The Consequences of Elite Action Against Elections[*]

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Abstract

Do governing elites who engage in undemocratic practices face accountability? We investigate whether American state legislators who publicly acted against the 2020 presidential election outcome sustained meaningful sanctions in response. We theorize that repercussions for undemocratic activities is *selective*—conspicuous, highly visible efforts to undermine democratic institutions face the strongest ramifications from voters, politicians, and parties. In contrast, less prominent actions elicit weaker responses. Our empirical analyses employ novel data on state legislators' anti-election actions and a weighting method for covariate balance to estimate the magnitude of punishments for undemocratic behavior. The results evidence heterogeneity, with the strongest consequences targeting legislators who appeared at the U.S. Capitol on January 6th, 2021, and weaker penalties for lawmakers who engaged in other antagonism toward democracy. We conclude that focusing sanctions on conspicuous acts against democratic institutions could leave less apparent—but still detrimental—efforts to undermine elections unchecked, ultimately weakening democratic health.

Keywords: January 6th; State legislators; Anti-election actions; Bill cosponsorship; Campaign finance; Elections

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"State Sen. Amanda Chase is calling for a forensic audit of Virginia's 2020 election results. Fortunately, Virginians have a vaccine for this political sophistry.... Voting remains the ideal antidote to 'the big lie' of election fraud."

—Letter to the Editor, *Richmond Times-Dispatch*, August 11, 2021.

1 Introduction

On January 6th, 2021, Virginia State Senator Amanda Chase traveled to Washington, D.C., and appeared outside the United States Capitol to protest the 2020 election. She posted an image of the scene to her official Facebook page with the caption "DC Rally! SHARE what the media won't show you." In the subsequent days, Senator Chase continued to spread election misinformation online, referring to those who attended the U.S. Capitol insurrection as "patriots" and accusing the Democratic Party of stealing the election. Consequences for these public actions soon followed. Chase was censured by the Virginia Senate two weeks later in a bipartisan vote and removed from her committee assignment. During the 2021 legislative session, she appeared as a cosponsor on only 16 bills, including just two with Republican leadership—considerably fewer than the 115 total bills and 27 with party leaders she cosponsored the previous year. When Chase ran for reelection to the Virginia Senate in 2023, the party donated $24,069 to her campaign—down from $77,778 in 2019. She ultimately lost that 2023 election in the Republican primary by a 1.7% vote margin to challenger Glen Sturtevant, who went on to win in the general.

Senator Chase's experience, on the surface, illustrates a familiar story of democratic accountability: an out-of-step legislator was voted out of office. Typical explanations for this outcome might be that Chase lacked ideological alignment with her district (Canes-Wrone et al. 2002) or her party (Pearson 2015). However, Chase was not substantially misaligned with her party or constituents on policy positions or ideology. Instead, journalistic coverage following January 6th suggests that she was sanctioned by her legislative peers, party, and voters for her public actions against a democratic process: the peaceful transition of power after an election. These multifaceted sanctions highlight an important question for scholars of representation: are democratically elected officials systematically held accountable if they engage in undemocratic practices?

We address this question with a case study of American state legislators' efforts to undermine the 2020 U.S. presidential election outcome—actions widely regarded as antagonistic toward American democracy due to the overwhelming evidence that the election was, in fact, free and fair (see Hall 2023; Grimmer and Ramaswamy 2024). We develop a theoretical framework that conceptualizes accountability as *selective.* In a democratic system, politicians who engage in undemocratic practices become out of step with democratic norms and thus threaten the political goals of voters, parties, and other elites. However, our theory suggests that accountability is selectively enforced, with legislators who engage in highly visible undemocratic actions facing the strongest ramifications. Conspicuous and clear norm violations by politicians are broadly observed and recognized as antagonistic to democratic principles, thus drawing widespread attention and commanding a response. Less visible anti-democratic actions, in contrast, elicit weaker accountability penalties. These behaviors garner less attention, are less recognizable or even disputed as pernicious to democracy, and, therefore, more often go unchecked.

We test our expectations using novel data on state legislators' public anti-election actions, which varied in both visibility and the extent to which they are recognized as anti-democratic. By analyzing these lawmakers' online actions (e.g., posts on social media), offline behaviors (e.g., signatures on letters to decertify the results), and presence at the U.S. Capitol on January 6th, we test how extreme a politician must act against a democratic process to receive attention and sanctions for their behavior. State legislators are uniquely situated to yield insight into this question, given the variety of anti-election actions they took before, on, and/or after the January 6th insurrection. We estimate the effects of these actions on state legislators' electoral fortunes in primaries and general elections, legislative connectedness to peers and party leaders, and campaign funding support from the party. The decision to take action(s) against the outcome of the 2020 presidential election was entirely under legislators' control. Our empirical strategy seeks to mitigate confounding due to this selection into treatment. We measure pretreatment versions of all outcome variables and compile data on other pertinent pretreatment covariates, such as legislators' ideology, district ideological preferences, and state-level presidential election vote margin in 2020. We balance these covariates with a weighting method and then estimate weighted regression models with state fixed effects to isolate the unique impacts of anti-election actions on outcomes of interest.

Our results provide evidence of consequences for election antagonism, but also suggest meaningful heterogeneity in the strength of accountability across types of anti-election action. Anti-election actions taken on social media and offline actions against the election generated limited repercussions and, in many cases, exerted negligible effects. We find that the strongest patterns of retribution appeared against the 16 sitting state legislators who joined the insurrection at the Capitol on January 6th, 2021. However, while these effects are consistently large in magnitude, so too are their associated uncertainty estimates. Thus, to triangulate evidence on our theorized accountability process, we also descriptively analyze the contents of news articles and letters to editors about state legislators who engaged in anti-election actions. We show that state legislators at the U.S. Capitol on January 6th received significantly more press coverage dedicated to describing their anti-election actions than legislators who engaged in no action and/or other types action. Moreover, we show media coverage of these state legislators consistently featured content related to their role in the Capitol riot for *two years* after the events on January 6th, 2021. These text-based results indicate further evidence of accountability, validating our primary analyses.

Overall, the results of our case study suggest a nuanced conclusion about the strength of accountability for undemocratic practices. We show that highly visible, widely recognizable efforts against a democratic process appear to generate meaningful repercussions. However, these actions may not represent the most significant threats to democracy. According to our findings, less conspicuous—but still detrimental—efforts to spread misinformation online, promote conspiracy, and undermine election procedures through policymaking channels saw weak or no accountability response from other elites or the mass public. If prominent, yet isolated, anti-election actions elicit a different reaction compared to less visible but more widespread behaviors, the threshold for responsiveness may be too high to safeguard democratic health. While not as iconic as the images of January 6th, often overlooked anti-democratic behaviors may prove more dangerous to the U.S. political system by enabling continued subversion of democratic principles over time.

2 The Role of State Legislatures in Democratic Backsliding

Political elites' influence on the rise and fall of democracy is well-established. This line of work documents how politicians with extremist and authoritarian tendencies manipulate democratic systems to benefit themselves, all while gaining voter approval through ideological appeals (Ziblatt and Levitsky 2018; Bartels 2023). Elite rhetoric has the power to sway public support for democratic principles and erode confidence in political institutions (Clayton et al. 2021; Berlinski et al. 2023; Braley et al. 2023). The result is a reinforcing cycle of democratic dysfunction, social polarization, and declining support for democracy (Kaufman and Haggard 2019). Common implications for democratic backsliding include weakening checks on the executive branch and diminishing legislative quality and policy deliberation (Sebők et al. 2023).

Given the considerable importance of political elites in sustaining democratic governance, a crucial aspect to consider when evaluating the health of American democracy is the presence of accountability for undemocratic practices by policymakers. Do legislators who engage in antagonistic behavior toward democratic institutions face consequences? Recent research on the U.S. finds that members of Congress (MCs) who voted against the certification of the 2020 presidential election saw reduced cross-party collaborations and lower legislative effectiveness (Curry and Roberts 2024) but did not suffer—and may have benefited—in their subsequent election (Bartels and Carnes 2023; but see Malzahn and Hall 2024). Experimental research similarly finds mixed evidence of backlash from citizens, voters, and elites against the anti-democratic actions of politicians (e.g., Carey et al. 2022; Krishnarajan 2023; Berlinski et al. 2023). These conflicting results suggest a need for additional investigation into the nuances of accountability for undemocratic practices. Furthermore, absent from this line of inquiry is an examination of anti-democratic behaviors among a critical set of politicians: state legislators.

Recent scholarship points to state governments as a significant source of democratic backsliding in the U.S. This trend can be attributed to a combination of historical inequities, modern affective polarization in state electorates, long-term demographic change in the states, and rising inequality (e.g., Mickey 2022; Olson 2025). American voters have, in recent years, become more tolerant of undemocratic behavior among elected officials (Graham and Svolik 2020; Gidengil et al. 2022; Krishnarajan 2023), who have themselves become increasingly polarized (Shor and McCarty 2022). State lawmakers tend to reject compromise and the institutions that promote it (Anderson et al. 2020; Kirkland and Harden 2022), yielding gridlock and, as a result, "tolerance for authoritarian leadership" (Mickey 2022, 119). Major policies enacted by state legislatures have exacerbated this polarization (Fordham 2024; Campos et al. 2024) and contributed to democratic decline (Grumbach 2023), particularly in states where the presidential election in 2020 was closely contested (Grumbach and Hill 2023).[1]

American state legislatures are uniquely positioned as high-leverage points for the health of democracy in the U.S. political system. State legislators are monitored by journalists, organized interests, and citizens; thus, their actions and rhetoric can lead the public to normalize—or reject—democratic principles, including confidence in elections. The influence of state legislators on elections is especially impactful due to the decentralized electoral system in the U.S., where state governments are responsible for administering free and fair elections. In the case of the 2020 presidential election, lawmakers in several states exerted informal pressure on election administrators to overturn the result and sought to weaken the powers of secretaries of state (Butler and Harden 2023). Some legislators also made the case for more formal control over elections within the legislature itself via the "independent state legislature" claim (Brown et al. 2023).[2]

State legislatures wield considerably more power than Congress in influencing state election administration. Therefore, studying the degree to which state lawmakers are penalized for undemocratic action is critical to understanding the prospects for the future of American democracy. Additionally, examining the behaviors of state legislators provides greater breadth in observed anti-election actions across different institutional and electoral contexts. As we detail below, we employ novel data on various types of anti-election actions perpetrated by state legislators to explore the repercussions—or lack thereof—that they subsequently faced. We examine accountability for un-

[1] Conversely, Druckman et al. (2023) find that state legislators express *less* partisan animosity and endorsement of undemocratic action compared to the general public.

[2] Specifically, this claim posits that "state legislators—not state courts or other state government officials—have the final say over the legality of the rules state legislatures set for the conduct of elections" (Brown et al. 2023, 209).

democratic behavior meted out by the voting public, within legislatures, and by party organizations. Our theoretical framework posits that accountability for undemocratic behaviors depends on these behaviors' visibility, with overt actions that are clearly antagonistic to democratic values eliciting the strongest accountability response.

3 Accountability for Anti-democratic Actions

We begin by considering two existing theoretical viewpoints about accountability for the undemocratic actions of publicly elected officials. There is a long-standing finding that attitudes toward democracy in the U.S. are positive and strong (e.g., Dahl 1966; Holliday et al. 2024) and backing for undemocratic practices is minimal (Druckman et al. 2023). This historical and continued commitment to democracy in America's political system invokes the default—but potentially naive—expectation that instances of anti-democratic action will consistently elicit punishment. Under this model of *total accountability*, public officials who engage in anti-democratic behaviors face ramifications for *any and all actions* that are out-of-step with the widespread preference for a democratic system of government. An alternative—and more pessimistic—perspective holds that politicians *do not face any* repercussions for their anti-democratic actions. Under this model of *failed accountability*, the public and other elites do not pursue accountability because they have insufficient motivation, knowledge about politicians' behaviors, and/or means for punishing democratic norm violations (Hutchings 2003; Rogers 2023). Accountability can also fail because anti-democratic actions are knowingly overlooked or unintentionally rationalized (Graham and Svolik 2020; Hassan et al. 2022; Carey et al. 2022; Krishnarajan 2023; Druckman 2024).

Our theory builds on these perspectives to introduce a framework of *selective accountability.* We argue that accountability does occur—but the punishment enacted depends on the visibility of undemocratic behaviors and their recognizability as anti-democratic. When voters are aware of out-of-step behavior, they hold elected officials accountable, which motivates officials to tailor their behaviors to public preferences (Fearon 1999; Canes-Wrone et al. 2002; Porter 2022). However, such public awareness is not inevitable. Without media attention, issue salience, and individual motivation, democratic accountability often fails (Hutchings 2003; Rogers 2023; Druckman 2024). Furthermore, undemocratic actions can only be penalized if other elites and the public identify such behaviors as antagonistic (Weingast 1997; Druckman 2024). Although there is general agreement about which anti-democratic actions most violate democratic principles, there is considerable *dis*agreement about what specifically constitutes a transgression (Carey et al. 2019). If anti-democratic actions are not consistently and accurately acknowledged, voters and elites cannot react appropriately.

Following this logic, we propose that accountability for undemocratic behaviors is selective, with highly visible, prominent, and unambiguous actions against democratic institutions producing the strongest accountability response. Lawmakers who openly and clearly violate democratic principles become focal points for criticism, drawing media coverage and public scrutiny. This attention elicits a response from other political actors who *sincerely* wish to protect democratic institutions or *strategically* want to avoid backlash from the perception that they failed to do so.[3] Elected officials who engage in more subtle or less visible undemocratic activities may be overlooked or purposefully ignored, leading to weaker ramifications. Importantly, however, we do *not* claim that these less conspicuous acts are any less harmful to democracy. Subtle actions taken by elites still can pose significant threats to democratic health. In sum, our proposed process of selective accountability hinges on the extent to which elites and the public (1) are aware of the action in question and (2) broadly agree that it is antagonistic to democratic principles.

3.1 Accountability within the Electorate

A large majority of the American public expresses support for democracy (Druckman et al. 2023). Accordingly, we expect that failure to represent pro-democratic views produces an accountability response similar to that for out-of-step representation on a policy or ideological dimension (e.g., Canes-Wrone et al. 2002; Hogan 2008; Birkhead 2015). However, recent work calls this standard model for accountability into question in state legislatures (Rogers 2023). The public may not

[3] Sincere and strategic motivations represent two distinct pathways in this process which may vary across political actors. We remain agnostic about which pathway is more dominant because both lead to the expectations we list below.

be aware enough to exert meaningful electoral penalties on out-of-step representatives. Moreover, some voters are willing to trade democratic principles for other conflicting considerations, such as political ideology or partisan loyalty (Graham and Svolik 2020; Carey et al. 2022), rationalizing politicians' undemocratic actions for fear of losing out politically (Krishnarajan 2023). These citizens may overlook legislators' anti-election transgressions if offenders are same-party members. Bartels and Carnes (2023) show support for this perspective in their analysis of MCs' electoral fortunes after January 6th.

Nonetheless, we posit that democratic accountability for undemocratic practices within state legislative electorates is plausible. The roadblocks to accountability that Rogers (2023) identifies are considerably weaker in the case of election denialism. Traditional and social media, Democratic Party organizations, good governance groups, and other organized interests extensively covered state legislators' actions in response to the 2020 election. This attention heightened awareness of state legislative politics, increasing the likelihood that citizens were adequately informed to hold their legislators accountable on this specific dimension. And while MCs did not generally suffer at the polls after January 6th (Bartels and Carnes 2023), additional research finds that election-denying officials in state governments who ran for reelection *did* experience a reduction in vote share (Malzahn and Hall 2024). Many voters likely perceived incumbent state legislators who engaged in anti-election actions as out-of-step representatives. The result, we predict, was diminished success in primaries and general elections.

H1a Compared to those who did not act against the 2020 election, anti-election state legislators (a) were less likely to advance from their primary election and (b) received a smaller general election vote share in the subsequent election cycle.

Press coverage about election denialism spanned many types of efforts, from 'stop the steal' claims on social media to calls for ballot audits. However, the events at the U.S. Capitol on January 6th garnered the most widespread attention, with reporting occurring at the local, state, national, and international levels. Moreover, online anti-election efforts and offline behaviors beyond attendance at the U.S. Capitol may have been viewed by some citizens as less obviously undemocratic and thus not sufficiently problematic to warrant a response (Carey et al. 2019). This combination of reduced visibility and heterogeneity in tolerance weakens the potential for democratic accountability. Although we expect to observe backlash for all types of anti-election actions (H1a), we posit that the magnitude of this accountability was stronger for January 6th attendees.

H1b State legislators who attended the U.S. Capitol insurrection faced larger electoral penalties compared to those who engaged in other types of action against the 2020 election.

3.2 Accountability within Legislative Institutions

On the institutional side, we theorize that undemocratic action introduces a messaging problem for legislators and their parties. Legislators must maintain strong interpersonal connections to accomplish policy goals and build their careers (e.g., Kirkland 2011; Fong 2020). Party leaders must similarly forge connections to enact their agendas. Compromise with the minority party and moderates in the majority is often required for legislative success (e.g., Jenkins and Monroe 2012; Curry and Lee 2020). In this context, a lawmaker who publicly acts against democratic principles is a political liability to parties and their members. Anti-election legislators are unlikely to be allies in passing legislation, supporting their party's agenda, or ensuring effective governance. Moreover, there is broad support among state legislators and their constituents for democratic practices (Druckman et al. 2023; Holliday et al. 2024); thus, election-denying state legislators are easy targets for condemnation. Advancing bills that are tied to offending legislators would likely disrupt the potential for bipartisanship and distract from the proposal's policy impacts, changing the narrative to a referendum on support for democracy.

For these reasons, we expect that engaging in anti-election action reduces a legislator's institutional standing with their colleagues in the legislature. An observable implication of this backlash is a reduction in the significance of inter- and intra-party bill cosponsorship. Cosponsorship of legislation is a meaningful relational indicator of collaboration and commitment to policy ideas (e.g., Bernhard and Sulkin 2013; Kirkland and Gross 2014; Schilling et al. 2023). All else equal, lawmakers with numerous, varied, and important cosponsorship ties are key players in the chamber (Kirkland 2011). Those who do not connect with their colleagues on legislation are isolated and lack clout. Because bill sponsorship is a regular and institutionalized process, legislators can update their cosponsorship choices in response to new information about their colleagues. We posit that lawmakers avoid working with peers who engage in undemocratic practices, leading to a decline in offending legislators' centrality within their legislature's cosponsorship network.

H2a Compared to those who did not act against the 2020 election, anti-election state legislators experienced a decrease in bill cosponsorship network connectedness within (a) their party, (b) the opposing party, and (c) their party's leadership in the subsequent legislative session.

Legislators constantly compete with the opposing party, which makes the choice to hold a legislator accountable for anti-election action highly strategic. Legislators may be reticent to "call out" same-party colleagues who engage in anti-democratic acts because taking such a position could be costly. Indeed, anti-democratic behavior can have political value, helping politicians and parties to attain power, maintain control, and ensure their preferred outcomes (Hassan et al. 2022). Other-party legislators also face a trade-off in enforcing accountability. Sanctioning legislative opponents for trespasses against democracy is a messaging opportunity. For example, Curry and Roberts (2024) highlight instances where Democratic MCs replaced election-denying Republican cosponsors on their bills with Republican colleagues who voted to certify the 2020 election results. However, punishment could backfire if the actions in question are not widely regarded as antagonistic to democracy. We contend that state legislators at the U.S. Capitol on January 6th faced the most severe punishment from both same- and other-partisans. These lawmakers' anti-election actions were salient and conspicuous, providing the most apparent need—or opportunity—for other legislators to distance themselves and their party from anti-democratic ideals.

H2b State legislators who attended the U.S. Capitol insurrection experienced the largest decrease in cosponsorship network connectedness.

3.3 Accountability within Party Organizations

The previous section establishes the expectation that parties may play a role in institutional accountability if legislative leaders discourage bill cosponsorship with colleagues who commit undemocratic acts. However, action within a legislative institution is not the only way parties could sanction their members for such behavior. An alternative avenue for accountability from the party could appear through resource allocation in elections. State legislative party campaign committees support the goal of gaining or maintaining legislative majorities (Gierzynski 1992; Kistner 2022). They accomplish this objective by selectively targeting campaign funds, typically to candidates in close races (see Hogan 2002). However, Gierzynski (1992) describes a more nuanced strategy for party organizations that considers the broader popularity of the party within the state. Maximizing efforts to support one specific legislator—even in a close race—may not be the best use of funds if doing so contradicts the party's overall appeal to voters. From this perspective, we posit that state parties deprioritize legislators who engage in undemocratic behaviors, providing less financial support to their campaigns. Anti-election legislators pose a political risk to their party and, therefore, are less worthy of investment than other legislators.

H3a Compared to those who did not act against the 2020 election, anti-election state legislators received less campaign funding support from their party in the subsequent election cycle.

The issue of visibility in anti-election action remains relevant in the context of party fundraising. The decision to reduce support to a legislator is less pressing if that legislator's anti-election behavior is not broadly observed or widely regarded as anti-democratic. In contrast, deprioritizing a lawmaker widely regarded as antagonistic toward democracy is a more straightforward political calculation. Even if an anti-election legislator can win their election, they will likely threaten the party's overall state-level reputation. Moreover, this reputational setback could carry forward into the subsequent legislative term. State legislators at the Capitol on January 6th engaged in a behavior broadly accepted as undemocratic; thus, we posit that party organizations punished these lawmakers most severely.

H3b State legislators who attended the U.S. Capitol insurrection experienced the largest decreases in campaign funding support from their party.

To summarize, our theoretical framework posits that lawmakers who act against democracy—and especially those whose behavior is broadly observed and widely regarded as anti-democratic—suffer punishment from a variety of political principals. This holistic depiction of accountability reflects the importance of democratic institutions to numerous public and private actors. We focus on a core set of principals to whom lawmakers respond: voters, peer legislators and party leaders, and party organizations. However, our theory could be generalized to other stakeholders in democratic governance as well, such as political action committees (PACs), private firms, organized interests, and more (see Li and DiSalvo 2023). We next discuss our empirical tests of these expectations. In the supplementary materials (SM), we theoretically and empirically assess the possibility of heterogeneous treatment effects due to contextual variation across state legislatures.

