The Legislative Legacy of Voter Identification Laws*

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Abstract

Does the implementation of voter identification (ID) laws intensify conflict between legislative parties? We examine the consequences of the American states' modern wave of voter ID requirements for polarization in their legislatures. We theorize that these laws alter legislative candidates' perceived uncertainty about their districts' median voters, which creates centrifugal forces on their ideological positions and ultimately magnifies partisan divisions. To test this theory, we leverage plausibly exogenous variation in voter ID law implementation induced by the timing of post-enactment legal challenges. We find empirical support; a law going in force widens interparty ideological distance. This divergence may be stronger for Republican parties, does not appear in other contentious policies, and is not an artifact of the legislative agenda. Thus, a crucial legacy of these laws is the amplification of political conflict. By creating conditions that stifle compromise and bipartisanship, voter ID requirements *indirectly* threaten the quality of American democracy.

Keywords: Voter identification laws; Party polarization; State legislatures; Electoral competition

*Supplementary material for this article is available in the online appendix. Replication files are available in the JOP Data Archive on Dataverse (https://dataverse.harvard.edu/dataverse/jop). The empirical analysis has been successfully replicated by the JOP replication analyst.

1 Introduction

How do voter identification (ID) laws affect politicians? Scholars have traditionally studied the consequences of voter ID for the mass public, focusing on voter registration and turnout (Highton 2017; Hajnal et al. 2017; Kuk et al. 2022; Fraga and Miller 2022), voter fraud (Cantoni and Pons 2021), and public opinion (Wilson and Brewer 2013; Kane and Wilson 2021). These outcomes are critical for scholarly and normative evaluations of voter ID requirements. However, current knowledge on these reforms is incomplete because scholars have not yet turned back to their implications for the original sources of the laws: state legislatures. Filling this gap is important because potential changes in voter behavior can alter the equilibrium behavior of politicians (Ashworth and Bueno de Mesquita 2014). Indeed, legislators must win support from the voters who make it to the polls, so laws that affect who can vote may impact lawmakers' campaign and policy platforms, decisionmaking, and/or electoral accountability in general. In this paper, we consider whether expected changes to the electorate from voter ID laws ultimately narrow the prospects for bipartisan cooperation in state legislatures.

Using a spatial proximity model, we theorize that implementing a voter ID requirement heightens state legislative candidates' perceived uncertainty regarding the location of the median voter in their districts, which increases the weight they place on their own preferences when choosing a policy platform. Further, we posit that state Republican parties place anti-centrist constraints on their candidates after voter ID law implementation. Consequently, relative to the counterfactual of no law in force, candidates in both parties take ideological positions farther away from center, with Republican candidates positioning themselves the farthest away. This divergence ultimately magnifies partisan conflict in the legislature once they take office.

We test our theory with data on voter ID laws and party polarization in American state legislatures during 2003–2018. This time period immediately followed the passage of the Help America Vote Act (HAVA)—a crucial bill in Congress that sparked a wave of new, heavily-restrictive voter ID requirements in the states. We leverage plausibly exogenous variation in the implementation of voter ID laws induced by the timing of the legal process. Due to their controversial nature, these laws have often been the subject of lawsuits and other legal obstacles that delayed when they were allowed to go in force. We argue that there is sufficient uncertainty in this regard such that the length of a legal delay is difficult to predict. Thus, states that passed voter ID legislation, but were forced to wait to execute it, can serve as credible counterfactuals for states that were subject to shorter (or no) delays. However, we do not rely *solely* on this proposed source of exogenous variation to mitigate confounding; we also combine it with a matching procedure for panel data as well as other means of covariate adjustment.

Under this multifaceted identification strategy, we find that a voter ID law going into effect exerts a notable increase in legislative polarization that lasts several years. It appears to be driven more by Republican parties' rightward shift, but we also find suggestive evidence of Democratic parties' move to the left. In a placebo test, we find that this pattern does *not* appear after the initial adoption of a law, indicating that implementation is the key moment in which voter ID requirements impact legislative politics. We also conduct several additional analyses of the causal mechanism. We demonstrate that other contentious policies are not associated with increased legislative polarization, nor were the more lax voter ID laws of the 1990s that pre-dated the controversial, modern set of restrictions. Original survey questions and experiments administered to a sample of state legislators in 2023 suggest support for our claim that voter ID increases legislative candidates' uncertainty about the electorate. Finally, we find that our results do not seem to emerge from changes to the composition of the legislative agenda.

Our evidence demonstrates that the implications of voter ID laws extend beyond any direct effects they may hold on political participation in the mass public. Moreover, they point to a previously-overlooked normative problem. Although voter ID laws represent just one of many policies that state legislatures consider, they create a lasting legacy of amplified political conflict among elites in government. This discord, in turn, hinders negotiation and efficient policymaking and may even further polarize citizens who respond to elite signals on the issue. Thus, regardless of their impact on mass political engagement, voter ID laws *indirectly* threaten the quality of the democratic process by inhibiting effective governance and the representation of citizen interests.

2 The Consequences of Voter ID Laws

The extant research on voter ID laws in the United States is predominantly focused on their consequences for the political lives of citizens. In particular, this work examines outcomes such as voter access to the polls, mobilization activity, voter fraud, and public opinion dynamics. The picture that emerges is complex and nuanced. While there is some heterogeneity in the findings, scholars have generally uncovered negligible or small effects of voter ID laws on voter access. At the same time, public preferences toward these laws reflect strong baseline support, but with clear differences between the parties.

Importantly, voter ID laws have become intrinsically racialized over time. Scholars argue that they disproportionately affect voters of color like welfare and other "disciplinary" policies that target racial and ethnic minorities as a "problem" (Soss et al. 2011). A common claim is that voter ID laws suppress turnout among the poor and minority groups by increasing the costs of registering and voting (e.g., Rocha and Matsubayashi 2014). These added costs have emerged alongside other factors associated with turnout deficits by race, such as political parties' general difficulties in activating minority voters (Fraga 2018).

However, the empirical evidence regarding voter ID laws and turnout is mixed (see Highton 2017). Some studies point to negative turnout effects in general (Hood and Bullock 2012) and specifically among nonwhite voters (Hajnal et al. 2017; Kuk et al. 2022). However, the data employed by these studies may not be up to the task of conclusively identifying the effects (Erikson and Minnite 2009; Highton 2017; Grimmer et al. 2018). Research that employs higher-powered designs and/or better data yields negative effects specifically among those who lack ID (Grimmer and Yoder 2022) and negligible effects overall (e.g., Cantoni and Pons 2021). But broad negative turnout effects could still be masked by the fact that some voters are more likely to be mobilized in the wake of voter ID laws (Valentino and Neuner 2017; Neiheisel and Horner 2019; Cantoni and Pons 2021). There is also clear evidence that citizens from marginalized groups are more likely to lack the proper ID that these laws require (Barreto et al. 2009; Fraga and Miller 2022).

An additional wave of research examines the development of public opinion toward voter ID laws. Scholars note that, prior to the adoptions in the early 2000s, the public held low information on voter ID. But public debate after HAVA soon increased mass knowledge and salience, generating both proponents and opponents of the laws as citizens sorted based on elite cues (Wilson and Brewer 2013; McKee 2015). These studies find overall diffuse support compared to opinion on other issues, but with noticeable partisan differences. About 60% of Democrats and more than 90% of Republicans favor the laws (Wilson and Brewer 2013; Kane and Wilson 2021). Most importantly, public opinion on the issue has evolved from popular consensus to preferences shaped by partisanship, ideology, and racial resentment as citizens have learned from the decisionmaking and rhetoric of elected officials (Wilson and Brewer 2013).

Another component of this literature documents how state governments enacted these laws in the first place, finding that they are the product of a highly partisan political environment. Specifically, these studies point to gubernatorial and legislative partisanship and ideology (Bentele and O'Brien 2013; Hicks et al. 2015; Biggers and Hanmer 2017), electoral competition (Hicks et al. 2016), and state racial composition (Rocha and Matsubayashi 2014; McKee 2015) as key determinants of voter ID law adoption. However, while this research focuses on state legislatures, it is silent on the question of how these laws affect future legislative politics. We contend that doing so is a crucial next step. If political discourse over voter ID is an elite-driven process, then scholars must understand how these laws impact elites *after the laws are implemented.* Indeed, if voter ID requirements emerge from a partisan environment, then subsequently increase tension between legislative parties, the passage of one law may lead to additional restrictions on voting in the future.

3 Voter ID Laws and Legislative Polarization

Our theoretical framework originates in the unique nature of voter ID laws. Unlike abortion, gun control, or other potentially contentious policies, voter ID laws can alter the composition of the electorate.[1] We posit that this feature changes legislative candidates' and parties' electoral

[1]To be sure, lawmakers writing voter ID bills do not generally describe their efforts in this manner. Instead, many supporters justify the laws based on their overall popularity in the public and their capacity to improve trust in elections (e.g., Fike 2021; Caruso 2021).

strategies, ultimately shifting the parties in the legislature farther apart in ideological space. We develop our theory using McCarty and Meirowitz's (2007) spatial proximity models of political competition as a framework (103–107; see also McCarty et al. 2019). We describe the theory and its associated hypotheses here. In the Supporting Information (SI) we develop the theoretical models formally and derive our hypotheses from their equilibrium predictions.

We begin with a set of assumptions. We assume that voters are arrayed by their ideal points in a unidimensional policy space. Candidates enter an election by choosing a platform in this space and voters select the candidate who is closest to them. Candidates also hold their own policy preferences, which may or may not be equal to their platforms. We assume Republicans' ideal points fall on the right of the policy space and Democrats' ideal points are on the left. Importantly, the ideal point of the median voter is unknown, but candidates hold beliefs about its location. Candidates compete within districts that concurrently elect representatives to a legislature. The candidate whose platform is closest to the median voter in each district wins that election and administers her platform via her roll call voting behavior.[2]

With these assumptions in place, consider candidates' strategies when a voter ID law is *not* in force. The entire electorate is eligible to vote in this baseline case. Thus, candidates' best response is to identify the median voter in the entire electorate, perhaps with historical turnout data, opinion polling, campaign activities, media reports, and other sources. They likely do not know the median exactly, but can use these resources to form relatively precise beliefs about it. Candidates from both parties are then incentivized to move toward that point. In this "normal" scenario, winning candidates tend to be those with comparably centrist platforms, which generate centripetal forces on the parties' average preferences in the legislature.

Now consider a change to this baseline case. The simplest expectation after the implementation of voter ID would be a *rightward shift* model, which we discuss formally in the SI. In brief, in

[2]Candidates may be more attuned to specific groups of voters, but they ultimately must win the median voter's support to win the election. Following Kousser et al. (2007), we assume that there is no difference between candidates' chosen platforms and their voting behavior in office.

this scenario all actors involved believe that the new law has deactivated more Democratic than Republican voters (Grimmer and Yoder 2022).[3] Thus, everyone agrees that the median voter has moved rightward. Candidates' best responses, then, are to choose platforms at that new median. Compared to the baseline case (no voter ID law), such a process yields the prediction of a rightward shift by all candidates, and thus *both* legislative parties move right in the next session.

However, we posit a competing process. The key mechanism in our model that is absent in the rightward shift model is candidates' perception of uncertainty about the new electorate. We expect that candidates in both parties become less confident about the median voter's location in their districts after voter ID implementation. Irrespective of the law's actual effects on the electorate, these candidates *believe* that the electorate as a whole has changed.[4] But predicting the magnitude and even the direction of that change *at the district level* is a noisy process. Candidates may still believe voter deactivation is stronger among Democrats than among Republicans. But they are also aware of a counterbalancing process—they must consider that the law could galvanize mobilization efforts on both sides and even *increase* voter motivation. For example, the law could spike enthusiasm among Republicans and/or anger Democratic voters (Valentino and Neuner 2017), increasing turnout. Indeed, in recent years the Democratic Party and its allied groups have used voter ID laws as a rallying point for get-out-the-vote endeavors (Neiheisel and Horner 2019; Cantoni and Pons 2021).

Simply put, we maintain that legislative candidates do not take the rightward shift as inevitable. In a particular district and election the expected *average* effects of voter ID laws may or may not be realized. Voter mobilization varies in targets, scope, and strength across parties, campaigns, organizations, and elections (e.g., Leighley 2001; Burch 2013). And there is little consensus on

[3]This deactivation could stem from voters lacking the necessary ID and/or from "alienation from the political process" (Fraga and Miller 2022, 1092).

[4]Indeed, the laws may exert minimal influence on who goes to the polls (Highton 2017) or election outcomes (Harden and Campos 2023). Our theory only requires that candidates *expect* a change in the electorate.

what mobilization approaches are consistently effective in the wake of voter ID laws. Individual campaigns lack clear guidance on how to activate voters and even the question of whether to notify voters about updated requirements is a matter of discussion (Citrin et al. 2014).[5] Accordingly, with less confidence regarding who will actually vote, candidates in both parties know comparatively less about their districts' median voters than they did before the law took effect.

The main consequence of this increase in perceived uncertainty about the district median is that candidates' own ideal points exert stronger influence on their platforms compared to the baseline case. We show formally in the SI that this result stems from the tension between candidates' preference to win elections with moderation and their more extreme sincere policy preferences. When the location of the median voter is highly certain, candidates have incentive to position themselves there because they value winning elections. But with less information about the median, winning becomes probabilistic, which gives candidates room to drift toward their ideal points without utility loss in expectation (Calvert 1985; McCarty et al. 2019). Substantively, in the absence of a clear signal from voters, candidates are justified in increasing their commitment to other sources of decisionmaking.[6] Consequently, they choose relatively more extreme platforms, which are reflected in the voting behavior of those candidates who ultimately win office (see note 2). This logic predicts divergence between the two parties in the legislature, which we formalize in our first hypothesis.

H1 The implementation of a voter ID law increases the ideological distance between the two major parties in state legislatures.

We also predict that the polarizing influence of these laws exhibits partisan imbalance that stems from the state parties' electoral strategies. In deciding which candidates to nominate and support in districts, parties regularly confront the tension between maintaining a cohesive brand

[5]More specifically, elites' attempts to announce new voting requirements may actually have a demobilizing effect because of negative stereotypes toward groups such as people of color (Citrin et al. 2014). These findings imply that candidates' efforts to help voters with ID requirements—an action that should reduce uncertainty, in theory—may exert the opposite effect in practice.

[6]Our state legislator survey demonstrates empirical support for this point (see the SI).

and winning control of the legislature. One means by which party leaders balance these competing goals is by placing "tethers" on their slate of candidates, or limits on how close to the ideological middle candidates' platforms can go (Merrill et al. 2014).[7] Loose tethers place these limits relatively close to the center, which helps candidates win in competitive districts, but also expands within-party heterogeneity in the legislature. Tight tethers improve party cohesiveness at the expense of electability in moderate constituencies. We contend that a voter ID law going in force leads to divergent strategies by the parties in this regard.

First, we maintain that the Republican Party would not exert such considerable efforts to enact voter ID laws if its leadership did not believe that they benefited Republican candidates, on average. The implied and overt racial aspects of many voter ID laws indicate that discouraging Democratic voters has been a major consideration of their proponents (Bentele and O'Brien 2013). Publicly, Republicans claim that these laws improve election security without affecting voter turnout (e.g., Olsen 2022). Nonetheless, the strategic aspect of racial targeting has come to light in a few instances. For example, documents revealed by court order describe a "meticulous and coordinated effort to deter black voters" in the creation of the 2015 voter ID bill in North Carolina (Wan 2016). Thus, despite the added uncertainty for individual candidates, we contend that state Republican parties believe that they have, on average, come out ahead after implementing such a law.[8]

As such, we expect that the implementation of a voter ID law leads Republican parties to adopt

[7]We define parties broadly as the network of activists, donors, elected officials, and other actors that make up the informal party organization (e.g., Bawn et al. 2012).

[8]The distinction between the Republican Party leadership's perspective and individual candidates' perspectives is important. Because it believes the laws benefit its candidates, the leadership acts rationally in placing voter ID on the agenda. But the result for individual candidates is additional uncertainty regarding how the laws will affect the electorate in their specific districts. These individuals may also believe that they or the party in general benefit from the laws. But even in such a case, we claim that there is more noise in their predictions for specific election outcomes. Indeed, our 2023 survey of state legislators discussed in the SI reveals that over one-third of Republicans in voter ID states are willing to admit that voter ID laws produce more uncertainty at the

a tether and demand more extreme platforms from their candidates. State and local parties can use their collective control over electorally valuable resources to tether candidates to certain ideological and policy positions. Candidate recruitment and candidate support efforts by these actors allow them to influence the candidate pool (Moncrief et al. 2001; Sanbonmatsu 2010), and they do so with ideological goals in mind (Broockman et al. 2021).[9] Nationally, the Republican Party greatly values ideological purity and prefers to avoid allowing its candidates to drift to the middle (Merrill et al. 2014; Grossman and Hopkins 2016). Implementing a voter ID law provides such an opportunity. As a consequence of its belief that a voter ID law has, on average, disproportionately deactivated Democratic voters, the Republican leadership holds credible leverage with which to prohibit centrist platforms among its candidates. It no longer needs to include moderates in its ranks to win control of the legislature because it believes the voter ID requirement has improved the electoral conditions for more extreme candidates. Thus, it enacts a tight tether on its candidates to improve the party's ideological cohesion in the legislature.[10]

In contrast, while state Democratic parties certainly hold non-centrist preferences and exert leftward pull on their candidates' platforms, we posit that this influence is comparatively weaker. As a diverse and pluralistic collection of groups, the Democratic Party accepts relatively more heterogeneity than do the Republicans (Grossman and Hopkins 2016). Additionally, even if the party wanted to impose a tight tether, organizational capacity is comparatively reduced among Democrats. The ideological left has not matched the growth of conservative donor networks and groups that have been instrumental in pushing for policy changes and coordinating Republican political activity across state legislatures (Hertel-Fernandez 2019). Finally, Democrats' strong opposition to these laws and efforts to pass expansive voting rights legislation to counteract them

individual level.

[9]Our analyses in the SI use data on state legislative candidate ideology to document the asymmetric polarization of candidate pools after voter ID implementation.

[10]Importantly, we also expect that the party does not publicly disclose this choice. Doing so would reveal its electoral strategy and conflict with its stated rationale for adopting voter ID laws. See the SI for more on this point.

suggests that Democratic leaders may believe that reactivation efforts could mitigate the expected loss of support. Nonetheless, this reaction is still largely a defensive strategy, meant to effectively reconstitute the baseline conditions. Thus, state Democratic parties likely cannot place as much ideological constraint on their candidates as can the Republicans.

In other words, Democratic leaders do not hold the same leverage as Republicans to demand that their candidates choose non-centrist platforms after voter ID law implementation. As such, we posit that they do not implement a tether on their candidates once a law goes in force. Compared to the Republicans, they are more willing to trade ideological homogeneity for electability. The combined result of these divergent partisan motivations is that the intensified polarization in the legislature predicted by H1 is disproportionately driven by Republican parties. We formalize this logic in our second hypothesis.

H2 The implementation of a voter ID law increases the ideological extremity of Republican parties in state legislatures more than it increases Democratic parties' ideological extremity.

In sum, this theoretical framework makes two predictions about the influence of voter ID laws on state legislatures. First, it predicts that the laws increase legislative candidates' uncertainty about their districts' ideological preferences, which leads them to place more weight on their own preferences when choosing a platform. Second, the laws strengthen state Republican parties' motivation to demand ideological purity among their candidates, but do not do so for Democratic parties. These processes unite to move candidates' ideological positioning away from center, especially among Republicans. Consequently, more candidates with non-centrist platforms win and enter the legislature compared to a baseline case with no voter ID law in force. Over time, these new representatives' roll call voting pulls the parties away from each other, increasing polarization in subsequent legislative sessions.

4 Research Design

Testing our hypotheses is a challenging task because of selection into treatment: the very same legislatures that we are studying previously made the choice to propose and pass voter ID laws.

Furthermore, this selection process is not random; the literature highlights several factors that predict the decision, including partisanship, electoral competition between the parties, and regional variation (Hicks et al. 2015, 2016). We must carefully guard against attributing the political dynamics that led to the laws' adoption as part of their actual effects after implementation.

In what follows we describe our data and multifaceted research design intended to identify the effects of voter ID laws going in force on future state legislative politics. This design also allows us to look for evidence where we do *not* expect to find it by comparing the effects of voter ID law *adoption* to those of implementation. Finally, we describe investigations into the underlying theoretical mechanism and an alternative explanation. We look to other contentious policies for evidence of polarizing effects and examine the consequences of voter ID laws outside of the roll call record.

4.1 Data

We constructed state-year panel data for the period 2003–2018 to test our theory.[11] This timeframe covers state laws adopted after the passage of HAVA in 2002. Three states developed voter ID laws prior to HAVA (Michigan, Louisiana, and Florida), but multiple analyses contend that these laws were considerably different from those that came after the legislation (Williams 2004; MIT Election Data + Science Lab 2021). The primary scope condition for this research is identifying the effects of post-HAVA laws that have been the subject of political controversy in recent years. In the SI we describe our justification for this choice in detail and use data from the three pre-HAVA states to conduct a placebo test.

4.1.1 Outcome Variables

We employ Shor and McCarty's (2011) ideal point measures to form our outcomes of interest. These data have long been the industry standard for research on state legislators' voting behavior. Shor and McCarty (2011) compute ideal point estimates for state legislators dating back to the early 1990s using roll call records. The estimates are then "bridged" into a common ideological space

[11]Our outcome variable and some covariates are only available through 2018. We omit Nebraska because its nonpartisan legislature renders some of the variables we analyze undefined.

via legislators' answers to a set of survey questions administered to state legislative candidates. The indicators have been validated and used extensively by the state politics research community since their introduction in 2011.

We are primarily concerned with several aggregate measures derived from Shor and McCarty's (2011) ideal point estimates. To test H1, we use the difference between major party median members' ideal points in each state-year, averaged across chambers, as our measure of party polarization.[12] Next, to test H2 we employ the absolute party median ideal points in each state-year, again averaged across chambers. This measure locates the median member of each legislative party in a given year, with larger values signaling a more extreme party, on average. It allows us to assess whether the effects of voter ID laws are stronger in one party compared to the other.

The Shor and McCarty (2011) measures accurately capture the concept of ideological distance between the parties. However, they do require the assumption that legislators' ideal points are static over time. This assumption is generally accepted, but has also faced criticism (for a review, see Kousser et al. 2007). It means that we can uncover effects that stem from the replacement of lawmakers from one year to the next (if they exist), but not any potential effects that are due to within-legislator ideological adaptation.[13] Previous research indicates that this framework for empirically observing polarization is reasonable (e.g., Olson and Rogowski 2020). In fact, two-thirds of polarization in Congress stems from member replacement (Theriault 2006). Moreover, ideology is one of the few reliable signals of differences between candidates in the low-information environment of state legislative elections (Birkhead 2015).

[12]Averaging across chambers mirrors Shor and McCarty's (2011) original analysis of these data. In the SI we conduct our analysis separately for each chamber.

[13]Incumbent extremity increasing over time is a possibility given our theory, but we are unable to test for such an effect in our data. See the SI for additional empirical evidence of a replacement effect.

4.1.2 Treatment

The National Conference of State Legislatures (NCSL) defines two dimensions of voter ID laws: (1) strict or non-strict and (2) photo or non-photo. Strict voter ID laws state that voters who lack the appropriate documentation of their identity must use a provisional ballot and take on a burden of some kind, such as additional steps *after* Election Day, for it to be counted (see National Conference of State Legislatures 2021). Photo ID laws stipulate that the primary acceptable documents are those with a photo of the voter. Several states' laws include both dimensions, but some include only one. For example, some states have enacted strict laws that allow non-photo IDs, such as a Medicare or Medicaid card. Other states' laws require a photo ID as the primary means of proving identity, but are non-strict by NCSL's definition because they allow for an alternative at the point of voting, such as signing an affidavit declaring a voter's identity or their inability to obtain photo ID (National Conference of State Legislatures 2021).

We contend that either one of these dimensions on its own is sufficient to impose the burdens on voters that are the subject of controversy. The former dimension clearly creates difficulty regardless of whether a photo is required because it necessitates an extra step after voting to make the votes count. But the photo dimension is independently restrictive as well because it forces voters to obtain an ID that they may not have the means to acquire. Indeed, much of the political debate around these laws has centered specifically on the photo versus non-photo question (see Fraga and Miller 2022). Common photo IDs, such as a driver's license or student ID card, can reflect socioeconomic privilege. Even the "free" photo IDs that some states have created to ease the burden of their laws cost money themselves and often require documentation that some citizens may not have, such as a birth certificate (Middleton 2012).

Accordingly, we contend that only one such dimension—additional steps after voting *or* a photo ID requirement—needs to be present to constitute a voter ID law from the perspective of our theoretical framework. In the analyses that follow, we consider a state treated in a given year if a law that is in force in that year stipulates *at least one* of the following two conditions:

- Voters without acceptable identification must vote on a provisional ballot and take on a substantial burden to officially count their votes;

- The primary acceptable identification documents are those with a photo of the voter.

We contend that either of these condition could deter voters, thus generating uncertainty among candidates about the electorate (see the SI for separate analyses of each condition). With respect to timing, we consider a state treated in a given year if the law was in force for general elections in that year or at least half of the year if no general elections took place.

4.1.3 Covariates

We use several time-varying covariates to mitigate confounding of our treatment effect estimates. These data—which we describe in detail in the SI—include variables that the literature suggests may be associated with the choice to adopt a voter ID law and could also plausibly correlate with our outcome variables.[14] Some are directly related to state legislatures, such as Holbrook and Van Dunk's (1993) measure of electoral competition between the parties, a categorical variable ranging from unified Republican control to unified Democratic control of the legislature (Klarner 2021), and one-year lags of the outcome variables. Additionally, we include an indicator for a Republican governor (Klarner 2021), Berry et al.'s (1998) measures of state governmental and citizen ideology, and a measure of the strength of the state court system (see the SI).

We employ a parsimonious set of covariates due to a relatively small sample (see below). However, in the SI we show that our results are robust to the inclusion of several other plausible confounders, including the severity of states' voter ID laws, the nonwhite proportion of state populations, state-level racial resentment, term limits, legislative professionalism, and the size of the legislative agenda. Our results are also robust to diffusion effects—the impact of *other* states' voter ID laws on legislative polarization in a state—which we measure with adoptions by geographic neighbors and policy "source" states (see the SI for complete details).

[14]We utilize multiple imputation to address missing data; all results presented below reflect the necessary adjustment to measures of uncertainty. See the SI for diagnostics on the imputation procedures.

4.2 The Timing of Law Implementation

On May 25, 2011, Wisconsin Governor Scott Walker signed Act 23 into law. The legislation established a strict ID requirement for elections in the state, requiring voters to present a photo ID and obligating additional action to count provisional ballots if voters lacked the proper documentation. However, unknown to Walker or the sponsors of the bill in the legislature at the time, it would be over four years before the law permanently governed elections in the state. The reason for this delay was a series of court cases which necessitated a hold on the law as it was reviewed by the judicial system. While the supporters of Act 23 undoubtedly knew that their bill would generate controversy—and perhaps even expected legal challenges—we argue that they could not have predicted exactly how those challenges would unfold, and thus did not know when the law would actually be in force.

