:554–80.

Cole, S. 2009. Fixing market failures or fixing elections? Agricultural credit in India. *American Economic Journal: Applied Economics* 1:219–50.

Cortés, K., Y. Demyanyk, L. Li, E. Loutskina, and P. Strahan. 2020. Stress tests and small business lending. *Journal of Financial Economics* 136:260–79.

Davis, S., J. Haltiwanger, and S. Schuh. 1996. *Job creation and destruction*. Cambridge: MIT Press.

Degryse, H., T. Lambert, and A. Schwienbacher. 2018. The political economy of financial systems: Evidence from suffrage reforms in the last two centuries. *Economic Journal* 128:1433–75.

Dehejia, R., and A. Lleras-Muney. 2007. Financial development and pathways of growth: State branching and deposit insurance laws in the United States, 1900–1940. *Journal of Law and Economics* 50:239–72.

Dennis, S., and D. Mullineaux. 2000. Syndicated loans. *Journal of Financial Intermediation* 9:404–26.

Dinç, S. 2005. Politicians and banks: Political influences on government-owned banks in emerging countries. *Journal of Financial Economics* 77:453–79.

Do, Q-A., Y-T. Lee, B. D. Nguyen, and K-T. Nguyen. 2020. Power, scrutiny, and congressmen's favoritism for friends' firm. Working Paper, Centre for Economic Policy Research.

Duchin, R., and D. Sosyura. 2012. The politics of government investment. *Journal of Financial Economics* 106:24–48.

Eggers, A., A. Fowler, J. Hainmueler, A. Hall, and J. Snyder Jr. 2015. On the validity of the regression discontinuity design for estimating electoral effects: New evidence from over 40,000 close races. *American Journal of Political Science* 59:259–74.

Englmaier, F., and T. Stowasser. 2017. Electoral cycles in savings bank lending. *Journal of the European Economic Association* 15:296–354.

Erel, I. 2011. The effect of bank mergers on loan prices: Evidence from the United States. *Review of Financial Studies* 24:1068–101.

Faccio, M., and J. J. McConnell. 2020. Impediments to the Schumpeterian process in the replacement of large firms. Working Paper, Purdue University.

Falato, A., and N. Liang. 2016. Do creditor rights increase employment risk? Evidence from loan covenants. *Journal of Finance* 71:2545–90.

Ferreira, M., and P. Matos. 2012. Universal banks and corporate control: Evidence from the global syndicated loan market. *Review of Financial Studies* 25:2703–44.

Foster, L., C. Grim, and J. Haltiwanger. 2016. Reallocation in the Great Recession: Cleansing or not? *Journal of Labor Economics* 34:S293–S331.

Garcia-Macia, D., C-T. Hsieh, and P. Klenow. 2019. How destructive is innovation? *Econometrica* 87:1507–41.

Giannetti, M., and A. Simonov. 2013. On the real effects of bank bailouts: Micro evidence from Japan. *American Economic Journal: Macroeconomics* 5:135–67.

Goodman-Bacon, A. 2021. Difference-in-differences with variation in treatment timing. *Journal of Econometrics* 225:254–77.

Gopalan, R., V. Nanda, and V. Yerramilli. 2011. Does poor performance damage the reputation of financial intermediaries? Evidence from the loan syndication market. *Journal of Finance* 66:2083–120.

Gropp, R., S. Ongena, J. Rocholl, and V. Saadi. 2020. The cleansing effect of banking crises. Working Paper, Halle Institute for Economic Research.

Guiso, L., P. Sapienza, and L. Zingales. 2004. Does local financial development matter? *Quarterly Journal of Economics* 119:929–69.

Heitz, A., Y. Wang, and Z. Wang. 2021. Corporate political connections and favorable environmental regulatory enforcement. *Management Science*. Advance Access published March 10, 2021.

Heng, Y., L. Zhang, and Q. Zhong. 2021. The politics of bank opacity. *Journal of Accounting and Economics*. Advance Access published September 25, 2021.

Huang, R. 2008. Evaluating the real effect of bank branching deregulation: Comparing contiguous counties across US state borders. *Journal of Financial Economics* 87:678–705.

Igan, D., and P. Mishra. 2014. Wall Street, Capitol Hill, and K Street: Political influence and financial regulation. *Journal of Law and Economics* 57:1063–84.

Igan, D., P. Mishra, and T. Tressel. 2012. A fistful of dollars: Lobbying and the financial crisis, *NBER Macroeconomic Annuals* 26:195–230.

Ivashina, V. 2009. Asymmetric information effects on loan spreads. *Journal of Financial Economics* 92:300–319.

