Tax Breaks for Swing States? Political Bargaining, Economic Redistribution, and Firm Outcomes*

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Abstract

We study how firms are affected by the political bargaining power of their home regions. Exploiting shocks to the strategic importance of swing states relative to partisan states stemming from partisan gridlock in the U.S. Senate, we show that corporate valuations and investments increase in response to predicted increases in regional political bargaining power. We further verify the valuation findings using an event study based on the 2021 Georgia runoff election that produced an unexpected balancing of the Senate. Reconciling our positive investment findings with previous studies that find a negative crowding-out effect, we show that tax incentives (rather than demand spillovers) form the driving mechanism behind our findings.

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1 Introduction

Private-sector firms are significantly affected by the federal government's policies on regulation, trade, spending, and taxation. In a representative democracy like the United States, policy decisions are made through a complicated bargaining process involving elected representatives who are accountable to their regional constituents. Through this process, firms are exposed to fluctuations in the political bargaining power of their home regions. A recent illustrative example comes from West Virginia, where coal companies benefited from Senator Joe Manchin's ability to leverage his pivotal vote in the U.S. Senate to carve out provisions protecting the coal industry during the passage of the Inflation Reduction Act.[1] With the U.S. government becoming more willing to pursue large-scale economic interventions aimed at overhauling the domestic economy,[2] private-sector firms are likely to find themselves increasingly affected by the politics surrounding economic redistribution.

In this paper, we study how firms respond to changes in the strategic political bargaining power of their home regions. When control of the presidency or Congress is at a tipping point, electorally competitive regions (i.e., "swing" regions) that can swing the balance of power in either direction may find themselves with strategic leverage to attract resources from national political actors. In particular, legislative representatives from swing regions often hold crucial swing votes that can be leveraged to bargain for policies that favor their constituents. Moreover, political parties seeking to win national elections often cater to swing regions in exchange for votes that could tip the electoral map in their favor (Lindbeck and Weibull, 1987; Dixit and Londregan, 1995), as recently evidenced by electoral battleground districts receiving higher amounts of federally-subsidized lending during the COVID-19 crisis (Duchin and Hackney, 2021).

The ultimate effect of such cyclical shifts in regional political bargaining power on local firms is not obvious ex ante. While firms may benefit from the ability of their regional representatives to shape public policy, elevated regional political bargaining power can come with economic downsides. First, politicians are ultimately more concerned with winning elections than maximizing shareholder value, raising the possibility that they may pursue policies that are not in their constituent firms'

[1] See `https://www.nytimes.com/2022/07/30/climate/manchin-climate-deal.html`.

[2] According to some estimates, nearly $2 trillion in federal funding for infrastructure upgrades, renewable energy projects, and domestic semiconductor manufacturing will flow into the U.S. economy over the next ten years. See `https://www.mckinsey.com/industries/public-and-social-sector/our-insights/reinvesting-in-america`.

interests. Notably, prior research has found that federal spending directed by powerful legislators toward their home regions crowds out private-sector investment (Cohen, Coval, and Malloy, 2011). Second, a region's increased influence over federal policy is transitory, as it depends on a national political landscape that can rapidly shift from election to election.[3] Based on research showing that higher uncertainty reduces the responsiveness of investment (Bloom, 2009), firms may be unwilling to take advantage of favorable policies that are fleeting in nature.

Our empirical strategy exploits state-level shifts in political bargaining power that arise from the changing balance of the U.S. Senate. Specifically, we compare firms located in electorally competitive states ("swing states") and non-competitive states ("partisan states") across periods when control of the Senate is evenly divided between the two parties ("gridlocked Senates") and when one party has consolidated control of the Senate ("consolidated Senates"). Importantly, we use within-state vote margins from the prior presidential election rather than the prior Senate election to classify swing and partisan states, as election results for the presidency are less likely to be correlated with state-level economic confounders than those for a Senate seat.[4] Essentially, voters are more likely to base their presidential vote on ideology rather than regional economic interests.

We predict that swing states should elevate their political bargaining power relative to partisan states in gridlocked Senates relative to consolidated Senates. We verify this by examining the allocation of federal financial assistance across states and find that assistance directed at swing states increases by over 4% (approximately $2.17 billion for the median state) relative to spending directed at partisan states when the Senate is relatively gridlocked. We also examine federal subsidies directed at individual firms and find evidence that swing-state firms receive a relative increase in tax credit subsidies during gridlocked Senates, consistent with abundant anecdotal evidence of centrist legislators using their political leverage to win favorable tax treatment for their home region's key industries.[5]

[3] Moreover, policy initiatives that directly affect firms, such as tax credit programs, are also often temporary in nature. Notably, the use of "sunset" provisions has become common in tax cut bills since 2001 (Gale and Orszag, 2003).

[4] We note that although Senator Manchin is considered a swing senator, he does not represent a swing state (West Virginia is a strongly Republican-leaning state). In our empirical robustness tests, we examine an alternative proxy for swing senators by measuring presidential votes *against* the party of incumbent senators.

[5] For example, two centrist senators from Maine used their leverage in a gridlocked Senate in 2010 to demand small business tax breaks and stimulus spending (in the case of Senator Olympia Snowe) and for industry tax breaks aimed at Maine timber companies (in the case of Senator Susan Collins). See `https://www.politico.com/story/2010/02/three-who-could-make-break-jobs-bill-033256`.

We examine several outcome variables in our firm-level analysis. Given that the balance of the Senate can change as frequently as every two years, our empirical design is best suited to detect the effects on leading indicators that respond rapidly to changes in expectations about government redistribution. Therefore, we focus mainly on forward-looking measures that capture shifts in expectations of future government rents when the balance of the Senate shifts around national elections. These include market valuation ratios (i.e., Tobin's Q), which should capture shifts in investors' expectations, and investment rates, which should capture shifts in managers' expectations. In contrast, the short-term effects on operating performance are more difficult to detect in our setting, as it may take several years for a firm's cash flows to be affected by the increased bargaining power of its home region.

We find that the valuations and investment rates of swing-state firms increase relative to those of partisan-state firms when the Senate is relatively gridlocked, indicating that firms benefit from the political bargaining power of their home regions. Specifically, swing-state firms' market valuation ratios (i.e., Tobin's Q) increase relative to those of partisan-state firms by 0.2 (5.5% of the sample mean) when the Senate is relatively gridlocked. There is a similar positive effect on investments, particularly in intangible capital as proxied by SG&A expenditures-to-assets (a 7.4% increase relative to the sample mean) and R&D expenditures-to-assets (an 11% increase relative to the sample mean). We find a smaller and statistically marginal effect on tangible investments (i.e., capital expenditures-to-assets), which is consistent with physical capital being less responsive to short-lived cyclical shifts in regional political power as it typically requires longer lead times to install than intangible capital (Hall and Hayashi, 1989) before it can benefit from favorable government policies.[6] We find no evidence of short-term improvements in operating performance, which suggests that our valuation and investment findings are not driven by regional demand trends.

To sharpen our identification of how regional political bargaining power affects firms' market valuations, we exploit a natural experiment arising from the U.S. Senate runoff elections in Georgia on January 5, 2021, in which two Democratic candidates defeated their Republican opponents by extremely narrow margins. This produced an unexpected swing in the margin of Senate control from a potential 52-48 Republican majority to a 50-50 split, thereby increasing the relative political

[6] Moreover, prior research has found that policy uncertainty can inhibit corporate investment (Julio and Yook, 2012; Gulen and Ion, 2016; Baker, Bloom, and Davis, 2016) and Ahn (2019) argues that physical investment responds to economic uncertainty more negatively than intangible investment.

bargaining power of swing states. Using an event study in which we exclude all firms based in Georgia, we find that the cumulative abnormal returns of swing-state firms outperformed those of partisan-state firms by 1.3 percentage points immediately following the runoff elections.

Using a state-level version of our benchmark panel analysis, we find that swing states increase their overall economic performance relative to partisan states when the Senate is relatively gridlocked. The effects range from a 0.93% increase in state-level private-sector employment to a 1.64% increase in state-level output. We further find that the effects on employment are broadly distributed across firms of different size categories. This suggests that our benchmark firm-level findings, based on a sample of large publicly traded firms, are not purely driven by political rents being directed exclusively at large firms with political connections.

We note that our positive findings on valuations and investments contrast against the negative crowding out effects documented in Cohen et al. (2011). This difference likely stems from the different policy mechanisms driving our respective findings. In particular, Cohen et al. (2011) focuses on earmark spending as the primary policy channel that crowds out private-sector investment, while we show tax credits serve as the driving mechanism behind increasing investments. This may ultimately stem from the difference in the bargaining power wielded by long-serving powerful congressional chairs who have time to push for long-term public spending programs and swing-state Senators who find it more expedient to deploy their short-lived political leverage on quickly implemented tax subsidy programs. Moreover, we document that incumbent Senators are more electorally vulnerable in swing states relative to partisan states, suggesting that they may have stronger incentives to stimulate short-run regional growth in order to court swing voters.

We further explore the tax incentive channel in greater depth by examining firms' effective tax rates. Importantly, effective tax rates capture the aggregate effect of all tax credit programs, including those targeted at entire industries in addition to those targeted at specific firms. We find that swing-state firms experience a relative decrease in federal tax rates of 59 basis points (6.1% of the sample mean) during gridlocked Senates relative to consolidated Senates. This translates to annual tax savings of $6.3 million for the median firm in the sample. Our estimates weaken significantly when we include industry-year fixed effects, suggesting that lower federal tax rates are driven by tax policies targeted at regionally concentrated industries rather than at individual firms. We conduct placebo tests using state and foreign effective tax rates (which should be unaffected

by the bargaining power of federal legislators) and find no effect, regardless of whether we include industry-year fixed effects.

We further investigate potential mechanisms by conducting heterogeneity tests on our main findings. First, we show that our benchmark firm-level findings are not concentrated in industries more sensitive to local economic demand (i.e., non-tradable sectors). This indicates our main findings are unlikely to be driven by local demand spillovers arising from a fiscal multiplier effect. Next, we split our sample into industries that are more and less exposed to import penetration and find that our benchmark findings are more pronounced in firms belonging to high-import-penetration-exposure industries, suggesting that favorable trade policies may partially explain our main findings. Lastly, we split our sample into industries that face high and low levels of federal regulation but do not find a significant difference in our benchmark results in these subsamples, indicating that regulatory policy does not play a significant role in explaining our findings.

Our paper contributes to the literature exploring the relationship between politicians' influence over policy and their constituents' economic fortunes. These studies largely focus on forms of formal political authority, such as official committee chairmanships and majority control over branches of the government. For example, prior studies have found that powerful congressional committee members and chairs direct greater shares of federal spending to their districts (Alvarez and Saving, 1997; Cohen et al., 2011; Duchin and Sosyura, 2012), and shape policies to benefit constituent companies (Roberts, 1990; Gropper, Jahera Jr, and Park, 2013; Akey, Heimer, and Lewellen, 2021). Others have used sudden shifts in congressional control to show that firms benefit from being aligned with the party in power (Jayachandran, 2006; Den Hartog and Monroe, 2008; Goldman, Rocholl, and So, 2013). In contrast, we study a form of political power that has received scant prior attention from economists: regional political power stemming from strategic bargaining between politicians. Notably, our positive findings on valuations and investments contrast against the negative crowding-out effects documented in Cohen et al. (2011), suggesting that the source and duration of political power matters in determining its economic effects.

Our paper also relates to the literature exploring the relationship between electoral competition and redistributive politics. Earlier studies provide a theoretical basis for the incentives of political parties to allocate more resources to swing voters (Lindbeck and Weibull, 1987; Dixit and Londregan, 1995) and subsequent papers have uncovered empirical evidence that politicians

tend to reward swing regions with higher amounts of spending (Wright, 1974; Bickers and Stein, 1996; Arulampalam, Dasgupta, Dhillon, and Dutta, 2009) and favorable trade policies (Muûls and Petropoulou, 2013; Ma and McLaren, 2018). In studies focusing on firms, Choi, Jia, and Lu (2015) finds political competition can constrain politicians from being influenced by corporate lobbying, Delatte, Matray, and Pinardon-Touati (2019) finds that incumbents facing contested elections influence regional banks to increase credit supply, and Christensen, Jin, Sridharan, and Wellman (2022) shows that firms balance their political connections across Republican and Democratic candidates to hedge against political risk induced by partisan competition. We contribute to the literature by showing the effect of regional electoral competition on firms depends on the balance of power at the national level.

Lastly, our paper relates to the growing literature in finance on the effects of political partisanship. Much of this literature focuses on how the partisan preferences of investors and financial intermediaries affect investors and asset prices. For example, studies have shown that partisan alignment shapes the beliefs of equity investors (Cookson, Engelberg, and Mullins, 2020; Sheng, Sun, and Wang, 2023), impacts the borrowing costs of local governments (Dagostino and Nakhmurina, 2023), affects the portfolio allocation decisions of fund managers (Wintoki and Xi, 2020), the loan pricing decisions of bankers (Dagostino, Gao, and Ma, 2023), and the patenting decisions of inventors (Engelberg, Lu, Mullins, and Townsend, 2023). Rather than examining the partisan alignment between economic agents and political parties, however, we exploit changes in overall partisan competition between political parties to identify shifts in regional political power.

2 Empirical Methodology

We exploit changes to the internal power dynamics of the U.S. Congress to identify changes in regional political bargaining power. Several institutional factors lead us to use the Senate as an empirical setting to identify changes in state-level political influence rather than the House of Representatives to identify changes in district-level political influence. First, as there are significantly fewer states (50) than congressional districts (435 members), an individual state possesses much greater political leverage than an individual district. Second, as congressional districts are typically quite small (much smaller than states), many districts do not contain any publicly traded firms

that form the units of our analysis. Third, our empirical strategy involves comparing periods of wide Senate majorities and narrow Senate majorities, and while the margin of control in the Senate has frequently been very narrow during our analysis period, the margin of control in the House of Representatives has rarely been close.[7]

2.1 Institutional background on the U.S. Senate

Each of the 50 U.S. states is represented by two senators who serve staggered terms of six years.[8] The party that holds a majority of the 100 seats forms the majority party, and in case of a 50-50 tie, the vice president has the power to cast the tie-breaking vote. However, a simple majority is not always sufficient to pass a bill through the Senate due to the use of "filibustering", a tactic in which the minority party prolongs debate indefinitely to prevent a bill from coming up for a floor vote. A filibuster can be defeated by three-fifths of the Senate (i.e., by 60 members) through the "cloture" procedure that forces debate to end, effectively making the 60th vote the pivotal one for many Senate bills (Krehbiel, 1998).

Although a 60-seat filibuster-proof majority represents an important threshold for Senate control, we focus on the 51-seat simple majority threshold in our empirical analysis for two reasons. First, our empirical strategy relies on comparisons between periods when power is held by a narrow majority (i.e., a "gridlocked" Senate) and periods when it is held by a wide majority (i.e., a "consolidated" Senate), but no party ever achieved a filibuster-proof majority during our sample period. The Democrats came closest, holding a 57-43 seat advantage during the 103rd Congress (1993-1994) and a 59-41 seat advantage during the 111th Congress (2009-2010).

Second, a simple 51-seat majority is crucial for bills related to spending and taxes. Specifically, a procedure known as "reconciliation" allows the majority party to pass budget bills even if they do not have the 60 votes necessary to prevent a filibuster.[9] In reconciliation, one or more congressional committees are tasked with drafting a budget resolution, which is then introduced to the Senate floor for debate. Importantly, the debate over the resolution is limited to 20 hours, removing the

[7] In our analysis period (1994-2020), the narrowest margin in the House of Representatives occurred in the 107th term of Congress (2001-2002). The Republican Party held the majority with a nine-seat margin.

[8] While all senators serve six years per term, not all senators are elected simultaneously. Every two years, elections are held for approximately one-third of the Senate's seats.

[9] A procedural constraint known as the "Byrd Rule" prevents reconciliation from being used more widely to expedite non-budget legislation.

possibility of indefinite filibustering and effectively allowing bills to be passed with a simple 51-seat majority. After the debate has expired and before the final Senate vote, a session colloquially known as "vote-a-rama" is held where senators can propose and vote on bill amendments until the parties agree to stop. This is when legislators can freely exercise their political leverage to insert desired provisions into the bill.

Importantly, the majority party must either marshal unified support from their own members or garner bipartisan support from members of the opposing party to pass budgetary bills during gridlocked Senates, even under the reconciliation process. Therefore, individual senators or small coalitions can leverage their votes to extract concessions from party leaders during gridlocked Senates. This may involve a majority party member receiving side benefits for toeing the party line or a minority member receiving side benefits for providing support from "across the aisle". As federal spending and tax policy form the primary channels through which the exercise of political influence affects private-sector firms, the reconciliation procedure makes the 51-seat majority threshold crucial to our analysis.

