Tax Breaks for Swing States?
Political Bargaining, Targeted Policies, and Firm Outcomes[*]

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Abstract

We examine how firms are affected by the political bargaining power of their home regions. Exploiting variation in the strategic importance of swing states stemming from shifting partisan balance in the U.S. Senate, we find that corporate valuations and investments respond positively to increases in regional political influence. We verify the valuation findings using an event study based on the 2021 Georgia runoff election that unexpectedly produced a 50-50 balance in the Senate. We investigate potential policy mechanisms and find that tax incentives constitute the most likely channel through which firms benefit from their home regions' political bargaining power.

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1 Introduction

Private-sector firms are significantly affected by the federal government's policies on spending, trade, regulation, and taxation. In a representative democracy like the United States, policy decisions are reached through a complicated bargaining process involving elected representatives who are accountable to their regional constituents. As a result, firms are exposed to fluctuations in the political bargaining power of their home regions. In a notable recent example, West Virginia coal companies benefited from the actions of Senator Joe Manchin, who leveraged 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 be increasingly affected by shifts in regional political influence.

In this paper, we examine how firms respond to changes in the strategic political bargaining power of their home regions. We identify variation in regional political bargaining power by exploiting the shifting alignment between political partisanship at the local and national levels. Specifically, regions with greater electoral competition between Democrats and Republicans (i.e., "swing" regions) should increase their ability to influence policy when power is evenly divided between the two major parties at the federal level. This is because the ability of swing region legislators to vote against their own party becomes more strategically valuable—i.e., swing votes can be used as a bargaining chip to insert amendments into new legislation—when the partisan balance of the legislature is close to a tipping point. Moreover, political parties aiming to win national elections frequently cater to swing regions in exchange for electoral support that could tip the balance in their favor (Lindbeck and Weibull, 1987; Dixit and Londregan, 1995).[3]

In our empirical analysis, we compare 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 ("balanced Senates") and when one party has consolidated control of the

[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`.

[3] Recent examples of swing regions exerting their influence can be seen in battleground districts receiving higher levels of federally subsidized lending during the COVID-19 crisis (Duchin and Hackney, 2021) and a disproportionate share of green energy subsidies from recent climate legislation (Khatib, 2024).

Senate ("consolidated Senates"). Based on our insight that swing states gain strategic value when political power is evenly divided at the national level, we predict that swing states should increase their bargaining power relative to partisan states when control of the Senate is relatively balanced. To alleviate concerns that regional election results may be endogenous to local economic conditions, we use state-level vote margins from *presidential* elections rather than regional legislative elections to classify swing and partisan states.[4]

The ultimate effect of regional political bargaining power on firms depends on the specific policy channels through which this power is exercised. While prior studies have largely focused on earmark spending as a form of regional favoritism, such one-time transfers are unlikely to spur private-sector investment or hiring given their transient nature. In fact, public-sector earmarks may crowd out private-sector activity, as documented by Cohen, Coval, and Malloy (2011), who find that politically directed government spending can displace private investment. In contrast, longer-lasting policies—such as tax incentives, regulatory relief, and trade protectionism—are more likely to influence firms' forward-looking decisions, including capital investments and R&D expenditures.

We consider how different policy levers available to politically influential senators—government spending, tax incentives, tariffs, and deregulation—affect corporate valuations, investment rates, and pre-tax cash flows. Each of these policies is likely to have distinct implications. For example, tax incentives may temporarily depress pre-tax cash flows by inducing firms to increase their expenditures. Regulatory and trade policies, on the other hand, may bolster cash flows by lowering costs and shielding firms from foreign competition. Given these heterogeneous effects, we aim to distinguish between potential policy mechanisms in our empirical analysis.

We pay special attention to corporate tax breaks, which are typically used to incentivize various types of private sector investments. In recent years, government tax incentive programs have increasingly been applied to specific industries rather than general investments,[5] creating economic winners and losers. Moreover, congressional legislators often insert tax riders into budget legislation for bigger-ticket items relating to military funding, disaster relief, and infrastructure spending. For example, an investigation by the Center for Public Integrity uncovered more than 30 industry-

[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 robustness tests, we examine an alternative proxy for swing senators by measuring presidential votes *against* the party of incumbent senators.

[5] See analysis from the Tax Foundation using Joint Committee on Taxation tax expenditure reports (`https://taxfoundation.org/blog/corporate-tax-breaks-expanded/`).

specific and firm-specific tax credit provisions in one 2018 budget bill.[6] In our empirical analysis, we explicitly examine the effect of regional political leverage on firm-level tax subsidies and effective tax rates.

Overall, our empirical findings provide evidence that firms benefit from increases in the political bargaining power of their home regions. First, we verify that swing states successfully bargain for larger shares of federal financial assistance when the Senate is relatively balanced. We find that assistance directed at swing states indeed increases by over 4% (approximately $2.17 billion for the median state) compared to spending directed at partisan states when the Senate is relatively balanced. This relative increase is most pronounced in direct payments and government loans, which both include subsidies to private-sector firms. We also examine federal subsidies directed at individual firms and find evidence that swing-state firms receive a relative increase in subsidies during balanced Senates. However, this increase is only observed for tax credit subsidies, but not for other types of subsidies such as federal and non-federal grants.

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 shifting government policies. Therefore, we focus mainly on forward-looking measures that capture shifts in expectations about future political 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.

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 balanced, 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 balanced. 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

[6] These include tax breaks for timber companies, biodiesel producers, utility companies, private colleges, and even individual companies such as the Star-Kist tuna canning company. See `https://apps.publicintegrity.org/tax-breaks-the-favored-few`.

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 reversible shifts in regional political power. This potentially stems from the longer lead times to install intangible capital before it can benefit from favorable government policies. Overall, the positive effect on forward-looking valuation and investment suggests that swing-state senators use their elevated influence to pursue policy initiatives with relatively lasting effects, such as tax credit initiatives, rather than pushing for short-term regional spending increases.

We also examine how regional political bargaining power affects firms' operating performance. We find no evidence of an effect on overall revenue, but a negative effect on pre-tax cash flows. This is consistent with the view that tax incentives encourage firms to increase SG&A and R&D expenditures, thereby reducing short-term earnings. These findings align with our broader evidence on firm-level tax subsidies and suggest that, while firms respond to political power by expanding investment, this expansion may temporarily depress operating margins. Importantly, such effects do not reflect declining firm health but rather the expected trade-off between near-term profitability and long-term value creation.

To sharpen our identification of how regional political bargaining power affects firm 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 bargaining power of swing states. Using an event study that excludes all firms based in Georgia, we find that the cumulative abnormal returns of swing-state firms outperformed those of partisan-state firms by 1.5 percentage points immediately after the runoff elections. We also examine long-run return differentials between swing-state and partisan-state firms and find that the divergence in returns becomes more pronounced—almost quadrupling—after the first post-election congressional budget session, when the political influence of swing-state senators became more evident.

We check whether the positive effects we document for valuations and investments translate to higher aggregate output and employment. 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 balanced. 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, stem from policies that stimulate broad-based economic activity, rather than through narrow rent extraction by large, politically-connected firms.

To further evaluate whether our benchmark findings stem from politically connected rent-seeking behavior or broader regional incentives tied to electoral accountability, we perform heterogeneity tests based on firms' corporate political activities. We find no evidence that our results are driven by firms exploiting political connections through lobbying or campaign contributions. We also examine whether changes in policy uncertainty may explain our results. Although uncertainty can depress investment and valuations, we find no systematic differences in regional electoral uncertainty or exposure to aggregate policy uncertainty between swing and partisan states across balanced and consolidated Senates. Moreover, we observe similar directional effects for capital expenditures and R&D investment in our benchmark findings—despite prior research showing that R&D responds positively to uncertainty Atanassov, Julio, and Leng (2024)—further undermining a policy uncertainty explanation. Lastly, heterogeneity analysis reveals no significant difference in our benchmark findings in subsamples of firms and time periods with varying degrees of political risk exposure.

We conduct follow-up tests to uncover the specific policy channels driving our main findings. First, we investigate 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 as well as those targeted at specific firms. We find that swing-state firms experience a relative decrease in effective federal tax rates of 59 basis points (6.1% of the sample mean) during balanced 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.

We further investigate potential policy mechanisms by conducting additional heterogeneity tests. 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), suggesting that local demand spillovers from fiscal multipliers are unlikely to drive our results. Next, we split the sample based on industry exposure to import penetration and find suggestive evidence that the effects are more pronounced in firms from highly exposed industries, consistent with favorable trade policies playing a role in explaining our main findings. However, we find little evidence that changes in import tariffs explain this pattern, leaving targeted subsidies for import-exposed industries as a plausible explanation. Lastly, we examine heterogeneity by industry-level federal regulation and find no significant differences, suggesting that the easing of regulatory burdens is unlikely to account for our main results.

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: influence over policymaking stemming from strategic bargaining between politicians.

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 that 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. Notably, 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.

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 that the effect of regional electoral competition on firms depends on the balance of political control 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, 2024), 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 Conceptual Framework

2.1 Regional political bargaining power in a representative democracy

In a representative democracy, voters do not directly set government policy; instead, they elect legislative representatives who make collective decisions on their behalf. These representatives, chosen by regional electorates, participate in a complex bargaining process in which each representative works to steer policymaking in their preferred direction through negotiation, coalition-building, and strategic compromises, all while balancing their constituents' interests and broader political pressures. Our research focuses on how firms are affected by the shifting balance of power in the U.S. Senate, where legislators navigate competing priorities to shape national policy.

In making collective decisions on public policy, each individual legislative representative's influence is constrained by their limited bargaining power. In this paper, we compare the political bargaining power of regions with varying levels of electoral competition. In the United States, national politics is defined by competition between two dominant parties, with some regions exhibiting relative evenness in electoral support for the Democratic and Republican candidates and other regions being deeply partisan in their support of one of the two parties. A priori, it is unclear whether the partisan balance of a region strengthens or weakens the political bargaining power of its legislative representatives.

On the one hand, politically moderate regions may have weaker bargaining power over policy due to the relatively precarious position of their elected representatives. Incumbent representatives from such regions tend to have shorter tenures, as they are more likely to lose their seats to challengers from the opposing party, and prior research shows that federal earmarks are positively correlated with electoral longevity (Boyle and Matheson, 2009). Shorter tenures also reduce the probability of ascending to powerful congressional committee chairmanships, which prior studies have shown to enhance bargaining power over policy (Cohen et al., 2011; Akey et al., 2021).[7]

Conversely, partisan balance can occasionally grant legislators with strategic leverage in bargaining for their preferred policies. Representatives of moderate regions often hold pivotal votes in passing legislation, which they use to negotiate for riders and amendments on new bills. Moreover,

[7] Greater electoral uncertainty also shortens a senator's *expected* tenure, weakening their ability to trade future votes for present support on their preferred bills (Fox, 2006).

because closely-contested regions play a crucial role in determining national election outcomes, party leaders have strong incentives to cater to these regions ahead of national elections (Dixit and Londregan, 1995, 1996). In this paper, we do not take a position on whether regional bargaining power is increasing or decreasing, in *absolute* terms, in the degree of partisan balance. Rather, we exploit changes in the *relative* strategic bargaining power held by swing states across different political regimes. Before detailing our empirical strategy, we first discuss how legislators' policy objectives are shaped by their political incentives.

2.2 Political incentives and demand for targeted policies

To study how political bargaining shapes regional economic outcomes—particularly for private sector firms—we consider a broad set of economic policies that affect specific regions. While many studies focus on the geographic allocation of redistributive transfers and government spending, we examine a wider array of measures, including taxation, trade, and regulatory policies, which may target specific industries and firms concentrated within a region rather than the region itself. For the remainder of the paper, we refer to this broad set of policies as "targeted policies."

Legislators' policy objectives can be driven by multiple motivations, with their desire to win elections representing a particularly strong motivator. This creates a strong electoral incentive for politicians to support policies that serve their constituents' economic interests by favoring the industries and firms that operate in their home regions.[8] By securing favorable economic policies for their constituents, legislators can enhance home region employment, investment, and economic stability, ultimately strengthening their political standing and increasing their chances of reelection. Notably, Besley and Case (1995) provides a reputation-building model of political behavior in which electoral accountability induces politicians to expend effort toward generating economic surplus for their constituents, and shows empirically that binding term limits, which weaken electoral incentives, are associated with lower regional incomes. Subsequent empirical studies have shown that politicians are held electorally accountable for the economic performance of their home regions across a wide range of settings, including in the U.S. Congress (Mitchell and Willett, 2006; de Benedictis-Kessner and Warshaw, 2020).

[8] Politicians may also be motivated by electoral pressures to advocate for policies that align with the ideological preferences of their constituents, but these policies typically relate to broader issues—for example, reproductive rights, gun control, or the federal deficit—which are less directly tied to regional economic interests.

