Political Shifts, Mortgage Drifts: Evidence from U.S. Special Elections*

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Abstract

This paper investigates how partisan turnover in U.S. congressional special elections affects mortgage lending. Using HMDA data from 1990–2023 and party-switching special elections as shocks to local political control, I show that banks aligned with the incoming party expand credit in the affected constituency: origination volume rises 9.6% at the state level and 14.5% at the district level in the election year. The credit expansion is unlikely to be driven by local demand conditions or investment opportunities. This lending response suggests a quid pro quo channel: aligned banks face lower enforcement intensity and capture more municipal-bond underwriting business after turnover. Mortgage lending also tilts toward conventional, minority, and investor loans, indicating higher risk exposure.

Keywords: Special Elections, Partisan Alignment, Political Influence, Mortgage Lending, Credit Risk Exposure

JEL Classification: D72, G21, G28

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1 Introduction

A large body of research documents that politicians shape credit allocation when they possess formal levers over banks. In settings with state ownership or comparable administrative control, elected officials have incentives to expand credit in ways that mitigate social discontent and thereby strengthen reelection prospects, particularly around election periods (e.g., Sapienza, 2004; Dinç, 2005; Khwaja and Mian, 2005). Because much of this evidence comes from environments with ownership rights, appointment authority, or explicit command-and-control mechanisms, a common inference is that political influence should be attenuated in private banking systems where such levers are absent.

However, this interpretation treats political influence as synonymous with direct control. If politicians cannot appoint managers or explicitly instruct whom to lend to, the presumption is that privately owned banks must not be meaningfully subject to political influence. The premise of this paper is that such a view is incomplete. Even under private ownership, incumbents retain the same reelection incentives and remain concerned with local economic conditions. At the same time, they also own the institutional means to pursue this objective by shifting the expected costs and benefits of lending through regulatory design, supervisory oversight, and enforcement intensity. Together, these features preserve both the motivation and the ability to affect credit allocation, implying that private banks may still need to adapt to the political landscape even in the absence of direct control.

Banks, in turn, may respond to this regime by cultivating relationships with officeholders. Such relationships could reflect ideological proximity, but they could also be motivated by forward-looking interests in a regulatory and supervisory environment in which elected officials play a central role. Consistent with this concept, privately owned banks with political connections have been shown to receive more favorable policy treatment, including relaxed

scrutiny and reduced enforcement (e.g., Lambert, 2018; Akey et al., 2021; Papadimitri et al., 2021). What remains uncertain is whether these relationships, as captured by banks' partisan positioning, translate into changes in credit supply when political representation changes, rather than simply tracking shifts in local economic fundamentals.

This uncertainty prompts a natural question: when political control turns over, do banks aligned with the incoming party adjust their credit supply accordingly? I study this question in the context of U.S. bank lending, leveraging partisan turnover in congressional representation as a shock to political alignment between banks and newly elected lawmakers. The core empirical challenge is to isolate variation in partisan turnover that is plausibly orthogonal to banks' local lending opportunities. To address this challenge, I focus on special elections that result in party switches. These contests are triggered by extraordinary events and conducted under fast-track electoral procedures, making outcomes unusually difficult to forecast in low-turnout, off-cycle environments.

Two threats to identification are immediate. First, party switches could be correlated with local political and economic conditions rather than representing plausibly exogenous turnover. Second, even if turnover is orthogonal to local lending opportunities, banks that become aligned with the incoming party may differ from non-aligned banks in ways that produce differential pre-election lending trends. I alleviate such concerns by testing whether pre-election political and economic indicators predict party switches and by using a dynamic event-study design to assess differential pre-trends around party-switching elections; I find no evidence that observables predict switches and no indication of anticipatory or pre-existing differences in lending dynamics across aligned and non-aligned banks.

To measure lending behavior, I draw on Home Mortgage Disclosure Act (HMDA) data from 1990 to 2023, aggregated to the bank–region–year level. Housing finance serves as a

natural setting for this exercise because it is politically salient, heavily regulated, and cleanly maps onto congressional constituencies (states for Senate contests and congressional districts for House contests).[1] Mortgage credit has also been found to matter for electoral outcomes (Antoniades and Calomiris, 2020). I use political action committee (PAC) contribution data to infer banks' partisan identity and define alignment based on whether that identity matches the party of the newly elected lawmaker after the special election.

I estimate a staggered difference-in-differences (DiD) model around party-switching special elections to compare the lending response of aligned and non-aligned banks. The results indicate a sizable alignment effect in both chambers: in the Senate, aligned banks increase lending amounts by 9.6% more than non-aligned banks in the year of the switch, with the effect persisting at 8.7% in the following year; loan counts rise by 6.5% in the event year and remain elevated by 6.9% the year after. In the House, lending amounts increase by 14.5% and loan counts by 13.4% in the event year. These results cannot be explained by observable local fundamentals or standard proxies for credit demand, and they remain robust to richer fixed effects, placebo tests, and alternative estimators designed for staggered treatment timing and heterogeneous effects (Sun and Abraham, 2021).

These lending responses lead to a follow-on question: why do aligned banks expand credit more sharply following partisan turnover? One explanation is an ideological optimism channel: once aligned with the incoming party, banks may raise their expectations about local economic prospects under the new leadership and increase lending accordingly. Another is a quid pro quo channel, in which banks view politically aligned lending as a means of secur-

[1] Housing finance in the United States has been repeatedly shaped by federal interventions and supervisory regimes, including the New Deal-era creation of institutions such as the Federal Home Loan Bank System, the FHA, and Fannie Mae; postwar VA guarantees; the development of Ginnie Mae and Freddie Mac; and, after the 2008 crisis, expanded oversight through agencies such as the FHFA and the CFPB. Statutes such as the Community Reinvestment Act further underscore the explicitly policy-driven nature of mortgage credit allocation.

ing reciprocal benefits. I evaluate these competing explanations in the Senate setting and find patterns that are difficult to reconcile with optimism-driven belief updating, pointing instead to an exchange-based interpretation. Following party-switching Senate special elections, aligned banks face fewer federal enforcement actions and, separately, gain additional municipal underwriting business in the state. Together, these results suggest that banks treat mortgage lending as an investment in political capital, with benefits materializing through supervisory outcomes and public-sector deal flow.

In light of this arguably transactional bank–politician nexus, an outstanding question is whether politically responsive credit expansion alters the risk profile of banks' mortgage portfolios. To the extent that aligned banks extend credit for reasons not accounted for by observable local conditions, marginal originations may be of lower credit quality. I assess this scenario by examining lending across higher-risk mortgage categories. After special-election turnover in both chambers, aligned banks skew their lending toward conventional mortgages, which lack federal guarantees and thus raise lenders' default exposure. In the Senate setting, aligned banks also scale up lending to minority applicants, a segment historically associated with higher delinquency. In the House setting, aligned banks increase lending for non-owner-occupied properties, where repayment relies more on rental income and investor cash flow. Overall, partisan turnover appears to affect not only the level of mortgage credit supplied by aligned banks, but also its composition along observably riskier margins.

Related Literature This paper contributes to three related bodies of literature on political connections and firm behavior, political incentives in the financial sector, and electoral cycles in bank lending. In particular, this study introduces U.S. congressional special elections as a source of abrupt, plausibly exogenous partisan turnover and examines how that turnover reshapes bank credit allocation through banks' partisan alignment with incoming legislators.

Existing work shows that political connections and partisanship shape firms' access to public resources, influence corporate decisions, and tilt market valuations. Firms' eligibility for government assistance often reflects ties to the ruling party, affecting the likelihood, timing, and structure of such interventions (Brown and Dinç, 2005; Faccio et al., 2006; Goldman et al., 2013; Adelino and Dinc, 2014; Akey, 2015; Duchin and Hackney, 2021; Igan et al., 2025). Related evidence documents that political connections are associated with more favorable bank-loan terms for connected borrowers (Charumilind et al., 2006; Houston et al., 2014; Zhou, 2023). Political ties also impact labor and operational decisions around elections, as firms adjust their activities in ways that support incumbents' reelection efforts (Faccio and Hsu, 2017; Bertrand et al., 2018). Partisan biases extend to financial markets, where intermediaries and analysts treat firms differently based on perceived ideological proximity (Wintoki and Xi, 2020; Kempf and Tsoutsoura, 2021; Dagostino et al., 2023; Kempf et al., 2023). While prior studies emphasize the returns to political connectedness, I contribute by identifying an activation channel: special-election turnover renders banks' partisan positioning economically relevant, and thus yields differential lending responses.

The paper also relates to research on political dynamics and the financial sector, much of which focuses on the 2008–2009 crisis and examines how political connections affected policy responses and resource allocation during that period (Mian et al., 2010; Duchin and Sosyura, 2012; Mian et al., 2013). Related work studies the interaction between mortgage credit and political incentives in crisis-era settings (Agrawal et al., 2018; Chavaz and Rose, 2019). Departing from this crisis-bound focus, I study mortgage lending over a longer horizon, from 1990 to 2023. The results suggest that political incentives have an effect on mortgage credit allocation not only during crises, but also in more typical periods when policy and oversight remain salient.

This study is most closely connected to work on electoral cycles and politically motivated lending, particularly in settings where public and private banks coexist. Prior research finds that state-owned banks adjust credit allocation around elections, for example by offering favorable terms in regions aligned with the ruling party (Sapienza, 2004), expanding credit in contested areas near elections (Cole, 2009; Carvalho, 2014), increasing aggregate lending in election years (Dinç, 2005; Khwaja and Mian, 2005; Claessens et al., 2008), and boosting financing to home-state governments (Koetter and Popov, 2021). In contrast, this paper investigates special elections, a political event that, to my knowledge, has not been studied in the electoral-lending literature and that triggers more immediate lending responses. By exploiting the plausibly exogenous timing and outcome of party-switching special elections, the analysis more cleanly isolates the causal effect of partisan turnover on credit allocation. Finally, whereas much of the prior evidence comes from smaller or institutionally weaker economies where state influence in banking is pronounced, this paper shifts the focus to the United States, a developed democracy with a predominantly private banking sector.

Paper Outline The remainder of the paper is organized as follows: Section 2 introduces the institutional framework governing congressional vacancies and special elections. Section 3 describes the data and sample construction. Section 4 develops the empirical setup and identification approach. Section 5 reports the baseline estimates for Senate and House special elections and presents robustness checks. Section 6 evaluates mechanisms and investigates alternative explanations. Section 7 examines implications for risk-taking and lending standards. Section 8 concludes.

2 Institutional Framework

Vacancies in the U.S. Congress arise from resignation, death, expulsion, or related contingencies, and are filled through procedures that differ by chamber and by state law. These provisions generate special elections that occur off-cycle and typically on accelerated timelines relative to standard congressional contests. This section outlines the institutional rules governing the timing and administration of special elections in both the Senate and the House of Representatives, emphasizing features relevant to identification.

2.1 Congressional Vacancies and Replacement Rules

U.S. Senate Senate vacancies can be triggered by resignation, death, expulsion, or the Senate's refusal to seat a senator-elect. Under Article I, Section 3, Clause 2 of the Constitution, governors were initially authorized to make temporary appointments during legislative recesses, with state legislatures subsequently selecting replacements. The Seventeenth Amendment (1913) introduced direct elections for senators and required governors to call special elections to fill vacancies, while allowing state legislatures to grant governors authority to appoint interim senators until a permanent replacement is elected.

State rules governing interim appointments and election timing vary along two margins: whether governors may appoint an interim senator, and when a special election is held. In five states, governors are not permitted to appoint temporary senators.[2] These states instead rely on expedited special elections to reduce the time a seat remains unfilled, and when vacancies occur late in a term, they may defer selection until the next regularly scheduled general election. In the remaining 45 states that permit gubernatorial appointments, the

[2]The 5 states that prohibit gubernatorial appointments to fill Senate vacancies are: Kentucky, North Dakota, Oregon, Rhode Island, and Wisconsin.

timing of the subsequent election differs. In 34 states, governors appoint interim senators who serve until the next general election, at which point a special election determines a permanent replacement.[3] States differ in how they define "next general election," with some linking it to the next federal, statewide, or municipal general election, and others applying alternative timing criteria. When a vacancy arises close to a regularly scheduled election, the appointee may serve until the subsequent election cycle. By contrast, the remaining 11 appointment states require accelerated special elections, commonly scheduled within months of the vacancy, to limit the duration of appointed service.[4] When a vacancy coincides with an upcoming general election, the special election may be held concurrently.

Beyond timing, several states impose party-affiliation constraints on gubernatorial appointees to maintain partisan continuity. In these cases, governors might be required to appoint a replacement from the departing senator's party, either by selecting from a party-provided list or by appointing an individual with a documented history of affiliation with the predecessor's party.

U.S. House of Representatives House vacancies occur when an incumbent leaves office before a term ends, and special elections may also be required if a representative-elect dies before Congress convenes. Unlike the Senate, the House has no appointment mechanism. Article I, Section 2, Clause 4 requires that all House vacancies be filled exclusively through special elections, which governors are responsible for ordering.

House special-election timing is set by state law, which generally prescribes a specific

[3]The 34 states allowing gubernatorial appointees to serve until the next general election are: Arizona, Arkansas, California, Colorado, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Michigan, Minnesota, Missouri, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, South Carolina, Tennessee, Utah, Virginia, West Virginia, and Wyoming.

[4]The 11 states requiring accelerated special elections are: Alabama, Alaska, Connecticut, Louisiana, Massachusetts, Mississippi, Montana, South Dakota, Texas, Vermont, and Washington.

window or a maximum number of days after a vacancy for conducting the election. During the first session of a Congress, states typically align special elections with existing local or regional election dates, such as municipal or county elections, to streamline administration. During the second session, the scheduling is more directly shaped by the time remaining until the next general election. If a vacancy takes place within roughly six months of Congress's end, some states leave the seat vacant, while others hold the special election concurrently with regular primaries and general elections.

Nomination procedures in House special elections vary substantially. Some states use primaries to determine party nominees, while minor-party and independent candidates typically qualify through petitioning. Other states rely on internal party selection mechanisms, such as caucuses or conventions. A small number employ distinctive formats, including "all-parties" primaries used in Louisiana and Texas, where all candidates compete on a single ballot and, if no candidate receives a majority, a runoff is held between the top two vote-getters.

2.2 Timing and Campaign Compression

These institutional rules translate into compressed timelines between vacancy and election, particularly relative to the 18 to 24 month campaign window typical of regular elections. In my sample, the median time between a Senate vacancy and its corresponding special election is approximately 10 months. For House special elections, where there is no interim appointment and where state laws often mandate rapid scheduling, the median vacancy-to-election gap is approximately 3.7 months. These shorter windows limit the scope for extended campaigning, coordinated voter mobilization, and extensive advertising, thereby distinguishing special elections from regularly scheduled congressional contests and contributing to their irregular electoral environments.

3 Data

3.1 Political Data: Special Elections to the U.S. Congress

I compile data on U.S. congressional special elections from the Federal Election Commission (FEC) Public Records Office and the MIT Election Data and Science Lab. I define 'PartySwitch' as an indicator equal to one for a constituency (state or congressional district) in a given year if a special election in that constituency results in a change in party control. I identify all Senate and House special elections held during the sample period 1990–2023 and code 'PartySwitch' accordingly.

Table 1 summarizes Senate and House special elections, their distribution across congressional cycles, and the reasons for their happening. Party switches occur in most congressional cycles. Figure 1 maps special-election incidence across states, with lighter shading indicating states that held special elections and darker shading indicating cases in which a special election resulted in a party switch. Figure 2 summarizes partisan changes resulting from special elections across congressional cycles in both chambers; party switches are roughly balanced across Democrats and Republicans.