4 Research Design

Our empirical tests involve estimating the effects of actions taken by state legislators against the 2020 election on outcomes measuring electoral success, institutional influence, and party support of campaigns. We define our population of interest as the Republican state legislators who were incumbent members of their legislatures in both 2020 and 2021.[4] This population is identified from Shor and McCarty's (2011) data on state legislators' ideological ideal points, official legislative rosters from the states, and Ballotpedia—a non-profit aggregator of U.S. elections data. As detailed below, we employ information on (1) the anti-election actions taken by lawmakers on and around January 6th, 2021, (2) bill cosponsorship in legislative sessions before and after January 6th, (3) campaign funding from state Republican Party organizations in prior and subsequent elections, and (4) state legislative election results in cycles before and after January 6th.

We relied on a wide variety of data sources to compile information on legislators' anti-election actions, as well as other pertinent covariates. The measures of anti-election actions we introduce

[4]Due to some minor constraints on outcome variable data availability, our analyses reported below omit various small subsets of these legislators.

represent an original data collection effort and a novel empirical contribution of this study. We merge these data on anti-election behavior with other large-scale databases, which include information on legislators themselves and their institutions, districts, and states. Importantly, this process yields a complete population of state legislators with limited missingness in outcome data only, which we note below. Thus, imputation procedures are not necessary in our analyses.

4.1 Treatment Variables

Data on state legislators' actions against the 2020 election are drawn from a reference document compiled by the Democratic Legislative Campaign Committee (DLCC), an organization within the Democratic Party that promotes the election of Democrats to state legislatures. The DLCC compiled extensive information in a 380-page document on the anti-election actions of state-level officials and candidates affiliated with the Republican Party, covering November 2020 through October 2022.[5] Although this resource was generated with partisan intent, it is structured as a source book with primary and secondary materials clearly documenting each purported action. Generally, supporting evidence came directly from legislators (e.g., their social media posts) or verified press reports on legislators' behavior.[6] The DLCC's complete Source Book is publicly available online; thus, its contents are easily verified (Democratic Legislative Campaign Committee 2022).[7] See the SM for direct examples from our coding process.

We collected data from the DLCC's materials on three types of action. We recorded indicator variables for each type, coding "1" if the legislator took action before the end of their state's 2021 legislative session and "0" otherwise.[8] Figure 1 illustrates the considerable state-level geographic dispersion in anti-election behaviors among our population of interest. Republican legislators in

[5]The DLCC's motivation was to compile a comprehensive list of state officials and candidates who engaged in anti-election actions; the organization did *not* focus only on those who were electorally vulnerable (Democratic Legislative Campaign Committee 2022). Our election outcome data (described below) support this point. Anti-election legislators' post-treatment electoral results ranged 25–243% of their expected vote share (see Section 4.1.1).

[6]The primary motivation behind the behavior we study was to *publicly* denounce the 2020 election. Thus, whether or not a legislator took anti-election action is quite clear in this context.

[7]The version of the DLCC Source Book we employ was archived on 02/05/2024 and is available in the SM.

[8]This end-of-session constraint maintains logical temporal ordering with our institutional outcome variables. The Capitol insurrection on January 6th, 2021, occurred before the session ended in all states. Most of the other actions we study occurred between November 3, 2020 (Election Day) and February 2021 (see the SM for details). We recorded the earliest date for legislators who took the same type of action multiple times.

42 states acted against the election, but the state-level proportion of legislators engaging in these acts varied widely. Fewer than 10% of lawmakers from the Republican Party in Alabama (2%), California (9%), and Indiana (1%) engaged in anti-election behaviors, while over 60% did so in Arizona (63%), Oklahoma (65%), and Pennsylvania (72%). Our empirical analyses account for this variation in selection into treatment with pretreatment outcomes, covariates, and state fixed effects (see Section 4.2).

The first action (Figure 1, panel a) is *posting anti-election content or information online* via social media. This variable denotes legislators who shared support for January 6th or election denialism on outlets such as X (then Twitter), Facebook, and others. Screenshots of these posts are generally included in DLCC documentation (see the SM). Typical examples include repeatedly sharing claims of a stolen election and posting content that downplayed the insurrection or promoted claims of voter fraud. This first definition of treatment is significant because state legislators are an active group of elites on social media. Thus, their messaging can potentially impact political discourse (Kim et al. 2022). At the same, this behavior may be less visible than other actions we study. Legislators frequently post and discuss various topics online, making it easy for any single message to be drowned out (Payson et al. 2022). The audience for a legislator's social media is also limited to followers of a specific platform, who are not representative of the broader public (Mellon and Prosser 2017). Additionally, there is potential for disagreement over whether the contents of social media posts truly conveyed anti-election sentiment or constituted a violation of democratic principles (see Carey et al. 2022).

Our next variable measures *offline anti-election actions* (Figure 1, panel b). This indicator tracks election denialism behaviors that occurred beyond social media. These "offline" actions include signing letters asking for a forensic audit of election results and attending rallies at state capitols supporting anti-election efforts. They are significant anti-election behaviors because they encompass actions taken by lawmakers in their capacities as public officials. State legislators face resource and time constraints (Harden 2016), so these offline actions indicate a non-trivial commitment to the anti-election movement. Importantly, however, the *individual* visibility of these

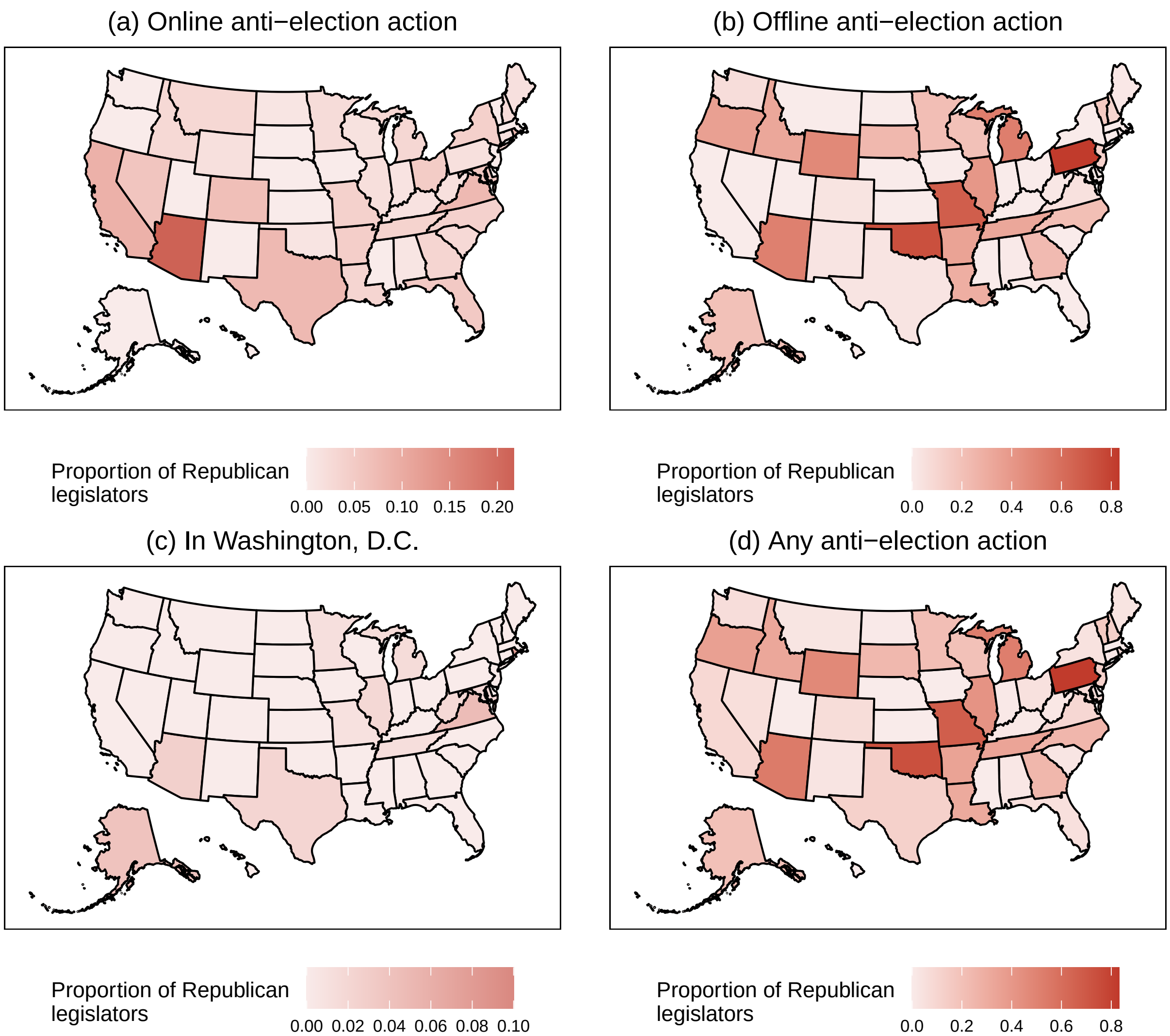


Figure 1: Geographic Distribution of State Legislators' Actions Against the 2020 Election

actions can be limited. In our data, nearly 600 state legislators engaged in anti-election actions beyond posting on social media or attending the U.S. Capitol insurrection. Reporting on these actions, though, usually focused on publishing large lists of names (e.g., all signatories on a letter), essentially distributing the accusation of undemocratic practice rather than singling out individuals.

Finally, we collected data on each legislator's *physical presence at the U.S. Capitol on January 6th* (Figure 1, panel c). Proof of attendance at the Capitol either came directly from legislators' statements on social media or indirectly through credible news reports linked in the DLCC's documentation.[9] This variable captures the most significant public display of antagonism against the

[9]Legislators may have attended the event covertly. However, given that the motivation behind anti-election efforts

2020 election. Images from the Capitol riot quickly became iconic representations of the deep divisions and distrust that threaten democratic practice in the United States. Lawmakers' decisions to appear at and directly associate with the riot on January 6th conveyed an unambiguous willingness to step outside democratic norms that maintain the health of political institutions.

Figure 1, panel (d) consolidates the three treatments outlined above into a distribution of *any* anti-election action. This graph illustrates the widespread nature of state legislators' undemocratic reactions to the 2020 election. However, as we outline in our theoretical framework above, these measures capture distinct conceptual aspects of undemocratic behavior. Empirically, the strongest pairwise correlation is just $+0.30$ (online action and appearance at the Capitol). Nearly all legislators coded as treated engaged in only one type of anti-election action. However, 12 of the 16 attendees at the U.S. Capitol also engaged in at least one other action besides their appearance on January 6th; seven of them took all three types of anti-election action.[10] In what follows, we estimate treatment effects separately for each action type. However, our results and conclusions are not dependent on this choice (see the SM).

4.1.1 Electoral Success Outcomes

To measure legislators' electoral performance, we rely on data for state election outcomes from Ballotpedia. Specifically, we employ data on each legislator's primary and general election performance for cycles before and after the January 6th, 2021 insurrection.[11] State legislative elections are often uncontested, especially at the primary stage (Rogers 2023). Accordingly, our first election-related outcome of interest indicates whether a legislator advanced out of the primary. Because general elections produce more competition, our second outcome of interest examines vote shares in these contests. We also employ data on whether legislators emerged to run for reelection. We account for election-related selection effects in our empirical strategy (see below).

was to publicly denounce the 2020 election and seek to overturn its result, we expect that such cases were very unlikely.

[10]Across the complete data, 80% of legislators are untreated, 19% engaged in exactly one action, and 1% engaged in two or more. In the SM we provide additional details on the legislators who appeared in Washington, D.C.

[11]Election results from Ballotpedia reflect data on contests held through December 2023. A total of 141 legislators in our population of interest had not run for reelection since January 6 by the end of 2023. We exclude these cases from our election and fundraising analyses, but include them in the bill cosponsorship analyses.

State legislative general elections often feature more than two competitive contenders. In these races, using the traditional measure of two-party vote share would mischaracterize the nature of competition. Following past work, we standardized general election vote shares to account for the total number of candidates in a race (e.g., Bonica 2020; Case and Porter 2024). We computed this standardized vote share measure for legislator i in race j as follows:

$$\text{Vote share}_{ij} = \frac{v_{ij}}{(\sum v_j / n_j)} \tag{1}$$

where v_{ij} is the raw vote total for legislator i in race j, v_j is the total number of votes cast in race j, and n_j is the total number of candidates in race j (see Bonica 2020, 266). Values greater than one on this measure indicate legislator i in race j over-performed relative to expectations, and values below one indicate under-performance.

4.1.2 Institutional Influence Outcomes

The institutional outcome variables we analyze come from data on bill cosponsorships in state legislatures. Using the legislation tracking service LegiScan, we downloaded the complete lists of bills for each legislature's regular sessions in 2019, 2020, and 2021.[12] These lists provide information on the names of sponsors and cosponsors for each bill and allow us to measure cosponsorship before and after January 6th, 2021.[13] Using these data, we created measures of legislators' institutional influence, operationalized as bill cosponsorship network centrality and network closeness with Republican party leadership.

To measure Republican members' centrality within networks, we generated undirected graphs with valued (i.e., weighted) edges representing the count of cosponsorship ties between two legislators.[14] The decision to cosponsor a bill ordinarily implies directionality (cosponsor $\longrightarrow$ sponsor);

[12] We used the 2019 legislative session information for the states that had limited sessions in 2020 due to the COVID-19 pandemic (see Birkhead et al. 2025).

[13] We omitted Alabama, Montana, and Nebraska from our institutional analyses because they do not list cosponsors. We also removed Arkansas because its legislature only began listing cosponsors in 2021. We omitted Idaho because only one cosponsor is listed per bill.

[14] In this context, valued edges reflect the strength of the connection between two legislators who cosponsor together (Gross and Kirkland 2019).

however, not all states differentiate between sponsors and cosponsors in bill metadata. For this reason, we used undirected graphs to maintain comparability across states. We calculated legislators' eigenvector centrality from these networks. This measure conceptualizes influence as strategic using the importance of a node's connections (Jackson 2008, 41). In our application, a legislator's influence (operationalized as cosponsorship centrality) increases with their tie count but also with the centrality of their connections (see the SM for an illustrative example). We computed centrality measures for the whole chamber and by party, which allows us to assess whether accountability toward treated legislators varied between the Republican and Democratic members.[15]

We additionally measure Republican legislators' connections with their party's leadership—defined as lower chamber speakers, upper chamber presidents, and majority or minority leaders. We first computed each lawmaker's average edge distance to party leaders in the full network of all members. This indicator captures their proximity to leaders within the context of all of the interdependent relationships encoded in the network. Next, we calculated lawmakers' average cosponsorship ties with party leadership. Counting direct ties to leaders provides an intuitive alternative to measuring connections based on network proximity.

As computed, these centrality and leader connection measures are specific to the population of legislators included in a particular graph—such as all members of a given state legislature or only members of one party within that legislature. To facilitate comparability, we transformed raw values into percentile ranks. This creates outcome variables scaled from 0 to 1, with higher values indicating either greater centrality or closer proximity to party leadership. A common scale allows for meaningful comparisons with legislators from different states and years in our data.

4.1.3 Party-Donated Campaign Funds Outcomes

Our final outcome variable measures the financial support received by legislators' campaigns from state party organizations. From a conceptual standpoint, we seek to assess the extent to which state parties supported the election or reelection of each legislator in a given cycle. Thus,

[15]Because all of the legislators in our data are Republicans, we computed centrality within the Republican Party using a single graph with all party members for each state. We computed centrality within each state's Democratic Party by iteratively generating graphs that included one Republican member with all Democrats.

we collected information on party contributions, as made available by the National Institute on Money in Politics (NIMP).[16] We recorded party fundraising by legislators for (a) their most recent election year prior to January 6th, 2021, with 2016 as the lower limit and (b) their most recent election year after January 6th, 2021.[17]

A key issue with measuring money in politics is specifying the appropriate denominator (Case and Porter 2024). For our purposes, the number of candidates and donors varies across states and time, creating comparability concerns with raw values. Our quantity of interest is a measure of party funding priorities. Accordingly, we collected the funds received by a legislator as a percentage of the total amount of funding the party gave to state legislative candidates in that election cycle. This approach accounts for variation in party expenditures across years. For instance, Amanda Chase received $77,778 from the Republican Party of Virginia for her 2019 state senate campaign, then only $24,069 for reelection in 2023. But those amounts do not encode the nearly $2 million in additional funds that the party gave to state legislative candidates in the latter election year. Thus, the difference in the party's prioritization of Chase's campaigns was starker than the raw data imply: about 1% of the total funds in 2019 and just 0.3% in 2023.

4.2 Identification

Empirically identifying the causal effects of anti-election actions is a significant challenge. By definition, our treatment variables are driven entirely by the decisions of the legislators we study.[18] Research demonstrates that those who did and did not choose to engage in anti-election practices differed systematically (Berkman et al. 2024). We lack exogenous variation in anti-election actions and thus must rely on observed covariates to mitigate confounding of our treatment effects. Our identification strategy contains multiple components to reduce bias in our estimates, including theoretically informed covariates and the use of weighting to balance them across treatment condi-

[16] NIMP defines party donations as those contributed by "political party committees or their employees."

[17] We limited the pre-January 6th search to 2016–2020 so that our measure reflects party funding priorities during the time period in which Donald Trump was a significant factor in national-level politics.

[18] We refer to the anti-election actions we analyze as "treatments," as is common practice in observational studies. The use of this term denotes these variables as our primary independent variables of interest. It is not meant to imply that we have experimental data.

tions. But it is important to acknowledge the core assumption underlying all of our analyses: that we have no unmeasured confounders (Morgan and Winship 2015).[19]

4.2.1 Pretreatment Outcomes and Covariates

Our first effort to reduce bias involves measuring all of our outcome variables before and after treatment. While our data are cross-sectional (one row per legislator), measuring each outcome at two points in time effectively produces a lagged dependent variable, which provides considerable control of units' pretreatment histories in the context of panel data (e.g., Ding and Li 2019).[20] Conditioning on pretreatment outcomes accounts for baseline levels, mitigating the potential for attributing observable variation in outcomes prior to January 6th, 2021 to anti-election treatment effects. For instance, in their analysis of cosponsorship patterns in Congress, Curry and Roberts (2024, 10) report that "[election-denying MCs] were already rarely included as cosponsors before January 6th." Measuring outcomes prior to treatment adjusts for possible biases of this type.

We collected a comprehensive set of additional pretreatment covariates to further mitigate confounding of our estimates. At the individual level, we collected data on legislators' ideal points (Shor and McCarty 2011), seniority, gender, term limit status, and legislative leadership status. We also recorded an indicator for whether a legislator belonged to their legislature's upper chamber. At the chamber-level, we measured majority insecurity with counts for shifts in chamber majority status from 2010–2020 (see Hinchliffe and Lee 2016). We additionally included measures for state legislative district ideology in 2020, as estimated by Tausanovitch and Warshaw (2013).[21] Finally, at the state level we collected the vote margin between Donald Trump and Joe Biden in 2020, Grumbach's (2023) state democracy index in 2018, Bowen and Greene's (2014) two dimensions of legislative capacity in 2019, and indicators for legislative term limits at the state and legislator

[19]See the SM for an analysis of the sensitivity of our estimated effects to omitted variable bias.

[20]The particular year recorded for these measures varies at the legislator level depending on timing and data availability. For instance, some lawmakers' most recent election prior to January 6th was in 2016 or 2017. Our guiding rule was to collect the most recent pretreatment year available. This year was within 2018–2020 (inclusive) for the vast majority of cases, but included earlier years for some variables and legislators.

[21]Tausanovitch and Warshaw's (2013) data are missing district-level estimates for 145 (4%) of the legislators we study. As an alternative, we used the estimates for the county with the most geographic overlap of the legislators' districts in these cases.

levels. In the SM we discuss additional details and provide summary statistics for these measures.

4.2.2 Balancing Weights and Estimation

Non-random selection into treatment almost certainly produced imbalance between legislators who did and did not take anti-election action in potential outcomes, yielding a significant threat to our inferences (Morgan and Winship 2015). While we cannot directly observe imbalance in the potential outcomes, we can generate positive evidence suggesting that it has been corrected by balancing our covariates across treatment status (Ho et al. 2007). Weighting methods are an effective strategy for accomplishing this task by creating a pseudo-sample of the data that exhibits covariate balance.[22] We employ Zubizarreta's (2015) stable balancing weights (SBW) algorithm. SBW estimates weights from the data by solving a constrained optimization problem subject to prespecified requirements of the empirical moments. The variance of SBW weights tends to increase with more constraints. Accordingly, we impose a relatively simple constraint on the weights to generate balance on the covariate means (i.e., no difference on average). We demonstrate in the SM that this method substantially improves balance on this quantity *and* across the covariate distributions in our data.[23]

Our treatment effect estimates come from weighted regression models; a unique set of weights is estimated for each treatment-outcome combination. We included the covariates described above in the weighting specifications as linear terms and generated weights within the control group to match the treated legislators (who each received a weight of 1). Given our assumptions, this strategy identifies the average treatment effect on the treated (ATT). Our weighted outcome models include the treatment of interest and state fixed effects.[24] Depending on the estimation method, we

[22]The process is similar to survey weighting. We generate weights for each unit in the sample such that one group (e.g., a survey sample or, here, a control group) looks similar to another group (a population or treatment group) on observed variables. In survey research the process improves generalizability; in our case, it mitigate the threat of confounders of the treatment effects. See Morgan and Winship (2015, Chapter 7) for additional details.

[23]Weights introduce heterogeneity, which reduces statistical power compared to unweighted analyses. We tested multiple weighting methods and chose SBW because it yielded strong covariate balance while maintaining the largest effective sample sizes. See the SM for weight summaries and additional details.

[24]The fixed effects control for unmeasured state-level confounders, although results are similar if we replace them with our covariates. Including the other two treatment variables in the outcome models as controls does not change our substantive conclusions (see the SM).

computed robust standard errors or bootstrapped standard errors to account for uncertainty from estimating multiple sets of quantities.