Elected legislators may also be influenced by the rent-seeking activities of special interest groups. For instance, legislators may seek to curry favor with specific industries or firms in exchange for political donations and lobbying (Wright, 1990), or to secure lucrative private-sector positions after leaving public office (Egerod, 2022). Since our focus is on how political bargaining power benefits a legislator's home region rather than geographically dispersed special interests, our empirical tests are designed to capture the effects of policies driven by electoral incentives. However, firms within a legislator's home region may also attempt to influence policy through making quid pro quo arrangements with their regional representatives. In such cases, it can be unclear whether firms passively benefit from legislators' efforts to serve constituents or actively seek favorable policies through political rent-seeking. To investigate this, we design heterogeneity tests to distinguish between these two channels in our empirical analysis.

2.3 The effect of regional political bargaining power on firms

What is the ultimate effect of regional political bargaining power on firms? The answer likely depends on the specific policy channels being targeted. Many prior studies have examined how regional political power affects the allocation of earmark spending,[9] but earmark provisions inserted into appropriation bills typically represent short-term, often one-time, transfers of government resources. Because firms make investment and hiring decisions based on forward-looking expectations about future economic conditions, such transfers are unlikely to induce firms to expand their investments or hiring. In contrast, long-term policies aimed at lowering taxes, reducing regulatory burdens, and erecting trade barriers to favor a particular region's industries are more likely to encourage expansionary economic activities.

In Table I, we provide a breakdown of how different policy channels are likely to affect various firm outcomes. We consider four potential policy levers available to senators with bargaining power over policymaking: directing government spending toward their home regions, enacting tax incentive programs that benefit home region industries, raising tariffs to protect home region industries from foreign import competition, and reducing regulatory burdens for home region industries. We consider the effects of these policies on corporate valuations, investment rates, and pre-tax cash

[9] See, for example, Lazarus and Steigerwalt (2009), Boyle and Matheson (2009), and Engstrom and Vanberg (2010).

flows.[10]

We predict that policies that change underlying economic conditions—including favorable changes to taxation, tariffs, and regulations—should lead to increases in corporate valuations and investment, driven by the forward-looking reactions of investors and managers. However, the effect of increased government spending and earmarks on these outcomes is more ambiguous. On the one hand, prior research by Cohen et al. (2011) documents that increased government spending directed by powerful politicians to their home regions crowds out private-sector activity. On the other hand, politicians may also use their influence to award long-term government contracts to their home region firms (Brogaard, Denes, and Duchin, 2021), which may have positive effects on valuations and investments.

The effect of regional political bargaining power on firms' pre-tax cash flows also depends on the types of policies targeted. Tax credit programs, for example, can reduce pre-tax cash flows in the short term by incentivizing expenditures on R&D and other investments (Rao, 2016; Agrawal, Rosell, and Simcoe, 2020). In contrast, policies that reduce regulatory burdens or protect home-region firms from import competition are likely to have more positive effects on pre-tax cash flows. For example, Akey et al. (2021) finds that powerful Senate committee chairs provide regulatory relief to their home state banks, resulting in improved operating performance.

As different policies have varying effects on firm outcomes, it is crucial to document the specific policy channels that drive these economic effects. In our empirical analysis, we directly examine tax subsidies and effective tax rates as outcome variables. We also explore other potential mechanisms, including direct government spending, regulation, and trade protectionism, that may drive the effect on firms. Further details on these alternative hypotheses are provided in the following section, where we also outline our empirical strategy.

The types of targeted policies that legislative representatives are likely to pursue depend on their political incentives. Prior research has shown that congressional representatives' election prospects are sensitive to regional unemployment rates (Mitchell and Willett, 2006) and regional income growth (de Benedictis-Kessner and Warshaw, 2020), indicating that electoral motives should incentivize legislators to pursue policies that stimulate economic growth in their home regions. In

[10] We focus on *pre-tax* income, as we also examine corporate income taxes separately to evaluate the tax channel explicitly.

such scenarios, political representatives should find it more effective to target policies with more persistent effects, such as enacting tax incentives targeted at home-region industries. On the other hand, policies with more transient effects, such as appropriations spending on earmarks, may allow politicians to trade political rents for private benefits but have limited effectiveness in stimulating regional economies.

Targeted policies may also vary in implementation time. For example, there may be long delays between when an earmarked infrastructure spending project is approved and it is "shovel ready".[11] In contrast, new tax incentive programs can be implemented relatively quickly through changes to the tax code. Political representatives motivated by electoral concerns face limited time horizons before facing their next election. Therefore, it is in their interest to target expedient policies that can be implemented relatively quickly. On the other hand, politicians motivated by access to private benefits, such as future career opportunities in the private sector, do not necessarily face such time constraints.

We note that weak electoral incentives may explain the crowding out effect documented in Cohen et al. (2011). Given that committee chairmanships are typically awarded based on seniority, congressional chairs are relatively insulated from electoral pressures as revealed by their lengthy tenures in office. Moreover, committee chairs are more likely to be the target of campaign contributions (Micky, Stephen, and Snyder, 2002) and lobbying (Hojnacki and Kimball, 1999), which makes them more beholden to special interests. Therefore, the finding that powerful committee chairs steer earmark spending toward their home regions in a way that crowds out private sector investment may not be generalizable to other legislators who face stronger electoral incentives to promote employment and income growth in their home regions.

3 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

[11] See `https://archive.nytimes.com/thecaucus.blogs.nytimes.com/2010/10/15/obama-lesson-shovel-ready-not-so-ready/`.

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 relatively small, 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.[12]

3.1 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.[13] 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 "balanced" Senate) and periods when it is held by a wide majority (i.e., an "consolidated" Senate), but there is only one brief instance in our entire sample period when a party achieved a filibuster-proof supermajority. During the 111th Congress (2009-2010), the Democratic Party held a 60-40 seat advantage in the Senate for about half a year due to unique circumstances.[14]

[12] 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.

[13] 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.

[14] From July 7, 2009 to February 4, 2010, the Democratic Party possessed a fluctuating supermajority due to a number of factors. On July 7, Senator Al Franken was sworn in, initiating a supermajority for the Democrats. However, on August 25, Senator Ted Kennedy died and was only replaced on September 24, which reduced their seat advantage to 59-40 for that period. From September 24, 2009, to February 4, 2010, the Democrats again held a supermajority in the Senate. On February 4, 2010, Scott Brown, a Republican, was elected and replaced Kennedy's interim successor, ending the Democratic supermajority. We thank an anonymous referee for pointing out this rare filibuster-proof Democratic supermajority.

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.[15] 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 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 procedure provides a clear avenue for legislators to freely exercise their political leverage to insert desired amendments 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 balanced Senates, even under the reconciliation process. Therefore, individual senators or small coalitions can leverage their votes to extract concessions from party leaders during balanced 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 the most relevant one in our analysis.

3.2 Identifying cyclical shifts in regional political bargaining power

To identify shifts in regional political bargaining power, we compare states with differing levels of electoral competition between the Democratic and Republican parties. As discussed in Section 2.1, it is unclear a priori whether the political bargaining power of moderate states (where the partisan support for both parties is relatively balanced) and partisan states (where the partisan support is tilted toward one party) should be higher. Our identification strategy instead exploits predicted shifts in the relative bargaining power of states with varying degrees of partisan balance. Specifically, we posit that states with greater partisan balance—i.e., "swing states"—should increase their

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

bargaining power over targeted policies relative to states with less partisan balance—i.e., "partisan states"—when overall control of the Senate is relatively balanced.

This relative shift in regional political bargaining power between swing states and partisan states is predicated on the idea that swing-state senators have more latitude to vote against their own party. When control of the Senate is relatively balanced, every senator's vote becomes more pivotal in the passage of budget-related bills, but swing-state senators can better leverage their pivotal votes by voting against (or threatening to vote against) party lines. This is because they must appeal to a politically moderate electorate, and therefore are less likely to face reprisals when they defy their party leaders during legislative voting.[16] In contrast, partisan-state senators risk alienating their base and facing primary election challenges if they vote with the opposing party.[17] Therefore, swing-state senators are better positioned to influence whether a bill ultimately passes or not. If they are a member of the majority party, they extract concessions from their own party by holding up the passage of bills. Conversely, they can extract concessions from the opposing party as a member of the minority party by offering bipartisan support on narrow votes.[18]

We note that swing states also hold strategic value in influencing national election outcomes, and this value may increase when control of the Senate is relatively balanced. Given the importance of the 51-seat margin for budget-related bills, political parties have a stronger incentive to tip the Senate in their favor when control of the Senate is balanced. This is because the marginal value of flipping or holding winnable seats (i.e., swing states) rises as control over the Senate's budget-setting powers approaches a tipping point. Consequently, parties are more likely to strategically direct government transfers to swing states during balanced Senates to secure or maintain control over the budget-setting process. Since a strong local economy typically benefits the incumbent party, the majority party controlling the legislative agenda is more likely to favor regions under its

[16] 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/`.

[17] 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`.

[18] While partisan-state senators may occasionally defy their party, swing-state senators are more likely to use balanced Senates as opportunities to bargain for regionally targeted economic policies that benefit their constituents. Lacking a reliable partisan base to win general elections, swing-state senators have greater incentives to "bring home the bacon" to sway swing voters. In contrast, partisan-state senators are more likely to seek ideological concessions appealing to their political base, often focusing on social issues related to the "culture war" or ideological economic issues over the national deficit. For example, during the 2023 hardline Republican revolt against Kevin McCarthy's selection as Speaker of the House, one key issue was imposing limits on overall federal spending.

political control rather than those aligned with the opposition. In our empirical tests, we exploit this dynamic to examine whether our results are driven by decentralized legislative bargaining or centralized strategic decisions of party leaders.

To implement our empirical strategy, we compare firms in swing states and partisan states during balanced and consolidated Senates using the following specification:

$$Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta BalSenate_t + \delta Swing_{it} \times BalSenate_t + X_{it}\beta + \epsilon_{it}, \tag{1}$$

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, $BalSenate_t$ 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.[19] Our coefficient of interest, δ, should capture the increase in the political bargaining power of swing states relative to that of partisan states during balanced Senates relative to consolidated Senates.

We employ a binary measure of state-level partisan balance, *Swing*, to simplify the interpretation of our estimate of δ as the change in relative political bargaining power between swing and partisan states. By using a median cutoff to define swing states, our measure differs from the narrower classification of battleground states commonly emphasized by the political press during elections. In Section 4, we verify that using a median cutoff captures meaningful variation in the likelihood of voting against one's own party—the key source of political bargaining power during balanced Senates in our framework. We also conduct robustness tests using alternative definitions of swing states based on tighter margin cutoffs.

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 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

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

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 as, 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. Targeted 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.[20]

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 (*BalSenate*). 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 elections at the state level while the Senate remains frequently balanced. 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,

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

regardless of whether the Senate is balanced.

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 as 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 balanced 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, as swing states lose bargaining power relative to partisan states in those Senate periods. Nevertheless, we perform dynamic versions of our benchmark tests to ensure that our findings are not driven by secular trends that happen to be centered around changes in Senate balance.

3.3 Hypotheses Development

As discussed in Section 2.3, the effect of regional political bargaining power on firm valuations and investment rates should depend on what types of policies politicians can influence. We consider four potential policy channels: tax credits, protectionist tariffs, regulatory changes, and direct government spending. For outcome variables, we focus mainly on forward-looking firm measures, including market valuation ratios (i.e., Tobin's Q), which should reflect investors' expectations about future political rents, and investment-to-asset ratios (for both tangible and intangible assets), which should reflect managers' expectations about future political rents.[21] We also examine firm performance outcomes, including sales growth and pre-tax income (scaled by assets).[22] Table I summarizes the effects of these different policy levers on firm outcomes and lists prior studies that document the channels through which each policy is expected to influence these outcomes.

Among the various policy levers available to lawmakers, tax policy has the most widespread impact on firms. While some firms may be largely unaffected by federal regulations or foreign competition, and others may not rely on government procurement, all businesses must navigate the tax code. Under the tax policy channel, we should expect the increased bargaining power of swing states during balanced Senates to result in higher valuations and increased investment rates. As firms increase their expenditures to earn newly available tax credits, we expect pre-tax income—but not revenue growth—to decrease. Moreover, we should expect swing-state firms to receive increases in tax subsidies and experience decreases in their effective tax rates.