2.2 Identifying cyclical shifts in regional political bargaining power

Our identification strategy is based on the insight that swing states should increase their political influence over redistribution relative to partisan states when the Senate is relatively gridlocked. We offer three reasons for this prediction. First, while a gridlocked Senate hypothetically makes every senator's vote more pivotal in the passage of budget-related bills, swing-state senators should have greater *credibility* in their ability to vote against their own party. This is because they must appeal to a more politically-moderate electorate, and therefore are less likely to face reprisals when they defy their party leaders during legislative voting.[10] In contrast, a partisan-state senator who votes with the opposing party risks upsetting his base and facing primary election challenges.[11] In our analysis, we verify this difference in legislative voting behavior using data on roll-call votes in the U.S. Senate.

[10] For example, Democrats were unable to punish senators Manchin and Sinema for blocking key legislative initiatives due to the fear of losing their seats in future elections. See `https://thehill.com/homenews/senate/3564936-democrats-have-no-appetite-to-punish-joe-manchin/`.

[11] In the most extreme example, House majority leader Eric Cantor was defeated by a Republican challenger after being accused of being soft on immigration and supporting amnesty for illegal immigrants. See `https://www.nytimes.com/2014/06/11/us/politics/eric-cantor-loses-gop-primary.html`.

Second, even if partisan-state senators are occasionally willing to defy their party, swing-state senators are more likely to use gridlocked Senates as an opportunity to bargain for *redistributive* economic policies that benefit their constituents. This is because swing senators cannot easily rely on a partisan base to win general elections, and therefore have a greater incentive to "bring home the bacon" to sway swing voters in their state. In contrast, partisan-state senators are more likely to use gridlocked Senates as a way to seek concessions related to ideological matters to appeal to their political base. This is more likely to involve social issues related to the "culture war" between Democrats and Republicans or ideological economic issues relating to aggregate taxes and spending and less likely to involve regional economic concerns.[12]

Lastly, given the importance of the 51-seat margin for budget-related bills, political parties have a greater incentive to tip the balance of the Senate in their favor when it is near gridlock. Essentially, the marginal value of flipping or holding onto winnable seats (i.e., swing states) increases from the perspective of party leaders when control of budget-setting powers in the Senate is near a tipping point. To this end, they are more likely to strategically direct government transfers to swing states during gridlocked Senates in an attempt to win or maintain control of the Senate budget-setting process.

Based on these insights, we compare firms in swing states and partisan states during gridlocked and consolidated Senates using the following specification:

$$Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta LockedSenate_t + \delta Swing_{it} \times LockedSenate_t + X_{it}\beta + \epsilon_{it}, \qquad (1)$$

in which Y_{it} represents a firm-level outcome, $Swing_{it}$ represents an indicator for whether a firm i is located in a state where the absolute vote margin between the Democratic and Republican party in the last presidential election before t was below the median vote margin in that election, $LockedSenate_t$ represents an indicator for whether the Senate majority margin in year t is no greater than six seats (the median seat margin in our sample), α_i and γ_t represent firm and year fixed effects, X_{it} represents a vector of control variables, and ϵ_{it} represents the residual error term.[13]

In our analysis, we examine several firm outcome variables. Given that the Senate can shift

[12] For example, during the recent hard-line Republican revolt against the selection of Kevin McCarthy as Speaker of the House, one of the sticking issues related to imposing limits on overall federal spending.

[13] The effects of $LockedSenate_t$ are absorbed by our year fixed effects. We include the variable in our regression specification for completeness.

rapidly between gridlocked and consolidated (as frequently as every two years), our specification is designed to identify short-term effects on outcomes that respond quickly to changes in regional political influence. Therefore, we focus mainly on forward-looking measures, including market valuation ratios (i.e., Tobin's Q), which should incorporate investors' expectations about future political rents, and investment rates (for both tangible and intangible assets), which should incorporate managers' expectations about future political rents.[14] Our coefficient of interest is δ, which should capture the increase in the political bargaining power of swing states relative to that of partisan states during gridlocked Senates relative to consolidated Senates. We should expect greater regional political bargaining power to increase valuation and investment ratios (i.e., $\delta > 0$) if politicians use their elevated influence to pursue policies that benefit their constituent firms. However, regional political bargaining power may come with economic downsides if the political interests of politicians are not aligned with the economic interests of the firms they represent. Notably, prior research shows that greater federal spending may unintentionally crowd out private-sector investment (Cohen et al., 2011; Kim and Nguyen, 2020).

We also examine operating performance measures, including sales growth and revenue growth, as outcome variables. Because our specification is designed to capture frequent cyclical shifts in regional political influence, the effect on relatively slow-reacting operating performance measures (in contrast to forward-looking valuations and investments) may be difficult to identify. For example, by the time a region's increased political bargaining power has translated into higher (or lower) corporate cash flows through implemented policies, the balance of the Senate may have already reverted to a less favorable state. In such instances, the estimated δ from Eq. 1 may even have the opposite sign from the actual (delayed) effect of regional political influence on corporate cash flows.[15] Therefore, we focus our attention primarily on forward-looking valuation and investment outcome measures.

The identifying assumption underpinning Eq. 1 is that any differences between swing states and partisan states that affect local firm outcomes do not systematically covary with Senate majority

[14] Essentially, changes in expectations about future government rents (e.g., tax subsidies) should affect a firm's NPV calculations and therefore have an immediate effect on investment decisions. This could be at the intensive margin (i.e., affecting the optimal scale of certain projects) or the extensive margin (i.e., affecting the viability of marginal projects).

[15] A possible solution is to use lagged regressors in estimating Eq. 1 to account for delays in cash flow effects, but the appropriate length of the delay is not clear ex ante.

margins, *except* for those arising from changes in regional political bargaining power. To the extent that the overall partisan balance of the Senate is exogenous to heterogeneity in economic trends across states, this assumption should be satisfied. In our empirical analysis, we perform heterogeneity tests to alleviate concerns about diverging regional trends biasing our δ estimates. Specifically, we compare δ estimates across samples of tradable-sector and non-tradable-sector firms. If our baseline estimates are driven by regional fundamentals, then the effects should be more pronounced in non-tradable sectors containing firms that are more exposed to local demand.

We double-cluster standard errors by state and industry (defined at the 4-digit NAICS level) in all panel regressions involving annual firm outcomes. We cluster by state because swing status is defined at the state level. We further cluster by industry to account for the possibility that senators may target their efforts at their home states' specialized industries. Redistributive policies related to spending, taxation, and regulation are likely to apply to entire industries rather than individual firms, and clustering by industry allows us to account for within-industry correlations in firm outcomes.[16]

We use state-level presidential election results to define our *Swing* indicator variable as it is less likely to be endogenous to local economic conditions or senator characteristics than Senate seat election results. This measure may not fully capture the political influence of senators who represent states that are deeply partisan in favor of the opposing party, as in the case of Democratic Senator Joe Manchin representing deeply conservative West Virginia. However, such instances are relatively rare. Moreover, the ability of a candidate to win a Senate seat against deep voter support for the opposing party may be endogenous to unobserved characteristics of that candidate and state. Nevertheless, we conduct robustness tests to check whether our results are robust to using an alternative proxy for swing senators based on the vote margin *against* the senator's party in the previous presidential election.

Our explanatory variable of interest is the interaction term between indicators for competitive elections at the state level (*Swing*) and a balanced Senate at the national level (*LockedSenate*). We note that there is no mechanical relationship between these variables, as close state-level elections do not automatically result in a balanced Senate. In fact, growing political polarization has created a sharp divide between deeply Democratic and Republican states, producing increasingly lopsided

[16] For this reason, we omit *Industry* $\times$ *Year* fixed effects in our benchmark specification.

elections at the state level while the Senate remains frequently gridlocked. Moreover, by defining *Swing* based on whether the vote margin was above or below the national median in a particular election, the proportion of swing states and partisan states remains roughly the same across time, regardless of whether the Senate is gridlocked.

In general, the *Swing* status for a given state tends to persist over time due to the long-term stability of political predispositions (Sears and Funk, 1999). For our findings to be driven by omitted variables related to infrequent changes in a state's *Swing* status, the confounding factor would also have to coincide systematically with changes in Senate balance. We address such endogeneity concerns by performing robustness tests in which we define time-invariant indicators for swing states. Specifically, we redefine *Swing* based on the time-series mode over our sample period—i.e., a state is classified as a swing state for the entire sample period as long it is more often a swing state than not—and check whether our benchmark findings are robust to this alternative definition.

Lastly, we note that our benchmark specification resembles a generalized difference-in-differences (DiD) framework. Since the timing of gridlocked and consolidated Senates is common to all firms, we avoid issues with two-way fixed effect estimates from difference-in-difference settings with *staggered* timing of treatment (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021; Athey and Imbens, 2022). However, a key difference between our specification and a DiD framework is that there is no "pre-treatment" period in our setting. While consolidated Senate periods form the baseline period in our specification, we should not necessarily expect economic trends for swing states and partisan states to be parallel during consolidated Senates. Nevertheless, we perform dynamic versions of our benchmark tests to ensure that our findings are not driven by long-run trends that happen to be centered around changes in Senate balance.

2.3 Georgia runoff election event study

We take advantage of higher-frequency data on stock returns to sharpen the identification of how regional political bargaining power affects firm value. Specifically, we conduct an event study based on the Senate runoff elections held in Georgia on January 5, 2021. During the regularly scheduled Senate elections held the previous November, the tightly-contested elections for Georgia's two Senate seats failed to produce winners as no candidate received more than 50% of the vote in either

election.[17] This resulted in the Republican Party holding a narrow 50-48 lead in the Senate, with only the two Georgia seats yet to be decided. In the days leading up to January 5, pre-election polling margins for both runoff races were tightly within the margin of error,[18] creating a high degree of uncertainty over the ultimate balance of the Senate.

Despite Republicans having held both Senate seats for Georgia since 2005, Democratic candidates Raphael Warnock and Jon Ossoff defeated their Republican opponents Kelly Loeffler and David Perdue, respectively. This swung the balance of the Senate from a potential 52-48 Republican majority to an even 50-50 split.[19] While the difference between these seat margins does not seem large, a 50-50 Senate split is special in that any single senator could determine the fate of key pieces of the party's legislative agenda without needing to coordinate with their colleagues to form a voting bloc.[20] In fact, political media outlets at the time noted that a 50-50 vote would amplify the voices of moderate legislators in the Senate.[21] Indeed, the first budget vote held one month after the runoff election passed by the narrowest possible vote of 51-50, with Vice President Kamala Harris casting the deciding vote. The budget's passage included several rounds of amendment votes (the "vote-a-rama" ritual described in Section 2.1) in which centrist senators led by Joe Manchin and Susan Collins demonstrated their bargaining power by inserting significant amendments into the resolution.[22]

Treating the sudden shift in the Senate's partisan balance as a positive shock to the political influence of swing states over partisan states, we compare the stock returns of firms located in swing states and partisan states around the time of the runoff election. Following the suggestion from MacKinlay (1997) to use disaggregated security-level data when an event occurs on the same day for many firms, we estimate a multivariate regression model with dummy variables for the event

[17] During normal times, only one Senate seat per state would be up for election. However, Republican Senator Johnny Isakson resigned in 2019, and a special election was held in 2020 to select a Senator to serve out the remainder of the term until January 3, 2023.

[18] See `https://www.cookpolitical.com/analysis/senate/georgia-senate/one-day-out-will-trump-torpedo-republican-chances-georgia-runoffs` and `https://centerforpolitics.org/crystalball/articles/gop-likely-needs-a-big-georgia-turnout-today/`.

[19] Since the U.S. Vice President held the tie-breaking vote in the Senate, this also gave the Democrats control of both chambers of Congress in addition to the presidency.

[20] A 50-50 split is rare, occurring only three other times in the Senate's history. The only other time it occurred within our sample period was in 2000, and that was short-lived, as Republicans took control with a two-seat margin in June 2001.

[21] See `https://www.politico.com/news/2020/09/17/50-50-senate-control-416424`.

[22] See `https://www.cnn.com/2021/02/05/politics/senate-budget-resolution-covid-19-relief/index.html`.

dates:

$$AR_{it} = \alpha_i + \gamma_t + \sum_{\tau} \pi_\tau Swing_i \times \tau_t + \epsilon_{it} \tag{2}$$

where AR_{it} represents firm i's abnormal return on date t, $Swing_i$ represents an indicator for whether firm i is located in a state where the absolute vote margin between the Democratic and Republican candidates in the 2020 presidential election is below the sample median, and τ_t is a dummy variable that indicates whether $t = \tau$, where τ takes on values starting from five trading days before the election to five trading days following the election. In addition to examining abnormal returns estimated using the CAPM and Fama-French 3-factor (FF3) models, we also examine raw returns unadjusted for risk. We further estimate Eq. 2 using cumulative abnormal returns (CAR_{it}) as the dependent variable, where we define CAR_{it} as the sum of daily abnormal returns from 10 trading days prior to the election to date t. In all our event study regressions, we double-cluster standard errors by state and date to account for cross-sectional correlations in stock returns. Moreover, we exclude all firms located in Georgia to rule out any direct effects the election may have had on firms within the state.

The ballot count on election day (January 5) showed a lead for both Democratic candidates, but the margins were extremely small, and major media outlets did not call either election until the following day, January 6.[23] If investors believe that firms benefit from the strategic political influence of their political representatives, then we should expect swing-state firms to experience relatively higher abnormal returns on election day ($\tau = January\ 5$), when the Democratic candidates established a lead in the ballot count, and on the following day ($\tau = January\ 6$), when media outlets called both elections for the Democrats. We should not expect any differences in the days leading up to the election ($\tau < January\ 5$), when media outlets reported the two races as too close to call.

3 Data

Our sample period spans from 1994 to 2020. It does not extend back before 1994 for two reasons. First, we obtain firms' historical headquarters locations using 10-K filings from EDGAR, and

[23] The election was called for Raphael Warnock around 1 am, early in the morning of January 6, and for Jon Ossoff around 2 pm, later in the afternoon.

EDGAR's coverage of firms before 1994 is poor. Second, we do not observe many periods of narrow Senate majorities before 1994, as the Democratic party tended to dominate both chambers of Congress before Republicans took power in 1994. Moreover, the political wave that swept the Republicans into power in 1994 has been seen by many as an important inflection point in American politics, marking the beginning of a long decline in bipartisanship and hence the point at which congressional swing votes became increasingly important.[24]

3.1 Political data

We obtain data on U.S. Senate seat margins directly from the U.S. Senate's website.[25] We clean the data to determine which party holds majority control of the Senate, the number of Senate seats held by both parties, and the seat margin between the majority and minority parties. We present our cleaned data in Table I.[26] We flag a congressional term as being "gridlocked" if the controlling party held a majority of six or fewer seats (i.e., a maximum seat margin of 53-47). Out of the fourteen terms in our sample period, the Senate is gridlocked for seven terms: the 104th, 107th, 108th, 110th, 112th, 115th, and 116th U.S. Congresses.

We use the six-seat margin threshold to define gridlock Senate terms as it constitutes the median Senate seat balance in our sample. When the Senate is at a 53-47 seat balance, as few as three Senators can form a voting bloc to swing the Senate. During our sample period, a 53-47 seat balance occurred during the 2011-2012 and 2019-2020 congressional terms. Anecdotal evidence suggests that both periods were marked by frequent impasses in the Senate, with the contentious 2012 debate over raising the debt ceiling serving as a notable example.[27] Nevertheless, we use tighter thresholds for defining gridlocked Senates, including a four-seat majority (i.e., a 52-48 seat margin) and a two-seat majority (i.e., a 51-49 seat margin), in robustness tests.

We obtain data on presidential vote margins from the MIT Election Lab (MIT Election Data

[24] See `https://thehill.com/blogs/congress-blog/politics/449336-the-unexpected-shadow-of-1994-25-years-later`.

[25] Available at `https://www.senate.gov/history/partydiv.htm`.

[26] Note that we label the 107th U.S. Congress as "Split" because there were four different majorities in the Senate in that term. All four majorities in the 107th Congress were gridlocked, with a maximum seat margin of two seats.