More specifically, at the time of passage the supporters of Act 23 likely did not know that separate suits would challenge the law in Wisconsin state court (October 2011) as well as federal court (December 2011). And even if they could have forecasted these developments, predicting the courts' rulings—as well as rulings on appeals of those decisions—would have been exceedingly difficult. As it turned out, the initial rulings at both the state and federal levels went against the law, prolonging its delay. It was not until 2014 when the Wisconsin Supreme Court upheld the law and then early 2015 when the U.S. Supreme Court declined to hear a challenge to it that the path was laid for the law to fully go into effect. In short, the Wisconsin legislature attempted to place ID requirements on its elections in 2011, but operated until 2015 under uncertainty before those requirements actually governed the process.

Wisconsin's experience with implementing a voter ID law is not unusual. Due to their contentious nature, many states have enacted such laws only to have them placed on hold through injunctions from the courts or other delays. Indiana's law passed in 2005 but did not go in force until after the U.S. Supreme Court upheld it in *Crawford v. Marion County Election Board* in 2008. The legal process has switched the laws on in some years and off in others in Arkansas and Texas over the last decade. Proposed laws dating back to the early 2010s in North Carolina and Pennsylvania stalled into the 2020s due to multiple challenges. Prior to 2013, several states were required to seek preclearance from the Department of Justice under Section 5 of the Voting Rights Act before implementation could commence. And in a few cases voter ID laws faced the uncertainty of a ballot initiative either before or after the legislature passed implementing legislation.[15]

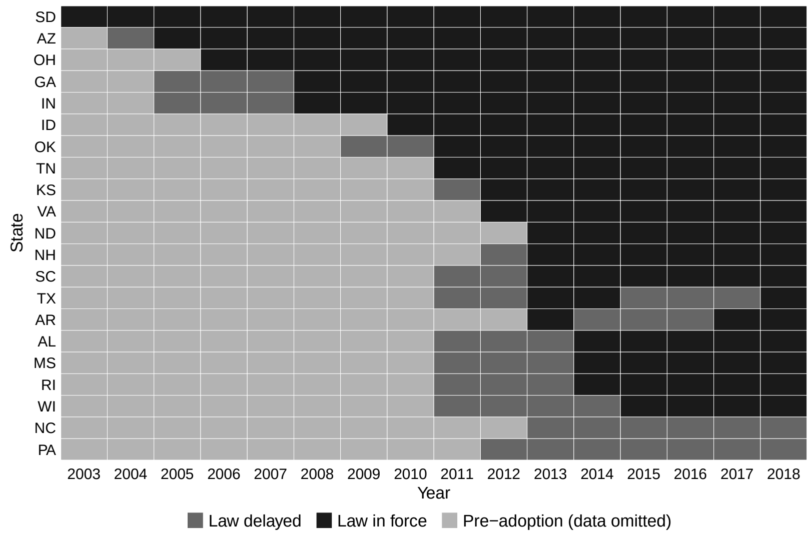
From an empirical standpoint, these delays represent the first component of our identification strategy. While voter ID laws are clearly not randomly assigned, we contend that the timing of their implementation in states that enact them is as-if random. When a governor signs a voter ID bill into law, supporters are uncertain about (1) whether or not the law will be challenged in court, (2) which court(s) will hear any potential challenges, (3) the outcome(s) of those courts' decisions, and (4) the timing of the entire process. Furthermore, preclearance requirements and/or ballot initiatives only add to this uncertainty. Thus, in one state the process may never begin and/or exert negligible effects on the implementation of the law while in others it may cause substantial delay. Accordingly, we argue that from the point of selection into treatment, the year in which a state is actually treated is plausibly exogenous. States whose voter ID laws were delayed can serve as better counterfactuals for states whose laws were not delayed compared to states that never adopted such laws, allowing us to identify the average treatment effect on the treated (ATT).

To leverage this source of variation, we focus our analyses on a subset of the data: an unbalanced panel of 21 states and 16 years (N = 194). Figure 1 displays the variation in state-year treatment status during 2003–2018 in this subset, which isolates delays in the implementation of voter ID laws. Light gray denotes state-years prior to the adoption of a law. Dark gray represents state-years in which a law was enacted, but implementation was delayed. Finally, state-years in black denote when and where a law was in force. We examine only states that adopted a law (as defined above) beginning in the year in which the law was adopted (e.g., 2011 for Wisconsin). That is, in our main analyses the sample consists only of those state-years denoted in dark gray or black in Figure 1.[16]

[15]In the SI we summarize the legal history and timing of all voter ID laws examined here.

[16]In addition to yielding a source of exogenous treatment variation, subsetting the data also reduces model dependence. The main results we report are quite robust across various estimators

Figure 1: The Timing of Voter ID Laws in the States, 2003–2018



Note: The graph displays the variation in treatment status after subsetting to isolate delays in the implementation of voter ID laws.

The key consequence of this subsetting strategy is that, conditional on adoption, the variation in our treatment variable comes *entirely* from the unpredictability of the implementation process. We are comparing, for example, South Carolina—which adopted its law in 2011 and received preclearance in late 2012—to Mississippi, which enacted a similar law in 2011 that was delayed until 2014. In both cases, the states passed a voter ID law at the same time, but the legal process happened to resolve more quickly for South Carolina.

In the SI, we provide empirical evidence in support of this identification strategy. First, we demonstrate that this subset design improves covariate balance compared to the full sample of data with all states. This improvement is particularly strong for variables such as party control of the legislature and governmental ideology. Next, we show that within the subset data the lengths of implementation delays are not associated with several covariates and do not vary greatly by

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and specification choices. However, the full data demonstrate more model dependence. Using all states, we can construct analyses that produce results similar to what we report below, larger effects, and null results. Nonetheless, we focus much of our efforts on the subset data because we believe there are strong theoretical and empirical justifications for doing so.

law severity. However, despite this evidence we must acknowledge that it is still possible for systematic factors to threaten our inferences. Thus, we must take further steps in our design to mitigate confounders, as we describe next.

4.3 Additional Covariate Adjustment and Estimation

Numerous options from across the social sciences are available for modeling panel data such as ours in a difference-in-differences (DID) framework. A natural first choice might be the two-way fixed effects estimator, which adjusts for unit- and time-specific confounders and identifies the ATT under the assumption of parallel trends—that the outcome trend would be the same in treated units in the absence of treatment (Imai and Kim 2021). However, our data exhibit several key characteristics that favor alternative strategies. We must accommodate treatment reversals, variable treatment timing (see Figure 1), and a few covariates with low temporal variation (e.g., party control of the legislature). Moreover, we are interested in allowing for heterogeneous treatment effects over time to understand the "legacy" of voter ID laws in several subsequent legislative sessions.[17] Two-way fixed effects models cannot fully accommodate these conditions (Imai and Kim 2021; Liu et al. 2022). Accordingly, we employ Imai et al.'s (2022) PanelMatch estimator, which handles these issues by adapting the logic of matching methods to panel data. We describe it briefly here; see the SI for details on our use of the method. Additionally, in the SI we assess parallel trends, present results with several alternative estimators (including two-way fixed effects) and justify our decision to utilize PanelMatch as the primary estimation tool.

PanelMatch is a design-based method that permits balance assessment in time-varying covariates and estimation of dynamic treatment effects. It is more robust to model misspecification than two-way fixed effects (Imai et al. 2022, 2), though several assumptions are important to note. First, it assumes no spillover effects. A state's potential outcomes are only affected by its own treatment history up to a specified number of lags. We employ the largest lag structure that our data permit to bolster this assumption's credibility (see below). Additionally, the assumption necessitates that the potential outcomes of one state are not dependent on the treatment status of another.

[17] We also compare the effects of earlier to later laws in calendar time (see the SI).

While policy diffusion is a well-known phenomenon in American state politics, this assumption is still reasonable. Even if states borrow these laws from each other, we contend that the internal policymaking dynamics of a state's legislature are relatively unlikely to be directly influenced by the post-adoption *timing* of law implementation in a different state.[18] Finally, like two-way fixed effects, PanelMatch assumes parallel trends (see the SI for supporting evidence).

The method begins by identifying a set of control states for each treated state that carry identical treatment histories as the treated state for a user-defined number of time periods. Specifying many time periods strengthens the credibility of the design, but reduces efficiency because finding matches becomes more difficult (Imai et al. 2022, 7). We are able to use two years of treatment history for this step.[19] Next, within each matched set, the estimator uses the covariates to further match the treated unit with its most similar control units via Imai and Ratkovic's (2014) covariate balancing propensity score (CBPS). Importantly, this step involves further trimming of the data beyond the subsetting discussed above.[20]

Next, PanelMatch implements the DID estimator on the matched sets (which now account for treatment history and covariates) for a user-defined number of time points in the future (Imai et al. 2022, 10).[21] Here we estimate effects up to four years after treatment, which allows for heterogeneity due to election cycles or other factors. Finally, the standard errors condition on the matching procedure and cluster at the state level (Imai et al. 2022, 12).

[18]Furthermore, our results are robust to the inclusion of two common diffusion covariates: ID law adoptions by geographic neighbors and adoptions by policy source states (see the SI).

[19]Our subsetting approach necessarily limits the available data prior to treatment. Nonetheless, two years of treatment history is comparable to existing applications of PanelMatch with data from the American states (e.g., McQueen 2021).

[20]See the SI for more on this process as well as robustness checks with methods that do not remove observations from the data.

[21]This estimator controls for time-invariant characteristics of states as well as potential shocks that affect all units at a given point in time (e.g., redistricting).

5 Results

We report estimated treatment effects from PanelMatch graphically to test our hypotheses. Additionally, along with these estimates we report results from a falsification test. We estimate the "effect" of treatment on the outcomes in the year *prior* to a voter ID law going in force.[22] The treatment cannot affect earlier outcomes, so this quantity is zero in expectation. Thus, estimates near zero in a finite sample can strengthen the credibility of the design. Put differently, the presence of a non-zero effect at $t-1$ could indicate anticipatory behavior of treatment by legislators that would violate the parallel trends assumption.

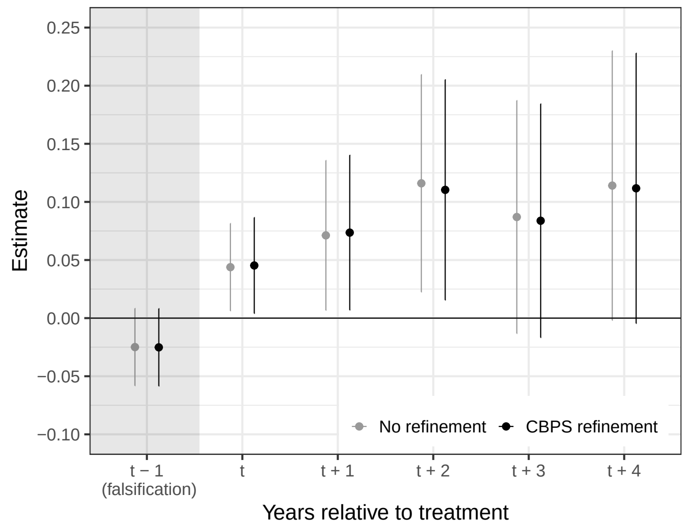
5.1 Voter ID Laws Increase Polarization

Figure 2 presents the estimated contemporaneous effect on party polarization (t) and estimates for one year prior and up to four years since treatment as well as their 95% confidence intervals. The estimates in gray represent equal weighting of the control states that comprise the matched set for each treated state. The estimates in black come from refining the matched sets with covariates via CBPS. The shaded area denotes the falsification estimates. Importantly, these results at $t-1$ demonstrate support for our research design; the estimates are small in magnitude and their confidence intervals include zero. In other words, voter ID laws do not influence polarization in the year before they go in force.

Figure 2 also shows support for H1. All of the treatment effect estimates are positive, indicating that a voter ID law going into effect increases polarization between the two parties in the legislature. The contemporaneous effects are relatively small, although their confidence intervals exclude zero. The estimates then increase noticeably in the next two periods, as more turnover occurs in the legislatures. By two years after treatment the effects on polarization are more than double the contemporaneous effects and their confidence intervals are also bounded away from zero. The estimates remain approximately at that level after three and four years, although our statistical

[22]We omit the lagged outcome as a covariate in these estimates; including it would invalidate the falsification test (Imai et al. 2022).

Figure 2: Estimated Effects of Voter ID Laws' Implementation on Party Polarization in State Legislatures, 2003–2018



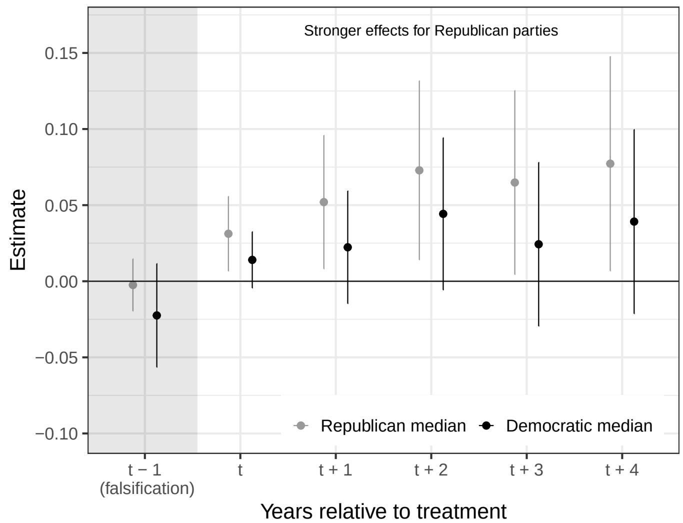
Note: The graph displays years relative to treatment (x-axis) against the estimated effects of voter ID laws going in force on the difference in party median ideal points in state legislatures (y-axis). Line segments indicate 95% confidence intervals. N = 194.

power declines at that point and the confidence intervals include zero again.

Most notably, these effects represent substantively plausible and meaningful effects. Beginning with the year after treatment, they represent 16–27% of a standard deviation in the raw outcome (including within- and cross-state variation), depending on the time point and refinement method. We view these results as noteworthy considering the fact that they show the impact of implementing just one law out of the many bills on which state legislatures deliberate. Moreover, these estimates are similar in magnitude to those reported in Olson and Rogowski's (2020) analysis of term limits and polarization—which employs the same outcome variable (see Table 1, 577). In other words, implementing a voter ID law is comparable in its amplification of ideological differences to the shifts in electoral and career incentives that come with the introduction of fixed legislative terms.

In Figure 3 we repeat the analysis with a new outcome variable to assess whether the effects in Figure 2 are evenly or asymmetrically distributed. The graph shows the treatment effects on ideal point extremity by party—the absolute value of the party medians—in a test of H2. For both parties, positive values indicate movement away from the center and negative values correspond with moves toward the center. All estimates come from matched sets with CBPS refinement.

Figure 3: Estimated Effects of Voter ID Laws' Implementation on Party Median Extremity in State Legislatures, 2003–2018



Note: The graph displays years relative to treatment (x-axis) against the estimated effects of voter ID laws going in force on the absolute value of party medians in state legislatures (y-axis). Line segments indicate 95% confidence intervals. N = 194.

We again see supportive evidence of our research design. The falsification estimates are small in magnitude and not statistically distinguishable from zero.[23] In terms of the treatment effects, the graph suggests that voter ID laws lead both parties to become more extreme. This finding supports our model over the simple rightward shift model discussed above—which predicts that both parties move right to capture the perceived "new" median voter. The effects are also stronger for Republicans compared to Democrats, in line with H2. However, the differences are not statistically distinguishable between parties and the estimates are only significantly different from zero among Republicans. Thus, Figure 3 is *suggestive* of the asymmetric effect that our theory predicts, but caution in interpretation is warranted due to statistical uncertainty.

Overall, these main analyses display general empirical support for our theoretical framework. Voter ID laws polarize state legislatures, especially by driving the average Republican party farther to the right. The SI contains several more analyses that further investigate this pattern. For

[23]However, balance is not quite as strong compared to the polarization variable (see the SI).

example, we find that the effects start small, but grow in subsequent years after implementation of a law as relatively extreme candidates who campaigned under heightened uncertainty begin to replace relatively moderate incumbents. A placebo test demonstrates that the process is specifically a result of voter ID *implementation*—not adoption of the laws. Finally, our findings are robust to numerous other potential confounders, several alternative estimation strategies, and that the effects display some heterogeneity over time and across chambers (see the SI for complete details).

5.2 Assessing the Mechanism

A key consideration in our analyses is the examination of our proposed mechanism: the role of uncertainty about the median voter for legislative candidates. The SI contains several empirical tests, including direct evidence from an original survey of state legislators in 2023. Through experiments and questions, we find supportive evidence that many legislators view predicting elections as more difficult with a voter ID law in place, take action to provide more information to voters, and defer to their own judgments more when they lack knowledge of their districts' preferences.

5.2.1 Other Contentious Policies

Another means of testing the mechanism involves the consideration of other policy issues. Our theory implies that voter ID laws are unique, even among contentious policies over which state legislatures debate, because they affect legislative candidates' perceptions of the composition of the electorate. Accordingly, policies that do not influence who can vote should not exert the same effects on polarization. Specifically, we next examine whether several other contentious state-level policies that are unrelated to election law exert similar polarizing effects.

We searched Boehmke et al.'s (2020) database for policies that diffused across the states during approximately the same time period and for which polling data indicate partisan opinion divergence in the mass public.[24] This search yielded five such policies: (1) in-state college tuition for undocumented persons, (2) permission of same-sex marriage, (3) pre-abortion ultrasound requirements, (4) stand your ground laws, and (5) bans on public accommodation discrimination due to gender identification. Importantly, while these policies feature opposing preferences by party, *none of*

[24] See the SI for diffusion times and summaries of polling data on these issues.

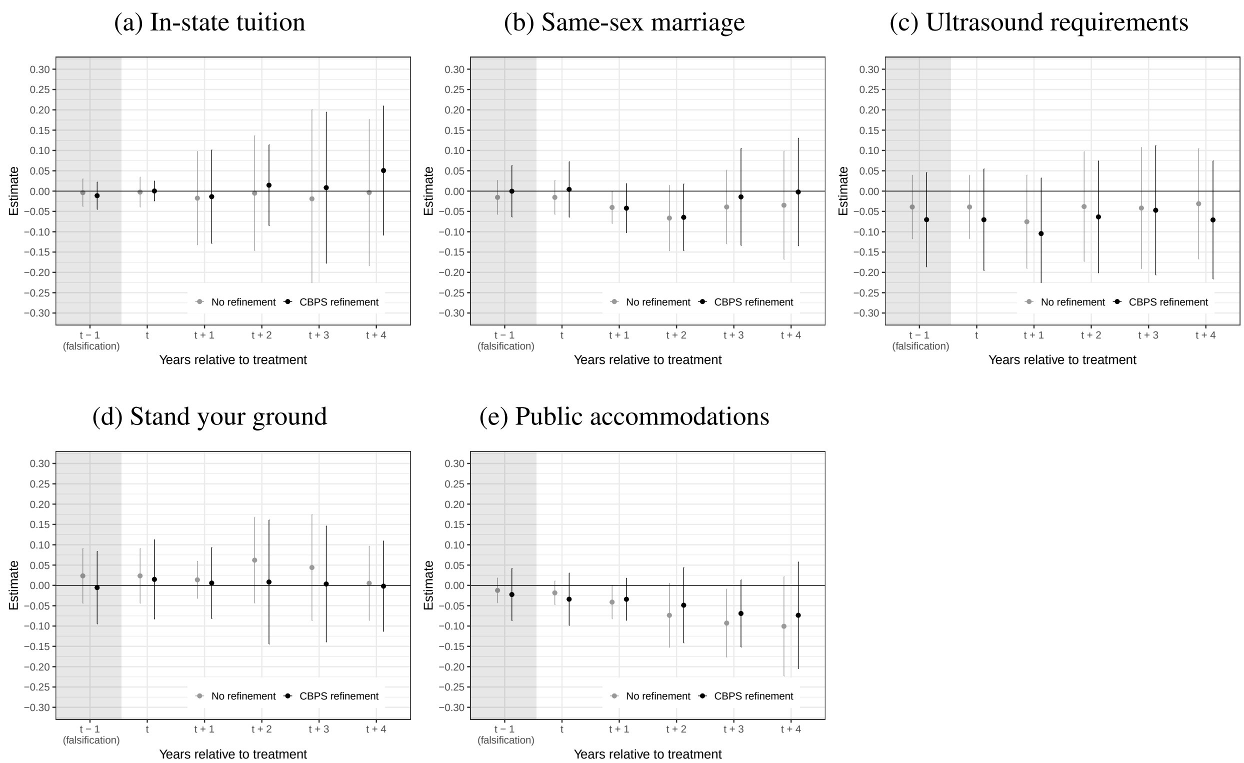
them target the composition of the electorate. Our proposed causal mechanism is absent, and thus our theory would predict that these policies do *not* polarize state legislatures.

To evaluate this contention we repeated the analysis described above with these new policies using the same covariates and the difference in party medians as the outcome. The policies did not exhibit the widespread legal challenges that characterized voter ID laws. They went in force—the key to beginning our theoretical process—without delays. Thus, we consider a state "treated" beginning in the year after a policy was adopted. We cannot use implementation delay as part of our identification strategy here; we must rely solely on the matching procedure and our covariates (measured for all states) to mitigate confounding. This empirical reality warrants relatively more caution regarding causal interpretation, but we maintain that the analysis is still informative as a mechanism test. Moreover, even without variation from implementation delays we find that PanelMatch is quite effective in balancing the covariates with these data (see the SI).

Figure 4 presents the estimates and 95% confidence intervals for each of the new policies. The graphs show essentially no evidence for polarizing effects. Some estimates are positive, but small (e.g., panel a, in-state tuition) while others indicate a decrease in polarization (panel c, ultrasound requirements). But virtually none of the estimates are statistically distinguishable from zero. As with the last analysis, some of the confidence intervals are wide, indicating that large positive (or negative) values are plausible given the data (e.g., panel d). But others are small enough to suggest truly *negligible* treatment effects (see panel e). Most importantly, the weight of the evidence in Figure 4 indicates a noticeably different pattern from that of voter ID laws (Figure 2).

This finding—that five other divisive and contemporaneous state policies are not associated with a clear increase in legislative polarization—further supports our proposed mechanism. A key difference between these policies and voter ID is that only the latter impacts who can vote. We contend that this targeting of voters produces the expectation of uncertainty among candidates necessary to increase polarization. Thus, we would only posit such an effect to result from the implementation of voter ID. That said, we acknowledge that this mechanism test is still not definitive. There are, for instance, other differences between these policies beyond whether or not they target

Figure 4: Estimated Effects of Other Policies' Implementation on Party Polarization in State Legislatures



Note: The graphs display years relative to treatment (x-axis) against the estimated effects of other policies going in force on the difference in party median ideal points in state legislatures (y-axis). Line segments indicate 95% confidence intervals. N = 784 in all analyses.

the electorate. In the SI we conduct several additional mechanism tests and find further support for our theoretical framework.

5.3 Sensitivity to Roll Calls

Our analyses to this point rely on the assumption that lawmakers' ideological preferences are accurately encoded in the roll call record (see note 2). While such an assumption is common in studies of legislative behavior, ideal point estimation from voting can be subject to biases from chamber norms and institutional constraints (Roberts 2007). In this case, perhaps lawmakers are actually no more or less polarized after voter ID law implementation, but the majority party's ensuing agenda is more politically divisive. Such a scenario could yield the appearance of increased polarization without any real change. Do our results reflect a true increase in discord between the parties or are an artifact of the measurement strategy?

Accordingly, we next consider whether we can observe evidence of heightened legislative contentiousness after law implementation *outside* of the roll call record. We turn to literature that employs the timing of the state budgeting process as an indicator of legislative performance (e.g., Klarner et al. 2012). Budget delay is a useful alternative to ideal point measures because it mitigates (though does not eliminate) the potential threat from agenda effects (Kirkland and Phillips 2018; Harden and Kirkland 2021). First, all states are legally required to adopt a new budget, so the majority party cannot leave it off the agenda. The deadline is known ahead of time by all lawmakers, and thus its timing is exogenous to the agenda as well. The stakes are also quite high; failure to pass a budget results in adverse consequences, such as government shutdowns (Klarner et al. 2012). Moreover, budgets are broadly similar across states, making them comparable despite potential differences in states' policy priorities. In general, budget delay is largely insulated from the so-called "denominator problem" in measuring legislative performance (Kirkland and Phillips 2018, 179). It attenuates the difficulty in accounting for variation in the demand for legislation. Thus, while no test can completely rule out agenda effects, budget delay is useful here as an alternative indicator of conflict between the parties. Specifically, we test one final hypothesis.

H3 The implementation of a voter ID law increases the probability of late state budget passage.

We employ data on state budget delay from Klarner et al. (2012) and updated through 2018 by Harden and Kirkland (2021). The outcome is a binary indicator of whether the state's budget was passed after the first day of the new fiscal year. Klarner et al. (2012) develop and test an extensive model of budget delay that we use as a baseline for our own analysis. Specifically, we employ a linear probability model that includes all of the time-varying covariates from Klarner et al. (2012), our own covariates described previously, and our treatment variable.[25] We also include

[25]We select a regression-based approach here instead of PanelMatch due to the large number of covariates in the model. The covariates we add to our existing list include: indicators for election years and divided government, the number of days between the end of the legislative session and

state fixed effects and either year fixed effects or a time counter.[26] Table 1 presents four versions of this model, including specifications using the implementation delay subset data analyzed above (models 1 and 2) and the full data with all states during 2003–2018 (models 3 and 4).[27] The SI reports coefficient estimates for the covariates.

Table 1: Estimated Effects of Voter ID Law Implementation on Late Passage of State Budgets, 2003–2018

	Delay subset		Full data	
	(1)	(2)	(3)	(4)
Voter ID law implemented	0.112 (0.084)	0.142 (0.111)	0.101* (0.046)	0.142* (0.039)
Covariates		✓		✓
State Fixed Effects	✓	✓	✓	✓
Year Fixed Effects			✓	✓
Time Counter	✓	✓		
Adjusted R^2	0.105	0.131	0.281	0.308
N	192	192	734	734

Note: Cell entries report linear probability model coefficients with standard errors multiway clustered by state and year in parentheses. The outcome is a binary indicator for a late state budget. The models with covariates include the time-varying covariates from Klarner et al. (2012) and the covariates used in the analyses above. * $p < 0.05$ (two-tailed).

The treatment effects are positive across the four specifications, indicating that the implementation of a voter ID law increases the probability of a late budget by 10–14 percentage points. These estimates reach statistical significance in the full data ($p < 0.05$), but not in the lower-powered subset. Most importantly, they are large in magnitude and substantively noteworthy in both samples.

the start of the new fiscal year, state personal income, budget size, an indicator for a budget surplus, and legislative salary. See the SI for more details on these variables.

[26]The time counter replaces year fixed effects in the subset data models because some years contain only one or two states, but results are not dependent on this choice. The linear probability model allows us to avoid the well-known incidental parameters problem in nonlinear models with fixed effects (Greene 2004), but results are similar with logistic regression.