Under the regulatory and trade policy channels, we expect similar positive effects on valuation and investment rates. However, unlike the tax policy channel—where pre-tax income is expected to decline—we anticipate that pre-tax income will increase as firms benefit from reduced regulatory costs and decreased import competition. While firm-level data on import competition and regulatory costs are unavailable, we differentiate between regulatory and trade policies through heterogeneity tests. Specifically, we estimate Eq. 1 separately for firms in high- versus low-regulation industries and for those in high- versus low-import-penetration-exposure industries. If regulatory and trade policies play a significant role, we should observe the positive effects concentrated in high-regulation and high-import-penetration-exposure industries.

Lastly, our predictions on various firm outcomes are ambiguous under the government spending channel. Given the crowding out effects documented by Cohen et al. (2011), government-funded projects draw resources away from private firms, increase local wages, or create inefficiencies in capital allocation. However, if government spending is directly allocated to firms through contracts

[21] Given that the Senate can shift rapidly between balanced 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.

[22] 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.

or procurement programs, the net impact could be positive. Firms receiving government contracts may experience persistent increases in cash flows and revenues, leading to higher investments and valuations. To check for direct evidence of a government spending channel, we check whether swing states experience relative increases in government contracts and federal grants during balanced Senates.

3.4 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.[23] 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 for both runoff races was tight, well within the margin of error,[24] 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.[25] While the difference between these seat margins does not seem large, a 50-50 Senate split represents a unique situation that amplifies the leverage of its individual members, particularly those from moderate states.[26] Indeed, political media outlets at the time noted that a 50-50 balance would amplify the voices of moderate legislators in the Senate.[27]

[23] 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.

[24] 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/`.

[25] 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.

[26] 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 2001. At the start of the term, the Senate was split 50-50, but in May 2001, Jim Jeffords officially switched parties, and Democrats took control of the Senate with a two-seat margin, 51-49. See `https://www.latimes.com/archives/la-xpm-2001-jun-24-mn-14081-story.html`.

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

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. We exclude all firms located in Georgia to rule out any direct effects the election may have had on firms within the state. Following the suggestion from MacKinlay (1997), we use disaggregated security-level data given that the event occurs on the same day for all firms. We also follow the recent approach of examining within-firm changes in abnormal returns using panel data to estimate the following specification:[28]

$$AR_{it} = \alpha_i + \gamma_t + \sum_{\tau} \pi_\tau Swing_i \times I(\tau)_t + \epsilon_{it} \tag{2}$$

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 $I(\tau)_t$ 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. In these regressions, we double-cluster standard errors by state and date to account for cross-sectional correlations in stock returns.

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.[29] 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 ($\tau = \textit{January 5}$), when the Democratic candidates established a lead in the ballot count, and on the following day ($\tau = \textit{January 6}$), when media outlets called both elections for the Democrats. We should not expect any differences in the days leading up to the election ($\tau < \textit{January 5}$), when media outlets reported the two races as too close to call.

We also examine stock price reactions at longer time horizons by comparing the cross-sectional

[28] See Lins, Servaes, and Tamayo (2017) and Sheng et al. (2024) for examples of stock event studies using within-firm variation in a panel framework.

[29] 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.

difference between the cumulative abnormal returns of swing and partisan state firms. Following Zeume (2017), we estimate a regression model for the cross-section of public firms:

$$CAR_i^N = \alpha_i + \pi Swing_i + \epsilon_i \tag{3}$$

where CAR_i^N is the dependent variable, defined as the sum of daily abnormal returns from 10 trading days prior to the election to N trading days after the election. We examine CAR reactions at the immediate horizon ($N = 1$) and a longer horizon ($N = 60$) . We cluster standard errors by state and, as with the daily returns regressions, we exclude all firms located in Georgia.

We expect the divergence in CAR between swing and partisan states to be more pronounced as the political leverage of swing states became clearer in the weeks following the January run-off election. In particular, the first budget vote of the new congressional term was held on February 4, 2021, when the Senate passed a budget resolution by the narrowest possible margin of 51-50, with Vice President Kamala Harris casting the tie-breaking vote. The vote-a-rama that occurred during the passage of the budget resolution involved 41 amendment votes, the fifth highest in congressional history, with the most significant amendments inserted by a bipartisan group of senators led by Joe Manchin and Susan Collins.[30] On March 5, 2021, the budget was formally passed through the budget reconciliation process, where 37 amendment votes were held during the vote-a-rama stage. These salient events further clarified the political leverage held by swing-state senators for market participants and should, therefore, be reflected in market prices.

4 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 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.

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

4.1 Political data

We obtain data on presidential vote margins from the MIT Election Lab (MIT Election Data 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, is below (swing state) or above (partisan state) 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 classifications.

As discussed in Section 3.2, our data-based definition of swing states is based on median vote thresholds, rather than the narrower categorizations of "battleground states" based on media narratives during presidential elections. Such categorizations are often subjective, vary across media outlets, and are intended to identify states that could plausibly swing the outcome of a presidential election. In the 2020 presidential election, for example, CNN listed several states with high presidential vote margins—including Colorado, New Hampshire, Virginia, Ohio, Iowa, and Minnesota—as battleground states. Colorado, in particular, saw the Democratic Party win by 13.5 percentage points, similar to our median cutoff point.[31]

In our context, political bargaining power arises from senators' capacity to vote against their own party. We demonstrate that our binary definition of swing states captures meaningful variation in voting flexibility by illustrating the relationship between *PresVoteMargin* and the frequency of vote deviations on legislative bills. To this end, we use senator-level roll call vote data from the Congressional Roll-Call Votes Database by Voteview (Lewis, Poole, Rosenthal, Boche, Rudkin, and Sonnet, 2021) and construct *LegVoteDev* to measure the rate at which a senator's vote deviates from their party—that is, the absolute difference between the senator's vote and the median vote of their party, where 'yea' is coded as one and 'nay' as zero.

We provide binned scatterplots illustrating the relationship between *PresVoteMargin* and *LegVoteDev* in Figure I. We see in subfigure (a) that there is an overall negative relationship between state-level presidential vote margins and senator vote defections. In subfigures (b) and (c), we see that this relationship is stronger for budget-related bills, which is relevant for the 50-seat threshold

[31] See `https://www.cnn.com/election/2020/results/president`.

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.

Importantly, subfigures (a) and (b) show that the average vote deviations on the left half of the plot are clearly higher than those on the right half. For example, subfigure (b) suggests that senators from states with below-median presidential vote margins exhibit similar propensities to vote against their own party on budget-related bills, while maintaining higher deviation levels than senators from states with above-median margins.[32] This pattern supports our choice to use a median-cutoff binary measure to capture variation in political bargaining power arising from senators' voting flexibility. Nevertheless, we also examine alternative definitions of swing states based on alternative margin cutoffs in our robustness tests.

We obtain data on the overall balance of the U.S. Senate from the U.S. Senate website.[33] We clean the data to identify which party holds majority control, the number of seats held by each party, and the seat margin between the majority and minority parties. We classify a congressional term as balanced if the controlling party holds a majority of six or fewer seats (i.e., a maximum seat margin of 53–47) and consolidated otherwise. We adopt this six-seat margin threshold because it represents 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 a legislative vote. In our empirical analysis, we also examine alternative thresholds in robustness tests.

We present the time series of overall Senate balance in Table III.[34] Of the fourteen terms in our sample period, the Senate is balanced for seven: the 104th, 107th, 108th, 110th, 112th, 115th, and 116th U.S. Congresses. In Figure B1 of our Internet Appendix, we provide a visualization of the time-series evolution of the *Swing* status of each state, as well as the evolution of the overall Senate balance over our sample period.[35]

As discussed in Section 3.2, our explanatory variable of interest is the interaction between partisan balance at the state level (*Swing*) and national level (*BalSenate*). Because we define *Swing*

[32] This implies the political leverage arising from voting flexibility is not confined to swing states as defined by narrower media narratives—i.e., those with extremely tight presidential vote margins next to the vertical axis.
[33] Available at `https://www.senate.gov/history/partydiv.htm`.
[34] We label the 107th U.S. Congress as "Split" because it experienced four different majorities during its term. All four majorities were balanced, with a maximum seat margin of two.
[35] The Internet Appendix is available at `https://papers.ssrn.com/sol3/papers.cfm?abstract_id=5266763`.

based on within-year median thresholds, there is no mechanical relationship between these variables, and the proportion of swing states and partisan states remains roughly balanced throughout our sample period. Nevertheless, we verify that partisan balance at the state level does not mechanically imply partisan balance at the national level. In subfigure (a) of Figure B2 of our Internet Appendix, we illustrate the shifts between balanced Senates (in gray) and consolidated Senates (in white) over time overlaid with the average of *PresVoteMargin* from the last presidential election across states for each year in our sample.[36] 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 balance. A formal t-test (p-value = 0.65) fails to reject the null hypothesis that *PresVoteMargin* is on average equal between periods of balanced and consolidated Senates.

Lastly, we verify that the overall Senate balance is uncorrelated with overall economic conditions or business cycles. In subfigure (b) of Figure B2 of our Internet Appendix, we plot the annual real GDP growth and the unemployment rate against periods of balanced and consolidated Senates, and find no clear correlation between aggregate economic conditions and Senate balance. Formal t-tests fail to reject the null hypothesis for both real GDP growth (p-value = 0.17) and the unemployment rate (p-value = 0.89).

4.2 State-level spending and economic data

We present summary statistics on the natural logs of annual state-level federal government spending amounts in columns (2) through (6) in 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.

[36] 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.

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.[37] 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.

We also present summary statistics for various state-level economic growth measures (expressed in gross growth rates) in the last rows of Panel A from 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 outcome variables in state-level regressions to check whether targeted policies affect aggregate economic performance and employment, outcomes that voters ultimately care about.

4.3 Firm-level data

In addition to the state-level federal government spending data presented in Panel A of Table II, we also obtain firm-level 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

[37] 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.

years before 2002. The data provides information on the form of each subsidy (e.g., grant, loan, tax credit, etc.), the government agency 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.[38] 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 the first four rows in Panel B of Table II, where we restrict the sample to firms that have ever received a direct subsidy of a particular type. For example, the statistics presented in the first row are based on a sample of firms that have received a tax credit subsidy at least once. The low observation counts indicate that very few firms ever receive any subsidies. Tax credits constitute the most common subsidy type, but we still lose over 90% of observations when restricting the sample to firms that have ever received a tax credit subsidy.

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*, *ROA* 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 return 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 Professor 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

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

window, defined as 10 trading days before the election to 10 trading days after the election. We 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 N as the sum of daily (abnormal) returns over the $[-10, N]$ trading-day interval (i.e., from the beginning of the event window to date N). We examine both short-term ($N = 1$) and long-term ($N = 60$) cumulative returns.

We present summary statistics on daily and cumulative returns in Panel C of Table II. The first three rows report statistics for daily raw returns (*RawRet*), CAPM-adjusted abnormal returns (*AR_CAPM*), and Fama–French three-factor-adjusted abnormal returns (*AR_FF3*) over a [-10, 10] window sample. The remaining rows present summary 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*), calculated over the [-10, 1] and the [-10, 60] windows, respectively.

5 Main Findings

5.1 Effect on federal transfers and subsidies

To check whether swing states' relative bargaining power over federal resource redistribution increases during balanced Senates, we estimate our benchmark specification (i.e., Eq. 1) using state-level federal financial assistance measures as the dependent variables. As discussed in Section 4, we use state-level data on federal spending on contracts, grants, loans, and payments to examine each spending category separately as well as total spending aggregated across all categories.

We present our findings in Table IV, which shows a consistently positive effect on federal assistance as evidenced by the positive coefficient estimates on $Swing \times BalSenate$. In column 1, we show that total assistance from the federal government is approximately 4% higher in swing states compared to partisan states during balanced 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 balanced 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). We 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 specific activities. The statistically insignificant estimate in column 2 suggests that government contracts are relatively unaffected by regional political bargaining power, which may be due to the rigid rules surrounding the awarding of contracts.[39] 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 use firm-level subsidy data from the Subsidy Tracker database to 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 firm sales) as the dependent variable in Table V. In Panel A, we report estimates from the unconditional sample, and in Panel B, we report estimates from the sample of firms that have received a directed subsidy at least once during our sample period. 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 balanced Senates relative to consolidated Senates. The point estimate is small, representing a 0.03 percentage-point increase in tax credit subsidies scaled by sales, which is due to the fact that most firms never receive any directed subsidies. The infrequency of targeted tax subsidies—as well as other subsidy types—is evidenced by the low number of observations for *SubsidyTaxCredit* in Panel B of Table II, where we restrict the sample in each column to firms that have received the tax credit subsidy being examined at least once in our sample. In column 1 of Panel B, we show that the effect on tax credit subsidies scaled by sales is larger, at 0.37 percentage points. Together, these results indicate that firms benefit from increased tax credits when the political bargaining power of their home states is

[39] 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.

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 legislators 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 frequent form of firm-directed federal subsidy.