3.2 Partisan Data: PAC Contributions

To classify banks' partisan identity, I utilize bank-level PAC contribution records from the Center for Responsive Politics (OpenSecrets), as reported to the FEC. Contributions are aggregated to the bank level for all federal elections from 1990 to 2023. In a given year, a bank is classified with the party receiving more than 60% of its contributions in the most recent two-year federal election cycle, following Kempf et al. (2023). If neither party exceeds the 60% threshold, the affiliation is set to missing for that year; I then carry forward the

most recent non-missing affiliation until it updates.[5]

3.3 Banking Data: HMDA Panel

I use mortgage origination data from the Home Mortgage Disclosure Act (HMDA) for 1990–2023. HMDA provides near-comprehensive coverage of mortgage applications and originations under prevailing reporting rules, including lender identifiers, property locations, loan amounts, application year, and origination outcomes. The data also contains borrower characteristics, including reported income and demographics.

The baseline sample includes all HMDA-reported originations across reporting institutions. I retain mortgages across types, purposes, lien status, property categories, and occupancy status, which I later use to characterize shifts toward observably riskier lending. Loans with incomplete geographic identifiers are excluded because precise location is required to map lending to congressional constituencies.

Table 2 reports the summary statistics. Panel A describes the bank–state–year sample used for Senate special elections, and Panel B describes the bank–congressional district–year sample used for House special elections. To account for time-varying congressional district boundaries, I use the county-to-congressional-district crosswalk developed by Ferrara et al. (2024), which assigns counties to districts using maps aligned with the nearest decennial Census at the start of each congressional term.[6]

[5] Results are robust to alternative thresholds of 50% and 55%.

[6] Congressional district boundaries are redrawn every ten years following the decennial U.S. Census to reflect population changes and maintain proportional representation, as required by Article I, Section 2 of the Constitution. This process begins with reapportionment, which allocates House seats among states based on population, followed by redistricting, where states adjust district lines to ensure equal representation. State legislatures usually oversee the latter task but, in some cases, it may be handled by independent commissions or other designated authorities.

3.4 Other Data

Enforcement Actions Bank enforcement actions are compiled from the public releases of three federal banking regulators: the Board of Governors of the Federal Reserve System (Federal Reserve), the Office of the Comptroller of the Currency (OCC), and the Federal Deposit Insurance Corporation (FDIC). These actions are formal supervisory interventions, ranging from written agreements and consent orders to civil money penalties, that impose corrective requirements and signal elevated regulatory scrutiny. I code enforcement activity across all severities at the bank holding company level and aggregate to the bank–year panel as a count of actions per year.

Municipal Bonds Municipal bond issuance data is obtained from SDC Platinum (Refinitiv Workspace), which provides deal-level information on underwriters, issuers, and bond characteristics. The analysis focuses on lead managers and summarizes their underwriting activity at the state–year level, capturing underwriting volume and fee revenues relative to overall issuance in the state. I also retain standard deal characteristics, including issuance size, credit ratings, offering type, yield, and maturity. Underwriting measures are aggregated to the bank–state–year level to align with the core panel.

Branch Network Branch-level data on the location and ownership of deposit-taking offices is drawn from the FDIC Summary of Deposits. I aggregate these records to the bank–region–year level and measure branch presence using the number of branches each bank operates in a given region–year, proxying for banks' local brick-and-mortar footprint.

Other Variables Local economic and housing-market controls come from the Bureau of Labor Statistics, the Census Bureau, and the Federal Housing Finance Agency. These

controls include population, the unemployment rate, median income, house price indices, and measures of housing utilization (homeownership and vacancy rates). State-level variables are used as reported, while county-level variables are aggregated to the congressional district level. Summary statistics for these variables are reported in Table 2. I also collect election characteristics, including election victory margins, the tenure of incumbents who vacated their seats, and voter turnout, from sources that include the Federal Election Commission, the Library of Congress, Vital Statistics on Congress, and the Office of the Clerk of the U.S. House of Representatives.

4 Empirical Framework

4.1 Identifying Assumptions

Special elections are triggered by unpredictable events, making their timing largely unforecastable ex ante. However, whether this unpredictability extends to their electoral outcomes as well is less clear. To evaluate the plausibility of treating party-switching special elections as exogenous political shocks, I proceed in two stages: (i) I summarize descriptive evidence that low and variable voter turnout limits ex ante forecasting in special elections, and (ii) I test empirically whether pre-vacancy regional economic or political conditions systematically predict party switches.

Special elections typically feature low and highly variable turnout, a point that pollsters, campaign professionals, and political analysts routinely emphasize as a source of outcome uncertainty.[7] Lower participation complicates forecasting because the realized electorate is

[7]See, for example, Philip Bump, "Who's Going to Win in Alabama? It's Almost Impossible to Say," *The Washington Post*, Dec. 4, 2017, on turnout variability and polling accuracy in the 2017 Alabama Senate special election; Jennifer Duffy, "Senate Bottom Line Revisions," *The Cook Political Report*, May

smaller and less representative, and projections hinge on assumptions about turnout composition and relative mobilization. In the sample from 1990 to 2023, Senate special-election turnout averages 11% lower than turnout in the preceding general congressional election, while House special-election turnout averages 49% lower. Figure 3 shows that turnout ratios also vary considerably across congressional terms, with many contests falling well below historical averages. This pattern is consistent with the institutional design of special elections: they are held off-cycle, receive less public attention, and are conducted on compressed timelines that constrain campaign organization and voter outreach. As a result, outcomes can be highly contingent on context-specific factors such as candidate events, media coverage, and other shocks that affect who turns out.[8]

A concrete illustration of this turnout-driven uncertainty is Alaska's at-large House special election in 2022. Turnout was 32.2%, well below Alaska's typical participation in general elections (roughly 60%), and the contest combined several unusual features: it was triggered by the death of Representative Don Young, a long-serving Republican who had held the seat for decades; it was the state's first federal election conducted under ranked-choice voting; and it included a high-profile candidate (former governor Sarah Palin). The election produced a hard-to-anticipate party switch, flipping the seat Democratic for the first time since 1972. In such settings, small shifts in who participates can materially influence the result, yielding outcomes that depart from historical baselines or campaign expectations.

23, 2013, discussing the unpredictability of low-turnout special elections, including the 2013 Massachusetts Senate special election; and Andy Barr, "Pollsters Uneasy About Mass.," *Politico*, Jan. 19, 2010, on turnout challenges in the 2010 Massachusetts Senate special election that resulted in a Republican flipping the seat.

[8]See, for example, Nate Cohn, "Turnout Data Reveals the Core of Democrats' Success in Special Elections," *The New York Times*, Feb. 2, 2024, which highlights how the unrepresentative electorate in special elections complicates predictions. These contests are dominated by partisan voters (88% registered as Democrats or Republicans, 92% with recent primary votes), while independents—32% of registered voters—constitute only 12% of turnout, and younger voters aged 18-29—15% of registered voters—represent just 4%. Such deviations from the broader population limit the predictive utility of past patterns, as slight shifts among these groups can disproportionately impact outcomes.

This forecasting challenge is compounded by the sensitivity of polling-based projections to turnout assumptions, so that even modest differences in the assumed electorate can yield sharply different predictions. The 2017 Alabama Senate special election between Roy Moore (R) and Doug Jones (D) provides a clear example: alternative turnout models implied projections spanning a sizable Democratic lead to a Republican advantage, reflecting wide uncertainty about the electorate's composition.[9] These discrepancies underscore that special-election forecasting often hinges on case-specific participation dynamics rather than stable historical regularities.

Building on this descriptive evidence, I next examine whether special-election outcomes, and in particular party switches, are systematically related to pre-existing state- or district-level conditions. This step is pivotal for assessing whether party-switching special elections can be treated as plausibly exogenous shocks in the empirical design.

To test for predictable selection into party switches, I employ linear probability, logit, and probit models, regressing the binary indicator 'PartySwitch,' which equals one if the special election changes party control, on pre-vacancy economic, political, and partisan covariates. Specifically, I estimate:

$$\Pr\left(\text{PartySwitch}_e = 1\right) = F\left(\alpha + \gamma' E_e + \delta' P_e + \lambda' Q_e + \varepsilon_e\right), \tag{1}$$

where e indexes a special-election episode, and $F(\cdot)$ is the identity in the linear proba-

[9]See, for example, Alex Seitz-Wald, "Pollsters, Pundits Say Alabama Senate Race Impossible to Predict," *NBC News*, Dec. 11, 2017, on the impact of turnout assumptions in the 2017 Alabama Senate special election, where SurveyMonkey projections varied by 17 percentage points. Similarly, Nate Cohn, "Why No One Knows What Will Happen in Alabama," *The New York Times*, Dec. 12, 2017, highlights polling swings, with Fox News predicting a 10-point Democratic win and Emerson College a 9-point Republican lead, reflecting challenges in estimating electorate composition in a polarized, low-turnout race.

bility model and the logistic (normal) CDF in logit (probit). The vector E_e includes the unemployment rate, log median household income, and log house price index, each computed as a three-year pre-vacancy average (state-level for Senate contests and district-level for House contests). These economic measures are standard correlates of electoral outcomes in regularly scheduled elections (e.g., Tufte, 1978; Abramowitz and Segal, 1986; Lewis-Beck and Rice, 1992). The vector P_e proxies for pre-vacancy incumbent strength, including log PAC contributions to the incumbent (a three-year pre-vacancy average), the incumbent's most recent pre-vacancy general-election log victory margin, and log tenure. The vector Q_e captures the local partisan environment in the year preceding the vacancy through binary indicators of whether the constituency voted for a presidential and a gubernatorial candidate of the same party as the departing incumbent in their respective most recent elections. These measures address the concern that the partisan orientation of the local electorate, as revealed by its voting behavior across multiple offices, may predict special election outcomes independently of local economic fundamentals or incumbent-specific political capital. Specifically, constituencies misaligned with the departing incumbent's party may be more susceptible to a switch, while those aligned with it may be more likely to preserve party control. Collectively, these covariates account for the three channels through which special election outcomes could be systematically predictable from pre-vacancy conditions.

The results provide no evidence that party switches are explained by these observables. Table 3 reports estimates of equation (1) for pooled Senate and House contests, including linear probability model coefficients and, for comparison, average marginal effects from logit and probit models. Across specifications, pre-vacancy economic conditions, incumbent characteristics, and local partisan preferences are not statistically associated with 'PartySwitch.' This null finding suggests that party-switching special elections are not systematically related

to the standard predictors of partisan turnover in regularly scheduled elections.

Taken together, the descriptive turnout patterns and the absence of observable predictors support treating party-switching special elections as plausibly exogenous political shocks. Under this interpretation, the variation in partisan turnover appears to be driven more by the unique circumstances surrounding special elections than by persistent regional economic or political trends.

Even if party-switching special elections are plausibly orthogonal to local lending opportunities, a second identification concern remains. The design compares banks that align with the post-election party to those that do not within the same state or district and thus requires that, absent turnover, the two groups would have followed parallel lending trajectories. This assumption may fail if the banks that end up aligned differ in ways that generate differential lending even in the absence of politics. For example, suppose mortgage demand is rising in a state around the time of a special election, but some banks have greater capacity to scale originations quickly in that market, owing to a larger local footprint, established origination channels, or underwriting infrastructure. If these higher-capacity banks are disproportionately represented among those that become aligned once the seat flips, their stronger lending response could be misattributed to partisan alignment rather than to pre-existing differences in responsiveness to local conditions. More generally, alignment status might be correlated, event by event, with trend dynamics that would violate the parallel-trends assumption. To address this concern, I implement a dynamic event-study framework that traces relative lending behavior in the years before and after party-switching special elections, directly testing for differential pre-trends between aligned and non-aligned banks.

4.2 Research Design

To implement the above event-study approach, I estimate the following staggered DiD specification:

$$y_{i,r,t} = \beta_0 + \beta_1 \text{BankAlign}_{i,r,t} + \sum_{\tau=-2}^{2} \beta_{2,\tau} \text{PartySwitch}_{r,t}^{\tau} + \sum_{\tau=-2}^{2} \beta_{3,\tau} \left(\text{BankAlign}_{i,r,t} \times \text{PartySwitch}_{r,t}^{\tau} \right) + \gamma' X_{r,t} + \alpha_r + \alpha_{i,t} + \varepsilon_{i,r,t}, \quad (2)$$

where $y_{i,r,t}$ denotes the annual log change in mortgage lending by bank i in region r (state or congressional district) and year t, measured either by the total originated amount or the number of originations. Let T_r denote the year of the party-switching election in region r. $\text{PartySwitch}_{r,t}^{\tau}$ is a relative event-time indicator defined as $\mathbb{1}[t - T_r = \tau]$, which equals one if year t is exactly τ years before or after the party switch in region r, and zero otherwise; $\tau = 0$ denotes the year of the party switch, $\tau > 0$ post-switch years, and $\tau < 0$ pre-switch years. $\text{BankAlign}_{i,r,t}$ equals one if bank i is politically aligned with the incumbent party in region r and year t, and zero otherwise. The coefficients $\beta_{3,\tau}$ trace the aligned–non-aligned lending differential around party switches, with inference relative to $t = -1$ for Senate elections and relative to never-switch and off-window years for House elections.

The specification includes region fixed effects (α_r) and bank–year fixed effects ($\alpha_{i,t}$). In an alternative specification, region fixed effects are replaced with bank–region fixed effects ($\alpha_{i,r}$), while retaining bank–year fixed effects. The vector $X_{r,t}$ includes time-varying regional controls, including the unemployment rate, the log house price index, the log resident population, and the log number of mortgage applications that result in either origination or denial (to proxy for credit demand). In state-level specifications, additional controls include

log real median household income, the homeownership rate, and the vacancy rate. In the congressional-district analysis, log total personal income replaces real median household income, and homeownership and vacancy rates are omitted due to data limitations. To account for broader political representation, the state-level specifications also include an indicator for bank alignment with the state's second senator.

Identification in this framework comes from within-region comparisons between banks that become aligned with the party assuming office after a party-switching special election and those that remain non-aligned. To fix ideas, consider two banks operating in Pennsylvania, one Democratic-leaning and one Republican-leaning. In April 1991, Senator John Heinz (R) dies, triggering a special election in November 1991 that elects Harris Wofford (D). After this election, the Democratic-leaning bank becomes aligned with the new officeholder, while the Republican-leaning bank does not. Accordingly, the effect of partisan alignment is identified from the difference in the change in mortgage lending between the two banks around the turnover, as captured by the interaction between 'BankAlign' and 'PartySwitch,' rather than from Pennsylvania-wide shifts in credit demand or other contemporaneous shocks common to both. Figure 4 provides a schematic illustration of this identification logic.

Although the party-switching election is a region-level political shock, the design does not require that non-aligned banks be unaffected. Instead, identification comes from differential exposure to the same region-year environment: all banks in region r operate under the same post-election political conditions, but only banks whose partisan orientation coincides with the party assuming office are classified as aligned. In this sense, non-aligned banks provide a within-region benchmark for common demand shifts, local macroeconomic conditions, and other contemporaneous influences that impact all banks but are not mediated by political alignment. The design is therefore most naturally interpreted as a DiD comparison in relative

differences, with the key identifying restriction being that, absent the alignment channel, the aligned–non-aligned lending gap within a region would not change systematically around party-switching special elections.