[27]Sample sizes are reduced slightly from the previous analyses because some observations are undefined for this outcome due to biennial budgeting.

Late budgets are a somewhat rare event—just 16% of the budgets passed since 1961 (Klarner et al. 2012). Thus, a 10 percentage point increase in the chance of budget passage after the start of the new fiscal year is potentially very consequential for state politics.

This last analysis tests our theoretical framework from a different conceptual angle. It demonstrates that voter ID laws are associated with gridlock in one of state legislatures' most important functions: funding the government for one or two more years. Delay in budget passage indicates a major failure to come to agreement, suggesting that the parties are far apart from each other in their preferences for how state government should operate. In short, our models of late budgets provide novel *indirect* evidence that voter ID laws polarize the parties. Importantly, the potential for bias from the formation of the legislative agenda is reduced (though not eliminated completely) in this analysis. When combined with our more conventional analyses of ideal point measures, the totality of our evidence lends strong empirical support to our theoretical framework.

6 Conclusions

In her 2006 ruling in favor of Indiana's voter ID law, U.S. District Judge Sarah Evans Barker chastised both sides in the case for the lack of evidence they produced to support their respective arguments. She stated that the Indiana Democratic Party "[had not] introduced evidence of a single, individual Indiana resident who will be unable to vote as a result of [the law]." Similarly, Judge Barker pointed out that "the State of Indiana is not aware of any incidents or person attempting vote, or voting, at a voting place with fraudulent or otherwise false identification" (*Indiana Democratic Party v. Rokita*). This assessment reflects an important puzzle in the study of voter ID laws. The extant research largely indicates that they exert negligible influence on both legal voters' access to the polls *and* fraudulent voting, and yet they represent a divisive topic among elites in state governments. How, then, do these laws actually affect American lawmakers?

In this paper we look beyond voter ID laws' effects on voters and find that they hold important implications for state legislatures. We theorize that implementing these laws introduces a new source of perceived uncertainty for legislative candidates regarding where in the policy space their districts' median voters are located. This expected loss of information about the electorate leads candidates to place more weight on their own preferences and, consequently, move away from the ideological center. We posit that this shift is especially strong for Republican candidates, who face more pressure from their party to remain ideologically cohesive after implementation of the law. Ultimately, this process results in an increase in legislative polarization over time as these candidates take office.

We identify the effects of voter ID laws on state legislative party polarization by leveraging delays in implementation that resulted from legal challenges. In many states, lawsuits and/or other hurdles created unpredictable postponements in the enforcement of enacted laws. We argue that this variation in treatment is plausibly exogenous; states that enacted a delayed law are the most appropriate counterfactuals for treated states. Combining this variation with matching and other means of covariate adjustment helps isolate the laws' unique effects. We then show that a law going in force does not affect polarization prior to treatment, but does produce large effects for years afterwards. This pattern may be most prominent among Republican parties and, as expected, only appears after implementation. Finally, the effects are unique to voter ID laws, driven by electoral replacement, and observable in negotiations over states' budgets, which are exogenous to the legislative agenda.

The results presented here are important for the continued study of (1) voter ID in the U.S. specifically and (2) the administration of elections in democracies more broadly. Examining voter turnout was a natural first step in this literature because the laws directly target who can go to the polls. However, politics is a dynamic process and changes in the public also impact elites in government (Ashworth and Bueno de Mesquita 2014). Thus, it is necessary to evaluate this institutional reform beyond its intended effects. We show here that it may actually exert its most notable impact on *lawmakers* rather than citizens. This finding illustrates the great potential for unexpected consequences to arise from efforts to restrict voter access.

Indeed, our findings underscore the point that even one policy can make a significant impact on the democratic process. If extreme partisanship among politicians is both a predictor of voter ID laws *and*—as we demonstrate—an effect of them, then implementing such a law is likely to expand restrictions on voter access in the future. Anecdotally, the debates over the many new restrictive voting bills proposed during the 2021 legislative sessions took place in several states that had already adopted voter ID laws (e.g., Georgia and Texas). Thus, a key legacy of voter ID laws may be to set the stage for future action against the franchise.

More broadly, elite polarization represents a major impediment to good governance in democratic societies. Scholarship makes clear that it is consequential for a range of issues, including redistricting, status quo bias, prospects for compromise, elections and voting, and even polarization in the electorate. Legislatures' capacity to efficiently translate mass preferences into policy is typically weakened by gridlock when the parties are polarized. Thus, any factors that escalate partisan tensions may hamper effective policymaking and the representation of citizen interests. In short, in addition to any effects they may exert on citizens' ability to vote, voter ID laws also hold the potential to *indirectly* obstruct the democratic process by inflaming antagonism among the lawmakers who are charged with addressing significant societal problems with public policy.

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The Legislative Legacy of Voter Identification Laws*

Alejandra Campos[†] Jeffrey J. Harden[‡] Austin Bussing[§]

Supporting Information

Contents

*Replication files are available in the JOP Data Archive on Dataverse (https://dataverse.harvard.edu/dataverse/jop). The empirical analysis has been successfully replicated by the JOP replication analyst.

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A1 Theoretical Model

Our theoretical model is an adaptation of a spatial model of electoral competition from McCarty and Meirowitz (2007, 103–107).[1] The baseline version of our model—which we use to illustrate candidates' decisionmaking when a voter ID law has not been implemented—features ideological candidates who are motivated by both policy and electoral prospects. Just as in McCarty and Meirowitz (2007, 103–105), we assume a policy space that ranges from 0 to 1. Candidates compete in elections by establishing policy platforms in this ideological space. Following Downsian logic, whichever candidate establishes a platform closest to the median voter in the electorate wins the election (Downs 1957). We further assume that the ideal point of the Democratic candidate (D) is 0, the ideal point of the Republican candidate (R) is 1, and the policy utility of each candidate is characterized by the commonly-used quadratic loss function. The policy-oriented portion of the utility function for each candidate can be written as:

$$U_{R_{(x)}} = -(1-x)^2$$

$$U_{D_{(x)}} = -x^2.$$

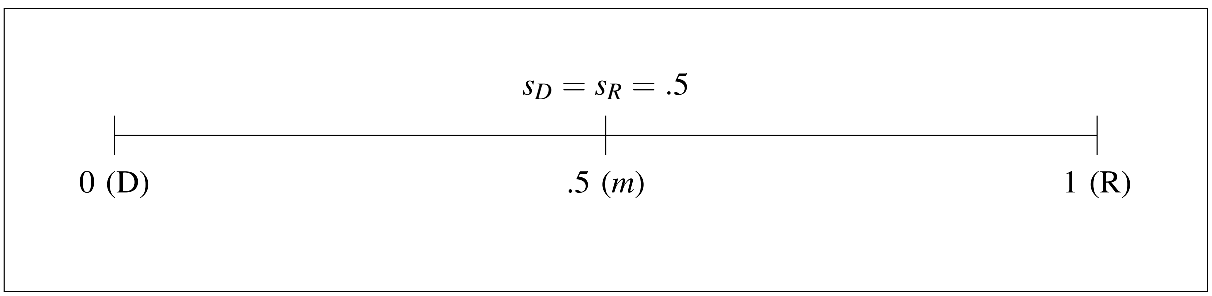
where x is the policy outcome that is realized after the election. These policy outcomes are implemented by the winning candidate, who is constrained to adopt the platform that they put forth in the election. Therefore, if the Democratic candidate won the election by running on a platform $s_D = .5$, she would have to implement a policy of $x = .5$, rather than behaving opportunistically and setting policy at her ideal point of 0.

[1]We use and extend this model primarily as a framework for developing and precisely describing our substantive theory. The model itself is comprised of components that are well known in the formal theory literature. Thus, much of this description will be familiar to readers who have experience with models of electoral competition (e.g., Wittman 1977, 1983; Calvert 1985; McCarty et al. 2019).

A1.1 Baseline Model

In the baseline version of the model, we assume that the location of the median voter is known with certainty by the candidates. This assumption is similar to our contention in the main text that candidates are relatively more knowledgeable about the distribution of voter preferences in their districts when a voter ID law is not in force. As demonstrated by McCarty and Meirowitz (2007, 104–105), the unique Nash equilibrium in this version of the game is that both candidates choose a platform identical to the ideal point of the median voter (m), such that $s^*_D = s^*_R = m$. When the ideal point of the median voter is known with certainty, there can be no unilateral deviation from this equilibrium, as any move away from the median voter would guarantee an electoral loss.[2] We illustrate this equilibrium in Figure A1, assuming that the location of the median voter (m) is .5.

Figure A1: Nash Equilibrium in the Baseline Model



A1.1.1 Rightward Shift Model

One possible result of implementing a voter ID law is that the eligible electorate becomes more conservative. In this scenario, a disproportionate number of voters on the left are deactivated, moving the median voter to the right. It is important to note that the convergence equilibrium described above would also apply to this more conservative median voter, $m > .5$. As long as the location of the median voter is known with certainty to both candidates, neither candidate

[2]The payoff functions that generate the equilibrium make an assumption that when the two candidates' platforms are identical (when $s_D = s_R$), elections are decided by a coin flip. In this scenario, a candidate's payoff is an even lottery between the policy utility they would get if they won and the policy utility they would get if their opponent won.

can benefit (within the constraints of the model) by choosing a platform anywhere other than that location. If either candidate chooses a platform that is in a different location than the median voter (say, to pursue their more extreme policy preferences), the other candidate can guarantee a victory by simply establishing a platform that is closer to the median voter. Because policies can only be implemented by the winning candidate, it is never worth it for a candidate to choose a platform closer to their ideal point if doing so means ensuring electoral defeat.

This logic underlies the rightward shift model. Consider the case in which a voter ID law simply makes the eligible electorate move right by deactivating relatively more voters on the left. Importantly, *if all candidates agree that this process has occurred*, we would not expect an increase in legislative polarization. Instead, we would simply expect candidates from both parties to move to the right in accordance with the logic of the baseline model. However, in our view this model omits the crucial role of uncertainty. We do not assume that all candidates agree the electorate has shifted to the right. We posit that they may believe the median voter could have moved in either direction due to mobilization efforts on both sides (see the main text and below).

A1.2 Treatment Model with Uncertainty (H1)

In the main text we posit that implementing a voter ID law increases the candidates' uncertainty about the composition of the electorate in their districts; specifically, they no longer know where the median voter is located. As demonstrated by McCarty and Meirowitz (2007, 105–107), when ideological candidates are uncertain about the location of the median voter, polarization is predicted to occur in equilibrium (see also McCarty et al. 2019). Our first hypothesis (H1) follows from this logic. To demonstrate this process formally, we take the baseline model described above and change it so that each candidate does not know the median voter's location, but instead believes the median voter is drawn from a uniform distribution over the interval $[0, 1]$. Applying this new assumption to the policy utilities of each candidate laid out above, expected utilities from this model can be written:

$$EU_{D(s_D,\, s_R)} = \begin{cases} -s_D^2(\frac{s_D+s_R}{2}) - s_R^2(1-\frac{s_D+s_R}{2}) \text{ if } s_D < s_R \\ -s_R^2(\frac{s_D+s_R}{2}) - s_D^2(1-\frac{s_D+s_R}{2}) \text{ if } s_D > s_R \end{cases}$$

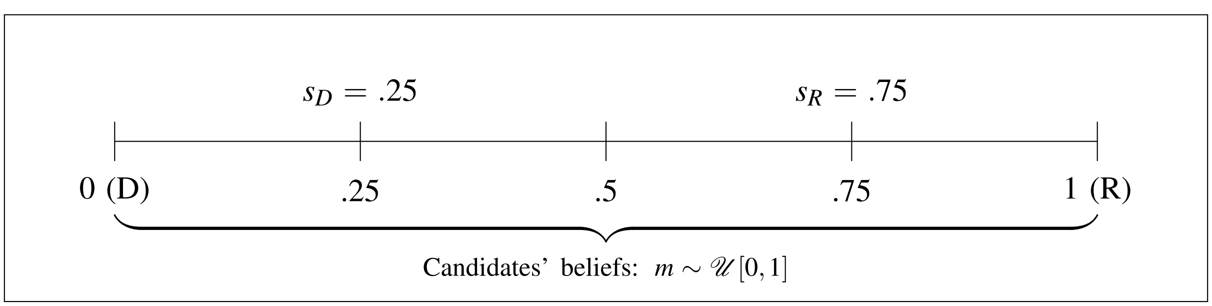
$$EU_{R(s_D,\, s_R)} = \begin{cases} -(1-s_D)^2(\frac{s_D+s_R}{2}) - (1-s_R)^2(1-\frac{s_D+s_R}{2}) \text{ if } s_D < s_R \\ -(1-s_R)^2(\frac{s_D+s_R}{2}) - (1-s_D)^2(1-\frac{s_D+s_R}{2}) \text{ if } s_D > s_R. \end{cases}$$

Again, following McCarty and Meirowitz (2007, 105–107), the following equilibrium platforms are derived from the expected utilities above:

$$s_D^* = \frac{s_R^*}{3}$$
$$s_R^* = \frac{s_D^* + 2}{3}.$$

The unique solution to this system of equations is $s_D = .25$ and $s_R = .75$. It constitutes a Nash equilibrium, as neither candidate would have an incentive to defect from these platforms. Figure A2 illustrates this equilibrium, which shows divergence between the candidates. This result provides the theoretical basis for our empirical expectation described in H1: the implementation of a voter ID law increases uncertainty among candidates, which creates centrifugal pressure on the platforms they adopt and ultimately polarizes the parties in the legislature.

Figure A2: Nash Equilibrium in the Treatment Model with Uncertainty



A1.3 Asymmetric Polarization (H2)

Our second hypothesis (H2) takes the result shown in Figure A2 a step further; it predicts that partisan divergence is stronger among Republicans in state legislatures compared to Democrats. In this section we describe an extension to the model described above that serves as the basis for this prediction. It demonstrates equilibrium conditions in which the Republican candidate moves farther to the right than the Democrat moves to the left.

A1.3.1 Party Tethers

Specifically, here we assume the probabilistic existence of a platform "tether" exogenously imposed by the state Republican party on its candidates for office. Conceptually, this tether is similar to the one-sided tether that Merrill et al. (2014) develop in their model—it establishes a point in ideological space beyond which candidates in the party are not allowed to adopt platforms.[3] We assume that Republican candidates have private information about their type (tethered or untethered), but Democratic candidates are uncertain about which type of Republican candidate they are facing.[4] Therefore, this extension of the model is a simultaneous game of incomplete information.

While Democratic candidates do not know whether they are facing a tethered or untethered Republican candidate, we assume that they do have two important pieces of information—α, which is the probability that a given Republican candidate is tethered, and t_R, which is the location of the tether in ideological space, if it exists. Solving for a Bayesian Nash equilibrium requires us to

[3]Technically, our tether is a simplified version of Merrill et al.'s (2014). Their tethers establish a maximum distance by which a candidate's platform can deviate from the platform of the national party. In the case of a one-sided tether, W_R, the most liberal position a Republican could take would be defined by $R - W_R$, where R is the ideological position of the national Republican party. Our simplified tether, rather than a distance, identifies a specific point in ideological space beyond which the candidate is not allowed to move. Formally, our tether, t, would be defined as $t = R - W_R$.

[4]Merrill et al.'s (2014) model allows for both parties to use tethers on their candidates. We do not do so here because we only expect Republican parties to adopt them as a result of a voter ID law.

know the optimal platform of a tethered Republican candidate ($s^*_{R_{(t=1)}}$), the optimal platform of an untethered Republican candidate ($s^*_{R_{(t=0)}}$), and the optimal platform of a Democratic candidate given the common prior, α ($s^*_{D_{(\alpha)}}$). We solve for each below.

In order to solve for the optimal platforms of both types of Republican, we first restate the utility function of a Republican candidate:

$$U_R = -(t_R - s_R)^2\delta - (1-x)^2(1-\delta).$$

where δ is a dichotomous (and exogenously determined) indicator for whether or not a tether is in place, t_R is the location of the tether, s_R is the platform of the Republican candidate, and x is the policy outcome realized after the election. When a tether is in place ($\delta = 1$), the Republican candidate's utility is purely determined by the ideological distance between their platform s_R and the tether t_R. Only untethered Republican candidates ($\delta = 0$) have a utility that is affected by the post-election policy outcome, x.

Given this utility function, it is straightforward to find the optimal platforms for each type of Republican candidate:

$$s_R{}^* = \begin{cases} t_R \text{ if } \delta = 1, \text{ or} \\ \frac{s_D{}^*+2}{3} \text{ if } \delta = 0. \end{cases}$$

The tethered Republican candidate maximizes her utility by setting $s_R = t_R$, whereas the untethered Republican candidate sets her optimal platform in terms of the Democratic candidate's optimal platform, as we have demonstrated in the previous sections.

We next find the Democratic candidate's optimal platform given α, the common prior about the probability that a Republican tether exists. We use the following expression of the Democratic candidate's expected utility to derive the optimal platform:

$$EU_D = \alpha\left(-s_D{}^2\left(\frac{s_D+t_R}{2}\right) - t_R{}^2\left(1-\frac{s_D+t_R}{2}\right)\right) +$$
$$(1-\alpha)\left(-s_D{}^2\left(\frac{s_D+s_R}{2}\right) - s_R{}^2\left(1-\frac{s_D+s_R}{2}\right)\right).$$

Taking the first derivative of this expected utility with respect to s_D, setting it equal to 0 and solving for s_D yields s_D as a function of s_R and the parameters t_R and α. The result is the following two functions:

$$s_D{}^* = \begin{cases} \frac{1}{3}(-\sqrt{\alpha^2(t_R - s_R)^2 + \alpha(3t_R{}^2 + 2t_R s_R - 5s_R{}^2) + 4s_R{}^2} + \alpha(s_R - t_R) - s_R \\ \frac{1}{3}(\sqrt{\alpha^2(t_R - s_R)^2 + \alpha(3t_R{}^2 + 2t_R s_R - 5s_R{}^2) + 4s_R{}^2} + \alpha(s_R - t_R) - s_R. \end{cases}$$

When we substitute the optimal platform for the untethered Republican candidate ($\frac{s_D+2}{3}$) in for s_R in the functions above, we can solve for s_D solely as a function of the parameters t_R and α. The result is the optimal platform for the Democratic candidate:

$$s_D{}^* = \begin{cases} \frac{-6\sqrt{\alpha^2 t_R{}^2 - 2\alpha^2 t_R + \alpha^2 + 8\alpha t_R{}^2 + 2\alpha t_R - 5\alpha + 4} + 9\alpha t_R - 4\alpha + 4}{5\alpha - 32} \\ \frac{6\sqrt{\alpha^2 t_R{}^2 - 2\alpha^2 t_R + \alpha^2 + 8\alpha t_R{}^2 + 2\alpha t_R - 5\alpha + 4} + 9\alpha t_R - 4\alpha + 4}{5\alpha - 32}. \end{cases}$$

The second of these two functions yields a negative Democratic platform over the range of the relevant parameters, so we eliminate that function and retain the first. This gives us a Bayesian

Nash equilibrium with the following best responses for each player:

$$s_{R(\delta=1)}^{*} = t_R$$

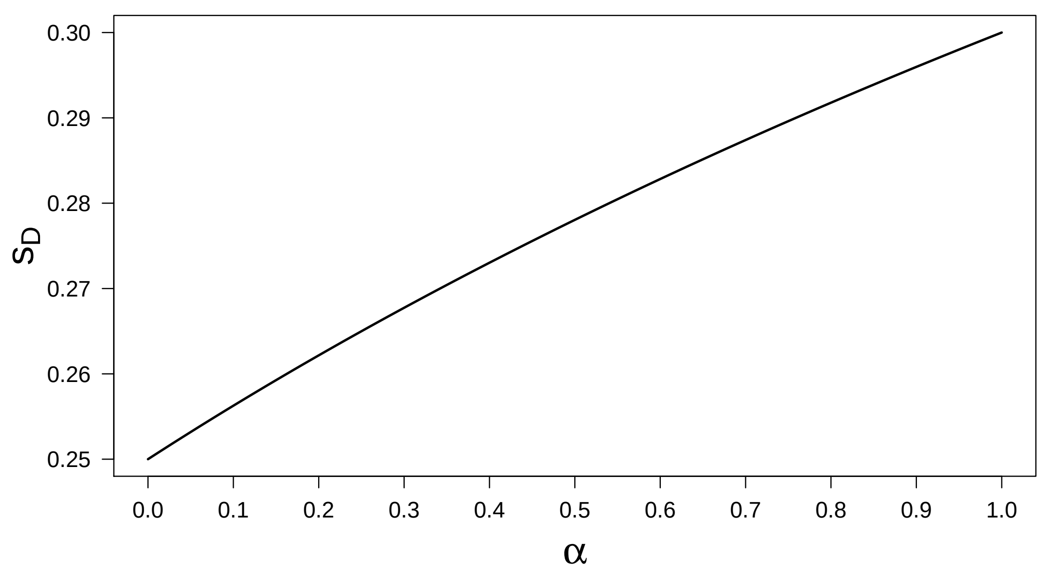
$$s_{R(\delta=0)}^{*} = \frac{s_D{}^* + 2}{3}$$

$$s_D{}^* = \frac{-6\sqrt{\alpha^2 t_R{}^2 - 2\alpha^2 t_R + \alpha^2 + 8\alpha t_R{}^2 + 2\alpha t_R - 5\alpha + 4} + 9\alpha t_R - 4\alpha + 4}{5\alpha - 32}.$$

A1.3.2 Comparative Statics

To explore how the optimal Democratic platform changes as a function of beliefs α with respect to a specific Republican tether, we assign the parameter t_R a value of .9, and derive comparative statics in Figure A3.

Figure A3: Optimal Democratic Platform as a Function of α, $t_R = .9$



The optimal Democratic platforms shown in Figure A3 are intuitive based on the equilibrium from our treatment game with uncertainty, outlined above. When the Democratic candidate is confident that a tether is not in place (i.e. when $\alpha = 0$), she will establish the optimal platform from the previous game (${s_D}^* = .25$). Conversely, when the Democratic candidate is confident that her Republican opponent will be subject to a tether $t_R = .9$ (when $\alpha = 1$), she will establish a platform at .3. Recall from the game in the previous section that the optimal Democratic platform in terms of the optimal Republican platform is ${s_D}^* = \frac{{s_R}^*}{3}$. When the Democratic candidate is confident that her Republican opponent is subject to a tether of .9, it is clear that $s_{R(\delta=1)}^* = t_R = .9$, and furthermore that ${s_D}^* = \frac{.9}{3} = .3$.

Of course, in reality, Democratic candidates will rarely be privy to the specific dynamics at play between a given Republican candidate and the state party organization, and therefore will be operating under uncertainty. To pinpoint a specific equilibrium outcome, and to demonstrate that our basic conjecture of asymmetry (i.e., $[s_R - .5] > [.5 - s_D]$) is supported, we specify a value for both α and t_R. Assuming $\alpha = .5$ and $t_R = .9$, platforms for the Democratic candidates and both types of Republican candidates are as follows:

$${s_D}^* = 0.2780464$$

$${s_{R_{(\delta=0)}}}^* = 0.7593488$$

$$s_{R(\delta=1)}^* = 0.9$$

Substantively, then, asymmetric polarization between the two candidates is a reasonable possibility in this version of the model. The knowledge that a Republican candidate may be subject to a more conservative ideological tether put in place by the state party actually compels Democrats to moderate their platforms. This moderating effect becomes stronger as the Democratic candidate becomes more confident of the existence of a tether—but even a Democratic candidate who is certain a tether is in place would not go all the way back to the median voter. Consider a version of the game in which the Democratic candidate is certain that her Republican opponent is tethered to adopt a platform of 1. Even this extreme scenario would only yield a maximum Democratic platform of .33. In short, Democrats stay left of the expected median (.5), but do not go farther to the left than in the previous version of the model.

When Republican candidates are actually tethered by their state parties, they automatically establish a platform as conservative as the tether requires. When they are not tethered but their Democratic opponent is uncertain of their type, they can take advantage of that uncertainty to establish a marginally more conservative platform than they would have in the previous version of the game (i.e. $\frac{s_D{}^*+2}{3}$, where $s_D{}^* > .25$). Thus, there are equilibria in which the Republican adopts a platform more conservative than .75 and the Democrat selects a platform greater than .25, but less than .5. It is only in one potential scenario—when the Republican candidate is untethered and the Democratic candidate knows this fact with certainty (i.e. $\alpha = 0$)—that polarization is symmetric around the location of the median voter in expectation. This case is the equilibrium outcome from the previous version of the game, in which $s_D = .25$ and $s_R = .75$.

This logic establishes our theoretical expectation in H2. We posit that, after implementing a voter ID law, state Republican parties believe the distribution of voters has shifted to the right, on average. Accordingly, they impose a tether on Republican candidates to strengthen ideological cohesion in their caucus in the legislature. This tether is private information. Announcing it publicly may compel Republican Party leaders to admit that gaining an electoral advantage is an objective of the law. Republican candidates adopt platforms far to the right in accordance with the tether and Democrats take moderate positions to the left, depending on their beliefs about the tether. The result is asymmetric polarization among candidates, which we expect moves the parties in the legislature farther apart mostly due to Republicans' move to the right.

A2 A Direct Test of the Mechanism: State Legislator Survey

In our placebo tests and other analyses of the mechanism presented in the main text and Section A8 below, we report suggestive evidence supporting the causal pathway of our theory. However, to our knowledge no existing research *directly* links the implementation of voter ID laws to increased uncertainty among legislative candidates. This link is important because it is the central connector between the treatment and outcome in our formal model above and in our hypotheses. Accordingly, in January/February 2023 we administered a survey—which included both experimental and direct questions—to current state legislators to assess our proposed mechanism. We describe the details of the survey and its results below.

A2.1 Design and Sample

We administered the survey online through the Qualtrics survey platform.[5] We emailed the link to 7,335 legislators (99% of the population) from all 50 states on January 31, 2023.[6] Our message asked potential respondents to take an anonymous survey about how state legislators "think about their chances of winning elections during campaign season." We sent one reminder email on February 7, 2023 and closed the survey on Feburary 14, 2023.[7] The instrument contained five total questions and the median completion time was 3.6 minutes. We received responses from 2.4% of the email addresses to which we sent it. Specifically, 256 respondents clicked on the link, 174 answered some the of questions, and 159 reached the end of the survey. It is a relatively small sample, so there are limits on our statistical power. But this response rate met our (modest) expectations given our compressed timeframe and the downward trend in responses to state legislator surveys over the past three decades.[8]

[5]This study was approved by the Institutional Review Board at [university name redacted].

[6]The list of legislator email addresses came from Druckman et al. (2023), who created it using Google's Civic Information API. We cleaned and updated the email addresses to reflect turnover from the 2022 elections.

[7]The timeframe was short compared to similar designs in the literature (e.g., Harden 2013; Druckman et al. 2023). This choice partially reflected our desire to field a low-impact survey. State legislators have been inundated with emails about researchers' surveys for the last decade and, in our case, many lawmakers were beginning legislative sessions when they received our request. We sought to balance the scientific value of the survey with an acknowledgment that it took time away from legislators' main responsibilities.