[27] See `https://www.cnn.com/2012/01/26/politics/senate-debt-vote/index.html`. Other examples of Senate gridlock during these terms included four Democrats blocking a legislative measure to end oil company subsidies (see `https://www.cnn.com/2012/03/29/politics/oil-subsidies/index.html`), and the Senate blocking President Obama's judge nominees (see `https://www.bloomberg.com/news/articles/2012-01-12/obama-nominees-cool-heels-as-divided-senate-stalls-on-confirmation-votes`).

and Science Lab, 2017). We use state-level margins from the previous presidential election to categorize states into swing and partisan states, based on whether *PresVoteMargin*, the absolute vote margin between the Democratic and Republican candidates, was above or below the sample median each year. In Panel A of Table II, we present summary statistics for *PresVoteMargin*. We can roughly interpret the sample median of 13.2% as a cutoff for our partisan-state and swing-state labels. Specifically, vote margins of 13.2% or higher for one party would indicate a partisan state, while the opposite would indicate a swing state. We note that our definition of swing states based on median thresholds is generally broader than the narrower labeling of swing states by the political media as those with "toss-up" elections. The concept of "swing" in our analysis relates to the likelihood of a legislator swinging their vote rather than the potential for the general election vote swinging from one party to another. As we show in the data, the likelihood of voting against one's party is more broadly distributed across states that are relatively contested between the two parties. Nevertheless, we also examine alternative definitions of swing states based on tighter margin cutoffs in our robustness tests.

Figure I plots the time series of gridlocked Senates (in gray) and consolidated Senates (in white). In subfigure (a), we plot the average of *PresVoteMargin* from the last presidential election across states for each year in our sample.[28] We see that the average margin has steadily climbed over time, consistent with deepening political polarization, but this trend does not appear to be correlated with Senate gridlock. A formal t-test (p-value = 0.65) fails to reject the null hypothesis that *PresVoteMargin* is on average equal between periods of gridlocked and consolidated Senates, indicating that more competitive elections at the state level do not automatically lead to a tighter balance in the Senate. In subfigure (b), we plot the annual real GDP growth and the unemployment rate against periods of gridlocked and consolidated Senates, and find no clear correlation between aggregate economic conditions and Senate gridlock. Formal t-tests fail to reject the null for both real GDP growth (p-value = 0.17) and the unemployment rate (p-value = 0.89).

We use data on legislative votes to verify that swing-state senators tend to vote against their own party more frequently than partisan-state senators. Specifically, we obtain data on senator-level roll call votes from the Congressional Roll-Call Votes Database from Voteview (Lewis, Poole,

[28] Note that this measure does not reflect the presidential vote margin at the national level but rather the average level of electoral competitiveness in each state.

Rosenthal, Boche, Rudkin, and Sonnet, 2021). We construct *LegVoteDev* to measure a senator's vote relative to their party. It is the absolute difference between the senator's vote and the median vote of their party (where "yea" is coded as one and "nay" is coded as zero).

We provide binned scatterplots illustrating the relationship between *PresVoteMargin* and *LegVoteDev* in Figure II. We see in subfigure (a) that, across all bill types, there is a relationship between the two variables, indicating that senators from swing states (i.e., states with below-median presidential vote margin) tend to vote more often against their own party. In subfigures (b) and (c), we see that this relationship is stronger for budget-related bills, which is relevant for the 50-seat threshold for reconciliation used in our empirical setting. In contrast, subfigures (d) and (e) show that the relationship is weaker for regulatory bills, suggesting swing-state senators have less leverage over legislation related to regulation.

These figures also reinforce our decision to use a broader definition of swing states than that used by the political media, as we see that voting against one's own party is not limited to a handful of states. Specifically, willingness to vote against one's own party is not confined to states with extremely tight presidential vote margins (i.e., points next to the vertical axis). For example, subfigure (a) suggests that senators from states with below-median presidential vote margins have similar propensities to vote against their own party, while being clearly higher than senators from states with above-median presidential vote margins.

We present summary statistics for various state-level economic growth measures (expressed in gross growth rates) in Panel A of Table II. These measures include the natural log of GDP (*lnGDP*), private-sector employment (*lnEmp*), total private-sector wage (*lnWage*), and total establishment count (*lnEstabs*), and are obtained from the Quarterly Census of Employment and Wages (QCEW) and the Bureau of Economic Analysis (BEA) databases. We use these measures as control variables in our main analysis and as outcome variables in state-level regressions to check if the economic fundamentals of swing states and partisan states diverge during periods of gridlocked and consolidated Senates.

3.2 Government spending data

We present summary statistics on the natural logs of annual federal government spending amounts in the bottom five columns of Panel A of Table II. The first entry (*lnTotSpend*) aggregates spending

across all spending categories for each state in a given year. The remaining rows summarize spending in the four major categories of federal spending: contracts, grants, loans, and payments. Contracts represent federal government spending on acquiring goods or services from a non-federal entity. Grants represent federal funding of projects that benefit the general public and stimulate the economy. Loans represent federal government subsidization of financial lending. Payments are a form of non-reimbursable cash transfer from the federal government to an individual, a private firm, or a private institution and include direct subsidies to private-sector firms, such as agricultural subsidies specified in the Farm Bill.

We obtain federal spending data from multiple sources. From 2010 onwards, we obtain data on all categories of federal spending from USASpending.gov, a government website that reports federal awards of more than $25,000. Crucially, USASpending.gov provides state-year totals for all categories of federal government spending via its API.[29] Since the API provides reliable state-year aggregates starting only in 2010, we supplement this data using raw transaction-level data from USASpending.gov and two other government data sources: the U.S. National Archives and Records Administration (NARA) and the U.S. Federal Procurement Data System (FPDS).

For contracts, NARA data covers only the years before 1998. FPDS provides annual reports on contract spending per state from 1998 to 2007, which we append directly to our NARA data. As FPDS does not produce reports past 2007, we rely on USASpending.gov's API contract spending aggregates for 2008 and 2009. We combine these three sources of contract spending data to generate a panel of state-year contract spending amounts from 1994 to 2009 that is consistent with the contract spending we observe from 2010 onwards. For "assistance" spending (all forms of spending besides contracts), NARA provides data for 1994 through 2009. We aggregate their transaction-level data to state-year totals for each assistance category in a manner consistent with the USASpending.gov-provided amounts for 2010 onwards.

In addition to the federal government spending data described above, we also obtain data on government subsidies from the Subsidy Tracker dataset provided by Good Jobs First. These data span from 1966 to 2023, though there is sparse coverage in the years before 2002. The data provides information on the form of each subsidy (e.g., grant, loan, tax credit, etc.), the government agency

[29] The API is available at `https://api.usaspending.gov`, specifically through the "spending by geography" endpoint. It provides data from 2008 onwards, but the data from 2008 and 2009 are not as well-populated as later years.

providing the subsidy, the identity of the subsidy recipient (i.e., firm name), and the subsidy amount. Each subsidy listed in the data is targeted at a specific recipient organization, and we match each subsidy to a Compustat firm using the recipient identity information in Subsidy Tracker.[30] Within this firm-matched subsidy dataset, we focus on the four largest subsidy categories in our subsidy analyses: federal tax credits/rebates, property tax abatements, non-federal grants, and federal grants. These categories comprise over two-thirds of the matched subsidies in the data (and over 70% of all subsidies).

We scale subsidy amounts by annual sales at the firm level and present the summary statistics for the scaled variables in Panel B of Table II. The small means for the subsidy variables indicate that most firms never receive a directly targeted subsidy from the government. Tax credits constitute the most common type of firm-directed subsidy, but are also received by only a small minority of the firms in our sample.

3.3 Firm-level data

We obtain annual firm-level data from the Compustat database and present the summary statistics of key firm-level outcomes in the remaining rows of Panel B from Table II. We broadly categorize firm-level outcomes into measures related to valuation (*TobinsQ*), operating performance (*SaleGr* and *Profitability*), investment decisions (*CapEx*, *SG&A*, and *R&D*), leverage (*Leverage*), and effective tax rates (*FedTaxETR*, *StateTaxETR*, and *ForeignTaxETR*). We use Tobin's Q as our main valuation measure following the valuation literature (e.g., Morck, Shleifer, and Vishny (1988)). All investment measures are scaled by lagged assets, and effective tax rates are scaled by adjusted pre-tax income following Dyreng, Hanlon, and Maydew (2008). Detailed definitions for all variables can be found in Appendix A.

We obtain daily returns data from the CRSP database to conduct our event analysis. To estimate abnormal returns, we obtain data on daily CAPM, SMB, and HML factor returns from Kenneth French's website. We estimate factor betas using an estimation window from 220 trading days before the election to 11 trading days before the election. We then examine returns in our event window, defined as 10 trading days before the election to 10 trading days after the election. We

[30] We use fuzzy string matching based on the "term frequency-inverse document frequency" (TF-IDF) approach to perform this matching.

define abnormal returns as the difference between realized returns and expected returns predicted by the CAPM and Fama-French 3-factor (FF3) models, respectively. We define cumulative (abnormal) returns for date t as the sum of (abnormal) returns over the $[-10, t]$ trading-day interval (i.e., from the beginning of the event window to date t).

We present summary statistics on daily returns and cumulative returns over the $[-10, 10]$ event window in Panel C of Table II. The first three rows show statistics for the daily raw returns (*RawRet*), CAPM-adjusted abnormal returns (*AR_CAPM*), and FF3-adjusted abnormal returns (*AR_FF3*). The remaining rows show the statistics for cumulative returns (*CUMRET*), cumulative abnormal returns based on the CAPM model (*CAR_CAPM*), and cumulative abnormal returns based on the FF3 model (*CAR_FF3*).

4 Main Findings

4.1 Effect on federal transfers

We first look to verify whether swing states' relative bargaining power over federal resource redistribution is elevated during gridlocked Senates. To this end, we examine state-level federal financial assistance measures as the dependent variables and estimate our benchmark specification (i.e., Eq. 1) at the state level. We present our findings in Table III, which shows a consistently positive effect on federal assistance as evidenced by the positive coefficient estimates on $Swing \times LockedSenate$. In column 1, we show that total assistance from the federal government is approximately 4% higher in swing states compared to partisan states during gridlocked Senates relative to consolidated Senates. For the median state in our sample that receives \$53.6 billion per year in total federal assistance, this translates to approximately \$2.17 billion in additional assistance for swing states during gridlocked Senates. This represents 1.14% of state-level real GDP for the median state in our sample with a GDP of \$190 billion.

Examining individual categories of assistance separately, we observe that the positive effect is most pronounced, both in terms of statistical significance and economic magnitude, for loans (column 4) and direct payments (column 5). Note that both these categories of assistance include subsidies directed at private-sector firms. In particular, direct payments encompass financial assistance provided directly to individuals, private firms, and other private institutions to subsidize or

encourage a particular activity. The positive effect on contracts (column 2) is insignificant, which may be explained by the rigid rules around the awarding of contracts.[31] The insignificant estimate in column 3 indicates that grants typically used to fund public projects are also relatively unaffected by shifts in regional political influence.

Next, we investigate whether the effect on federal financial assistance at the state level translates to increased subsidies granted directly to firms in our sample. We present the results of our benchmark analysis using various firm-level subsidy measures (scaled by sales) as the dependent variable in Table IV. In Panel A, we report the estimates from the unconditional sample, and in Panel B, we report estimates from the sample of firms that have received a directed subsidy (of a particular type) at least once in our sample. For example, the estimate in column 1 of Panel B is based on the sample of firms that received a tax credit subsidy at least once during our sample period.

Focusing first on the unrestricted sample, we show in column 1 of Panel A that swing-state firms receive a relative increase in corporate tax credit subsidies during gridlocked Senates relative to consolidated Senates. The point estimate is small, representing a 0.03 percentage-point increase in tax credit subsidies scaled by sales, mainly stemming from the fact that most firms never receive any directed subsidies, as evidenced by the small sample mean for *SubsidyTaxCredit* in Panel B of Table II. In Panel B of Table IV, we show in column 1 that the effect on tax credit subsidies scaled by sales is larger, at 0.37 percentage points, when we restrict our sample to firms that have received a tax credit subsidy at least once in our sample. Together, these results indicate that firms benefit from increased tax credits when the political bargaining power of their home states is elevated.

Columns 2-4 in both Panels A and B show that there is no evidence of an effect on property tax rebates, non-federal grants, or federal grants. The estimates in columns 2 and 3 are precisely estimated zeros, consistent with the fact that property taxes are not materially important to most firms and that federal politicians do not directly control non-federal grants. The estimates in column 4 in both panels are positive but insignificant, potentially due to the smaller number of firms that receive direct federal grants. Indeed, the observation counts in columns 2-4 of Panel B are significantly smaller than those in column 1, indicating that tax credits constitute the most

[31] Government contracts are strictly regulated under the Competition in Contracting Act (CICA), and contract approvals are governed by a rigid approval process that often requires independent cost estimates.

frequent form of firm-directed federal subsidy.

Overall, our findings appear to verify that swing states elevate their bargaining power over economic redistribution during gridlocked Senates as measured by loans, direct payments, and corporate tax credit subsidies. We note that such measures do not exhaustively capture all the channels through which regional political bargaining power can benefit local firms. For example, while our analysis presented in Table IV focuses on subsidies directed at individual firms, firms may also benefit from their home region's elevated political bargaining power through more common industry-directed tax credits. We examine this potential channel, as well as trade and regulatory policies, in greater detail in Section 5.

4.2 Effect on firm valuation and investment

We further investigate how firms benefit from increases in the political bargaining power of their home regions, focusing, as discussed in Section 2.2, on forward-looking firm-level valuation and investment measures that are likely to respond quickly to changes in regional political bargaining power. Given our findings on federal transfers and firm-level tax subsidies, we should expect such forward-looking measures to respond positively when the political bargaining power of swing states is elevated during gridlocked Senates.

We present the results of our analysis in Table V. The first four columns present coefficient estimates in the absence of control variables. Across all four columns, we observe a positive coefficient on $Swing \times LockedSenate$, indicating that predicted increases in regional political influence result in higher valuation and investment ratios. Specifically, the estimate in column 1 indicates that swing-state firms experience a relative increase in Tobin's Q of 0.2 (5.5% of the sample mean) during gridlocked Senates relative to consolidated Senates. This suggests that shifts in the balance of regional political bargaining power are quickly incorporated into market prices as investors adjust their expectations of future political transfers. We later corroborate this with an event study involving a sharp shift in the balance of the Senate.

In the next three columns, the positive coefficient estimates on $Swing \times LockedSenate$ indicate that greater regional political influence leads firms to increase their capital expenditures, SG&A expenditures, and R&D expenditures (all scaled by lagged assets). While the effect on capital expenditures is relatively marginal, we find a 5.31 percentage point increase in $SG\&A$ (7.4% of

the sample mean) in column 3 and a 2.19 percentage point increase in *R&D* (11% of the sample mean) in column 4. A potential explanation for the stronger effect on intangible assets may stem from the political uncertainty stemming from possible future policy reversals creating a stronger drag on investments in physical capital relative to intangible capital.[32] In particular, Ahn (2019) argues that tangible investments respond more negatively to economic uncertainty as they tend to be lump-sum and intermittent.[33] Moreover, physical capital typically takes longer to install than intangible capital, and firms may find it imprudent to commit to long-term tangible capital projects when their region's political power is elevated, as this power may dissipate as soon as the next national election.

We note that the positive effect on valuations and investment that we document contrasts with Cohen et al. (2011), which finds that spending directed by powerful congressional committee chairs toward their home regions results in lower private-sector investment. We posit that this difference stems from the fact that the strategic leverage of swing-state senators tends to be transitory, as it depends on the changing balance of the Senate, while the formal authority of long-serving committee chairs tends to persist over time. Essentially, electorally challenged swing-state senators may find it more expedient to deploy their short-lived political capital on quickly implemented tax subsidy programs rather than capital spending projects that generally take years to deploy.[34] This may explain why our findings appear to be driven by taxes, as we later discuss in more detail in Section 5, in contrast to the earmark spending channel that drives the crowding-out findings in Cohen et al. (2011).

In the last four columns, we repeat our analysis while including additional control variables. Following the advice of Angrist and Pischke (2009) and Roberts and Whited (2013), we include only control variables that are likely to be fixed at the time the regressor of interest was determined (i.e., "good controls") while omitting time-varying economic measures that may also be affected

[32] For example, tax incentive programs are typically not indefinite and have "sunset" provisions commonly built in (Gale and Orszag, 2003).

[33] This stems from firms' incentives to maintain intangible investments even during uncertain times due to new investments in intangible capital being complementary to the existing stock of intangible capital (Klette, 1996; Doraszelski and Jaumandreu, 2013).