Overall, our findings indicate that swing states gain greater bargaining power over economic redistribution—measured by loans, direct payments, and corporate tax credit subsidies—during balanced Senates relative to consolidated Senates. At the firm level, we find direct evidence that swing-state firms receive higher tax subsidies during consolidated Senates. We note that additional policy channels may also allow regional political bargaining power to benefit local firms. For example, while the analysis presented in Table V focuses on subsidies directed at individual firms, firms may also benefit indirectly from tax credit programs targeted at regional industries. We explore this potential channel, among others, in greater detail in Section 6.

5.2 Effect on firm outcomes

Having provided evidence that our benchmark specification captures variation in regional political power, we now turn to our main analysis of firm outcomes. As discussed in Section 3.3 and summarized in Table I, the effect of regional political power on firms should depend on the specific policy channels through which that power is exercised. We first focus on forward-looking firm-level valuation and investment measures that are likely to respond quickly to changes in regional political bargaining power. Our findings on firm-level tax subsidies suggest that corporate valuations and investments should respond positively when the political bargaining power of swing states is elevated during balanced Senate terms. Alternatively, federal earmark spending may negatively affect corporate investments through a crowding-out effect, as documented in Cohen et al. (2011).

We present the results of our analysis in Table VI. The first four columns present coefficient estimates in the absence of control variables. Across all four columns, we observe a positive coefficient on $Swing \times BalSenate$, 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 balanced Senates relative to consolidated Senates. This suggests that shifts in regional political bargaining power are quickly incorporated into market prices as investors adjust their expectations about impending targeted policies. 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 $Swing \times BalSenate$ 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 modest, 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. One possible explanation for the weaker effect on capital expenditures is that firms may be more hesitant to undertake committal investments in physical capital in response to favorable targeted policies due to the risk of future policy reversals.[40] In particular, physical capital generally takes longer to install than intangible capital (Hall and Hayashi, 1989), and firms may be reluctant to commit to long-term tangible projects when their region's political influence may dissipate by the next national election.

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 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), and *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

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

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.[41] We observe that the coefficient estimates for *Swing* $\times$ *BalSenate* are nearly identical regardless of whether we include controls or not.

Overall, these findings indicate that increases in regional political bargaining power lead to higher corporate valuations and investments. As discussed in Section 3.3, this is consistent with policies that have persistent effects on local economic conditions—–such as the introduction of new tax credits, favorable regulatory changes, and competition-reducing import tariffs—rather than one-time spending earmarks. This contrasts with the findings of Cohen et al. (2011), which suggest that political power results in higher government spending that crowds out private sector investments. In Section 6, we examine how this difference can be explained by the distinct policy mechanisms underlying our setting and theirs, and how these differences are shaped by contrasting political incentives.

Next, we examine measures of firms' operating performance as dependent variables in our benchmark specification. We present our findings in Table VII, where the effect on *SaleGr* (sales growth) is provided in column 1. If firms benefit from direct windfalls from federal government transfers when their home region gains political bargaining power, then we should expect to find a positive effect on revenues. However, the estimate presented in column 1 shows that the effect on sales growth (*SaleGr*) is positive but insignificant. This is consistent with our prior null findings on government contracts, the most immediate channel through which federal spending can positively affect firms' revenues.

We also examine the effect of regional political bargaining power on measures of operating income, including *ROA* (pre-tax income scaled by lagged assets) and *Profitability* (pre-tax income scaled by sales). As discussed in Section 3.3, the predicted effect on pre-tax income can vary depending on the policies that politicians pursue. For example, policies that reduce regulatory costs or import competition may lead to higher profits, whereas new tax incentives may have the opposite effect by encouraging firms to increase expenditures. In particular, our findings on *SG&A*

[41] 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, sources the above list of influential committees from Edwards and Stewart III (2006).

and *R&D* expenditures indicate higher operational spending, which may negatively affect operating performance in the short term. Indeed, the estimates in columns 2 and 3 show a negative effect on *ROA* and *Profitability*, with the estimate in column 2 for *ROA* being statistically significant. This is consistent with a tax incentive channel, aligning with our findings on firm-level tax subsidies.

We caveat our findings on firms' operating performance because, unlike valuations and investments that can react quickly based on changes in expectations about political rents, revenues and profits may take longer to respond—particularly if they are not directly impacted by windfall revenues from government contracts. For example, while tax incentive programs may eventually lead to higher future revenues and profits by stimulating expansion, such long-run effects are difficult to detect in our setting due to frequent shifts between balanced and consolidated Senates.

In the final column of Table VII, we examine the effect on firm leverage, based on the predicted relationship between leverage and expected future cash flows. According to the trade-off theory of capital structure, higher expected earnings should encourage firms to increase leverage to benefit from greater interest tax shields and lower financial distress costs, while lower future taxes should prompt firms to reduce leverage due to diminished tax shield benefits. The estimate in column 4 indicates a negative effect on *Leverage* (long-term debt scaled by total assets), though it is only weakly significant. This finding, like our other results, aligns with the tax incentive channel, which we explore in greater detail in Section 6.

5.3 Baseline difference between swing and partisan states

We note that, for many of our firm-level findings presented in Table VI, the coefficient estimate for *Swing* is negative and statistically significant. This indicates that, during periods of consolidated Senates, firms experience lower valuations and reduce their investments when the partisan support for Republicans and Democrats becomes more balanced in their home states. This pattern suggests that swing states may face a baseline political disadvantage when the Senate is consolidated.

A potential explanation for this baseline difference between swing states and partisan states lies in the electoral vulnerability of swing-state legislators, As discussed in Section 2.1, incumbents from swing states are less likely to survive election challenges and, therefore, are less likely to ascend to key positions on influential congressional committees that confer significant political bargaining power.[42] Moreover, having a less secure Senate seat limits swing state senators' ability to reciprocate political favors in the future, and hence reduces their colleagues' willingness to support their legislative proposals. Lastly, the unpredictability of competitive elections results in uncertainty over political representation, and prior research has documented that political uncertainty dampens corporate 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 Senate seat election margins as the dependent variable and present the findings in Table B1 of our Internet Appendix. 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 B1, that Senate election margins are wider in states with wider margins in the previous presidential election. These tests confirm that senators from swing states are, indeed, more vulnerable.

We acknowledge that the negative coefficient estimates for *Swing* in Table B1 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* $\times$ *BalSenate* 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 B1 that swing-state elections do not become less competitive during balanced 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 balanced Senates.

5.4 Dynamics

We examine the dynamics underlying our benchmark results by conducting year-by-year comparisons between swing-state and partisan-state firms in the years surrounding shifts in Senate balance. As discussed in Section 3.2, our specification treats consolidated Senate terms as "baseline" periods

[42] In our sample, the tenure of swing-state senators is, on average, 3.85 years shorter than that of partisan-state senators.

against which balanced Senate terms are compared. However, consolidated Senates do not constitute a true "pre-treatment" benchmark period, as swing-state firms should experience a relative *loss* of regional political influence during consolidated Senate terms. Because the resulting effects on valuations and investment may vary over time within a consolidated Senate period, we should not necessarily expect parallel trends to hold. Nevertheless, comparing the dynamics of swing-state and partisan-state firms allows us to check whether our benchmark findings are driven by secular 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 balanced 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, Table III shows that both the 2000 and 2016 elections were preceded by four years of a consolidated Senate and followed by four years of a balanced 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 nearest reference elections. Specifically, we estimate the following regression:

$$Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \Sigma_{\tau=-3}^{3} \delta_\tau Swing_{it} \times I(\tau)_t + X_{it}\beta + \epsilon_{it}, \tag{4}$$

where $I(\tau)_t$ is a dummy variable indicating that year t is τ years away from the nearest focal election (in 2000 or 2016). For example, $I(1)_t$ would take on a value of one for $t \in (2001, 2017)$ and $I(-1)_t$ would take on a value of one for $t \in (1999, 2015)$. We examine dynamics up to three years before and after the focal election, as additional dynamic interaction terms would capture the effects of the previous elections (i.e., 1996 and 2012) or the following elections (i.e., 2004 and 2020).

We estimate Eq. 4 using a sample that spans from 1997 (three years before the 2000 election) to 2019 (three years after the 2016 election). The years between these two election windows serve as the baseline against which our dynamic effects are estimated, as $I(\tau)_t = 0$ for these years. This baseline period includes two congressional terms with balanced Senates—the 109th (2005–2006) and 111th (2009–2010) Congress—and two terms with consolidated Senates—the 110th (2007–2008) and 112th (2011–2012) Congress. Given the relatively even distribution of balanced and consolidated

Senates in the baseline period, we interpret the δ_τ coefficients in Eq. 4 as capturing, relative to an "average baseline" of swing state political leverage, the downside for swing states of losing political leverage during consolidated Senates (for $\tau < 0$), and the upside of gaining political leverage during balanced Senates (for $\tau > 0$).

Figure II illustrates the coefficients on the dynamic interaction terms $Swing \times I(\tau)_t$ in our dynamic specification using the firm-level outcomes that we examined in Section 5.2. Subfigure (a) shows that the difference in *Tobin's Q* between swing-state and partisan-state firms decreases during consolidated Senates and increases during balanced Senates, although the estimated decrease during consolidated Senates is just under the threshold of statistical significance. Importantly, year zero (i.e., the election year) marks a clear inflection point where swing-state firms increase in their valuation relative to partisan-state firms. This should alleviate concerns that our baseline findings are driven by long-run secular trends that happen to be centered around changes in Senate balance.

Subfigures (b) through (d) illustrate the dynamic effects on various firm investment measures. In all subfigures, year zero marks the inflection point at which swing-state firms' investment rates begin to rise relative to those of partisan-state firms. Notably, subfigure (b) shows positive coefficients during the pre-election period at $\tau = -3$ and $\tau = -2$, in contrast to the other subfigures. Although these estimates have limited statistical significance, they may help explain the weaker baseline findings for capital expenditures. In particular, investment in physical capital may be slow to react to changes in political regimes due to the option value of waiting for policy uncertainty to be resolved.[43] The positive pre-election estimates in subfigure (b) may therefore reflect delayed reactions to favorable targeted policies enacted during the *last* balanced Senate term preceding the focal elections in 2000 and 2016 (i.e., the 1994–1995 and 2010–2011 congressional terms).[44] However, given the weak statistical significance of these estimates, this interpretation remains speculative.

The last four subplots of Figure II further show no evidence of a pre-election trend in sales growth, return on assets, profitability, and leverage. This should further alleviate concerns that

[43] For example, Clark and Sichel (1993) finds that the response of demand for equipment capital to tax incentive programs is delayed by at least one year.

[44] In general, the effects shown in the dynamic plots appear larger than our baseline estimates. This likely reflects the fact that the baseline estimate is averaged across all shifts between balanced and consolidated Senates, including shorter two-year oscillations. In such cases, delays in the effect of regional political bargaining power on firms are more likely to bias the baseline estimate downward.

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 balanced.

We note that the sharp increase at year zero in subfigures (a) through (d) is consistent with the notion that forward-looking market prices and investment decisions quickly 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 Democratic and Republican parties.[45] Therefore, the immediate 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 Senate balance to analyze higher-frequency stock returns data around a particular election.

5.5 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 3.4. The election outcome shifted the Senate from a potential 52–48 Republican majority to an even 50–50 split, thereby elevating the relative political bargaining power of swing-state senators. If investors anticipated that this shift would benefit firms located in swing states, we should expect to observe higher returns for those firms relative to firms in partisan states. As discussed earlier, we exclude Georgia from the analysis to avoid capturing the direct effects of the election on local firms.

We first examine the dynamics of daily returns and abnormal returns by estimating Eq. 2, and present our findings in Table VIII. The estimates, which are visually illustrated in Figure III, show that raw daily returns and daily abnormal returns of swing-state and partisan-state firms do not diverge in the 5 days leading up to the January 5 election. Subsequently, swing-state firms outperformed partisan-state firms by a statistically significant 73 basis points on January 5, when 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. These coefficient

[45] See `https://www.nytimes.com/interactive/2016/upshot/senate-election-forecast.html`.

estimates are unchanged if we examine raw or abnormal returns and are comparable in magnitude to estimates from prior political event studies.[46]

Figure III shows that the divergence in daily returns was most pronounced on January 6, when major media outlets officially confirmed the victories of the Democratic candidates. However, this was followed by a minor reversal on January 7 and 8, which may be attributed to the U.S. Capitol attack on the afternoon of January 6, when supporters of President Trump stormed the Capitol building in protest of the 2020 presidential election results. Although the overall market did not appear to be significantly affected by the attack,[47] concerns about potential civil unrest in swing states where the presidential vote was close could have negatively impacted firms in those regions in the following days. We observe evidence of a rebound on January 9 and 10, as it became evident that the Capitol attack would not have major economic consequences.