[8]For example, Carey et al.'s (1998) 1995 mail survey obtained a 47% response rate, Harden (2013) reported a response rate of 18% for a 2011 email survey, and Anderson (2019) received a 3.1% response rate for a 2017 email survey. More recently, Druckman et al. (2023) improved on this trend with a 7.8% response rate to an email survey in 2022.

In Table A1 we provide descriptive statistics of our sample compared to the population of state legislators. We received responses from legislators in 42 states. Compared to the population, our sample is over-represented by female legislators (39% in the sample versus 29% in the population), Democrats (49% in the sample versus 43% in the population), and White legislators (89% in the sample versus 79% in the population). Nonetheless, these differences are not unusual compared to other state legislator survey samples (e.g., Harden 2013; Druckman et al. 2023) and so—with appropriate caution—we proceed to our analyses.[9]

Table A1: State Legislator Survey Sample Descriptive Statistics

Variable	Population	Sample	Difference	Ratio
Voter ID state	0.45	0.47	−0.02	0.96
Lower chamber	0.73	0.79	−0.06	0.92
Full-time legislature	0.21	0.20	0.01	1.05
Hybrid legislature	0.52	0.36	0.16	1.44
Part-time legislature	0.28	0.44	−0.16	0.64
Term-limited state	0.27	0.18	0.09	1.50
Female	0.29	0.39	−0.10	0.74
White	0.79	0.89	−0.10	0.89
Black	0.09	0.06	0.03	1.50
Latino	0.05	0.01	0.04	5.00
Asian/Pacific-Islander	0.02	0.03	−0.01	0.67
American Indian/Native American	0.01	0.01	0.00	1.00
Multiracial	0.01	0.01	0.00	1.00
Republican	0.53	0.49	0.04	1.08
Democrat	0.43	0.49	−0.06	0.88
Unaffiliated	0.03	0.01	0.02	3.00
Midwest	0.24	0.22	0.02	1.09
Northeast	0.18	0.37	−0.19	0.49
South	0.32	0.20	0.12	1.60
West	0.26	0.20	0.06	1.30

Note: Population proportions for the race and ethnicity variables come from the National Conference of State Legislatures (2020 data).

Respondents were asked a combination of experimental and direct survey questions designed to

[9]We do not use survey weights in the analyses below. Weighting reduces statistical power (Druckman et al. 2023), which is already in short supply in our case due to the small sample size.

assess how voter ID laws affected their certainty in predicting the results of elections. We describe each question and summarize our analysis below. Full results and the exact wording of the survey instrument are available in the replication files.

A2.2 List Experiment

We began with a list experiment, which is designed to elicit opinions on topics that may be sensitive and subject to social desirability bias (Blair and Imai 2012). Our expectation during the design phase of the survey was that legislators may be wary of providing opinions on a potentially polarizing topic. Thus, we elected to use a list experiment as the first question respondents viewed to maximize our chances at obtaining truthful responses without any priming from previous questions. Respondents were randomly assigned into treatment and control groups, presented with a list of items (in random order), and asked to provide the number of those items that made them "less certain about predicting the outcome of [an election]." The control group received the following list of six nonsensitive items:[10]

1. One candidate has raised and spent a lot more campaign money than the other;
2. Polling data suggest that the race will be close;
3. One candidate's party has won the seat in the last three elections;
4. Traditional media (newspapers, TV) is giving a lot of attention to the race;
5. Social media includes a lot of discussion about the race;
6. Neither candidate has held elected office before.

Respondents in the treatment group saw the nonsensitive items plus the following additional item:

7. The state just implemented a voter identification law requiring a photo of the voter to access a ballot.

[10]The experiment passed Blair and Imai's (2012) test for a design effect, indicating that legislators did *not* respond differently to the nonsensitive items across the treatment and control groups.

Legislators in both experimental groups then provided a count of the items they viewed that made them less certain about predicting elections.

Next, we followed the list experiment with a related, but distinct question. We directly asked all respondents which of the following two perspectives "is more likely to be true."

1. Voter ID laws make predicting election outcomes easier;
2. Voter ID laws make predicting election outcomes more difficult.

This approach gives us two methods of asking the same question, allowing us to compare results.

The quantity of interest with list experiments is the estimated proportion of respondents affirming the sensitive item (that voter ID laws make them less certain about predicting election outcomes). It can be computed as the difference in means between the two experimental groups. Additionally, we model the list experiment outcome with Blair and Imai's (2012) item count technique (ICT) regression, which allows for the inclusion of covariates. In particular, we consider whether partisanship and experience with voter ID are associated with legislators' responses. We specify a model with an indicator for Republican legislators, an indicator for respondents in current voter ID states, and an interaction between the two. As a comparison, we also model responses to our direct question with logistic regression using the same specification plus several covariates.[11]

Table A2 presents the results in the form of estimated proportions affirming the sensitive item (list experiment) and estimated probability of responding that voter ID laws make predicting elections more difficult (direct question). See the replication materials for full ICT regression and logit model coefficient estimates and model results.

The top row of the table gives the overall estimates, averaging across the sample. With both question types, we find that over 40% of state legislators believe that voter ID increases uncertainty in predicting elections ($p < 0.05$). One perspective on this finding is that a majority of legislators

[11]Specifically, we control for members of lower chambers, gender, race, legislative session length (full-time, part-time, or hybrid), and the treatment indicator from the list experiment (to adjust for priming).

Table A2: Estimated Proportion Affirming the Sensitive Item, List Experiment and Direct Questioning

	List experiment		Direct question	
Variable	Estimate	SE	Estimate	SE
All respondents	0.436	0.207	0.419	0.120
Republicans in Voter ID States	0.343	0.247	0.293	0.111
Republicans in Non-voter ID States	0.458	0.756	0.358	0.128
Democrats/Unaffiliated in Voter ID States	0.411	0.797	0.608	0.136
Democrats/Unaffiliated in Non-voter ID States	0.518	0.506	0.462	0.127

Note: Cell entries report the estimated proportions and standard errors (SE) of respondents affirming the sensitive item (voter ID laws reduce certainty in predicting elections) for each combination of partisanship and voter ID state status. The first two columns show list experiment results and the second two columns report probability estimates from a logit model of the direct question. $N_{list} = 173$; $N_{direct} = 155$.

disagree with the statement. However, we view it as a large and meaningful group, even if it is a numerical minority. We contend that this result provides at least some preliminary evidence for our uncertainty mechanism. The results also show some variation by partisanship and voter ID status: the estimates are lower for Republicans and those in voter ID states.[12] This heterogeneity may indicate real differences of opinion. However, it could also stem from Republicans, especially in voter ID states, refusing to respond negatively toward laws that their party enacted. Accordingly, we also asked alternative questions to examine our mechanism in different ways.

A2.3 Assistance to Voters

We also asked respondents whether they provided "potential voters with any information about the requirements necessary to vote" in their last election. We model this binary outcome with a logit model using a similar specification as above: partisanship interacted with the indicator for legislators in voter ID states, lower chamber, gender, race, and session length. Table A3 reports the results as estimated probabilities of a "yes" response to the question.

[12] Across the two question types the results are generally similar. However, the estimates are larger in the list experiment for Republicans, but not Democrats. In other words, Republicans were more forthcoming about a potential problem with voter ID in the list experiment (as expected), but Democrats pointed to the problem more often in the direct question. This pattern suggests, unsurprisingly, that respondents viewed the questions through partisan lenses.

Table A3: Estimated Probabilities of Providing Voting Requirements Information

Variable	Estimate	SE
All respondents	0.597	0.106
Republicans in Voter ID State	0.547	0.121
Republicans in Non-voter ID States	0.402	0.128
Democrats/Unaffiliated in Voter ID States	0.863	0.078
Democrats/Unaffiliated in Non-voter ID States	0.628	0.117

Note: Cell entries report logit model estimates of the probabilities and standard errors (SE) of respondents stating that they provided information about voting requirements in their last election. N = 159.

The results show that providing voter information is common—the estimate averaging across respondents is nearly 60%. However, there are large differences by party and voter ID status. Democrats (and unaffiliated legislators) are much more likely to have provided assistance in their last elections compared to Republicans. But more importantly, within each partisan group there is a notable increase in probability moving from a non-voter ID state to one with a voter ID law. Among Republicans this increase is nearly 15 percentage points (although not significant) while for Democrats/Unaffiliated it is about 24 percentage points ($p < 0.05$). This result demonstrates that legislators in voter ID states—and even those in the Republican Party—are more likely than their counterparts without voter ID laws to take on a costly behavior in an attempt to reduce voters' uncertainty. It suggests that they view the campaign environment as more uncertain with a voter ID law in place.

A2.4 Sources of Information Under Uncertainty

The second part of our theoretical logic posits that, with less information about the median voter's location, candidates move toward their own preferences. This aspect of the mechanism can also be evaluated empirically. Our next survey item posed the following question to lawmakers:

> Imagine that a new bill is up for debate and you are unsure about your constituents' opinions on the issue. Which of the following actions would you most likely take? Choose any that apply.

The response options, presented in random order, were as follows:

1. Conduct surveys or meet constituents to learn their opinions;
2. Rely on my own analysis and judgment of the bill;
3. Consult with members of my party;
4. Listen to advice from lobbyists.

The most commonly chosen response was “rely on my own analysis and judgment” (71%), followed by “conduct surveys or meet constituents” (60%), “consult my party” (36%), and “listen to lobbyists” (21%).[13] The “rely on my own judgment” proportion is statistically significantly different from the other three ($p < 0.05$). We interpret this finding as favorable evidence toward the other component of our theoretical model. When elites are uncertain about their constituents' opinions, they most often use their own preferences to make a decision.

A2.5 Prediction Experiment

Finally, we presented a simple vignette experiment with two conditions to our respondents. The short vignette (reproduced below) described a hypothetical election scenario. The text in brackets, which signaled whether or not the state in the scenario required a photo ID to vote, was randomized between the treatment and control groups.

> An incumbent legislator retired in 2022 and both the Republican and Democratic parties recruited and helped fund candidates to fill the seat. Historically, the district is about 46% Democratic voters, 40% Republican voters, and 14% independents. [Additionally, a new voter ID law requiring a photo to access a ballot went into effect for the general election in the state./The state does not require photo identification to access a ballot.]

After randomly viewing one version of the vignette, respondents were then asked to “estimate the chance that the Republican candidate won this race.” The response options were given on a 0–100 scale, with 0 indicating no chance the Republican won, 100 indicating that the Republican definitely won, and 50 indicating an equal chance of the Republican and Democrat winning.

[13] These results are substantively similar after stratifying the sample by voter ID state.

We then created a measure of uncertainty by folding this variable to a range from 0 to 50, with lower values indicating relatively less uncertainty (i.e., values near 0 or 100 on the original scale) and higher values signifying more uncertainty. A value of 50 indicates the most uncertainty in the outcome—that the race is as unpredictable as a fair coin flip. Our proposed mechanism—that voter ID increases candidate uncertainty—implies the prediction that respondents in the treatment group (voter ID law in effect) registered more uncertainty in their predictions compared to the control group. Table A4 reports the results of regressing a standardized version of the outcome on the treatment indicator, first without covariates, then with controls for party, voter ID status, lower chamber, gender, race, and session length.

Table A4: Estimated Treatment Effects in the Prediction Experiment

	Estimate	SE
No covariates	0.174	0.160
With covariates	0.119	0.166

Note: Cell entries report standardized treatment effect estimates and standard errors (SE) with and without covariates in the prediction experiment. N = 157.

The estimates are positive in both cases, indicating that the treatment group was 12-17% of an outcome standard deviation more uncertain than the control group. However, neither estimate reaches statistical significance at conventional levels. These results suggest support for our expectation, but we must be cautious in our inferences because negative estimates are plausible given the data. Nonetheless, this experiment was unique as an examination of our mechanism in that it asked legislators to make a guess about a hypothetical election outcome rather than simply answer questions about their ability to predict election results.

A2.6 Conclusions

Our short survey provides (to our knowledge) the first direct test of the claim that voter ID laws increase legislative candidates' uncertainty about the electorate in their districts. We asked a wide range of question types: a list experiment, direct questioning about uncertainty, questions about how legislators conduct their jobs, and a vignette experiment. Across all of these designs, the data suggest support for our claim. We also find evidence that lawmakers address uncertainty about their districts by using their own judgment to make choices.

Of course, we must readily acknowledge two key limitations. First, as noted above the relatively small sample size reduces our statistical power to make inferences. Some of our estimates are statistically significant, but in other cases we can only rely on the direction and magnitude of the estimates as descriptive evidence of our expectations in the sample. Second, our respondents are legislators serving in 2023, years after the implementation of voter ID in many states. Their present-day perspectives on voter ID may or may not be the same as the perspectives of legislative candidates running for office when these laws were new.

That said, the results of all of the questions we asked consistently point in favor of our theoretical process as a plausible reality. This pattern appears in experiments and questions that probe different aspects of the mechanism and in different ways. As such, we believe that this survey yields at least preliminary evidence in favor of our uncertainty mechanism. We hope that this work will motivate future research to improve on our design, evidence, and theory underlying the theory that we posit in this research.

A3 Background on States' Voter ID Laws

Here we report additional information on voter ID laws in the states and our coding decisions. We discuss scope conditions for our analyses, summarize histories of voter ID law implementation, and present variation in the sources of legal delays across states.

A3.1 Analyzing Post-HAVA Voter ID Laws

In this research we aim to assess the impacts of the voter ID laws that have been the primary subject of political controversy in recent years. Multiple analyses point to the federal government's 2002 Help America Vote Act (HAVA) as a catalyst for this modern wave of laws (Williams 2004; MIT Election Data + Science Lab 2021). Additionally, the scholarly work on voter ID primarily employs data generated after HAVA (e.g., Hajnal et al. 2017; Grimmer et al. 2018; Cantoni and

Pons 2021).[14] HAVA arose, in part, out of the controversy surrounding the 2000 presidential election. Among its many provisions, the law established a baseline set of ID requirements:

- New voters must provide a driver's license number, last four digits of a social security number, or voter registration number;
- Voters who registered to vote by mail after January 1, 2003, who have not voted previously in a federal election in the state, and who do not fall within certain exceptions are permitted to vote on a machine only after presenting one of the following items to election officials: current and valid photo identification, utility bill, bank statement, government check, pay check, or government document that shows the voter's name and address.

States were charged with implementing these requirements and many pursued more restrictive identification rules along the way. This federal mandate to take action marked a political moment that set these states on a course to the current controversy. The ID provisions listed above "were a concession made by Democratic sponsors of HAVA to Republicans to get the law passed in the Senate" (MIT Election Data + Science Lab 2021). Soon after, Republican-controlled states seized on this momentum to make ID requirements more strict. These post-HAVA laws were the first enacted and implemented during a period in which voter ID was an issue with relatively more divergence in opinion between the parties, especially among elites. Prior to HAVA, voter ID simply was not controversial (MIT Election Data + Science Lab 2021).

The few laws that existed before HAVA took effect—in Michigan (1996),[15] Louisiana (1997), and Florida (1998)—were quite weak by modern standards (MIT Election Data + Science Lab 2021). In a summary of voter ID history in the United States, the National Conference of State Legislatures (NCSL) refers to post-HAVA laws as a "new form" of voter ID law that reflected

[14]Importantly, these studies face data constraints that prevent analysis prior to HAVA. Our outcome variable exists going back to 1993, but we mostly focus on the post-HAVA period for theoretical reasons, as we describe here (see Section A8.3 for a placebo test using pre-HAVA laws).

[15]Despite its relatively weak requirements, Michigan's law did face a significant implementation delay due to the court system. It was not in force until 2007 (Lowe 2007).

a trend away from "requesting" ID—as had been done in the states with pre-HAVA laws—and toward requiring it (National Conference of State Legislatures 2021). Indeed, the pre-HAVA voter ID laws simply did not impose the same level of burden on voters lacking proper ID compared to laws passed after HAVA. Provisional ballots were easy to obtain (even before provisional ballots were required nationally by HAVA) and voters using them did not need to take steps after voting or take on significant legal risk in using them. ReckDahl (2015) reports that, in Louisiana, "the poll commissioners' handbook distributed by the Secretary of State's Office says on page 25, in red capital letters, 'DO NOT TURN AWAY A VOTER FOR LACK OF PHOTO ID.'" This lax approach contrasts with the post-HAVA laws, which include at least some burden, and in many cases, substantial burdens on voters who lack ID (National Conference of State Legislatures 2021).

Without a major burden on voters lacking ID, a law cannot be expected to meaningfully impact the electorate. Thus, our theoretical mechanism of increased uncertainty among candidates is absent. While our coding scheme does allow for some heterogeneity in the laws, we contend that these pre-HAVA laws simply do not exhibit a sufficient obligation on voters to animate our theory. Accordingly, we focus only on laws passed in 2003 or later in our analyses. We code the pre-HAVA states as untreated in our data for the main analyses and most of the analyses in this SI document. The one exception appears in Section A8.3. There we leverage our claim that the pre-HAVA laws do *not* animate our theoretical framework to empirically construct a placebo test. We estimate negligible effects of those laws on party polarization, providing further support for our theory.

A3.2 Implementation Histories

Table A5 provides brief summaries of states' voter ID laws passed since 2003 (the year after HAVA was passed). The column labeled "Strict" refers to whether the laws are coded as strict from the NCSL's definition (see the main text). The column "Photo ID" denotes whether the law includes a photo ID requirement. The Enacted and In Force columns report years in which the laws were enacted and began in force, respectively. The last column describes the varying legal histories of each law, which constitute the first component of our identification strategy.

Recall that we consider a state treated in a given year if the law was in force for general elections in that year or at least half of the year if no general elections took place. For instance, the North Carolina voter ID law was very briefly in effect for the primary elections of 2016, but not the general elections in that year (Grimmer and Yoder 2022). Thus, we consider the state untreated in 2016 (although our substantive conclusions do not depend on this choice).

Table A5: Summaries of Voter ID Laws' Implementation Histories

State	Strict	Photo	Enacted	In Force	Summary
Alabama	✓	✓	2011	2014	Implementation delayed while the state sought preclearance from the Department of Justice; Law was challenged in federal court in 2015, upheld in 2020.
Arizona	✓		2004	2005	Passed by ballot initiative and immediately challenged in federal court; Law was very briefly enjoined in 2004 and 2006, but the voter ID requirement was upheld by the Supreme Court in 2013.
Arkansas	✓	✓	2013	2013, 2017	In force in 2013, but struck down by the Arkansas Supreme Court in 2014; Subsequent efforts to revise the law resulted in passage of the photo ID component in 2017.
Georgia	✓	✓	2005	2008	Implementation delayed while the state sought preclearance from the Department of Justice; Multiple challenges were filed in state and federal courts and were ultimately resolved in favor of the law in late 2007.
Idaho		✓	2010	2010	Implemented with no significant challenges or delays.
Indiana	✓	✓	2005	2008	Immediately challenged in federal court; Law was upheld by the Circuit Court of Appeals in 2007 and Supreme Court in 2008.
Kansas	✓	✓	2011	2012	Implementation delay to seek preclearance from the Department of Justice (some localities covered by Section 5); Law was challenged in federal court in 2012, but the case was dismissed in 2014 without further delaying implementation.
Mississippi	✓	✓	2011	2014	Passed by ballot initiative in 2011, but required implementing legislation in 2012; Implementation delay to seek preclearance from the Department of Justice.
New Hampshire		✓	2012	2013	Implementation delay to seek preclearance from the Department of Justice (some localities covered by Section 5).

Continued...

Table A5, continued

State	Strict	Photo	Enacted	In Force	Summary
North Carolina	✓	✓	2013	—	Law was immediately challenged in federal court, causing a delay; State approved a constitutional amendment and implementing legislation in 2018 that were also subject to lawsuits in state and federal courts through 2021.
North Dakota	✓	✓	2013	2013	Law was immediately challenged in federal court; Some elements of the law were briefly enjoined in 2016, but the legislature modified the law and the photo ID requirement remained throughout; Injunction was vacated by the Court of Appeals in 2019.
Ohio	✓		2006	2006	Implemented with no significant challenges or delays.
Oklahoma		✓	2009	2011	Placed on ballot in 2009 and passed by initiative in 2010; Implemented with no other significant challenges or delays.
Pennsylvania	✓	✓	2012	—	Law was struck down by state courts in 2014; Subsequent legislative efforts to revive the law since then have not been successful.
Rhode Island		✓	2011	2014	Implementation delay to seek preclearance from the Department of Justice (some localities covered by Section 5); Implemented with no significant court challenges.
South Carolina		✓	2011	2013	Implementation delayed while the state sought preclearance from the Department of Justice; Preclearance was initially denied in 2011 and 2012, then granted in late 2012 to begin in 2013.
South Dakota		✓	2003	2003	Implemented with no significant challenges or delays.
Tennessee	✓	✓	2011	2011	Law was challenged in state and federal courts and modified slightly, but not altered substantially and never delayed; Law was upheld by the Court of Appeals in 2012 and state supreme court in 2013.

Continued…

Table A5, continued

State	Strict	Photo	Enacted	In Force	Summary
Texas	✓	✓	2011	2013, 2018	Implementation delayed while the state sought preclearance from the Department of Justice; Preclearance was initially denied in 2012, but this denial was reversed in 2013 when Section 5 of the Voting Rights Act was overturned; Numerous legal challenges ensued, leaving the law unenforced during 2015–2017 (with temporary, less restrictive rules in place); The photo ID component returned in 2018.
Virginia	✓	✓	2012	2013	Implementation delayed while the state sought preclearance from the Department of Justice; Implemented with no other significant challenges or delays.
Wisconsin	✓	✓	2011	2015	Immediately challenged in state and federal courts; Law was upheld by the state supreme court in 2014 and when the Supreme Court declined to hear the case in early 2015.

Note: Cell entries summarize voter ID laws in the states since 2003. The column labeled Strict refers to whether the laws are coded as strict from the NCSL's definition (see the main text). The column Photo ID denotes whether the law includes a photo ID requirement. The Enacted and In Force columns report years in which the laws were enacted and began in force, respectively.

A3.3 Sources of Implementation Delay

One potential threat to our claim that the timing of voter ID law implementation is exogenous is systematic variation in state court systems. For instance, some states' courts may be more sympathetic to proponents' or opponents' cases or maintain greater capacity to make a decision on the laws. We control for this possibility directly in our main analyses with a state-year measure of court strength. However, an additional piece of evidence that addresses this concern is variation in the sources of implementation delay. Table A6 presents which of four main sources of delay apply to each state: state courts, federal courts, Section 5 preclearance requirements, or ballot initiative.

Table A6: Primary Sources of Voter ID Law Implementation Delay

State	State courts	Federal courts	Section 5	Ballot initiative
Alabama		✓	✓	
Arizona		✓		✓
Arkansas	✓			
Georgia	✓	✓	✓	
Idaho				
Indiana		✓		
Kansas		✓	✓	
Mississippi			✓	✓
New Hampshire			✓	
North Carolina	✓	✓		
North Dakota		✓		
Ohio				
Oklahoma				✓
Pennsylvania	✓			
Rhode Island			✓	
South Carolina			✓	
South Dakota				
Tennessee	✓	✓		
Texas		✓	✓	
Virginia			✓	
Wisconsin	✓	✓		

Note: Cell entries report the primary sources of delay across the treated states: state courts, federal courts, Section 5 preclearance requirements, or ballot initiative.

The table shows that only six of the 21 treated states experienced implementation delay from the state court system. Moreover, in four of those cases, delays originated from other sources beyond just state courts. Only two states (Arkansas and Pennsylvania) experienced delay solely from their state court systems. Thus, in addition to controlling for court strength directly, many states in our sample are insulated from the potential threat to exogeneity of that factor.

A3.4 Treatment Coding Notes

Table A7 reports our notes on coding the treatment variable for every state. In all cases we began with the NCSL database (National Conference of State Legislatures 2021), then expanded our search to other sources, including primary source documents such as bill text, previous scholarly literature on voter ID laws, media coverage of the legislation and state governments, and information aggregators such as Ballotpedia.org.

Our coding is generally aligned with NCSL, although in a few states we disagree slightly, as noted in the table. For instance, NCSL codes Alabama as non-strict, but includes the following note:

> Some might call Alabama's law a strict photo identification law, because voters who don't show a photo ID will generally be asked to cast a provisional ballot and then must bring the required ID to an election office by 5 p.m. on Friday after Election Day. However, there is an alternative: two election officials can sign sworn statements saying they know the voter (National Conference of State Legislatures 2021).

Following Hajnal et al. (2017), we code Alabama as strict because we view the alternative of two sworn statements from election officials to be extremely burdensome, and thus not a reasonable substitute for a photo ID.

Table A7: Treatment Variable Coding for All States

State	Notes
Alabama	Passed in 2011 to seek preclearance; In force in 2014; Coded as strict due to major provisional ballot burden
Alaska	Not in NCSL voter ID history; Acceptable ID includes utility bills; No major provisional ballot burden
Arizona	Passed by ballot initiative (Proposition 200) in 2004; Court challenges never delayed significantly; Upheld by SCOTUS in 2013
Arkansas	Strict photo in 2013; Struck down 2014-2016; Photo beginning in 2017, includes major provisional ballot burden
California	Not in NCSL voter ID history; ID is only required for first time voters (non-photo)
Colorado	Acceptable ID includes utility bills; No major provisional ballot burden
Connecticut	Not in NCSL voter ID history; Acceptable ID includes utility bills; No major provisional ballot burden
Delaware	Not in NCSL voter ID history; Acceptable ID includes multiple non-photo options
Florida	Photo ID required; Law pre-dates HAVA (coded as untreated); No major provisional ballot burden
Georgia	Passed in 2005; Implemented in 2008 due to court challenges
Hawaii	ID required only if requested (non-photo)
Idaho	Photo ID required; Some provisional ballot burden
Illinois	Not in NCSL voter ID history; ID not required
Indiana	Passed in 2005; Implemented in 2008 due to court challenges
Iowa	Acceptable ID includes multiple non-photo options
Kansas	In force beginning in 2012
Kentucky	Acceptable ID includes multiple non-photo options; No major provisional ballot burden
Louisiana	Photo ID required; Law pre-dates HAVA (coded as untreated); No major provisional ballot burden
Maine	Not in NCSL voter ID history; ID not required
Maryland	Not in NCSL voter ID history; ID not required
Massachusetts	Not in NCSL voter ID history; ID not required
Michigan	Photo ID required; Law pre-dates HAVA (coded as untreated); No major provisional ballot burden; Passed in 1996, delayed in state court system until 2007
Minnesota	Not in NCSL voter ID history; ID not required
Mississippi	Citizen initiative passed in 2011; Legislation in 2012; In force in 2014
Missouri	Strict photo passed and struck down by the state Supreme Court in 2006
Montana	Acceptable ID includes utility bills; Major provisional ballot burden
Nebraska	Not in NCSL voter ID history; ID not required
Nevada	Not in NCSL voter ID history; ID not required
New Hampshire	NCSL labels it non-strict, non-photo, but ID requirements are all photo; Moderate provisional ballot burden; Received preclearance in 2012
New Jersey	Not in NCSL voter ID history; ID not required
New Mexico	Not in NCSL voter ID history; ID not required
New York	Not in NCSL voter ID history; ID not required
North Carolina	Strict photo in 2013; Revised 2015 (photo only); Not in force until 2021
North Dakota	Code as non-strict in 2016; Photo ID required only in 2015-2016
Ohio	Non-photo options are acceptable; Major provisional ballot burden
Oklahoma	Placed on ballot by legislature in 2009, approved by voters in 2010; NCSL labels it non-strict, non-photo, but ID requirements are primarily photo
Oregon	Not in NCSL voter ID history; ID not required
Pennsylvania	Enacted 2012, struck down by state courts in 2014, not yet in force
Rhode Island	Enacted 2011; In force 2014; Major provisional ballot burden
South Carolina	Enacted 2011; In force 2013; Moderate provisional ballot burden
South Dakota	No major provisional ballot burden
Tennessee	Challenged in court, but never delayed
Texas	Preclearance in 2012 initially denied; Temporary lax rules in place 2016-2017; Photo ID back in force beginning 2018
Utah	Photo ID not required; No major provisional ballot requirements
Vermont	Not in NCSL voter ID history; ID not required
Virginia	Strict, photo through 2018
Washington	Not in NCSL voter ID history; Most people vote by mail; No major provisional ballot requirements
West Virginia	Multiple non-photo ID options; Moderate provisional ballot requirements
Wisconsin	Passed in 2011; Fully in force in 2015
Wyoming	Not in NCSL voter ID history; Strict law passed in 2021

Note: Cell entries report our notes on coding the treatment variable for every state. In all cases we began with the NCSL database (National Conference of State Legislatures 2021), then expanded our search to other sources, including primary source documents, media coverage of state government, and information aggregators such as Ballotpedia.org.