5 Results

Before discussing treatment effect estimates, it is important to consider the nuances of interpreting uncertainty in these analyses. We defined a specific population—Republican state legislators serving in 2020 and 2021—and collected information on that entire population for a discrete historical event. Our data include all treated units that comprise this case study (according to our definitions of treatment) and all of the relevant untreated units from which we can use covariate adjustment to make counterfactual comparisons. One perspective on our population-based design would de-emphasize the role of measures for uncertainty (e.g., Desbiens 2007). This logic suggests that our analyses are conducted on the universe of observations relevant to the study rather than one sample from the population, thus obviating the need to consider sampling variability. Simply reporting the estimates as sample descriptors is sufficient for testing our hypotheses. This perspective is not universally shared among social scientists (e.g., Gelman 2011). Thus, we still report standard errors and confidence intervals below.[25]

We consider this perspective as useful context for evaluating our results, especially given that (1) we must use a covariate adjustment method (e.g., weighting) to mitigate bias, (2) these methods naturally reduce statistical power in a bias/variance tradeoff (Ho et al. 2007; King et al. 2017), and (3) our treatments are relatively rare events (see note 10). In our case study, there exists no readily available means of increasing power while holding bias reduction constant.[26] Put differently, there is not another potential design we could leverage to produce unbiased estimates with appreciably smaller standard errors. Thus, in our interpretations, we privilege effect magnitudes and substantive significance over null hypothesis significance testing to evaluate support for our theoretical framework.

[25]Additionally, in the SM we report p-values associated with all of our estimates, which we adjust for the family-wise error rate because we use multiple treatments and outcomes to test our hypotheses.

[26]As previously noted, we cannot collect more data because our dataset already contains the population. Leveraging a source of exogenous variation in treatment assignment is also not an option.

5.1 Electoral Success (H1)

Our first set of outcomes measure electoral accountability for anti-election actions. Table 1 reports the effects of treatment on legislators' success in primary elections. We estimate treatment effects using a weighted sample selection model (Heckman 1976) with bootstrapped standard errors. This estimator accounts for two stages in the reelection process: (1) the decision to enter the primary and (2) advancement out of the primary. We estimate the selection equation (stage 1) with a probit model that includes the covariates discussed above (section 4.2.1); we estimate the outcome equation (stage 2) with a linear probability model that includes state fixed effects. Following Hoffmann and Kassouf (2005), we report estimates of four quantities of interest:

(1) The probability of entering the primary;

(2) The effect of quantity 1 on advancing out of the primary (i.e., the treatment-based selection effect on the stage 2 outcome);

(3) The probability of advancing given the decision to enter the primary;

(4) The total treatment effect—the sum of quantity 2 and quantity 3.

Table 1 reports the estimated effects of anti-election action on a legislator's probability of primary election victory in their subsequent election. Legislators who engaged in online and offline anti-election actions were not any less likely to run for reelection (Table 1, quantity 1); insurrection attendees were approximately two percentage points less likely to enter their next primary. Turning to the conditional probability of primary election victory given the decision to enter the race (Table 1, quantity 3), we find that the effects of online and offline action were virtually zero. These results do not support our hypothesis that all anti-election actions elicit punishment from voters (H1a). In contrast, the choice to appear at the Capitol corresponded with an eight percentage point decline in a legislator's probability of advancing out of the primary compared to the counterfactual (Table 1, quantity 4). This estimate reflects a substantively significant shift; during the period 1994–2020, 98% of state legislative incumbents won their primary election (Rogers 2023, 216–217). These results also align with our hypothesis that January 6th insurrectionists faced larger electoral penalties than did state legislators who engaged in other anti-election actions (H1b).

Table 1: Estimated Treatment Effects on Primary Election Entrance and Advancement

Quantity	Treatment	Estimate	SE	95% lower	95% upper
(1) Pr(Enter primary)	Online action	0.005	0.053	−0.102	0.106
	Offline action	0.004	0.023	−0.040	0.048
	In Washington, D.C.	−0.017	0.120	−0.252	0.206
(2) Selection effect on Pr(Advance)	Online action	0.003	0.032	−0.058	0.068
	Offline action	0.003	0.015	−0.024	0.030
	In Washington, D.C.	−0.010	0.109	−0.142	0.159
(3) Pr(Advance) | Enter primary	Online action	−0.018	0.017	−0.048	0.017
	Offline action	0.012	0.010	−0.008	0.031
	In Washington, D.C.	−0.069	0.071	−0.159	0.058
(4) Total effect on Pr(Advance)	Online action	−0.015	0.046	−0.099	0.077
	Offline action	0.014	0.023	−0.029	0.058
	In Washington, D.C.	−0.079	0.174	−0.290	0.193

Note: Cell entries report estimated treatment effects, bootstrapped standard errors (SE), and 95% confidence intervals on quantities related to the probability of entrance and probability of advancement in primary elections after January 6th, 2021. All estimates come from a sample selection model weighted by covariate balancing weights from Zubizarreta's (2015) SBW algorithm and with state fixed effects included in the outcome specification (advancement). The data include 3,116 total state legislators, with 2,141 who entered a post-treatment primary election for a state legislative seat.

Table 2 reports the second component of our electoral accountability analysis: the estimated effects of anti-election action on general election vote shares. We again employ a weighted sample selection model with similar specifications. The probit selection model (quantity 1) estimates essentially the same quantity as quantity 4 in Table 1 (with a different specification and functional form). The outcome model (quantity 4)—a linear regression with state fixed effects—estimates the effect of treatment on a legislator's standardized general election vote share.

Almost all estimates in Table 2 are negative, aligning with our expectation for cross-cutting anti-election penalization (H1a). The total effects of each treatment represent decreases of 15% (online action), 6% (offline action), and 45% (in Washington, D.C.) of a within-state vote share standard deviation. Importantly, these effects almost entirely emerge from the selection stage (Table 2, quantity 2). The estimated effects on vote share given that the legislator entered the general election (Table 2, quantity 3) are near zero. Put differently, the sizeable total effect of Capitol presence is attributable to the lowered probability that a legislator faced general election

Table 2: Estimated Treatment Effects on General Election Entrance and Standardized Vote Share

Quantity	Treatment	Estimate	SE	95% lower	95% upper
(1) Pr(Enter general)	Online action	−0.036	0.054	−0.153	0.071
	Offline action	−0.010	0.024	−0.054	0.038
	In Washington, D.C.	−0.114	0.130	−0.364	0.152
(2) Selection effect on vote share	Online action	−0.040	0.060	−0.167	0.082
	Offline action	−0.011	0.028	−0.062	0.044
	In Washington, D.C.	−0.130	0.178	−0.415	0.192
(3) Vote share | Enter general	Online action	−0.001	0.011	−0.022	0.020
	Offline action	−0.005	0.008	−0.021	0.009
	In Washington, D.C.	0.009	0.050	−0.088	0.096
(4) Total effect on vote share	Online action	−0.040	0.061	−0.160	0.086
	Offline action	−0.016	0.028	−0.071	0.044
	In Washington, D.C.	−0.121	0.180	−0.424	0.188

Note: Cell entries report estimated treatment effects, bootstrapped standard errors (SE), and 95% confidence intervals on quantities related to the probability of entrance and standardized vote share in general elections after January 6th, 2021. All estimates come from a sample selection model weighted by covariate balancing weights from Zubizarreta's (2015) SBW algorithm and with state fixed effects included in the outcome specification (standardized vote share). The data include 3,116 total state legislators, with 1,895 who entered a post-treatment general election for a state legislative seat and 107 who won by default due to lack of competition (2,002 total).

voters again in the first place. Conditional on the opportunity and decision to run in the general, treated legislators' vote share was not meaningfully different from the counterfactual. In short, while we find evidence of electoral accountability, it does not perfectly align with the standard "out of step, out of office" story. Instead, electoral penalties reflect sanctioning at the primary election stage (Table 1).

5.2 Institutional Influence (H2)

Our next set of results explores connectedness in bill cosponsorship networks. Table 3 presents treatment effect estimates, robust standard errors, and 95% confidence intervals from linear regressions with balancing weights applied to mitigate confounding. The outcome variables are legislators' percentile ranks of bill cosponsorship network eigenvector centrality in 2021 within the entire legislature, the Republican Party, and the Democratic Party. We use covariates to generate weights and include state fixed effects in all outcome model specifications.

Table 3: Estimated Treatment Effects on Bill Cosponsorship Centrality

Outcome	Treatment	Estimate	SE	95% lower	95% upper
All legislators	Online action	0.012	0.034	−0.055	0.079
	Offline action	0.008	0.021	−0.033	0.048
	In Washington, D.C.	−0.070	0.071	−0.210	0.069
Republicans only	Online action	−0.003	0.036	−0.073	0.067
	Offline action	0.033	0.022	−0.009	0.076
	In Washington, D.C.	−0.070	0.082	−0.230	0.090
Democrats only	Online action	−0.032	0.032	−0.095	0.032
	Offline action	−0.006	0.020	−0.045	0.033
	In Washington, D.C.	−0.140	0.085	−0.306	0.026

Note: Cell entries report estimated treatment effects, robust standard errors (SE), and 95% confidence intervals. The outcome variables are percentile ranks (scaled 0–1) of bill cosponsorship network eigenvector centrality in 2021 for graphs containing all legislators, Republicans only, and each Republican legislator graphed with Democrats only. All estimates come from linear regression models weighted by covariate balancing weights from Zubizarreta's (2015) SBW algorithm and with state fixed effects included in the specification. The data include 2,926 state legislators.

Across all quantities in Table 3, estimates for online and offline action are small and near zero. These results indicate little change to lawmakers' connectedness in the legislature post-treatment, which do not align with our expectations (H2a). In contrast, the effects for representatives who traveled to Washington, D.C. on January 6th are notably stronger. For networks including all legislators and Republicans only, the D.C. treatment group dropped an average of seven percentile points in centrality. These effects are substantively noteworthy, reflecting a 25–26% decrease in the standard deviation of the within-state outcome variation. In the Democratic network, the effect doubles with a 14 percentile point reduction in centrality, or 47% of a within-state outcome standard deviation. This result aligns with our hypothesis that U.S. Capitol insurrectionists experienced the largest decrease in cosponsorship network connectedness (H2b).

Table 4 continues our test of H2 by examining treatment effects on connections to party leaders within the all-legislators cosponsorship network. Across measures for proximity and tie counts, the effects of online action are negligible. Offline action estimates show diverging trends: a moderate negative effect on network proximity (i.e., increased distance) and a large positive effect on tie count (more direct ties). Finally, the Washington, D.C. effects show a clear pattern whereby party

Table 4: Estimated Treatment Effects on Bill Cosponsorship Connections to Party Leadership

Outcome	Treatment	Estimate	SE	95% lower	95% upper
Average proximity	Online action	0.006	0.034	−0.061	0.072
	Offline action	−0.032	0.022	−0.075	0.011
	In Washington, D.C.	−0.047	0.056	−0.157	0.063
Average tie count	Online action	0.017	0.036	−0.054	0.088
	Offline action	0.069	0.022	0.026	0.113
	In Washington, D.C.	−0.041	0.085	−0.207	0.126

Note: Cell entries report estimated treatment effects, robust standard errors (SE), and 95% confidence intervals. The outcome variables are percentile ranks (scaled 0–1) of average proximity to party leaders and average count of cosponsorship ties with party leaders in the all legislators 2021 cosponsorship network. All estimates come from linear regression models weighted by covariate balancing weights from Zubizarreta's (2015) SBW algorithm and with state fixed effects included in the specification. The data include 2,926 state legislators.

leaders distanced themselves from January 6th attendees. Legislators who appeared at the Capitol dropped about five percentile points in average network proximity to Republican leadership and four percentile points in direct cosponsorships with their party leaders, on average. These estimates represent 16% and 13% declines in within-state standard deviations for the respective outcomes.

5.3 Party-Donated Campaign Funds (H3)

Finally, we examine party campaign finance support in Table 5. We employ the same sample selection model framework as our analyses above, with entering the general election as the selection model (stage 1) and share of party funds as the outcome model (stage 2).[27] The estimates demonstrate heterogeneity across treatments. The effects of offline action and presence in Washington, D.C. are consistently negative, in line with expectations (H3a). On the other hand, online action produces a small positive effect, indicating that a slightly *larger* share of party money was allocated to these legislators. Once again, the treatment effects of attending the Capitol insurrection are the largest in magnitude, which aligns with our hypothesis (H3b). The total effect (Table 5, quantity 4) initially appears small. However, that estimate is more than one-fifth of the median share of party funds among legislators who received funds (0.55%) and about 9% of the interquar-

[27] We estimated the former process with a probit model and the latter with linear regression and state fixed effects.

Table 5: Estimated Treatment Effects on Share of Party-Donated Campaign Funds

Quantity	Treatment	Estimate	SE	95% lower	95% upper
(1) Pr(Enter general)	Online action	−0.036	0.056	−0.146	0.072
	Offline action	−0.009	0.024	−0.056	0.038
	In Washington, D.C.	−0.113	0.134	−0.381	0.147
(2) Selection effect on share of party funds	Online action	−0.012	0.019	−0.049	0.025
	Offline action	−0.003	0.008	−0.018	0.013
	In Washington, D.C.	−0.037	0.058	−0.141	0.057
(3) Share | Enter general	Online action	0.035	0.033	−0.030	0.097
	Offline action	−0.031	0.022	−0.074	0.010
	In Washington, D.C.	−0.080	0.048	−0.163	0.001
(4) Total effect on share of party funds	Online action	0.023	0.040	−0.059	0.103
	Offline action	−0.034	0.025	−0.084	0.013
	In Washington, D.C.	−0.117	0.087	−0.265	0.013

Note: Cell entries report estimated treatment effects, bootstrapped standard errors (SE), and 95% confidence intervals on quantities related to the probability of general election entrance and the share of the party's funds received in the next election cycle after January 6th, 2021 (scaled 0–100%). All estimates come from a sample selection model weighted by covariate balancing weights from Zubizarreta's (2015) SBW algorithm and with state fixed effects included in the outcome specification (share of funds). The data include 3,116 total state legislators, with 1,895 who entered a post-treatment general election for a state legislative seat and 107 who won by election cancellation due to no other candidates (2,002 total).

tile range for that group (1.34%). In this context, our results suggest that state Republican parties meaningfully decreased their prioritization of lawmakers who attended the Capitol riot on January 6th. Additionally, the model indicates that these sanctions on the Capitol attendees were rooted in contribution reductions rather than an artifact of legislators' absence from general elections (Table 5, quantity 2). The negative effect on the share of funds given entrance into the general (Table 5, quantity 3) is nearly 70% of the total effect (Table 5, quantity 4).

5.4 Discussion

Overall, the weight of our evidence favors our theory for selective accountability. We find sporadic and limited indications of accountability effects for online and offline anti-election actions. But these results are weaker and less consistent than expected. Treatment effects for attendance at the Capitol riot consistently support our theory that prominent, antagonistic anti-election actions see meaningful punishment. On average, lawmakers present in Washington, D.C. on January 6th incurred major electoral penalties from voters, lost substantial institutional influence with legislative peers and party leadership, and subsequently ran for reelection with diminished fundraising support from their party organizations.[28] Importantly, this conclusion depends on our interpretation of estimates as meaningful descriptions of a specific population of interest. As Tables 1–5 show, most of the estimates are not statistically significant at the 0.05 level. This pattern necessitates appropriate caution regarding inference. Nonetheless, we contend that even as sample-based quantities, these estimates provide meaningful evidence relevant to our research question. Our finding that state legislators who appeared at the Capitol on January 6th suffered substantial punishment from a variety of political actors yields important insight into accountability within institutions, party organizations, and the electorate.

6 Assessing the Mechanism

A central but unexplored mechanism underpinning our theory relates to the visibility of anti-election actions. We hypothesize that an act must be widely regarded as antagonistic to democratic values and broadly observed to elicit strategic or sincere accountability from various political actors. Existing research investigates the views of the public and elites regarding various types of undemocratic behaviors. Acts of political violence, such as those seen on January 6th in Washington, D.C., received limited support and are perceived as violations of democratic values (Westwood et al. 2022; but see Kalmoe and Mason 2022). Other anti-election behaviors, such as those carried out by state legislators online and offline, received mixed responses as harmful to democracy. This heterogeneity in tolerance for undemocratic acts aligns with our theorized mechanism for accountability. The connection between state legislators' actions against the election and these actions' visibility is less well-documented. Surveys conducted after the January 6th insurrection indicated that 70% of U.S. adults said they heard "a lot" about the riot (Pew Research Center 2022). However, these surveys did not ask about public recognition of other kinds of anti-election actions, nor

[28] In the SM we detail the pre- and post-treatment outcomes for each state legislator present at January 6th. These individual-level results underscore the average effects reported here, but also illustrate that there was nuance and heterogeneity in patterns of accountability even for this extreme anti-election action.

about these acts in the context of state legislators' participation.

Recent research on state legislatures suggests that media attention plays a crucial role in shaping public awareness of state lawmakers' representational behaviors (Rogers 2023). We assess the quantity and quality of press attention on state legislators to gauge the likelihood that citizens were made aware of their representatives' anti-election actions. To execute our analysis, we collected news stories published online and in print by journalistic media outlets from January 6th, 2021 to December 31st, 2022.[29] Text was accessed online via NexisUni; see the SM for a complete discussion of our data collection process. We gathered press coverage for all state legislators who were in Washington, D.C. on January 6th and a random sample of state legislators who engaged in online and offline anti-election action.[30] We additionally collected news coverage for a sample of legislators who *did not* engage in any anti-democratic acts (in our definition of treatment) to establish a baseline for news coverage quality and quantity.

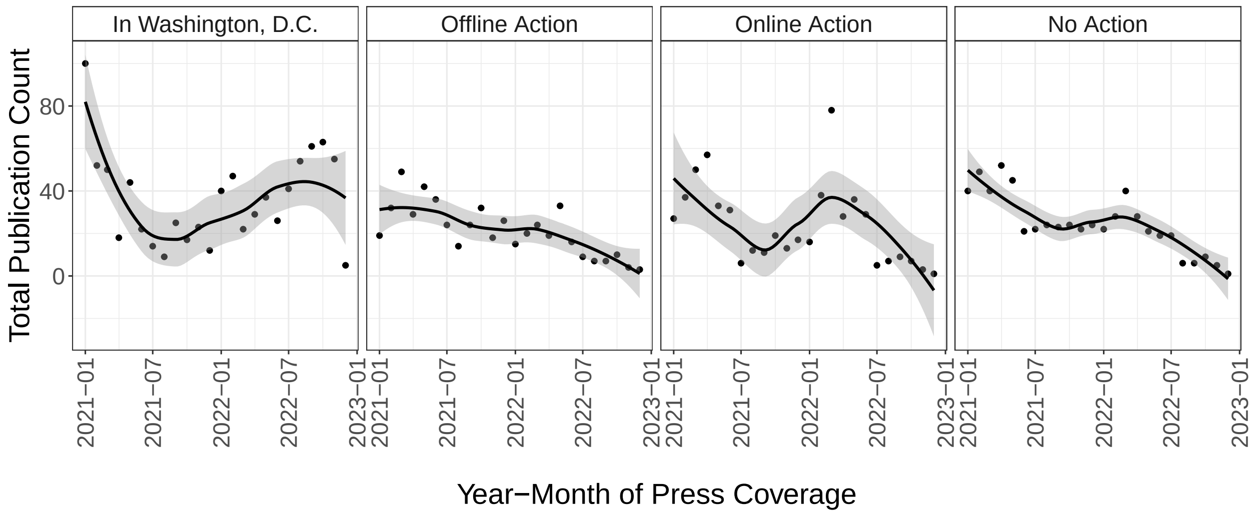
Figure 2 plots monthly counts of news articles published between 2021 and 2022 that referenced those state legislators in our sample. Publication counts are presented by treatment group. Perhaps surprisingly, news coverage across treated and non-treated state legislators was relatively consistent, with the exception of a spike in coverage during January 2021 for legislators who were in Washington, D.C. during the insurrection. Importantly however, these plots do not reveal the extent to which this news coverage discussed state legislators' anti-election actions.

To that end, we paired text data from news articles with a semi-supervised topic model to estimate the proportion of anti-election related content in each document. We specifically employed a keyword-assisted topic model (keyATM), as proposed by Eshima et al. (2024), which allows for topic specification using a small number of keywords prior to model fitting. To directly test our expectations about anti-election news coverage, we fit a keyATM that included a topic specified with keywords related to election denialism. More details on model specification and the robustness of our findings to alternative specifications are available in the SM. Upon estimation, our keyATM

[29]We employ a truncated time series here because many state legislators in our sample left office by 2023.

[30]State legislators were randomly sampled from the population of cases that only received one treatment (i.e., engaged in one type of anti-election action).

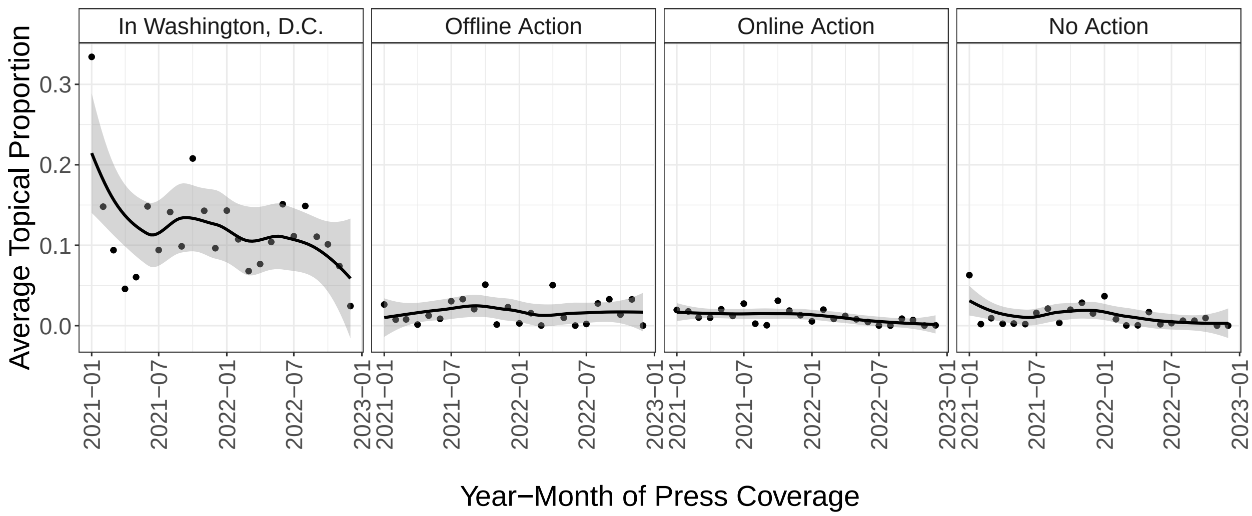
Figure 2: Count of News Publications about State Legislators by Month, 2021–2022



produced an "election denial" keyword topic with terms that include: "election", "trump", "january", "insurrection", "overturn", and "conspiracy." To demonstrate that this topic corresponds with our quantity of interest, we provide excerpts from a random selection of documents with corresponding topical proportions in the SM.

