Banks, Political Capital, and Growth*

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December 22, 2020

Abstract

We show that politically connected banks influence economic activity. We exploit shocks to individual banks' political capital following close US 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 rather than 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 classification codes: D72, E65, G18, G21

Keywords: campaign contributions, close elections, corporate lending, creative destruction, economic growth, political connections, productivity

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1. Introduction

A large body of empirical 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 empirical 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 economic activity? 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 may also 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 resulting change in the behaviour 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. Similar to Akey (2015) and several others, we focus on campaign contributions to candidates in close US congressional elections. We consider changes in political connections arising from close, "coin-flip" elections in the 2002-2014 cycles. Specifically, we calculate shocks

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[1] Efficiently allocating scarce resources to their greatest value use has been associated with economic growth at least since Bagehot (1873), who argue 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) links economic growth to the ability of banks to identify and fund the entrepreneurs with the greatest chances of success.

[2] A number of studies shows 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 US (Benmelech and Moskowitz, 2010; Rajan and Ramcharan, 2011). These studies are consistent with the idea at the basis of the private interest group theory of regulation, associated with work by Olson (1965), Stigler (1971), Peltzman (1976), and Becker (1983), that interest groups or constituencies can use their political power to preserve or extract rents at the expense of others.

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

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 identifying assumption is that there is randomness in what determines the outcome of a close election (Lee, 2008). To study the extent to which politically connected banks affect economic activity, we exploit the geographical dispersion of banks across regions; hence, shocks to individual banks translate into regional shocks. Specifically, we develop an indicator of net connections based on the pre-determined market shares of banks in each region for each election cycle. Using exogenous variation arising from close elections outcomes, we then 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 US economy: A one unit increase in our MSA-level indicator of net connections leads to a 0.56 percentage point (pp) increase in the annual growth rate. This effect is highly statistically significant and holds after performing a battery of robustness and placebo tests. However, we also find that this growth effect is not a permanent one: it vanishes after one year 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, while entry (through forming new establishments) is not affected. 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 is consistent with the notion that banks' investment in political capital is used to support incumbent firms, rather than encouraging the process of creative destruction.

We then explore the channel through which banks' investment in political capital may affect economic activity. The finance and growth 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). The reason for this behaviour is that connected banks can expect to obtain favorable treatment that partially insulates them from the negative consequences of their risk taking (moral hazard).[4] Such behavior is likely to distort the allocation of capital, rather than improving it. Our analysis on economic activity points to this latter channel as we find that (*i*) output growth is only temporary, (*ii*) only incumbent firms gain from it, and (*iii*) 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 US (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. These results are in line with politically connected banks taking more risks under the "favorable treatment" channel.

Related Literature. Our paper belongs to the finance and growth literature (see Popov, 2018, for a recent survey). A significant part of this literature has exploited within-country heterogeneity deriving from the implementation of policies that promote banking sector development. In the US,

[4] Examples for favorable treatment of politically connected banks are preferential bailouts (Duchin and Sosyura, 2012), beneficial regulation (Igan and Mishra, 2014), regulatory forbearance (Kang, Lowery, and Wardlaw, 2015), fewer supervisory sanctions (Lambert, 2019), and lower supervisory effectiveness (Lim, Hagendorff, and Seth, 2019).

Jayaratne and Strahan (1996) find that state banking deregulation is associated with a 0.51-1.19 pp increase in real per capita state growth. Huang (2008) examines changes in growth rates for contiguous 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 US 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 US 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: Rice and Strahan (2010) present evidence that small-business loan terms improve after deregulations in the US; Erel (2011) shows that small borrowers are generally charged lower interest rates by US commercial banks expanding their operation through mergers; and Keil and Müller (2020) report that debt contract types shifted from syndicated to bilateral loans following branching expansion, a shift that also reflected in a decrease in interest rates on bilateral loans. 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. 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 (see Lambert and Volpin, 2018, for an overview of this literature). Ample evidence in this literature shows—predominantly in the context of developing economies—that politically connected firms enjoy preferential access to and better terms of credit (see Khwaja and Mian, 2005, for evidence in Pakistan; Claessens, Feijen, and Laeven, 2006, in Brazil; Li et al., 2008, in China; and Agarwal et al., 2016, in Mexico).[5] 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 Vallée (2017) show that banks designed financial securities (structured loans) enhancing incumbent politicians' likelihood of re-election. Closer to our paper, there are also studies examining 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, 2020), and small business loan subsidies (Raina and Xu, 2020) in the US. We also find 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. Our paper thus represents the first attempt (to our knowledge) to bring the "micro" literature on the politics of finance together with the "macro" literature on finance and growth.

The rest of the paper is organized as follows. We present the identification strategy in Section 2, and we describe the data in Section 3. We report results on the direct effect of banks' political connectedness on output growth and creative destruction in Section 4, and on the "favorable treatment" channel through which political capital affects economic activity in Section 5. Finally, in Section 6, we conclude.

2. 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.

[5] These results bear some similarity to the literature on political lending cycles, which documents that credit is used politically to secure votes, see Dinç (2005), Cole (2009), Carvalho (2014), Englmaier and Stowasser (2017), Bircan and Saka (2020), and Koetter and Popov (2020).

We address these challenges by exploiting close US congressional elections in order to obtain exogenous variation in a bank's political capital (Akey, 2015; Akey and Lewellen, 2017; Heitz, Wang, and Wang, 2019; Do et al., 2020). Specifically, we consider election outcomes for which the ex-post margin of victory is less than 5%. Conceivably, banks may still have some limited ability to predict the winner, even in such tight races. However, as shown by Lee (2008), the only requirement for identification is that banks are not able to *perfectly* determine the winning (or losing) candidate: as long as there is some randomness in the outcome process, inference is as good as if treatments were randomized.[6] 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, 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).[7] The Federal Reserve regulates and supervises BHCs, which are large corporations controlling their subsidiaries operating across regions. Political connections 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.[8] Consider, for instance, CIT Group Inc. (a BHC headquartered in New York) 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.