A4 Additional Empirical Information

Here we present additional information regarding our empirical analyses. We summarize the variables used in the analyses, report results from the factor analysis model of court strength, and present treatment effect estimates by legislative chamber.

A4.1 Variable Summaries

Here we discuss the measurement and sources of the variables used in our analyses. Table A8 reports descriptions, data sources, and means and standard deviations (SD) for each variable, categorized as outcomes and treatment, covariates used in the main text, and additional covariates used in Section A6.1 of this document. The summary statistics reflect the legal delay subset data (N = 194) that we use for most of our analyses. See Section A9 for a description of the data used in the late budget models.

The electoral competition measure is computed as follows (Shufeldt and Flavin 2012): $100 - \frac{\text{Average \% vote for winners + Average margin of victory + \% uncontested seats + \% safe seats}}{4}$. A "safe seat" is defined as a race where the winning candidate receives more than 55% of the vote. Policy "source" states come from Desmarais et al. (2015). They infer latent networks of states based on their repeated policy adoption patterns over several decades. A state is a source for another state if its decision to adopt many policies is highly predictive of subsequent adoption by the second state.

Table A8: Variable Descriptions and Summary Statistics

Variable	Description	Source	Mean	SD
		Outcomes and Treatment		
State legislative party polarization	Difference in party median ideal points, averaged over chambers	Shor and McCarty (2011)	1.586	0.433
Polarization without first-year legislators	Same as above, omitting legislators in their first year	Shor and McCarty (2011)	1.575	0.429
Republican extremity	Absolute median ideal points for Republican parties	Shor and McCarty (2011)	0.868	0.219
Democratic extremity	Absolute median ideal points for Democratic parties	Shor and McCarty (2011)	0.718	0.352
Treatment	Voter ID law in force	NCSL	0.758	0.430
Adoption (full data)	Voter ID law adopted	NCSL	0.249	0.433
In-state tuition treatment (full data)	Placebo law adopted	Boehmke et al. (2020)	0.226	0.418
Same sex marriage treatment (full data)	Placebo law adopted	Boehmke et al. (2020)	0.231	0.422
Ultrasound requirement treatment (full data)	Placebo law adopted	Boehmke et al. (2020)	0.324	0.468
Stand your ground treatment (full data)	Placebo law adopted	Boehmke et al. (2020)	0.324	0.468
Public accommodations treatment (full data)	Placebo law adopted	Boehmke et al. (2020)	0.230	0.421
		Main Covariates		
Electoral competition	4-year average of state legislative election results; includes victory margins, safe seats, uncontested seats	Holbrook and Van Dunk (1993); Klarner (2021)	37.241	10.244
Legislative party control	1 = Complete Republican control; 3 = One party controls each chamber; 5 = Complete Democratic control	Klarner (2021)	1.278	0.919
Republican governor	Indicator for Republican governor in office	Klarner (2021)	0.809	0.394
State governmental ideology	Liberalism of state's political leaders	Berry et al. (1998)	30.352	12.430
State citizen ideology	Liberalism of state's citizens	Berry et al. (1998)	43.493	11.606
State court strength	Latent measure of state court system's capacity to rule on a voter ID law	See Section A4.2	−0.051	0.094
		Additional Covariates		
Proportion nonwhite	Linear interpolation between Census years	U.S. Census	0.286	0.121
State racial resentment	Survey-based estimate of state-level racial resentment	Smith et al. (2020)	0.667	0.036
Term limits	Indicator for legislative term limit in effect	NCSL	0.309	0.463
Legislative professionalism	Multidimensional scaling (first dimension) of salary, session length, expenditures, and staff	Bowen and Greene (2014)	−0.191	1.299
Total bills introduced	Logged count of substantive (non-resolution) bills introduced in a legislative session	Council of State Governments (2018)	7.312	0.919
Neighbors adopting	Proportion of a state's geographic neighbors that are treated	NCSL	0.264	0.271
Sources adopting	Proportion of a state's policy sources that are treated	Desmarais et al. (2015)	0.361	0.273

Note: Cell entries report variable descriptions, data sources, and means and standard deviations (SD). See Section A6.1 of this document for analyses with the additional covariates. Unless noted otherwise, the summary statistics reflect the legal delay subset data (N = 194) that we use for our main analyses.

A4.2 Court Strength Factor Analysis Model

We conceptualize court strength as a latent variable, which we estimate using the following measures:

- An indicator for divided state government. Hall and Windett (2015) find that courts are stronger in states with divided government.
- An indicator for states that appoint judges in courts of last resort. The literature suggests that appointed judges are more insulated from the public compared to elected judges (Canes-Wrone et al. 2014; Mallinson and Zimmerman 2022). Thus, appointed judges may be less constrained in their capacity to strike down a voter ID law.
- The Squire Index of professionalism of state courts of last resort (Squire and Butcher 2021). We posit that court strength increases in professionalism.
- The standard deviation of the justices' ideal points on the state's court of last resort (Windett et al. 2015). We expect that court strength decreases in the court's ideological heterogeneity. That is, homogeneous courts can act more decisively with a unified voice whereas heterogeneous courts are more likely to generate dissent.
- The ideological position of the median justice on the state's court of last resort (Windett et al. 2015). We posit that implementation delay may be systematically related to court conservatism. Liberal courts might be more likely to delay voter ID laws (and extend delays) while conservative courts might avoid delaying them or shorten the delays.[16]
- The logged count of court curbing bills proposed by state legislatures (Leonard 2022). We posit that, on average, court strength decreases in court curbing activity.

[16] An alternative perspective would suggest that this variable measures the court system's *sympathy* toward the law, rather than its strength. We also considered a two-factor model with latent measures of both court sympathy *and* court strength. Our treatment effect estimates controlling for these two factors were substantively unchanged from what we report controlling for the indicator from the simpler, one-factor model.

- An indicator of voter ID law severity, which measures whether a law includes extra steps to count a ballot without ID, a photo requirement, or both conditions.

We employed confirmatory factor analysis (CFA) to combine these indicators into a latent measure of court strength using our full data (N = 784). Some of the variables above do not cover all years in our data; we estimate the model using full information maximum likelihood (FIML), which generates factor scores for all cases, even those with missingness on some predictors.[17] Table A9 summarizes the CFA model, with the loadings in the top panel and variances in the bottom. Note that the latent variable is scaled with the divided government variable, which fixes its loading at 1. Larger (smaller) values of the latent factor indicate more (less) court strength.

Table A9: Confirmatory Factor Analysis of the State Court Strength Indicators

	Loadings				
Variable	Estimate	SE	Z	p	Std. estimate
Divided government	1.000	–	–	–	0.220
Appointment state	1.688	0.356	4.744	0.000	0.427
Court professionalism	1.476	0.523	2.820	0.005	0.159
Ideal point heterogeneity	−0.145	0.105	−1.380	0.168	−0.064
Court median	−3.444	1.338	−2.574	0.010	−0.826
ln(Court curbing bills)	−0.130	0.340	−0.381	0.703	−0.018
Strict and photo law	−0.220	0.126	−1.751	0.080	−0.089
	Variances				
Variable	Estimate	SE	Z	p	Std. estimate
Divided government	0.234	0.013	18.431	0.000	0.952
Appointment state	0.151	0.013	11.199	0.000	0.817
Court professionalism	0.993	0.051	19.561	0.000	0.975
Ideal point heterogeneity	0.061	0.003	19.733	0.000	0.996
Court median	0.066	0.049	1.351	0.177	0.318
ln(Court curbing bills)	0.632	0.032	19.795	0.000	1.000
Strict and photo law	0.072	0.004	19.550	0.000	0.992

Note: Cell entries report factor loadings and variances from confirmatory factor analysis (CFA) of the state court strength indicators. N = 784; $\chi^2(14)$ = 141.154 ($p < 0.05$); SRMR = 0.071.

[17]Specifically, the FIML approach computes the likelihood values of each observation using all available data in each case (Rosseel 2012).

The results indicate that the court median, appointment state indicator, and divided government indicator load the strongest, with standardized estimates of -0.826, 0.427, and and 0.220, respectively. All of those estimates reach statistical significance at the 0.05 level. Court curbing is the weakest variable, with the smallest loading (top panel) and largest unexplained variance (bottom panel). The overall test of the model (χ^2) is statistically significant ($p < 0.05$). The Standardized Root Mean Square Residual (SRMR) is 0.071, which corresponds to a "good" fit in the literature (MacCallum et al. 1996; Hu and Bentler 1999).

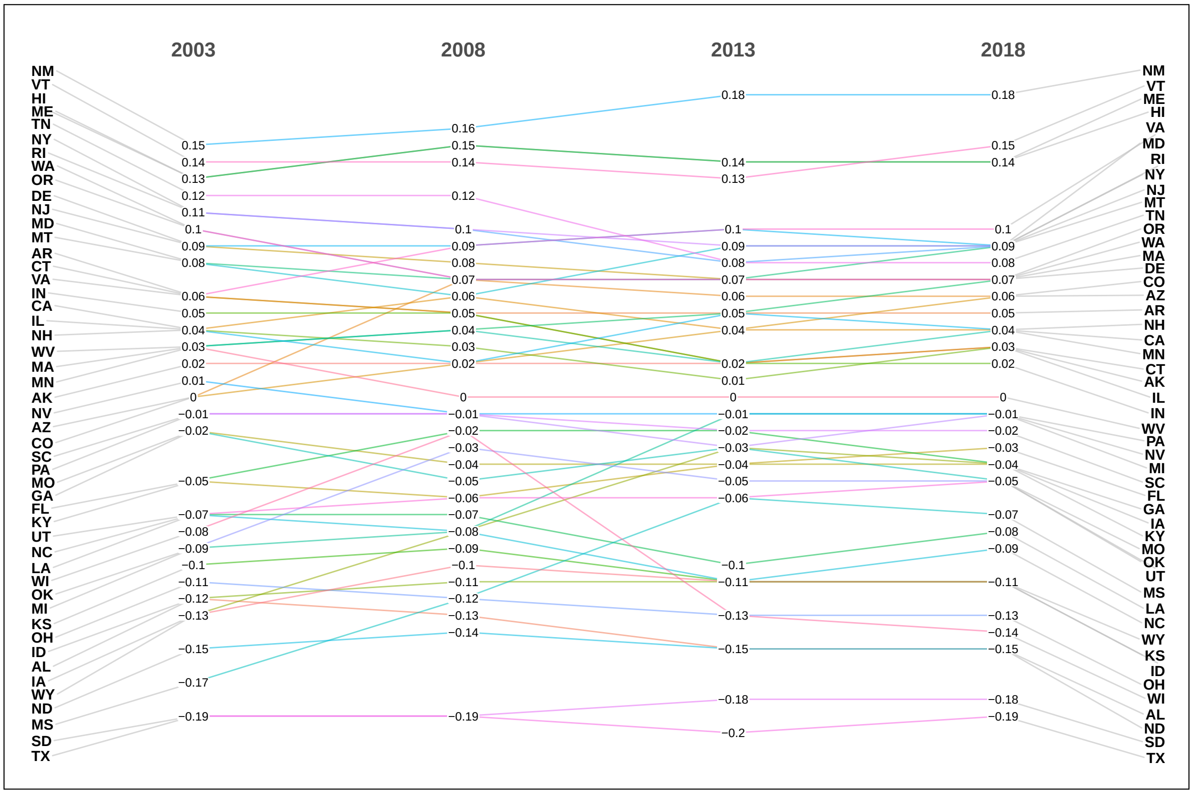
Figure A4 graphs temporal change in states' rankings on the latent court strength measure. The results show a meaningful amount of variation across states and over time, with much of the changes in ranking occurring in the first 10 years of the time period. They also suggest good face validity. The strongest courts appear mostly in traditionally liberal states while conservative states tend to have weaker courts, which fits expectations from judicial politics scholarship (Hall and Windett 2015). States in New England are particularly known for strong courts and many of those states appear near the high end of our measure across time.

A4.3 Adoption as a Placebo Test

Our theoretical framework emphasizes the *implementation* of a voter ID law as the key point at which legislative candidates are incentivized to move away from center. We expect that candidates' uncertainty about the district median does not change until the law actually affects who can vote. As our data indicate, this step typically occurs years after its passage. Our previous analysis of the effects of law implementation provides supporting evidence for our theory. However, we can further evaluate the theory as well as our proposed mechanism through a placebo test: an analysis in which we look for a polarizing effect where we would *not* expect to find one.

Specifically, we next estimate the effects of voter ID law *adoption* on party polarization. That is, we employ the same coding of treatment as in our last analysis, but move back the indicator for treated status to the years in which state legislatures passed the laws—ignoring implementation dates. This approach defines treatment as the point at which states decided to require ID, but (in

Figure A4: State Court Strength, 2003–2018



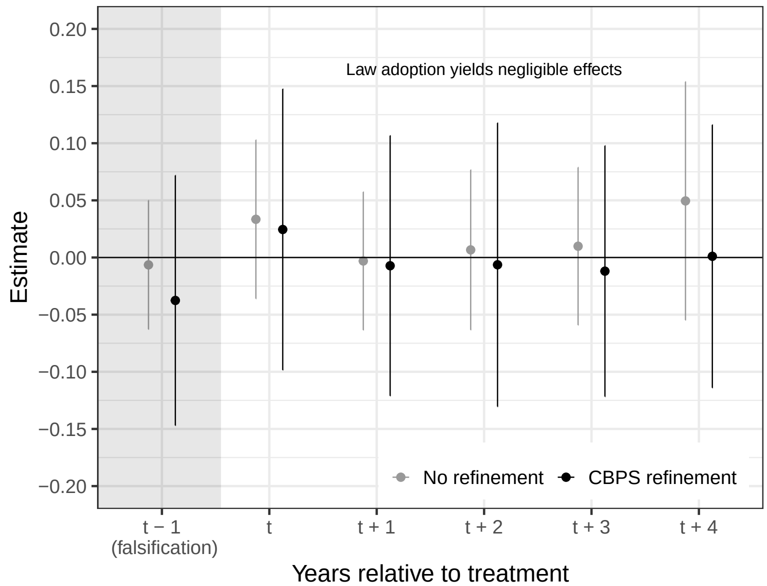
Note: The graph displays temporal change in states' rankings on the latent court strength measure. States are sorted in descending order. The mean of the latent measure in the full data is 0 and the standard deviation is 0.09.

most cases) not the point at which they began to enforce it.[18] We lose our data subset identification strategy here, which warrants appropriate caution. But our covariates and PanelMatch specification from main text Figure 2 remain. We include all states during the period 2003–2018 in the analysis and achieve good balance on the covariates after CBPS refinement (see Section A5.4 below for details). The estimated treatment effects appear in Figure A5.

Although we are unable to leverage as-if random legal delays here, the falsification test nonetheless shows support for the matching component of our research design. Moving to the treatment

[18]Adoption and implementation dates are equivalent in the few states with no legal delays (see Figure 1 in the main text). But the delay period in the other states suggests that the average effect of adoption should be noticeably weaker than the implementation effect. See Section A8.3 of this document for an alternate placebo test using pre-HAVA voter ID laws.

Figure A5: Estimated Effects of Voter ID Laws' Adoption on Party Polarization in State Legislatures, 2003–2018



Note: The graph displays years relative to treatment (x-axis) against the estimated effects of voter ID laws' adoption on the difference in party median ideal points in state legislatures (y-axis). Line segments indicate 95% confidence intervals. N = 784.

effects, the graph shows a small uptick in polarization in year *t* that is slightly smaller than the initial effects of implementation (see Figure 2). Moreover, the estimated effects quickly move to near zero and stay there for several successive time periods. In contrast to our main results, all of the confidence intervals in Figure A5 include zero. Some of them are quite wide, so large effects are at least *plausible* given the data. But overall the adoption of a voter ID law does not appear to polarize legislative parties like implementation does. This finding lends additional support to our theory and uncertainty mechanism. The immediate *passage* of a law produces no divergence in the parties' ideological positions because candidates' perceptions of the electorate do not change in the manner we hypothesize at that time. It is only after the laws can actually affect the composition of the electorate and increase candidates' uncertainty about the median voter that we observe their polarizing influence.

A5 Covariate Balance

We next assess the extent to which our identification strategy improves covariate balance. In the main text we discuss multiple facets of this strategy: (1) subsetting the data to isolate treatment variation stemming from delays in implementing voter ID laws and (2) our use of PanelMatch. Here we examine balance improvement from both of these aspects of our analysis.

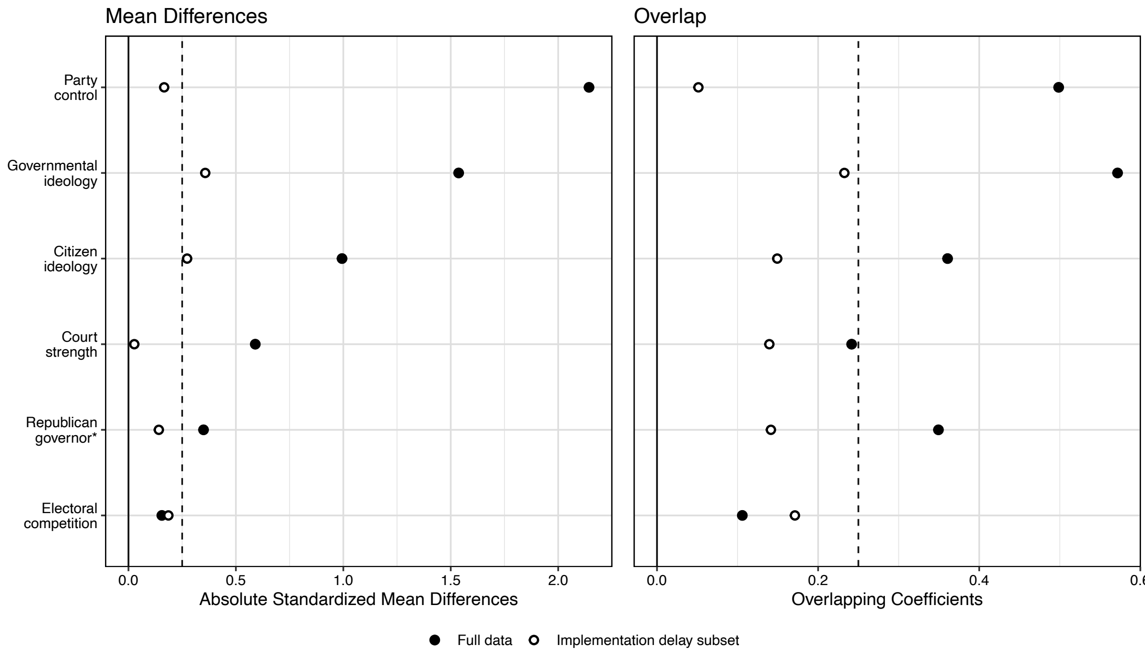
A5.1 Data Subsetting for Legal Delays

In the main text we argue that the timing of voter ID law implementation in states that choose to enact them is as-if random. To evaluate this claim, we first compare covariate balance in the full data—which include states that never adopted a law and state-years in adopting states prior to adoption—to our subset. Figure A6 graphs two quantities: absolute standardized mean differences (left panel) and overlapping coefficients (right panel) in both samples.[19] The former measures the average similarity between the treated and control observations. The latter measures the overlap in the densities of the covariate for each group, scaled such that lower values indicate more overlap (Franklin et al. 2014). We set a standard target threshold of 0.25 to denote good balance in a covariate (Austin 2009; Stuart et al. 2013).

The graphs show substantial balance improvement for several key variables. Party control of the legislature moves from a mean difference of about two standard deviations to about 17% of a standard deviation. Court strength moves from just over half of a standard deviation in difference to nearly no difference. Governmental ideology and citizen ideology fall just outside the threshold, but both of those variables demonstrate major imbalance reductions as well. The indicator for Republican governors and the measure of electoral competition between the parties also improve, although those variables are reasonably well-balanced in the full data. Overlap generally improves as well. All of the overlapping coefficients fall below our threshold. And in all but one case the quantity decreases (indicating more overlap between groups) from the full sample to the subset.

[19]We average over the imputed datasets in the calculation of these measures as recommended by Greifer (2021*a*).

Figure A6: Covariate Balance Before and After Subsetting the Data



Note: The graphs present absolute standardized mean differences (left panel) and overlapping coefficients (right panel) in the full and subset samples. All statistics are computed averaging over the imputed datasets. The mean difference for Republican governor is not standardized.

The only exception is electoral competition, which is similar and below the threshold in both samples.

Overall, in the full sample there are large differences between treated and control observations on key covariates, but our subsetting strategy reduces these differences and improves similarity between the two groups. These results provide evidence in favor of our identification strategy. Nonetheless, to further bolster the robustness of our design it is important that we perform additional diagnostics on our claim of a plausibly exogenous treatment and take additional steps to balance covariates within the subset data that we use for estimation.

A5.2 Balance with PanelMatch

We next move to balance assessment after matching with the PanelMatch estimator. In our analysis, PanelMatch creates matched sets for treated cases based on shared treatment history (Imai et al. 2022). Here, we set the method to look two years pretreatment to create matched sets.

As Table A10 shows, it is able to do so for five treated states: Alabama (2014), Mississippi (2014), Rhode Island (2014), South Carolina (2013), and Texas (2013). That is, PanelMatch uses only the data from these five states and the states that it selects into their matched sets in estimation of the treatment effects. We acknowledge that this process further reduces a sample that is already small, including dropping some treated states. Such is the tradeoff of matching; we are pruning the data down to only those cases that credibly identify the treatment effect. See Table 1 in the main text and Section A7.5 below for analyses with alternative estimation strategies that use the entire data. Importantly, those results align with the PanelMatch results we report in the main text.

After creating the matched sets, the estimator then further refines the set of control states based on covariates and Imai and Ratkovic's (2014) covariate balancing propensity score (CBPS).[20] Table A10 also summarizes this process. It reports the matched sets constructed by PanelMatch for each of the treated states in the estimation sample. The last column indicates whether a given control state is included in the refinement set.

Next we consider balance improvement in the matched sets. Does matching further improve the similarity between treated and control states, even after our subsetting step described previously? Figure A7 graphs years relative to treatment on the x-axes against standardized mean differences in our covariates between treated and control states. Additionally, we include a lag of the outcome variable in this assessment, as recommended by Imai et al. (2022). Recall the threshold of 0.25 that we set previously, which is commonly used (Austin 2009; Stuart et al. 2013). The graph on the left displays balance using all control states in a matched set and the graph on the right shows balance after refining to only the two most similar control states based on the covariates for a given treated state.

Figure A7 shows good balance in the unrefined matched sets. Most of the covariates' mean differences are within or near the $\pm$ 0.25 threshold, with electoral competition falling the furthest

[20]PanelMatch can also apply weights to the entire matched set using CBPS. This method produces substantively identical results, but does not improve balance quite as much as matching with CBPS.

Table A10: Matched Sets and Refinement from PanelMatch

Treated State	Treatment Year	Control States	Refinement Set
Alabama	2014	Pennsylvania	✓
		Wisconsin	✓
Mississippi	2014	Pennsylvania	✓
		Wisconsin	✓
Rhode Island	2014	Pennsylvania	✓
		Wisconsin	✓
South Carolina	2013	Alabama	✓
		Mississippi	✓
		Rhode Island	
		Wisconsin	
Texas	2013	Alabama	✓
		Mississippi	✓
		Rhode Island	
		Wisconsin	

Note: Cell entries report the matched sets constructed by PanelMatch using a treatment history of two years and the refinement set within each matched set.

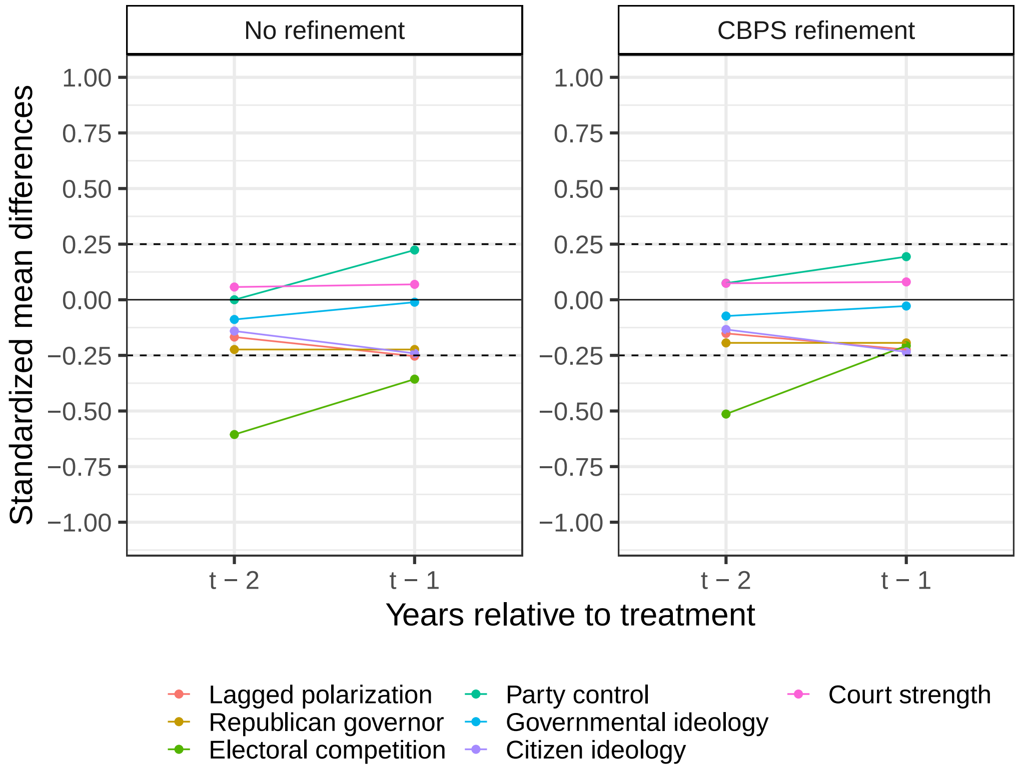
away. However, when we move to the refined matched sets, balance improves even more. All but one of the mean differences are less than 0.25 in absolute value, with electoral competition at time $t-2$ the lone exception. We interpret this result as favorable evidence for our identification strategy. After subsetting and one or two rounds of matching, our estimation can proceed as if implementation of a voter ID law was more or less decided by a fair coin flip.