5 Baseline Results

5.1 Main Estimates

Table 4 reports estimates of equation (2) at the bank–state–year level, using an event window spanning two years before and two years after party-switching Senate special elections. This window is chosen to isolate the effects of the special-election turnover while minimizing overlap with subsequent regularly scheduled elections, which may follow closely when vacancies arise late in a Senate term. The dependent variables are annual log changes in mortgage origination volumes and counts, shown in columns (1)–(3) and (4)–(6), respectively. Columns (1) and (4) include state fixed effects, which absorb time-invariant state characteristics, and bank–year fixed effects, which absorb time-varying bank-level shocks to credit supply. The interaction coefficients imply that aligned banks expand mortgage origination volume by 9.6% in the event year and 8.7% in the subsequent year, and increase mortgage counts by 6.5% in the event year and 6.9% in the subsequent year.

Columns (2) and (5) tighten identification by replacing state fixed effects with bank–state fixed effects, while retaining bank–year fixed effects. This specification accounts for all time-invariant bank–state pair heterogeneity, including persistent market footprint, baseline competitive positioning, and other stable bank–state relationships that could otherwise bias the alignment estimates. The resulting coefficient magnitudes are closely aligned with those in columns (1) and (4), indicating that the estimated alignment effects are not driven by

time-invariant bank–state sorting.

A remaining concern is that special elections may influence lending through channels unrelated to partisan turnover, such as campaign promises, anticipated policy shifts, or the resolution of political uncertainty once the race is decided. To probe this possibility, columns (3) and (6) augment the baseline specification with an indicator, 'SpecialElection,' equal to one if a special election takes place in region r at relative time $\tau = 0$, and zero otherwise. This indicator is entered into the same lead–lag structure to allow for anticipatory effects tied to the occurrence of a special election itself. The interaction coefficients remain similar in both magnitude and statistical significance, supporting an interpretation centered on partisan realignment rather than the election event per se.

Table 5 reports estimates of equation (2) for House special elections. The unit of observation is bank–congressional district–year, reflecting the House's finer geographic representation relative to the Senate. Because representatives face regular elections every two years, the analysis uses a narrower event window that spans one year before the special election, the event year, and one year after. This choice limits contamination from the subsequent regularly scheduled election cycle. Aside from this adjustment, all the specifications parallel those in Table 4, facilitating comparison across chambers.

The estimates point to both commonalities and differences. As in the Senate sample, aligned banks expand mortgage lending in the event year of a party-switching House special election. The contemporaneous effects are larger: lending volume rises by 14.5% and loan count by 13.4%. In contrast to the Senate, the increase in House-aligned lending does not persist into the subsequent year. One interpretation of the cross-chamber differences in both size and duration is institutional: House constituencies are smaller and more geographically concentrated, so a given expansion in credit supply can be deployed more locally, and thus

register as a larger proportional change in district-level mortgage activity. At the same time, the House's shorter electoral horizon compresses the window for cultivating and sustaining political relationships, whereas the Senate's longer terms provide greater scope for longer-lived engagement. Collectively, the evidence suggests that partisan turnover in the House is associated with a sharper but more transitory lending response, whereas Senate turnover is associated with smaller, more durable effects.

5.2 Identification Checks

In this subsection, I provide three pieces of evidence on design validity. First, I examine event-time dynamics to test for differential pre-trends. Second, I utilize placebo randomizations to evaluate whether the empirical setup mechanically generates spurious significance. Third, I re-estimate the event study using an interaction-weighted estimator robust to staggered timing and heterogeneous effects.

5.2.1 Parallel Trends Diagnostics

For both the Senate and House samples, the event-study estimates indicate that alignment effects arise at $\tau \geq 0$, with no evidence of differential pre-trends. Specifically, the coefficients on the interaction terms for $\tau < 0$ are statistically indistinguishable from zero across specifications, suggesting that aligned and non-aligned banks display similar pre-event lending dynamics. Figures 5 and 6 plot the corresponding lead–lag coefficients for the baseline Senate and House specifications, respectively, and visually reinforce the absence of anticipatory movements in mortgage lending.

The lack of pre-event differential dynamics strengthens the interpretation of the post-event coefficients as responses to partisan turnover operating through bank alignment. If

the estimated effects reflected endogenous selection or omitted factors correlated with the impending switch, systematic divergence between aligned and non-aligned banks would be expected in the lead periods. Instead, the estimates are consistent with a discrete adjustment following the election outcome.

5.2.2 Randomization Inference and Placebo Tests

A complementary check uses placebo tests to assess whether the empirical design mechanically generates "effects" under artificial treatment. I repeatedly randomize the assignment of party-switching special elections and the corresponding bank–party alignment, re-estimate the baseline specification under each placebo draw, and collect the resulting t-statistics for the interaction effects. Under valid identification and correctly sized inference, these placebo t-statistics should be approximately distributed as a standard normal; systematic departures would indicate that the estimator is picking up latent trends or persistent differences unrelated to the actual event.

The top panels of Figures 7 and 8 (Senate and House) show that the placebo t-statistics are centered around zero with dispersion close to one and only a small fraction exceeding $|1.96|$. The bottom-panel P–P plots also show close agreement with the standard normal distribution. In sum, these placebo results suggest that the DiD design does not generate spurious significance in the absence of true treatment, bolstering confidence that the baseline estimates are not an artifact of pre-existing dynamics.

5.2.3 Staggered-Adoption Bias and Interaction-Weighted Estimates

Finally, I gauge robustness in light of recent advances in staggered adoption designs, which show that conventional two-way fixed effects (TWFE) estimates can be biased when treat-

ment timing varies and effects are heterogeneous across cohorts or evolve over event time. TWFE implicitly pools multiple comparisons, including those where already-treated units serve as controls for not-yet-treated units. In such settings, the resulting estimand may deviate from the average treatment effect of interest and, in some cases, assign non-intuitive weights to particular comparisons (Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Borusyak et al., 2024).

The issue is most acute when treated cohorts enter the control group for later-treated cohorts. If effects are dynamic, outcomes for early-treated units reflect post-treatment adjustments and therefore provide an invalid counterfactual for newly treated units, potentially distorting both level estimates and the event-time profile. This concern is relevant here because party-switching special elections occur at different times across states and districts, and the lending response may differ across cohorts and over event time.

To address these concerns, I re-estimate the event study using the interaction-weighted estimator proposed by Sun and Abraham (2021). Their approach identifies cohort-specific event-time effects by using not-yet-treated units (or pre-treatment periods) as the comparison group, then aggregates these effects transparently, avoiding "already-treated as control" comparisons. Figures 9 and 10 illustrate the interaction-weighted estimates. The results closely match the baseline profiles in sign, statistical significance, and magnitude, and continue to show no evidence of differential pre-trends. Overall, robustness to an estimator appropriate for staggered timing and heterogeneous effects supports the interpretation that TWFE weighting artifacts do not drive the baseline findings.

6 Mechanisms

This section examines economic forces that may explain the baseline alignment effects. It first disentangles the source of the estimated post-switch differential by assessing whether it reflects changes in lending only among aligned banks or among non-aligned banks as well. It then examines mechanisms that could explain the post-switch lending expansion among aligned banks, contrasting a partisan-bias view with a quid pro quo interpretation.

6.1 Decomposing Aligned and Non-Aligned Responses

Before interpreting the baseline alignment effect as expansion by aligned banks, it is useful to establish whether non-aligned banks also respond to party-switching special elections. This distinction matters for three reasons. First, the baseline interaction identifies a within-region differential between aligned and non-aligned banks; therefore, systematic reactions by non-aligned banks would mechanically enter the estimate and complicate a one-sided interpretation. Second, whether non-aligned banks adjust is informative about the mechanism: retrenchment would point to displacement or defensive responses to the changed political environment, whereas a null response would concentrate the action on aligned banks. Third, the distinction clarifies the economic content of the results by distinguishing net changes in credit supply from reallocation across bank types within the same constituency.

Although retrenchment by non-aligned banks is plausible ex ante, it is not a sharp prediction in the U.S. institutional setting. Members of Congress have limited direct leverage over bank-level lending decisions, and the channels through which a newly elected lawmaker could credibly impose region-specific disadvantages on non-aligned banks are not obvious. Absent a clear and credible threat, non-aligned banks may have little incentive to contract

lending purely because political representation shifts to the opposite party. At the same time, indirect channels cannot be ruled out a priori, so the sign and magnitude of any non-aligned response remain ultimately empirical questions.

To address this issue, I decompose the baseline interaction within the event-study framework of equation (2) by estimating event-time responses separately by alignment status. Specifically, I construct group-by-event-time indicators for aligned and non-aligned banks and estimate a single regression that includes both sets of indicators, together with the same control variables and fixed effects as in the baseline specification. The event-time coefficients are normalized relative to the year immediately preceding the switch. Figure 11 shows that the non-aligned coefficients are flat and statistically indistinguishable from zero before and after the election in both chambers. This pattern provides no evidence of systematic post-switch retrenchment among non-aligned banks, and supports an interpretation in which the baseline differential is driven primarily by affirmative expansion among aligned banks.

6.2 Ideological Optimism

A natural explanation for the post-switch lending expansion is ideological optimism. Under this hypothesis, banks ideologically aligned with the party holding or gaining political representation revise their expectations of local economic conditions upward and adjust lending accordingly. This mechanism operates through belief updating rather than through political interaction or anticipated reciprocity. Related work indicates that corporate decision-makers exhibit greater confidence and adapt their policies when their preferred party performs well in elections, even when contemporaneous policy changes are limited (e.g., Knill et al., 2022; Arikan et al., 2023; Rice, 2024; Stuart et al., 2024).

The ideological-optimism hypothesis has a direct empirical implication in this setting:

the lending response should not depend on party switches. If optimism is the driver, then an aligned bank's lending should respond when the affiliated party wins a special election and continues to control the seat, not only when the seat changes hands. The reason is that the election outcome still resolves uncertainty and confirms the preferred party's continued representation. To gauge this prediction, I compare aligned-bank behavior across two dimensions: (i) party-switching special elections, and (ii) no-switch special elections. Table 6 shows that aligned banks increase lending following party-switching elections, while aligned banks exhibit no detectable lending response around no-switch special elections. The absence of a reaction in no-switch settings is difficult to reconcile with a pure optimism channel, which does not require a change in partisan control to generate belief updating.

I complement this comparison with a second test that targets a slower-moving, capital-intensive margin: branch footprint expansion. If alignment improves banks' perceived local economic outlook, the response should not only be confined to short-horizon credit flows. It should also be reflected, at least to some extent, in investments with meaningful fixed costs and limited reversibility, such as opening or acquiring branches (Cohen and Mazzeo, 2010). Compared to mortgage origination, branch adjustment reflects longer-horizon expectations about profitability and demand (Zdrojewski, 2024), making it a stringent test of optimism-based belief updating.

Using the FDIC Summary of Deposits data, I re-estimate equation (2), replacing mortgage-lending outcomes with a measure of bank branch presence at the region–year level. The resulting coefficients trace whether aligned banks expand regional branch presence relative to non-aligned banks following party-switching special elections. Figure 12 shows no evidence of post-switch branch expansion by aligned banks. The House estimate is economically small and statistically indistinguishable from zero. The Senate estimate is negative in the election

year, indicating modest contraction rather than expansion.

In summary, the findings do not support an ideological-optimism interpretation of the baseline lending effects. The lending response is specific to party-switching elections and does not generalize to branch expansion. The next subsection considers an alternative explanation based on political exchange.

6.3 Quid Pro Quo

Another explanation is a quid pro quo exchange, in which aligned banks expand lending after partisan turnover in anticipation of reciprocal benefits from the newly seated lawmaker. The mechanism posits that local credit expansion is politically salient: it can support near-term economic activity within the constituency and generate outcomes that can be readily framed as early gains under the new leadership. By enabling the lawmaker to claim credit for improved credit availability, aligned banks can strengthen the lawmaker's incentives to act on their behalf. Although elected officials do not directly control bank supervision and enforcement, they can expend political capital in ways that, at the margin, shape banks' expected regulatory and business environments, for example, by lobbying bank regulators or facilitating access to public-sector business opportunities. In this setup, the observed lending expansion is consistent with an arrangement in which banks supply politically valuable credit and lawmakers use political influence to deliver regulatory or contracting advantages.

6.3.1 Enforcement Actions

A quid pro quo interpretation implies that the post-switch lending expansion can be followed by more accommodative supervisory outcomes for aligned banks. A natural place to look is supervisory enforcement. Federal banking agencies issue enforcement actions to require

corrective measures when examinations identify unsafe or unsound practices, managerial deficiencies, or violations of laws and regulations. These actions are consequential because they can constrain operations, trigger remediation costs, and increase ongoing supervisory scrutiny. Because the mechanism concerns regulatory accommodation on net, rather than the particular form it takes, I do not distinguish across enforcement-action types and instead study the overall incidence of publicly disclosed enforcement actions issued by the Federal Reserve, the FDIC, and the OCC.

A threshold question is whether federal banking regulators, which operate as nominally independent agencies, are meaningfully responsive to congressional influence. Prior evidence suggests they are: federal regulatory agencies have been shown to adjust their enforcement decisions systematically in response to the preferences of their congressional overseers, with shifts in legislative composition translating into measurable changes in agency output (Weingast and Moran, 1983).

I focus this test on Senate party-switching special elections because the channels through which elected officials can affect prudential regulation are plausibly stronger for senators. Senators serve longer terms, represent statewide constituencies, and often accumulate greater seniority, which can increase their leverage in repeated oversight interactions with federal banking agencies. Further, the Senate's role in confirming presidential nominees, as well as its capacity to convene high-profile oversight hearings, may raise the expected cost to regulators of sustained conflict with influential senators. This leverage is not confined to senior members: a newly elected senator arrives as part of a caucus with shared supervisory interests, and the relevant margin for regulators is the party's collective oversight posture rather than any individual member's standing. Consistent with this institutional logic, senators can press for adjustments to regulatory initiatives through both private engagement and public

oversight tools, including hearings, formal correspondence, and coordinated letters.[10]

I estimate a DiD Poisson model using counts of publicly disclosed enforcement actions. Because party-switching Senate special elections occur at the state–year level while enforcement actions are observed at the bank–year level, I map state-level political shocks into bank-level treatment intensity by aggregating a bank's exposure across the states in which it operates. Exposure is constructed using a fixed pre-election mortgage footprint from HMDA: for each state, I weight the party-switch event-time indicator by the bank's pre-election share of mortgage originations in that state and then sum across states. Holding the footprint fixed ensures that exposure reflects predetermined geographic presence rather than post-election reallocation of lending.

Formally, I model the conditional mean of enforcement actions as:

$$\mathbb{E}\left[\text{Actions}_{i,t} \middle| \cdot \right] = \exp\left(\sum_{\tau=-2}^{2} \beta_{2,\tau} \sum_{s} w_{i,s}^{\text{pre}} \text{PartySwitch}_{s,t}^{\tau} + \sum_{\tau=-2}^{2} \beta_{3,\tau} \sum_{s} w_{i,s}^{\text{pre}} \left(\text{BankAlign}_{i,s,t} \times \text{PartySwitch}_{s,t}^{\tau} \right) + \alpha_i + \alpha_t + \gamma' B_{i,t} \right), \quad (3)$$

where $\text{Actions}_{i,t}$ is the count of publicly disclosed enforcement actions faced by bank i in year t. $\text{PartySwitch}_{s,t}^{\tau}$ is an event-time indicator equal to one when year t is τ years relative to a party-switching Senate special election in state s, with $\tau = 0$ denoting the year of the switch.