[6] The intuition for this result is that a treatment sufficiently close to the threshold is driven by chance (as long as the treatment function is continuous), and hence treatment and control groups on either side of the threshold have identical characteristics. Importantly, Lee (2008) also provides an empirical test of the validity of the inference based on comparing the characteristics of treatment and control groups in terms of baseline characteristics.
[7] We employ "BHC" and "bank" interchangeably in the text.
[8] 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).

Table 1 presents summary statistics for the political capital variables. The average value of $NetCloseWins_{bc}$ varies widely across election cycles (see Panel A). For example, this variable is negative with -0.400 in 2008, whereas it is as high as 1.490 in 2002. The average value across all election cycles is 0.743, 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.[9] 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 elections near the threshold. As explained previously, the requirement for identification is only that banks are not fully able to predict close election outcomes (Lee, 2008). Complete predictive power is clearly inconsistent with the data as banks would then only donate to winning candidates. We also follow the test suggested by Lee (2008) to verify the validity of the identifying assumption. In our context, this test requires comparing (observable) characteristics of banks that contribute to close-election winners with the ones that contribute to close-election losers. Panel B of Table 1 shows that there are no statistically significant differences between both groups, thus confirming the identifying assumption.

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 pre-determined 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. The deposit data are sourced from the Summary of Deposits database provided by the Federal Deposit Insurance Corporation (FDIC). Our regional indicator

[9] 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.

for an election cycle is then obtained by summing the political shocks of all BHCs active in that region, weighting 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.[10] Panel C of Table 1 reports an average value for $NetCloseWins_{cr}$ of 0.635, with again wide variation across election cycles (see also Figure 1). Importantly, the standard deviations reported in Panel C also show that there is 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:

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

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

We next 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 capital shocks on aggregate economic outcomes.[11] The specification is given by:

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

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,

[10] The variation in the $NetCloseWins_{cr}$ indicator only arises from banks contributing to close elections, as 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.

[11] Standard difference-in-differences models are characterized by a treatment group and a control group. Here, both groups are treated: one group receives a positive shock, while the other group receives a negative shock.

and $Election_{ct}$ is a dummy variable that takes the value of one on the two years following the election year, and zero in the two years preceding the election cycle c. As we illustrate in Figure 2, this means that the years (*t=-1* and *t=0*) corresponding to the election cycle c under consideration are excluded.[12] 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 nationwide temporal variation (and thus sweep away the $Election_{ct}$ 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 among the two groups prior to the election years.

The second part of our study also examines lending decisions by banks. We thus specify a version of equation (1) at the bank-year level:

$$Y_{bt} = \alpha + \beta NetCloseWins_{bc} \times Election_{ct} + \gamma Controls_{bt} + \eta_{bc} + \mu_t + \varepsilon_{bct}. \qquad (2)$$

Here Y_{bt} is a measure of either the issuance or the pricing of loans, $NetCloseWins_{bc}$ the BHC-level shock to political capital, and $Controls_{bt}$ 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 indices and parameters are defined as in equation (1). Standard errors are clustered at the BHC level.

[12] We exclude the years of the election cycle under consideration to cleanly compare output growth before and after the election year. Including the years of the election cycle has, however, no material incidence on our results.

3. 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. The exact variable definitions can be found in Table A1 of the Appendix and the summary statistics for all variables are in Table 2.

3.1. Economic activity

To study regional economic activity, we use data from the US 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 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 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 in 2001-2017.[13] Our analysis primarily focuses on MSAs because, as economically integrated areas, they are likely to be affected by the same regional shocks.[14] Our final main sample consists of 378 unique MSAs (in sub-section 4.2 we also consider county-level data).

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

[14] 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 less than 50,000 inhabitants. Both statistical areas include one or more counties, and some contain counties from several states (e.g., 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 US territories and only contains MSAs because GDP data are not available for micropolitan statistical areas.

3.2. 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 (see, e.g., Bord, Ivashina, and Taliaferro, 2018, for a more comprehensive description of CRA data). 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 extent 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 two years, as in Cortés et al. (2020). We also use loan value that we define as the total $ amount of new loans originated per year in each MSA by BHCs. The resulting sample covers 366 MSAs over the period 2000-2016.

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 US.[15] We use detailed information on syndicated loans from the Thomson Reuters LPC Dealscan database. We use data on loan contract facility, 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 match our Dealscan data with BHC-level data from the Federal Reserve FR Y-9C filings. From the latter source, we extract data on total assets, return on assets, liquidity ratio, non-performing 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 (in case multiple banks have the same highest capital allocation we drop the

[15] 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), Keil and Müller (2020).

observation). We only use syndicated loans to firms in the US. 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 (271 out of 499 BHCs). For some specifications in our analysis on 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.

3.3. 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,[16] 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 deposit institutions in SDI. For banks without a BHC, we treat them as individual banks.[17]

Then, we measure banks' political connectedness by focusing on contributions to politicians running for office in the US House of Representatives or the US 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 PAC contributions to election PACs our measure of a bank's political connectedness.

[16] We require the entity to: (*i*) have positive values for total assets; (*ii*) be either a BHC or a thrift holding company; (*iii*) be a corporation as legal structure; (*iv*) have as charter type either a holding company or a securities broker/dealer (except for Goldman Sachs, Morgan Stanley, Ally Financial, American Express); (*v*) not be a grandfathered savings and loan holding company; and (*vi*) not be a lower-tier holding company whose parent also files FR Y-9C.
[17] We require the entity to: (*i*) have positive values for total assets; (*ii*) have non-missing RSSD ID (a unique identifier assigned to financial institutions by the Federal Reserve); and (*iii*) be not covered by FR Y-9C.