To examine whether the run-off election led to a lasting divergence in equity valuations, we perform a cross-sectional comparison of cumulative abnormal returns (CARs) for swing-state and partisan-state firms. We present our findings in Table IX. The first three columns report estimates of short-term CARs, calculated by summing daily (abnormal) returns from 10 days before to 1 day after the election. The last three columns report estimates of long-term CARs, based on daily (abnormal) returns from 10 days before to 60 days after the election. We observe a significant positive difference at both time horizons, with a 1.5 percentage point difference using the short-term window and a 5.9 percentage point difference using the long-term window. These estimates remain robust regardless of whether we use raw returns (columns 1 and 4), adjust for market risk (columns 2 and 5), or adjust for the market, size, and value factors (columns 3 and 6).

The larger effects observed for long-term CARs suggest that financial markets did not fully appreciate the shift in swing states' political bargaining power immediately following the run-off election. As discussed in Section 3.4, two key events likely clarified the elevated influence of swing states in a 50–50 Senate to market participants. On February 4, the Senate narrowly passed a budget resolution by a 51–50 vote, with Vice President Kamala Harris casting the tie-

[46] 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 abnormal returns of -1.68% for firms located in the hometown of recently deceased politicians.

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

breaking vote. The resolution's passage included 41 amendment votes—the fifth-highest total in congressional history—during the vote-a-rama process, where a bipartisan group of senators led by Joe Manchin and Susan Collins exercised their political leverage by successfully inserting key amendments.[48] A month later, on March 5, the full budget was passed through the reconciliation process, again involving a high volume of amendments, with 37 votes cast—the eighth-highest total in congressional history—during the vote-a-rama process.

In Figure IV, we plot the evolution of CARs for swing-state and partisan-state firms at different time horizons. The first three subfigures display CARs separately for swing-state and partisan-state firms, while the last three subfigures show the mean difference in CARs between the two groups, along with standard error bands. We observe that the short-term divergence immediately following the election is modest, but the divergence becomes more pronounced in the weeks that follow. Specifically, the long-term divergence begins after the budget resolution on February 4 and peaks around the budget reconciliation on March 5. This pattern is consistent with the market gradually recognizing the increased political leverage of swing states during the negotiation of the budget through the Senate.

We note that the March 2021 budget laid the groundwork for the Inflation Reduction Act of 2022,[49] the centerpiece of President Biden's industrial policy initiative, which was later found to have directed a significant share of green energy subsidies to swing states. Specifically, over half of the $63 billion in green energy subsidies went to just seven states—Pennsylvania, Arizona, Georgia, Michigan, Nevada, North Carolina, and Wisconsin—after moderate Senator Democrats, including Joe Manchin and Kyrsten Sinema, pushed to narrow President Biden's broader policy proposals into a more targeted package.[50]

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

[49] The March 2021 budget included a major COVID-19 relief component and served as the foundation for President Joe Biden's Build Back Better Plan, a legislative framework encompassing pandemic response, infrastructure investment, and social policy initiatives. While the Build Back Better Plan passed the Democratic-controlled House, it faced resistance from moderate Senate Democrats. These negotiations ultimately produced the more targeted Inflation Reduction Act of 2022.

[50] See `https://www.theguardian.com/us-news/2024/sep/24/biden-harris-inflation-reduction-act-swing-states-election`.

6 Mechanisms

6.1 Political mechanisms

We explore the potential political mechanisms that underpin our findings. First, we present evidence that our findings are driven by firms passively benefiting from the electoral accountability of their home state senators rather than the rent-seeking behavior of politically connected firms. Then, we show that our findings are unlikely to be driven by differential exposure to political uncertainty. Lastly, we show that our findings are attenuated in cases when we expect individual senators' bargaining power to be limited by centralized party discipline.

6.1.1 Electoral accountability and state-level economic performance

As discussed in Section 2.2, incumbent legislators have an incentive to serve the economic interests of their constituents to increase their re-election prospects. Given prior findings showing that incumbents benefit from higher employment (Mitchell and Willett, 2006) and higher income growth (de Benedictis-Kessner and Warshaw, 2020), we should expect that swing state senators should use their elevated political bargaining power during balanced Senates to push for policies that stimulate overall employment and economic growth in their home states. Therefore, we estimate our benchmark specification using state-level employment and output outcomes to check whether the positive effects we document at the firm level is reflected in the broader regional economy.

In Panel A of Table X, we present the results of estimating our benchmark specification at the state level using log GDP ($lnGDP$), log private-sector employment ($lnEmp$), log total private-sector wages ($lnWages$), and log private-sector establishment counts ($lnEstabs$) as the dependent variables. We focus on private-sector measures to ensure our estimates do not capture any mechanical effects on public sector activity. The estimates indicate increases in GDP (1.64%), employment (0.93%), and total wages (1.54%) for swing states relative to partisan states during balanced Senates relative to unbalanced Senates. Although the estimates are moderate in economic magnitude and statistical significance, they are consistent with politicians being motivated by electoral incentives to stimulate the economies of their home regions.[51]

[51] We caution that one cannot directly infer estimates of fiscal multipliers by comparing the estimates from these analyses to those from Table IV, as politicians can wield their political leverage via other channels to benefit their home regions.

However, these positive stimulus effects—as well as our earlier firm-level findings—may also be driven by rent seeking activity by politically connected firms, given the extensive literature documenting how firms benefit from political connections.[52] Our firm-level analysis in particular focuses on publicly-traded companies that are relatively likely to have political connections through having greater resources available for political donations and lobbying (Kerr, Lincoln, and Mishra, 2014).

To evaluate the possibility that our findings reflect the results of rent-seeking by large connected firms, we examine the effect on private-sector 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.[53] We 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 our findings are largely driven by rent-seeking activities from 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 X, which shows that the positive employment effect is not confined to the largest firms. This indicates that the economic benefits we document are broadly distributed across firms of various sizes, suggesting our findings are unlikely to be driven by corporate rent seeking. If anything, the increase in employment is larger for smaller firms, as reflected in the first two columns, where estimates are higher and statistically more significant than in the last two columns. One possible explanation for this is that larger firms are more likely to have pre-existing connections to powerful politicians—established through lobbying or campaign contributions—and are therefore less dependent on their local political representatives. We explore this possibility in more detail in the following section.

[52] 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 et al., 2021)

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

6.1.2 Political connections and quid pro quo arrangements

We perform additional heterogeneity tests to further evaluate the role of rent-seeking behavior by politically connected firms in explaining our findings. If our results are driven by politicians allocating political rents toward corporate special interests, then our firm-level findings should be more pronounced for firms that engage in political lobbying activities or make campaign contributions. To this end, we collect firm-level lobbying data from the LobbyView database provided by Kim (2018) to identify lobbying firms and firm-level campaign contributions data from opensecrets.org to identify contributing firms. We present the results of our heterogeneity tests in Table XI.

In Panel A, we present the results of estimating our benchmark firm-level regressions with additional interaction terms involving *Lobbied*, a dummy variable indicating whether a firm lobbied the U.S. Senate in a given year. Across various dependent variable outcomes, the coefficient on the triple interaction term is negative, although it is not always statistically significant. In Panel B, we present the findings from a similar set of regressions that include additional interaction terms involving *Contributed*, a dummy variable indicating whether a firm contributed to Senate candidates through a political action committee (PAC) in a given congressional term. Similar to Panel A, we observe consistently negative triple interaction coefficient estimates.

Both sets of findings suggest that the positive effect of political bargaining power on corporate valuations and investments is attenuated for firms that lobby or make PAC contributions. This indicates that the positive effect we document on corporate valuations and investments is unlikely to be driven by rent-seeking behavior by politically connected firms. As discussed in the previous section, this likely stems from larger, politically connected firms being less reliant on the bargaining power of their home state senators when compared with smaller, less connected firms. This is because large, politically active firms typically establish relationships with many politicians, not just those representing their home state.[54]

Do firms calibrate their political lobbying and contribution activity to respond to fluctuations in politicians' bargaining power? To answer this question, we estimate our benchmark specification (Eq. 1) with measures of lobbying and PAC contributions as the dependent variable. We present

[54] Firms that contribute through corporate PACs contribute, on average, to 13.9 legislators across 5.7 states per term in our sample. Over the entire sample, the average contributing firm contributes to 62.3 candidates across 25.6 states. This limits our ability to design empirical tests that compare firms that contribute only to swing-state candidates against those that contribute only to partisan-state candidates.

the results in Table B2 of our Internet Appendix, where we examine both the extensive margin (columns 1 and 3) and intensive margin (columns 2 and 4). Columns 1 and 3 show no evidence that firms become more likely to engage in lobbying or make political contributions when their home state gains political bargaining power.[55] Columns 2 and 4 show no evidence that politically active firms increase their lobbying or contributions in response to increases in home state political bargaining power.[56]

Overall, there is little evidence that our findings can be explained by the active rent-seeking behavior of politically connected firms. In the following section, we evaluate the possibility that our findings may be explained by firms reacting to changes in political uncertainty.

6.1.3 Exposure to policy uncertainty

There is a well-established literature showing that political uncertainty can negatively affect corporate activity (Pástor and Veronesi, 2013; Çolak et al., 2017) and investment (Julio and Yook, 2012; Gulen and Ion, 2016; Jens, 2017). This raises the possibility that the increased investment and valuations we document for swing-state firms during balanced Senates are driven by reductions in political uncertainty. In Section 5.3, we showed that Senate elections in swing states do not become more or less competitive during balanced Senates relative to during consolidated Senates, mitigating concerns that changes in regional electoral uncertainty drive our findings.

However, we consider the possibility that swing states and partisan states have different exposures to aggregate policy uncertainty that varies between balanced and consolidated Senates. Prior research by Duquerroy (2019) finds that, at the state level, divided governments—where different parties control the executive and legislative branches—experience lower policy uncertainty because passing legislation becomes more difficult. Given the similarities between divided governments and balanced Senates in constraining the majority party's legislative agenda, we evaluate whether our findings could be driven by swing states being more exposed to policy uncertainty and overall policy uncertainty declining during balanced Senates.

First, we note that the effects we document on investment in intangible capital, particularly

[55] This may be partially explained by the fact that very few firms engage in political lobbying or make political contributions (Tullock, 1989).

[56] The marginally negative estimate in column 2 suggests the opposite, which may be explained by a substitutability channel—i.e., firms do not need to lobby as much when their home state gains political bargaining power.

R&D expenditures, are inconsistent with a policy uncertainty explanation. Specifically, Atanassov et al. (2024) shows that, unlike capital investment, R&D expenditures respond positively to increases in policy uncertainty. This is because R&D spending creates new real investment options—whereas capital expenditures represent the exercise of existing investment options—making R&D more valuable as uncertainty rises in models of investment under uncertainty. However, we find the same directional effects on valuations, capital expenditures, and R&D expenditures, contradicting the expected response to policy uncertainty.

Second, we examine whether policy uncertainty differs between periods of balanced and consolidated Senates, similar to the differences observed between unified and divided governments. In Figure B3 of our Internet Appendix, we present a time-series plot of the Economic Policy Uncertainty (EPU) index developed by Baker, Bloom, and Davis (2016), which has been shown to predict decreases in investment, output, and employment. There is little visual evidence that balanced Senates—shaded in grey—are associated with lower EPU index scores. A formal t-test comparing the EPU index during balanced and consolidated Senates suggests that, if anything, the EPU index is slightly higher during consolidated Senates. However, this difference is statistically weak, with a p-value of 0.071.

Next, we estimate our baseline specification with additional interaction terms involving *UnifiedGov*, a dummy variable indicating whether the same party controls the presidency and both branches of Congress. We present the results in Table B3 of our Internet Appendix. First, we see that the coefficient estimates on *Swing* × *UnifiedGov* are positive and insignificant, indicating that swing state firms do not perform worse than partisan state firms under unified governments, when policy uncertainty should be elevated according to Duquerroy (2019). Second, we see that the triple interaction coefficient estimates on *Swing* × *Unified* × *BalSenate* are negative and insignificant, which indicates that our benchmark findings do not differ between periods of unified and divided government. These findings further suggest that our findings are not driven by differential exposure to aggregate policy uncertainty.

Lastly, we perform heterogeneity tests based on *PRisk*, a widely used firm-level measure of political risk exposure from Hassan, Hollander, Van Lent, and Tahoun (2019).[57] If our findings are

[57] Hassan et al. use data from quarterly earnings conference calls and computational linguistic techniques to construct *PRisk*. The data they provide is available only starting in 2002, and we backfill to the beginning of our sample period by taking the earliest *PRisk* for a particular firm and making it the *PRisk* measure for all prior years.

explained by exposure to policy uncertainty, they should be especially pronounced for firms with high *PRisk* exposure. In Table B4 of our Internet Appendix, we present the results of estimating our baseline specification with additional interaction terms involving *PRisk*. From the insignificant triple interaction coefficient estimates, we observe that there is no evidence of our benchmark findings being more pronounced in high *PRisk* firms.