Figure 3 plots the average document-level proportion for the anti-election topic in news coverage by month. In January 2021, news articles that included the names of state legislators who were in Washington, D.C. during the insurrection, on average, had over 30% of their content dedicated to election denialism. This topic's prevalence in news coverage declined in the months that followed but remained notable with an average document-level proportion of 12%. Conversely, news coverage about state legislators who engaged in other anti-election acts had negligible content related to election denialism. The prevalence of this topic in the news coverage of other treatment legislators is indistinguishable from that of state legislators who *did not* engage in anti-election action. Trends presented in Figure 3 provide descriptive evidence that January 6th participation was consistently linked to offending state legislators—even months or years later. These trends also speak to the high visibility of this particular anti-election action, providing support for the second component of our theoretical mechanism.

Figure 3: Average Document-Level Proportion of Election Denial Topic by Month, 2021–2022



7 Conclusions

In a February 22, 2022 *Alaska Dispatch News* op-ed titled "Lawmakers should not be lawbreakers," a non-partisan group of West Point alumni castigated fellow alum and current Alaska House member David Eastman for his participation in the Capitol insurrection on January 6th, 2021. The group contended that "a lawmaker in a democratic government [who] is willing to subvert and destroy the democratic traditions upon which our society is built. . . [is not] fit to serve." Further, the authors asked the other members of the Alaska legislature to "stand with us in condemning David Eastman's gross misconduct, and. . . take appropriate actions to cull his influence from your chambers." In this paper, we investigated whether this argument for holding state legislators accountable for actions taken against the 2020 election became a widespread reality. Did the American democratic system respond to attacks on its core principles from current public officials?

To answer this question, we theorized that accountability for undemocratic action is holistic, encompassing multiple principals to whom lawmakers must answer, and selective, emerging most prominently in cases of highly visible, unambiguous anti-democratic behavior. Numerous actors could animate this process, but here we focus primarily on three central players: voters, legislators' peers, and the parties. To test our expectations, we collected novel data on American state legislators' actions against the 2020 election, including online content and misinformation, offline subversive behaviors, and in-person participation in the Capitol insurrection on January 6th. We combined these data with existing sources on lawmakers' fortunes in primary and general elections, connectedness within the legislature, and campaign finance receipts from state party organizations.

We find heterogeneous evidence suggesting that, among this particular group of elites, only the most extreme anti-election actions appeared to be consistently punished. Posting online or working to subvert the election via other channels did not weigh heavily on state legislators in the months and years that followed. But attendance at January 6th uniquely cost legislators at the polls, isolated them from their colleagues, and motivated parties to shift their financial support to other candidates. In short, the calls for accountability in the West Point alumni's op-ed reflected motivations held and acted upon by political stakeholders around the country—but only with respect to the sitting state legislators who appeared at the Capitol. This finding may help to clarify some of the mixed results on consequences for anti-election behavior among MCs and statewide officials (cf. Bartels and Carnes 2023; Malzahn and Hall 2024; Curry and Roberts 2024). Our results suggest that repercussions were highly context-dependent; this study and others indicate considerable variation in responses across different actors, levels of government, and extremity of action.

Indeed, the rarity of our third category of anti-election action—just 16 legislators were present at the Capitol—prompts important normative implications from this research. The American system functioned reasonably well in responding to this highly visible, but uncommon and mostly symbolic, action against the democratic process. And any accountability is, of course, normatively beneficial compared to none. But a host of less salient, yet still deeply problematic, behaviors largely went unchecked by the public, other legislators, and/or parties. The online messaging of political elites can easily reach millions of people. Legislators' actions intended to influence election administration in November and December of 2020 appeared to place the results of a free and fair contest at considerable risk. These behaviors could be just as dangerous or even worse than participation in a violent protest. And with little or no punishment for engaging in them, the offending elected officials may remain in office and potentially continue antagonizing the process over time, perhaps leading to further erosion of important democratic norms and institutions.

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The Consequences of Elite Action Against Elections

Supplementary Materials

Contents

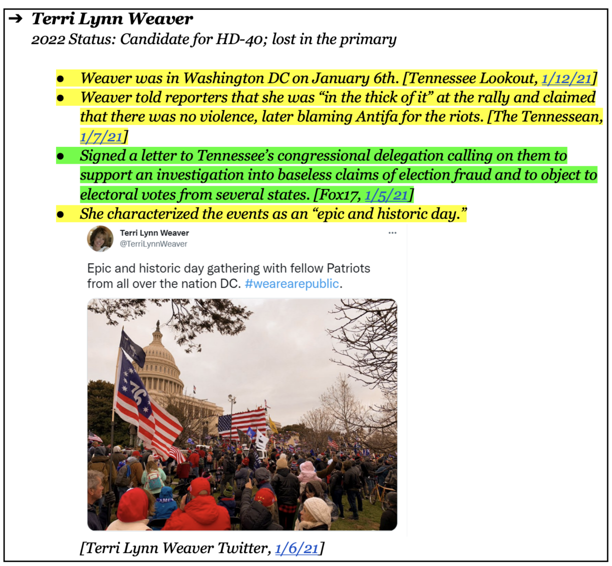
A1 Treatment Coding

Here we provide additional details on our treatment coding procedures.

A1.1 DLCC Source Book Examples

Figure A1 provides two examples of entries from the Democratic Legislative Campaign Committee's (DLCC) Threats to Democracy Source Book. The highlighted colors denote how we coded information from the Source Book into our three different treatment variables. We used the text itself as well as the primary sources linked in the text for all of the legislators in our data. The complete Source Book is publicly available online, and thus its contents are easily verified (Democratic Legislative Campaign Committee 2022).

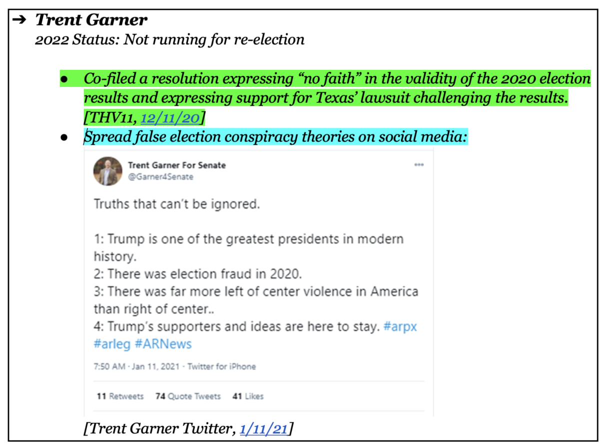
Figure A1: Example Entries from the DLCC Threats to Democracy Source Book



➔ ***Terri Lynn Weaver***
2022 Status: Candidate for HD-40; lost in the primary

- *Weaver was in Washington DC on January 6th. [Tennessee Lookout, 1/12/21]*
- *Weaver told reporters that she was "in the thick of it" at the rally and claimed that there was no violence, later blaming Antifa for the riots. [The Tennessean, 1/7/21]*
- *Signed a letter to Tennessee's congressional delegation calling on them to support an investigation into baseless claims of election fraud and to object to electoral votes from several states. [Fox17, 1/5/21]*
- *She characterized the events as an "epic and historic day."*

[Terri Lynn Weaver Twitter, 1/6/21]



➔ ***Trent Garner***
2022 Status: Not running for re-election

- *Co-filed a resolution expressing "no faith" in the validity of the 2020 election results and expressing support for Texas' lawsuit challenging the results. [THV11, 12/11/20]*
- *Spread false election conspiracy theories on social media:*

[Trent Garner Twitter, 1/11/21]

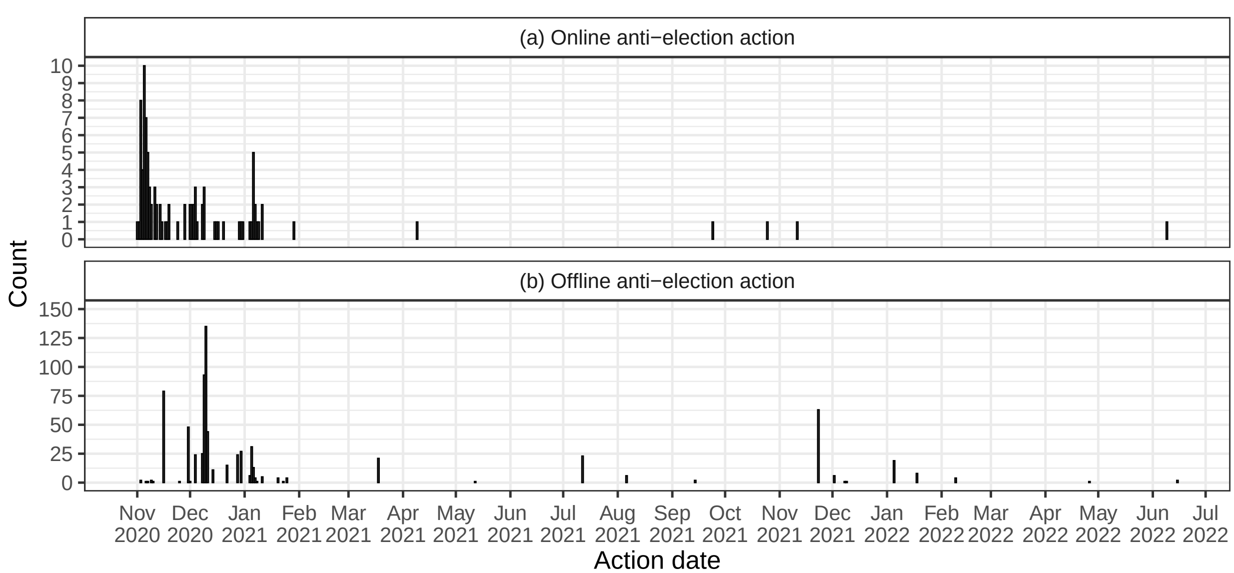
Note: The images present examples of entries from the DLCC Source Book. Blue text denotes online action against the election and green text indicates offline anti-election action. Text in yellow indicates evidence of presence in Washington, DC on January 6th, 2021.

The blue-highlighted text is an example of online action. In this case, the Source Book states that Trent Garner (Arkansas Senate) "spread false election conspiracy theories on social media." The screenshot of the tweet from his official Twitter account provides evidence of this claim. Next, green text denotes offline anti-election behavior. Terri Lynn Weaver (Tennessee House) signed a letter to her state's Congressional delegation supporting an investigation of the election. The source, date, and link to the story supporting this statement appear at the end of the text. Trent Garner also conducted offline action (according to a local news story) by supporting a resolution in the chamber expressing "no faith" in the 2020 election. Finally, the yellow text indicates that Terri Lynn Weaver was present at the U.S. Capitol on January 6th, 2021. The source material includes a link to news reporting her attendance, a quote from her about the event, and a screenshot from her Twitter account with a picture—presumably from her perspective—outside the Capitol building.

A1.2 Treatment Timing

As noted in the main text, we coded our treatment variables as 1 if a legislator took an action prior to the end of their state's legislative session and 0 otherwise. This end-of-session constraint maintains logical temporal ordering with our institutional outcome variables. In cases where a legislator took the same action on multiple dates, we used the earliest date to determine our coding. The action date is constant for one of our treatments (presence at the U.S. Capitol on January 6th, 2021), which occurred prior to the end of the legislative sessions in all states. Figure A2 summarizes the timing of all online and offline actions contained in the DLCC Source Book.

Figure A2: Timing Distributions of Online and Offline Anti-Election Actions



Note: The graphs present the distributions of action dates for online and offline anti-election actions. For legislators who took the same action on multiple dates, the graphs only present the earliest date of that action.

A2 Covariate Descriptions and Summary Statistics

This section provides summaries of the covariates used in our analyses. Table A1 provides descriptions and source information for all of the covariates and Table A2 reports summary statistics of those variables. We use the state-level term limits variable (term-limited state) in all of the analyses presented here, but results are substantively identical if we use the individual-level variable (term-limited) instead.

Table A1: Covariate Descriptions and Summaries

Variable	Description	Source
Pretreatment outcomes	Electoral results, cosponsorship centrality, average network proximity to party leaders, average tie count with party leaders, and share of party funds (%) prior to 2021	Ballotpedia, LegiScan, and National Institute on Money in Politics
Ideal points	Roll call and survey-based measures of conservatism	Shor and McCarty (2011)
Seniority	Total years served in the state legislature	Shor and McCarty (2011)
Gender	Name-based prediction of female or male legislators	gender R package (Mullen 2021)
Term-limited*	Legislator did not run for reelection after 2020 due to term limits	Ballotpedia
Legislative leader	Indicator for lower chamber speaker, upper chamber president, majority leader, or minority leader	Ballotpedia
Upper chamber	Indicator for members of upper chambers	Authors' coding
Chamber majority insecurity	Count of shifts in majority party control of the chamber, 2010–2020	Hinchliffe and Lee (2016)
District ideology	Estimated conservatism of the district (96% of cases) or county with largest overlap of the district (4%) in 2020	Tausanovitch and Warshaw (2013)
State Trump-Biden vote margin	Absolute difference in state-level 2020 presidential election vote share	Authors' computation
State democracy index	Indicator of the quality of democratic institutions in the states in 2018	Grumbach (2023)
Legislative capacity	Estimates of two dimensions of capacity in 2019	Bowen and Greene (2014)
Term-limited state*	Legislative term limits in effect in 2020	National Conference of State Legislatures

Note: * We use the state-level term limits variable (term-limited state) in all of the analyses presented here, but results are substantively identical if we use the individual-level variable (term-limited) instead.

Table A2: Summary Statistics of Variables Included in Estimation

Type	Variable	Min.	Max.	Mean	SD
Outcomes (2021–2023)	Entered primary election	0	1	0.687	0.464
	Entered general election	0	1	0.642	0.479
	Advanced from primary election	0	1	0.644	0.479
	General election standardized vote share	0.167	4.106	1.197	0.283
	Eigenvector centrality percentile rank (All legislators)	0	1	0.414	0.279
	Eigenvector centrality percentile rank (Republicans only)	0	1	0.436	0.287
	Eigenvector centrality percentile rank (Democrats only)	0	1	0.465	0.312
	Average network proximity to party leaders percentile rank	0	1	0.342	0.298
	Average tie count with party leaders percentile rank	0	1	0.473	0.337
	Share of party campaign funds (%)	0	71.329	0.419	1.966
Treatments	Online action	0	1	0.027	0.163
	Offline action	0	1	0.186	0.390
	In Washington, D.C.	0	1	0.005	0.069
Covariates	Upper chamber	0	1	0.761	0.427
	Ideal point conservatism	−0.333	3.408	0.888	0.410
	Seniority (years)	1	28	7.183	5.575
	Female legislator	0	1	0.171	0.377
	Term-limited*	0	1	0.031	0.175
	In leadership	0	1	0.032	0.176
	Majority insecurity	0	4	0.743	1.072
	District ideology	−0.422	0.617	0.150	0.158
	State Trump-Biden vote margin	0.002	0.434	0.165	0.113
	State democracy index	−3.100	1.920	−0.419	1.063
	Capacity (1d)	−1.532	6.515	0.100	1.375
	Capacity (2d)	−3.295	3.357	−0.143	0.795
	Term-limited state*	0	1	0.281	0.450
Pretreatment outcomes (prior to 2021)	General election standardized vote share	0.621	4	1.258	0.282
	Eigenvector centrality percentile rank (All legislators)	0	1	0.463	0.293
	Eigenvector centrality percentile rank (Republicans only)	0	1	0.500	0.303
	Eigenvector centrality percentile rank (Democrats only)	0	1	0.469	0.309
	Average network proximity to party leaders percentile rank	0	1	0.347	0.303
	Average tie count with party leaders percentile rank	0	1	0.484	0.345
	Share of party campaign funds (%)	0	100	1.198	4.285

Note: Cell entries report the minimum, maximum, mean, and standard deviation of each variable. The sample size is 3,116 except for the eigenvector centrality percentile ranks (N = 2,926). * We use the state-level term limits variable (term-limited state) in all of the analyses presented here, but results are substantively identical if we use the individual-level variable (term-limited) instead.

A2.1 Descriptive Information on January 6th Attendees

Table A3 presents summary information about the 16 sitting state legislators who attended the riot at the U.S. Capitol on January 6th, 2021. It includes information on their additional anti-election behavior, demonstrating that many of the January 6th attendees committed multiple undemocratic acts in response to the 2020 election. The table also contains pre- and post-treatment information on our outcome variables: election results, bill cosponsorship networks, and campaign funds from the party. Note that data for Doug Mastriano (PA) are incomplete because his first election since January 6th is scheduled for November 2024. Additionally, we omit Anthony Kern (AZ) and Derrick Evans (WV). Kern was a legislator in 2020 and appeared at the Capitol. However, he lost his reelection bid in 2020 and thus was not a member of the legislature in 2021 (he then won election again in 2022). Evans won office for the first time in 2020, but never served because he resigned in January 2021 immediately after appearing at the U.S. Capitol.

Table A3: Summary Profiles of State Legislators Present at the U.S. Capitol on January 6th

			Other actions		**Election results**				**Bill cosponsorship networks**				**Party funds**	
Name	State	Chamber	Online action	Offline action	Post primary	Pre vote	Post election	Post vote	Pre centrality	Post centrality	Pre proximity	Post proximity	Pre funds	Post funds
Azinger, Mike	WV	Upper	✓	✓	Advanced	1.15	Won	1.31	0.14	0.41	0.01	0.52	0.00	0.00
Biedermann, Kyle	TX	Lower			Did not run	1.50	Did not run		0.81	0.69	0.00	0.04	0.01	0.00
Chase, Amanda	VA	Upper	✓		Lost	1.09	Did not run		0.24	0.01	0.88	0.00	1.14	0.27
Cox, Dan	MD	Lower			Did not run	1.24	Did not run		0.75	0.17	0.82	0.02	0.00	0.00
Drazkowski, Steve	MN	Lower	✓	✓	Did not run	1.33	Did not run		0.35	0.29	0.12	0.01	0.48	0.00
Eastman, David	AK	Lower		✓	Advanced	1.48	Won	1.57	0.23	0.10	0.26	0.12	0.11	0.00
Finchem, Mark	AZ	Lower	✓	✓	Did not run	1.37	Did not run		0.30	0.09	0.53	0.39	6.03	0.00
Hill, Justin	MO	Lower	✓	✓	Did not run	1.29	Did not run		0.00	0.12	0.00	0.00	0.00	0.00
LaRock, Dave	VA	Lower	✓	✓	Advanced	1.14	Won	1.17	0.58	0.51	0.17	0.53	0.88	1.19
Maddock, Matthew	MI	Lower	✓	✓	Advanced	1.19	Won	1.16	0.78	0.36	0.35	0.46	0.00	0.00
Mastriano, Doug	PA	Upper		✓	Advanced	1.37			0.14	0.09	0.80	0.25		
McGuire, John III	VA	Lower	✓		Advanced	1.22	Won	1.23	0.76	0.77	0.00	0.01	0.64	0.04
Miller, Chris	IL	Lower	✓		Advanced	1.52	Won	1.00	0.77	0.57	0.14	0.17	0.03	0.07
Paxton, Angela	TX	Upper			Advanced	1.02	Won	1.73	0.77	0.74	0.03	0.04	4.04	0.00
Price, Justin	RI	Lower	✓		Advanced	1.04	Lost	1.34	0.07	0.03	0.79	0.10	2.56	0.00
Weaver, Terri Lynn	TN	Lower	✓	✓	Lost	1.58	Did not run		0.26	0.33	0.98	0.61	0.00	0.00

Note: Cell entries report summary information on state legislators serving in 2020 *and* 2021 who attended the riot at the U.S. Capitol on January 6th, 2021. The other actions columns denote the other two treatment variables we analyze. Election results include post-treatment primary status, pretreatment standardized vote share, post-treatment election result, and post-treatment standardized vote share. The bill cosponsorship networks columns include pre- and post-treatment eigenvector centrality percentile rank and pre- and post-treatment network proximity to party leadership percentile rank. Party funds denotes pre- and post-treatment share of the party's funds given to state legislative candidates in the election cycle. Some data are missing for Doug Mastriano (PA) because his first post-treatment election is scheduled for November 2024.

Table A3 emphasizes the heterogeneity in response to even the most extreme action against democratic practice in our data. Several legislators' fortunes reflect the average effects we report in the main text. Amanda Chase (VA) and Terri Lynn Weaver (TN) lost primary elections, Chase and Dan Cox (MD) became much less connected in the cosponsorship network and distanced from party leadership, and John McGuire (VA) and others received less funding support from their state party organizations. On the other hand, several of these lawmakers won primary and general elections, Mike Azinger (WV) and Weaver became more central bill cosponsors, and Dave LaRock (VA) actually received *more* party funds than he did during his pretreatment election campaign.[1] The overall pattern reflects punishment for these legislators' actions. But there was clearly nuance in each case that warrants some caution when interpreting our average effects.