[34] Major public works projects generally require a long review process involving federal agencies, local governments, and private builders. For example, some estimate that it will take a decade before President Biden's $1.2 billion infrastructure bill delivers tangible results for citizens (see `https://www.washingtonpost.com/us-policy/2021/08/10/infrastructure-senate-spending-nepa/`). Such lengthy commitments constitute a luxury that cannot easily be afforded by embattled swing-state senators looking ahead to their next election challenge.

by changes in regional political leverage (i.e., "bad controls"). Our included controls consist of *Democrat* (an indicator for both senators of the firm's state being Democrats), *Republican* (an indicator for both senators of the firm's state being Republicans), *MajParty* (an indicator for both senators of the firm's state being in the Senate majority party), *MinParty* (an indicator for both senators of the firm's state being in the Senate minority party). To control for the effects of powerful Senate committee chairs as documented by Cohen et al. (2011), we further include *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*, which denote dummy variables indicating whether a firm is headquartered in a state with a senator on one of the top one, three, and five most influential Senate committees, respectively.[35]

We note that the coefficients on *Swing* in Table V are consistently negative and, at times, statistically significant. This suggests that swing-state firms face a *baseline* political disadvantage when the Senate is *not* in gridlock. This may be attributed to the fact that elections in swing states tend to be more competitive and thus more likely to produce vulnerable incumbent senators. In particular, electorally vulnerable incumbent senators likely possess limited political capital when bargaining with their colleagues, as uncertainty over their ability to reciprocate political favors in the future may reduce their colleagues' willingness to support their legislative proposals. Moreover, the unpredictability of competitive elections results in uncertainty over political representation, and prior research has documented that political uncertainty has a dampening effect on valuations (Pástor and Veronesi, 2013; Çolak, Durnev, and Qian, 2017) and investments (Julio and Yook, 2012; Gulen and Ion, 2016; Jens, 2017).

To verify that swing states' senators are more electorally vulnerable, we estimate Eq. 1 with senator election margins as the dependent variable and present the findings in Table BI of Appendix B. In the first two columns, we observe that Senate election margins (as measured by *SenWinMargin*, the margin of the election winner, and by *SenWinPct*, the percentage of votes received by the election winner, respectively) are indeed lower in swing states. We repeat the same exercise using a continuous measure of presidential election closeness (*PresVoteMargin*), and see, in the last two columns of Table BI, that Senate elections margins are wider in states with wider margins in the

[35] Cohen et al. (2011) find that chairmanships of only the most influential committees have an effect on home-state firm investment. Accordingly, we identify the following committees as the five most influential committees in order of decreasing influence: (1) Finance; (2) Veterans' Affairs; (3) Appropriations; (4) Rules and Administration; and, (5) Armed Services. Note that Cohen et al. (2011), in turn, source the above list of influential committees from Edwards and Stewart III (2006).

previous presidential election. These tests confirm that swing states' senators are, indeed, more electorally vulnerable.

We acknowledge that the negative coefficient estimates for *Swing* in Table BI could be driven by other cross-sectional differences between regions, and we emphasize that our focus is on identifying changes in regional political bargaining power as captured by the coefficient on the *Swing* × *LockedSenate* interaction term rather than explaining the cross-sectional differences between swing states and partisan states. Importantly, we see from the interaction term coefficient estimates in Table BI that swing-state elections do not become less competitive during gridlocked Senate periods. This indicates that the positive effects on valuations and investments we document in our benchmark tests are unlikely to be driven by decreasing political uncertainty in swing states during gridlocked Senates.

4.3 Effect on operating performance

Next, we examine whether the increases in valuations and investments we detect in our benchmark analysis are accompanied by improvements in operating performance as measured by sales growth (*SaleGr*), return on assets (*ROA*), and profitability (*Profitability*). Given that market valuations are more forward-looking than revenues and profits, it is unclear whether we should expect to find an effect on operating performance. For example, we find that swing-state firms experience increases in tax subsidies during gridlocked Senates, but tax subsidies do not immediately translate to higher pre-tax earnings and sales. Tax incentive programs may eventually result in higher future revenues and profits by stimulating expansions, but such long-run effects are difficult to detect in our setting given the frequent switching between gridlocked and consolidated Senates. On the other hand, direct revenue windfalls resulting from increased regional political leverage (e.g., government contracts) should translate relatively quickly into firms' revenues and earnings.

We estimate our benchmark specification using operating performance measures as dependent variables and present the findings in the first three columns of Table VI. While the coefficient estimate on the interaction term in column 1 for sales growth (total annual sales scaled by lagged annual sales) is positive, it lacks statistical significance. This indicates that our benchmark findings are unlikely to be driven by direct revenue windfalls from federal government transfers. This is consistent with the fact that we did not previously detect a significant increase in government con-

tracts, which constitute the most immediate channel by which federal spending impacts corporate revenues.

The estimates in columns 2 and 3 show a negative effect on *ROA* (ordinary income before depreciation scaled by assets) and *Profitability* (ordinary income before depreciation scaled by sales), with only the column 2 estimate being statistically significant. These negative estimates may at first appear counterintuitive, but they are consistent with the positive effect that we document for investment. First, the decrease in operating cash flows may mechanically result from the increase in SG&A expenditures (which are subtracted from earnings) we previously documented. Second, firms may take on more marginal projects that lower the average return on assets (while still being NPV positive) when they scale up their investments under decreasing returns to scale.[36] Lastly, investments that produce delayed future cash flows can have a mechanical negative effect on *ROA* in the short run as the denominator (i.e., assets) responds more quickly than the numerator (i.e., cash flows). By the time that the investment's cash flows are eventually realized, the balance of the Senate may have already shifted.

To further explore how regional political influence affects firms' cash flows, we examine financial leverage as our outcome variable of interest. According to the trade-off theory of capital structure, higher cash flows should induce firms to increase their financial leverage to take advantage of higher interest tax shields and lower financial distress costs. However, column 4 of Table VI indicates that swing-state firms decrease *Leverage* (long-term debt scaled by total assets) relative to partisan-state firms during gridlocked Senates relative to consolidated Senates, though the negative estimate is only marginally significant and small in economic magnitude. Rather, it is consistent with government tax subsidies reducing the value of interest tax shields by lowering effective tax rates.[37] We later examine effective tax rates in more detail in Section 5.

[36] For example, consider a government tax subsidy that turns an otherwise NPV-negative investment project into an NPV-positive one. Since the project is marginal for the firm, it should have a lower pre-tax return on investment relative to the firm's other projects. Thus, the investment (under the tax subsidy) should lower the firm's average return on investment while also creating value for investors.

[37] This also suggests that the negative effect we document on earnings may be driven by firms increasing their expenditures to accumulate tax credits in response to a more favorable political environment.

4.4 Dynamics

We examine the dynamics underlying our benchmark results by making year-by-year comparisons of swing-state and partisan-state firms in the years leading up to and following a shift in the balance of the Senate. As discussed in Section 2.2, we use consolidated Senate terms as the benchmark period in our specification, but these terms do not constitute a "pre-treatment" period during which we should expect parallel trends between swing states and partisan states. In particular, swing-state firms should be negatively affected by their relative *loss* of regional political influence during consolidated Senate terms. Moreover, this effect on investment may vary over time *within* a given Senate term. Nevertheless, comparing the dynamics of swing-state and partisan-state firms allows us to check whether our benchmark findings are driven by long-run economic trend differences between swing states and partisan states that happen to be centered around shifts in Senate balance.

The challenge in analyzing investment dynamics in our setting is that gridlocked and consolidated Senates alternate very frequently—in many cases, every two years. Such frequent switching prevents us from examining dynamics beyond a year before and after an election. However, we see from Table I that both the 2000 and 2016 elections were preceded by four years of a consolidated Senate and followed by four years of a gridlocked Senate. Therefore, we focus on these two elections and estimate a dynamic version of our benchmark specification in which we interact *Swing* with dummy variables indicating the year relative to the two reference elections. For example, the dummy for $Year = -1$ would take on a value of one for both 1999 and 2015. We confine the sample to a [-3 year, +3 year] window around the reference elections to avoid contamination from the previous elections (i.e., 1996 and 2012) and the following elections (i.e., 2004 and 2020).

Figure III illustrates the coefficients on the dynamic interaction terms ($Swing \times (Year = N)$) in our dynamic specification using the firm-level outcomes that we examined in Sections 4.3 and 4.2. Following standard conventions, we define the pre-election year (i.e., $t = -1$) as the omitted period that serves as the comparison point for all other years. The dynamic plots show that our results are unlikely to be driven by long-run trends that happen to be centered around changes in Senate balance. In the first four subfigures, the change from a consolidated Senate to a gridlocked Senate (i.e., year 0) marks the inflection point where valuations rise (illustrated in subfigure (a)) and

investment increases (illustrated in subfigures (b), (c), and (d)).

We note that the sharp uptick at year zero in these subfigures is consistent with the notion that forward-looking market prices and investment decisions are quick to incorporate shifts in regional political bargaining power. Moreover, this shift is likely to be partially anticipated, as the balance of the Senate is predictable in the months leading up to election day. For example, advanced polling leading up to the 2016 election showed that majority control of the Senate was closely contested between the Democrat and Republican parties.[38] Therefore, the sharp election-year inflection we observe for valuations and investments may partially reflect anticipated shifts in the political bargaining power between swing states and partisan states. In the following section, we exploit a rare case of a surprise shift in the balance of the Senate to examine the predicted effect on daily stock returns.

The last four subplots of Figure III further show no evidence of a pre-election trend in sales growth, return on assets, profitability, and leverage. This should further alleviate concern that our findings are driven by diverging trends in economic fundamentals across regions. Subfigure (f) shows a sharp drop in *ROA* starting in year 0, but as previously discussed, this can be attributed to the sharp concurrent increase in investment as the Senate becomes gridlocked.

4.5 Georgia runoff election event study

To sharpen our identification of the effect of regional political bargaining power on firm value, we conduct an event study based on the 2021 Georgia runoff elections as described in Section 2.3. We present the results of estimating Eq. 2 in Table VII and provide visual illustrations of π_τ coefficient estimates in Figure IV. The first three table columns and first three subfigures show that raw daily returns and daily abnormal returns are not systematically different between swing-state firms and partisan-state firms in the 5 days leading up to the January 5 election. The election on January 5 coincides with a sharp divergence in returns, with swing-state firms outperforming partisan-state firms by a statistically significant 73 basis points on January 5 (the Democratic candidates established their lead on the ballot count) and by a statistically significant 87 basis points on January 6 (when the election results were finalized). Moreover, the coefficient estimates are identical regardless of whether we examine raw or abnormal returns.

[38] See `https://projects.fivethirtyeight.com/2016-election-forecast/senate/`

In the last three columns of Table VII and the last three subfigures of Figure IV, we see a similar pattern for cumulative raw returns and CARs, with swing-state firms and partisan-state firms following parallel paths before the election date. In this case, the divergence in returns is visually evident on January 5, but it is not until January 6 (when results were finalized), that the difference in cumulative returns becomes statistically significant. Specifically, the cumulative returns and abnormal returns of swing-state firms outperform those of partisan-state firms by 1.3 percentage points. The magnitude of this estimate is comparable to estimates from notable prior event studies examining the value of political connections, which find cumulative abnormal returns ranging from 0.59 percentage points to 1.43 percentage points.[39] Again, the coefficient estimates are the same regardless of whether we examine cumulative raw returns or cumulative abnormal returns.

Overall, our findings indicate that *expectations* about the benefits of increased regional political influence were quickly reflected in stock prices. As we described in Section 2.3, the election result swung the Senate from a potential 52-48 Republican majority to an even 50-50 split. Moreover, the fact that the divergence was most pronounced on January 6 is consistent with the election results being officially confirmed on that day. In the days following the finalization of the election results, we observe some reversal in the gains that swing-state firms experienced relative to partisan-state firms. However, we observe that swing-state firms always maintained higher cumulative returns and cumulative abnormal returns relative to partisan-state firms in the days following January 6.

We note that January 6 also marked the U.S. Capitol capital attack, when supporters of President Trump stormed the Capitol building in protest of the results of the presidential election. While the overall market did not appear to be greatly affected by the attack,[40] states where the presidential vote was close may have faced elevated risks of civil unrest over disputed election results. However, such concerns should have had a negative effect on the firms located in those states, in contrast to the positive effect on returns we detect. Indeed, the return reversal observed on January 7 and 8 may stem from such concerns, and the post-January 6 estimates may potentially understate

[39] Specifically, Roberts (1990) estimates abnormal returns of -1.33% for firms affected by the sudden death of Senator Scoop Jackson, Fisman (2001) estimates abnormal returns of -0.59% for Indonesian firms affected by the deteriorating health of President Suharto, Faccio (2006) estimates the abnormal returns of 1.43% for firms that have large shareholders and officers that are connected to the government, and Faccio and Parsley (2009) finds the abnormal returns of -1.68% for firms located in the hometown of received deceased politicians.

[40] See `https://www.washingtonpost.com/business/2021/01/14/stocks-capitol-riot/`.

the positive effect of expected political bargaining power on market valuations.

4.6 Effect on overall state economy

We investigate whether the relative increase in political bargaining power swing states receive during gridlocked Senates has a measurable impact on aggregate economic activity. To this end, we estimate our benchmark specification at the state level using the natural log of state-level GDP (*lnGDP*), private-sector employment (*lnEmp*), total private-sector wages (*lnWages*), and private-sector establishment counts (*lnEstabs*) as the dependent variable.[41] We present the findings of this analysis in Panel A of Table VIII. The estimates indicate increases in GDP (1.64%), employment (0.93%), and total wages (1.54%) for swing states relative to partisan states during gridlocked Senates relative to consolidated Senates. However, the estimates are small in economic magnitude and weak in statistical significance. Nevertheless, they suggest that politicians are able to use their political leverage to stimulate the local economies of their home regions.

We further note that the estimates here are smaller than those in our analysis of federal transfers presented in Table III. However, one cannot directly infer estimates of fiscal multipliers by comparing the estimates from these analyses, as politicians can wield their political leverage via other channels to benefit their home regions. Some channels, such as pushing for more regulatory policies that favor firms in one's home district, may be difficult to measure directly. Nevertheless, we explore the role of such alternative policy channels in Section 5.

We also use aggregate state-level data to investigate whether the economic stimulus effect of regional political leverage is broadly distributed across firms of different sizes. There is an extensive literature documenting how firms benefit from political connections,[42] and our firm-level analysis focuses on publicly-traded companies that are relatively likely to have political connections through their large size and greater resources available for political donations and lobbying (Kerr, Lincoln, and Mishra, 2014). Therefore, the question arises whether our findings are driven by political rents exclusively directed at large, connected crony firms.

[41] We focus only on private-sector measures to ensure our estimates do not capture any mechanical effects of government activities.

[42] Prior research has shown firms to benefit from political connections through personal relationships (Amore and Bennedsen, 2013; Cohen and Malloy, 2014), board memberships (Goldman et al., 2013), lobbying (Borisov, Goldman, and Gupta, 2016) and campaign donations (Claessens, Feijen, and Laeven, 2008; Akey, 2015; Brogaard, Denes, and Duchin, 2021)

To answer this question, we examine the effect of political rents on firm employment across a broader range of firm sizes using data from the Quarterly Workforce Indicator (QWI) database provided by the U.S. Census Bureau. The QWI data provides state-level private sector employment for firms with 0 to 19 employees, 20 to 49 employees, 50 to 249 employees, 250 to 499 employees, and 500 and more employees.[43] We then compute log employment at the state-year level for each firm-size category and estimate Eq. 1 using these measures as the dependent variable. If political rents are mostly directed toward large politically-connected companies, then we should expect any positive employment effect to be more pronounced for larger firms.

We present the results of this analysis in Panel B of Table VIII, which show that the positive employment effect is not confined to the largest firms. This suggests that political rents are broadly distributed across firms rather than being captured by a few large connected firms, indicating that the rents are more likely to be motivated by politicians' electoral accountability to their constituents than by their relationship with connected cronies. If anything, the increase in employment is larger for smaller firms, with the estimates in the first two columns being larger and statistically more significant than the estimates in the last two columns. This may be due to larger firms being more likely to have pre-existing connections to powerful politicians established through lobbying or campaign contributions and therefore not being as dependent on their *local* political representatives.

5 Potential Mechanisms

In this section, we investigate various mechanisms that may explain our benchmark findings. Specifically, we examine whether increased regional political bargaining power may benefit firms through industry-level tax credits, local demand spillovers, changes in federal regulations, or trade policy shifts.