6.1.4 The role of party discipline

To provide further evidence that our findings are driven by regional political bargaining power, we examine situations where decentralized political bargaining power may be limited by the ability of party leaders to discipline their members. In particular, the Republican Party has long been regarded as more disciplined and unified than the Democratic Party (Mayer and Polsby, 2018),[58] and empirical evidence suggests that Republican senators have become more ideologically partisan than their Democratic counterparts over the past several decades (Lewis, 2018). This suggests that Republican senators are generally less flexible in leveraging their votes to bargain for targeted policies.

We explore this partisan difference by estimating our baseline specification with additional interaction terms involving *Republican*, a dummy variable indicating whether a state's senators are members of the Republican party. We present the results of this heterogeneity analysis in Panel A of Table B5 of our Internet Appendix, where the triple interaction term $Swing \times Republican \times BalSenate$ shows suggestive evidence that our baseline findings are attenuated in Republican-senator states. Specifically, the estimates are mostly negative, although only statistically significant in column 3 for SG&A expenditures, which is consistent with the idea that Republican politicians have less credibility in voting against their own party.

Next, we check whether our findings are different for states with senators who belong to the majority party or the minority party. If our findings are driven by decentralized bargaining between legislators, then we should expect our baseline effects to be weaker for majority-senator states. This is because the majority party can instill greater discipline in its members. For example, the Senate majority leader exercises significant control over what bills reach the floor and can use the threat of blocking bills or nominations to pressure party members to cooperate with the rest of the party.

[58] See `https://www.politico.com/story/2007/01/the-difference-between-the-ds-and-the-rs-002417`.

Moreover, the majority party can threaten to relegate uncooperative members to an unimportant committee or strip them of a committee chairmanship to enforce loyalty.[59] On the other hand, if our findings are driven by the centralized decisions of party leaders to pursue policies that benefit incumbent members of their own parties, we should expect our baseline effects to be stronger for majority-senator states. This is because policies that stimulate the local economy tend to benefit incumbent candidates in elections, and the party that controls policy should strategically set policies that benefit its own incumbents rather than opposition-party incumbents.

We estimate our benchmark tests with additional interaction terms involving *MajPty*, a dummy variable indicating whether a firm is located in a state represented by senators from the Senate majority party, and present the findings in Panel B of Table B5 of our Internet Appendix. The negative triple interaction coefficient estimates indicate that our benchmark effects are weaker for states with majority-party senators.[60] This suggests that our findings are unlikely to be driven by the party leaders strategically allocating rents toward swing states during balanced Senates. Instead, it aligns with the interpretation of minority-party senators, being less constrained by their party leaders, providing bipartisan support to the majority party in exchange for targeted policies that benefit their constituents.

6.2 Policy mechanisms

Next, 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, and trade protectionism.

6.2.1 Industry tax incentives

Our findings on firm-specific subsidies presented in Panel B of Table V indicate that firms in swing states benefit from favorable tax policies when the Senate is relatively balanced. However, tax subsidies are rarely directed at individual firms and are much more commonly targeted at broader industries. Therefore, our findings on firm-targeted subsidies may understate the importance of

[59] For example, Democratic Majority Leader Harry Reid threatened Senator Joe Lieberman with the stripping of his chairmanship of the Homeland Security Committee to push him to continue voting with the Democrats following Lieberman's change from a Democrat to an independent candidate.

[60] We include only states where both senators are members of the majority party or minority party. This accounts for the smaller sample size in Panel B.

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 industries clustered in their home regions.[61]

To better understand how regional political bargaining power affects corporate taxes, we examine 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 XII. 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 to only be the case when examining federal tax rates and not state or foreign tax rates.

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 62 basis points (6.5% relative to the sample mean) during balanced 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 \$6.6 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 null findings should alleviate concerns that confounding factors, such as diverging trends in tax avoidance strategies between swing states and partisan states, drive our federal tax rate results.

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* × *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 XII. 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 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 II

[61] 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`.

(while dropping *Industry* $\times$ *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 that 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.

6.2.2 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 IV, 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 B6 of our Internet Appendix.[62] We see that across all columns, the positive

[62] Note that our sample is smaller for this analysis because some firms do not belong to either a tradable or non-tradable

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 statistically significant, 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.

6.2.3 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 policy measures to prevent jobs in his home state from moving overseas when courted by President Trump to support his Supreme Court nominee in 2017.[63] We investigate whether swing-state senators use their political leverage to protect firms in their home states that are exposed to foreign import competition. This protection can come in the form of subsidies directed toward domestic producers—in Senator Donnelly's case, he was seeking tax credits that would reward companies that keep jobs in the United States—or

sector based on Mian and Sufi (2014).

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

higher import tariffs aimed at limiting competition from foreign imports.

Using U.S. import data from Professor Peter Schott's website,[64] 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 B7 of our Internet Appendix. The positive coefficient on the triple interaction term, $Swing \times HighIPR \times BalSenate$, 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.

We also consider whether import tariffs may explain the more pronounced effects we detect for industries more exposed to import penetration. As discussed in Section 3.3, we should expect trade-exposed firms to benefit from increased tariffs by insulating them from foreign competition. However, this should increase firms' pricing power and consequently their profits, but we find a negative effect on pre-tax income (*ROA*) in our empirical analysis, which suggests that tariffs do not play a primary role in driving our benchmark findings.

To evaluate the effect of political bargaining power on import tariff rates and import volumes, we examine how tariff rates and import penetration vary across swing states and partisan states between balanced and consolidated Senates. Because we do not possess data on tariffs at the state level, we transform industry-level import tariff rates provided by Feenstra, Romalis, and Schott (2002) to state-level measures by taking the weighted average tariff rate across all industries for each state, where we weight each industry by the share of employees within a state for that particular industry. We perform a similar transformation to construct a state-level measure of import volume using our industry-level import data. We then estimate our baseline specification at the state level with tariff rates (*TariffRate*) and import volume growth (*ImportGr*) as the dependent variables.

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

We present our findings in Table B8 of our Internet Appendix, which shows no evidence of any effect on import tariff rates or volume growth.

Taken together with our negative findings on corporate cash flows, our findings suggest that changes to import tariffs are unlikely to explain our findings. This is not altogether surprising, as tariff rates change relatively infrequently, as they typically occur around the establishment or renegotiation of trade agreements or during trade disputes. Rather, our findings are likely driven by special subsidies directed at industries facing foreign competition. These subsidies may come in the form of tax credits, but they can also involve direct payments occasionally awarded to firms and industries deemed strategically important.[65]

6.2.4 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 I and discussed in Section 4, swing-state senators appear less willing to vote against their own party on regulation-related bills, suggesting they wield less bargaining power 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 B9 of our Internet Appendix. We see that the coefficient on the triple interaction term, *Swing* $\times$ *Regulated* $\times$

[65] For example, the U.S. steel industry occasionally receives grants as a part of special business development programs. One recent example involves U.S. Steel receiving a large grant to fund a training center in Pennsylvania. See `https://investors.ussteel.com/news-events/news-releases/detail/707/u-s-steel-to-fund-training-center-with`.

BalSenate, is insignificant across all specifications. This suggests that swing-state firms do not benefit from favorable regulatory policies during balanced Senates, consistent with the reasons we have outlined above.

6.2.5 Stimulus vs. crowding out

We note that the primary mechanism for our results being tax incentives rather than direct government spending may explain why we find a positive economic stimulus effect in our setting in contrast to the crowding out effects documented in Cohen et al. (2011). Specifically, Cohen et al. find that powerful congressional chairs direct earmark spending toward their home regions, which results in decreases in private sector investment.[66] As discussed in Section 3.3, the effect of targeted government spending on firms can be negative, as government-funded projects may draw resources away from private firms, increase local wages, or create inefficiencies in capital allocation. Indeed, Kim and Nguyen (2020) uses changes to the allocation of federal funds based on population count formulas to also show a crowding out effect. On the other hand, the effect of regionally targeted corporate tax credits should have a more unambiguous positive effect on that region's firms.

One possible explanation for the different policy mechanisms underlying our findings and those of Cohen et al. (2011) is the difference in the electoral incentives of strategically pivotal swing politicians and formally powerful congressional chairs. While Cohen et al. (2011) focus on powerful committee chairs who are typically long-tenured and electorally secure, we focus on electorally vulnerable swing-state senators. Facing stronger electoral pressures, the swing-state senators in our setting are more likely to be responsive to the local economic conditions in their home regions, and therefore more likely to pursue policies that benefit their home-region firms. In Besley and Case's reputational model, for example, electoral incentives are at their minimum when the incumbent is guaranteed to win or lose re-election.[67]

Moreover, the strategic leverage of swing-state senators tends to be transitory, as it depends on the changing balance of the Senate. In contrast, the formal authority of 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 credits rather than capital

[66] We note that Snyder and Welch (2017) questions whether one can really interpret these results as the consequence of elevated political power.

[67] In a legislative setting, Bernecker (2014) finds that legislators with safe seats tend to shirk their duties.

spending projects that generally take years to deploy.[68] These factors indicate that the external validity of our findings may be limited to instances where political bargaining power is wielded by politicians with sufficiently strong electoral incentives.

7 Robustness Tests

7.1 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 balanced. In our baseline analysis, we define balanced Senates (i.e., $BalSenate = 1$) 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 4, 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 B10 of our Internet Appendix, we present the results of using *BalSenate2* (a dummy variable indicating a maximum seat margin of two seats) and *BalSenate4* (a dummy variable indicating a maximum seat margin of four seats) as our indicator of a balanced 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 4, 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 B11. We see that our firm-level findings are qualitatively

[68] 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.

unchanged.

In Panel D of Table B11, 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 willing to vote against their own party due to intense partisan opposition in their home 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 B12, we present our results using *SwingMode*, a time-invariant measure for the *Swing* status for each state, as the interaction variable with *BalSenate*.[69] We see that our findings are again largely unchanged under this time-invariant measure of state-level partisan balance.

7.2 Political bargaining during 60-40 filibuster-proof Senates

While we focus on a 50-50 balanced 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 3.1, 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 for a full term 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 briefly held a 60-40 majority.

We define *BalSuperMaj* 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 B13. We see that the coefficient estimate on *Swing* $\times$ *BalSuperMaj* is of the same sign and similar in magnitude to that of the coefficient estimate on *Swing* $\times$ *BalSenate* across several outcome variables. However, the estimates generally

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

lack statistical significance, potentially due to the limited statistical power stemming from the infrequency of large majorities in the Senate.

8 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, regional political bargaining power increases tax credit subsidies and decreases effective federal tax rates, leading to higher corporate valuations and investments. Follow-up tests provide evidence that our findings are driven by swing-state politicians using their elevated leverage during balanced Senates to stimulate overall regional economic activity rather than favoring large, well-connected firms. 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.