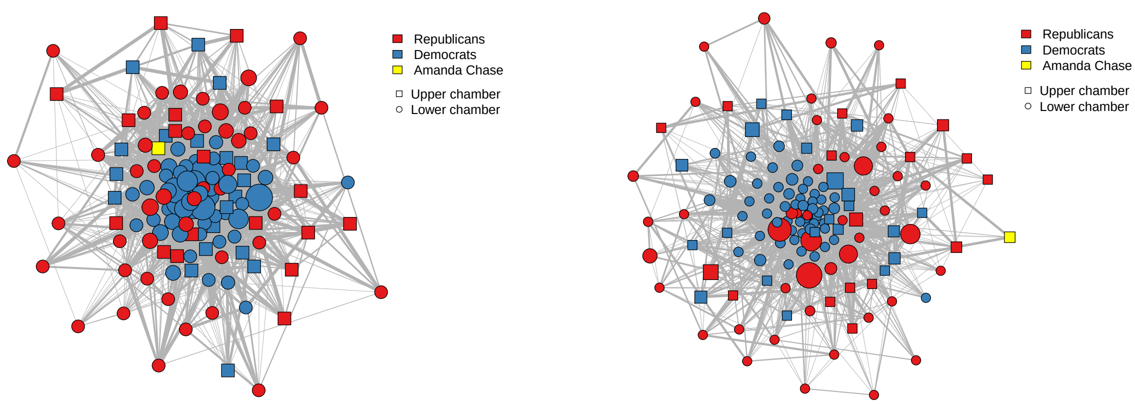
A2.2 Bill Cosponsorship Network Example

Figure A3 presents an illustration of the bill cosponsorship networks we analyze in the main text. The graphs plot cosponsorship networks in the Virginia General Assembly in 2020 (panel a) and 2021 (panel b). Node size is proportional to eigenvector centrality and edge thickness is proportional to the count of cosponsorships between two nodes. Additionally, the graph embedder (GEM) algorithm used to draw the graphs attempts to place more central nodes closer to the center of the space (among other contraints; see Frick et al. 1995). Thus, more central legislators tend to group in the middle and more isolated members appear farther out. Note that Senator Amanda Chase, who we discuss in the main text, moved from a position of some centrality in 2020 (24th percentile) to the fringe of the legislature (first percentile) in 2021 after she attended the riot at the U.S. Capitol.

[1] This result contrasts with the other two Virginia legislators who attended the riot and received *less* party funding in their next elections (Amanda Chase and John McGuire).

Figure A3: Virginia General Assembly Bill Cosponsorship Networks, 2020–2021

(a) 2020 (b) 2021



Note: The graphs present the bill cosponsorship networks in the Virginia General Assembly (lower and upper chambers) in 2020 (panel a) and 2021 (panel b). Node size is proportional to eigenvector centrality and edge thickness is proportional to the count of cosponsorships between two nodes. The graphs are drawn with the graph embedder (GEM) algorithm (Frick et al. 1995), which attempts to place more central nodes closer to the center of the space.

A3 Balancing Weight Summaries

Table A4 reports summary statistics of the weights estimated for our three different analyses and three treatment variables using the stable balancing weights (SBW) algorithm (Zubizarreta 2015). The final column (ESS) reports the effective sample size, which is a measure of information loss compared to unweighted data. In all cases the ESS is smaller than the raw sample sizes, indicating that the bias reduction provided by balancing with the weights comes at the expense of efficiency. We selected SBW over a range of other options because it improves balance (see below) while maintaining larger effective sample sizes compared to the other methods.

A4 Covariate Balance

We consider two measures of covariate balance. Absolute standardized mean differences are commonly used because they are intuitive and perform well in simulation studies (Franklin et al. 2014). Smaller differences indicate stronger balance, but standardization promotes balance evaluation with a decision criterion. Typical values range from 0.10 to 0.25 (e.g., Ho et al. 2007; Harder et al. 2010). We consider a stricter criterion of 0.10 in our evaluations. Additionally, we com-

Table A4: Summary Statistics of Balancing Weights

Data	Treatment	Min.	Max.	Mean	SD	ESS
Cosponsorship centrality (All legislators)	Online action	1e−8	8.655	1	1.192	1,236.965
	Offline action	1e−8	7.492	1	1.207	1,397.282
	In Washington, D.C.	1e−8	16.420	1	1.826	684.660
Network distance (average proximity)	Online action	1e−8	9.228	1	1.207	1,219.524
	Offline action	1e−8	7.136	1	1.191	1,411.546
	In Washington, D.C.	1e−8	16.906	1	1.812	692.962
Elections	Online action	1e−8	9.592	1	1.274	1,221.049
	Offline action	1e−8	7.736	1	1.182	1,513.900
	In Washington, D.C.	1e−8	16.427	1	1.692	814.916
Campaign funds	Online action	1e−8	9.476	1	1.275	1,219.624
	Offline action	1e−8	7.657	1	1.182	1,514.167
	In Washington, D.C.	1e−8	16.462	1	1.694	813.361

Note: Cell entries report the minimum, maximum, mean, standard deviation, and effective sample size (ESS) of each set of estimated balancing weights. The unweighted sample sizes are 2,926 (cosponsorship centrality and network distance) and 3,116 (elections and campaign funds).

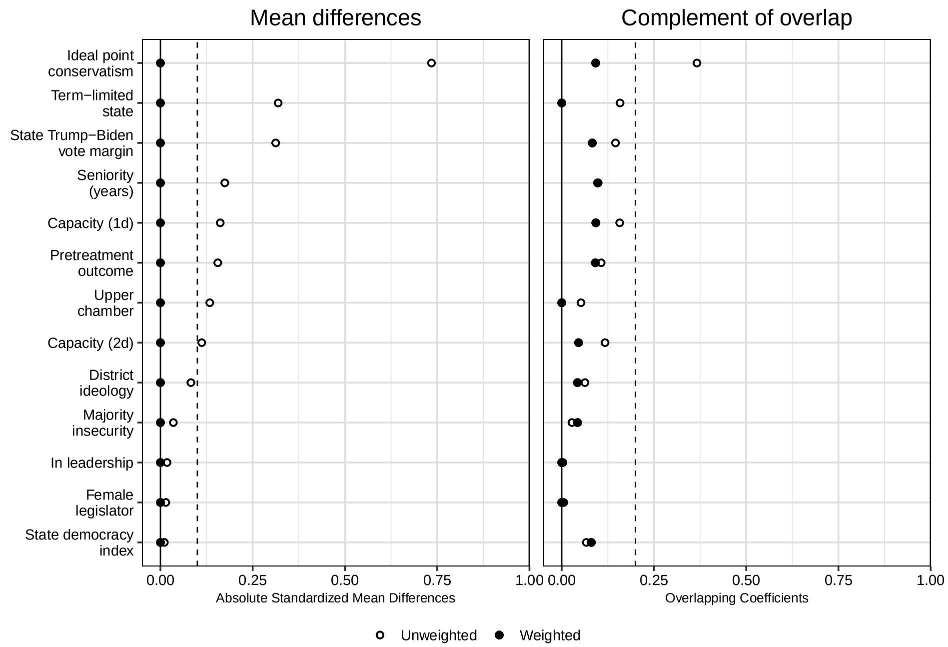
pute the complement of overlap coefficient. This statistic measures the proportion of total density between the two distributions that does *not* overlap (see Franklin et al. 2014). It provides an assessment of balance across the full covariate distribution. Lower values indicate better balance; a value of zero would imply identical distributions. We use a criterion of 0.20 to indicate balance with this measure.

Figures A4–A6 present these measures for all covariates in each of our dataset-treatment variable combinations before (open circles) and after (closed circles) weighting. To conserve space, the graphs do not present every outcome variable used in each dataset, but those additional results are essentially identical (see the replication materials). The graphs indicate that substantial imbalance exists in the raw data. The mean differences especially suggest large differences between treated and control legislators on variables such as ideal point conservatism, term limited states, and Trump-Biden vote margin.

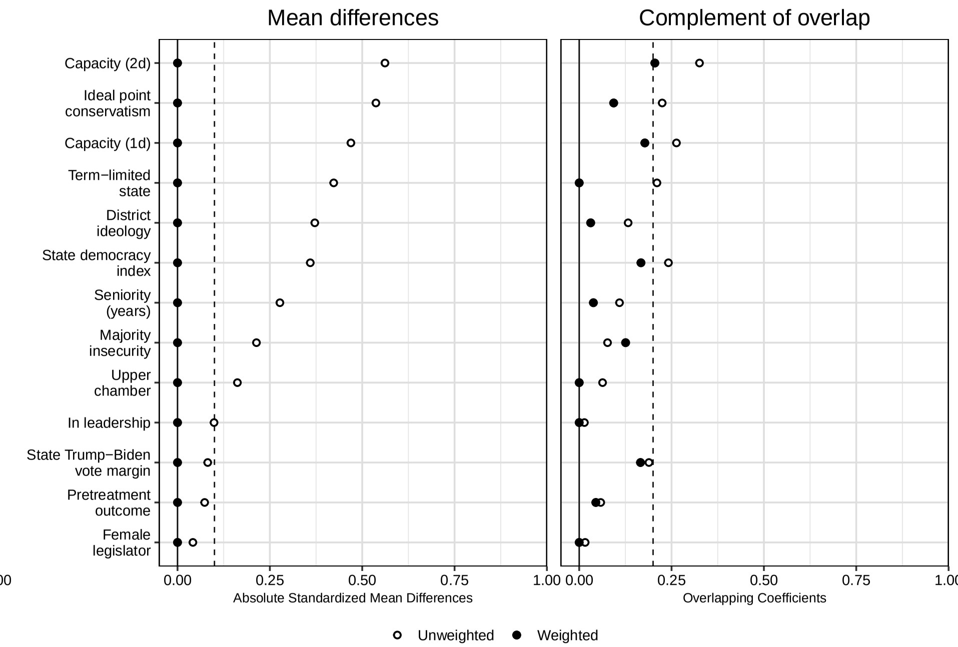
Our implementation of the SBW algorithm was designed to reduce the average covariate differences to zero, which the left graphs within each panel of Figures A4–A6 verifies. In other words, weighting eliminates the mean difference imbalances completely. And while the weights were not optimized specifically to minimize complement of overlap, the right graphs consistently show that balance improves on that measure as well. Thus, weighting is highly effective in controlling any confounding influence of our observed covariates. This finding bolsters our confidence in the validity of the estimated treatment effects.

Figure A4: Covariate Balance in the Elections Data

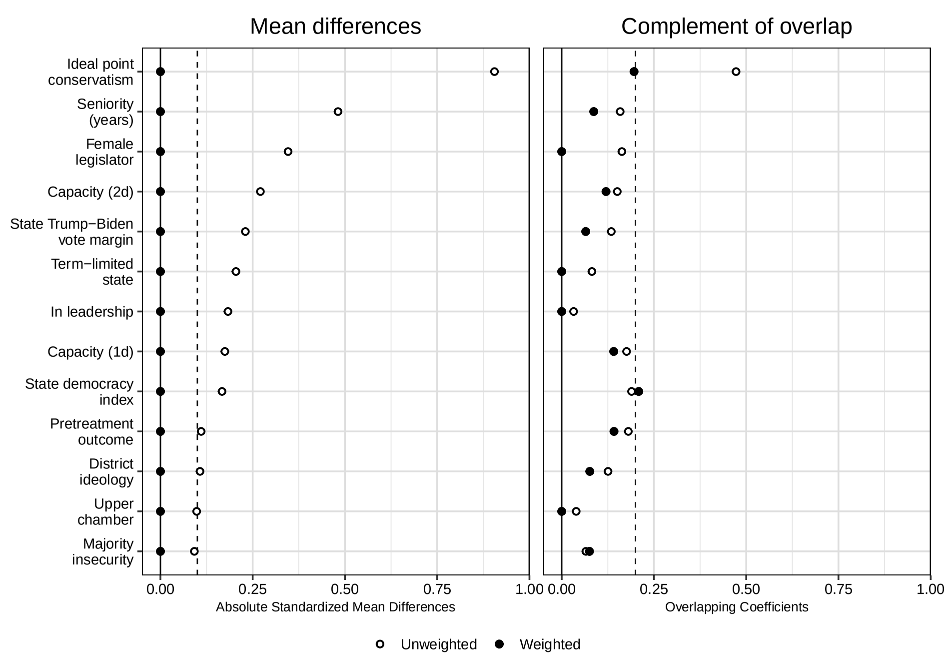
(a) Online anti-election action



(b) Offline anti-election action



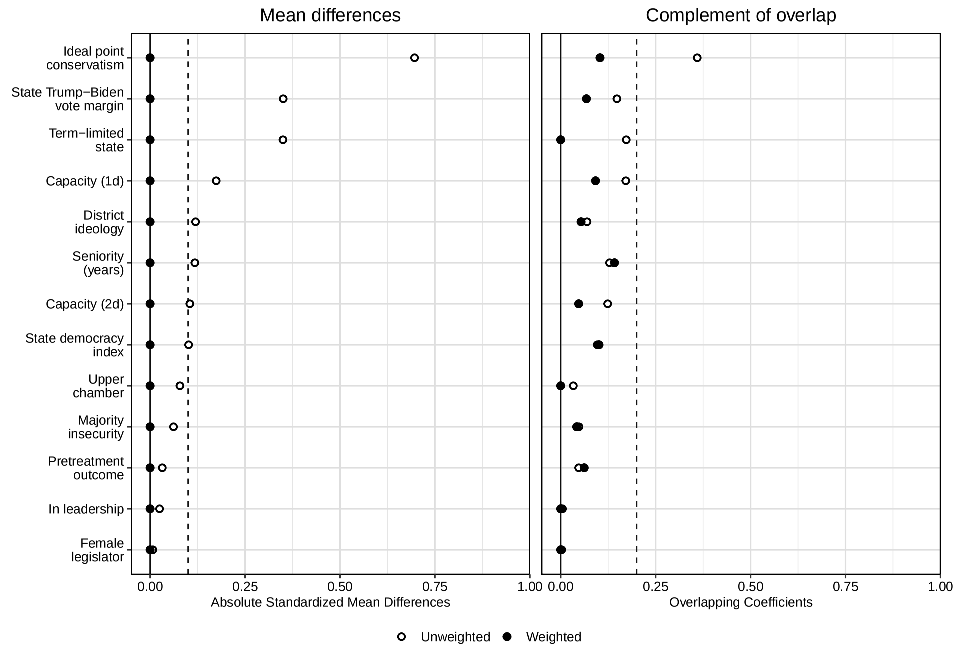
(c) In Washington, D.C.



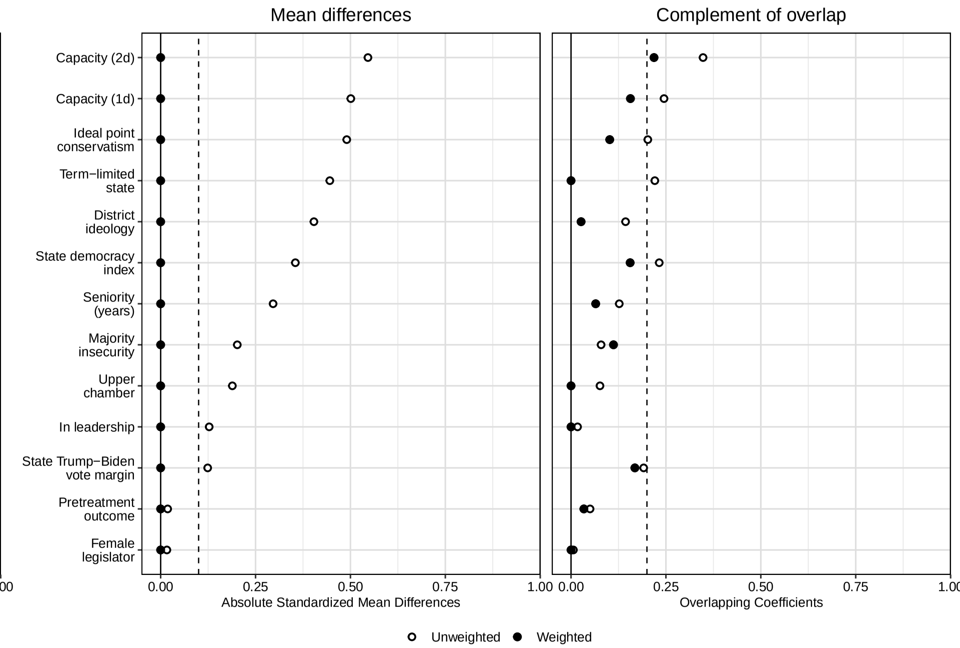
Note: The graphs report absolute standardized mean differences and complement of overlap statistics before and after weighting with covariate balancing weights from Zubizarreta's (2015) SBW algorithm.

Figure A5: Covariate Balance in the Cosponsorship Centrality Data

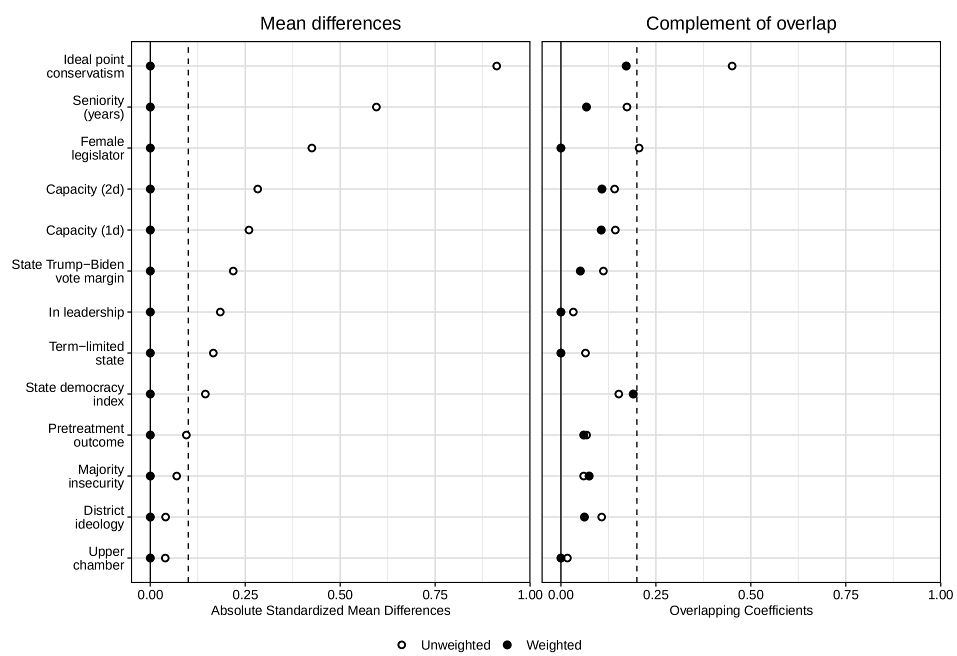
(a) Online anti-election action



(b) Offline anti-election action



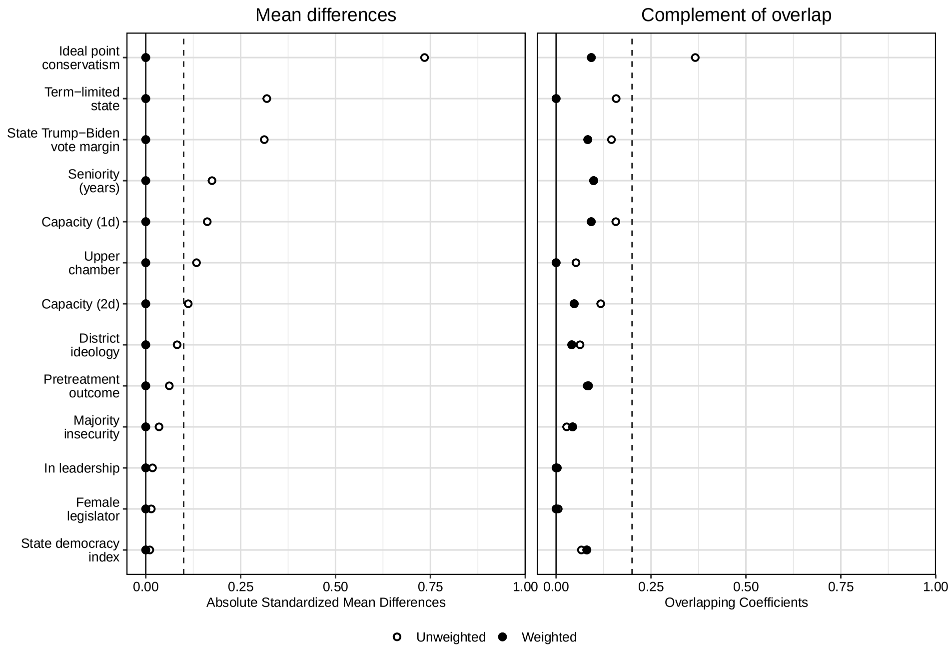
(c) In Washington, D.C.



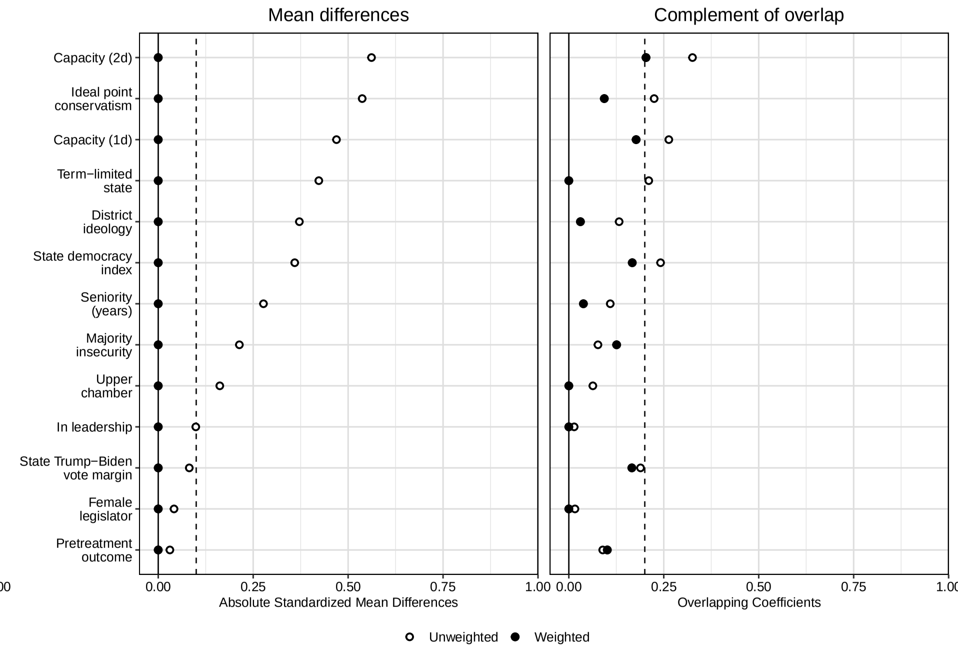
Note: The graphs report absolute standardized mean differences and complement of overlap statistics before and after weighting with covariate balancing weights from Zubizarreta's (2015) SBW algorithm. The outcome variable is eigenvector centrality percentile rank in the all legislators network.