5.1 Industry tax incentives

Our findings on firm-specific subsidies presented in Panel B of Table IV indicate that firms in swing states benefit from favorable tax policies when the Senate is near gridlock. However, tax

[43] See `https://www.census.gov/data/developers/data-sets/qwi.html#ownership` for more details on the QWI data.

subsidies are rarely directed at individual firms and are much more commonly targeted at entire industries. Therefore, our findings on firm-targeted subsidies may understate the importance of the tax savings channel in explaining our findings on valuation and investment if politicians use their political leverage to push for tax credit programs that favor the regional industry clusters.[44]

To measure the effect on corporate taxes more comprehensively, we examine how regional political bargaining power affects firms' effective tax rates. To this end, we estimate Eq. 1 with various firm-level tax measures as the dependent variable, and present the results of our analysis in Table IX. If senators use their political leverage to reduce the tax burdens of firms in their home states, then we should expect δ to be negative. However, we should expect this only to be the case when examining federal tax rates and not state or foreign tax rates, as our focus is on political bargaining power at the national level.

We see from column (1) that swing-state firms indeed experience a relative decrease in their effective federal tax rate (defined as total current federal taxes scaled by pre-tax income) of 59 basis points (6.1% relative to the sample mean) during gridlocked Senates relative to consolidated Senates. For the median firm in our sample with $1.072 billion in annual adjusted pre-tax income, this translates to annual tax savings of approximately $6.3 million per year. In the next two columns, we see that there is no effect on the effective state tax rate (current state taxes scaled by pre-tax income) and the effective foreign tax rate (current foreign taxes scaled by pre-tax income). These placebo results serve to alleviate concerns that our federal tax rate result is driven by confounding factors, such as diverging trends in tax avoidance strategies between swing states and partisan states.

Next, we explore the idea that swing-state senators push for industry-specific tax policies that disproportionately benefit their home states. To this end, we design an indirect test in which we include *Industry* $\times$ *Year* fixed effects in our benchmark specification. If industry-targeted tax policies are behind the lower effective federal tax rates we document, then our benchmark estimates should be absorbed by the new fixed effects. We present the results in the last three columns of Table IX. Indeed, column 4 shows that the estimated effect on federal effective tax rates is no longer significant, consistent with the estimate in column (1) being driven by changes to industry-level

[44] For example, Louisiana Senator Mary Landrieu used her leverage as a swing senator to defend tax breaks for the oil and gas industry in 2011. See `https://www.nytimes.com/2011/05/18/us/politics/18congress.html`.

tax policy.

Lastly, we examine the dynamics of how a shift in Senate balance affects the relative effective tax rates of swing-state firms and partisan-state firms. We use the same specification as in Section III (while dropping *Industry* × *Year* fixed effects) to examine changes in *FedTaxETR*, *StateTaxETR*, and *ForeignTaxETR* around the 2000 and 2016 elections, and present the estimates of our dynamic interaction terms in Figure V. Subfigure (a) shows the decrease in federal taxes begins to take effect two years after the election, the same length as a congressional session during which new legislation is passed. Importantly, we see no evidence of a decreasing trend in tax rates during the pre-election period. Moreover, subfigures (b) and (c) confirm that state taxes and foreign taxes do not decrease following the reference election.

5.2 Tradable vs. non-tradable sectors

We perform heterogeneity tests on our benchmark firm-level findings on valuation and investment by comparing industries in the tradable and non-tradable sectors. As non-tradable-sector firms are, by definition, more exposed to local demand factors than firms in tradable sectors, this analysis allows us to check whether our benchmark estimates may be subject to bias stemming from diverging regional economic trends across swing and partisan states. Specifically, we should expect our δ estimates in Eq. 1 to be more pronounced in non-tradable-sector firms if our estimates are driven by diverging regional economic fundamentals, as such firms have greater exposure to local demand shocks.

Moreover, given that we document a positive effect on federal transfers in Table III, a more pronounced δ estimate in the sample of non-tradable-sector firms may also indicate local demand spillovers arising from a fiscal multiplier effect. For example, restaurants and retail shops in the non-tradable sector would benefit more from government stimulus that drives up local spending to a greater extent than would manufacturing plants in the tradable sector. If, conversely, we do not find our estimates to be more pronounced in the non-tradable sector, that would alleviate concerns about our benchmark findings being driven by diverging economic fundamentals, as well as rule out local demand spillovers as the driving mechanism behind our findings.

Classifying firms in our sample into belonging to tradable and non-tradable sectors following Mian and Sufi (2014), we estimate our benchmark regressions including additional interaction terms

with *Tradable*, a dummy variable indicating that a firm belongs to a tradable sector, and present the results in Table X.[45] We see that across all columns, the positive effect of regional political influence is more pronounced for firms in the tradable sector, although the difference is statistically significant only for *R&D* in column (4). The fact that the effect is not stronger for firms in the non-tradable sector alleviates concerns of diverging regional fundamentals driving our results while also suggesting that local demand spillovers do not constitute the driving mechanism behind our findings.

While the positive triple interaction estimates are not consistently significant statistically, they suggest that firms in tradable sectors may benefit more from elevated regional political bargaining power than firms in non-tradable sectors. A possible explanation may be that tradable-sector firms tend to cluster together geographically (Delgado, Porter, and Stern, 2016). While non-tradable goods and services (e.g., retail and restaurants) can be produced anywhere, the production of tradable goods and services often requires the input of specialized knowledge that benefits from geographic agglomeration effects (e.g., the Research Triangle for pharmaceuticals and Silicon Valley for information technology) or a bountiful regional supply of specific natural resources (e.g., Maine timber or West Virginia coal). Such geographic clustering of industries creates powerful regional political interests that local political representatives have a strong incentive to serve. This would explain why tradable-sector firms are more likely to benefit when the political leverage of their region is elevated.

5.3 Trade policy

Given that we find stronger effects for firms in the tradable sector, we investigate the role of trade policy in explaining our benchmark findings. Anecdotal evidence suggests that politicians leverage their political bargaining power to push for trade policies favorable to their home regions. For example, Senator Joe Donnelly from Indiana sought trade protections for Indiana firms when courted by President Trump to support his Supreme Court nominee in 2017[46] and Senator Zell Miller from Georgia used his bipartisan credentials to bargain for tariff reductions on ceiling fan

[45] Note that our sample is smaller for this analysis because some firms do not belong to either a tradable or non-tradable sector based on Mian and Sufi (2014).

[46] See `https://www.indystar.com/story/news/politics/2017/09/13/donnelly-emerges-trump-dinner-tax-reform-claim-support-his-own-legislation/660509001/`.

imports on behalf of Georgia-based Home Depot in 2005.[47] Motivated by these examples, we look for evidence that swing-state senators use their political leverage to protect firms in their home states that are exposed to foreign import competition

Using U.S. import data from Professor Peter Schott's website,[48] we follow Bertrand (2004) to construct an import penetration index at the industry level measured as the total industry-level import as a proportion of imports plus domestic production, and define *HighIPR* as an indicator for whether a particular industry is above or below the sample median for import penetration each year. We then include *HighIPR* as an interaction term with the other variables of interest in Eq. 1. Since import data is available only for the manufacturing sector, we limit our analysis to manufacturing firms in this analysis.

We present the results of our analysis in Table XI. Overall, we find evidence for favorable trade policy as a potential channel through which firms benefit from the elevated political bargaining power of their home regions. Across a variety of firm outcomes, the positive coefficient on the triple interaction term, $Swing \times HighIPR \times LockedSenate$, indicates that our benchmark effects are more pronounced for industries with greater exposure to import competition. though only the estimates in columns (2) and (3) are statistically significant. These findings suggest that manufacturing firms benefit from increased trade protection when the political bargaining power of their home region is elevated.

5.4 Regulation

Lastly, we look for evidence that swing-state senators use their political leverage to pursue federal regulation changes that are favorable to their home state firms. While changes in federal regulation can significantly affect firms, the nature of our empirical setting makes it an unlikely channel to explain our benchmark findings. First, regulatory policies are often unrelated to taxes and spending and, hence, are not eligible for budget reconciliation in passing the Senate. This means they are likely less sensitive to the 51-vote threshold that forms the basis of our empirical design. Second, as shown in Figure II and discussed in Section 3, swing-state senators appear less willing to vote against their own party on regulation-related bills, suggesting they wield less bargaining power

[47] See `https://www.tampabay.com/archive/2004/10/14/how-new-tax-bill-gave-business-more-and-more/`

[48] See `https://faculty.som.yale.edu/peterschott/international-trade-data/` for data access and Schott (2008) for a description of the data.

on regulatory policies. Lastly, important details about regulatory policy are often decided by bureaucrats at federal agencies who are shielded, to a certain degree, from political pressures.

We obtain industry-level regulatory data from the QuantGov RegData database (McLaughlin and Sherouse, 2021), an open-source data project that uses machine learning and natural language processing to count individual regulatory restrictions in the U.S. Code of Federal Regulations. We define *Regulated* as an indicator for whether a particular industry is above or below the sample median for industry-level federal regulatory restrictions, and include this as an interaction term with the other variables of interest in Eq. 1. We present the results of our analysis in Table XII. We see that the coefficient on the triple interaction term, $Swing \times Regulated \times LockedSenate$, is insignificant across all specifications. This suggests that swing-state firms do not benefit from favorable regulatory policies during gridlocked Senates, consistent with the reasons we have outlined above.

6 Robustness Tests

6.1 Alternative variable definitions

We conduct several tests to verify the robustness of our benchmark firm-level findings. First, we vary the threshold at which we define Senate majorities to be gridlocked. In our baseline analysis, we define gridlocked Senates (i.e., $LockedSenate = 1$) as terms when the seat margin between the majority and minority parties is no greater than six seats (i.e., a 53-47 majority), the median Senate balance in our sample. As discussed in Section 3, as few as three senators can form a voting bloc to hold up legislation when the Senate balance is at a 53-47 margin. Nevertheless, we examine our results using tighter thresholds, including a two-seat margin (i.e., a 51-49 majority where one senator can hold up legislation) and a four-seat margin (i.e., a 52-48 majority where two senators can hold up legislation). In Table BII of Appendix B, we present the results of using *LockedSenate2* (a dummy variable indicating a maximum seat margin of two seats) and *LockedSenate4* (a dummy variable indicating a maximum seat margin of four seats) as our indicator of a gridlocked Senate in Panels A and B, respectively. We see that our benchmark firm-level findings remain qualitatively unchanged when we use these alternative thresholds.

We also examine alternative measures of state-level partisanship. As we discussed in Section 3,

our benchmark definition of *Swing* is based on whether the presidential vote margin in the prior election was above or below the sample median. Here, we explore alternative thresholds based on fixed vote margin thresholds, including vote margins of 6 percentage points (i.e., a 53-47% margin), 8 percentage points (i.e., a 54-46% margin), and 10 percentage points (i.e., a 55-45% margin). We present the results of our benchmark firm-level tests using these alternative thresholds for defining *Swing* in the first three Panels of Table BIII. We see that our firm-level findings are qualitatively unchanged.

In Panel D of Table BIII, we present the results of our benchmark analysis using an alternative definition of swing states based on the vote margin against the state's incumbent Senators. Specifically, we define *PartisanAgainst* to take on a value of one if the average vote margin against the state's incumbent Senators is above or below the sample median in a given year. This measure better captures cases of senators who face intense partisan opposition in their own state, such as Senator Joe Manchin of West Virginia. Again, we see that our results are qualitatively similar to our benchmark findings.

Lastly, we estimate our benchmark tests where *Swing* does not vary over time to address concerns that changes in the partisan balance within states are systematically correlated with changes in the overall partisan balance of the Senate in a way that relates to changing economic trends. In Table BIV, we present our results using *SwingMode*, a time-invariant measure for the *Swing* status for each state, as the interaction variable with *LockedSenate*.[49] We see that our findings are again largely unchanged under this time-invariant measure of state-level electoral competitiveness.

6.2 Gridlock near 60-40 filibuster-proof majorities

While we focus on a 50-50 gridlocked Senate as the relevant threshold in our benchmark analyses, we also examine periods when one party was close to achieving a 60-40 seat majority. As described in Section 2.1, a 60-40 majority constitutes an important threshold for Senate control because it allows the majority party to bypass filibustering by the opposing party on non-budgetary legislation. However, no party achieved a filibuster-proof majority during our sample period. Nevertheless, we examine two periods when the Democratic party came close: during the 103rd Congress (1993-1994), when they held a 57-43 seat margin, and during the 111th Congress (2009-2010), when they

[49] Note that *SwingMode* is absorbed by fixed effects as it is time invariant.

held a 59-41 margin.

We define *LockedSuperMaj* as an indicator variable for these two congressional terms, and include it as an additional interaction term with *Swing* in Eq. 1. We present the results for estimating this new specification on our main firm outcomes in Table BV. We see that the coefficient estimate on *Swing* × *LockedSuperMaj* is of the same sign and similar in magnitude to that of the coefficient estimate on *Swing* × *LockedSenate* across several outcome variables. However, the estimates generally lack statistical significance, potentially due to the limited statistical power stemming from the infrequency of large majorities in the Senate.

7 Conclusions

In this paper, we find evidence that firms benefit from increases in the political bargaining power of their home regions. Comparing swing states and partisan states during periods of narrow and wide Senate majority margins, we find that predicted increases in regional political influence are associated with higher amounts of federal financial assistance at the state level. At the firm level, increases in regional political bargaining power result in increases in tax credit subsidies and decreases in effective federal tax rates, leading to higher corporate valuations and investments. We also find suggestive evidence that firms benefit from favorable trade policies but not from favorable regulation. To sharpen identification of the effect on firm value, we design an event study around the 2021 Georgia runoff election, and find that an unexpected balancing of the Senate around a 50-50 tipping point resulted in higher returns for swing-state firms relative to partisan-state firms.

Overall, our findings indicate that shifts in the balance of strategic political power across geographic regions have economic consequences. In an increasingly polarized political landscape where the swing votes of strategically important political representatives are crucial to the passage of legislation, it is important to understand the nature of those economic consequences and the mechanisms through which strategic political capital creates or destroys value for private-sector firms. While we do not make normative claims about allocative efficiency, it is natural to ask whether firms located in regions of greater strategic importance in federal politics are in greater need of government transfers and favorable policies. We leave this question for future investigation.

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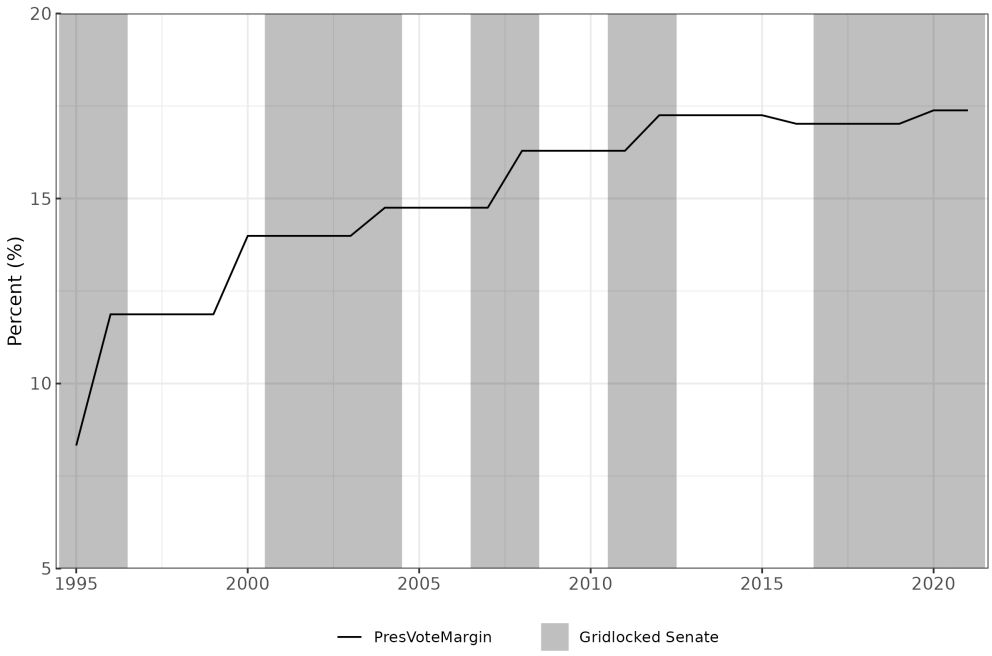
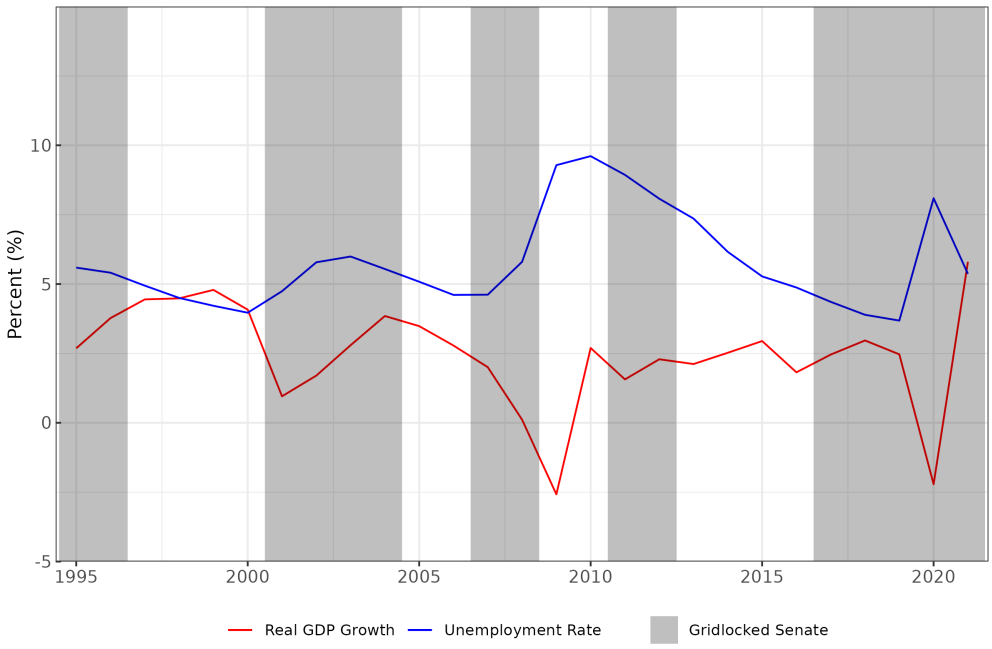