A5.3 Determinants of Delay Time

We argue that the time between passage of a voter ID law and its implementation is unpredictable. We support that claim directly by regressing delay time (measured in years) on the covariates described above in our subset data.[21] Given this small sample, we estimate bivariate linear regressions with each predictor and compute 95% confidence intervals with a nonparametric bootstrap rather than rely on the model-based estimates of uncertainty. The results appear as

[21]Delay time ranges from zero to seven with a mean of 2.238, a median of 2, and a standard deviation of 2.071.

Figure A7: Covariate Balance in the Matched Sets



Note: The graphs present standardized mean differences in the covariates over time after generating matched sets with PanelMatch. The left panel gives results using all control states in a matched set and the right panel shows results after refining the matched sets with CBPS.

standardized coefficient estimates in Table A11.[22]

The results indicate that none of the covariates are strongly associated with the delay time between voter ID law enactment and implementation. The largest estimate is just 0.161 in absolute value (electoral competition), indicating that a standard deviation increase in a predictor (or moving from a Democratic to Republican governor) corresponds with at most 16% of a standard deviation shift in delay time. None of the confidence intervals are bounded away from zero, although that finding is partially due to our small sample size.[23] We interpret these results as another piece of evidence in favor of our identification strategy.

[22]The results are substantively unchanged with a Cox proportional hazards model.

[23]These data (N = 21) do not provide sufficient power for equivalence tests (Hartman and Hidalgo 2018), so we simply focus on the coefficient estimate magnitudes. We contend that doing so

Table A11: Bivariate Associations Between Covariates and Implementation Delay Length

Predictor	Estimate	95% Lower	95% Upper
Republican governor	0.151	−0.522	0.823
Electoral competition	−0.161	−0.835	0.513
Party control	0.077	−0.725	0.879
Governmental ideology	−0.064	−0.426	0.298
Citizen ideology	0.130	−0.262	0.522
Court strength	−0.104	−0.542	0.333

Note: Cell entries report standardized coefficient estimates and 95% confidence intervals from bivariate linear regressions of delay on the covariates. N = 21.

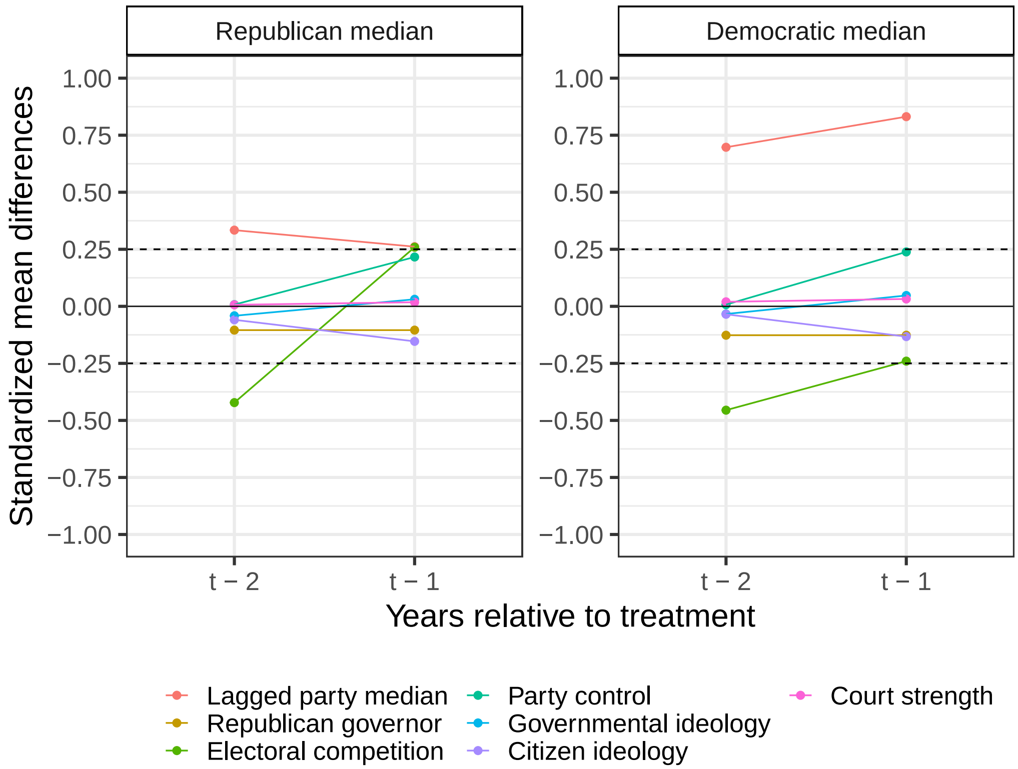
A5.4 Alternative Outcome Balance Results

Figure A8 displays covariate balance with two other outcome variables: absolute value of the party medians (H2) and polarization by legislative chamber (Section A6.3 below). These results indicate that balance improvement is not quite as good with these outcomes. Several of the mean differences fall within the dashed lines, but others do not. The lagged outcome for the Democratic party median is a particularly large outlier, though it still remains below a standard deviation in mean difference. Thus, we must interpret the results from those outcomes with more caution relative to our main outcome. However, it is important to remember that the data pass the falsification test with both of these outcomes (see the main text and Figure A12 below).

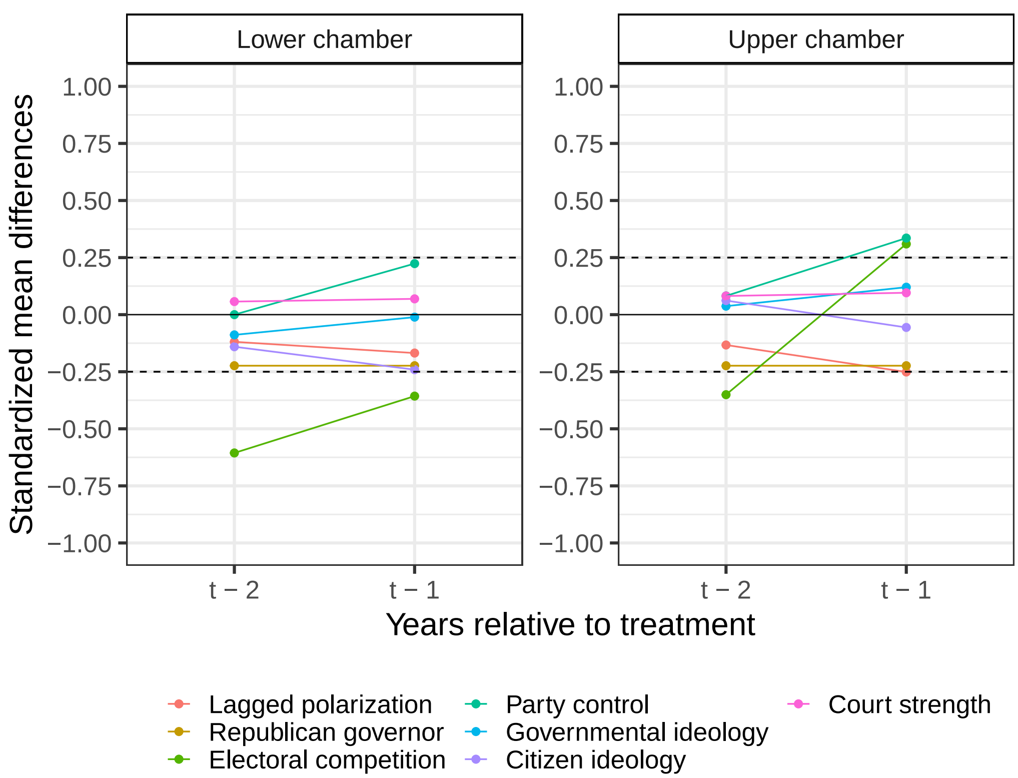
is still informative. See Section A7.4.1 for an equivalence test assessment of pretreatment trends.

Figure A8: Covariate Balance in the Matched Sets with Additional Outcome Variables

(a) Party median ideal points



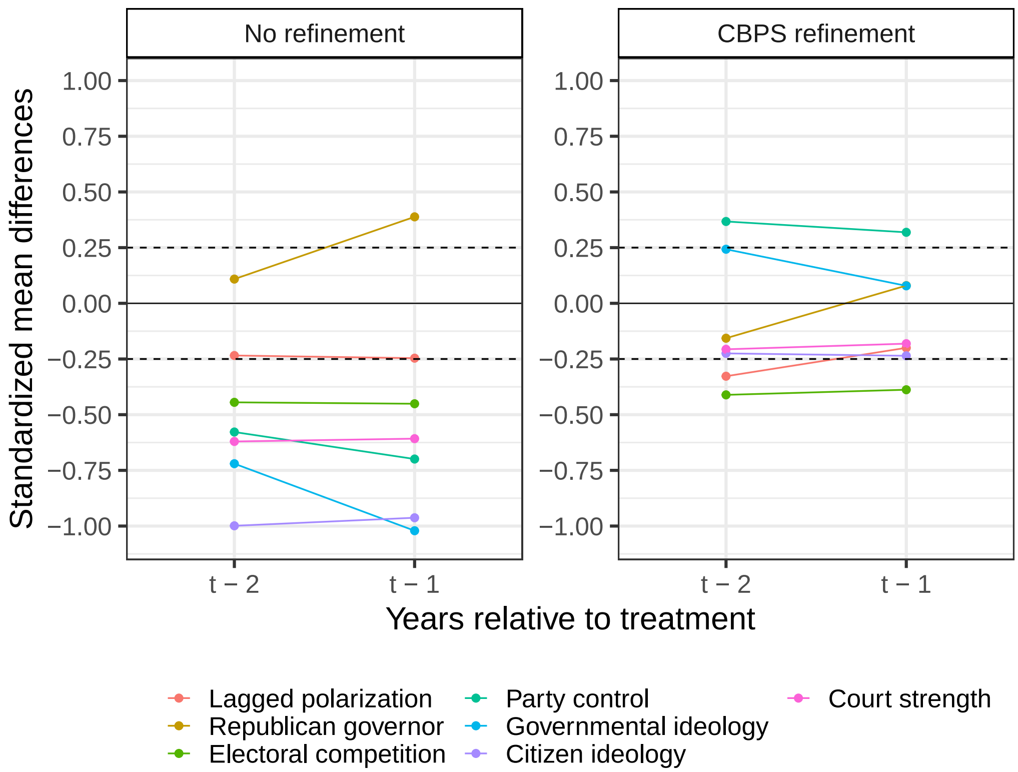
(b) Polarization by chamber



Note: The graphs present standardized mean differences in the covariates over time after generating matched sets with PanelMatch. The top panels give results for the party median ideal points and the bottom panels show results for polarization by chamber.

Figure A9 presents covariate balance for our analysis of the effects of voter ID law *adoption* (the placebo test above). The graphs show that balance improvement is quite poor without covariate refinement. However, it improves after matching refinement with CBPS. Thus, we place relatively more trust in the estimates generated from the refined matched sets, although they are substantively similar to the unrefined estimates (see the main text).

Figure A9: Covariate Balance in the Matched Sets with Adoption as Treatment



Note: The graphs present standardized mean differences in the covariates over time after generating matched sets with PanelMatch, using voter ID law adoption as treatment.

A6 Controlling for Other Confounders

Here we present estimation results after accounting for several other factors that may confound the effects of voter ID laws on polarization.

A6.1 Additional Covariates

Here we present results with several other covariates that may plausibly correlate with both treatment status and the outcome, biasing our estimated treatment effects. Adding covariates is challenging in a small sample because the matching procedure demands more of a fixed amount of data. Thus, balance improvement is not as strong in these estimations as with our main results (see the replication materials). Accordingly, we must interpret the results with appropriate caution. Nonetheless, it is still useful to consider the estimated effects after accounting for these additional variables.

To partially address the problem of expanding the specification with a small sample, we group the additional covariates and add them to the original specification separately. Due to statistical identification issues, we remove the electoral competition covariate from these analyses only. The variables we add and their groups are as follows:

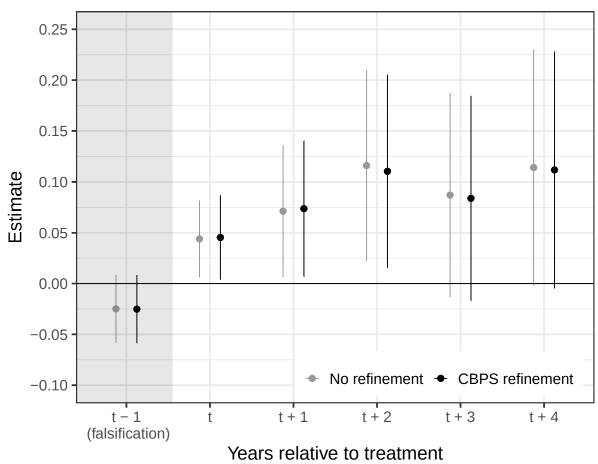
- **Racial**: (1) Nonwhite proportion of the population, (2) State racial resentment;
- **Institutional**: (1) Term limits in effect, (2) Legislative professionalism, (3) Agenda size;
- **Diffusion**: (1) Percentage of neighboring states adopting a voter ID law, (2) Percentage of source states adopting a voter ID law.

The racial variables capture the possibility that Republican lawmakers' motivation to enact voter ID legislation may increase as the share of the state's population that is not white increases and/or as the state's average racial resentment increases. The former variable comes from Census data and the latter comes from Smith et al. (2020). Next, we consider some plausible institutional features that might correlate with voter ID adoption and also impact polarization: term limits in effect (Olson and Rogowski 2020), legislative professionalism (Bowen and Greene 2014), and agenda size, which we measure as the number of bills introduced in a state-year (logged). Finally, we consider the possibility of diffusion effects—the potential influence of *other states* adopting voter ID laws on polarization in a state. Two common means of controlling for diffusion are (1) the proportion of a state's geographic neighbors and (2) the proportion of a state's policy "sources" that have adopted in a given year (Desmarais et al. 2015).

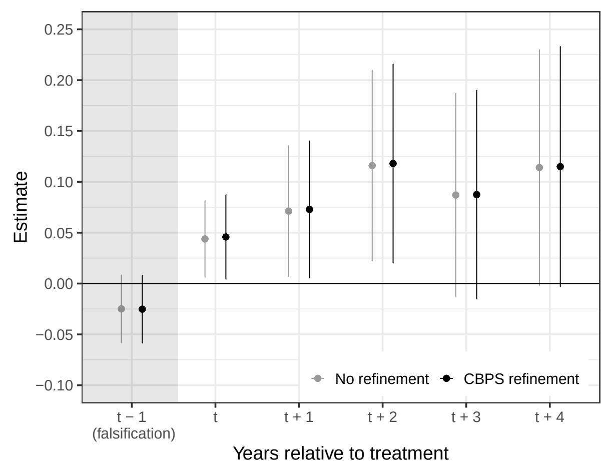
Figure A10 presents the estimates with these covariates added. Panel (a) presents the original results from the main text. Panels (b)–(d) present results after adding each group of additional covariates to the original specification. Across the four panels, the estimates show a great deal of stability. The gray estimates remain the same throughout because they do not reflect refinement from matching on covariates. The black estimates differ slightly, but not enough to change substantive conclusions. Thus, we find that our results are robust to these additional covariates.

Figure A10: Estimated Effects with Additional Covariates

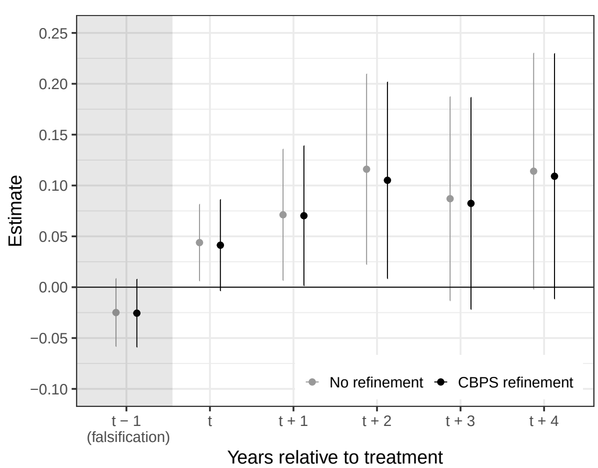
(a) Original



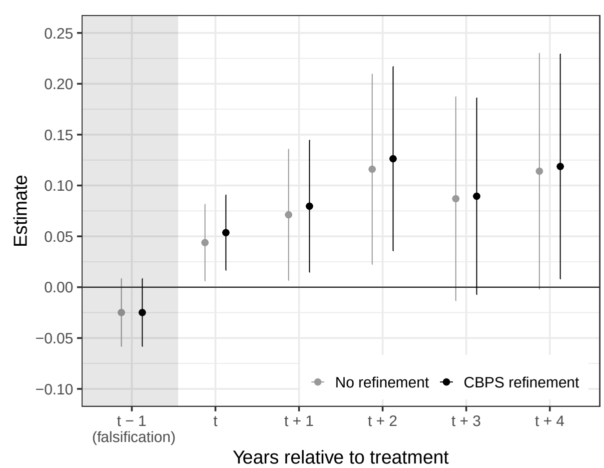
(b) Racial covariates



(c) Institutional covariates



(d) Diffusion covariates



Note: The graphs present treatment effect estimates on the difference in party median ideal points with additional covariates. Panel (a) presents the original results from the main text. Panels (b)–(d) present results after adding each group of additional covariates to the original specification. N = 194.

A6.2 Separate Estimates for Strict and Photo Laws

In the main text we define voter ID laws as those that fulfill at least one of the following conditions:

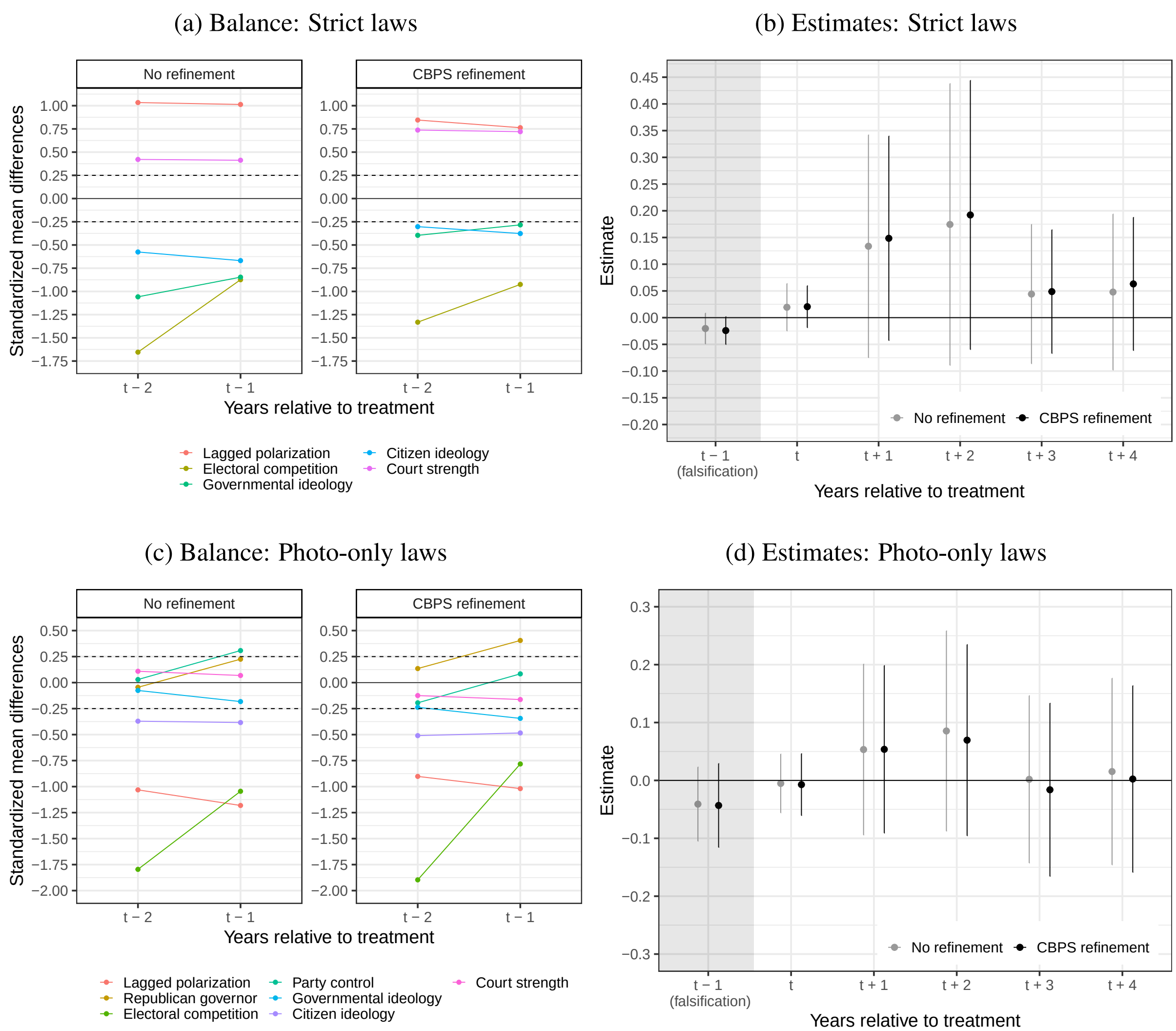
- Voters without acceptable identification must vote on a provisional ballot and take on a substantial burden to officially count their votes (strict);
- The primary acceptable identification documents are those with a photo of the voter (photo).

This definition best connects to our theoretical framework because either of these conditions could deter voters, creating uncertainty about the electorate in candidates' minds. However, this approach does bundle two dimensions, leading to some ambiguity with respect to which condition matters most. In Figure A11 we consider the effects of these two conditions separately. The graphs present covariate balance and treatment effect estimates for strict laws with or without a photo component (i.e., only the first condition) and non-strict, photo-only laws (only the second condition).

The first point to note is that separating these two conditions holds empirical consequences. We are demanding more from the same data and have less treatment variation in each analysis. The result is a weakening of covariate balance and statistical power. In the top panels, balance is suboptimal and does not improve much with CBPS refinement. In fact, in the case of strict laws some covariates cannot be included in the matching procedure due to lack of variation. All of the covariates can be included in the photo only analysis, but still balance is weak. As such, we must be quite cautious in our interpretation of the results. Moving to the right panels, the estimates show mostly positive effects, as we find in our main results. Thus, there is a baseline level of robustness from these analyses. Nonetheless, the confidence intervals are quite wide and do not permit us to statistically distinguish the estimated effects from zero. Furthermore, the estimates themselves are stronger for strict laws (condition 1 only). Panel (b) shows treatment effects that are roughly similar to our main set of results in Figure 2; the estimates at time $t+2$ are even somewhat larger. In contrast, the effects shown in panel (d) are relatively smaller in magnitude.

These analyses highlight an empirical case for bundling the strict and photo conditions into one treatment. Doing so offers substantial covariate balance and statistical power improvements. However, they also show that the general pattern we report here still emerges—at least to some degree—from separate analyses of the two conditions. Both types of law appear to be associated with increased legislative polarization, with the strict definition likely exerting the strongest effect.

Figure A11: Estimated Effects with Strict Laws and Photo Laws



Note: The graphs present treatment effect estimates on the difference in party median ideal points with treatment defined as strict laws (top panels) and photo-only laws (bottom panels). N = 194.

A6.3 Results by Chamber

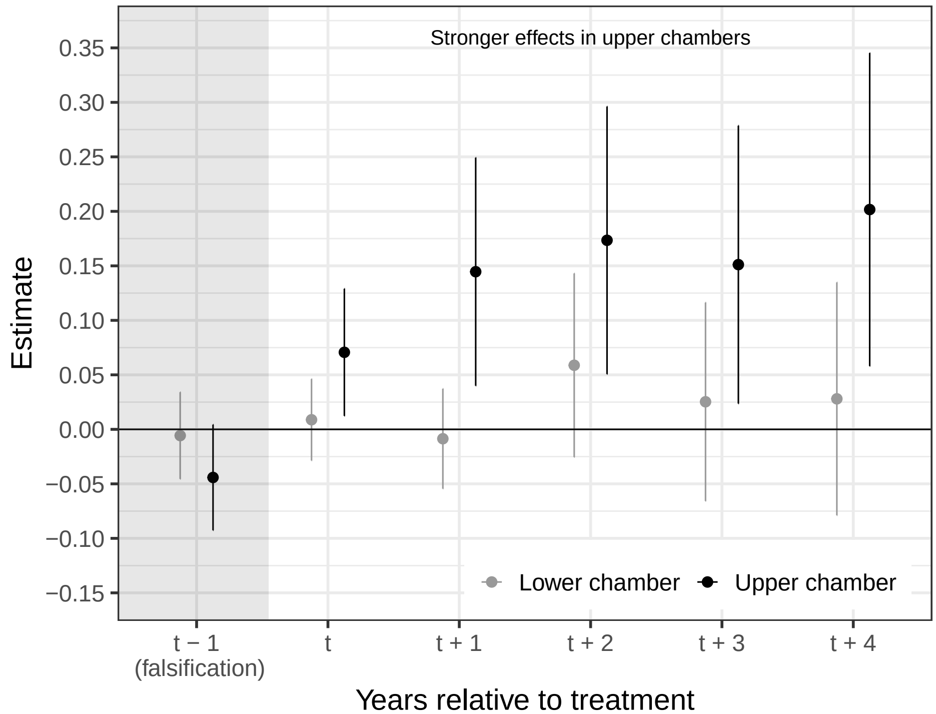
In Figure A12 we repeat the main analysis with a new outcome variable to assess whether the effects differ by legislative chamber. We do not hold strong expectations, but suspect that the polarizing effect of voter ID laws may be stronger in the lower chambers of state legislatures than it is in the upper chambers. The United States Senate is historically known for its unwritten norms and "folkways" (Matthews 1959) that, at least until the end of the twentieth century, kept it relatively bipartisan compared to the House (Theriault 2006). Indeed, scholars show that sharp ideological divides between the parties in the Senate are, in part, a product of polarizing House members eventually winning seats in the Senate (Theriault and Rohde 2011). The genesis of polarization in the House stems from a myriad of factors, such as geographic sorting of constituents, election timing, and procedural rules (Theriault and Rohde 2011; Ragusa 2016).

While this evidence comes from Congress, much of the logic could potentially apply to state legislatures as well. The institutional variation that is a hallmark of state governments certainly characterizes state houses and senates, but in a broad sense legislators at the state level face many of the same constraints and incentives as their counterparts in Congress and structure their institutions similarly (Squire and Hamm 2005). The available empirical evidence suggests that, as in Congress, lower chambers are the central source of polarization in state legislatures. Olson and Rogowski (2020), for instance, demonstrate that the polarizing effect of term limits is almost entirely concentrated in lower chambers, finding negligible effects in the upper chambers.