[10] Illustrative episodes include: (i) August 17, 2018: a group of seven Republican senators led by Senator David Perdue sent a letter to Randal Quarles (Fed Vice Chair for Supervision) calling for immediate regulatory relief and more tailored supervision, including objections to continued enhanced oversight and stress testing for banks in the $100B–$250B asset range; (ii) October 8, 2020: Senator Mike Crapo wrote directly to senior leadership at the Fed, CFPB, FDIC, OCC, and NCUA urging the agencies to use discretion to minimize regulatory impacts associated with pandemic-era balance sheet growth and threshold crossings linked to PPP participation; and (iii) November 14, 2023: Senator Tim Scott and 38 other Republican senators sent a coordinated letter urging the Federal Reserve, FDIC, and OCC to withdraw the proposed "Basel III endgame" capital rule, timed immediately ahead of scheduled Senate Banking Committee testimony by representatives of the three agencies.

$\text{BankAlign}_{i,s,t}$ equals one if bank i is aligned with the incumbent senator's party in state s and year t, and zero otherwise. $w_{i,s}^{\text{pre}}$ is bank i's share of nationwide mortgage originations that occur in state s in the year immediately preceding a party-switching special election ($\tau = -1$), and is held fixed thereafter for that bank–state pair:

$$w_{i,s}^{\text{pre}} = \frac{\text{OrigCount}_{i,s,\tau=-1}}{\sum_{s'} \text{OrigCount}_{i,s',\tau=-1}}.$$

The specification includes bank fixed effects (α_i) to absorb time-invariant differences in enforcement propensity across banks, and year fixed effects (α_t) to absorb common shifts in supervisory stringency and regulatory regimes over time. I additionally control for time-varying bank fundamentals $B_{i,t}$ that may independently predict enforcement activity, capturing size (log total assets), capitalization (equity-to-assets ratio), profitability (ROA), balance-sheet risk (loans-to-deposits ratio), and funding structure (deposits-to-assets ratio).

The evidence indicates that banks face fewer enforcement actions when they are more exposed to party-switching Senate special elections that result in partisan alignment. Table 7 reports estimates of equation (3). Interpreting these estimates, a one–standard-deviation increase in exposure is associated with approximately a 4.1% decrease in enforcement actions in the switch year and a 9.7% decrease two years after the switch. Two aspects of this pattern are economically informative. First, the decline is concentrated in the same window in which aligned banks expand lending, consistent with a time-linked exchange rather than a coincidental shift in supervisory posture. Second, the decline persists after conditioning on various balance-sheet characteristics, which is difficult to reconcile with improvements in bank fundamentals that would mechanically warrant reduced enforcement intensity, and instead points to a channel operating through political engagement and reciprocation following

partisan turnover.

6.3.2 Municipal Bond Underwriting

A quid pro quo account may operate through regulatory accommodation, but it may also take the form of preferential access to public-sector business. States and state-linked entities rely heavily on municipal securities issuance to finance infrastructure and other long-lived expenditures, and underwriting mandates allocate meaningful fee revenue and relationship capital to the intermediaries that place these bonds with investors. Because the allocation of municipal underwriting business is a discretionary procurement decision with material rents at stake, it provides an additional setting in which politically connected financial institutions may benefit from favorable access in the aftermath of partisan turnover.

A valid concern is that municipal issuance and underwriter selection are formally controlled by state actors, such as governors, legislatures, and state treasurers, rather than by federal officeholders. Nonetheless, the hypothesis here is not that senators directly choose underwriters, but that senators, as statewide political figures, can shape the environment in which these choices are made by coordinating with state-level party networks, influencing fiscal priorities tied to statewide projects, and leveraging connections that matter for high-value procurement. This influence is plausibly amplified when state financing is intertwined with federal resources and legislative levers that senators help secure, which can affect the timing and composition of states' capital plans and financing needs. More broadly, the municipal market is widely understood to be vulnerable to political influence (Butler et al., 2009), motivating pay-to-play restrictions and professional guidance that explicitly warns against linking political contributions to the award of underwriting business.[11]

[11] For example, MSRB Rule G-37 imposes a two-year ban on a dealer's municipal securities business with an issuer following certain political contributions by the dealer or its municipal finance professionals, reflecting

I use municipal bond issuance data from SDC Platinum, which reports deal-level characteristics, the issuer, and the underwriters' syndicate roles, including the lead manager. I map lead managers to bank holding companies by linking each underwriting entity to its dealer or broker-dealer affiliate within the same consolidated organization, and then aggregate underwriting activity to the bank–state–year level. The primary outcomes capture changes in a bank's position in a state's underwriting market. For each state–year, I compute: (i) the bank's lead-managed par share, defined as lead-managed par amount divided by total municipal par issued, and (ii) the bank's lead-underwriting fee share, defined analogously using the state–year fee pool. In the main specifications, I use annual changes in these shares to isolate within-state reallocations of lead-manager mandates across banks over time, rather than differences driven by state-level issuance cycles.

To estimate dynamic effects, I use the following staggered DiD event-study specification:

$$y_{i,s,t} = \beta_0 + \beta_1 \text{BankAlign}_{i,s,t} + \sum_{\tau=-2}^{2} \beta_{2,\tau} \text{PartySwitch}_{s,t}^{\tau} + \sum_{\tau=-2}^{2} \beta_{3,\tau} \left(\text{BankAlign}_{i,s,t} \times \text{PartySwitch}_{s,t}^{\tau} \right) + \gamma' Z_{s,t} + \delta' W_{s,t} + \alpha_{i,s} + \alpha_t + \varepsilon_{i,s,t}, \quad (4)$$

where $y_{i,s,t}$ denotes either the first difference in bank i's lead-underwriting par share in state s and year t, or the first difference in its lead underwriting fee share. $\text{PartySwitch}_{s,t}^{\tau}$ is an event-time indicator equal to one when year t is τ years relative to a party-switching Senate special election in state s, with $\tau = 0$ denoting the year of the switch. $\text{BankAlign}_{i,s,t}$ equals one if bank i is aligned with the incumbent senator's party in state s and year t, and zero otherwise.

The specification includes bank–state fixed effects ($\alpha_{i,s}$) to absorb persistent relationships

longstanding concerns that underwriting mandates can be exposed to political influence.

and comparative advantage within a given state, and year fixed effects (α_t) to absorb common shocks to municipal underwriting. Because underwriting assignments and fees depend on bond deal characteristics in a given year, I include a set of state–year issuance-composition controls, $Z_{s,t}$, constructed as par-weighted shares. These controls capture the distribution of deal size (par amount issued), credit quality (ratings category, e.g., Moody's/Fitch), offering method (competitive, negotiated, or private placement), pricing (yield on the final maturity), and maturity structure (years to final maturity). Accounting for $Z_{s,t}$ ensures that estimated shifts toward aligned banks reflect reallocations of lead-manager mandates within a state–year rather than changes in the types of deals being brought to market that would mechanically alter underwriting opportunities and fee pools. I also control for time-varying state-level economic conditions, $W_{s,t}$, including log per-capita income, the unemployment rate, and log population, to absorb concurrent state-level demand for public finance and local economic conditions that could jointly influence issuance volumes, pricing, and the intensity of underwriting competition. This set of controls additionally includes the partisan alignment between bank i and the sitting state governor, to account for any independent influence of executive-branch partisanship on state underwriting mandates, given governors' direct authority over public finance procurement.

Table 8 reports estimates of equation (4) for municipal underwriting outcomes. In the switch year, municipal underwriting allocations tilt toward aligned banks: aligned banks' lead-underwriting par share and fee share rise by approximately 1.4% and 1.7%, respectively. The response is immediate and coincides with the period when aligned banks expand mortgage lending most strongly, suggesting that underwriting gains and credit expansion move together rather than reflecting a gradual adjustment. Because the specifications condition on issuance composition and state economic conditions, these shifts are difficult to

attribute to contemporaneous changes in deal mix or local fundamentals. Instead, they are in line with aligned banks capturing fee-generating public-sector mandates in the same narrow window in which they expand politically valuable credit.

7 Lending Risk and Political Alignment

The results indicate that partisan turnover in special elections leads to economically meaningful changes in bank lending. Banks that become politically aligned with the newly elected official expand mortgage originations in the affected region. Earlier tests show that this expansion is not explained by observable shifts in local credit demand or concurrent movements in regional fundamentals, pointing instead to a supply-side response. This conclusion raises a final question: If aligned banks expand lending following political turnover, does this come at the expense of prudent credit standards?

This question matters because politically induced departures from conservative screening can lead to credit misallocation and increase vulnerability to future losses. If aligned banks scale lending by extending credit to riskier borrowers or by shifting toward loan products with higher expected default, the observed credit expansion may carry financial-stability implications beyond its immediate volume effects.

A plausible mechanism is that aligned banks expand credit along riskier margins by relaxing screening intensity in politically aligned regions. Such behavior can be privately optimal if the expected benefits from a strengthened political relationship, whether regulatory accommodation, access to public-sector business, or broader relational rents, outweigh the anticipated costs of higher default exposure from weaker underwriting. Prior work on political influence in lending finds that credit expansions around electoral events can be

followed by weaker loan performance (Khwaja and Mian, 2005; Englmaier and Stowasser, 2017; Fungáčová et al., 2023). While these environments differ from U.S. special elections and typically emphasize pre-election cycles, they support the broader premise that politically mediated lending can reflect a volume-versus-screening trade-off.

To assess whether the alignment-induced lending response shifts toward riskier segments, I examine outcomes that proxy for exposure to higher expected credit risk. First, I consider conventional mortgages, which are not federally guaranteed (unlike FHA or VA loans) and therefore leave lenders more directly exposed to default losses. Second, I examine lending to Black and Hispanic borrowers. Because of persistent disparities in income, wealth, and access to credit, these groups have historically faced higher average default risk in mortgage markets (e.g., Bayer et al., 2016; Bhutta et al., 2022; Wei and Zhao, 2022; Gerardi et al., 2023). Third, I study loans for non-owner-occupied properties, which proxy for investor borrowing and are often more sensitive to cash-flow volatility and housing-market downturns.

Ideally, one would assess ex post loan performance, such as delinquencies and defaults, to speak directly to the riskiness of the post-switch lending expansion. However, HMDA does not track post-origination outcomes. I therefore treat these measures as indirect, origination-stage proxies for shifts in the risk composition of credit supply and banks' exposure at origination, rather than as evidence on realized loan performance.

Empirically, I extend the baseline event-study framework in equation (2) by re-estimating it on each of the three risk-related segments: conventional loans, loans to Black and Hispanic borrowers, and loans for non-owner-occupied properties. Tables 9 and 10 report results for the Senate and House settings, respectively. In each table, columns (1) to (3) present annual log changes in origination amounts, and columns (4) to (6) present annual log changes in origination counts for the three segments.

In the Senate sample, alignment is associated with a significant expansion in conventional lending in the election year, with loan volume rising by 10% and loan counts by 6.6%. Minority lending also increases: loan amounts rise by 6.6% in the election year and 7.2% in the following year, while loan counts increase by 6.2% in the election year. Taken together, these estimates indicate that the post-switch lending response is not confined to safer or more insulated segments. Instead, it includes margins that plausibly carry higher expected risk exposure at origination. By contrast, I find no corresponding increase in non-owner-occupied lending in the Senate setting.

The House results differ in the margin along which risk shifts. Conventional lending again increases sharply in the election year, with loan volume rising by 15.4% and loan counts by 15.3%. Minority lending does not exhibit a detectable change. Instead, the clearest movement toward riskier lending appears through investment-property credit: non-owner-occupied loan volume increases by 38.9% and loan counts by 29.7%, both in the year after the election.

One interpretation is that banks adjust the composition of politically responsive lending in ways that depend on the political and geographic environment. Across both chambers, conventional mortgages expand, consistent with a shift toward lending where banks bear more credit risk directly. The remaining margins differ: in the Senate sample, the expansion tilts toward minority borrowers, whereas in the House sample it tilts toward investment properties. These patterns are consistent with the idea that banks select the most salient margin for local expansion in the relevant constituency, while operating within constraints imposed by underwriting technology, product availability, and local market conditions.

Overall, the evidence suggests that partisan alignment affects not only the level of lending but also its composition along dimensions associated with higher expected credit risk. While

these tests do not observe realized defaults, the segment-level shifts indicate that political incentives may be related to an expansion of credit in riskier parts of the mortgage market, raising concerns about the prudential and welfare consequences of politically influenced credit allocation.

8 Conclusion

This study examines how partisan realignment following congressional special elections shapes private bank mortgage lending. Banks that become politically aligned with newly elected officeholders expand credit in regions that experience a party-switching special election, and the timing and cross-sectional patterns are difficult to explain by shifts in credit demand or contemporaneous regional conditions.

I then ask why banks would find it privately optimal to scale lending in this manner. The evidence developed in the quid pro quo analysis points toward an exchange-based interpretation: aligned banks expand politically salient credit during the post-switch window when attribution to the new officeholder is most valuable, and they appear to receive reciprocal rents through channels such as regulatory accommodation and access to public-sector business. In parallel, the composition of the lending expansion tilts toward margins associated with higher exposure at origination, suggesting that political incentives can be accompanied by weaker screening or a reallocation toward riskier segments.

Taken together, the findings document politically motivated lending in a setting where partisan turnover is plausibly exogenous, and banks have the capacity to reallocate credit across regions. More broadly, they suggest that political alignment can distort the allocation of mortgage credit away from purely risk-based criteria, with potential consequences for

credit market efficiency.

Future work could refine these conclusions in two directions. First, linking originations to subsequent performance would provide a more direct assessment of the risk consequences of politically aligned lending than origination-stage proxies. Second, extending the analysis to other credit markets, such as small-business or agricultural lending, would assess whether similar political distortions arise when discretion and relationships are salient.