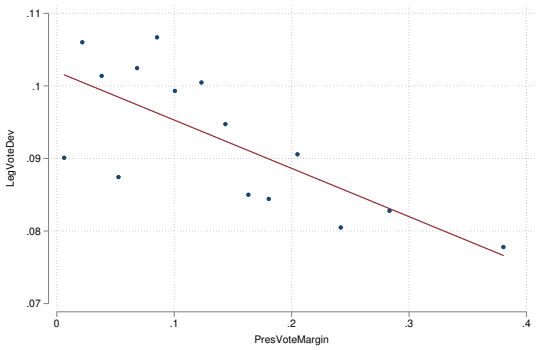
(a) Average state-level presidential vote margin



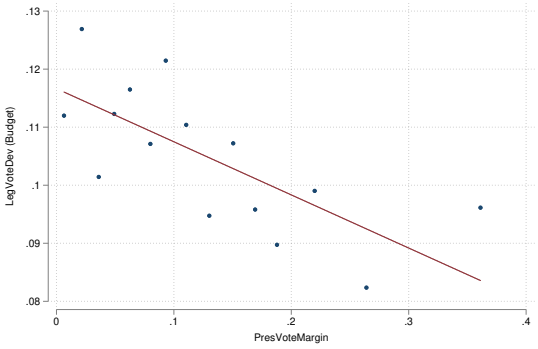
(b) Real GDP growth and unemployment rate

Figure I: Time series trends and Senate gridlock

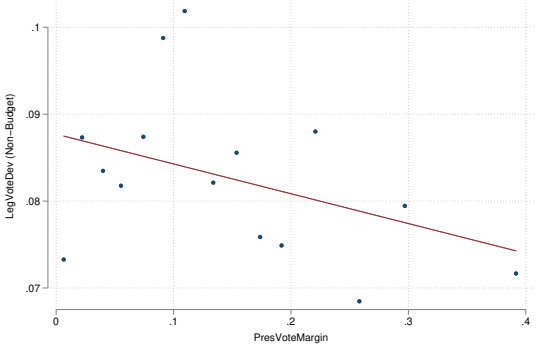
This figure illustrates the time series average of state-level presidential vote margins (*PresVoteMargin*) in subfigure (a) and the annual U.S. real GDP growth and unemployment rate in subfigure (b), where both time series plots show periods of gridlocked Senate terms (based on a six-seat margin threshold) in grey and periods of consolidated Senate terms in white.



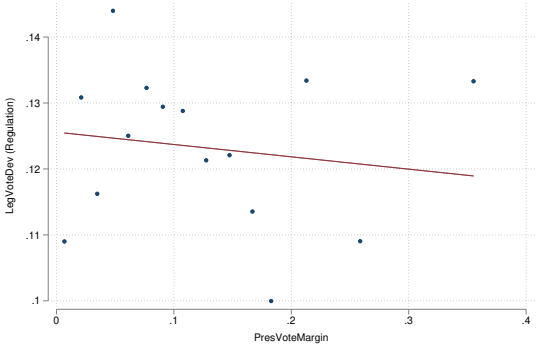
(a) All bills



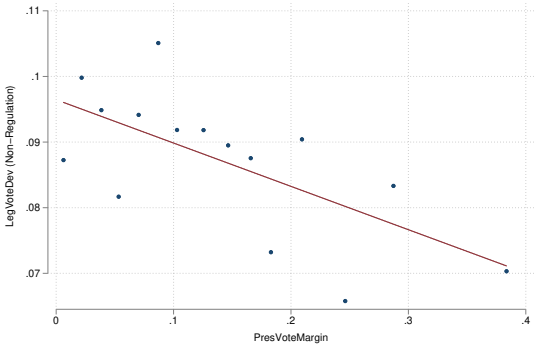
(b) Budget Bills



(c) Non-Budget Bills



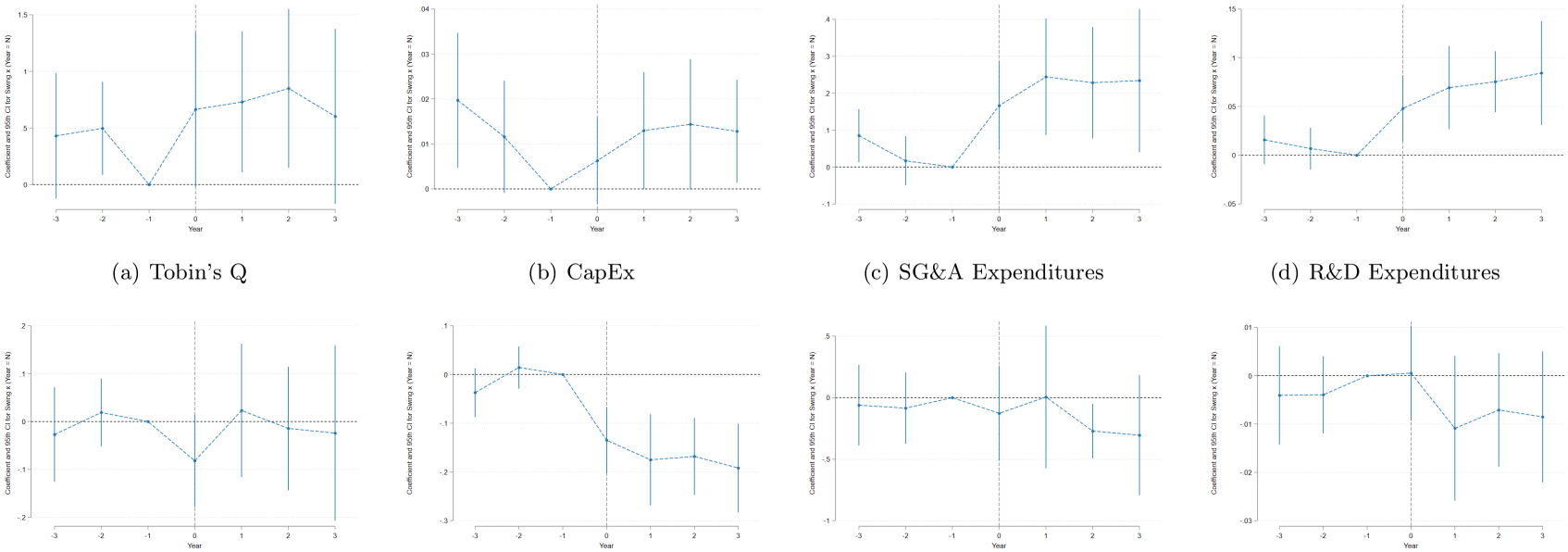
(d) Regulatory Bills



(e) Non-Regulatory Bills

Figure II: Presidential vote margins vs. legislative vote deviations

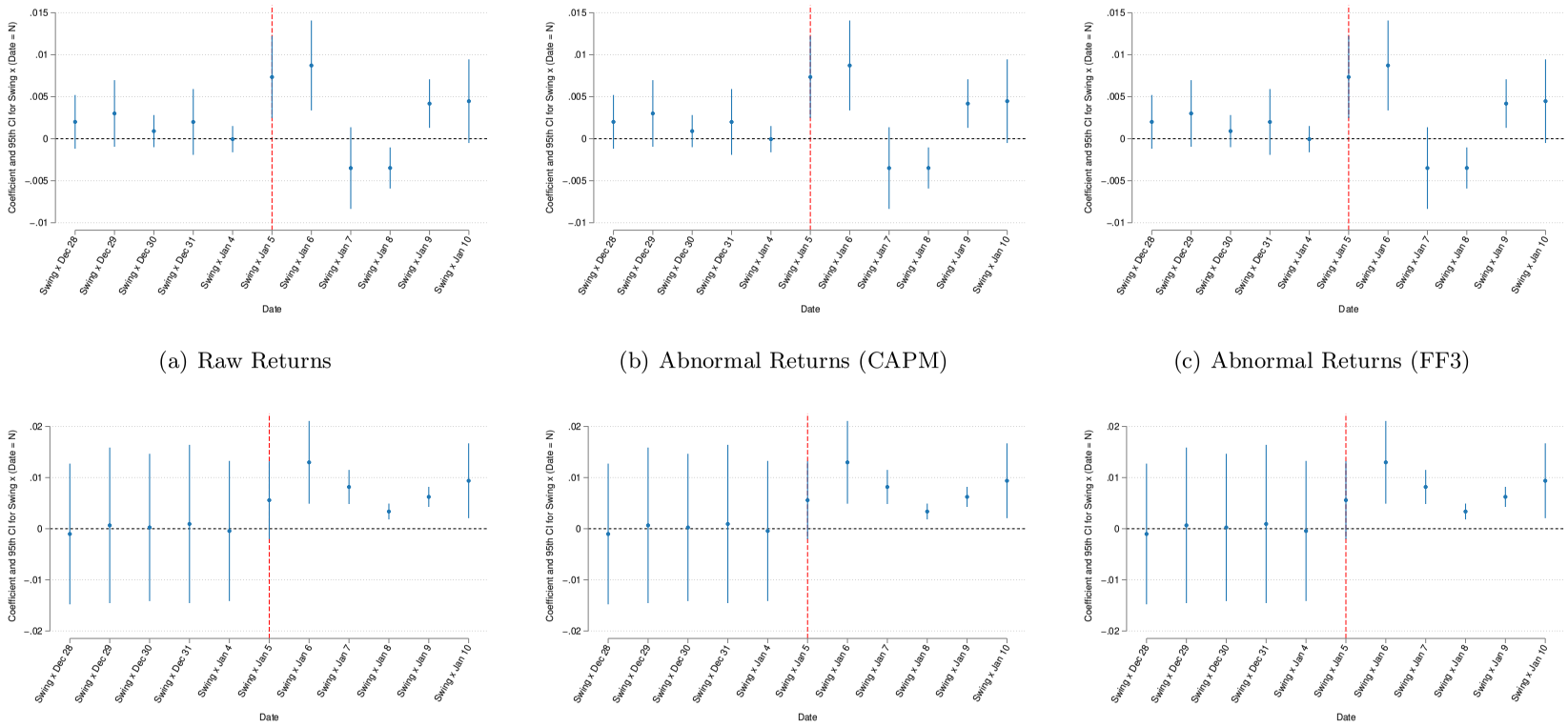
These figures show binned scatterplots representing the relationship between *PresVoteMargin* (the state-level presidential vote margin in the prior election) and *LegVoteDev* (the rate at which a State's Senators votes against their party). Subfigure (a) shows this relationship for all bills, subfigure (b) shows this relationship for budget-related bills only, subfigure (c) shows this relationship for non-budget-related bills only, subfigure (d) shows this relationship for regulatory-related bills only, and subfigure (e) shows this relationship for non-regulatory-related bills only.



(a) Tobin's Q (b) CapEx (c) SG&A Expenditures (d) R&D Expenditures

(e) lnSaleGr (f) ROA (g) Profitability (h) Leverage

Figure III: Year-by-year dynamic comparison of swing-state and partisan-state firms around shifts in Senate balance
These figures show the point estimates and 95% confidence intervals for δ_τ from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \Sigma_{\tau=-3}^{3} \delta_\tau Swing_{it} I(\tau)_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level investment variables denoted in the figure captions and $I(\tau)_t$ represents a dummy variable indicating that year t is τ years before/after either the reference election in 2000 or 2016. All specifications include additional control variables: *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry.



(a) Raw Returns

(b) Abnormal Returns (CAPM)

(c) Abnormal Returns (FF3)

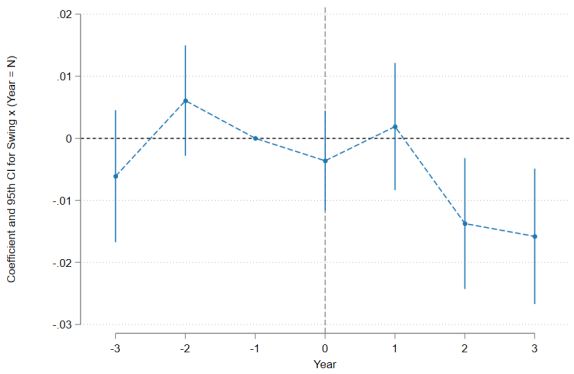
(d) Cumulative Returns

(e) Cumulative Abnormal Returns (CAPM)

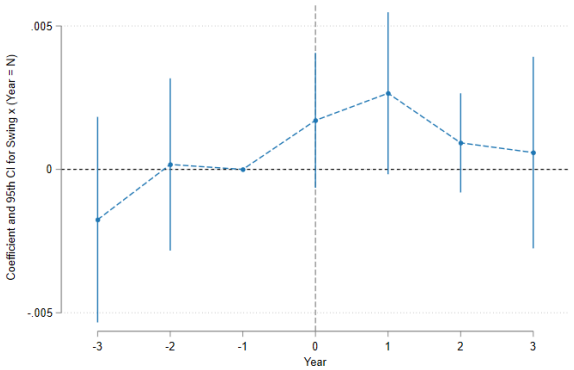
(f) Cumulative Abnormal Returns (FF3)

Figure IV: Georgia runoff election event study

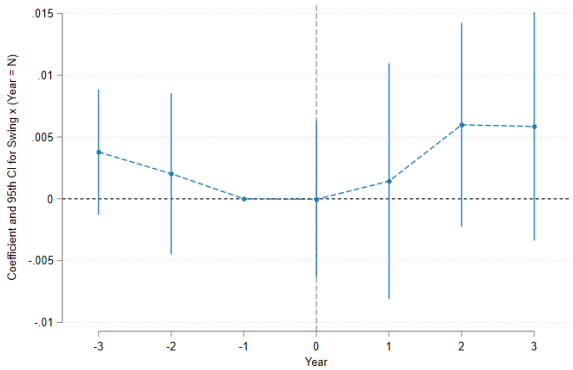
These figures show the point estimates and 95% confidence intervals for π_τ from estimating $R_{it} = \alpha_i + \gamma_t + \sum_\tau \pi_\tau Swing_i \times \tau_t + \epsilon_{it}$, where R_{it} represents return measures indicated by the figure sub-captions, and τ_t denotes dummy variables indicating whether $\tau = t$ where τ takes on values from 5 trading days before to 5 trading days after Jan 5, 2021. In the first three subfigures, the dependent variables constitute daily (abnormal) returns, and in the last three subfigures, the dependent variables constitute cumulative (abnormal) returns calculated as the sum of daily (abnormal) returns over the $[-10, t]$ trading-day interval. Abnormal returns based on the CAPM model use CAPM betas estimated over a [-220,-11] estimation window, and abnormal returns based on the Fama-French 3-factor model are based on CAPM, SMB, and HML betas estimated over a [-220,-11] estimation window.



(a) FedTaxETR



(b) StateTaxETR



(c) ForeignTaxETR

Figure V: Year-by-year dynamic comparison of swing-state and partisan-state firms around shifts in Senate balance

These figures show the point estimates and 95% confidence intervals for δ_τ from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \Sigma_{\tau=-3}^{3} \delta_\tau Swing_{it} I(\tau)_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level tax rate variables denoted in the figure captions and $I(\tau)_t$ represents a dummy variable indicating that year t is τ years before/after either the reference election in 2000 or 2016. All specifications include additional control variables: *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry.