Figure A6: Covariate Balance in the Campaign Funds Data

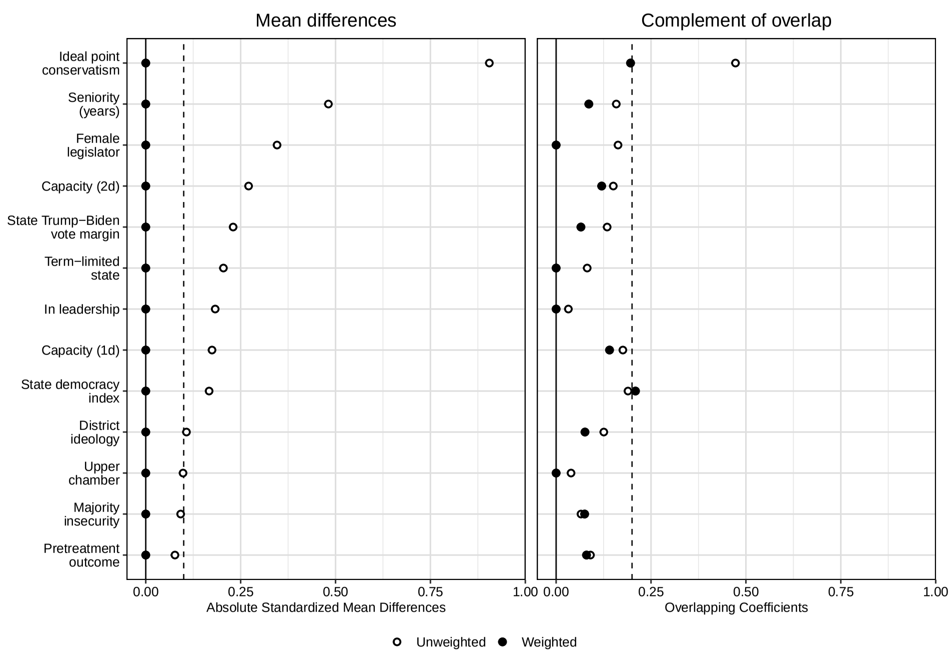
(a) Online anti-election action



(b) Offline anti-election action



(c) In Washington, D.C.



Note: The graphs report absolute standardized mean differences and complement of overlap statistics before and after weighting with covariate balancing weights from Zubizarreta's (2015) SBW algorithm.

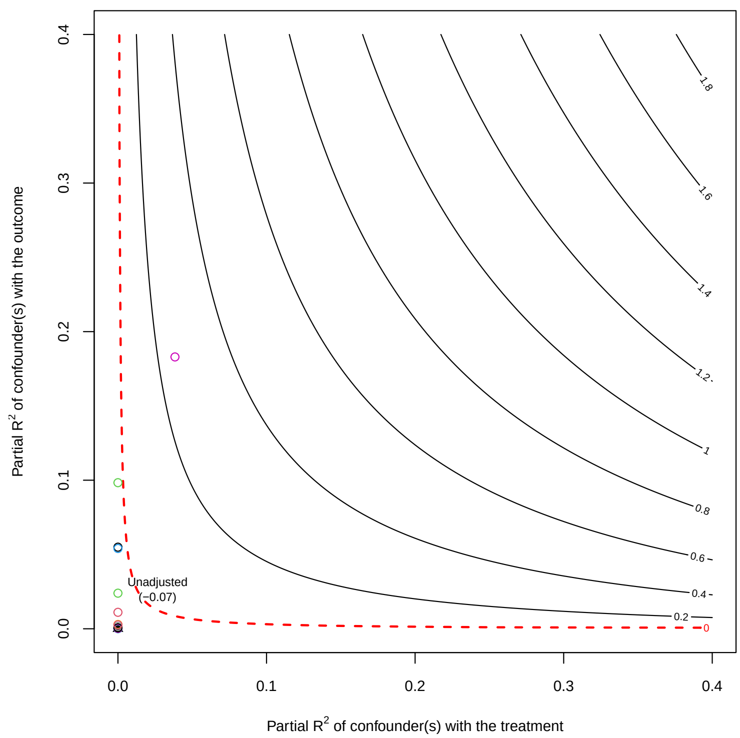
A4.1 Sensitivity to Omitted Variables

As noted in the main text, our identification of the effects of anti-election actions relies on a selection-on-observables strategy. Because treatment assignment was driven entirely by legislators' own choices, we must control for differences between those who chose to take action(s) or not. Our pretreatment outcomes, list of covariates in Table A1, and balancing weights are intended to accomplish that objective. However, even after adjusting for these factors, a "hidden" confounder that we did not measure may still bias our results. How strong would such an omitted variable need to be such that the estimates we report would reduce to zero (i.e., no effect)?

Cinelli and Hazlett (2020) provide a means of answering that question through a sensitivity analysis. Their methodology permits the analyst to examine a treatment effect estimate under a hypothetical scenario in which the assumption of no omitted variables is in doubt. Specifically, consider two quantities: (1) the explanatory power of a hypothetical confounder (or multiple confounders) on the treatment and (2) the confounder(s) explanatory power on the outcome variable. As either quantity increases, the estimated effect would also change. Figure A7 graphs these quantities and the implications for the treatment effect using as an example the total effect of presence at the U.S. Capitol on centrality in the bill cosponsorship network (all legislators).

The starting point is the (assumed) unconfounded estimate of -0.07 at the origin of the graph. The contours demonstrate the implications of relaxing the assumption of no omitted variables. As the first quantity (the confounder's explanatory power of the treatment) increases along the x-axis and/or the second quantity (explanatory power of the outcome) increases along the y-axis, the estimate weakens. It becomes zero at the dashed red line, then positive (counter to our original finding) with additional increases to either axis. The open circles indicate the explanatory powers of the observed covariates (R^2 values from bivariate regressions). In this case, most of the covariates do not explain the treatment well, but have some explanatory power of the outcome. These points fall left of the dashed red line, so omitted variables similar to our existing covariates would not change the sign of the estimated effect. The exception to this pattern is the state fixed effects, estimated jointly and shown by the purple circle at (0.04, 0.18), which are adjusted-R^2 values.

Figure A7: Sensitivity Analysis of the Estimated In Washington, D.C. Treatment Effect on Bill Cosponsorship Network Centrality to Omitted Variables

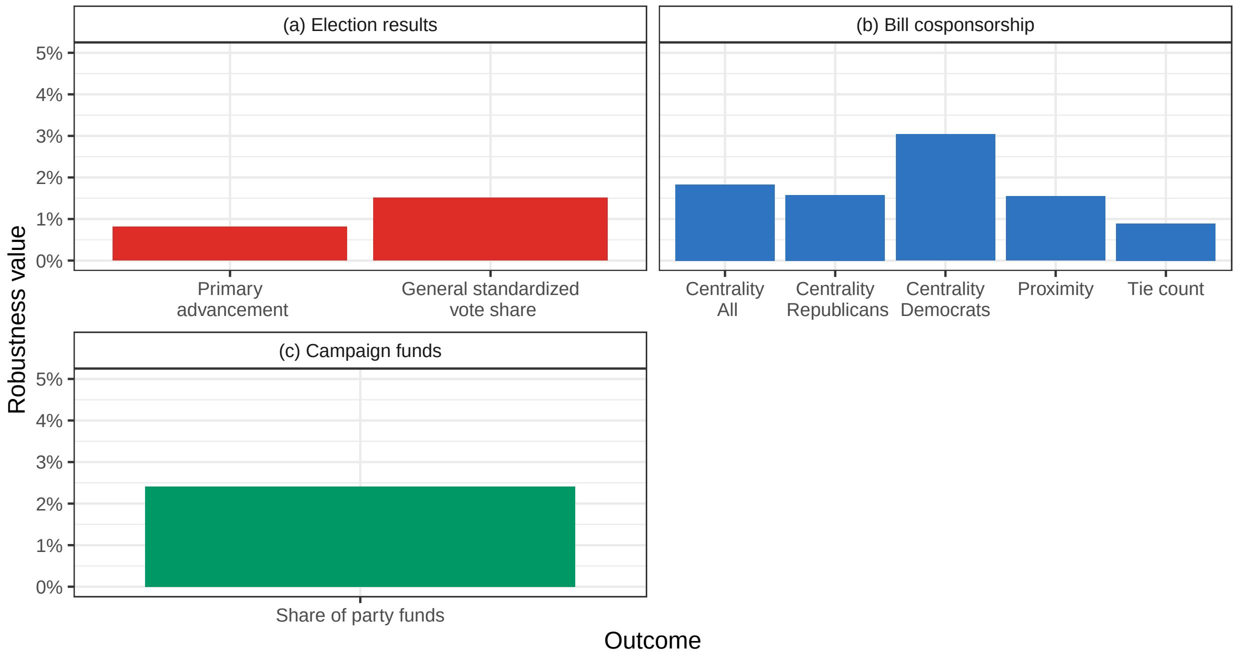


Note: The graph presents change in the estimate treatment effect as the explanatory power of a hypothetical confounder (or multiple confounders) on the treatment (x-axis) and on the outcome (y-axis) increases. The dashed red line indicates the point at which the treatment effect is zero. The open circles indicate the explanatory powers of the observed covariates.

Figure A8 summarizes the sensitivity analysis for all of the In Washington, D.C. effects.[2] Specifically, the graphs report a robustness value that describes how much an unobserved confounder that is orthogonal to all of the observed covariates would need to explain in the residual variance of the treatment *and* outcome to bring the estimated treatment effect to zero (Cinelli and Hazlett 2020). These values generally range 1–3%, which initially suggests some sensitivity. However, the observed covariates represent a comprehensive list and individually do not explain a great deal of treatment variance (see Figure A7). Identifying a covariate that we did not include and is more powerful than those we did include is possible, but would likely be difficult. Thus, we interpret these results as suggesting a moderate level of robustness to hidden confounders.

[2]We omit the other treatment variables' effects for simplicity and because they are generally small in magnitude.

Figure A8: Sensitivity Analysis of the Estimated In Washington, D.C. Treatment Effects to Omitted Variables



Note: The graphs present robustness values describing how much an unobserved confounder—orthogonal to the observed covariates—would need to explain in the residual variance of the treatment *and* outcome to bring the estimated treatment effect to zero (Cinelli and Hazlett 2020).

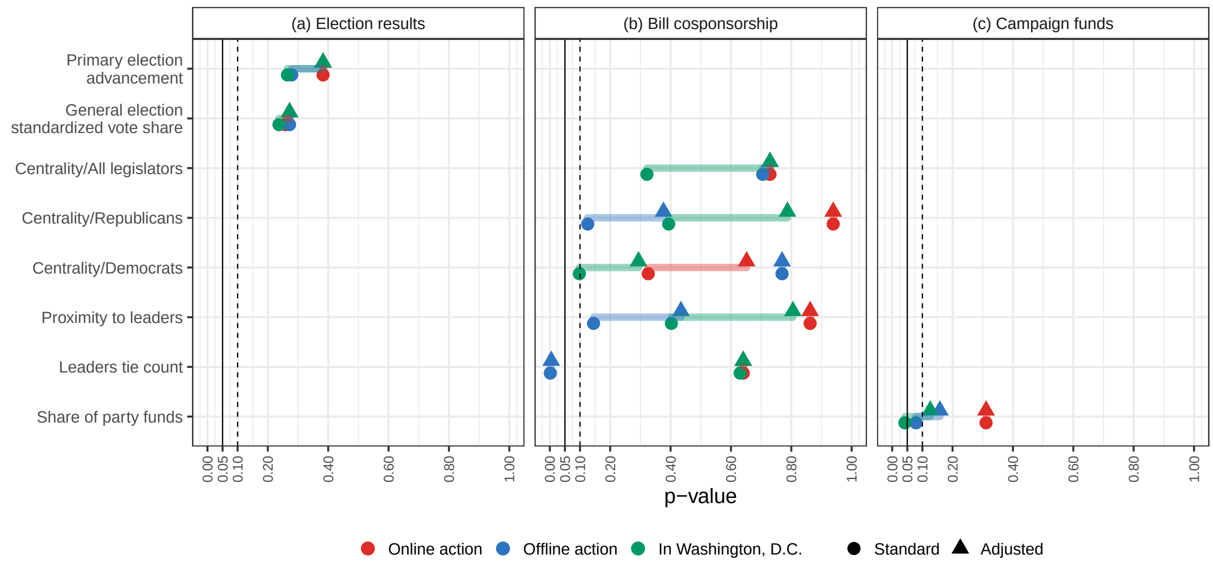
A5 Adjusting for Multiple Comparisons

Figure A9 reports p-values associated with the treatment effect estimates in Tables 1–5 (testing a null hypothesis of zero).[3] We plot the standard p-value (circles) as well as an adjusted version (triangles) that accounts for the family-wise error rate (FWER). This latter category guards against false positives that could arise from multiple comparisons. With three treatments and multiple outcome variables, our design effectively yields multiple chances to observe a statistically significant estimate in the direction of one of our hypotheses.

Consistent with Tables 1–5, the results indicate that only a few estimates are statistically distinguishable at the 0.05 (or 0.10) level. The FWER adjustment reduces that number. However, as we discuss in Section 5, we view the treatment effects as substantively significant and relevant even

[3]The graphs include total effects only for the estimates from sample selection models.

Figure A9: Standard and Adjusted Treatment Effect p-values



Note: The graphs present p-values for the treatment effect estimates reported in the main text (total effects for estimates from sample selection models). Circles represent standard p-values and triangles represent p-values adjusted for the family-wise error rate.

with a lack of statistical significance.

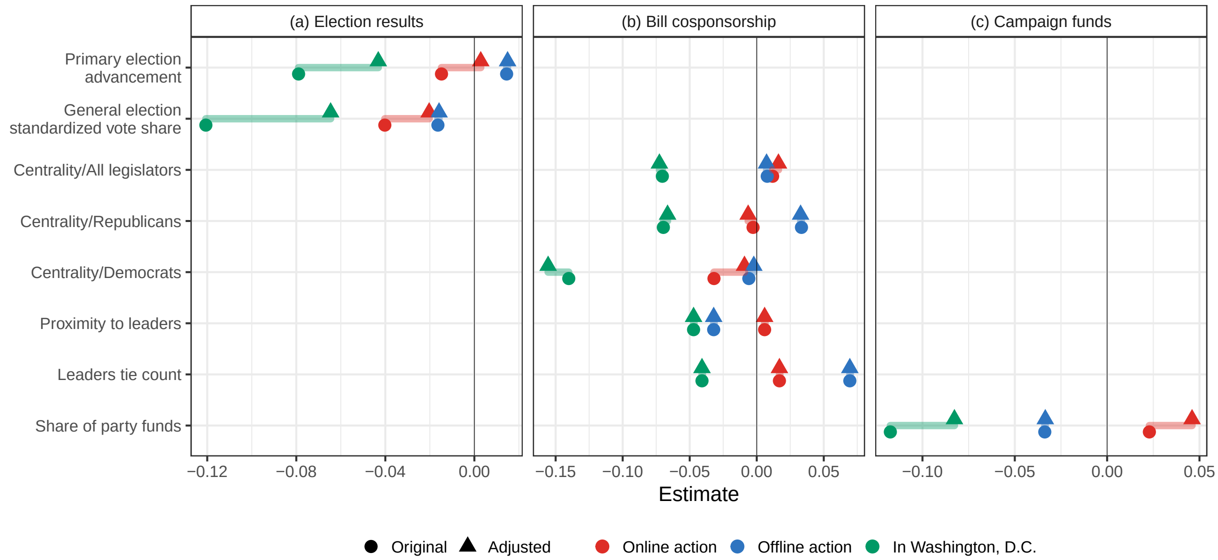
A6 Controlling for the Other Treatment Variables

The main text reports treatment effect estimates without adjustments for the other treatment variables. The motivation for that decision is that for some legislators, one variable could be post-treatment to another. For example, online action taken after January 6th would be post-treatment for attendance at the Capitol insurrection or offline action taken on that day. However, as a robustness check we consider specifications with those adjustments here. Figure A10 presents all of the treatment effect estimates with (circles) and without (triangles) controls for the other two treatment variables in the outcome models.[4]

Overall, the results are similar with and without the other treatments included. In a few instances the estimates get stronger in magnitude (e.g., campaign funds, online action) and in others they are weaker (election results). But the signs of almost all of the estimates never change and

[4]To conserve space and simplify this analysis, we omit confidence intervals and present only the total effects from the sample selection models. See the replication materials for the complete set of estimates.

Figure A10: Estimated Treatment Effects Controlling for the Other Treatment Variables



Note: The graphs present treatment effect estimates with (circles) and without (triangles) controls for the other two treatment variables in the outcome models.

most of the weaker estimates are still reasonably large in substantive terms. Thus, our general conclusions are robust to controls for the different types of anti-election actions we examine here.

A7 Heterogeneous Treatment Effects

As we mention in the main text, an advantage of studying state legislators is the potential for examining the role of contextual effects, such as institutional variation across state legislatures. The states have a long history of experimenting with institutional design, which could hold implications for accountability in their legislatures (e.g., Harden and Kirkland 2021; Olson 2025). In this section we consider heterogeneous treatment effects based on two factors that might be relevant to our case study. Specifically, we examine whether the timing of state election cycles (e.g., Anzia 2013) and/or the security of the majority in legislative chambers (Lee 2016; Hinchliffe and Lee 2016) moderate our estimated effects.

A7.1 Election Timing

We first investigated the possibility of treatment effect heterogeneity due to variation in the timing of elections. Specifically, in a variant on H1 we hypothesized that the strongest electoral accountability effect would emerge in elections conducted soon after the anti-election behavior occurred. In contrast, elections taking place later would produce weaker effects. The underlying logic is that the motivation for accountability may decay over time, especially as new issues and events emerge in state and national politics.

H1c The negative association between anti-election actions and electoral success becomes weaker (i.e., moves toward zero) as time between the action and the legislators' next election increases.

Our data include post-treatment election results from 2021 (4% of legislators), 2022 (89%), and 2023 (7%). To test H1c, we interacted our treatment variables with an indicator for elections occurring in 2021 (coded 1) or later (coded 0).[5] We focus on the total effect of treatment in both the primary and general election analyses. These estimates appear in Figure A11.

For primary elections (panel a), our results indicate minimal differences for online and offline action by election year. In both cases, the estimates are close to zero. The effects of presence at the Capitol show more heterogeneity, although not in the pattern we expected. Both estimates are negative, in line with H1. But the effect in 2022 or 2023 is much larger in magnitude than the 2021 effect. The confidence intervals are wide enough that neither estimate is distinguishable from the other (or from zero). But the pattern contradicts our expectation in H1c.

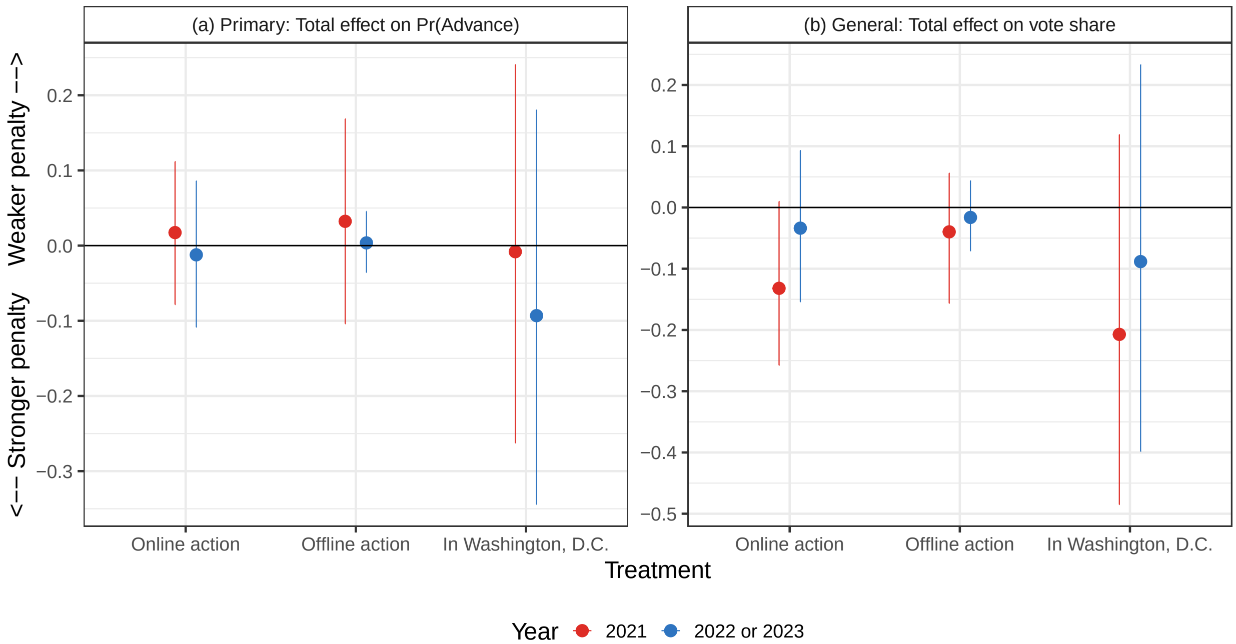
Moving to the general election results in panel (b), we find estimates more in line with our hypothesis. For all three treatment variables, the effect is stronger in magnitude (a larger decrease in standardized vote share) in 2021 compared to 2022/2023. Again, the estimates are not statistically different from each other. But the general pattern is more consistent with our expectation of decay in the accountability effects over time.

A7.2 Majority Security

Next, we expect that the institutional backlash towards anti-election actions could be conditional on whether partisan control of the institution is closely contested. The Republican Party's sanctions of its member may vary based on whether that member is needed to maintain control of

[5] We collapsed 2022 and 2023 due to statistical identification issues arising from data sparsity in 2023 elections.

Figure A11: Estimated Total Effects of Treatment on Primary Election Advancement and General Election Standardized Vote Share by Election Year



Note: The graphs present estimates of the total effects of treatment and 95% confidence intervals by legislators' first election year after January 6th, 2021. The outcomes are bill cosponsorship network eigenvector centrality in 2021 for graphs containing all legislators (panel a), Republicans only (panel b), and each Republican legislator graphed with Democrats only (panel c).

the chamber. Accordingly, we expect that the relationships posited in H2 weaken when the party is under threat to lose its majority status (majority parties) or poised to take control of the chamber (minority parties). As Lee (2016) shows, when the likelihood of changing majority control of Congress is high, the parties must act carefully and strategically to coalesce support within their ranks, turn public preferences against the other party, and maintain (majority) or win (minority) control.[6] Keeping all (or nearly all) individual members on board with policy proposals that are important to the party's agenda is pivotal under these circumstances.