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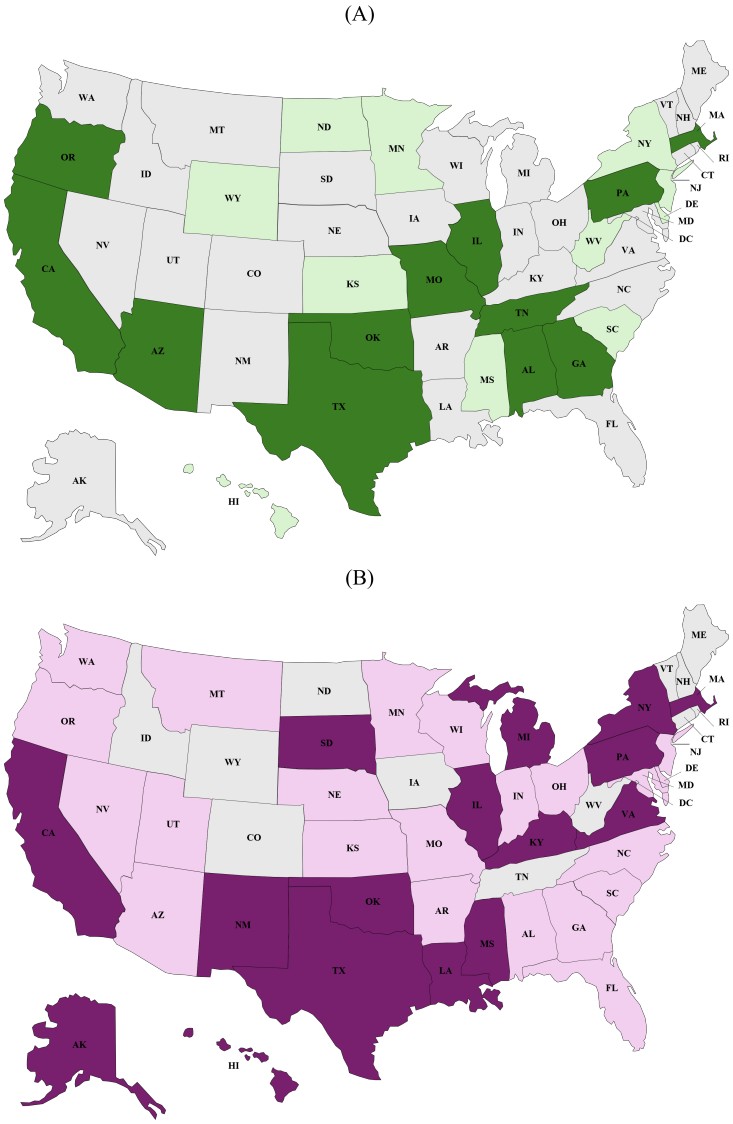
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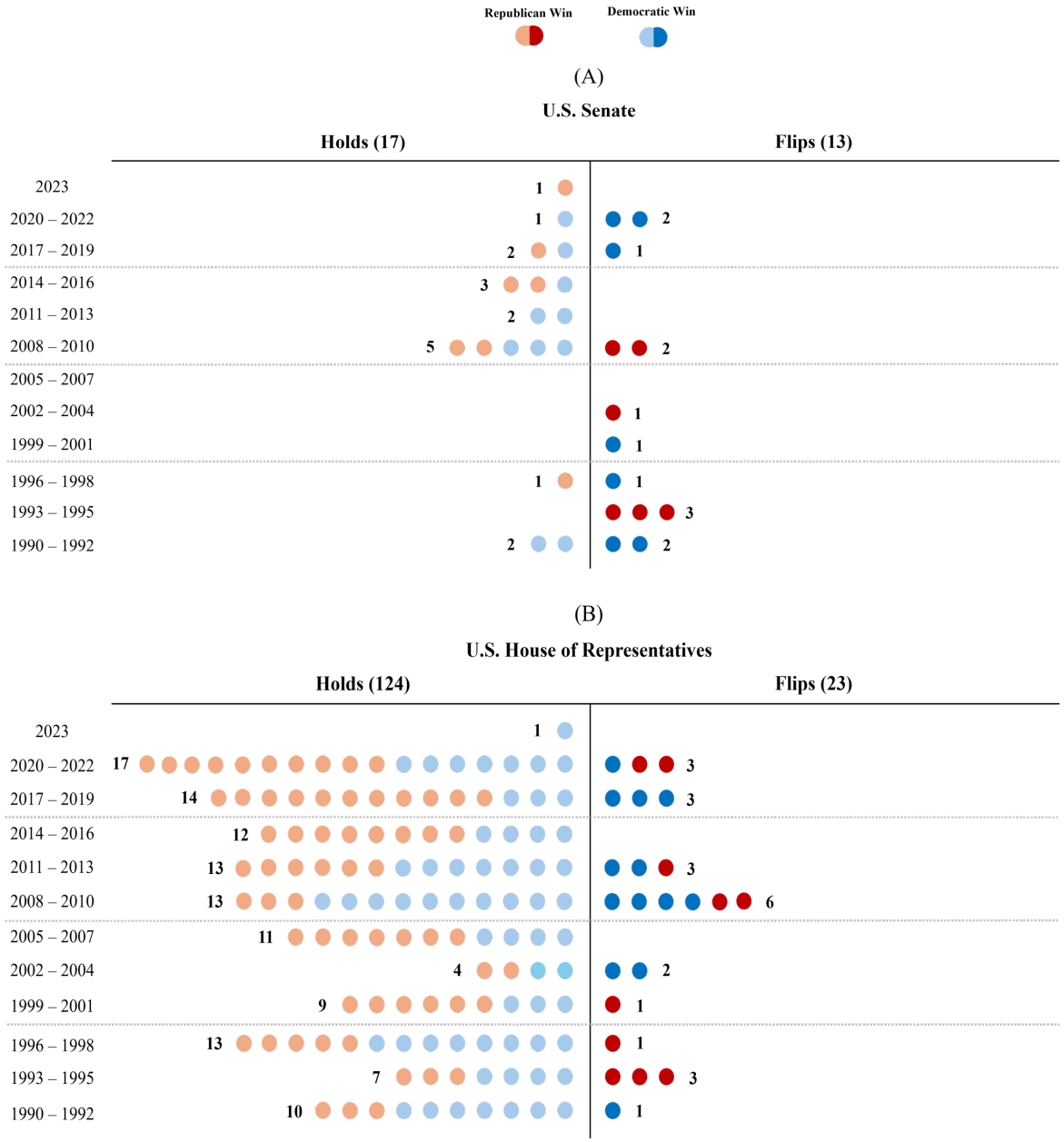
Figures and Tables

Figure 1: Geographical Distribution of Special Elections and Party Switches



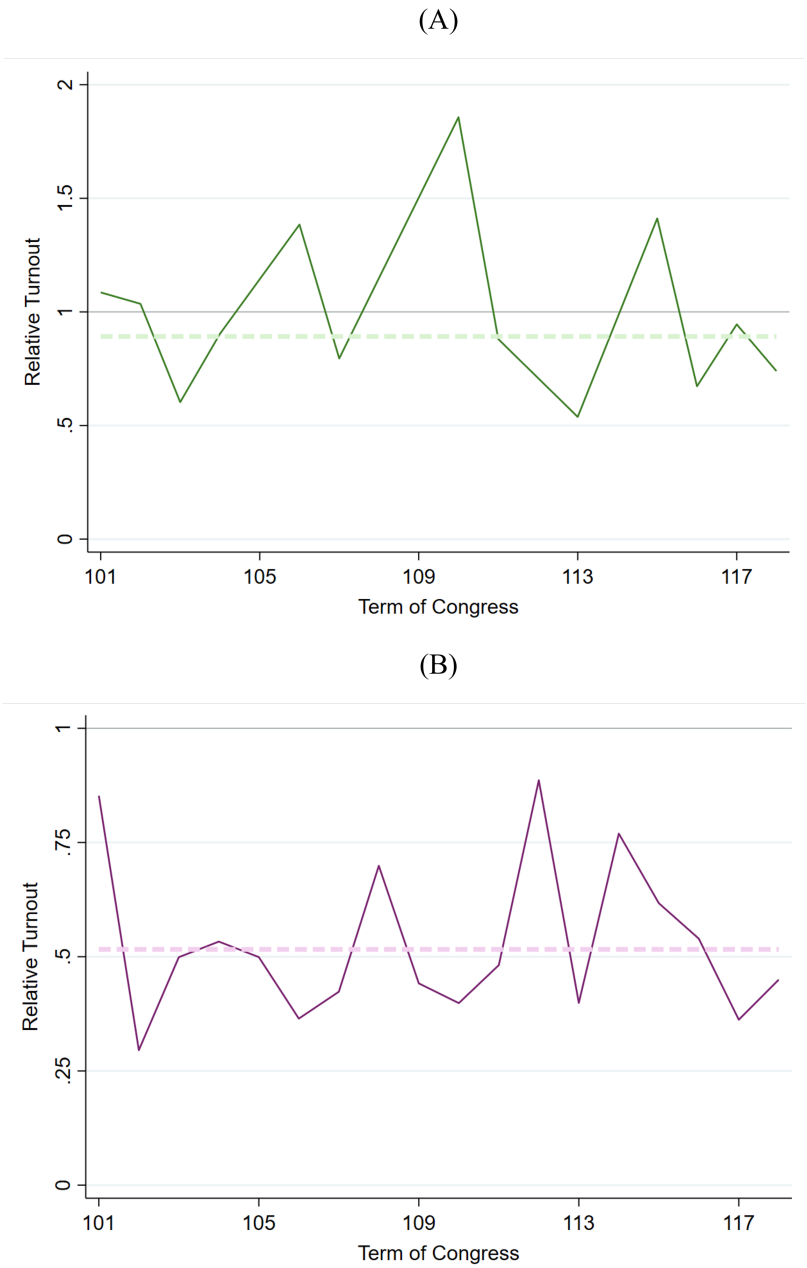
Notes: This figure maps the geographic distribution of U.S. congressional special elections and associated party switches over 1990–2023. Panel A shows Senate contests (green shading) and Panel B shows House contests (purple shading). In each panel, light shading denotes states that experienced at least one special election, and dark shading denotes states in which at least one special election resulted in a party switch.

Figure 2: Special Election Outcomes Across Congressional Cycles



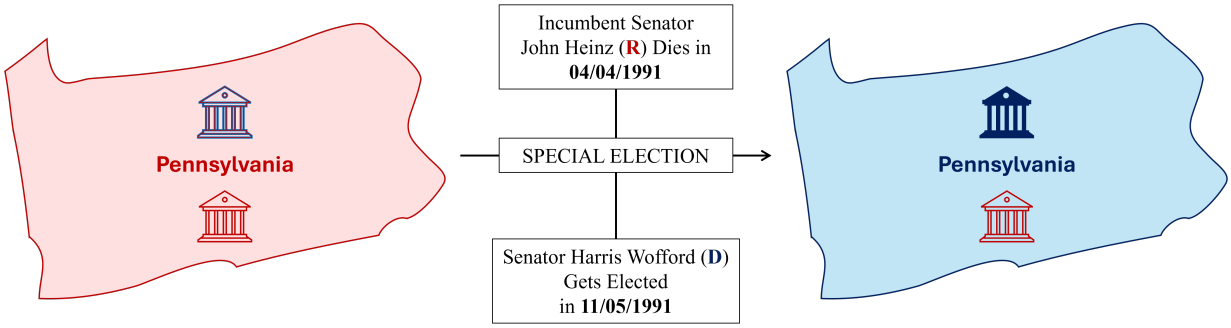
Notes: This figure summarizes special-election outcomes by congressional cycle over 1990–2023 for the Senate (Panel A) and the House (Panel B). Each dot represents one seat. Blue (red) dots indicate seats won by Democrats (Republicans). Light shades denote outcomes with no party switch, while dark shades denote special elections that resulted in a party switch.

Figure 3: Special Election Turnout Relative to Prior General Election Turnout



Notes: This figure plots turnout in special elections relative to the immediately preceding general congressional election for the same constituency, separately for the Senate (Panel A) and the House (Panel B). The solid line shows the turnout ratio, defined as special-election turnout divided by prior general-election turnout (state for Senate contests; congressional district for House contests). The dashed line reports the average turnout ratio across all congressional terms in the 1990–2023 sample.

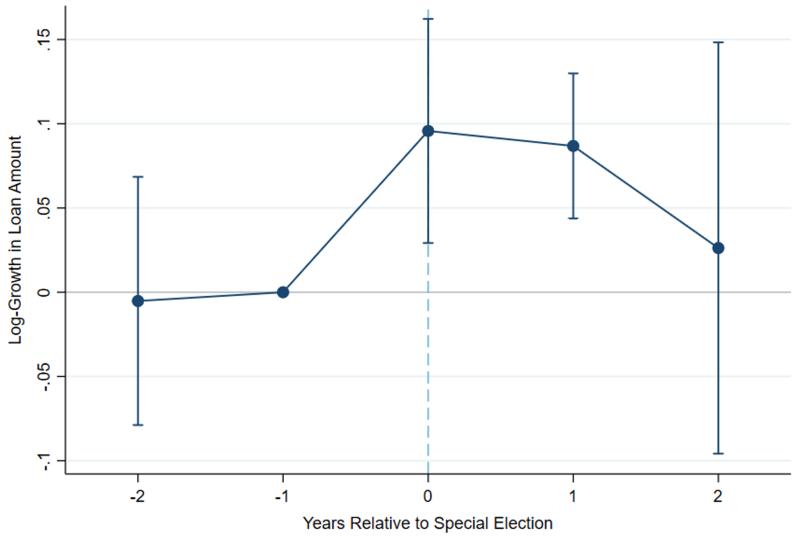
Figure 4: Partisan Shifts and Lending Behavior: An Example of Identification



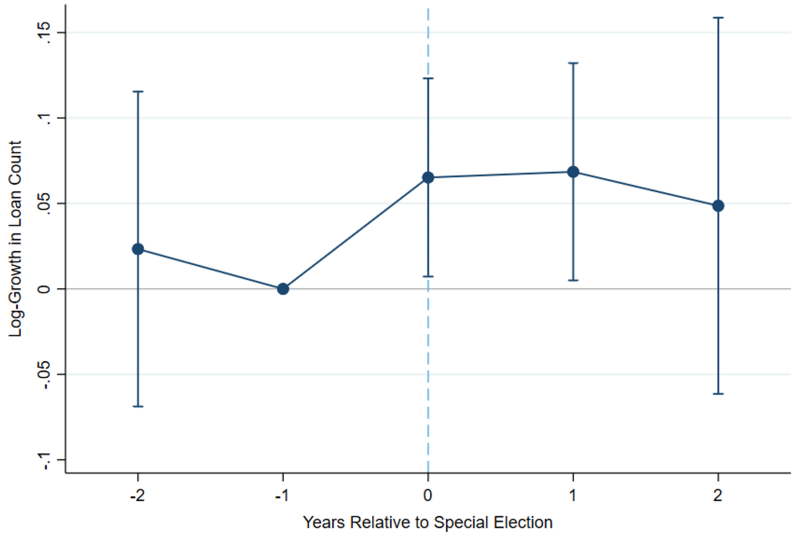
Notes: This figure provides a stylized illustration of the identification logic using Pennsylvania's 1991 Senate vacancy episode. Following Senator John Heinz's (R) death in April 1991 and the special-election victory of Senator Harris Wofford (D) in November 1991, Pennsylvania's partisan control switches from Republican to Democratic. The figure depicts two banks operating in the state, one Democratic-leaning (blue) and one Republican-leaning (red). After the switch, the Democratic-leaning bank becomes aligned with the incoming senator and is shown to increase lending, while the Republican-leaning bank remains non-aligned and nonreactive. The contrast illustrates how the empirical design identifies differential changes in mortgage lending by aligned versus non-aligned banks around party-switching special elections.

Figure 5: Dynamic Effects of Senate Special Elections on Mortgage Lending

(A)



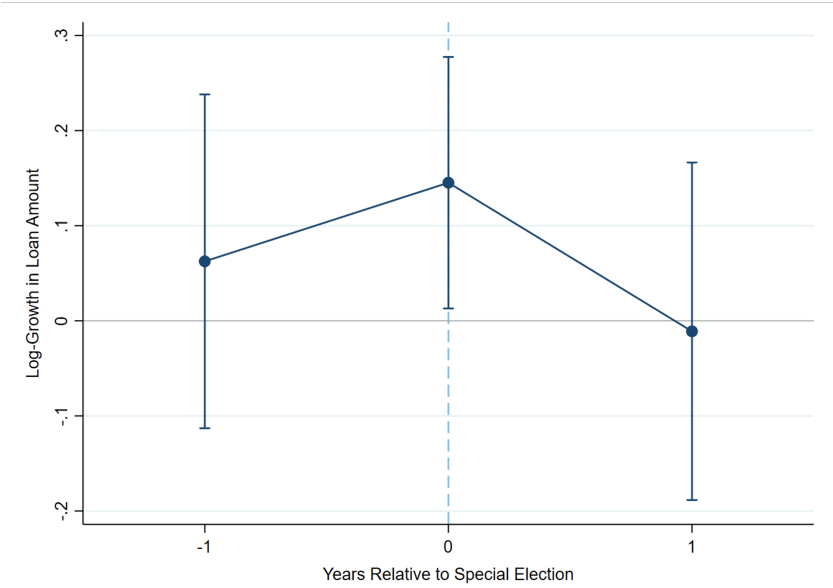
(B)



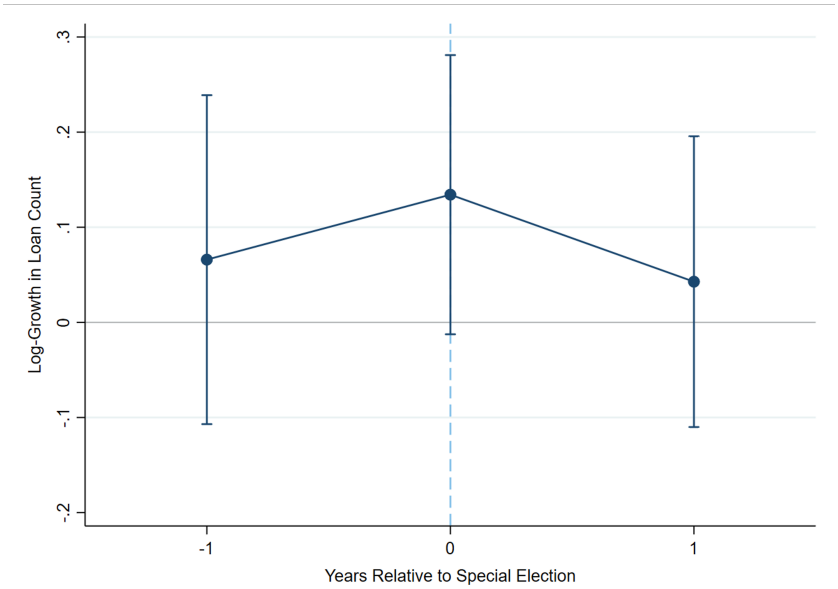
Notes: This figure plots the event-time coefficients $\beta_{3,\tau}$ from equation (2), which capture differential changes in lending by aligned versus non-aligned banks around party-switching Senate special elections. The horizontal axis indexes event time τ relative to the election year ($\tau = 0$). Coefficients are normalized relative to $\tau = -1$, the year immediately preceding the party switch, which is omitted as the reference period. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively. Panel A reports estimates for loan amount growth, and Panel B for loan count growth. Error bars show 95% confidence intervals based on standard errors clustered at the state level.