Table I: Senate Margins

This table presents the Senate seat margins for each congressional term from 1993 to 2020. Each row provides the time period, the party with majority control of the Senate, the seat balance between the majority and minority parties, and the seat advantage margin of the majority party.

U.S. Congress	Time Period	Majority Party	Seats	Margin
103	1993-1994	Democratic	57-43	14
104	1995-1996	Republican	52-48	4
105	1997-1998	Republican	55-45	10
106	1999-2000	Republican	55-45	10
107	2001-2002	Split*	50-50	0
108	2003-2004	Republican	51-49	2
109	2005-2006	Republican	55-45	10
110	2007-2008	Democratic	51-49	2
111	2009-2010	Democratic	59-41	18
112	2011-2012	Democratic	53-47	6
113	2013-2014	Democratic	55-45	10
114	2015-2016	Republican	54-46	8
115	2017-2018	Republican	51-49	2
116	2019-2020	Republican	53-47	6

* From the start of the term (3 Jan 2001) to 19 Jan 2001, Democrats controlled a split Senate due to the tie-breaking vote of the Democratic Vice President. From 20 Jan 2001 to 5 Jun 2001, Republicans controlled a split Senate due to the Republican Vice President. From 6 Jun 2001 to 11 Nov 2002, Democrats controlled the Senate with a two seat margin. From 12 Nov 2002 to the end of the term (2 Jan 2003), Republicans controlled the Senate with a two seat margin.

Table II: Summary Statistics.

These tables present summary statistics for the main regression variables used in this paper. Panel A provides statistics on state-level variables. Panel B provides statistics on firm-level variables. Panel C provides statistics on daily returns and cumulative returns. Detailed definitions for all variables can be found in Appendix A.

Panel A: State-Level Variables

	Observations	Mean	Std Dev	P25	Median	P75
PresVoteMargin	1,350	0.148	0.105	0.063	0.132	0.211
lnTotSpend	1,350	24.671	1.087	23.840	24.705	25.497
lnContracts	1,350	21.779	1.401	20.801	21.922	22.791
lnGrants	1,350	22.473	1.018	21.702	22.471	23.177
lnLoans	1,284	20.009	2.569	17.777	20.878	21.947
lnPayments	1,350	23.320	1.134	22.503	23.338	24.158
lnGDP	1,200	12.097	1.041	11.182	12.153	12.844
lnEmp	1,350	14.121	1.012	13.225	14.192	14.823
lnWage	1,350	24.669	1.135	23.774	24.738	25.533
lnEstab	1,000	11.517	0.944	10.731	11.583	12.175

Panel B: Annual Firm Variables

	Observations	Mean	Std Dev	P25	Median	P75
SubsidyTaxCredit	134,569	0.001	0.123	0.000	0.000	0.000
SubsidyPropTax	134,569	0.000	0.000	0.000	0.000	0.000
SubsidyFedGrant	134,569	0.001	0.221	0.000	0.000	0.000
SubsidyNonFedGrant	134,569	0.001	0.368	0.000	0.000	0.000
TobinsQ	125,341	3.690	6.958	1.136	1.648	2.924
CapEx	131,698	0.074	0.122	0.014	0.035	0.078
SG&A	120,006	0.717	1.730	0.151	0.323	0.610
R&D	81,540	0.194	0.423	0.007	0.056	0.185
SaleGr	124,839	0.107	0.423	-0.043	0.077	0.238
ROA	132,448	-0.271	1.549	-0.091	0.091	0.174
Profitability	134,168	-1.842	9.435	-0.066	0.077	0.162
Leverage	141,810	0.204	0.277	0.000	0.101	0.311
FedTaxETR	124,451	0.096	0.203	0.000	0.000	0.202
StateTaxETR	123,513	0.018	0.047	0.000	0.000	0.031
ForeignTaxETR	127,299	0.025	0.103	0.000	0.000	0.010

Panel C: Event Study Returns

	Observations	Mean	Std Dev	P25	Median	P75
RawRet	70,356	0.007	0.052	-0.011	0.002	0.017
AR_CAPM	70,356	-0.003	0.052	-0.020	-0.007	0.007
AR_FF3	70,356	-0.002	0.053	-0.023	-0.004	0.012
CumRet	70,356	0.053	0.177	-0.006	0.021	0.076
CAR_CAPM	70,356	-0.034	0.173	-0.096	-0.043	-0.006
CAR_FF3	70,356	-0.014	0.173	-0.070	-0.028	0.008

Table III: Effect on Federal Transfers

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta LockedSenate_t + \delta Swing_{it} \times LockedSenate_t + \epsilon_{it}$, where Y represents various state-level measures of federal spending. The sample consists of state-year observations during the 1994-2020 time period. Only coefficients for *Swing* and *Swing* × *LockedSenate* are reported, as *LockedSenate* is subsumed by year fixed effects. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and clustered by state. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) lnTotSpend	(2) lnContracts	(3) lnGrants	(4) lnLoans	(5) lnPayments
Swing	-0.0494	-0.0231	-0.0075	-0.1414*	-0.0456
	(0.0340)	(0.0408)	(0.0135)	(0.0706)	(0.0493)
Swing x LockedSenate	0.0405***	0.0362	0.0124	0.1690**	0.0503***
	(0.0139)	(0.0272)	(0.0118)	(0.0830)	(0.0176)
State FE	X	X	X	X	X
Year FE	X	X	X	X	X
Observations	1,350	1,350	1,350	1,284	1,350
Adjusted R-squared	0.958	0.961	0.987	0.925	0.943

Table IV: Effect on Firm Subsidies

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta LockedSenate_t + \delta Swing_{it} \times LockedSenate_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level subsidy variables denoted in column headings, where all subsidy variables are scaled by sales. In Panel A, the sample consists of firm-year observations during the 1994-2020 time period, and in Panel B, the sample consists of firm-year observations during the 1994-2020 time period for firms that received at least one non-zero subsidy of the type indicated by the column heading. Only coefficients for *Swing* and *Swing* × *LockedSenate* are reported, as *LockedSenate* is subsumed by year fixed effects. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

Panel A: Unrestricted sample

	(1) SubsidyTaxCredit	(2) SubsidyPropTax	(3) SubsidyNonFedGrant	(4) SubsidyFedGrant
Swing	-0.0002	0.0000	0.0001	0.0045
	(0.0001)	(0.0000)	(0.0002)	(0.0040)
Swing x LockedSenate	0.0003***	0.0000	-0.0000	0.0024
	(0.0001)	(0.0000)	(0.0002)	(0.0021)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	133,636	133,636	133,636	133,636
Adjusted R-squared	0.413	-0.003	0.442	0.115

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Panel B: Subsidized firms

	(1) SubsidyTaxCredit	(2) SubsidyPropTax	(3) SubsidyNonFedGrant	(4) SubsidyFedGrant
Swing	-0.0018	0.0000	0.0034	0.2205
	(0.0012)	(0.0000)	(0.0047)	(0.1905)
Swing x LockedSenate	0.0037***	0.0000	0.0026	0.1259
	(0.0010)	(0.0000)	(0.0052)	(0.1243)
Control Variables	No	No	No	No
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	10,082	2,370	6,411	2,230
Adjusted R-squared	0.443	0.056	0.473	0.157

Table V: Effect on Firm Valuation and Investment

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta LockedSenate_t + \delta Swing_{it} \times LockedSenate_t + \epsilon_{it}$, where Y represents various firm-level investment variables denoted in column headings. The sample consists of firm-year observations during the 1994-2020 time period. The specifications used in columns 5-8 include additional control variables: *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D	(5) TobinsQ	(6) CapEx	(7) SG&A	(8) R&D
Swing	-0.2851***	-0.0019	-0.0367	-0.0142**	-0.2793***	-0.0018	-0.0357	-0.0141**
	(0.0945)	(0.0016)	(0.0239)	(0.0056)	(0.0921)	(0.0017)	(0.0237)	(0.0054)
Swing x LockedSenate	0.2045***	0.0033*	0.0531**	0.0219***	0.1921***	0.0033*	0.0522**	0.0207***
	(0.0759)	(0.0019)	(0.0217)	(0.0077)	(0.0694)	(0.0019)	(0.0210)	(0.0075)
Democrat					-0.1611	0.0023	-0.0566	-0.0321***
					(0.1184)	(0.0035)	(0.0713)	(0.0064)
Republican					-0.3626***	-0.0006	-0.0876	-0.0328***
					(0.1320)	(0.0037)	(0.0737)	(0.0099)
MajPty					0.3279***	-0.0031	0.0605	0.0362***
					(0.0720)	(0.0033)	(0.0698)	(0.0067)
MinPty					0.3612***	0.0009	0.0805	0.0415***
					(0.1216)	(0.0038)	(0.0770)	(0.0098)
CommChairTop1					-0.0429	-0.0063	-0.0423	-0.0137
					(0.1693)	(0.0059)	(0.0583)	(0.0179)
CommChairTop3					0.1558	-0.0038	-0.0182	0.0023
					(0.1778)	(0.0041)	(0.0338)	(0.0136)
CommChairTop5					-0.0801	0.0030*	0.0108	0.0088
					(0.0688)	(0.0018)	(0.0101)	(0.0062)
Control Variables	No	No	No	No	Yes	Yes	Yes	Yes
Firm FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X
Observations	124,220	130,035	118,339	80,475	124,220	130,035	118,339	80,475
Adjusted R-squared	0.605	0.381	0.490	0.489	0.605	0.381	0.490	0.489

Table VI: Effect on Operating Performance and Leverage

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta LockeSenate_t + \delta Swing_{it} \times LockeSenate_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level performance variables denoted in column headings. The sample consists of firm-year observations during the 1994-2020 time period. All specifications include additional control variables: *Democrat*, *Republican*, *MajParty*, *MinParty*, *GDPgr*, *EmpGr*, *WageGr*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Only coefficients for *Swing* and *Swing* × *LockeSenate* are reported to conserve space. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) lnSaleGr	(2) ROA	(3) Profitability	(4) Leverage
Swing	-0.0202	0.0263*	-0.0005	0.0036
	(0.0166)	(0.0156)	(0.0683)	(0.0034)
Swing x LockedSenate	0.0167	-0.0476**	-0.0131	-0.0046*
	(0.0227)	(0.0184)	(0.0944)	(0.0027)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	82,673	130,801	133,222	141,004
Adjusted R-squared	0.330	0.523	0.486	0.505

Table VII: Georgia Runoff Election Event Study

This table reports the results from estimating $R_{it} = \alpha_i + \gamma_t + \sum_\tau \pi_\tau Swing_i \times \tau_t + \epsilon_{it}$, where R represents daily returns and abnormal returns in columns (1)-(3), and cumulative returns and cumulative abnormal returns in columns (4)-(6). Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and date. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) RawRet	(2) AR_CAPM	(3) AR_FF3	(4) CumRet	(5) CAR_CAPM	(6) CAR_FF3
Swing x Dec 28	0.0020	0.0020	0.0020	-0.0010	-0.0010	-0.0010
	(0.0015)	(0.0015)	(0.0015)	(0.0065)	(0.0065)	(0.0065)
Swing x Dec 29	0.0030	0.0030	0.0030	0.0007	0.0007	0.0007
	(0.0019)	(0.0019)	(0.0019)	(0.0072)	(0.0072)	(0.0072)
Swing x Dec 30	0.0009	0.0009	0.0009	0.0002	0.0002	0.0002
	(0.0009)	(0.0009)	(0.0009)	(0.0068)	(0.0068)	(0.0068)
Swing x Dec 31	0.0020	0.0020	0.0020	0.0009	0.0009	0.0009
	(0.0018)	(0.0018)	(0.0018)	(0.0073)	(0.0073)	(0.0073)
Swing x Jan 4	-0.0000	-0.0000	-0.0000	-0.0004	-0.0004	-0.0004
	(0.0007)	(0.0007)	(0.0007)	(0.0065)	(0.0065)	(0.0065)
Swing x Jan 5	0.0073***	0.0073***	0.0073***	0.0056	0.0056	0.0056
	(0.0023)	(0.0023)	(0.0023)	(0.0036)	(0.0036)	(0.0036)
Swing x Jan 6	0.0087***	0.0087***	0.0087***	0.0130***	0.0130***	0.0130***
	(0.0026)	(0.0026)	(0.0026)	(0.0038)	(0.0038)	(0.0038)
Swing x Jan 7	-0.0035	-0.0035	-0.0035	0.0082***	0.0082***	0.0082***
	(0.0023)	(0.0023)	(0.0023)	(0.0015)	(0.0015)	(0.0015)
Swing x Jan 8	-0.0035***	-0.0035***	-0.0035***	0.0034***	0.0034***	0.0034***
	(0.0011)	(0.0011)	(0.0011)	(0.0006)	(0.0006)	(0.0006)
Swing x Jan 9	0.0042***	0.0042***	0.0042***	0.0062***	0.0062***	0.0062***
	(0.0014)	(0.0014)	(0.0014)	(0.0008)	(0.0008)	(0.0008)
Swing x Jan 10	0.0045*	0.0045*	0.0045*	0.0094**	0.0094**	0.0094**
	(0.0023)	(0.0023)	(0.0023)	(0.0033)	(0.0033)	(0.0033)
Firm FE	X	X	X	X	X	X
Date FE	X	X	X	X	X	X
Observations	70,356	70,356	70,356	70,356	70,356	70,356
Adjusted R-squared	0.058	0.041	0.071	0.737	0.726	0.724

Table VIII: Effect on Overall State Economy

These tables report the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta LockedSenate_t + \delta Swing_{it} \times LockedSenate_t + \epsilon_{it}$, where Y represents various state-level economic measures denoted in the column headings. In Panel A, Y represents the natural log of state-level GDP in column 1, the natural log of private-sector employment in column 2, the natural log of total private-sector wages within the state in column 3, and the natural log of the number of private-sector establishments in the state in column 4. In Panel B, *lnEmpsX* represents the natural log of employment across all firms in state i within a specific firm-size category X. The sample for both panels consists of state-year observations for the 1994-2020 time period. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and clustered by state and 4-digit NAICS industry in Panel A and by state in Panel B. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

Panel A: Effect on State Economic Outcomes

	(1) lnGDP	(2) lnEmp	(3) lnWages	(4) lnEstabs
Swing	-0.0099	0.0038	0.0000	-0.0050
	(0.0102)	(0.0054)	(0.0102)	(0.0066)
Swing x LockedSenate	0.0164*	0.0093*	0.0154**	0.0046
	(0.0088)	(0.0047)	(0.0070)	(0.0032)
State FE	X	X	X	X
Year FE	X	X	X	X
Observations	1,200	1,350	1,350	1,000
Adjusted R-squared	0.996	0.998	0.995	0.997

Panel B: Employment Effects for Large vs. Small Firms

	(1) lnEmps0to19	(2) lnEmps20to49	(3) lnEmps50to249	(4) lnEmps250to499	(5) lnEmps500plus
Swing	-0.0027	-0.0008	0.0030	0.0069	0.0044
	(0.0060)	(0.0058)	(0.0084)	(0.0086)	(0.0074)
Swing x LockedSenate	0.0121**	0.0154**	0.0118**	0.0093	0.0095*
	(0.0053)	(0.0061)	(0.0058)	(0.0080)	(0.0053)
State FE	X	X	X	X	X
Year FE	X	X	X	X	X
Observations	1,159	1,159	1,159	1,159	1,159
Adjusted R-squared	0.997	0.997	0.997	0.994	0.998

Table IX: Effect on Effective Tax Rates

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta LockedSenate_t + \delta Swing_{it} \times LockedSenate_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level tax variables denoted in column headings. The sample consists of firm-year observations during the 1994-2020 time period. All specifications include additional control variables: *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Only coefficients for *Swing* and *Swing* × *LockedSenate* are reported to conserve space. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) FedTaxETR	(2) StateTaxETR	(3) ForeignTaxETR	(4) FedTaxETR	(5) StateTaxETR	(6) ForeignTaxETR
Swing	0.0016	-0.0004	-0.0016	0.0033	-0.0001	-0.0002
	(0.0022)	(0.0008)	(0.0015)	(0.0020)	(0.0008)	(0.0014)
Swing x BalSenate	-0.0059***	0.0002	0.0021	-0.0021	0.0003	0.0013
	(0.0019)	(0.0005)	(0.0015)	(0.0018)	(0.0006)	(0.0015)
Firm FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
NAICS4-Year FE				X	X	X
Observations	123,538	122,579	126,412	122,374	121,415	125,234
Adjusted R-squared	0.245	0.186	0.247	0.265	0.202	0.252