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. Our approach to identify close-election candidates is similar to Akey (2015), Akey and Lewellen (2017), Heitz, Wang, and Wang (2019), and Do et al. (2020). 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 276 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.[18] This matching process leads to 499 matches between BHCs and close-election PACs (see Panel A of Table 1 for a breakdown per election cycle). The BHCs in the sample contributed a total of $1.52 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.

4. Results – Political Connectedness and Economic Activity

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

4.1. Output growth

Table 3 reports the coefficients of regression models derived from equation (1) using GDP growth as dependent variable in columns 1-3 and private sector GDP growth in columns 4-6. 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

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

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 0.57-0.58 pp increase in annual GDP growth.[19] In column 3, we decompose our $NetCloseWins_{cr}$ indicator into close wins and close losses indicators (as defined in Section 2). 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 variation in regional exposure to both close election outcomes.

We now exclusively focus on the private sector to investigate whether our effects are driven by public sector spending by incumbent politicians. In columns 4-6, 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 1-3: the coefficient β is positive and statistically different from zero across specifications. From columns 4 and 5, the effect is economically similar, indicating that annual private sector GDP growth increases by 0.54-0.56 pp following positive regional political capital shocks. And, from column 6, we still observe that the loadings on the close wins and close losses indicators are roughly symmetric in size and opposite in sign. 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 $Election_{ct}$ dummy variable with five dummy variables, $Election_{ct^{-(+)n}}$, taking the value of one on the n^{th} year before (after) the year t of the election cycle c, and zero otherwise. The $Election_{ct^{-n}}$ 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 on the $Election_{ct^{-n}}$ dummy variable is

[19] For ease of interpretation we consider a one unit change throughout the text. This is also close to the standard deviation of $NetCloseWins_{cr}$, which is 0.891 (see Table 2).

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. It thus appears that the growth effect is a temporary one as it vanishes in the years thereafter. This temporary effect is also illustrated in Figure 3, which compares output growth in MSAs with positive and negative shocks.

4.2. 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 5 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) and preferential bailouts (Duchin and Sosyura, 2012), subsequently affecting aggregate output (Gropp et al., 2020). Excluding election cycles overlapping with the banking crisis yields even stronger results to our baseline results.

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, it rules out a demand-side interpretation of our results (local households/firms may change their behaviour after a close election in their state). We observe in column 2 that our results do not change materially, although the size of the coefficient β is slightly smaller than in the baseline. The results in column 2 also suggest that political connections do not exclusively matter at the local level where lending takes place and that, in the BHC context, investment in political capital effectively works at the federal level. This is consistent with at least some extent of centralized decision-making at BHCs.

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 very 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 0.44 pp increase in annual, county-level GDP growth. In column 5, 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 4, we show our placebo tests that randomly perturb components of our indicator of interest. We construct 1,000 placebo samples that randomize close election outcomes and re-run the same specification as in column 5 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 during the same election cycle. 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 $NetCloseWins_{bc}$ 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 4 show that the coefficients (histogram on the left) and *t*-statistics (histogram on the right) on our placebo versions of the interaction term, $NetCloseWins_{cr} \times Election_{ct}$, 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.

4.3. Allocative efficiency and productivity

What is the source of the temporary increase in output growth? We now turn to answer this question, and investigate 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 sub-section, 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.

The results are presented in Table 6. This table uses the specification of column 5 of Table 3 first replacing GDP growth with proxy variables for allocative efficiency. We find that political capital shocks do not affect entry of new establishments to the market, but lower exit of establishments. 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 US 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, and find that the coefficient of interest, β, is indistinguishable from zero. 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 leads to a 0.14 pp decrease in establishment exit rate. This corresponds to a decrease in exit rate of about 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 analyse 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 extensive and intensive qualifies this finding. As can be seen in column 4, the coefficient of interest, β, is negative and statistically different from zero at the 10% level, while in column 5, β is positive and statistically different from zero at the 1% level. In economic terms, a one unit increase in our MSA-level indicator of shock to banks' political capital is associated with a 0.06 pp reduction in job creation by new entrants (extensive margin) and a 0.10 pp increase in job creation by incumbents (intensive margin). 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 sub-section 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., fuelling creative destruction).

5. 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.

5.1. 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 analyse loan issuance volume directed toward both small and large businesses. Our analysis on small loans uses MSA-level data, while our analysis on larger (syndicated) loans uses loan-level information. Together, our analyses speak to a very significant fraction of the total corporate loan issuance in the US 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 1.09 pp in MSAs where banks experience a positive shock to their political capital. This is a sizable magnitude relative to the unconditional mean of -1.18% and the standard deviation of 20.31% reported in Panel A of Table 2. We obtain very consistent results in column 2 where we use loan value as dependent variable.

Table 8 turns to the BHC-level results on syndicated loans. We regress loan issuance by a given BHC in a given MSA and year on the $NetCloseWins_{bc}$ indicator interacted with the $Election_{ct}$ dummy variable, as specified in equation (2). In the regressions, we also control for a host of bank-level characteristics (namely, size, earnings, liquidity, non-performing loans, capital adequacy) and fixed effects (namely, BHC-election cycle and year).[20] The results illustrate that, consistent

[20] The definitions of the control variables can be found in Table A1.

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 10.86% (from column 1) for each unit increase in our bank-level indicator of shock to political capital, while the total facility amount increases by 12.95% (column 2).[21]

5.2. 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? In the politics and finance literature, Igan, Mishra, and Tressel (2012) and Kostovetsky (2015) show, among several others, that banks take on more risk due to moral hazard. The favorable treatment banks enjoy thanks to their political connections partially isolates them from the negative consequences of risk taking, leading them to originate riskier loans.[22] In this sub-section, we attempt to answer the aforementioned question by examining loan pricing and borrower attributes.