Figure 6: Dynamic Effects of House Special Elections on Mortgage Lending

(A)



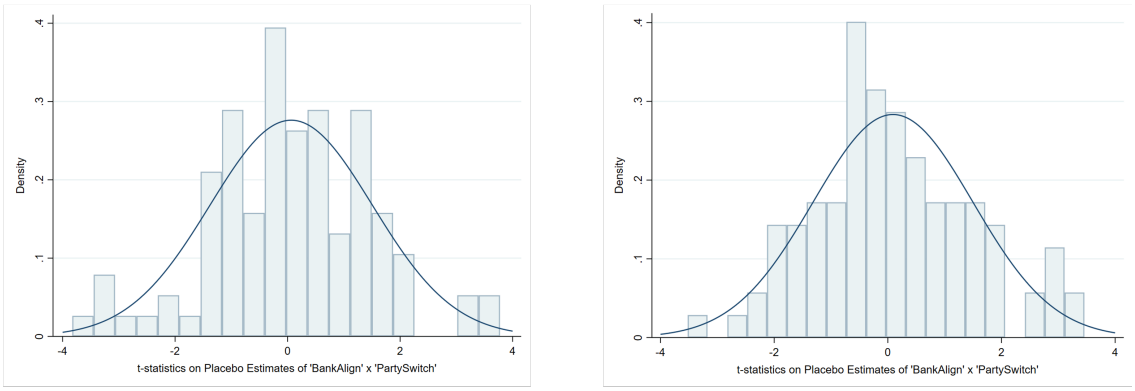
(B)



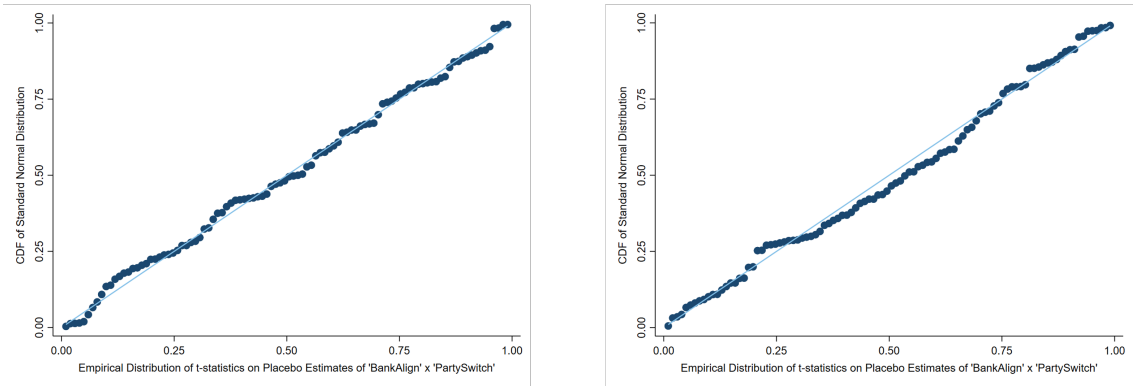
Notes: This figure plots the event-time coefficients $\beta_{3,\tau}$ from equation (2), which capture differential changes in lending by aligned versus non-aligned banks around party-switching House special elections. The horizontal axis indexes event time τ relative to the election year ($\tau = 0$). The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively. Panel A reports estimates for loan amount growth, and Panel B for loan count growth. Error bars show 95% confidence intervals based on standard errors clustered at the congressional district level.

Figure 7: Placebo Test of Senate Special Elections and Mortgage Lending

(A)



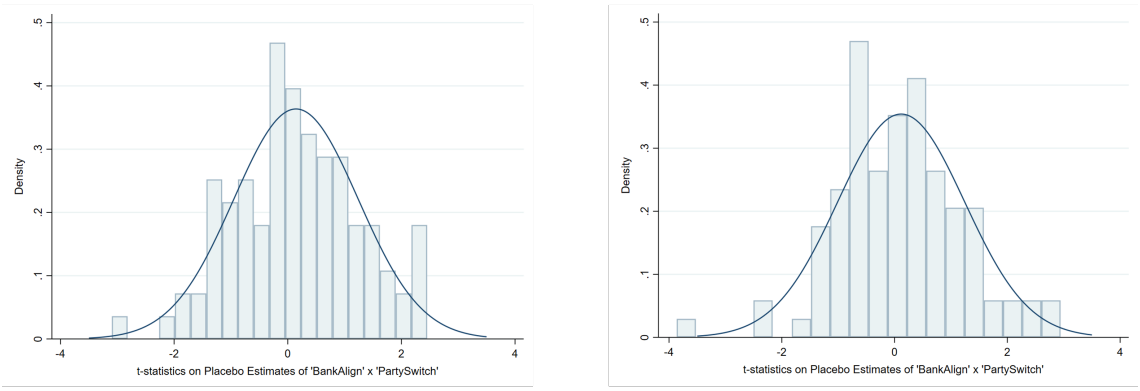
(B)



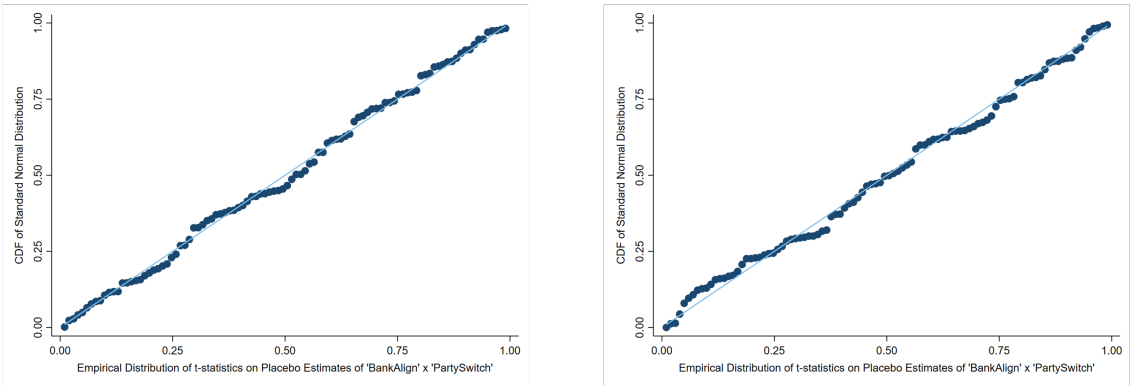
Notes: This figure reports placebo estimates for the $\tau = 0$ effect of party-switching Senate special elections on mortgage lending under the specification in Table 4. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively, and are shown in the left and right columns. The placebo exercise randomly assigns party-switching Senate special elections and bank–senator partisan alignment, holding fixed the total number of events observed in the sample; the procedure is repeated 100 times. Panel A plots histograms of the resulting placebo t-statistics for the interaction 'BankAlign' $\times$ 'PartySwitch,' with standard errors clustered at the state level. Panel B reports P–P plots assessing the normality of the placebo distribution.

Figure 8: Placebo Test of House Special Elections and Mortgage Lending

(A)



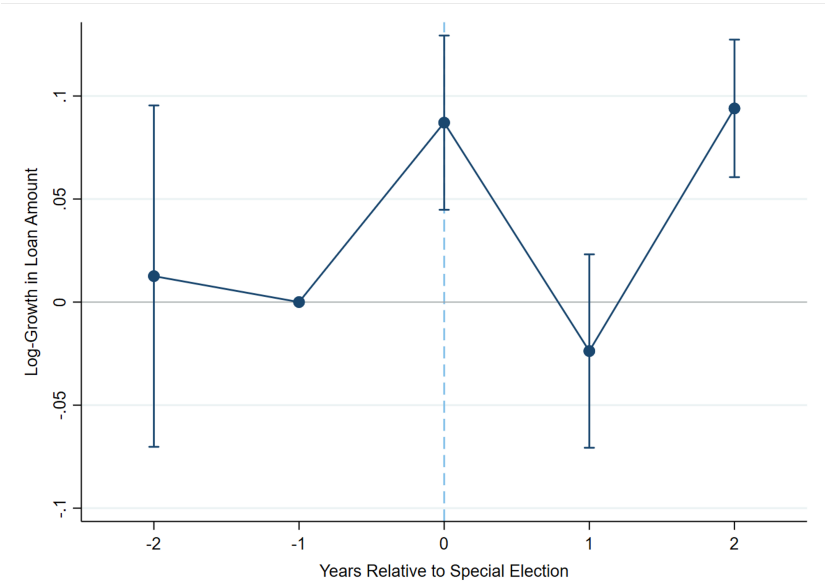
(B)



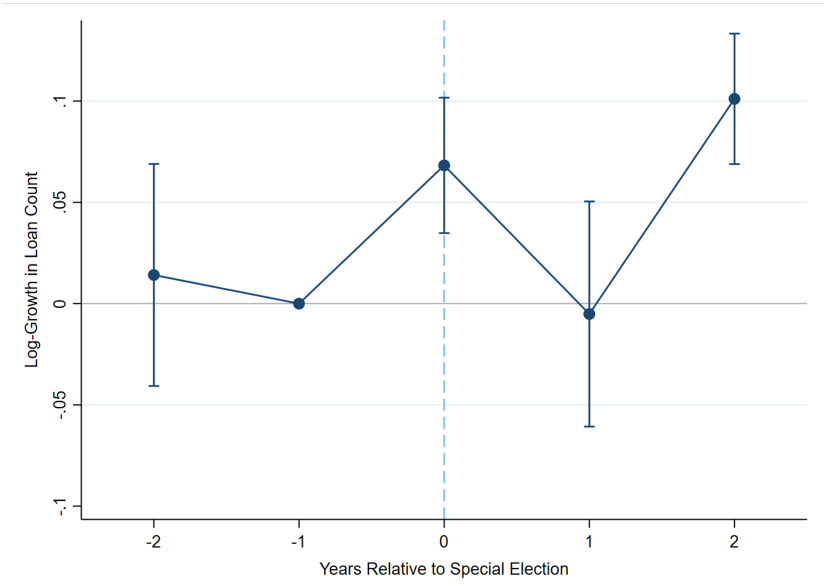
Notes: This figure reports placebo estimates for the $\tau = 0$ effect of party-switching House special elections on mortgage lending under the specification in Table 5. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively, and are shown in the left and right columns. The placebo exercise randomly assigns party-switching House special elections and bank–representative partisan alignment, holding fixed the total number of events observed in the sample; the procedure is repeated 100 times. Panel A plots histograms of the resulting placebo t-statistics for the interaction 'BankAlign' × 'PartySwitch,' with standard errors clustered at the congressional district level. Panel B reports P–P plots assessing the normality of the placebo distribution.

Figure 9: Dynamic Effects of Senate Special Elections: Robust Event-Study Analysis

(A)



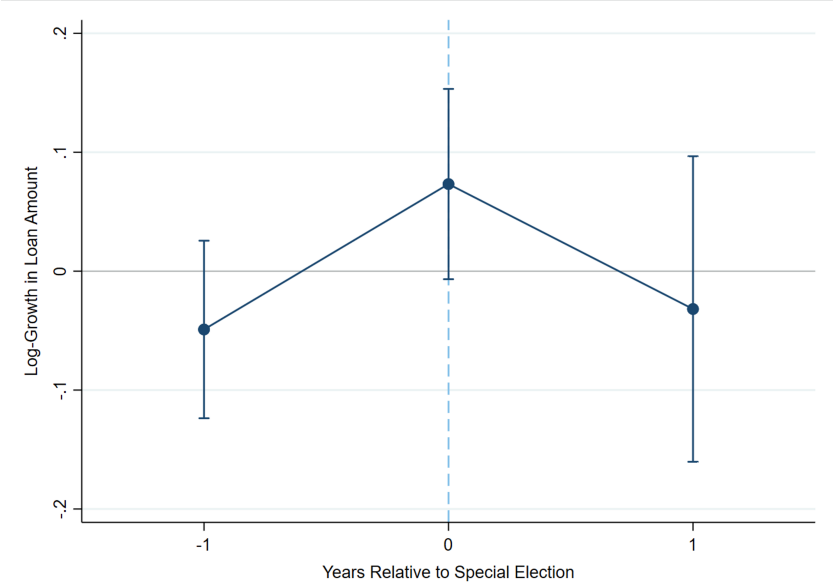
(B)



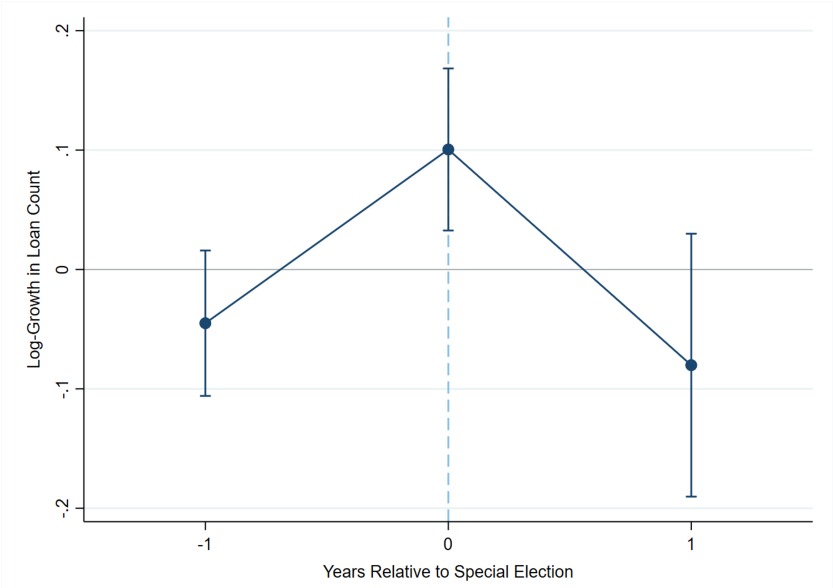
Notes: This figure plots the event-time coefficients $\beta_{3,\tau}$ from equation (2), estimated using the interaction-weighted event-study estimator of Sun and Abraham (2021). The horizontal axis indexes event time τ relative to a party-switching Senate special election ($\tau = 0$), with leads and lags defined relative to each cohort's election timing under the interaction-weighted estimator. Coefficients are normalized relative to $\tau = -1$, the year immediately preceding the party switch, which is omitted as the reference period. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively. Panel A reports estimates for loan amount growth, and Panel B for loan count growth. Error bars show 95% confidence intervals based on standard errors clustered at the state level.