Simply put, lawmakers in states with insecure majorities who have taken anti-election actions are needed to ensure the long-term survival of their party, despite their liabilities. In such a case, we expect that the hypothesized effects in H2 move toward zero, but only with respect to cosponsorship within the legislator's own party. In contrast, if one party is likely to dominate for the foreseeable

[6] Hinchliffe and Lee (2016) extend this logic to the context of state legislatures.

future, such within-party consolidation becomes comparatively less crucial and the in-party has more flexibility to punish anti-democratic members without risking its chances of maintaining or winning control of the chamber. Additionally, we expect that out-party legislators take advantage of any chance to exact punishment on the opposition. Thus, they are just as likely to penalize anti-election actions of a legislator from the other party under secure *and* insecure majorities.

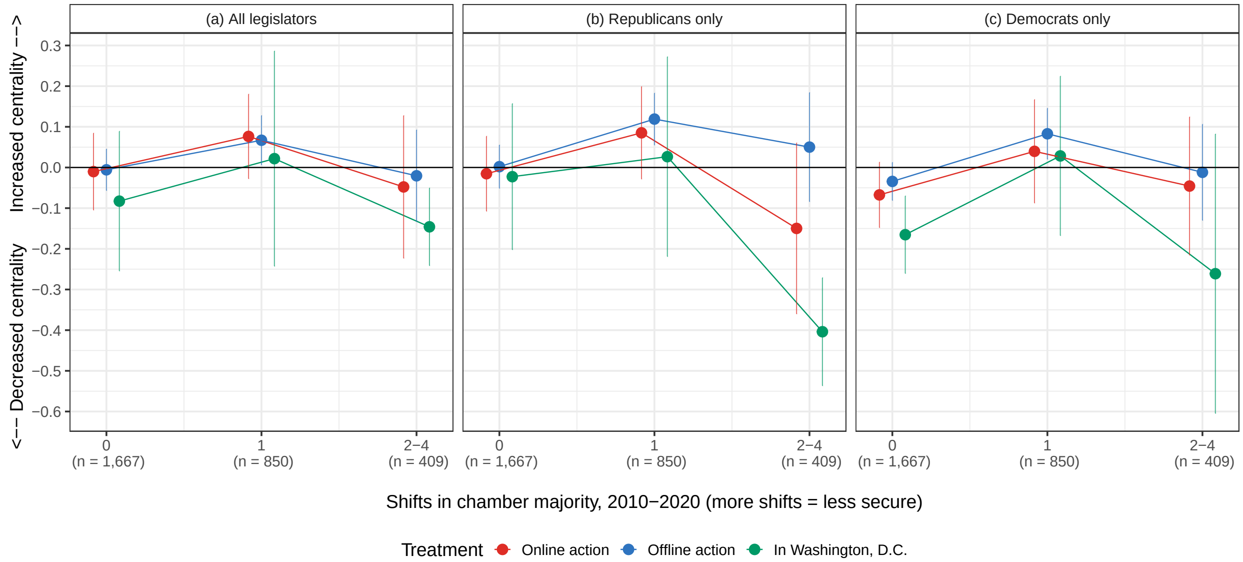
H2c The negative associations between anti-election action and (1) cosponsorship centrality within the in-party network and (2) proximity to party leaders become weaker (i.e., moves toward zero) as the security of the majority party in the chamber decreases.

To test this expectation, we interacted our treatment variables with our measure of shifts in the chamber majority in the decade prior to 2020. Figure A12 reports the results for all three versions of our cosponsorship centrality outcomes. The majority shifts variable ranges 0–4, but due to data sparsity we graph results for no shifts in the last decade, one shift, and two or more shifts. Positive (negative) values on the y-axes indicate treatment effects that increase (decrease) legislators' centrality percentile rank. If H2c is correct, we would expect to see no variation by majority shifts among Democrats and the strongest negative effect among Republicans when majority shifts is zero. The pattern when including all legislators would fall somewhere in the middle of the partisan estimates.

Figure A12 shows minimal support for our hypothesis. The pattern of estimates is roughly similar across all outcome-treatment combinations. The change from no prior majority shifts to one corresponds with an increase in the estimates, but then they decrease moving from one to two or more shifts. We find that the strongest punishment of anti-election behavior occurs with *insecure* majorities in the Republican outcome measure (panel b), which contradicts our theoretical expectation.

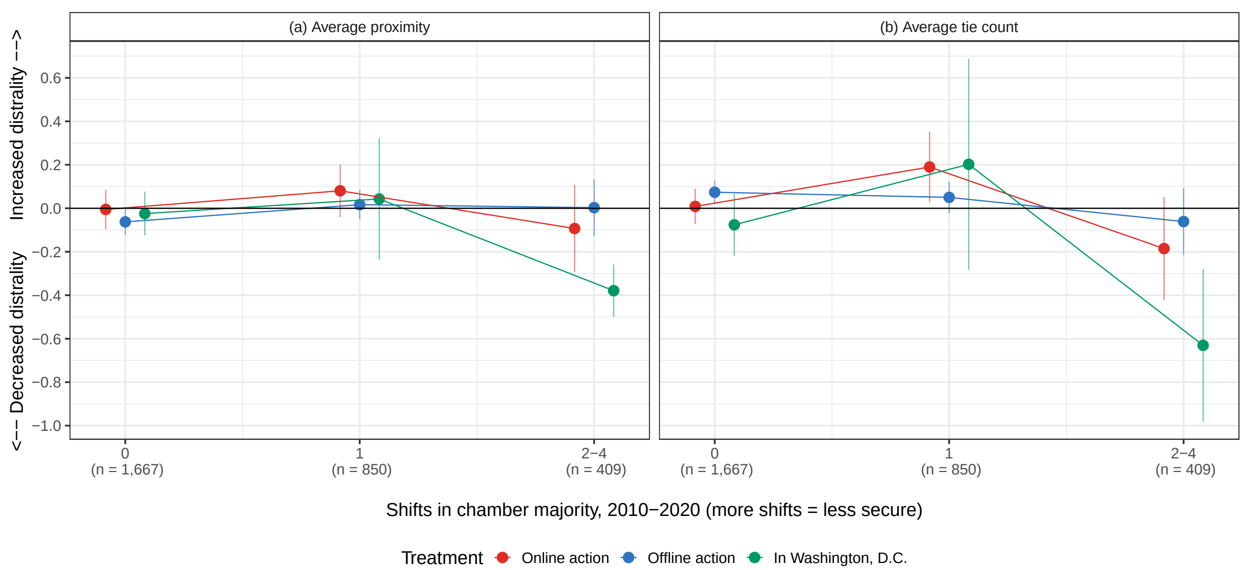
Figure A13 graphs the same quantities for the network distance to party leaders variables: average proximity (panel a) and average tie count (panel b). The results show largely the same pattern as with the centrality outcomes. The effects increase slightly from zero to one shift, then drop from one to two or more. Thus, we continue to find limited support for H2c.

Figure A12: Estimated Treatment Effects on Bill Cosponsorship Centrality by Shifts in Chamber Majorities, 2010–2020



Note: The graphs present treatment effect estimates and 95% confidence intervals by shifts in chamber majority status over the decade prior to treatment. The outcomes are bill cosponsorship network eigenvector centrality in 2021 for graphs containing all legislators (panel a), Republicans only (panel b), and each Republican legislator graphed with Democrats only (panel c).

Figure A13: Estimated Treatment Effects on Bill Cosponsorship Proximity to Party Leaders by Shifts in Chamber Majorities, 2010–2020



Note: The graphs present treatment effect estimates and 95% confidence intervals by shifts in chamber majority status over the decade prior to treatment. The outcomes are average network proximity to the party leadership (panel a) and average tie count with party leaders (panel b).

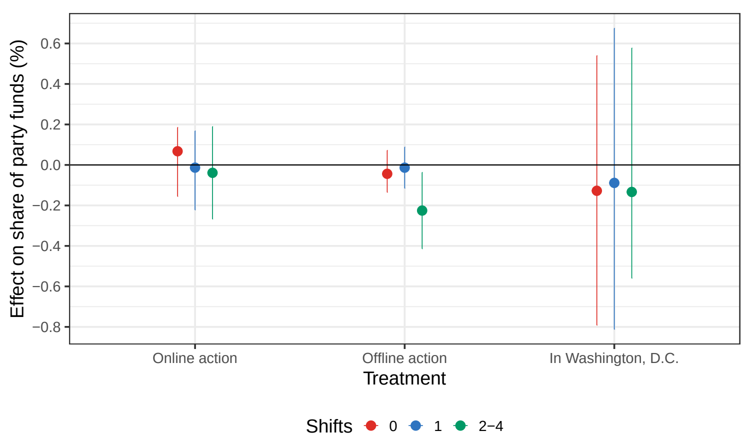
A7.2.1 Campaign funds

Finally, we also tested for heterogeneity by majority security in the effects on party campaign funding support. The theoretical logic mirrors that of H2c: we expect the strongest negative effect in chambers where the majority is secure, then a weakening of that effect as the number of previous majority shifts increases.

H3c The negative association between anti-election action and campaign funding support becomes weaker (i.e., moves toward zero) as the security of the majority party in the chamber decreases.

Our empirical strategy to test this hypothesis was again to interact the treatment variables with the majority shifts covariate. Figure A14 presents the treatment effect estimates by majority shifts. Similar to our test of H2c, we find no support for our expectation. The estimates generally display minimal heterogeneity and the variation that does appear does not reflect the pattern posited by H3c. In short, majority security does not appear to condition the accountability effects of anti-election actions.

Figure A14: Estimated Treatment Effects on Share of Party-Donated Campaign Funds by Shifts in Chamber Majorities, 2010–2020



Note: The graphs present treatment effect estimates and 95% confidence intervals by shifts in chamber majority status over the decade prior to treatment. The outcome is the share of the party's funds received in the next election cycle after January 6th, 2021 (scaled 0–100%).

A8 Text Collection & Analysis

Here we provide additional details on our text collection and analysis procedure.

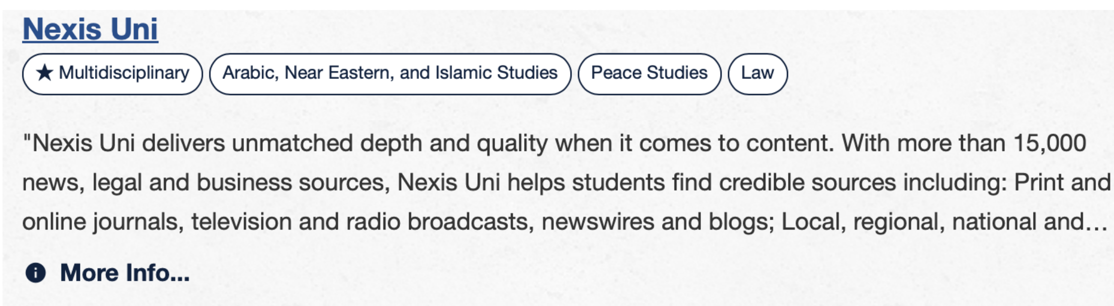
A8.1 Procedure for Text Collection

The following text reproduces the procedures we employed for text collection.

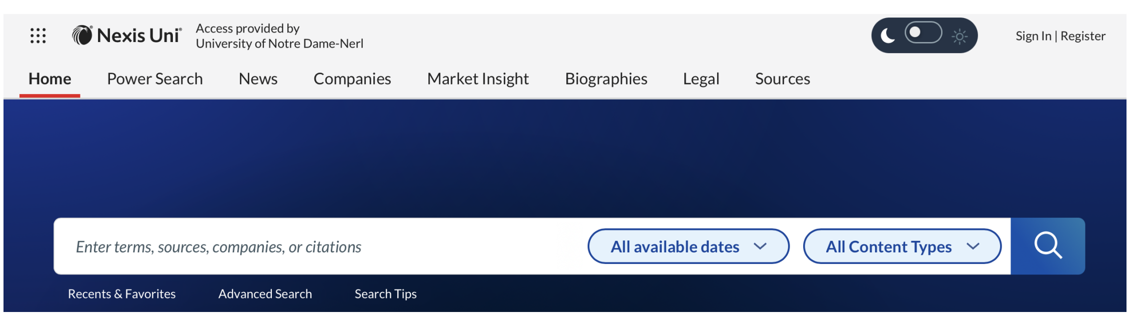
1. You will be working with multiple tabs in your browser. First, within our shared Dropbox folder, access the folder titled "Text_Folder" and within that, the folder titled "YOURNAME_candidates." Within this subfolder, open the files "YOURNAME_candidate_names" and "YOURNAME_text."

2. In another tab, access [University Name]'s research databases.

3. Click on the letter "N" and scroll down until you see NexisUni. You will use this database to collect all of the news documents. After clicking Nexis Academic, you will be redirected to [University Name]'s Okta page. Complete the authentication process.



4. After completing the authentication process, you should see the following interface:



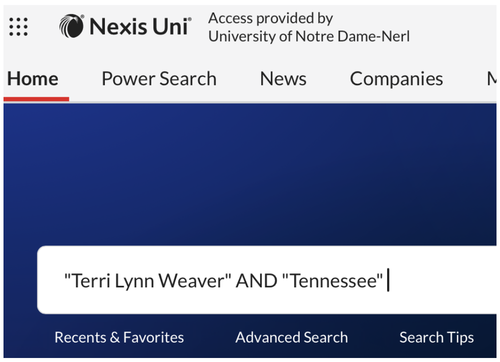
5. Click on the “Sign In/Register” option in the top right corner of the screen. You will need to create an account with your [University Name] email account (otherwise, when downloading documents, you can only download small batches at a time because the access is restricted to a guest user).

6. Reference the “YOURNAME_candidates_names” to determine which legislators you will be collecting news coverage data on. Before you can collect news documents for the assigned legislators, you must first determine the end-date of the timeline search criteria. To do this, you will need to go to Ballotpedia. From here, paste each legislator’s Ballotpedia id from the “YOURNAME_candidates_names” file to find election details. Once you determine the general (primary election date is fine if the individual did not move on to the general election, or a general election has not yet been held) election date, add two (2) days to this date and paste it into the refine_date_2 cell. Two additional days are added to account for follow up news coverage of the election. For example, if the general election is held on November 8, 2022, the refine_date_2 would be 11/10/22. If you find that the legislator you are collecting election data for did not run for any elective office again, use the date for the primary election that they *would* have been a candidate in. If an individual does run for elective office, but not for the same seat (e.g., a legislator runs for a statewide seat in the subsequent election), use that election date (remembering to add 2 days to the date reported in refine_date_2).

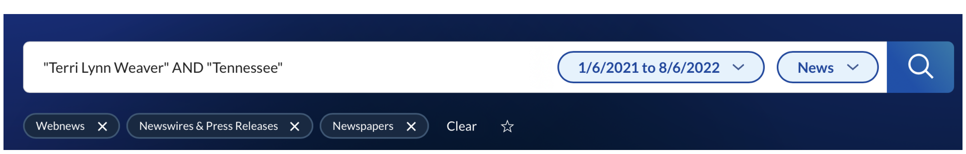
7. In the search bar, for each individual legislator, you will type the individual’s first and last name, as well as their state’s name (e.g., “Terri Lynn Weaver” AND “Tennessee”). In the instance that the legislator’s Ballotpedia name is likely a first name with a common nickname, you will want to include it as a search parameter (e.g., Matt Maddock and Matthew Maddock). It is helpful to do a quick Google search of a legislator if you believe this is likely the case. To account for a legislator being referenced by a first name and nickname, you will need to include both names, separated with an OR Boolean criterion. For example, PA legislator Bradley Roae goes by Brad, though his name and Ballotpedia name both report

Bradley. In these instances, add an additional OR command (e.g., “Bradley Roae” OR “Brad Roae” AND “Pennsylvania”).

(a) **Important Note:** When typing these search strings into the search bar, you will be making use of “and” commands outside of quotation marks. For example, if you were collecting news coverage for Terri Lynn Weaver of Tennessee, you would look up her news coverage materials by the following specification:

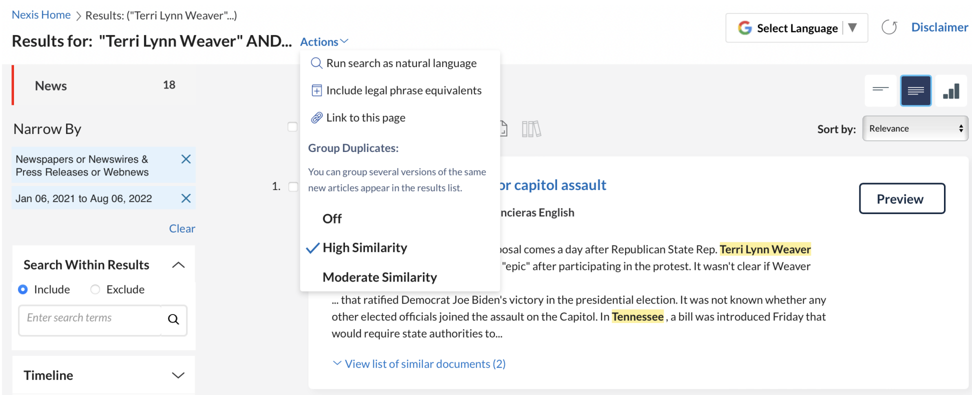


8. After typing in the relevant search strings, separated by the appropriate Boolean criteria, you will also need to refine the search—after all, we are interested in news coverage on and after January 6th. To do this, change the default timeline from “Previous Two Years” in the search bar to “Custom Dates.” From here, you will narrow the search by **January 6, 2021 until the date of their next general election after the event (unless they were defeated in the primary election, in which case the primary date is used**, the value you imputed for refine_date_2. Additionally, we want to constrain the types of hits received. Change the default option of “All Content Types” to **“News,” then “Newspapers,” “Newswires & Press Releases,” and “Webnews.”** Then, press search.

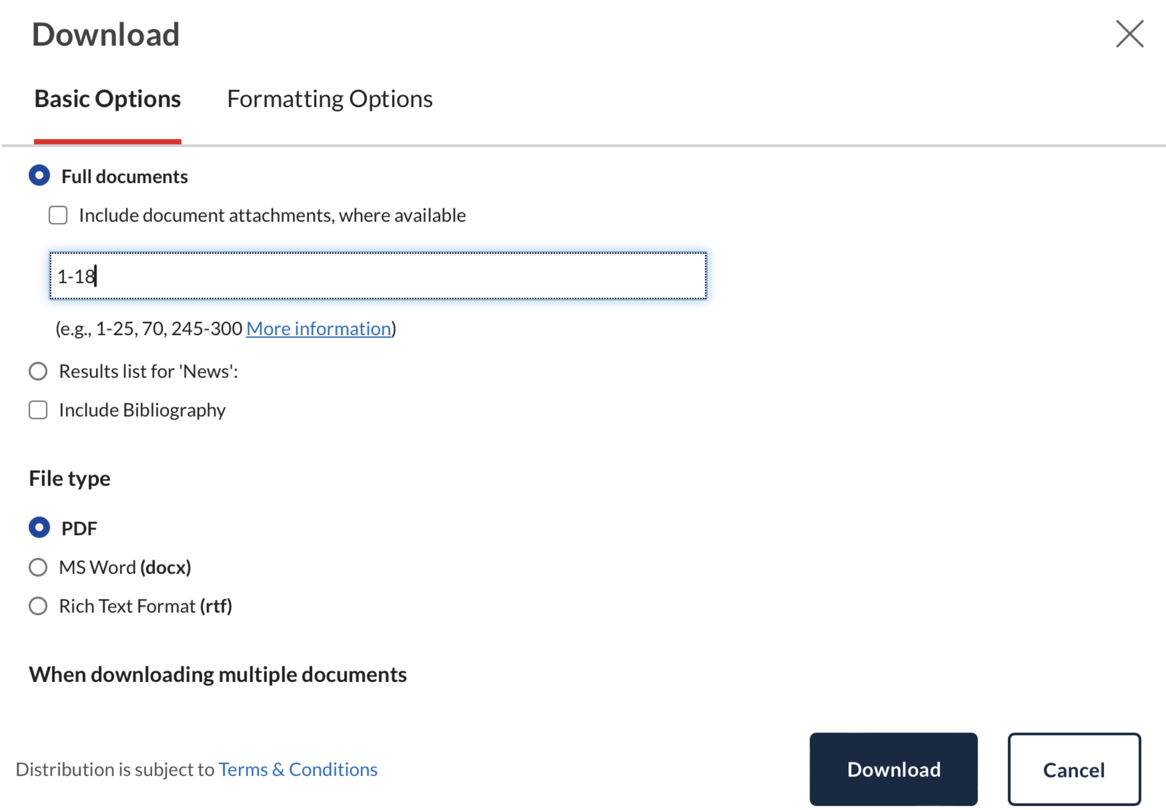


9. Once search results are yielded, we want to ensure that duplicates are not kept. In the top left corner of you should see an “Actions” option beside of the search criteria. Click the drop

down option and then choose "High Similarity." This is known as a sticky setting, meaning that within a session of working in NexisUni (until prompted to re-authenticate), this setting will be remembered by your machine, such that you should not have to turn off Group Duplicates again. Record the total number of documents (after turning on the High Similarity option) in the num_docs column in "YOURNAME_candidate_names."



Click the download icon. That will prompt you to view the following options:

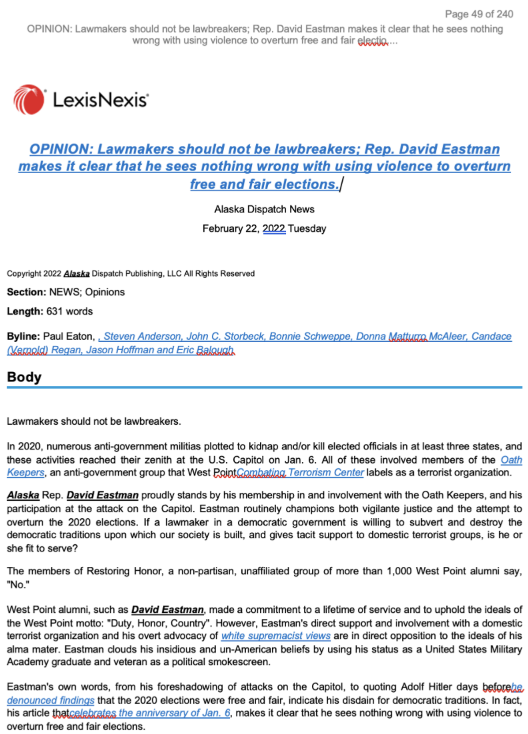


NexisUni permits signed-in users to download 100 news articles within a single export. For Rep.Weaver, her search only yielded 18 results, so you would specify you want to download articles 1-18 in the search bar (If your assigned legislator has over 100, you would do this in batches, such as 1-100, 1-1-150). Next, you will want to make sure that "MS Word" is selected as the file type. This will make copying the text easier. Click download.