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 $Election_{ct}$ 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. We cluster standard errors at the BHC level.

The estimate of the coefficient on 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

[21] We obtain the 10.86% increase using the coefficient of column 1 and the mean reported in Panel B of Table 2 (11.68/107.55 = 10.86), and the 12.95% increase from the coefficient reported in column 2 and the mean in Panel B of Table 2 (4.70/36.28 = 12.95).
[22] See Footnote 4 for examples of favorable treatments typically granted to politically connected banks.

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 on the $Election_{ct}$ dummy variable indicates that loans are 23.10 basis points (bps) more expensive following close elections regardless of the political capital shock received by BHCs.[23] Then, the coefficient on the interaction term, β, is negative and statistically different from zero, with a magnitude of -5.75. 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 5.75 bps. Relative to the general increase in loan spreads after close elections of 23.10 bps, this corresponds to a drop of more than 20%.

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 $Election_{ct}$ 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 $Election_{ct}$ dummy variable shows up indistinguishable from zero.

In the remaining columns, we further exploit borrower heterogeneity to test whether cheaper lending conditions are directed toward riskier borrowers. To test whether differences in terms of risk across borrowers are important mediators, 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 $NetCloseWins_{bc}$ indicator and the $Election_{ct}$ dummy variable on borrower characteristics. From columns 3 and 4, we uncover that interest rate decreases are indeed concentrated in riskier

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

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 on 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 5.62 bps 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 2.29 bps decrease in spreads.

Overall, the effects documented in this sub-section—though less precisely estimated—suggest that BHCs receiving a shock to their political capital after close election outcomes charge lower interest rates, especially to riskier borrower firms. These findings are consistent with banks taking more risks due to moral hazard under the "favorable treatment" channel.

6. 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 US 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 0.56 pp when the banks active in the region experience a positive shock to their political capital due to close election outcomes. While this effect is economically sizable, we also find that it temporary, vanishing after one year from the election.

We then show that this growth effect is primarily due to relative sclerosis. There is fewer 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 more establishment entries as well as 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 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 things get bad).

Collectively, our findings reveal that, although the interference between banks and politicians appears beneficial for the US 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.

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Appendix

Table A1. Variable definitions and sources

Panel A: MSA-level variables

Variable	Definition	Source
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 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.	BDS
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* and year *t-1*.	FFIEC
Loan Value	Total amount (in $ million) 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 an election cycle *c* (see Section 2).	FEC and FDIC

CloseWins	An indicator variable measuring the number of close-election winners that BHCs contributed to during an election cycle *c*, weighted by their respective pre-determined market share in an MSA (see Section 2).	FEC and FDIC
CloseLosses	An indicator variable measuring the number of close-election losers that BHCs contributed to during an election cycle *c*, weighted by their respective pre-determined market share in an MSA (see Section 2).	FEC and FDIC
Election	A dummy variable equal to one on the two years following the election year, and zero in the two years preceding the election year (see Figure 2).	Own calculation

Panel B: Bank- and loan-level variables

Variable	Definition	Source
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 loan facilities lent by BHC *b* in year *t*.	Dealscan
Size	The log of total assets.	Fed and FDIC
ROA	The ratio of net income over total assets.	Fed and 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 and 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 and FDIC
Tier1	Tier 1 risk-based capital ratio.	Fed and 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 non-investment 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 $ million.	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
Political connections		
NetCloseWins	An indicator variable measuring the shock to a BHC's *b* political capital during an election cycle *c* (see Section 2).	FEC
Borrower NetCloseWins	An indicator variable measuring the shock to a firm borrower's political capital during an 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

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 marks an outliers. The red horizontal line shows 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.