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 legislative 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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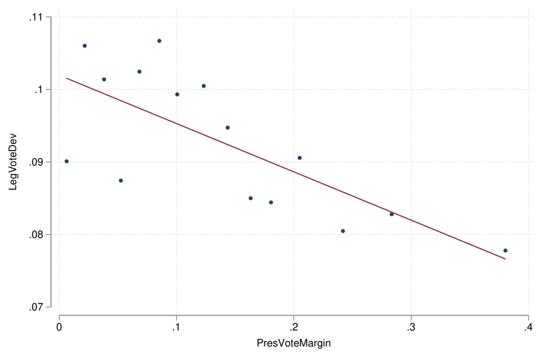
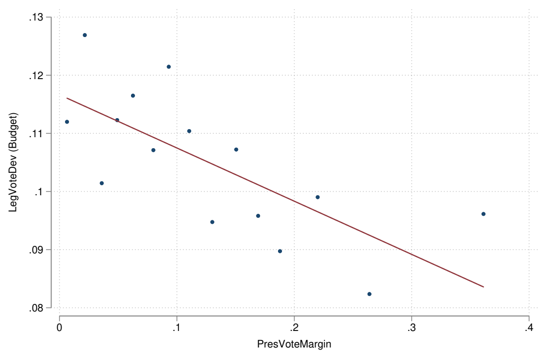
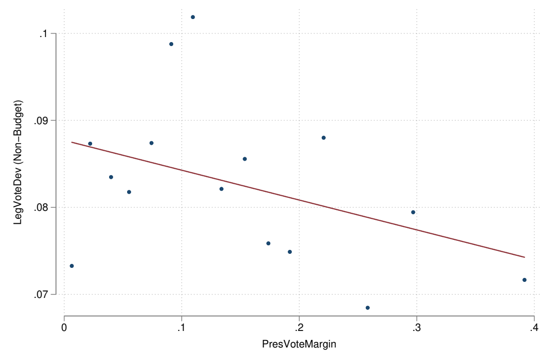


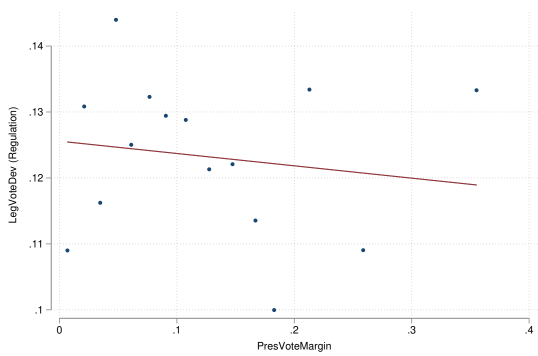
(a) All bills



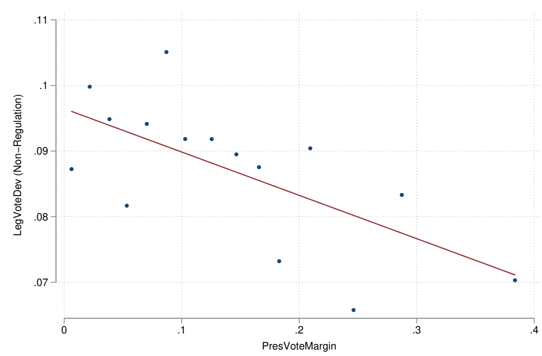
(b) Budget Bills



(c) Non-Budget Bills



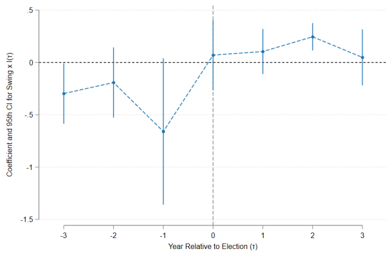
(d) Regulatory Bills



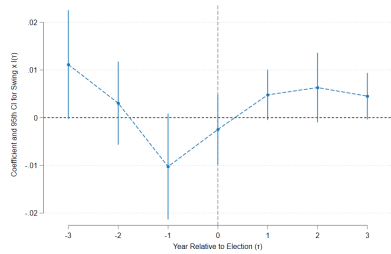
(e) Non-Regulatory Bills

Figure I: Presidential vote margins vs. legislative vote deviations

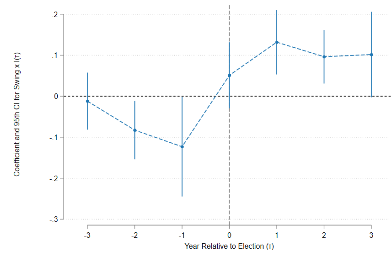
These figures show binned scatterplots representing the relationship between *PresVoteMargin* (the state-level vote margin in the most recent presidential election) and *LegVoteDev* (the rate at which a state's Senator votes against their own party). Subfigure (a) shows this relationship for all bills, 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.



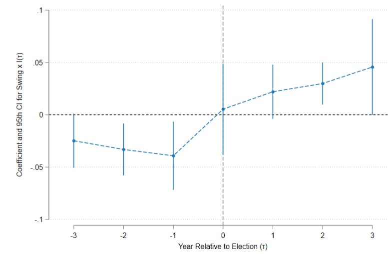
(a) Tobin's Q



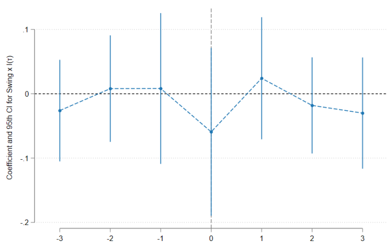
(b) CapEx



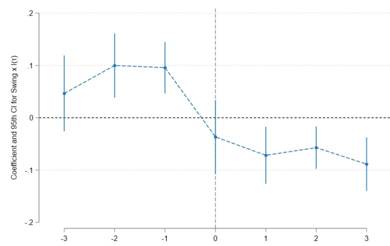
(c) SG&A



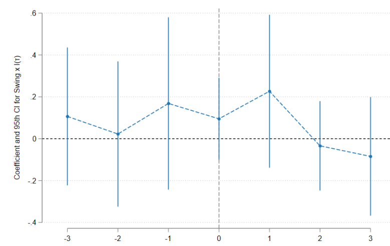
(d) R&D



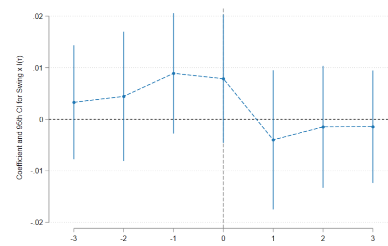
(e) lnSaleGr



(f) ROA



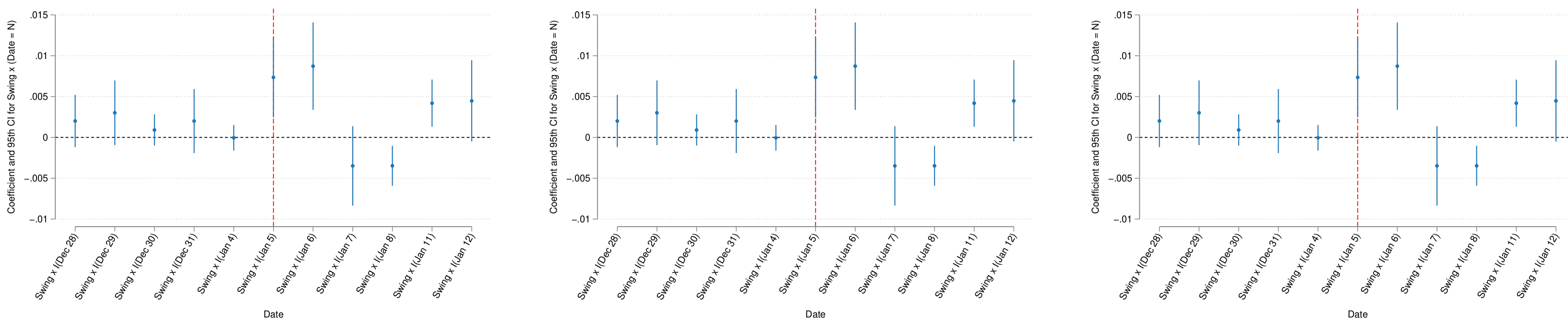
(g) Profitability



(h) Leverage

Figure II: Year-by-year 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 outcome variables indicated by the figure captions and $I(\tau)_t$ represents a dummy variable indicating that year t is τ years before/after either nearest focal 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.



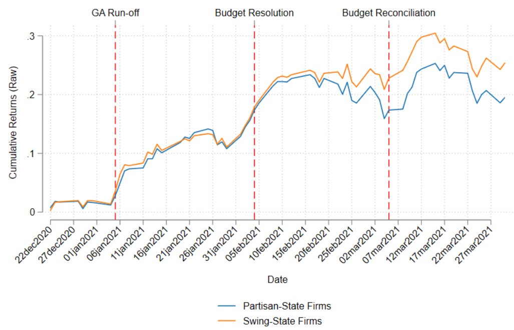
(a) Raw Returns

(b) Abnormal Returns (CAPM)

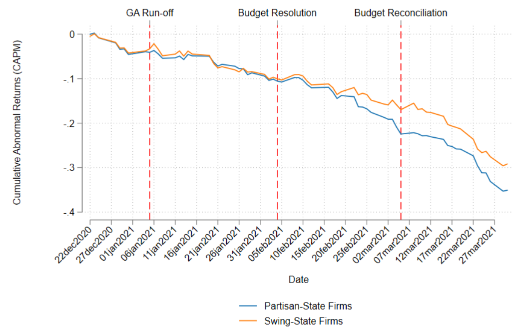
(c) Abnormal Returns (FF3)

Figure III: Georgia runoff election event study

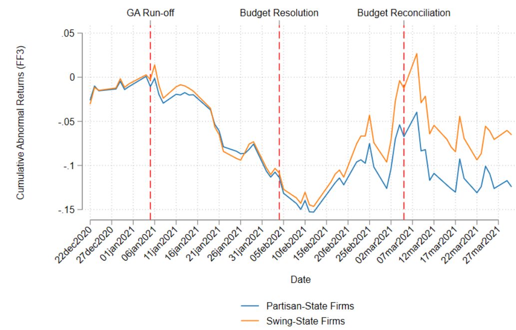
These figures show the point estimates and 95% confidence intervals for π_τ from estimating $R_{it} = \alpha_i + \gamma_t + \sum_\tau \pi_\tau Swing_i \times I(\tau)_t + \epsilon_{it}$, where R_{it} represents return measures indicated by the figure captions, and $I(\tau)_t$ represents dummy variables indicating whether $\tau = t$ where τ takes on values from 5 trading days before to 5 trading days after Jan 5, 2021. 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.



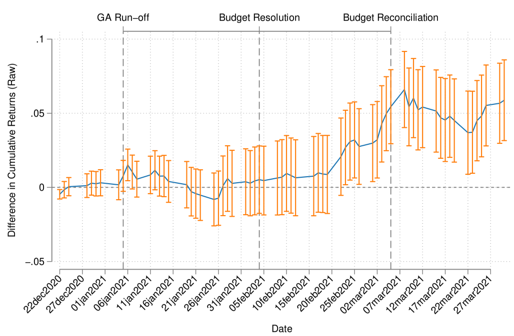
(a) Cumulative Returns



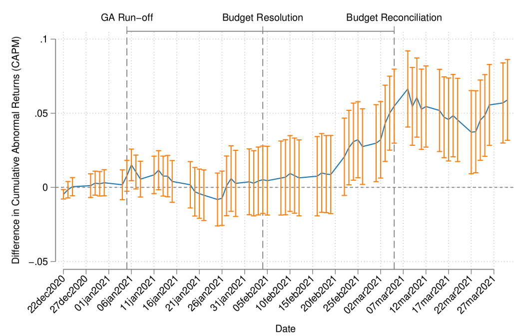
(b) Cumulative Abnormal Returns (CAPM)



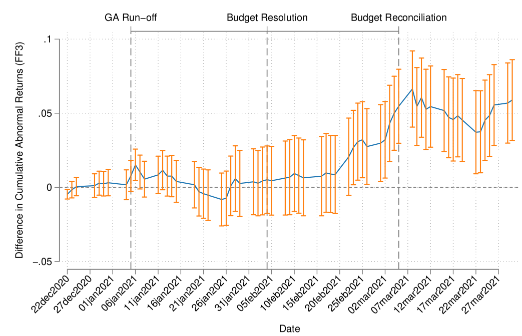
(c) Cumulative Abnormal Returns (FF3)



(d) Δ Cumulative Returns



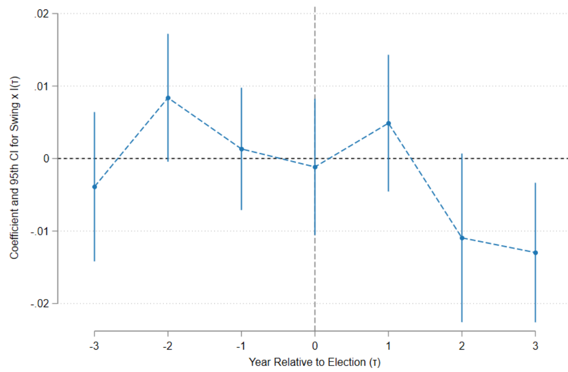
(e) Δ Cumulative Abnormal Returns (CAPM)



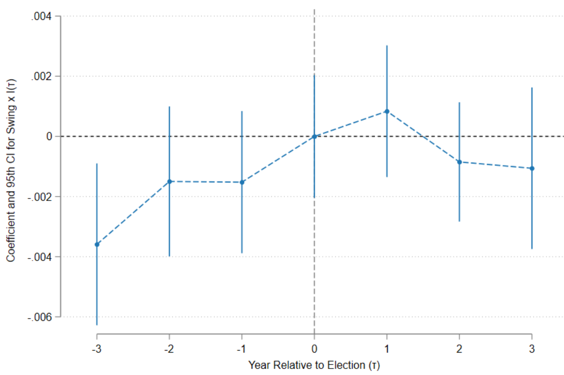
(f) Δ Cumulative Abnormal Returns (FF3)

Figure IV: Georgia Runoff Election Event Study Cumulative Returns

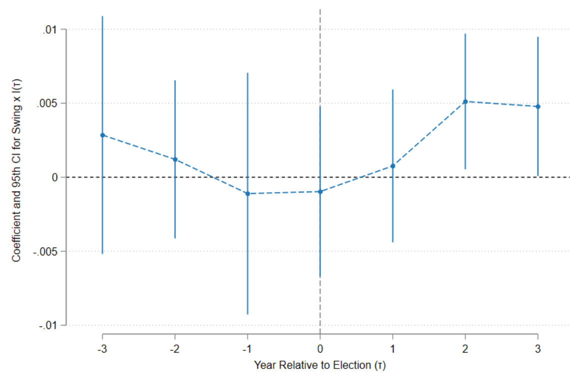
These figures show the mean cumulative returns and cumulative abnormal returns over a [-10,60] trading-day window around the January 5, 2021 Georgia runoff election, where the cumulative (abnormal) return for date t is defined as the sum of (abnormal) returns over the $[-10, t]$ trading-day window. The first three subfigures illustrate the cumulative (abnormal) returns for swing-state firms (in orange) and partisan-state firms (in blue) separately, and the last three subfigures illustrate the mean and 95% confidence interval of the difference between the cumulative (abnormal) returns of swing-state firms and partisan-state firms. All returns are calculated excluding firms located in Georgia. Subfigures (a) and (d) use cumulative returns, subfigures (b) and (e) use cumulative abnormal returns from a CAPM model estimated over a [-220,-11] estimation window, and subfigures (c) and (f) use cumulative abnormal returns from a Fama-French 3-factor model estimated over a [-220,-11] estimation window.