The falsification estimates are again small in magnitude and not statistically distinguishable from zero.[24] The graph also shows heterogeneity in our estimated treatment effects by chamber, but not in the way we posited. The polarizing effect of the treatment is concentrated almost entirely in state *upper* chambers. While the estimates are positive at all time points for both chambers, they are notably larger in state senates compared to houses. The confidence intervals mostly overlap, although the effects at $t+1$ are statistically distinguishable from one another ($p < 0.05$). One potential explanation could stem from our reliance on replacement of legislators for temporal

[24] See Figure A8 for an assessment of covariate balance for this analysis.

Figure A12: Estimated Effects of Voter ID Laws on Party Polarization in State Upper and Lower Chambers, 2003–2018



Note: The graph displays years relative to treatment (x-axis) against the estimated effects of voter ID laws going in force on the difference in party median ideal points by state legislative chambers (y-axis). All estimates come from matched sets with CBPS refinement. Line segments indicate 95% confidence intervals. N = 194.

variation in polarization with these data (Shor and McCarty 2011). Upper chambers are smaller than lower chambers, and thus the replacement of one member constitutes a larger change to the ideological distribution of the chamber.

A7 Parallel Trends and Alternative Estimators

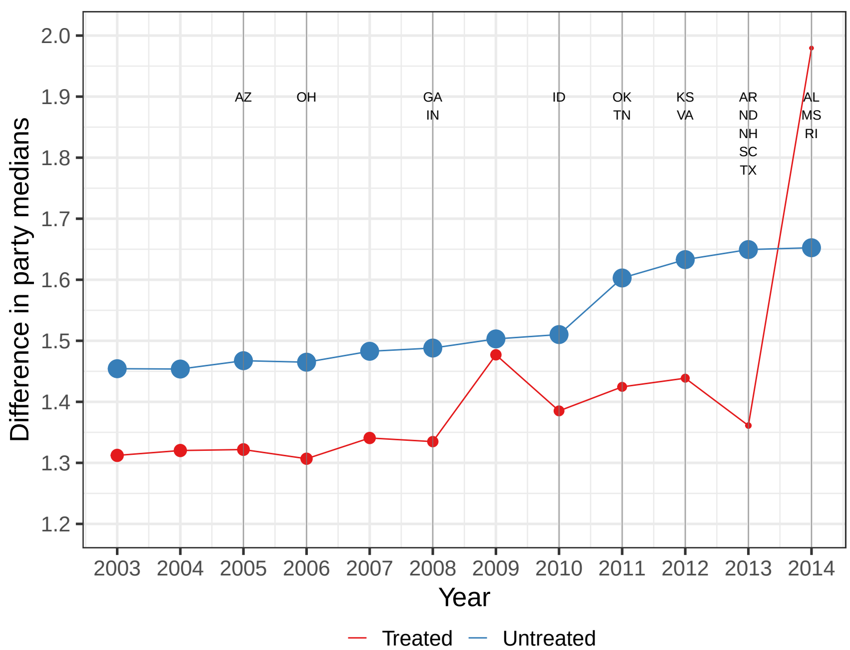
Here we consider the validity of the parallel trends assumption, which is commonly invoked for estimating treatment effects in the difference-in-differences (DID) framework with panel data. We also report results with alternative estimation strategies, including panel data estimators and weighting methods.[25] We also discuss why we ultimately select PanelMatch as our main estimation method among the many options available for panel data.

[25] In the interest of brevity, we focus only on tests of H1 with these alternative methods.

A7.1 Parallel Trends

The parallel trends assumption states that the difference between treated and control units is constant over time in the absence of treatment (Angrist and Pischke 2008). As a check on this assumption, we compare the pretreatment trends in our main outcome variable for every state that became treated (voter ID law implemented) during the time period we study. Figure A13 graphs the average outcome for treated (red) and untreated (blue) states up to 2014, the year before the last treatment period (Wisconsin). The vertical lines reflect treatment years for the treated states listed on the graphs. Dot sizes are proportional to the number of states in a group.

Figure A13: Pretreatment Means in the Main Outcome for Treated and Untreated States



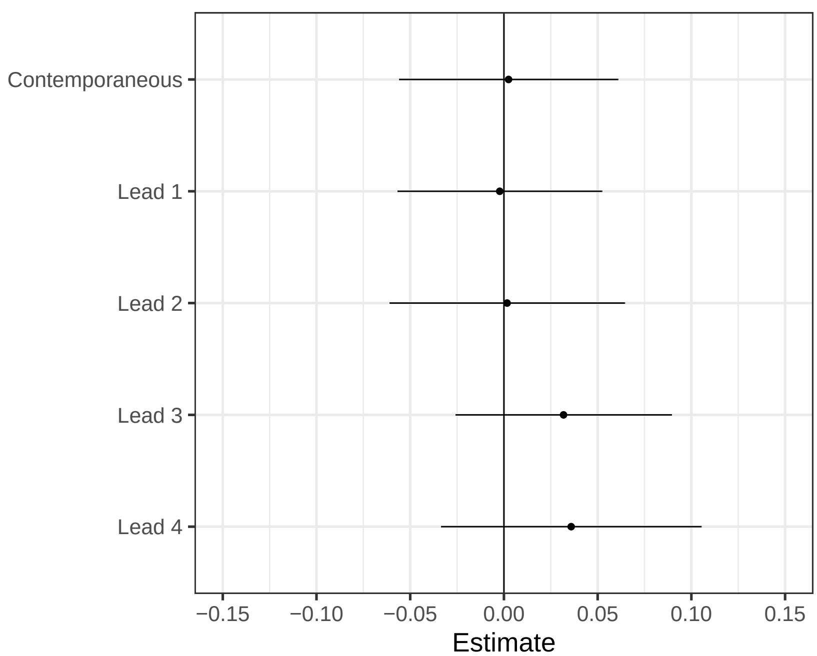
Note: The graph presents the average outcome for treated (red) and untreated (blue) states up to 2014, the year before the last voter ID law implementation in the data (Wisconsin). The vertical lines reflect implementation dates for the treated states listed on the graphs. Dot sizes are proportional to the sample sizes of states.

The graphs suggest that the parallel trends assumption is generally reasonable, although there are two points where the two lines appear to diverge: 2009 and 2014. These deviations suggest violations of parallel trends. Accordingly, it is important to further investigate the assumption. We do so next with several alternative estimators.

A7.2 Two-way Fixed Effects

The two-way fixed effects estimator—a regression model with unit and time fixed effects—is a popular tool for estimation with panel data. It is simple to estimate and interpret and allows for the estimation of treatment leads, which can further assess the parallel trends assumption. Specifically, treatment leads give the effect of treatment in a given year on the outcome in prior years. Strong validation of the design appears if the lead effects are near zero, indicating pretreatment similarity between treated and untreated states. Figure A14 reports estimates with the contemporaneous effect of treatment as well as leads of 1–4 years using the full sample of data.[26] All estimates come from separate two-way fixed effects models that include all covariates mentioned in the main text.

Figure A14: Two-way Fixed Effects Estimates



Note: The graph presents the estimated treatment effects and 95% confidence intervals from two-way fixed effects models estimated on the full sample of data. The contemporaneous estimate is the effect of the actual treatment and the treatment leads from 1 to 4 years represent tests of the parallel trends assumptions.

The first point to acknowledge from Figure A14 is that the actual treatment effect (contemporaneous estimate) from this method is nearly zero and not statistically significant. Thus, these

[26]Results are similar with the legal delay subset data.

results diverge from the main results we present. However, this estimator is not well-suited to our data for several reasons. It cannot accommodate heterogeneous treatment effects, such as varying effects over time, which we argue are relevant for understanding the impact of voter ID laws (see the main text). It is also less robust to model misspecification bias compared to PanelMatch (Imai et al. 2022). And it is subject to bias due to the fact that treatment occurs at different time periods for different states (de Chaisemartin and D'Haultfœuille 2020; Sun and Abraham 2021; Callaway and Sant'Anna 2021; Goodman-Bacon 2021). Nonetheless, for the sake of transparency we report it here to show the full range of our empirical analyses of these data.

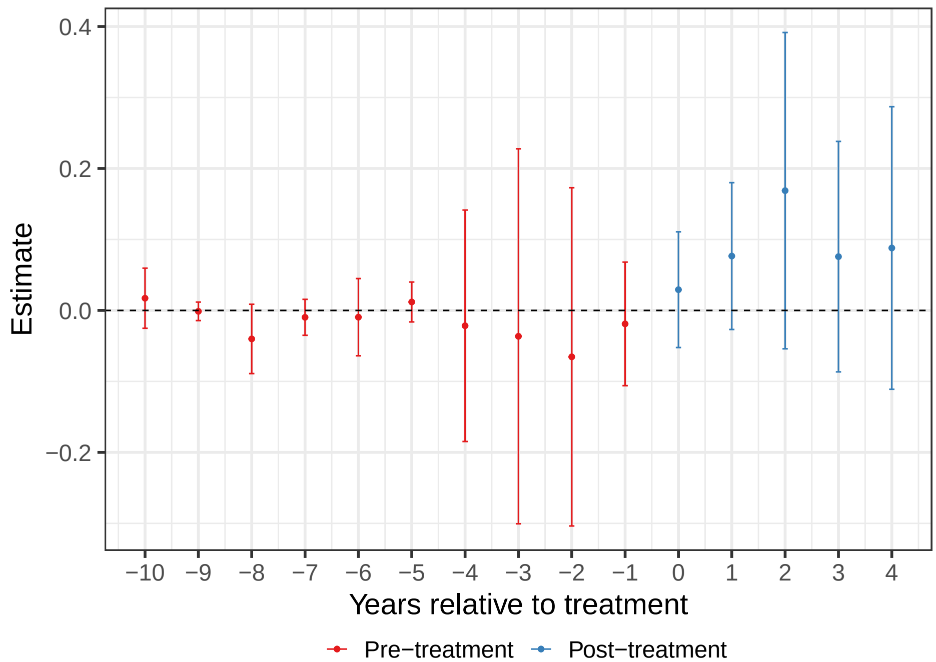
Moving back to the parallel trends assumption, the lead estimates in Figure A14 are also near zero with confidence intervals that include zero. The estimates for leads 3 and 4 show a positive trend, however, suggesting that we should continue our assessment of the assumption with additional methods. We do so via an event study as well as the use of another method for estimating treatment effects in panel data.

A7.3 Event Study

The concept of treatment leads can be expanded into an event study, or dynamic DID, which reports average differences between the treated and control units across a range of time periods relative to treatment. Recent literature emphasizes the need to conduct such studies carefully when the treatment timing varies across units, as it does in our data (e.g., Sun and Abraham 2021; Callaway and Sant'Anna 2021). Here we use the dynamic DID estimator from Callaway and Sant'Anna (2021) to conduct the event study presented in Figure A15 on our full dataset. The estimates in red (blue) are pretreatment (posttreatment) event time average treatment effects with 95% simultaneous confidence intervals.

The pretreatment estimates show evidence supporting the parallel trends assumption. The points are near zero and their confidence intervals include zero. The event time average treatment effects (blue) are positive, in line with our expectations, but their confidence intervals also include zero. However, these estimates represent effects grouped by event time. Another useful way to group effects is by calendar time to assess how the effect of the treatment may have changed

Figure A15: Event Study Estimates (Callaway and Sant'Anna 2021)



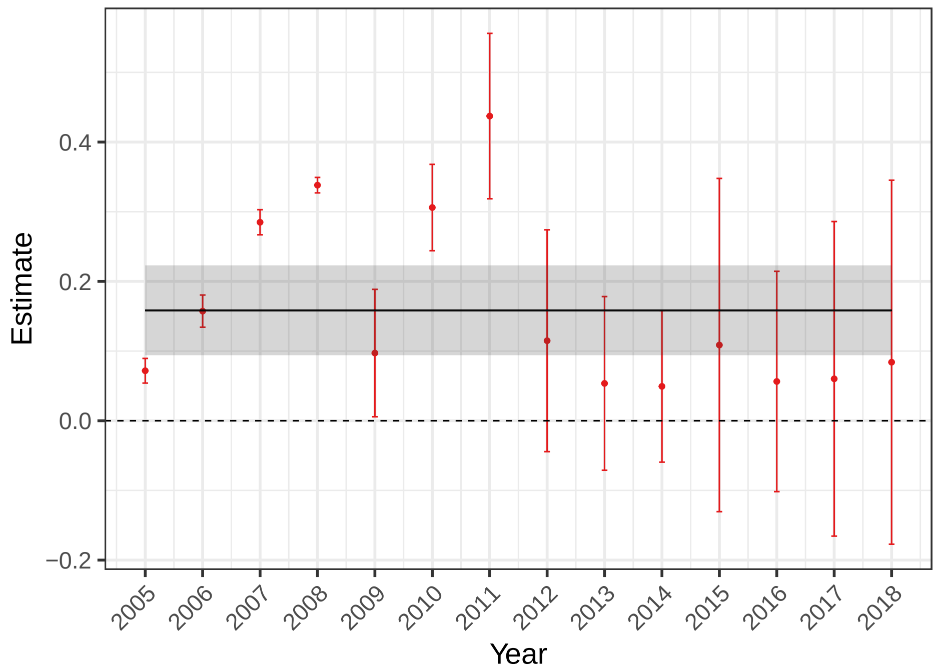
Note: The graph presents coefficient from an event study using the dynamic DID estimator developed by Callaway and Sant'Anna (2021). The estimates in red (blue) are pretreatment (posttreatment) event time average treatment effects with 95% simultaneous confidence intervals.

from earlier to later periods. Figure A16 presents these results.

The graph shows positive effects of voter ID laws in roughly the first half of the time period under study—treated years from 2005 to 2011.[27] These estimates are large in magnitude—reaching about 0.40 in 2011, or roughly one standard deviation of the outcome. And their confidence intervals exclude zero. Then beginning in 2012 the effects move toward zero and their confidence intervals include zero. The average of these effects and its confidence interval, denoted by the black line and shading, are 0.159 [0.094, 0.223]. This result is comparable to the PanelMatch estimates we report in the main text. Thus, using a different method we see evidence of a polarizing effect of voter ID laws, especially among the first several laws that were adopted.

[27] The method does not estimate effects for the first two years of the data because it uses treated units that have not yet been treated to construct counterfactuals.

Figure A16: Estimated Effects by Calendar Time (Callaway and Sant'Anna 2021)



Note: The graph presents dynamic DID estimates from Callaway and Sant'Anna (2021), grouped by calendar time. The points represent point estimates and lines represent 95% confidence intervals. The black line and shading denote the average of the effects and its 95% confidence interval.

A7.4 Fixed Effects Counterfactual Trends

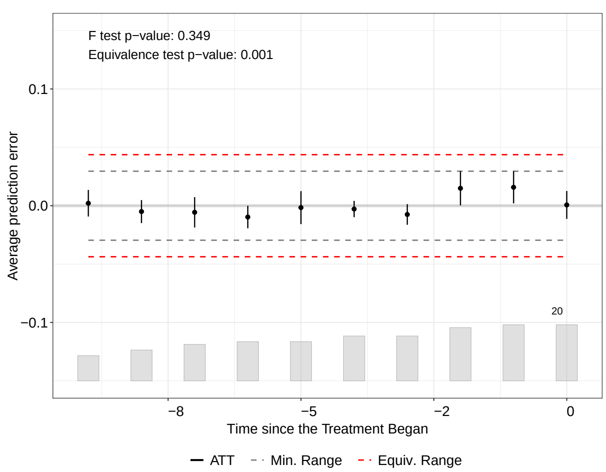
We next consider Liu et al.'s (2022) Fixed Effects Counterfactual Trends (FECT) estimator. This method comprises a family of related techniques for imputing counterfactual outcomes for treated observations in panel data and visualizing diagnostics of pretreatment trends as well as dynamic treatment effects. Among the many competing estimators available, PanelMatch and FECT are the most flexible and accommodating. They both allow for heterogeneous effects, treatment reversals, conditioning on time-varying covariates, and provide useful diagnostic tools. Other approaches, such as two-way fixed effects and weighted DID estimators do not allow for all of these features (see Liu et al. 2022, 16).

A7.4.1 Pretreatment Trend Equivalence Tests

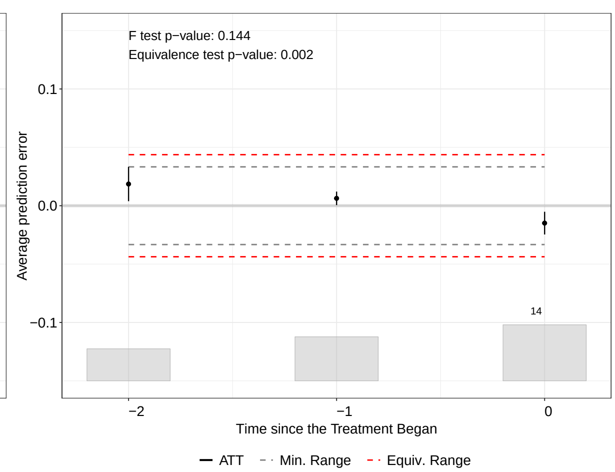
Figure A17 presents a FECT equivalence test for a trend in the pretreatment difference between treated outcomes and imputed counterfactual outcomes (y-axes) over time (x-axes). Panel (a) uses the full data and panel (b) uses the legal delay subset. Ideally, the points and 95% confidence intervals in these graphs should be near zero, which would indicate no outcome trend prior to treatment. The red lines denote the bounds of the equivalence test, or the threshold at which the null hypothesis of the existence of a pretrend would be rejected (Hartman and Hidalgo 2018). The gray lines give the maximum range of the estimated confidence intervals. Strong evidence for no pretrend would come from the gray lines falling *inside* the red lines.

Figure A17: Fixed Effects Counterfactual Trends Equivalence Test for Pretreatment Trends

(a) Full data



(b) Legal delay subset



Note: The graphs present FECT equivalence tests for a trend in the pretreatment difference between treated outcomes and counterfactual outcomes (y-axes) over time (x-axes). Panel (a) uses the full data and panel (b) uses the legal delay subset.

The results in Figure A17, panel (a) suggest the absence of a pretrend in the full data. The errors fall near zero and their confidence intervals stay within the thresholds for equivalence (red lines). A joint F-test of the null that all of the estimates are simultaneously zero is not statistically significant at the 0.05 level, indicating we fail to reject that null. However, the equivalence p-value *is* statistically significant, indicating that we *can* reject the null that a pretrend exists using the more powerful equivalence testing approach.

Panel (b) indicates a similar pattern that strongly justifies our legal delay subsetting strategy. We cannot use as many pretreatment time periods in this case, but the results indicate clear absence of a pretrend after subsetting. The errors are near zero and fall within the equivalence range—note that the gray lines are again within the red equivalence lines here. The F-statistic is nonsignificant at conventional levels, and importantly, the equivalance test p-value is statistically significant at the 0.05 level. Thus, we can reject the null of a pretrend using this subset.

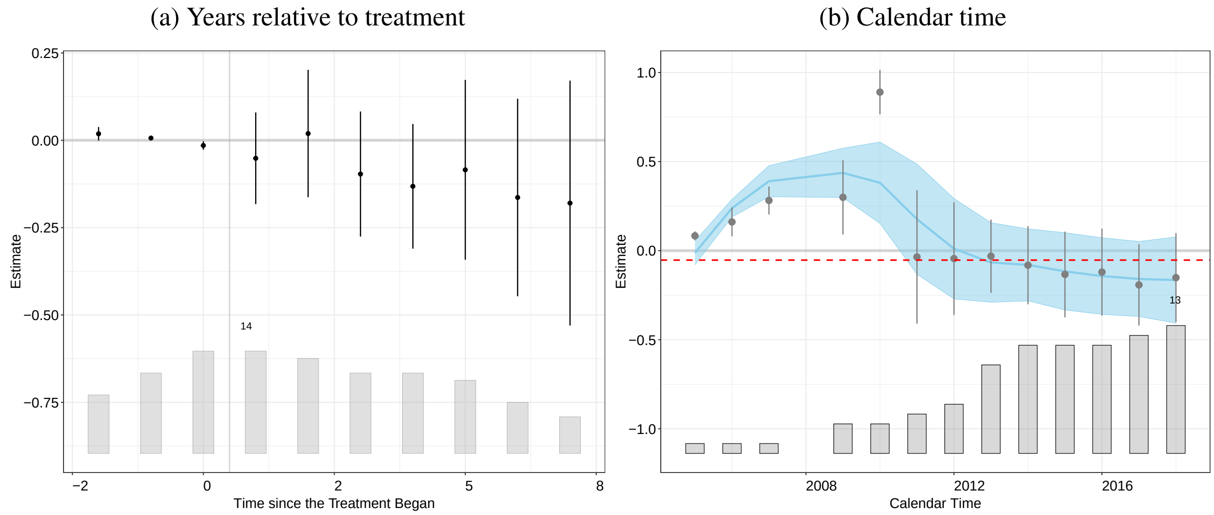
A7.4.2 Estimated Effects

In Figure A18 we report the estimated treatment effects from FECT.[28] We group these effects in two different ways: panel (a) shows the estimates by years relative to treatment while panel (b) reports them in calendar time, or an estimate in each year (as in Figure A16). The former results indicate no effect of voter ID laws—the point estimates are consistently near zero and the confidence intervals include zero. However, grouping by calendar time displays considerable heterogeneity in the results, similar to those from the Callaway and Sant'Anna (2021) estimator above. The effects of voter ID laws in the early years (prior to 2011) were large and positive with confidence intervals bounded away from zero. But moving from left to right shows that the estimates then shrunk to near zero in later years.

FECT also reports averages of these temporal treatment effects and uses two different methods to compute the average. The first weights all treated observations (i.e., state-years) equally, which produces an estimate of -0.053 and a 95% confidence interval of $[-0.256, 0.150]$. The second method weights all treated units (states) equally and yields an estimate of 0.432 [0.248, 0.616]. Thus, one method suggests a null effect of voter ID laws while the other method gives one of the largest effects in support of our expectation across all of the estimators we report in the main text and SI. When combined with the fact that we are unable to use all of our covariates (see note 28), these findings reduce our confidence in the FECT results.

[28]The state government ideology and state court strength covariates lead to unstable estimates in the subset data, so we omit them in this case.

Figure A18: Estimated Effects from FECT by Years Relative to Treatment and Calendar Time



Note: The graphs present the estimated effects of voter ID laws going in force on the difference in party median ideal points in state legislatures, grouped as years relative to treatment (panel a) and calendar time (panel b). Line segments indicate 95% confidence intervals. N = 194.

A7.4.3 PanelMatch vs. FECT

Liu et al. (2022) report that PanelMatch and FECT are two of the most flexible and accommodating methods for estimating treatment effects with panel data. Most notably, both are improvements over two-way fixed effects models, which do not allow for heterogeneous treatment effects and are susceptible to model specification issues (Imai et al. 2022). With respect to differences between them, FECT uses all of the available data, but PanelMatch prunes data in its matching routine. However, PanelMatch allows analysts to condition on time-invariant covariates, whereas FECT does not (Liu et al. 2022, 16).

Ultimately, we select PanelMatch as our main estimation method of choice for two reasons. First, although none of our covariates are completely time-invariant, several change slowly over time, such as party of the governor or partisan control of the legislature. PanelMatch can better accommodate this feature. Second, in practice the results reported by FECT in our data are quite sensitive to model specification choices—a point illustrated above. In contrast, as we show throughout the main text, SI, and replication materials, our results are quite robust to various user choices in PanelMatch. Thus, we regard PanelMatch as the safer option with respect to the problem of researcher degrees of freedom (Simmons et al. 2011).

A7.5 Weighting

Because the main drawback to PanelMatch is that it involves "pruning" the data to identify treatment effects, we next consider one more estimation approach—weighting—that uses all available data. Weighting methods involve generating observation-level weights to balance the data, then estimating a model of the outcome with those weights. Doing so allows us to retain all of the data in the subset sample rather than just cases in matched sets.[29] We also use this estimation strategy to continue investigating the possibility of temporal heterogeneity in our treatment effect.

We utilize Huling and Mak's (2022) energy balancing algorithm to generate weights based on the covariates discussed in the main text. This method minimizes an "energy statistic" related to imbalance in the covariate distributions. It has numerous advantages over competing methods (see Huling and Mak 2022), although the results we present below are robust to several of these alternatives. However, energy balancing is primarily designed for weighting in a cross-sectional framework; it has not been extended to time-series data (Greifer 2021*b*). Thus, we view this analysis only as a secondary method that is useful for assessing robustness.[30] As a feasible solution, we adjust for the temporal component of the data by including a time counter and its squared term as additional covariates on which to balance. After estimating weights, we then regress our outcome on the treatment and state fixed effects to account for baseline differences across states.

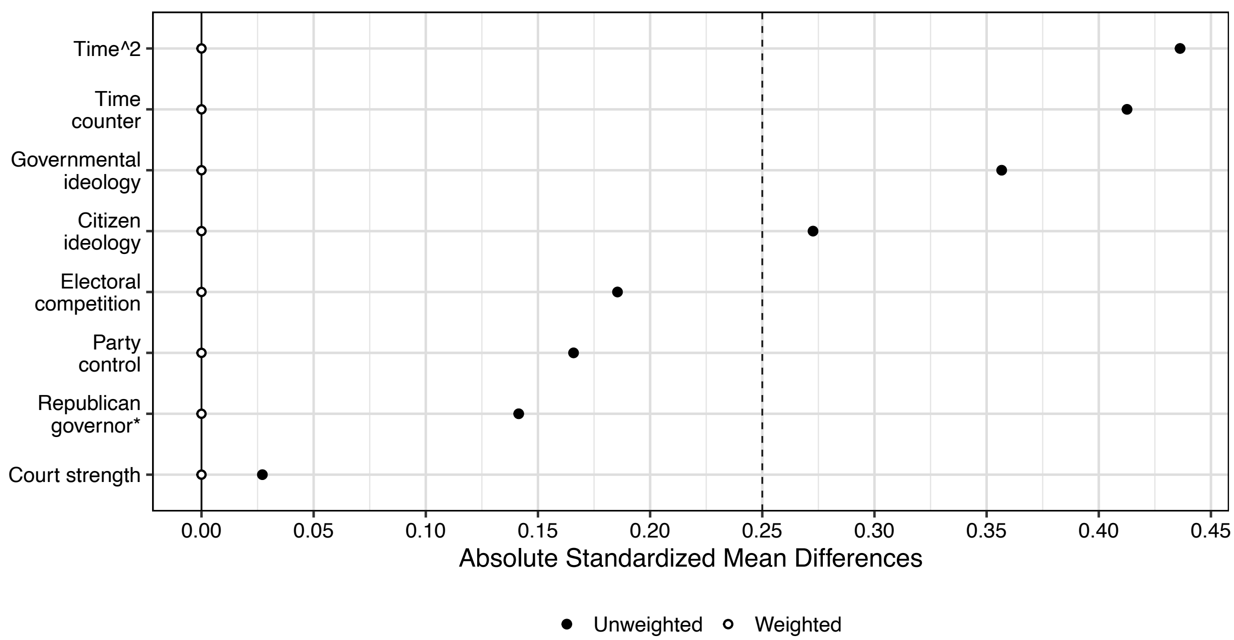
[29]We still restrict the sample to only the legal delay subset. However, results are substantively similar if we use the full sample of data.