10. Once all of the relevant files have been downloaded, you need to organize these articles. For an example on how to do this, within our shared Dropbox folder, see the subfolder titled "david_eastman" housed within the "example_candidates" subfolder in primary folder, "Text_Folder."

11. The text you are copying should be saved in the Excel file called "YOURNAME_text." If you would like to see how the text file should be organized, reference the example_text file found in the following pathway: "Text_Folder" > "example_candidates" > "example_text."

    If an item is an opinion piece, or a letter to the Editor, you will want to type "1" in the opinion_news column. Most opinions and letters are immediately obvious from the article's title, such as in the following example:



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OPINION: Lawmakers should not be lawbreakers; Rep. David Eastman makes it clear that he sees nothing wrong with using violence to overturn free and fair electio....

LexisNexis

OPINION: Lawmakers should not be lawbreakers; Rep. David Eastman makes it clear that he sees nothing wrong with using violence to overturn free and fair elections.

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Section: NEWS; Opinions

Length: 631 words

Byline: Paul Eaton, , *Steven Anderson, John C. Storbeck, Bonnie Schweppe, Donna Matturro McAleer, Candace (Vernold) Regan, Jason Hoffman and Eric Balough*

Body

Lawmakers should not be lawbreakers.

In 2020, numerous anti-government militias plotted to kidnap and/or kill elected officials in at least three states, and these activities reached their zenith at the U.S. Capitol on Jan. 6. All of these involved members of the *Oath Keepers*, an anti-government group that West Point*Combating Terrorism Center* labels as a terrorist organization.

Alaska Rep. ***David Eastman*** proudly stands by his membership in and involvement with the Oath Keepers, and his participation at the attack on the Capitol. Eastman routinely champions both vigilante justice and the attempt to overturn the 2020 elections. If a lawmaker in a democratic government is willing to subvert and destroy the democratic traditions upon which our society is built, and gives tacit support to domestic terrorist groups, is he or she fit to serve?

The members of Restoring Honor, a non-partisan, unaffiliated group of more than 1,000 West Point alumni say, "No."

West Point alumni, such as ***David Eastman***, made a commitment to a lifetime of service and to uphold the ideals of the West Point motto: "Duty, Honor, Country". However, Eastman's direct support and involvement with a domestic terrorist organization and his overt advocacy of *white supremacist views* are in direct opposition to the ideals of his alma mater. Eastman clouds his insidious and un-American beliefs by using his status as a United States Military Academy graduate and veteran as a political smokescreen.

Eastman's own words, from his foreshadowing of attacks on the Capitol, to quoting Adolf Hitler days before*he denounced findings* that the 2020 elections were free and fair, indicate his disdain for democratic traditions. In fact, his article that*celebrates the anniversary of Jan. 6*, makes it clear that he sees nothing wrong with using violence to overturn free and fair elections.

In some cases, though, an opinion (or letter) may not be noted in the title. So, it is good practice to *always* reference the "Section:" information, which always classifies items.

A8.2 Text Preprocessing & Model Estimation

We take the following steps when pre-processing text from our corpus new stories:

- String pattern 'Jan. 6' to 'January 6'
- Remove all numbers
- Remove punctuation
- Remove symbols
- Remove separators
- Remove URLs
- All text to lowercase
- Remove stop words
- Remove tokens with less than three characters
- Remove tokens that appear in the corpus fewer than ten times
- Remove tokens across fewer than ten documents in the corpus

Following Denny and Spirling (2018), we estimate our model with different pre-processing specifications to ensure that our findings are not an artifact of these choices; we find consistent results across these robustness exercises. Though, the quality of our election denialism topic does decline with a larger dictionary of terms. We fit our model with a single keyword topic defined by the following terms: “insurrection”, “capitol”, “january”, “overturn”, “oath”, “steal”, “conspiracy”, “riot”, and “violence.” We allow for 19 non-keyword topics, set iterations to 3000, and thinning to 10. Our results are similarly robust to alternative modeling specifications. The figure below depicts model fit criteria, which is working as expected. Upon model fitting, the list of top terms associated with our keyword election denial topic are as follows (specified keywords in bold): **capitol**, trump, state, election, president, **january**, republican, rally, **oath**, house, **conspiracy**, **violence**, donald, party, **insurrection**, **overturn**.

To demonstrate the robustness of our findings, we replicate model estimation in Figures A15 through A18, dropping one keyword at a time from the above mentioned keyword list. We demonstrate that dropping specific keywords from our keyword-defined topic does not alter our conclusions regarding the estimated prevalence of January 6th topical content across all types of treated and non-treated units. Additionally, in Figures A19 through A22, we replicate our model estimation with varying numbers of non-topic keywords. Once again, our conclusions remain consistent.

Figure A15: Average Document-Level Proportion of Election Denial Topic by Month, DC Treatment with Dropped Keywords

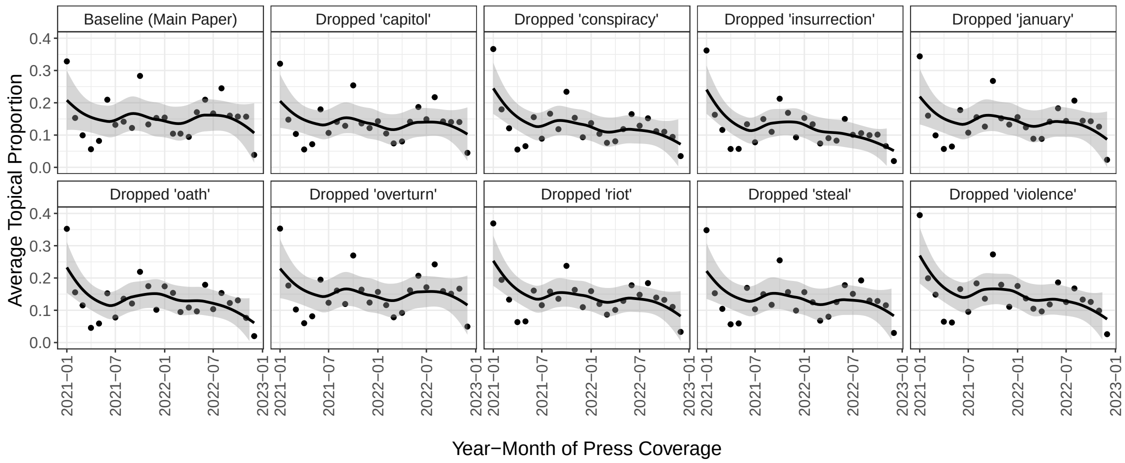


Figure A16: Average Document-Level Proportion of Election Denial Topic by Month, Offline Treatment with Dropped Keywords

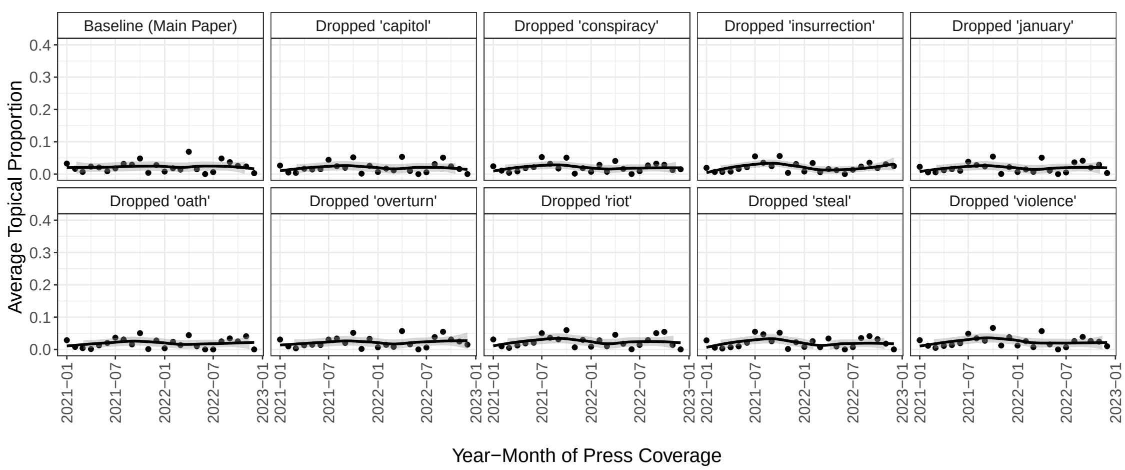


Figure A17: Average Document-Level Proportion of Election Denial Topic by Month, Online Treatment with Dropped Keywords

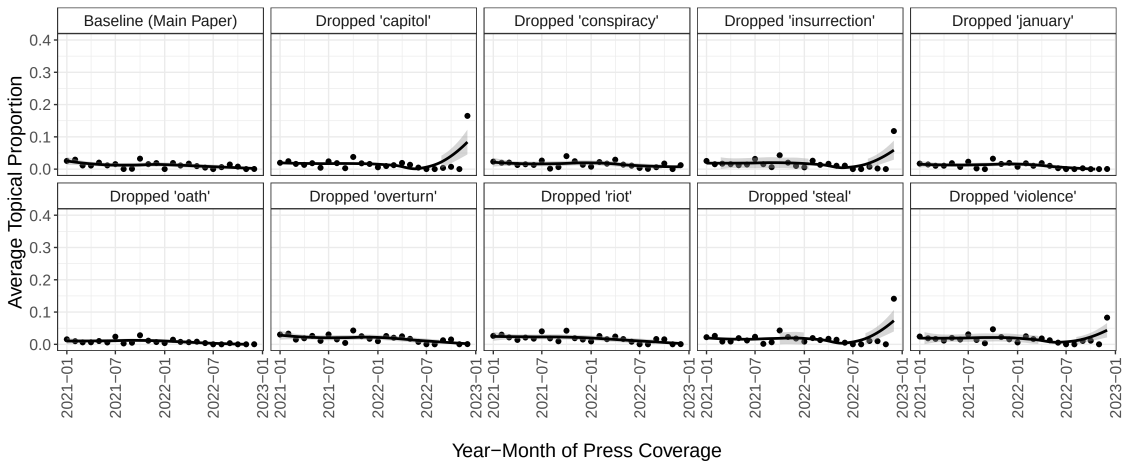


Figure A18: Average Document-Level Proportion of Election Denial Topic by Month, No Treatment with Dropped Keywords

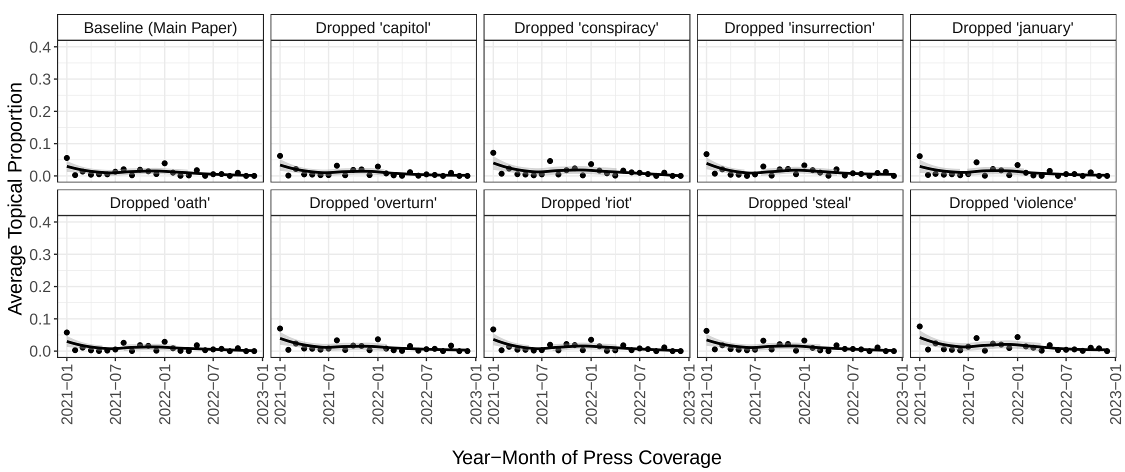


Figure A19: Average Document-Level Proportion of Election Denial Topic by Month, DC Treatment with Varying Topics

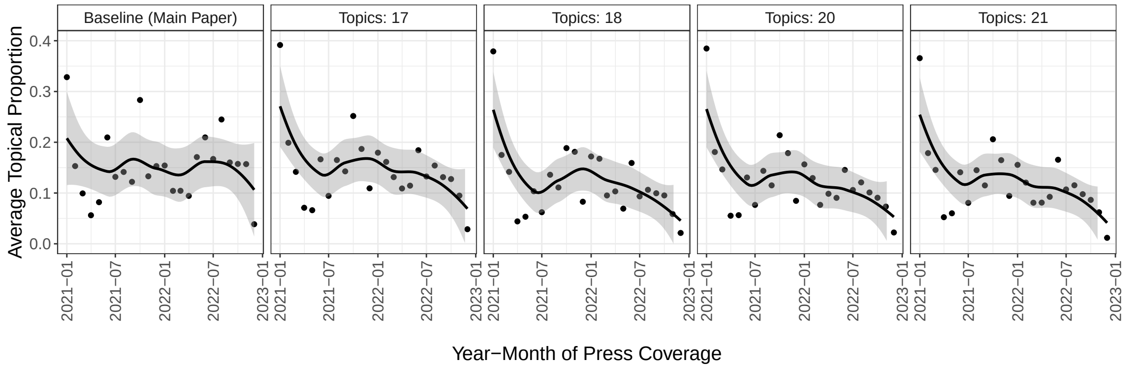


Figure A20: Average Document-Level Proportion of Election Denial Topic by Month, Offline Treatment with Varying Topics

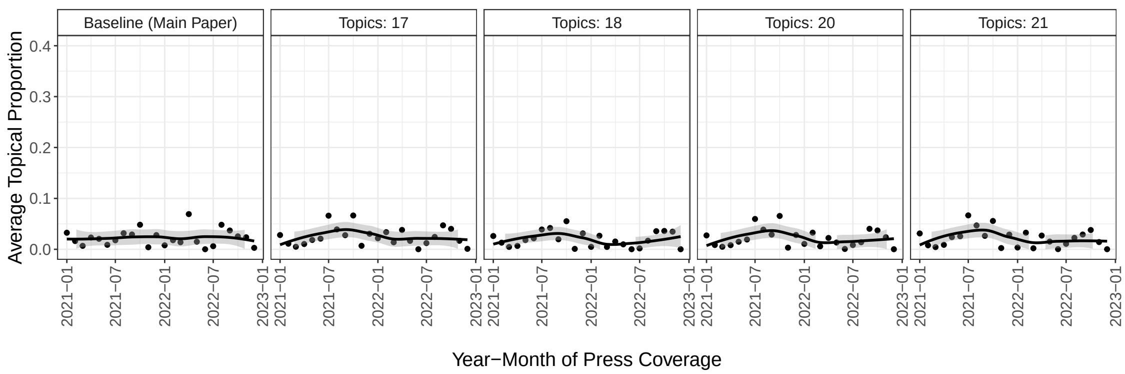


Figure A21: Average Document-Level Proportion of Election Denial Topic by Month, Online Treatment with Varying Topics

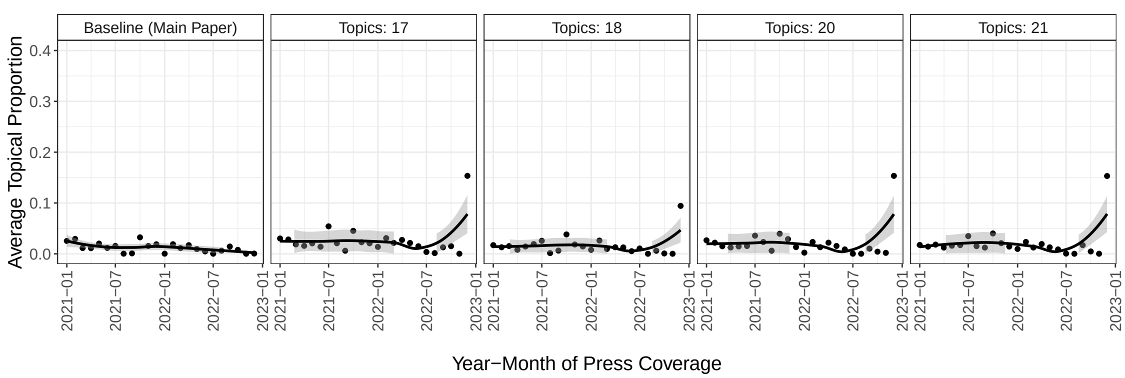
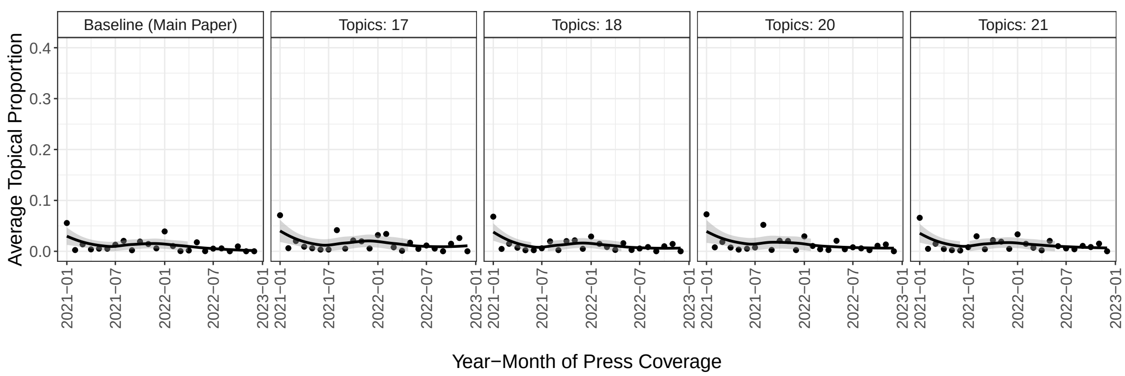


Figure A22: Average Document-Level Proportion of Election Denial Topic by Month, No Treatment with Varying Topics



A8.3 Example News Documents & Associated Topical Proportions

The example text below was randomly drawn from our corpus of journalistic news articles and letters to editors. We display the first five lines of each article. The full text of each article mentions at least one legislator from our population of interest, but many of these texts also mention other state and federal politicians, as show in several of these examples.

Example Text	Topical Proportion
A West Virginia lawmaker who filmed himself and supporters of President Donald Trump storming into the U.S. Capitol is being widely condemned as federal prosecutors step up their pursuit of violent perpetrators. State Del. Derrick Evans was among several lawmakers from across the country who traveled to Washington, D.C...	0.78
The Virginia Senate is advancing a resolution to censure a GOP lawmaker for "fomenting insurrection against the United States," saying she helped incite the storming of the U.S. Capitol earlier this month. A Senate committee voted Tuesday...	0.49
The following information was released by the Michigan Democratic Party: Keeping the "Big Lie" alive is more important to the MI GOP than preserving our right to vote "We are once again truly disappointed by the actions of the former Secretary of State, Senator Ruth Johnson...	0.46
This week, Glenn Youngkin suffered several brutal setbacks in his already weak, unsteady, desperate campaign for governor of Virginia. First, it was reported that Glenn's abysmal track record at Carlyle includes harming workers with disabilities. Then, it became public that Youngkin will be headlining an "election integrity rally" sponsored by extreme Republicans who attended the January 6th insurrection...	0.30
This week, the U.S. House of Representatives delivered a bipartisan rebuke of Congresswoman Marjorie Taylor Greene over her support for extremist conspiracy theories, executing Democratic leaders, and racist, antisemitic, and Islamophobic views. Not wanting to be left behind...	0.26
The January 6th committee held its second hearing yesterday, further clarifying how far Republicans are willing to go to destroy democracy and overthrow an election. The hearings so far have painted a disturbing, but clear picture of a president who knew he had lost the 2020 election, but was willing to do anything to grasp onto power...	0.23

One leading candidate seeking to become Georgia's chief elections official, Republican Jody Hice, is a Congressman who voted to overturn Democrat Joe Biden's 2020 presidential win in the hours after the Jan. 6 riots at the U.S. Capitol. Hice had posted on social media earlier that day: "This is our 1776 moment," referencing the American Revolution...	0.16
One leading candidate seeking to become Georgia's chief elections official, Republican Jody Hice, is a Congressman who voted to overturn Democrat Joe Biden's 2020 presidential win in the hours after the Jan. 6 riots at the U.S. Capitol. Hice had posted on social media earlier that day: "This is our 1776 moment," referencing the American Revolution...	0.16
The fate of our democracy doesn't hinge on the battle for the House or the fight for control of the Senate, but on state elections for a once sleepy office: secretary of state. No elected officials will be more pivotal to protecting democracy — or subverting it — than secretaries of state. While their responsibilities vary from state to state, most oversee elections, a role in which they wield a tremendous amount of power. Secretaries of state own the bully pulpit on voting, and they control the machinery of elections...	0.12
Facing a dramatically different political landscape than Republicans had expected, with the possibility of Democrats in control of the state House, the incoming Senate president pro tempore, Kim Ward, struck a conciliatory tone on Tuesday shortly after her election as the first woman ever to lead the chamber. When Ms. Ward, R-Hempfield, moves into her new role on Dec. 1, it will mark the first time in Pennsylvania's 235-year history that a woman has held a legislative office created by the state constitution. She'll hold the office on an interim basis until Jan. 2, when senators elect a permanent leader for the next session of the General Assembly...	0.06
Pennsylvania Democrat John Fetterman's rocky debate performance fueled concern inside his party on Wednesday, as leaders assessed whether it would significantly shift a race that could decide control of the U.S. Senate and the future of Joe Biden's presidency. Appearing on stage five months after his stroke, Fetterman, Pennsylvania's 53-year-old lieutenant governor, struggled to complete sentences, and he jumbled words throughout the hourlong televised event. That was no surprise for medical professionals, who noted that the format...	0.02

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