Figure 10: Dynamic Effects of House Special Elections: Robust Event-Study Analysis

(A)



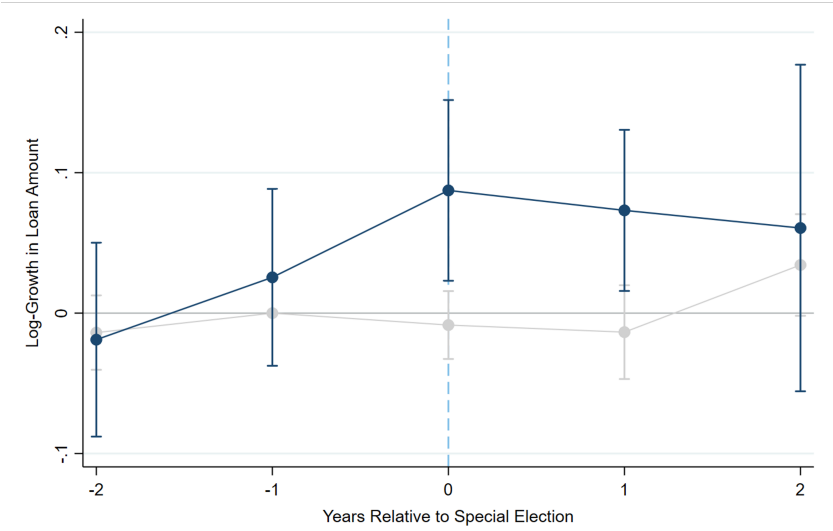
(B)



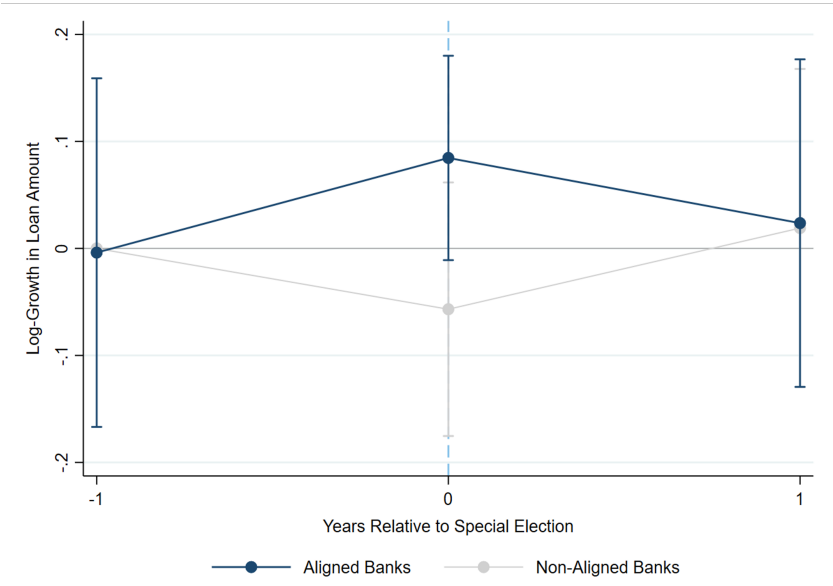
Notes: This figure plots the event-time coefficients $\beta_{3,\tau}$ from equation (2), estimated using the interaction-weighted event-study estimator of Sun and Abraham (2021). The horizontal axis indexes event time τ relative to a party-switching House special election ($\tau = 0$), with leads and lags defined relative to each cohort's election timing under the interaction-weighted estimator. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively. Panel A reports estimates for loan amount growth, and Panel B for loan count growth. Error bars show 95% confidence intervals based on standard errors clustered at the congressional district level.

Figure 11: Dynamic Effects of Special Elections on Mortgage Lending: Aligned vs. Non-Aligned Banks

(A)



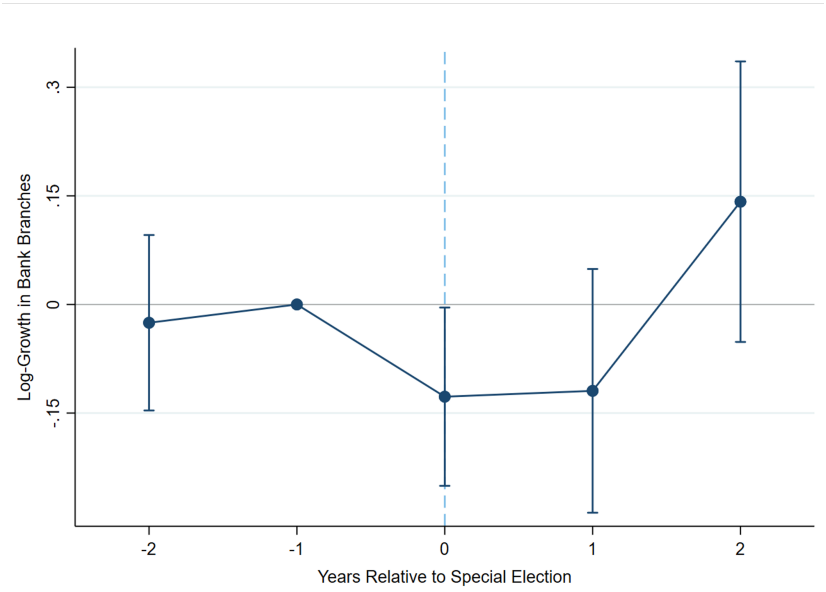
(B)



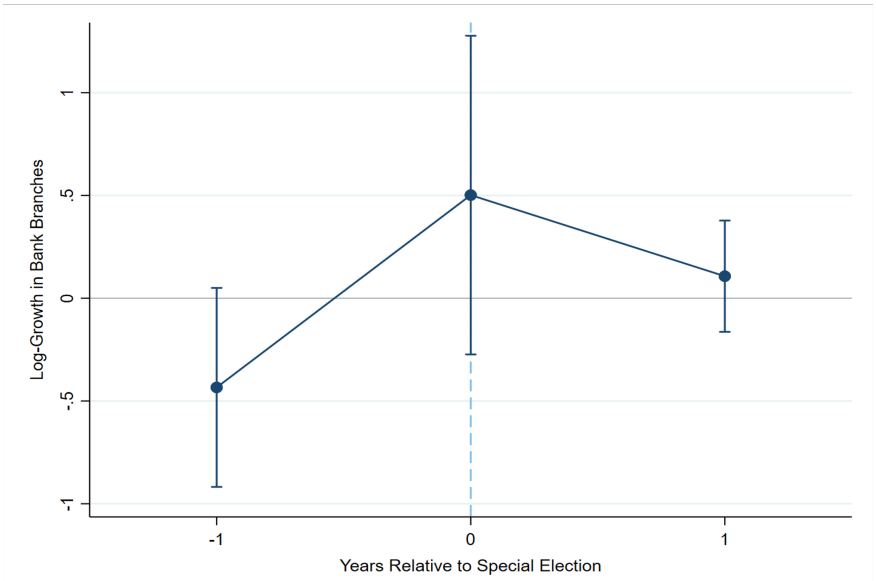
Notes: This figure plots the event-time coefficients $\beta_{3,\tau}$ from equation (2). The horizontal axis indexes event time τ relative to the party-switch year ($\tau = 0$). The blue line denotes aligned banks, and the gray line denotes non-aligned banks. Coefficients are normalized relative to non-aligned banks at $\tau = -1$, the year immediately preceding the party switch, which is omitted as the reference period. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively. Panel A reports estimates for Senate special elections, and Panel B reports the corresponding estimates for House special elections. Error bars show 95% confidence intervals based on standard errors clustered at the state level in Panel A and at the congressional-district level in Panel B.

Figure 12: Dynamic Effects of Special Elections on Bank Branching

(A)



(B)



Notes: This figure plots the event-time coefficients $\beta_{3,\tau}$ from equation (2), which capture differential changes in bank branching by aligned versus non-aligned banks over event time. The dependent variable, 'Bank Branch Growth,' is defined as the annual log change in bank branch counts. Panel A reports estimates for Senate special elections, and Panel B reports estimates for House special elections. The horizontal axis indexes event time τ relative to the party-switch year ($\tau = 0$). Error bars show 95% confidence intervals based on standard errors clustered at the state level in Panel A and at the congressional-district level in Panel B.

Table 1: Special Elections Summary Statistics

Panel A: Number of Special Elections		
	Senate	House of Representatives
2023	1	1
2020–2022	3	20
2017–2019	3	17
2014–2016	3	12
2011–2013	2	16
2008–2010	7	19
2005–2007	0	11
2002–2004	1	6
1999–2001	1	10
1996–1998	2	14
1993–1995	3	10
1990–1992	4	11
Total	30	147
Panel B: Reasons for Special Elections		
Death	11	40
Health Issues	4	7
Higher Office/Public Role	10	41
Legal/Ethical Issues	2	26
Private Sector	3	22
Personal Reasons/Retirement	0	8
Other	0	3
Total	30	147

Notes: This table reports summary statistics for U.S. congressional special elections in 1990–2023. Panel A reports the number of special elections by congressional cycle, separately for the Senate and the House of Representatives. Panel B classifies the reasons for special elections into the categories shown: Death, Health Issues, Higher Office/Public Role, Legal/Ethical Issues, Private Sector, Personal Reasons/Retirement, and Other.

Table 2: Special Elections Summary Statistics

Panel A: Bank–State–Year						
	N	Mean	Median	Min	Max	Std. Dev.
Loan Amount (in $M)	328,236	68.73	0	0	91,300	795.92
Loan Count	328,236	365.42	0	0	339,602	2,946.99
Conventional Loans (%)	99,598	0.91	0	0	1	0.20
Minority Applicant (%)	99,598	0.06	0.02	0	1	0.11
Non-Owner-Occupied (%)	99,598	0.03	0	0	1	0.15
Real Median Household Income (in $)	318,763	66,246	65,220	23,360	111,000	12,278
Unemployment Rate (%)	328,236	5.45	5.15	1.88	13.73	1.91
Resident Population (in 000s)	328,236	6,512	4,566	453.40	39,503	6,968
All-Transaction House Price Index	328,236	319.33	279.26	85.12	1,158	159.40
Homeownership Rate (%)	328,236	67.59	68.80	35	81.30	6.38
Vacancy Rate (%)	328,236	1.73	1.70	0.20	5.30	0.72
Total Mortgage Applications	328,236	156,657	69,203	428	3,463,069	272,391
Panel B: Bank–Congressional District–Year						
Loan Amount (in $M)	500,894	44.81	32.95	1	23,743	235.25
Loan Count	500,894	238.33	21	1	76,117	859.85
Conventional Loans (%)	500,894	0.89	1	0	1	0.21
Minority Applicant (%)	500,894	0.06	0.01	0	1	0.14
Non-Owner-Occupied (%)	500,894	0.07	0	0	1	0.19
Total Personal Income (in $000s)	486,165	21,611	8,583	75.09	721,000	41,581
Unemployment Rate (%)	505,412	5.53	5.10	1.49	29.40	2.18
Resident Population (in 000s)	500,505	935.40	711.87	7.028	10,094.87	913.96
All-Transaction House Price Index	499,996	126.61	114.68	6.22	451.56	52.38
Total Mortgage Applications	500,894	27,665	16,116	1	758,727	43,040

Notes: This table reports summary statistics for the primary variables used in the analysis, organized by observation level: bank–state–year (Panel A) and bank–congressional district–year (Panel B), for 1990–2023. Reported statistics include the number of observations (N), mean, median, minimum, maximum, and standard deviation. Loan-related variables expressed as percentages are computed as shares of total loan counts within each observation unit.

Table 3: Special Election Outcomes and Pre-Vacancy Local Conditions

Dependent Variable	PartySwitch		
	(1)	(2)	(3)
Unemployment Rate	0.007	0.006	0.008
	(0.019)	(0.022)	(0.020)
log (Real Median Household Income)	-0.083	-0.086	-0.108
	(0.218)	(0.225)	(0.221)
log (House Price Index)	-0.108	0.118	0.130
	(0.133)	(0.111)	(0.110)
log (PAC Contributions)	-0.007	-0.010	-0.009
	(0.011)	(0.010)	(0.009)
log (Victory Margin)	-0.058	-0.057	-0.059
	(0.049)	(0.048)	(0.046)
log (Tenure)	-0.018	0.036	0.033
	(0.051)	(0.047)	(0.045)
Presidential Lean	0.006	-0.092	-0.098
	(0.111)	(0.095)	(0.094)
Gubernatorial Lean	-0.010	-0.087	-0.091
	(0.085)	(0.086)	(0.081)
Chamber FE	Yes	No	No
Observations	116	116	116
Within R-squared	0.051		
Pseudo-R-squared		0.048	0.050

Notes: This table reports regressions of the probability that a special election flips party control on pre-vacancy local economic conditions (state-level for Senate contests and district-level for House contests), political characteristics of the vacating officeholder, and local partisan preferences, as specified in equation (1). Column (1) reports OLS estimates from a linear probability model. Columns (2) and (3) report average marginal effects from logit and probit models, respectively. The dependent variable, 'PartySwitch,' equals one if the special election results in a party switch, and zero otherwise. The independent variables include three-year pre-vacancy averages of the unemployment rate, log real median household income, log house price index, and log PAC contributions, as well as the incumbent's most recent pre-vacancy general-election log victory margin and log tenure as of the vacancy, and binary indicators for presidential lean and gubernatorial lean, equal to one if the constituency voted for a candidate of the same party as the departing incumbent in their respective most recent elections preceding the vacancy, and zero otherwise. Standard errors are clustered at the state level and reported in parentheses. For Columns (2) and (3), standard errors for marginal effects are calculated using the delta method. Inference is robust to wild cluster bootstrap-t tests. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Senate Special Elections and Mortgage Lending

Dependent Variable	Loan Amount Growth			Loan Count Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
BankAlign	-0.001	-0.003	0.000	-0.005	-0.009	-0.005
	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.013)
PartySwitch ($\tau = -2$)	-0.014	-0.014	-0.012	-0.020	-0.020*	-0.021
	(0.014)	(0.014)	(0.016)	(0.012)	(0.012)	(0.015)
PartySwitch ($\tau = 0$)	-0.009	-0.010	0.002	0.007	0.006	0.011
	(0.012)	(0.012)	(0.016)	(0.012)	(0.012)	(0.014)
PartySwitch ($\tau = +1$)	-0.014	-0.013	-0.030	-0.0045	-0.005	-0.010
	(0.017)	(0.018)	(0.019)	(0.017)	(0.017)	(0.018)
PartySwitch ($\tau = +2$)	0.034*	0.034**	0.017	0.027	0.027	0.009
	(0.018)	(0.017)	(0.023)	(0.018)	(0.018)	(0.021)
BankAlign × PartySwitch ($\tau = -2$)	-0.005	-0.010	-0.005	0.023	0.022	0.023
	(0.038)	(0.037)	(0.038)	(0.047)	(0.046)	(0.047)
BankAlign × PartySwitch ($\tau = 0$)	0.096***	0.093***	0.095***	0.065**	0.063**	0.065**
	(0.034)	(0.034)	(0.034)	(0.030)	(0.031)	(0.030)
BankAlign × PartySwitch ($\tau = +1$)	0.087***	0.080***	0.086***	0.069**	0.065*	0.068**
	(0.022)	(0.023)	(0.022)	(0.032)	(0.034)	(0.032)
BankAlign × PartySwitch ($\tau = +2$)	0.026	0.019	0.025	0.049	0.046	0.048
	(0.062)	(0.063)	(0.062)	(0.056)	(0.057)	(0.056)
State Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	No	Yes	Yes	No	Yes
Bank × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank × State FE	No	Yes	No	No	Yes	No
Observations	213,437	213,396	213,437	213,437	213,396	213,437
Adjusted R-squared	0.376	0.363	0.376	0.433	0.422	0.433