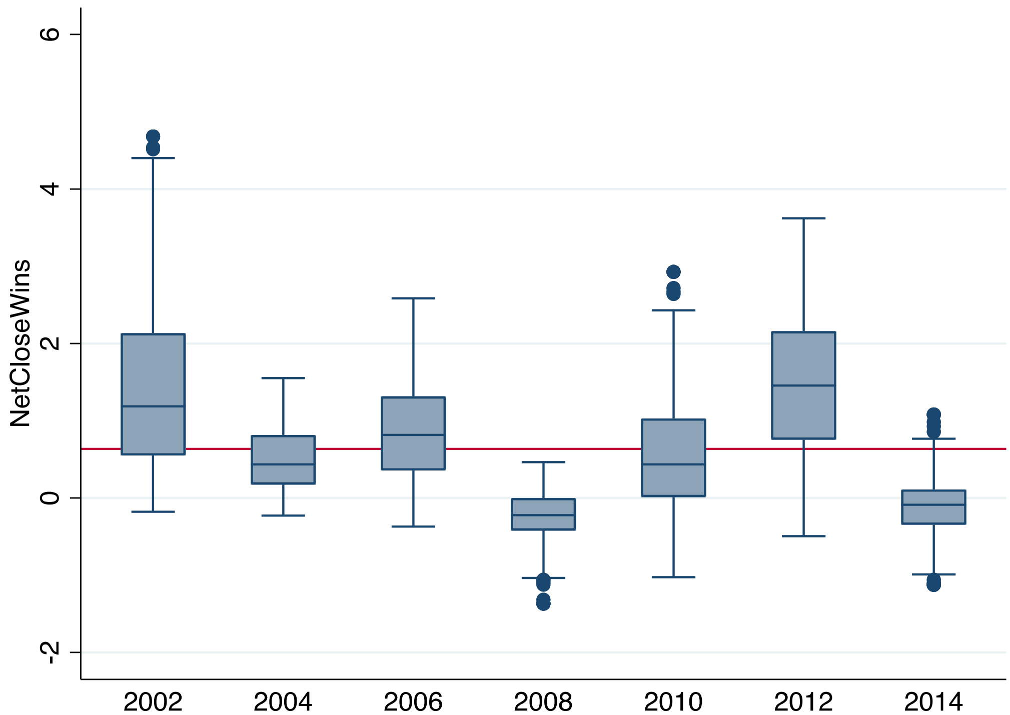


Figure 2. Illustration of the empirical strategy

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

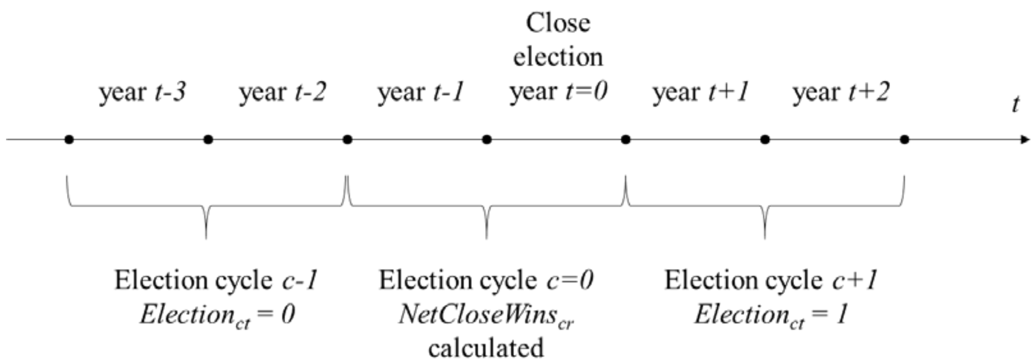


Figure 3. Output growth around close elections

This figure shows in red (blue) the average private sector GDP growth 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 bars around the dots are the 95% confidence intervals. Observations in the recession year 2009 are dropped for clarity purpose, but do not change the overall pattern shown in this figure. Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.

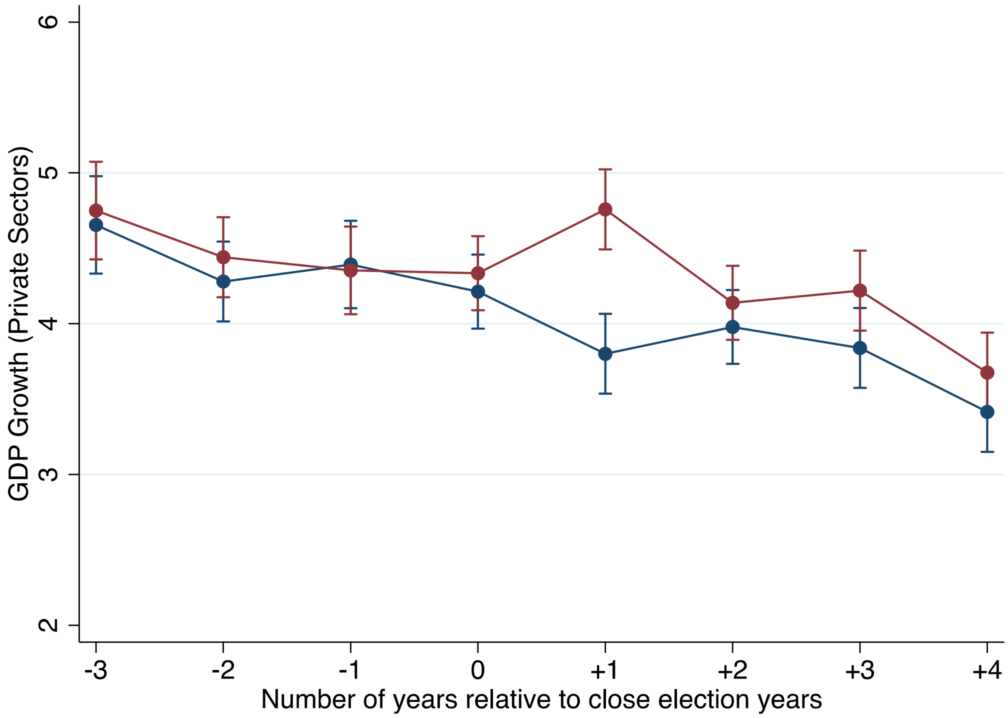
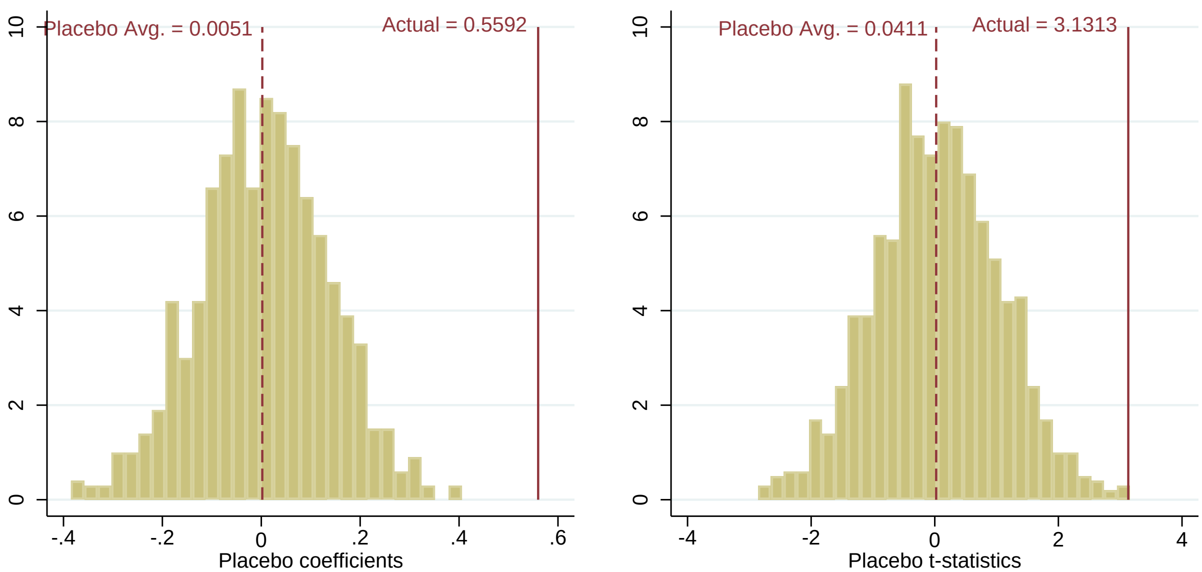