Table X: Tradable vs. Non-Tradable

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \eta Tradable_{it} + \mu Tradable_{it} \times LockedSenate_t + \theta LockedSenate_t + \delta Swing_{it} \times LockedSenate_t + \phi Swing_{it} \times Tradable_{it} + \kappa Swing_{it} \times Tradable_{it} \times LockedSenate_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level variables denoted in column headings. All specifications include additional control variables: *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Only select coefficients are reported to conserve space. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing	0.0131	0.0027	0.0068	0.0009
	(0.1229)	(0.0057)	(0.0285)	(0.0070)
Tradable x LockedSenate	-0.1672	0.0013	-0.0120	-0.0262***
	(0.1519)	(0.0033)	(0.0239)	(0.0069)
Swing x LockedSenate	0.1051	0.0009	0.0281	-0.0027
	(0.1229)	(0.0029)	(0.0407)	(0.0050)
Swing x Tradable	-0.3164**	-0.0071	-0.0527	-0.0159*
	(0.1346)	(0.0084)	(0.0469)	(0.0094)
Swing x Tradable x LockedSenate	0.1420	0.0015	0.0300	0.0260***
	(0.1435)	(0.0041)	(0.0445)	(0.0066)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	79,382	82,482	75,806	62,497
Adjusted R-squared	0.618	0.400	0.517	0.501

Table XI: Heterogeneity in High. vs. Low Import Penetration Exposure

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \eta HighIPR_{it} + \mu HighIPR_{it} \times LockedSenate_t + \theta LockedSenate_t + \delta Swing_{it} \times LockedSenate_t + \phi Swing_{it} \times HighIPR_{it} + \kappa Swing_{it} \times HighIPR_{it} \times LockedSenate_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level variables denoted in column headings. The sample consists of firm-year observations during the 1994-2020 time period. All specifications include additional control variables: *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Only select coefficients are reported to conserve space. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing	-0.0498	0.0002	0.0009	-0.0084
	(0.1953)	(0.0036)	(0.0313)	(0.0060)
HighIPR	0.1339	0.0075	0.0484	0.0230*
	(0.2926)	(0.0061)	(0.0438)	(0.0133)
HighIPR x LockedSenate	-0.2361	-0.0108***	-0.0991***	-0.0385***
	(0.1776)	(0.0033)	(0.0357)	(0.0069)
Swing x LockedSenate	0.1082	-0.0010	0.0032	0.0127
	(0.1673)	(0.0031)	(0.0310)	(0.0092)
Swing x HighIPR	-0.2888	-0.0105**	-0.0661	-0.0248***
	(0.3325)	(0.0050)	(0.0477)	(0.0083)
Swing x HighIPR x LockedSenate	0.0216	0.0071**	0.0988**	0.0208
	(0.3094)	(0.0035)	(0.0456)	(0.0171)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	26,957	28,012	26,363	22,684
Adjusted R-squared	0.667	0.324	0.592	0.571

Table XII: Heterogeneity in High. vs. Low Regulatory Exposure

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \eta Regulated_{it} + \mu Regulated_{it} \times LockedSenate_t + \theta LockedSenate_t + \delta Swing_{it} \times LockedSenate_t + \phi Swing_{it} \times Regulated_{it} + \kappa Swing_{it} \times Regulated_{it} \times LockedSenate_t + X_{it}\beta + \epsilon_{it}$ where Y represents various firm-level variables denoted in column headings. The sample consists of firm-year observations during the 1994-2020 time period. All specifications include additional control variables: *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Only select coefficients are reported to conserve space. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing	-0.2679*	-0.0043	0.0195	-0.0142
	(0.1338)	(0.0036)	(0.0379)	(0.0140)
Regulated	0.3949	-0.0062	0.1945	0.0745**
	(0.4113)	(0.0037)	(0.1231)	(0.0318)
Regulated x LockedSenate	-0.1080	0.0100***	-0.0274	-0.0213**
	(0.0708)	(0.0023)	(0.0372)	(0.0098)
Swing x LockedSenate	0.2279**	0.0051**	0.0694***	0.0281**
	(0.0984)	(0.0023)	(0.0244)	(0.0107)
Swing x Regulated	-0.1459	0.0011	-0.1423*	-0.0126
	(0.1339)	(0.0074)	(0.0773)	(0.0152)
Swing x Regulated x LockedSenate	-0.0218	-0.0053	-0.0345	0.0005
	(0.0913)	(0.0053)	(0.0359)	(0.0154)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	53,966	56,487	49,560	34,584
Adjusted R-squared	0.637	0.426	0.550	0.503

Appendix A Variable Definitions

PresVoteMargin is the absolute vote margin between the Democratic and Republican parties within a given state during the previous presidential election.

Swing is an indicator variable that takes on a value of one if *PresVoteMargin* is above the sample median within a given year, and zero otherwise.

LockedSenate is an indicator variable that takes on a value of one if the margin of the majority party that controls the Senate is less or equal to six, and zero otherwise.

LockedSenate2 is an indicator variable that takes on a value of one if the margin of the majority party that controls the Senate is less or equal to two, and zero otherwise.

LockedSenate4 is an indicator variable that takes on a value of one if the margin of the majority party that controls the Senate is less or equal to four, and zero otherwise.

LockedSuperMaj is an indicator variable that takes on a value of one if one party is close to holding a 60-40 Senate majority (the 103rd and 111th Congress), and zero otherwise.

lnTotSpend is the natural log of total obligated federal spending in a given state.

lnContracts is the natural log of total obligated federal contracts awarded to a given state.

lnGrants is the natural log of total obligated federal grants awarded to a given state.

lnLoans is the natural log of total obligated federal loans awarded to a given state.

lnPayments is the natural log of total obligated federal direct payments awarded to a given state.

LegVoteDev is an indicator variable that takes on a value of one if a senator votes differently than the median member of her party on a given vote, and zero otherwise.

SubsidyTaxCredit is the total amount of tax credit subsidies directed at a given firm scaled by its annual sales.

SubsidyPropTax is the total amount of property tax subsidies directed at a given firm scaled by its annual sales.

SubsidyFedGrant is the total amount of federal grant subsidies directed at a given firm scaled by its annual sales.

SubsidyNonFedGrant is the total amount of non-federal grant subsidies directed at a given firm scaled by its annual sales.

TobinsQ is the ratio between the market value of assets ($AT + PRCC_F \times CSHO - CEQ - TXDITC$) and book value of assets (AT).

ROA is the return on asset (Ordinary income before depreciation scaled by lagged AT).

lnSaleGr is the natural log of the annual sale growth rate ($SALE$ scaled by lagged $SALE$).

Profitability is ordinary income before depreciation (OIBDP) scaled by sales (SALE).

CapEx is capital expenditures ($CAPX$) scaled by lagged total assets (AT).

SG&A is selling, general and administrative expenses ($XSGA$) scaled by lagged total assets (AT).

R&D is research and development expenses (XRD) scaled by lagged total assets (AT).

Leverage is long-term debt ($DLTT$ scaled by AT).

FedTaxETR is current federal taxes ($TXFED$) scaled by adjusted pre-tax income ($PI - SPI$).

StateTaxETR is current state taxes (TXS) scaled by adjusted pre-tax income ($PI - SPI$).

ForeignTaxETR is current foreign taxes ($TXFO$) scaled by adjusted pre-tax income ($PI - SPI$).

RawRet is the daily return (adjusted for dividends) for a company's stock.

AR_CAPM is the abnormal daily return defined as the difference between realized returns and expected returns as predicted by the CAPM.

AR_FF3 is the abnormal daily return defined as the difference between realized returns and expected returns as predicted by the Fama-French 3-factor model.

CumRet is the sum of daily raw returns in the [-10,t] trading day window around the January 5, 2021 Georgia runoff elections.

CAR_CAPM is the sum of *AR_CAPM* in the [-10,t] trading day window around the January 5, 2021 Georgia runoff elections.

CAR_FF3 is the sum of *AR_FF3* in the [-10,t] trading day window around the January 5, 2021 Georgia runoff elections.

MajParty is an indicator variable that takes a value of one if a senator is in the majority party, and zero otherwise. At the state level, it is an indicator variable that takes on a value of one if both Senators for a given state are members of the majority party, and zero otherwise.

MinParty is an indicator variable that takes a value of one if a senator is in the minority party, and zero otherwise. At the state level, it is an indicator variable that takes on a value of one if both Senators for a given state are members of the minority party, and zero otherwise.

Democrat is an indicator variable that takes on a value of one if both Senators in a state are members of the Democratic party, and zero otherwise.

Republican is an indicator variable that takes on a value of one if both Senators in a state are members of the Republican party, and zero otherwise.

CommChairTopX is an indicator variable that takes on a value of one if the firm is headquartered in a state with a senator on one of the top X most influential Senate committees, respectively (committee influence defined according to Edwards and Stewart III (2006)).

lnGDP The natural log of state-level GDP.

lnEmp The natural log of state-level private-sector employment.

lnWages The natural log of state-level private-sector total wages.

lnEstabs The natural log of state-level private-sector establishment counts.

lnEmpsX is the natural log of state-level employment in a given year across firms within size category X, where X includes *All* (all sizes), *0to19* (0 to 19 employees), *20to49* (20 to 49 employees), *50to249* (50 to 249 employees), *250to499* (250 to 499 employees), and *500plus* (500 employees or greater).

Tradable is an indicator variable that takes on a value of one if a firm belongs to a tradable-sector industry as defined in Mian and Sufi (2014).

HighIPR is an indicator variable that takes on a value of one if the import penetration index for a given industry is above the sample median within a given year.

Regulated is an indicator variable that takes on a value of one if the RegData regulatory restriction index for a given industry is above the sample median within a given year.

Appendix B Tables

Table BI: Swing States and Senator Electoral Vulnerability

This table reports the results from estimating $M_{it} = \alpha_i + \gamma_t + \lambda X_{it} + \theta LockedSenate_t + \delta X_{it} \times LockedSenate_t + \epsilon_{it}$, where M represents *SenWinMargin*, the vote margin between the winning senate election candidate and the runner up, in columns (1) and (3), and *SenWinPct*, the proportion of votes captured by the winning senate candidate in columns (2) and (4) and X represents *Swing* in columns (1) and (2) and *PresVoteMargin* in columns (3) and (4). Standard errors are corrected for heteroskedasticity and clustered by state. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) SenWinMargin	(2) SenWinPct	(3) SenWinMargin	(4) SenWinPct
Swing	-0.0798***	-0.0388***		
	(0.0254)	(0.0131)		
PresVoteMargin			0.6062***	0.2804***
			(0.0980)	(0.0521)
LockedSenate	-0.0055	0.0004	-0.0408*	-0.0174
	(0.0225)	(0.0116)	(0.0215)	(0.0110)
Swing x LockedSenate	-0.0301	-0.0132		
	(0.0269)	(0.0142)		
PresVoteMargin x LockedSenate			0.0827	0.0504
			(0.1314)	(0.0665)
Observations	481	481	481	481
Adjusted R-squared	0.079	0.067	0.161	0.136

Table BII: Alternative Definitions of Gridlocked Senates

These tables report the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta T_t + \delta Swing_{it} \times T_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level variables denoted in column headings, and T denotes *LockedSenate2* in Panel A and *LockedSenate4* in Panel B. The sample consists of firm-year observations during the 1994-2020 time period. All specifications include additional control variables *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Only select coefficients are reported to conserve space. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

Panel A: Two Senator Control Margin

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing	-0.2532***	-0.0005	-0.0296	-0.0092***
	(0.0782)	(0.0019)	(0.0221)	(0.0027)
Swing x LockedSenate2	0.2291***	0.0011	0.0656**	0.0177**
	(0.0716)	(0.0029)	(0.0257)	(0.0071)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	124,220	130,035	118,339	80,475
Adjusted R-squared	0.605	0.381	0.490	0.489

Panel B: Four Senator Control Margin

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing	-0.2623***	-0.0010	-0.0278	-0.0066**
	(0.0814)	(0.0018)	(0.0216)	(0.0027)
Swing x LockedSenate4	0.1958***	0.0021	0.0449**	0.0070
	(0.0572)	(0.0020)	(0.0186)	(0.0058)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	124,220	130,035	118,339	80,475
Adjusted R-squared	0.605	0.381	0.490	0.489

Table BIII: Alternative Definitions of Swing States

These tables report the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda C_{it} + \theta LockedSenate_t + \delta C_{it} \times LockedSenate_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level variables denoted in column headings, and C denotes *Swing6* in Panel A, *Swing8* in Panel B, *Swing10* in Panel C, and *PartisanAgainst* in Panel D. The sample consists of firm-year observations during the 1994-2020 time period. All specifications include additional control variables *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Only select coefficients are reported to conserve space. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

Panel A: Defining Swing States with 6% Threshold

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing6	-0.1317*	0.0022	-0.0060	-0.0085
	(0.0721)	(0.0021)	(0.0258)	(0.0085)
Swing6 x LockedSenate	0.2092**	0.0004	0.0500**	0.0194**
	(0.0966)	(0.0031)	(0.0246)	(0.0078)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	124,220	130,035	118,339	80,475
Adjusted R-squared	0.605	0.381	0.490	0.489

Panel B: Defining Swing States with 8% Threshold

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing8	-0.1745**	0.0004	-0.0163	-0.0166*
	(0.0772)	(0.0022)	(0.0238)	(0.0092)
Swing8 x LockedSenate	0.2126**	0.0005	0.0536**	0.0215***
	(0.0960)	(0.0026)	(0.0246)	(0.0065)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	124,220	130,035	118,339	80,475
Adjusted R-squared	0.605	0.381	0.490	0.489

Panel C: Defining Swing States with 10% Threshold

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing10	-0.2352**	-0.0032	-0.0253	-0.0183**
	(0.0903)	(0.0024)	(0.0301)	(0.0082)
Swing10 x LockedSenate	0.1935**	0.0030	0.0605**	0.0248***
	(0.0830)	(0.0025)	(0.0254)	(0.0088)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	124,220	130,035	118,339	80,475
Adjusted R-squared	0.605	0.381	0.490	0.489

Panel D: Defining Swing State using Opposition to Incumbent Party

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
PartisanAgainst	-0.2644***	-0.0071***	-0.0717***	-0.0243***
	(0.0870)	(0.0018)	(0.0236)	(0.0069)
PartisanAgainst x LockedSenate	0.1396**	0.0039**	0.0467***	0.0183***
	(0.0548)	(0.0015)	(0.0135)	(0.0064)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	124,220	130,035	118,339	80,475
Adjusted R-squared	0.605	0.381	0.490	0.490

Table BIV: Time-Invariant Measure of Swing State

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda SwingMode_{it} + \theta LockedSenate_t + \delta SwingMode_{it} \times LockedSenate_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level variables denoted in column headings. The sample consists of firm-year observations during the 1994-2020 time period. All specifications include additional control variables *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Only select coefficients are reported to conserve space. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
SwingMode x LockedSenate	0.1449**	0.0037**	0.0547***	0.0172***
	(0.0695)	(0.0016)	(0.0158)	(0.0049)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	121,844	127,542	116,048	79,154
Adjusted R-squared	0.604	0.380	0.489	0.491

Table BV: Filibuster-Proof Senate Majorities

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta LockedSenate_t + \mu LockedSuperMaj_t + \delta Swing_{it} \times LockedSenate_t + \omega Swing_{it} \times LockedSuperMaj_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level variables denoted in column headings. The sample consists of firm-year observations during the 1994-2020 time period. All specifications include additional control variables: *Democrat*, *Republican*, *MajParty*, *MinParty*, *CommChairTop1*, *CommChairTop3*, and *CommChairTop5*. Only select coefficients are reported to conserve space. Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and double-clustered by state and 4-digit NAICS industry. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing	-0.3509***	-0.0022	-0.0377	-0.0175**
	(0.1165)	(0.0016)	(0.0265)	(0.0071)
Swing x LockedSenate	0.2648**	0.0038*	0.0542**	0.0240***
	(0.1017)	(0.0021)	(0.0235)	(0.0087)
Swing x LockedSuperMaj	0.3315*	0.0022	0.0086	0.0152
	(0.1696)	(0.0027)	(0.0347)	(0.0112)
Firm FE	X	X	X	X
Year FE	X	X	X	X
Observations	124,220	130,035	118,339	80,475
Adjusted R-squared	0.605	0.381	0.490	0.489