Notes: This table reports dynamic treatment effects of party-switching Senate special elections on mortgage lending by aligned versus non-aligned banks, estimated from equation (2). The unit of observation is the bank–state–year, covering 1990–2023. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively. 'PartySwitch' equals one in state–years in which a special election results in a party switch, and zero otherwise. 'BankAlign' equals one if the bank is aligned with the incumbent senator's party in that state–year, and zero otherwise. 'SpecialElection' equals one in state–years in which a special election occurs, and zero otherwise. Columns (3) and (6) also include 'SpecialElection' event-time indicators (from $\tau = -2$ to $\tau = +2$); these coefficients are omitted for brevity. Coefficients are normalized relative to $\tau = -1$, the year immediately preceding the party switch, which is omitted as the reference period. State-level controls include the unemployment rate, log real median household income, log resident population, log house price index, the homeownership rate, the vacancy rate, log mortgage applications, and alignment with the state's second senator. Fixed effects are indicated in rows beneath the coefficients. Standard errors are clustered at the state level and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5: House Special Elections and Mortgage Lending

Dependent Variable	Loan Amount Growth			Loan Count Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
BankAlign	0.002	-0.014	0.007	0.006	-0.008	0.009
	(0.012)	(0.018)	(0.012)	(0.010)	(0.015)	(0.010)
PartySwitch ($\tau = -1$)	-0.059	-0.063	-0.082	-0.040	-0.022	-0.050
	(0.046)	(0.046)	(0.061)	(0.042)	(0.043)	(0.056)
PartySwitch ($\tau = 0$)	-0.078	-0.080	-0.094	-0.042	-0.045	-0.059
	(0.056)	(0.063)	(0.063)	(0.062)	(0.063)	(0.068)
PartySwitch ($\tau = +1$)	0.016	-0.031	-0.029	-0.009	-0.016	-0.043
	(0.071)	(0.071)	(0.074)	(0.064)	(0.063)	(0.066)
BankAlign × PartySwitch ($\tau = -1$)	0.063	-0.006	0.061	0.066	0.080	0.065
	(0.089)	(0.126)	(0.089)	(0.088)	(0.098)	(0.088)
BankAlign × PartySwitch ($\tau = 0$)	0.145**	0.137	0.144**	0.134*	0.190*	0.134*
	(0.067)	(0.118)	(0.066)	(0.075)	(0.103)	(0.075)
BankAlign × PartySwitch ($\tau = +1$)	-0.011	0.049	-0.012	0.043	0.059	0.042
	(0.090)	(0.093)	(0.090)	(0.078)	(0.079)	(0.078)
Congressional District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Congressional District FE	Yes	No	Yes	Yes	No	Yes
Bank × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Congressional District FE	No	Yes	No	No	Yes	No
Observations	167,395	159,265	167,395	167,395	159,265	167,395
Adjusted R-squared	0.406	0.377	0.406	0.457	0.434	0.457

Notes: This table reports dynamic treatment effects of party-switching House special elections on mortgage lending by aligned versus non-aligned banks, estimated from equation (2). The unit of observation is a bank–congressional district–year, covering 1990–2023. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively. 'PartySwitch' equals one in congressional district–years in which a special election results in a party switch, and zero otherwise. 'BankAlign' equals one if the bank is aligned with the incumbent representative's party in that congressional district–year, and zero otherwise. 'SpecialElection' equals one in congressional district–years in which a special election occurs, and zero otherwise. Columns (3) and (6) also include 'SpecialElection' event-time indicators (from $\tau = -1$ to $\tau = +1$); these coefficients are omitted for brevity. Congressional district-level controls include the unemployment rate, log total personal income, log resident population, log house price index, and log mortgage applications. Fixed effects are indicated in rows beneath the coefficients. Standard errors are clustered at the congressional district level and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Aligned Mortgage Lending: Switch vs. No-Switch Special Elections

Dependent Variable	Loan Amount Growth		Loan Amount Count	
	(1)	(2)	(3)	(4)
AlignSwitch ($\tau = 0$)	0.084**	0.071*	0.067**	0.095
	(0.033)	(0.041)	(0.031)	(0.057)
AlignNoSwitch ($\tau = 0$)	-0.053	-0.039	-0.021	-0.026
	(0.059)	(0.046)	(0.038)	(0.039)
Region Controls	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	Yes	Yes	Yes
Observations	213,437	167,395	213,437	167,395
Adjusted R-squared	0.376	0.406	0.433	0.457

Notes: This table reports the effects at $\tau = 0$ of party-switching versus no-switch special elections on the mortgage lending by aligned banks. The unit of observation is a bank–region–year, covering 1990–2023. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively. 'AlignSwitch' equals one in region–years in which a special election results in a party switch change, and zero otherwise. 'AlignNoSwitch' equals one in region–years in which a special election occurs without a party switch, and zero otherwise. Columns (1) and (3) report results for Senate contests (regions defined as states); columns (2) and (4) report results for House contests (regions defined as congressional districts). Region-level controls are identical to those used in Tables 4 and 5. Fixed effects are indicated in rows beneath the coefficients. Standard errors are clustered at the region level and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Senate Special Election and Enforcement Actions

Dependent Variable	Number of Enforcement Actions
$\sum$ PartySwitch $(\tau = -2)$	-0.509
	(0.519)
$\sum$ PartySwitch $(\tau = -0)$	0.267
	(0.595)
$\sum$ PartySwitch $(\tau = +1)$	0.063
	(0.568)
$\sum$ PartySwitch $(\tau = +2)$	-0.388
	(0.914)
$\sum$ BankAlign $\times$ PartySwitch $(\tau = -2)$	0.631
	(0.971)
$\sum$ BankAlign $\times$ PartySwitch $(\tau = 0)$	-2.010*
	(1.095)
$\sum$ BankAlign $\times$ PartySwitch $(\tau = +1)$	-2.484
	(2.005)
$\sum$ BankAlign $\times$ PartySwitch $(\tau = +2)$	-6.768**
	(2.685)
Bank Controls	Yes
Bank FE	Yes
Year FE	Yes
Observations	4,242
Pseudo-R-squared	0.359

Notes: This table reports Poisson pseudo-maximum likelihood estimates of the dynamic treatment effects of party-switching Senate special elections on enforcement actions by aligned versus non-aligned banks, estimated from equation (3). The unit of observation is the bank–year, covering 1990–2023. The dependent variable, 'Number of Enforcement Actions,' is defined as the count of publicly disclosed enforcement actions. 'PartySwitch' equals one in state–years in which a special election results in a party switch, and zero otherwise. 'BankAlign' equals one if the bank is aligned with the incumbent senator's party in that state–year, and zero otherwise. State shocks are aggregated to the bank–year level by weighting each state's event-time indicator by a fixed pre-election mortgage footprint $w^{\text{pre}}$and summing across states in which the bank operates. Coefficients are normalized relative to $\tau = -1$, the year immediately preceding the party switch, which is omitted as the reference period. Bank-level controls include log total assets, equity-to-assets ratio, ROA, loans-to-deposits ratio, and deposits-to-assets ratio. Fixed effects are indicated in rows beneath the coefficients. Standard errors are double clustered at the state and year level and reported in parentheses. $***$, $**$, and $*$ denote significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Senate Special Election and Municipal Bonds

Dependent Variable	Δ (Lead Par Share)	Δ (Lead Fee Share)
BankAlign	-0.006	-0.005*
	(0.003)	(0.003)
PartySwitch ($\tau = -2$)	-0.000	0.000
	(0.001)	(0.002)
PartySwitch ($\tau = 0$)	-0.003*	-0.001
	(0.001)	(0.002)
PartySwitch ($\tau = +1$)	-0.000	-0.000
	(0.001)	(0.002)
PartySwitch ($\tau = +2$)	0.001	-0.001
	(0.002)	(0.002)
BankAlign × PartySwitch ($\tau = -2$)	0.002	0.002
	(0.004)	(0.004)
BankAlign × PartySwitch ($\tau = 0$)	0.014**	0.017**
	(0.005)	(0.008)
BankAlign × PartySwitch ($\tau = +1$)	0.001	0.004
	(0.005)	(0.005)
BankAlign × PartySwitch ($\tau = +2$)	-0.000	0.002
	(0.007)	(0.006)
Deal Controls	Yes	Yes
State Controls	Yes	Yes
Bank × State FE	Yes	Yes
Year FE	Yes	Yes
Observations	27,758	27,758
Adjusted R-squared	-0.044	-0.042

Notes: This table reports dynamic treatment effects of party-switching Senate special elections on municipal-bond lead-underwriting allocations by aligned versus non-aligned banks, estimated from equation (4). The unit of observation is the bank–state–year, covering 1990–2023. The dependent variables, 'Δ (Lead Par Share)' and 'Δ (Lead Fee Share),' are defined as the annual changes in the bank's state–year shares of lead-managed par amount and lead-underwriting fee revenue, respectively, where shares are computed relative to the corresponding state–year totals. 'PartySwitch' equals one in state–years in which a special election results in a party switch, and zero otherwise. 'BankAlign' equals one if the bank is aligned with the incumbent senator's party in that state–year, and zero otherwise. Coefficients are normalized relative to $\tau = -1$, the year immediately preceding the party switch, which is omitted as the reference period. Deal-composition controls include par-weighted state–year shares for par amount, credit ratings, offering method, yield on final maturity, and years to final maturity. State-level controls include the unemployment rate, log per capita income, log resident population, and alignment with the state's second senator and governor. Fixed effects are indicated in rows beneath the coefficients. Standard errors are clustered at the state level and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Senate Special Elections and Mortgage Lending Risk Proxies

Dependent Variable	Loan Amount Growth			Loan Count Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
BankAlign	-0.012	-0.022**	-0.004	-0.015	-0.021*	-0.015
	(0.012)	(0.008)	(0.014)	(0.012)	(0.011)	(0.014)
PartySwitch ($\tau = -2$)	-0.007	-0.003	-0.013	-0.013	-0.011	-0.014*
	(0.015)	(0.013)	(0.009)	(0.010)	(0.008)	(0.008)
PartySwitch ($\tau = 0$)	-0.006	-0.011	0.007	0.011	-0.009	0.002
	(0.012)	(0.011)	(0.013)	(0.013)	(0.009)	(0.010)
PartySwitch ($\tau = +1$)	-0.016	-0.013	0.003	-0.007	-0.009	0.000
	(0.018)	(0.013)	(0.013)	(0.018)	(0.012)	(0.011)
PartySwitch ($\tau = +2$)	0.032*	-0.004	0.000	0.025	0.001	0.007
	(0.017)	(0.009)	(0.010)	(0.017)	(0.008)	(0.009)
BankAlign × PartySwitch ($\tau = -2$)	-0.003	0.0323	-0.037	0.025	0.029	-0.007
	(0.039)	(0.020)	(0.064)	(0.047)	(0.021)	(0.048)
BankAlign × PartySwitch ($\tau = 0$)	0.100***	0.066***	0.033	0.066**	0.062**	0.030
	(0.031)	(0.023)	(0.037)	(0.029)	(0.023)	(0.028)
BankAlign × PartySwitch ($\tau = +1$)	0.095***	0.072***	0.025	0.067**	0.025	0.038*
	(0.022)	(0.020)	(0.021)	(0.029)	(0.017)	(0.021)
BankAlign × PartySwitch ($\tau = +2$)	0.049	0.063*	0.018	0.059	0.051**	0.032
	(0.068)	(0.033)	(0.023)	(0.058)	(0.021)	(0.022)
State Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	212,454	194,216	189,422	212,454	194,216	189,422
Adjusted R-squared	0.366	0.342	0.327	0.429	0.374	0.386

Notes: This table reports dynamic treatment effects of party-switching Senate special elections on mortgage lending risk proxies for aligned versus non-aligned banks, estimated from equation (2). The unit of observation is a bank–state–year, covering 1990–2023. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively. Results are reported for three lending risk proxies: conventional mortgages (columns (1) and (4)), loans to minority borrowers (columns (2) and (5)), and loans to non-owner-occupied properties (columns (3) and (6)). 'PartySwitch' equals one in state–years in which a special election results in a party switch, and zero otherwise. 'BankAlign' equals one if the bank is aligned with the incumbent senator's party in that state–year, and zero otherwise. Coefficients are normalized relative to $\tau = -1$, the year immediately preceding the party switch, which is omitted as the reference period. State-level controls are identical to those used in Table 4. Fixed effects are indicated in rows beneath the coefficients. Standard errors are clustered at the state level and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 10: House Special Elections and Mortgage Lending Risk Proxies

Dependent Variable	Loan Amount Growth			Loan Count Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
BankAlign	-0.001	-0.026*	0.003	0.004	-0.025*	-0.003
	(0.012)	(0.014)	(0.017)	(0.010)	(0.014)	(0.014)
PartySwitch ($\tau = -1$)	-0.072	-0.102	0.101*	-0.048	-0.090	0.071
	(0.049)	(0.069)	(0.057)	(0.041)	(0.057)	(0.050)
PartySwitch ($\tau = 0$)	-0.084	-0.176	0.042	-0.052	-0.134	0.112
	(0.061)	(0.124)	(0.092)	(0.066)	(0.113)	(0.090)
PartySwitch ($\tau = +1$)	0.038	0.090	-0.096	0.007	0.087	-0.105
	(0.068)	(0.075)	(0.085)	(0.064)	(0.065)	(0.085)
BankAlign × PartySwitch ($\tau = -1$)	0.121	0.100	-0.025	0.110	0.106	0.073
	(0.095)	(0.106)	(0.107)	(0.090)	(0.057)	(0.100)
BankAlign × PartySwitch ($\tau = 0$)	0.154**	0.205	-0.030	0.153**	0.186	-0.021
	(0.069)	(0.151)	(0.141)	(0.075)	(0.139)	(0.137)
BankAlign × PartySwitch ($\tau = +1$)	-0.033	0.064	0.389**	0.028	0.085	0.297**
	(0.093)	(0.143)	(0.196)	(0.083)	(0.101)	(0.143)
Congressional District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Congressional District FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	161,706	83,871	61,475	161,706	83,871	61,475
Adjusted R-squared	0.392	0.306	0.322	0.449	0.325	0.374

Notes: This table reports dynamic treatment effects of party-switching House special elections on mortgage lending risk proxies for aligned versus non-aligned banks, estimated from equation (2). The unit of observation is a bank–congressional district–year, covering 1990–2023. The dependent variables, 'Loan Amount Growth' and 'Loan Count Growth,' are defined as annual log changes in originated loan amount and loan counts, respectively. Results are reported for three lending risk proxies: conventional mortgages (columns (1) and (4)), loans to minority borrowers (columns (2) and (5)), and loans to non-owner-occupied properties (columns (3) and (6)). 'PartySwitch' equals one in congressional district–years in which a special election results in a party switch, and zero otherwise. 'BankAlign' equals one if the bank is aligned with the incumbent representative's party in that congressional district–year, and zero otherwise. Congressional district-level controls are identical to those used in Table 5. Fixed effects are indicated in rows beneath the coefficients. Standard errors are clustered at the congressional district level and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.