Figure 4. Distribution of estimated coefficients and t-statistics across placebo samples

This figure shows the distribution of estimated coefficients (left) and t-statistics (right) when we run the specification in column 5 of Table 3 for 1,000 placebo samples that randomize close election outcomes. In Pane 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, $NetCloseWins_{cr} \times Election_{ct}$. 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 5 of Table 3 using actual close election outcomes (solid line). Variables (defined in Table A1) are winsorized at the 1st and 99th percentiles.

Panel A: Random assignment of $NetCloseWins_{cr}$ to MSAs



Panel B: Random assignment of $NetCloseWins_{bc}$ to BHCs

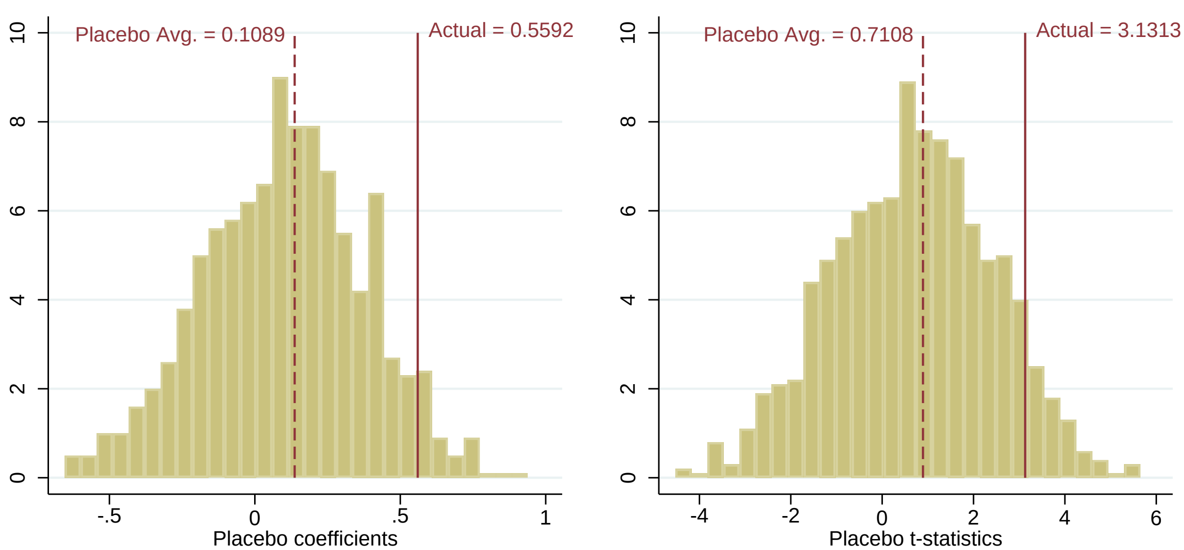


Table 1. Statistics for the political variables

This table presents summary statistics for the political 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 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.

Panel A: Bank-level political variables

	N	Mean	Std	p25	p50	p75
Across election cycles						
NetCloseWins	499	0.743	2.019	0.000	1.000	1.000
CloseWins	499	3.034	3.185	1.000	2.000	4.000
CloseLosses	499	2.291	2.564	0.000	1.000	3.000
By election cycles						
NetCloseWins in 2002	102	1.490	2.043	1.000	1.000	2.000
… 2004	59	1.186	1.537	0.000	1.000	2.000
… 2006	77	0.351	2.070	-1.000	0.000	1.000
… 2008	55	-0.400	1.606	-2.000	0.000	1.000
… 2010	68	0.868	2.164	-1.000	1.000	2.000
… 2012	67	0.970	2.037	0.000	1.000	2.000
… 2014	71	0.282	1.876	-1.000	0.000	1.000

Panel 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)

Panel C: MSA-level political variables

	N	Mean	Std	p25	p50	p75
Across election cycles						
NetCloseWins	2630	0.635	0.944	-0.010	0.409	1.146
By election cycles						
NetCloseWins in 2002	376	1.417	1.115	0.549	1.186	2.135
… 2004	371	0.515	0.429	0.174	0.434	0.816
… 2006	377	0.862	0.645	0.355	0.816	1.316
… 2008	375	-0.249	0.329	-0.422	-0.221	0.000
… 2010	378	0.540	0.739	0.010	0.436	1.030
… 2012	376	1.478	0.922	0.754	1.457	2.161
… 2014	377	-0.118	0.390	-0.348	-0.089	0.112

Table 2. Summary statistics

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.