Jayaratne, J., and P. Strahan. 1996. The finance-growth nexus: Evidence from bank branch deregulation. *Quarterly Journal of Economics* 111:639–70.

Kang, A., R. Lowery, and M. Wardlaw. 2015. The costs of closing failed banks: A structural estimation of regulatory incentives. *Review of Financial Studies* 28:1060–102.

Keil, J., and K. Müller. 2020. Bank branching deregulation and the syndicated loan market. *Journal of Financial and Quantitative Analysis* 55:1269–303.

Kerr, W., and R. Nanda. 2009. Democratizing entry: Banking deregulations, financing constraints, and entrepreneurship. *Journal of Financial Economics* 94:124–49.

Khwaja, A., and A. Mian. 2005. Do lenders favor politically connected firms? Rent provision in an emerging economy. *Quarterly Journal of Economics* 120:1371–411.

King, R., and R. Levine. 1993. Finance and growth: Schumpeter might be right. *Quarterly Journal of Economics* 108:717–37.

Koetter, M., and A. Popov. 2021. Political cycles in bank lending to the government. *Review of Financial Studies* 34:3138–80.

Kostovetsky, L. 2015. Political capital and moral hazard. *Journal of Financial Economics* 116:144–59.

Kroszner, R., and T. Stratmann. 1998. Interest group competition and the organization of Congress: theory and evidence from financial services' political action committees. *American Economic Review* 88:1163–87.

Lambert, T. 2019. Lobbying on regulatory enforcement actions: Evidence from U.S. commercial and savings banks. *Management Science* 65:2545–72.

Lambert, T., and P. Volpin. 2018. Endogenous political institutions and financial development. In *Handbook of finance and development*, eds. T. Beck, and R. Levine, 477–500. London: Edward Elgar.

Li, H., L. Meng, Q. Wang, and L-A. Zhou. 2008. Political connections, financing and firm performance: Evidence from Chinese private firms. *Journal of Development Economics* 87:283–99.

Lim, I., J. Hagendorff, and S. Armitage. 2019. Is the fox guarding the henhouse? Bankers in the Federal Reserve, bank leverage and risk-shifting. *Journal of Corporate Finance* 58:478–504.

Lim, J., B. Minton, and M. Weisbach. 2014. Syndicated loan spreads and the composition of the syndicate. *Journal of Financial Economics* 111:45–69.

Nordhaus, W. 1975. The political business cycle. *Review of Economic Studies* 42:169–90.

Olson, M. 1965. *The logic of collective action: Public goods and theory groups*. Cambridge, MA: Harvard University Press.

Pagano, M., and P. Volpin. 2005. The political economy of corporate governance. *American Economic Review* 95:1005–30.

Peltzman, S. 1976. Towards a more general theory of regulation. *Journal of Law and Economics* 19:211–40.

Pérignon, C., and B. Vallée. 2017. The political economy of financial innovation: Evidence from local governments. *Review of Financial Studies* 30:1903–34.

Perotti, E., and E-L. von Thadden. 2006. The political economy of corporate control and labor rents. *Journal of Political Economy* 114:145–74.

Popov, A. 2018. Evidence on finance and economic growth. In *Handbook of finance and development*, eds. T. Beck and R. Levine, 63–104. London: Edward Elgar.

Raina, S., and S-J.. Xu 2020. Trading credit (subsidies) for votes: The effect of local politics on small business lending. Working Paper, University of Alberta.

Rajan, R., and R. Ramcharan. 2011. Land and credit: A study of the political economy of banking in the United States in the early 20th century. *Journal of Finance* 66:1895–931.

Rajan, R., and L. Zingales. 2003. The great reversals: The politics of financial development in the 20th century. *Journal of Financial Economics* 69:5–50.

Rice, T., and P. Strahan. 2010. Does credit competition affect small-firm finance? *Journal of Finance* 65:861–89.

Sapienza, P. 2004. The effects of government ownership on bank lending. *Journal of Financial Economics* 72:357–84.

Schivardi, F., E. Sette, and G. Tabellini. 2020. Identifying the real effects of zombie lending. *Review of Corporate Finance Studies* 9:569–92.

Schumpeter, J. 1912. *Theorie der wirtschaftlichen entwicklung [Theory of economic development]*, 1934 trans. ed. Cambridge, MA: Harvard University Press.

Stigler, G. 1971. The theory of economic regulation. *Bell Journal of Economics and Management Science* 2:3–21.

Sufi, A. 2007. Information asymmetry and financing arrangements: Evidence from syndicated loans. *Journal of Finance* 62:629–68.

Sun, L., and S. Abraham. 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics* 225:175–99.