(a) FedTaxETR



(b) StateTaxETR



(c) ForeignTaxETR

Figure V: Year-by-year comparison of swing-state and partisan-state effective tax rates 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 indicated by 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: Predicted Effects of Targeted Policies on Firm Outcomes

This table provides a summary of the predicted effect of various categories of targeted policies on firm outcomes. Each prediction is accompanied by a description of the channel by which the policy affects the particular set of firm outcomes, accompanied by a list of reference studies. A "+" symbol indicates a predicted positive effect, and a "-" symbol indicates a predicted negative effect.

Targeted policy	Predictions	Channel(s)	Prior studies
Spending/earmarks	- Investment, valuation, pre-tax income	Crowding out private-sector activity	Cohen et al. (2011), Kim and Nguyen (2020)
	+ Investment, valuation, pre-tax income	Long-term government contracts	Brogaard et al. (2021)
Tax credits	+ Investment, valuation	Reducing tax burdens	Clark and Sichel (1993), Gupta and Hofmann (2003)
	- Pre-tax income	Higher expenditures (to qualify for tax credits)	Rao (2016), Agrawal et al. (2020)
Import tariffs	+ Investment, valuation, pre-tax income	Lower import competition, greater pricing power	Autor, Dorn, Hanson, Pisano, and Shu (2020), Scherer and Huh (1992), Hombert and Matray (2018), Xu (2012)
Regulations	+ Investment, valuation, pre-tax income	Reduce regulatory burdens, reduce compliance costs	Fullenbaum and Richards (2020), Calomiris, Mamaysky, and Yang (2020), Akey et al. (2021)

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 annual 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	10,084	0.008	0.451	0.000	0.000	0.000
SubsidyPropTax	2,371	0.000	0.001	0.000	0.000	0.000
SubsidyFedGrant	2,230	0.081	1.712	0.000	0.000	0.000
SubsidyNonFedGrant	6,412	0.022	1.685	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[-10,1]	3,909	0.057	0.170	-0.001	0.039	0.092
CAR_CAPM[-10,1]	3,909	-0.029	0.170	-0.087	-0.047	0.007
CAR_FF3[-10,1]	3,909	0.006	0.170	-0.052	-0.012	0.042
CumRet[-10,60]	3,909	0.224	0.435	0.041	0.155	0.307
CAR_CAPM[-10,60]	3,909	-0.321	0.435	-0.504	-0.391	-0.238
CAR_FF3[-10,60]	3,909	-0.094	0.435	-0.277	-0.164	-0.011

Table III: 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 24 May 2001, Republicans controlled a split Senate due to the Republican Vice President. From 24 May 2001 to 25 Nov 2002, Democrats controlled the Senate with a two seat margin. From 25 Nov 2002 to the end of the term (2 Jan 2003), Republicans controlled a split Senate due to the Republican Vice President, though Congress was out of session for that entire period.

Table IV: Effect on Federal Government Spending

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta BalSenate_t + \delta Swing_{it} \times BalSenate_t + \epsilon_{it}$, where Y represents various state-level measures of federal spending denoted in the column headings. The sample consists of state-year observations during the 1994-2020 time period. Only coefficients for *Swing* and *Swing* × *BalSenate* are reported, as *BalSenate* 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 BalSenate	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 V: Effect on Firm Subsidies

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta BalSenate_t + \delta Swing_{it} \times BalSenate_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level subsidy variables denoted in the 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 all 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 $Swing \times BalSenate$ are reported, as *BalSenate* 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 BalSenate	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

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 BalSenate	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 VI: 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 the 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 BalSenate	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 VII: Effect on Operating Performance and Leverage

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta BalSenate_t + \delta Swing_{it} \times BalSenate_t + X_{it}\beta + \epsilon_{it}$, where Y represents various firm-level performance variables denoted in the 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* × *BalSenate* 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 BalSenate	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 VIII: Georgia Runoff Election Event Study (Daily Returns)

This table reports the results from estimating $R_{it} = \alpha_i + \gamma_t + \sum_\tau \pi_\tau Swing_i \times I(\tau)_t + \epsilon_{it}$, where R represents raw daily returns in column 1, CAPM daily abnormal returns in column 2, and Fama-French 3-factor daily abnormal returns in column 3. 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
Swing x I(Dec 28)	0.0020	0.0020	0.0020
	(0.0015)	(0.0015)	(0.0015)
Swing x I(Dec 29)	0.0030	0.0030	0.0030
	(0.0019)	(0.0019)	(0.0019)
Swing x I(Dec 30)	0.0009	0.0009	0.0009
	(0.0009)	(0.0009)	(0.0009)
Swing x I(Dec 31)	0.0020	0.0020	0.0020
	(0.0019)	(0.0019)	(0.0019)
Swing x I(Jan 4)	-0.0000	-0.0000	-0.0000
	(0.0007)	(0.0007)	(0.0007)
Swing x I(Jan 5)	0.0073***	0.0073***	0.0073***
	(0.0023)	(0.0023)	(0.0023)
Swing x I(Jan 6)	0.0087***	0.0087***	0.0087***
	(0.0025)	(0.0025)	(0.0025)
Swing x I(Jan 7)	-0.0035	-0.0035	-0.0035
	(0.0023)	(0.0023)	(0.0023)
Swing x I(Jan 8)	-0.0035***	-0.0035***	-0.0035***
	(0.0012)	(0.0012)	(0.0012)
Swing x I(Jan 11)	0.0042***	0.0042***	0.0042***
	(0.0014)	(0.0014)	(0.0014)
Swing x I(Jan 12)	0.0045*	0.0045*	0.0045*
	(0.0024)	(0.0024)	(0.0024)
Firm FE	X	X	X
Date FE	X	X	X
Observations	70,356	70,356	70,356
Adjusted R-squared	0.041	0.024	0.054

Table IX: Georgia Runoff Election Event Study (Cumulative Returns)

This table reports the results from estimating cross-sectional regression $CAR_i^N = \alpha + \pi Swing_i + \epsilon_{it}$, where CAR represents cumulative returns (columns (1) and (4)), and cumulative abnormal returns based on the CAPM model (columns (2) and (5)), and cumulative abnormal returns based on the Fama-French 3-factor model (columns (3) and (6)). The first three columns report results based on CARs calculated using a [-10,1] window ($N = 1$) and the last three columns report results based on CARs calculated using a [-10,60] window ($N = 60$). Detailed definitions for all variables can be found in Appendix A. Standard errors are corrected for heteroskedasticity and clustered at the state level. Standard errors are in parentheses, with *, **, and *** denoting significance at the 10%, 5%, and 1% level, respectively.

	(1) CumRet	(2) CAR_CAPM	(3) CAR_FF3	(4) CumRet	(5) CAR_CAPM	(6) CAR_FF3
Swing	0.0152**	0.0152**	0.0152**	0.0588**	0.0590**	0.0589**
	(0.0072)	(0.0072)	(0.0072)	(0.0268)	(0.0267)	(0.0267)
CAR Window	[-10,1]	[-10,1]	[-10,1]	[-10,60]	[-10,60]	[-10,60]
Observations	3,909	3,909	3,909	3,909	3,909	3,909
Adjusted R-squared	0.002	0.002	0.002	0.004	0.004	0.004

Table X: Effect on Overall State Economy

These tables report the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta BalSenate_t + \delta Swing_{it} \times BalSenate_t + \epsilon_{it}$, 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 the state 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 in both panels. 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 BalSenate	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 BalSenate	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 XI: Heterogeneity by Corporate Political Activity

These tables report the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \eta W_{it} + \mu W_{it} \times BalSenate_t + \theta BalSenate_t + \delta Swing_{it} \times BalSenate_t + \phi Swing_{it} \times W_{it} + \kappa W_{it} \times Lobbied_{it} \times BalSenate_t + X_{it}\beta + \epsilon_{it}$. where Y represents various firm-level variables denoted in column headings. In Panel A, W represents *Lobbied*, a dummy variable indicating whether a firm made lobbying expenditures directed at the Senate. In Panel B, W represents *Contributed*, a dummy variable indicating whether a firm made political contributions through a political action committee. All specifications include additional control variables: *Democrat*, *Republican*, *MajParty*, *MinParty*, *lnPop*, *lnSenTenure*, *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: Lobbying

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing	-0.3162***	-0.0022	-0.0410	-0.0181***
	(0.1048)	(0.0020)	(0.0260)	(0.0067)
Lobbied	-0.5923***	-0.0060**	-0.1148***	-0.0568**
	(0.1606)	(0.0028)	(0.0362)	(0.0229)
Lobbied x BalSenate	0.2607*	0.0058***	0.0654*	0.0305**
	(0.1387)	(0.0018)	(0.0337)	(0.0129)
Swing x BalSenate	0.2108**	0.0037*	0.0581**	0.0227***
	(0.0801)	(0.0019)	(0.0240)	(0.0082)
Swing x Lobbied	0.3563**	0.0046	0.0558*	0.0370***
	(0.1453)	(0.0029)	(0.0325)	(0.0134)
Swing x Lobbied x BalSenate	-0.2110	-0.0046**	-0.0599*	-0.0229*
	(0.1443)	(0.0022)	(0.0324)	(0.0126)
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

Panel B: Contributions

	(1) TobinsQ	(2) CapEx	(3) SG&A	(4) R&D
Swing	-0.3012***	-0.0025	-0.0395	-0.0164**
	(0.1001)	(0.0019)	(0.0254)	(0.0061)
Contributed	-0.3497*	-0.0127**	-0.0779***	-0.0299**
	(0.1758)	(0.0048)	(0.0267)	(0.0127)
Swing x BalSenate	0.1959***	0.0036*	0.0546**	0.0213***
	(0.0731)	(0.0020)	(0.0223)	(0.0075)
Swing x Contributed	0.2978*	0.0105***	0.0577*	0.0351**
	(0.1744)	(0.0037)	(0.0295)	(0.0141)
Contributed x BalSenate	0.0706	0.0040**	0.0496**	0.0291**
	(0.0864)	(0.0016)	(0.0233)	(0.0121)
Swing x Contributed x Locked Senate	-0.0826	-0.0054*	-0.0447*	-0.0188**
	(0.1299)	(0.0028)	(0.0258)	(0.0079)
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 XII: Effect on Effective Tax Rates

This table reports the results from estimating $Y_{it} = \alpha_i + \gamma_t + \lambda Swing_{it} + \theta BalSenate_t + \delta Swing_{it} \times BalSenate_t + X_{it}\beta + \epsilon_{it}$, 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* × *BalSenate* 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	0.0015	-0.0005	-0.0016	0.0031	-0.0002	-0.0003
	(0.0022)	(0.0008)	(0.0015)	(0.0020)	(0.0008)	(0.0014)
Swing x BalSenate	-0.0062***	0.0002	0.0021	-0.0023	0.0003	0.0013
	(0.0019)	(0.0006)	(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

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.

BalSenate 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.

BalSenate2 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.

BalSenate4 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.

BalSuperMaj 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.

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

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

CAR_FF3^N is the sum of *AR_FF3* in the [-10,N] 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).

Lobbied is a dummy variable indicating whether a firm had lobbying expenditures directed at the Senate in a given year.

Contributed is a dummy variable indicating whether a firm had political contributions directed at the Senate in a given year.

PRisk is the political risk score for a given firm taken from Hassan et al. (2019).

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.