Panel A: MSA-level variables

	N	Mean	Std	p25	p50	p75
Economic activity						
GDP Growth	9397	3.852	4.262	1.487	3.786	6.130
GDP Growth (Private Sectors)	9397	3.873	4.894	1.225	3.782	6.438
Per Capita GDP Growth (Private Sectors)	9397	2.917	4.700	0.390	2.981	5.331
Establishment Entry Rate	8326	10.245	2.311	8.600	10.000	11.600
Establishment Exit Rate	8326	9.188	1.494	8.100	9.000	10.000
Job Creation Rate	8326	14.049	2.959	11.900	13.700	15.900
Job Creation Rate by Births	8325	5.041	1.632	3.900	4.800	6.000
Job Creation Rate by Continuers	8325	8.986	1.795	7.700	8.900	10.100
Job Destruction Rate	8326	13.107	2.614	11.300	12.800	14.500
Job Destruction Rate by Deaths	8325	4.089	1.295	3.200	3.900	4.700
Job Destruction Rate by Continuers	8325	9.002	1.861	7.700	8.800	10.100
Reallocation Rate	8326	24.415	4.306	21.400	24.000	27.000
Wage Growth	9768	2.847	1.715	1.776	2.847	3.900
Patent Growth	9241	8.247	41.992	-15.385	0.000	22.424
Population Growth	9768	0.934	1.007	0.231	0.796	1.462
Total Deposits	9768	15.273	1.282	14.342	14.948	15.887
Number of Branches	9768	4.516	1.015	3.807	4.277	5.050
Corporate lending (CRA)						
Loan Growth	8638	-1.177	20.306	-9.841	1.420	10.276
Loan Value	8638	402.585	540.096	105.216	193.786	435.715
Political connections						
NetCloseWins	9768	0.574	0.891	-0.034	0.361	1.092
CloseWins	9768	2.506	1.677	1.190	2.211	3.570
CloseLosses	9768	1.940	1.270	0.929	1.764	2.762

Panel B: Bank- and loan-level variables

	N	Mean	Std	p25	p50	p75
Corporate lending (Dealscan)						
Number of Loans	1004	107.552	230.787	1.000	8.000	85.000
Facility Amount	1004	36.277	102.427	0.015	0.706	11.194
Size	1004	18.099	1.662	16.988	18.177	19.095
ROA	1004	3.670	3.603	2.315	4.067	5.511
Liquidity	1004	27.407	14.493	17.868	23.615	32.312
NPL	1004	2.793	2.649	1.063	1.781	3.564
Tier1	1004	10.187	4.452	7.988	9.448	11.037
Interest rate spread	73281	249.037	144.972	150.000	225.000	325.000
Junk Borrower	15271	0.527	0.499	0.000	1.000	1.000
Political connections						
NetCloseWins	1004	0.811	2.060	-1.000	1.000	2.000
Borrower NetCloseWins	4224	0.634	2.433	-1.000	0.000	2.000

Table 3. Output growth

This table documents the effects of political capital shocks on regional output growth. Columns 1-3 present the difference-in-differences model (illustrated in Figure 2) using *GDP Growth* as dependent variable. Columns 4-6 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. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	GDP Growth			*GDP Growth (Private Sectors)*		
NetCloseWins × Election	0.5728***	0.5802***		0.5393***	0.5592***	
	(3.6873)	(3.7705)		(3.0402)	(3.1313)	
CloseWins × Election			0.4965***			0.4569**
			(3.0123)			(2.3801)
CloseLosses × Election			-0.4653**			-0.4158*
			(-2.2924)			(-1.7571)
Population Growth		1.0859***	1.0889***		1.1377***	1.1411***
		(7.5941)	(7.6079)		(7.0649)	(7.0791)
Total Deposits		-1.9368***	-1.9098***		-2.4178***	-2.3964***
		(-3.6533)	(-3.5414)		(-3.8645)	(-3.7663)
Number of Branches		-1.6491*	-1.6600*		-1.4027	-1.4199
		(-1.6660)	(-1.6774)		(-1.2294)	(-1.2449)
Adj. R-squared	0.243	0.269	0.269	0.212	0.235	0.234
N	9397	9397	9397	9397	9397	9397
MSA × Election Cycle FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 4. Output growth dynamics

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 *Election* (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 the election cycle *c* under study (t-1 and $t=0$) are dropped. The interaction term including *Election* (year $t \leq -3$) as well as the *Election* (year $t \leq -3$) 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. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	GDP Growth (Private Sectors)
NetCloseWins × Election (t-2)	-0.0919
	(-1.1680)
NetCloseWins × Election (t+1)	0.2686***
	(3.5233)
NetCloseWins × Election (t+2)	0.0496
	(0.6274)
NetCloseWins × Election (t+3)	0.1155
	(1.1721)
NetCloseWins × Election (t≥+4)	-0.0002
	(-0.0028)
Election (t-2)	0.0309
	(0.7695)
Election (t+1)	-0.1706***
	(-3.7427)
Election (t+2)	-0.0364
	(-0.8042)
Election (t+3)	-0.0958
	(-1.4610)
Election (t≥+4)	-0.0130
	(-0.3071)
Population Growth	1.2832***
	(9.5591)
Total Deposits	-1.5458***
	(-2.8973)
Number of Branches	-2.6296***
	(-2.7585)
Adj. R-squared	0.212
N	30813
MSA × Election Cycle FE	Yes
Year FE	Yes

Table 5. Robustness tests

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, *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. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	GDP Growth (Private Sectors)				*Per Capita GDP Growth (Private Sectors)*
NetCloseWins × Election	0.7663***	0.4502***	0.5038***	0.4370***	0.5593***
	(3.7317)	(2.8054)	(2.8309)	(4.0214)	(3.1590)
Population Growth	1.2006***	1.1390***	1.1627***	0.6620***	0.0319
	(6.4473)	(7.0698)	(7.4611)	(8.2796)	(0.1829)
Total Deposits	-2.2302***	-2.3450***	-2.4110***	-4.7243***	-2.3959***
	(-3.3808)	(-3.8121)	(-3.8461)	(-8.6413)	(-3.8704)
Number of Branches	0.5378	-1.5228	-1.3705	0.2811	-1.2976
	(0.4076)	(-1.3221)	(-1.1988)	(0.4004)	(-1.1485)
Description	Exclude 2008-2010 election cycles	Non-local MSA *NetCloseWins*	Only election outcomes ≤ 1% margins	County-level specification	Specification as in column 5 of Table 3
Adj. R-squared	0.231	0.235	0.235	0.134	0.186
N	6385	9356	9364	50741	9397
MSA × Election Cycle FE	Yes	Yes	Yes	-	Yes
County × Election Cycle FE	-	-	-	Yes	-
Year FE	Yes	Yes	Yes	Yes	Yes

Table 6. Allocative efficiency and productivity

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. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Establishment Entry Rate	*Establishment Exit Rate*	*Job Creation Rate*	*Job Creation Rate by Births*	*Job Creation Rate by Continuers*	*Job Destruction Rate*	*Job Destruction Rate by Deaths*	*Job Destruction Rate by Continuers*	*Reallocation Rate*	*Wage Growth*	*Patent Growth*
NetCloseWins × Election	0.0344	-0.1398***	0.0365	-0.0583*	0.1045**	-0.2061***	-0.0624*	-0.1551***	-0.1329	0.0683	0.4222
	(1.2706)	(-4.8065)	(0.648)	(-1.8665)	(2.1916)	(-3.0369)	(-1.7948)	(-2.8830)	(-1.3382)	(1.2387)	(0.3733)
Population Growth	0.3400***	-0.3449***	0.6540***	0.1704***	0.4625***	-0.5454***	-0.1484***	-0.4195***	-0.1423	0.1629***	1.8977
	(9.2488)	(-9.6682)	(8.7995)	(5.3860)	(9.2379)	(-8.3508)	(-3.8897)	(-9.3785)	(-1.5860)	(3.3754)	(1.3868)
Total Deposits	0.3749***	0.1074	-0.2095	0.0695	-0.3111**	0.3758*	-0.0229	0.3729*	-0.4284	-0.4000**	2.1295
	(2.6712)	(1.1958)	(-0.9668)	(0.4977)	(-2.3592)	(1.6543)	(-0.2515)	(1.8990)	(-1.1435)	(-2.0357)	(0.5895)
Number of Branches	-0.6306***	0.2868	-0.6163	-0.6222**	-0.1475	-0.0742	-0.4016	0.3595	-0.4865	-0.4514	20.3051**
	(-2.6807)	(1.4149)	(-1.2240)	(-2.1632)	(-0.4009)	(-0.1613)	(-1.3774)	(0.9411)	(-0.6522)	(-1.2282)	(2.1541)
Adj. R-squared	0.918	0.814	0.699	0.633	0.547	0.606	0.450	0.549	0.665	0.385	-0.053
N	8326	8326	8326	8325	8325	8326	8325	8325	8326	9768	9241
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

Table 7. Loan issuance – CRA data

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. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
	Loan Growth	*Loan Value*
NetCloseWins × *Election*	1.0885**	6.5041*
	(2.0228)	(1.7017)
Population Growth	1.1732**	2.4401
	(2.5344)	(0.5589)
Total Deposits	-0.0741	36.2051**
	(-0.0430)	(2.0445)
Number of Branches	-10.3789***	17.8792
	(-2.7226)	(0.5557)
Adj. R-squared	0.447	0.968
N	8563	8563
MSA × Election Cycle FE	Yes	Yes
Year FE	Yes	Yes

Table 8. Loan issuance – Dealscan data

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 one 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 1^{st} and 99^{th} percentiles. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) *Number of Loans*	(2) *Facility Amount*
NetCloseWins × *Election*	11.6789**	4.6952**
	(2.1826)	(2.0476)
Size	2.2192	2.4824
	(0.1767)	(0.4474)
ROA	-1.7618	-0.8473
	(-0.9040)	(-1.0057)
Liquidity	0.2309	0.3915
	(0.4715)	(1.4304)
NPL	1.2378	0.9225
	(0.4857)	(0.5286)
Tier1	2.0455	1.3648*
	(1.2653)	(1.7474)
Adj. R-squared	0.911	0.892
N	1000	1000
BHC × Election Cycle FE	Yes	Yes
Year FE	Yes	Yes

Table 9. Loan pricing

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 $Election_{ct}$ 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 $Election_{ct}$ 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. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Interest rate spread			
NetCloseWins × Election	-5.7542**	-4.4563**	-2.0203*	-3.5554***
	(-2.3795)	(-2.1553)	(-1.9387)	(-2.8880)
Elections	23.0993***	17.0722**	21.1098***	20.6608***
	(6.0666)	(2.6197)	(6.8980)	(3.9428)
Borrower NetCloseWins		-0.4336		
		(-0.5770)		
Borrower NetCloseWins × Election		0.7329		
		(1.4637)		
Junk Borrower			71.6733***	
			(13.3739)	
Junk Borrower × Election			-6.8709	
			(-1.5023)	
NetCloseWins × Junk Borrower			4.6097***	
			(3.4241)	
NetCloseWins × Junk Borrower × Election			-5.6172***	
			(-2.9326)	
Small Borrower				7.9322
				(1.1844)
Small Borrower × Election				-1.3401
				(-0.4873)
NetCloseWins × Small Borrower				2.1169*
				(1.9891)
NetCloseWins × Small Borrower × Election				-2.2881**
				(-2.1755)
Description	Baseline	Borrower net connections	Borrower risk	Borrower size
Adj. R-squared	0.374	0.482	0.491	0.392
N	73256	3722	14073	30448
Loan-level control variables	Yes	Yes	Yes	Yes
BHC × Election Cycle FE	Yes	Yes	Yes	Yes