[30]We considered a weighting method that allows for longitudinal treatments—Inverse Probability of Treatment Weighting (IPTW, see Blackwell 2013). Treatment effect estimates with this method were similar to what we report below. However, balance improvement was quite poor using IPTW, so we abandoned it in favor of energy balancing, which provides substantial balance improvement (see below).

A7.5.1 Balance

We begin by assessing our weighting procedure's effects on covariate balance.[31] Figure A19 reports absolute standardized mean differences between the treated and control cases before and after weighting. Imbalance in the unweighted data is not extremely severe (note the scale of the x-axis). Indeed, as we show in Figure A7, the subsetting process significantly improves balance. However, while there are four covariates within the threshold of 0.25, two other covariates (governmental ideology and citizen ideology) and the newly-added time variables are outside the threshold. Energy balancing improves balance by reducing the mean differences to zero on all of these covariates.[32]

Figure A19: Covariate Balance After Weighting with Energy Balancing



Note: The graph presents absolute standardized mean differences before and after weighting with Huling and Mak's (2022) energy balancing algorithm.

[31] We employ the covariates presented in the main text here, but results are substantively the same if we include the additional covariates discussed in Section A6.1 above.

[32] This reduction to zero occurs because we set a balance constraint on the weights such that they guarantee exact balance on the first moment of the distributions (Greifer 2021*b*).

A7.5.2 Estimated Effects

Table A12 reports regression estimates of voter ID laws' effects. The first two columns use the full subset data (all years). The first column reports unweighted results (i.e., a standard regression) and the second column reports results with the energy balancing weights applied. Both models include state fixed effects, but no other covariates (because they were used to generate the weights). We also consider whether the estimated effects vary over time. The second two columns report results from the data prior to 2011. The final two columns report results with data from 2011 to 2018.

Table A12: Treatment Effect Estimates with Energy Balancing Weights

	2003–2018		2003–2010		2011–2018	
Estimate	0.088	0.094	0.187	0.236	0.071	0.042
SE	0.029	0.041	0.099	0.114	0.021	0.031
95% Lower	0.030	0.005	−0.023	−0.018	0.030	−0.025
95% Upper	0.146	0.183	0.396	0.491	0.111	0.108
Weights		✓		✓		✓
State Fixed Effects	✓	✓	✓	✓	✓	✓
N	194	194	35	35	159	159
Adjusted R^2	0.92	0.94	0.89	0.94	0.97	0.97

Note: Cell entries report treatment effect estimates, standard errors (SE), and 95% confidence intervals with and without energy balancing weights. The outcome is the difference in party median ideal points. The first two columns report results from the full subset sample with all years and the remaining pairs of columns report results for data from 2003 to 2010 (second pair) and 2011 to 2018 (third pair).

The results support our previous findings. All of the estimates are positive, in line with H1, and the 95% confidence intervals exclude zero in the all years models. Additionally, as we saw with the FECT results (Figure A18), the largest effects appear for earlier laws implemented during 2003–2010, although our statistical power is lower in those models. The effects of later laws (post-2010) are still positive here, but smaller in magnitude. In short, this analysis shows the robustness of our findings with a different estimation strategy and provides more evidence that earlier laws may have been more polarizing than later voter ID requirements.

A8 Mechanism and Placebo Tests

Here we report additional information about the test of the mechanism presented in the main text as well as a second mechanism test. Finally, we present an additional placebo test.

A8.1 Details on the Other Contentious Policies

In our mechanism test in the main text we estimate the effects of several other contentious state policies on party polarization in state legislatures. Here we provide additional information on the policies and those analyses.

A8.1.1 Policy Selection

We selected the policies from Boehmke et al.'s (2020) State Policy Innovation and Diffusion (SPID) database, which tracks hundreds of policies that diffused across the American states dating back to the 19th century. We looked for policies that (1) diffused roughly around the 2003–2018 timeframe that we analyze and (2) exhibited clear partisan divergence in public opinion during the time period in which they were diffusing.[33] Table A13 summarizes the five policies in SPID that satisfied these criteria.

We identified summaries of public opinion polls from reputable firms to verify that the issues represented in these policies do, in fact, divide the two major parties in the electorate. The polling data for in-state tuition and same-sex marriage come from 2011 Pew Research Center polls. The abortion opinion data come from 2007 Pew Research Center polls that asked respondents if they favored "legal abortion in all or most cases." The stand your ground data come from a 2013 Quinnipiac University poll. The public accommodations data come from a 2017 Public Religion Research Institute that asked respondents if they opposed so-called "bathroom bills" that require transgender people to use the bathrooms that correspond with their sex at birth rather than their gender identity.[34]

[33] We omit states that adopted these policies prior to 2003 from the analyses.

[34] See the replication materials for press release summaries of each survey.

Table A13: Summary of Other Policies Used in the Mechanism Test

Policy	Adoption years	Total states	% Party support	
			Republican	Democrat
In-state college tuition for undocumented persons	2001–2014	19	33%	56%
Permission of same-sex marriage	2000–2012	18	27%	56%
Pre-abortion ultrasound requirements*	1996–2012	24	63%	39%
Stand your ground laws	1994–2011	23	75%	38%
Bans on public accommodation discrimination due to gender identification*	1993–2014	17	36%	65%

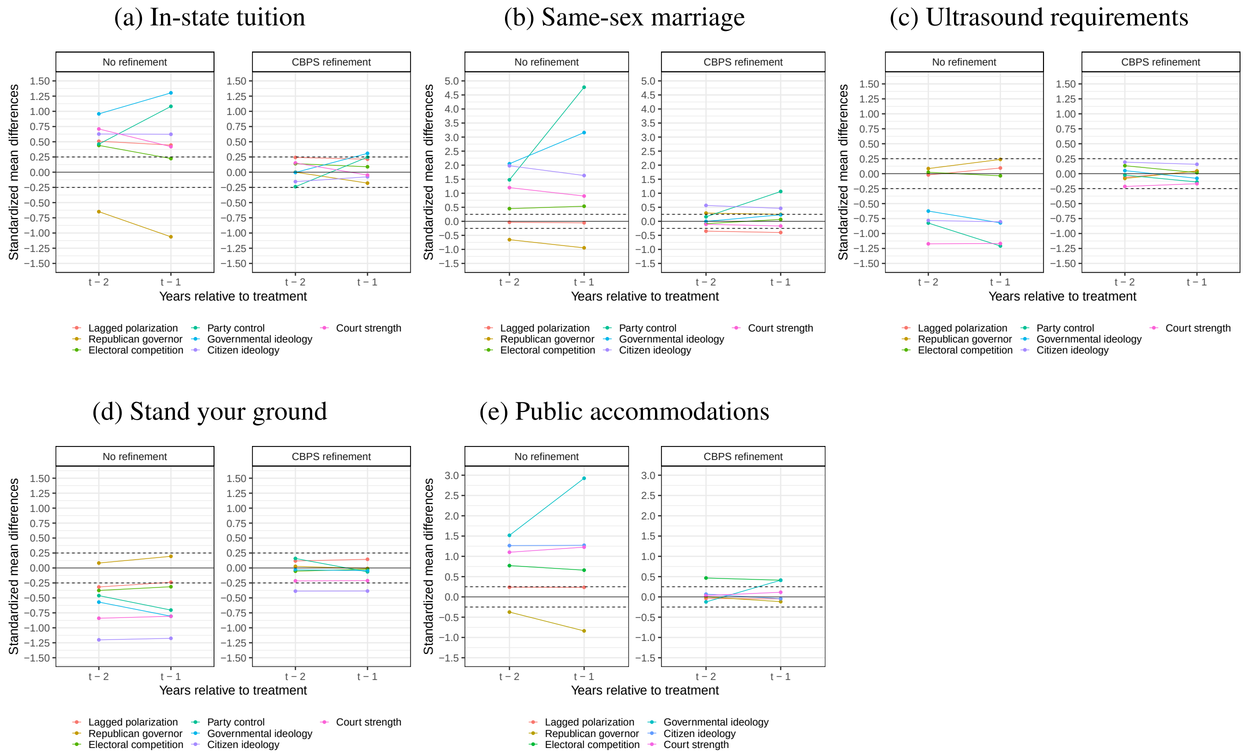
Note: Cell entries summarize the policies used in the mechanism test analysis. The adoption data come from SPID (Boehmke et al. 2020). *Denotes a different question used in polling data (see the text).

A8.1.2 Covariate Balance

Figure A20 summarizes covariate balance for the analyses presented in the main text mechanism test (Figure 4). As we note, the implementation delay subsetting strategy is not available for these analyses because the laws were not subject to widespread legal challenges as with voter ID laws. Accordingly, we must rely exclusively on the matching procedure and covariates to mitigate confounding in these cases. This lack of plausibly exogenous variation is evident in the fact that balance improves considerably between the left (no refinement) and right (CBPS refinement) panels in each graph. Clearly the covariates are necessary, giving us the most confidence in the estimates based on CBPS refinement presented in Figure 4.

Most importantly, the balance after CBPS refinement is quite strong. The right panels in Figure A20 show that the standardized mean differences between treated and untreated cases with respect to lagged polarization, electoral competition, partisan control of the executive and legislative branches, state ideology, and court strength are consistently within or near the threshold. Thus, despite the fact that we are unable to leverage plausibly exogenous variation due to legal delays, Figure A20 gives us some confidence that the results we report in Figure 4 are not spurious

Figure A20: Covariate Balance in the Matched Sets with Other Policies



Note: The graphs present standardized mean differences in the covariates over time after generating matched sets with PanelMatch for the other policies used in the mechanism test.

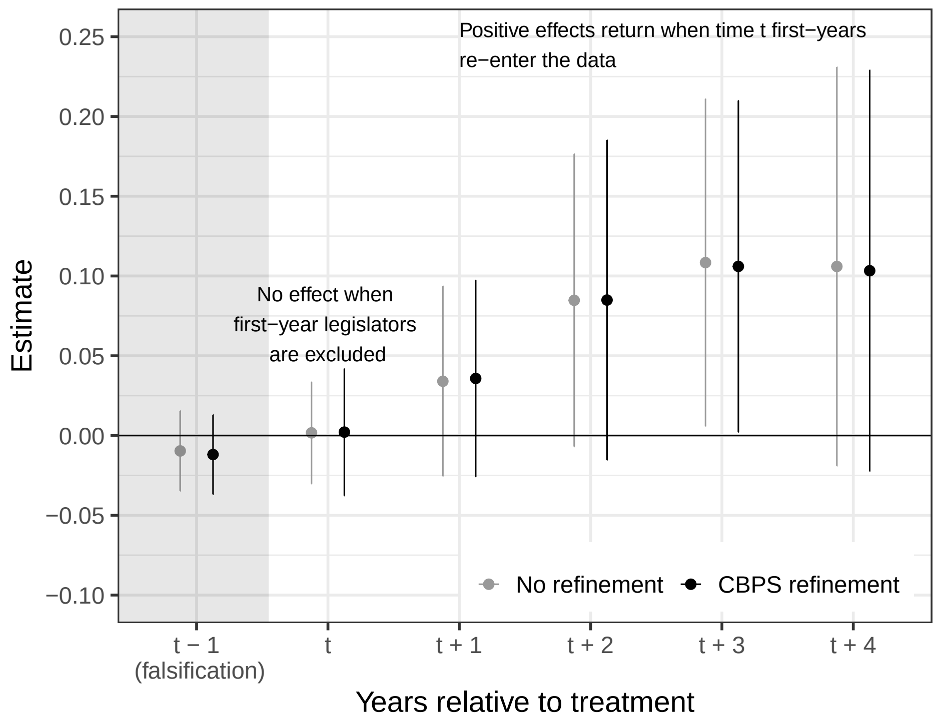
associations driven by the political factors that motivated states to adopt these policies.

A8.2 Removing First-year Legislators

Our proposed theoretical mechanism is that voter ID laws alter legislative candidates' perceived uncertainty about the median voter's location, which moves their campaign platforms away from center. An observable implication of this mechanism is that candidates joining the legislature immediately after law implementation are more ideologically extreme, on average, compared to incumbents. Recall that the temporal variation in our outcome variables stems only from legislator turnover. Thus, our main results already provide at least partial empirical validation of this contention. Here we conduct further investigation by examining the impact of first-year legislators on our estimates. If the addition of new members after law implementation contributes to polarization, then we would expect to find no treatment effect when those first-year lawmakers are excluded from the data.

More specifically, we report results with a new version of our main outcome variable in which we omitted any lawmakers in their first years in office before computing the difference in party medians. All other aspects of the research design remain the same as in Figure 2 (main text). Legislators contribute to the computation of the outcome in their second years and beyond, but *not* in their first years. With these anticipated "ideological movers" removed, we expect a null effect immediately after law implementation. Figure A21 reports PanelMatch results with this alternative version of the outcome.

Figure A21: Estimated Effects of Voter ID Laws' Implementation on Party Polarization in State Legislatures without First-year Members, 2003–2018

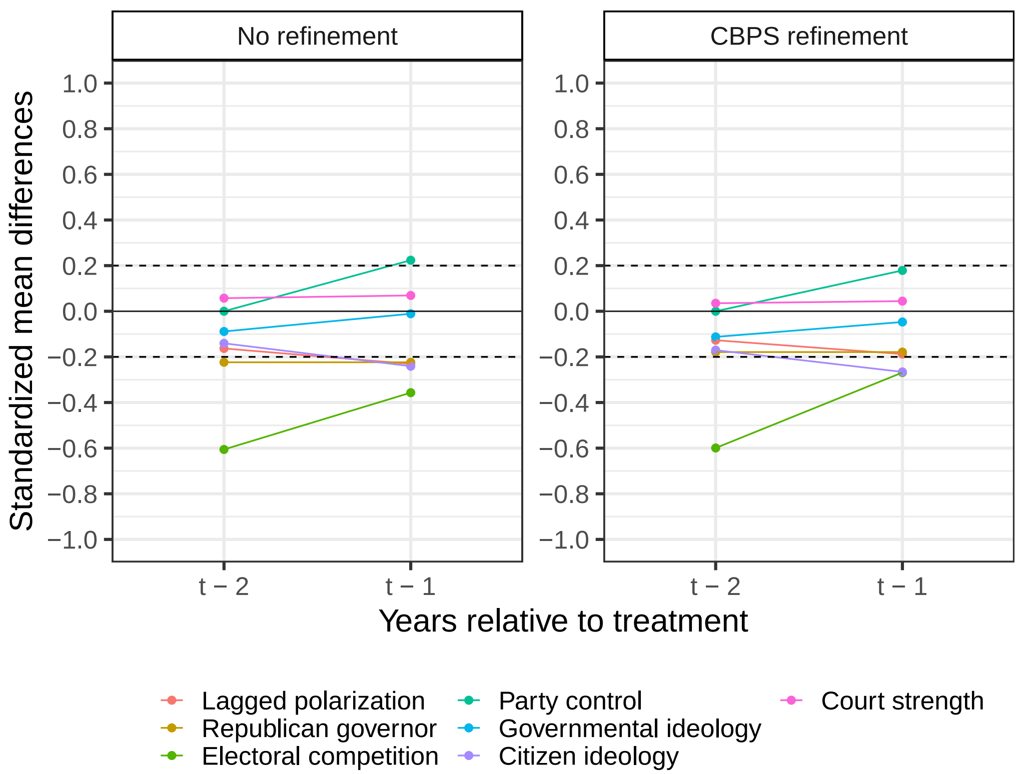


Note: The graph displays years relative to treatment (x-axis) against the estimated effects of voter ID laws going in force on the difference in party median ideal points in state legislatures with first-year members omitted (y-axis). Line segments indicate 95% confidence intervals. N = 194.

The falsification test again indicates no anticipatory effect, providing support for the research design. Most importantly, the estimated effect at time *t* in Figure A21 is essentially zero and not statistically significant. But in the subsequent time periods, the effect increases again and becomes comparable to the results with the main outcome (main text, Figure 2). In other words, when first-year lawmakers are excluded (time t), voter ID laws appear to exert no effect on polarization. Then when those first-year legislators re-enter the computation of the outcome—no longer as first-years—the positive effects appear again. This pattern provides additional empirical support for the theoretical process we propose.

Figure A22 presents covariate balance for this analysis with first-year legislators omitted. The graphs show that balance improvement is reasonable without covariate refinement and even stronger after matching refinement with CBPS. Thus, we again place the most trust in the estimates generated from the refined matched sets, although they are again substantively similar to the unrefined estimates.

Figure A22: Covariate Balance in the Matched Sets with First-year Legislators Omitted



Note: The graphs present standardized mean differences in the covariates over time after generating matched sets with PanelMatch. The outcome variable is computed after dropping first-year legislators.

A8.3 The Effects of Pre-HAVA Voter ID Laws

In Section A3.1 we discuss our decision to focus on the effects of post-HAVA voter ID laws on legislative polarization. This choice reflects the fact that post-HAVA laws were substantively different than those passed before HAVA. Specifically, pre-HAVA laws reflected "requests" for ID while post-HAVA laws involved more demanding requirements (National Conference of State Legislatures 2021; MIT Election Data + Science Lab 2021). We claim that pre-HAVA laws did not place the same burdens on voters as post-HAVA laws (see Section A3.1), and thus the former cannot be expected to meaningfully impact the electorate or increase uncertainty among candidates. This contention suggests a placebo test of our theory. We posit that pre-HAVA voter ID laws exerted no effect on legislative party polarization.

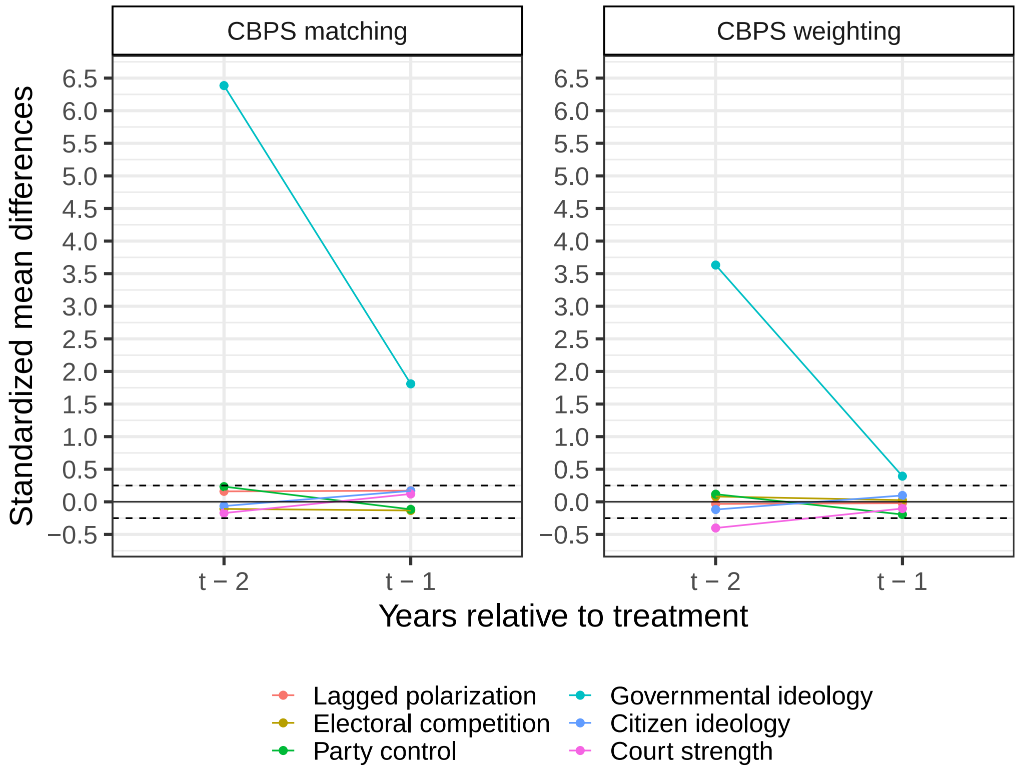
Our data extend back to 1993, which covers the implementation of the three pre-HAVA laws: Michigan in 1996 (implemented in 2007), Louisiana in 1997 (1997), and Florida in 1998 (1998). We created a new treatment indicator for state-years in which a pre-HAVA law was in force. In the case of Michigan, we subset the data to 1996–2018, which limits the treatment variation to only the period after the state adopted the law. However, because Louisiana and Florida experienced no delays, we use 1993–2018 in those cases. To facilitate as comparable of counterfactuals as possible, our "control" states are those that adopted a post-HAVA voter ID law in our data (i.e., the states listed in Figure 1 of the main text).[35]

Figure A23 reports covariate balance with this design. Matching by treatment timing alone (i.e., no covariate refinement) performs poorly, so we do not consider that approach here. Instead, we refine the matched sets with further matching using CBPS (left panel) and by weighting the data with CBPS. The graphs demonstrate that doing so yields excellent covariate balance for the most part. However, we are unable to balance on the indicator for a Republican governor due to limited variation among the three "placebo treated" states. Additionally, the governmental ideology is highly imbalanced, especially at time $t-2$. Balance improves at $t-1$, especially with

[35] Results are unchanged if we use the full sample of states as controls or if we include the full 1993–2018 time period for Michigan.

CBPS weighting. But we nonetheless must acknowledge caution in that case. The other covariates demonstrate very good balance at both time periods.

Figure A23: Covariate Balance in the Matched Sets of Pre-HAVA Voter ID Laws

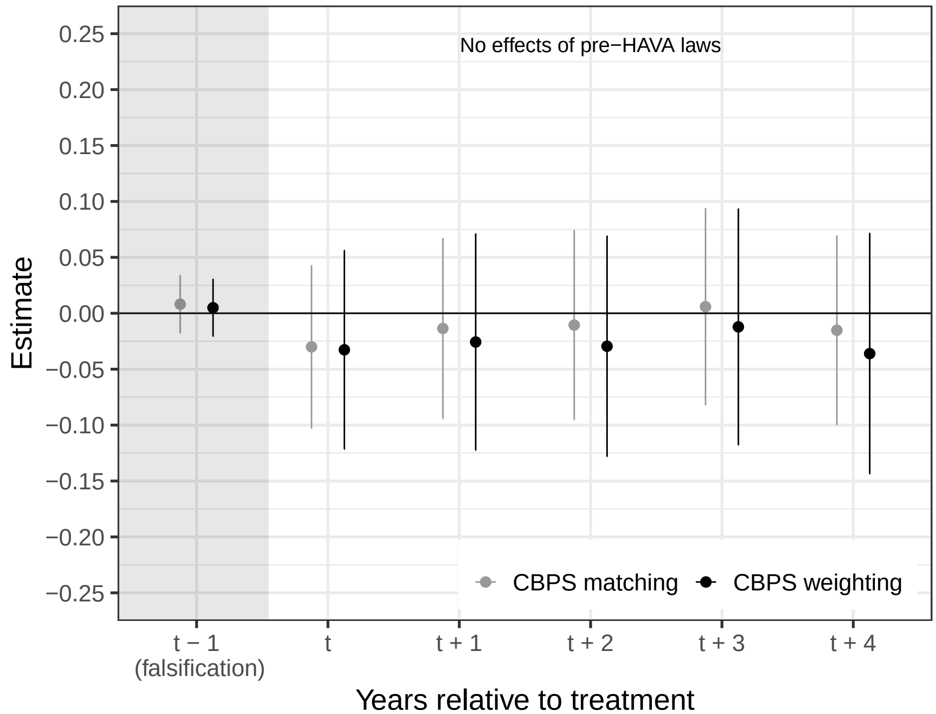


Note: The graphs present standardized mean differences in the covariates over time after generating matched sets with PanelMatch. The left panel gives results from matching with CBPS and the right panel gives results from CBPS weighting.

Figure A24 reports the estimated placebo effects of pre-HAVA laws on polarization. Regardless of refinement method, all of the estimates are near zero with confidence intervals that include zero. The ends of the confidence intervals suggest that large effects in magnitude are plausible. But the point estimates themselves are quite small in substantive terms: less than 5% of a standard deviation of the outcome, on average.

Moreover, all but one of the estimates (excluding falsification) are negatively signed, indicating a (negligible) *decrease* in polarization. Of course, the confidence intervals indicate that positive effects are possible. But overall, these results indicate a noticeably different pattern from the one we hypothesize. This finding yields additional support to our theory. We contend that the

Figure A24: Estimated Effects of Pre-HAVA Voter ID Laws' Implementation on Party Polarization in State Legislatures, 1993–2018



Note: The graph displays years relative to treatment (x-axis) against the estimated effects of pre-HAVA voter ID laws going in force on the difference in party median ideal points in state legislatures (y-axis). Line segments indicate 95% confidence intervals. N = 621.

uncertainty mechanism does not exist in this case because the "treated" states' voter ID laws are relatively weak in the demands they place on voters. Thus, legislative candidates do not have reason to become more uncertain about the composition of the electorate and so we would expect to see the negligible effects we find here.

A8.4 Non-competitive Districts and Polarization

One last possible causal mechanism we consider is the role of electoral competition at the district level. If competition is simply a confounding variable, then including the Holbrook and Van Dunk (1993) measure as a covariate in our analyses is an appropriate means of addressing the threat to bias in our estimates. However, competition could also be a mediating variable: perhaps voter ID laws suppress competition and this suppressed competition is associated with more extreme candidates.[36] If competition is a mediator, then controlling for it would lead to unbiased estimates with the assumption of no intermediate confounders (Acharya et al. 2016).

We investigated this possibility of mediation from competition by examining the relationships between (1) voter ID implementation and average electoral competition in a state-year and (2) competition and candidate extremity from Bonica's (2016) Database on Ideology, Money in Politics, and Elections (DIME). The key question is whether the polarization we find in this research is actually driven by noncompetitive districts. This pattern would emerge if (1) voter ID laws suppressed average competition and (2) this suppressed competition was associated with more extremity among legislative candidates, on average. Table A14 presents regression estimates for these two analyses, including bivariate models (1 and 3) and models with our main set of covariates used throughout this research and state and year fixed effects (2 and 4).

Table A14: Average Electoral Competition and Candidate Ideological Extremity, 2003–2018

	Electoral competition		Candidate extremity	
	(1)	(2)	(3)	(4)
Strict voter ID law implemented	−0.172 (0.145)	−0.006 (0.240)		
Electoral competition			0.370* (0.091)	0.029 (0.024)
Covariates		✓		✓
State Fixed Effects		✓		✓
Year Fixed Effects		✓		✓
Adjusted R^2	0.003	0.045	0.136	0.951
N	784	784	784	784

Note: Cell entries report regression coefficients with standard errors multiway clustered by state and year in parentheses. The outcome in models (1) and (2) is Holbrook and Van Dunk's (1993) measure of electoral competition and the outcome in models (3) and (4) is candidate extremity from the DIME data (Bonica 2016). Models (1) and (3) are estimated without covariates and models (2) and (4) include our main set of covariates and state and year fixed effects. * $p < 0.05$ (two-tailed).

The results indicate essentially no support for this proposed mediating relationship. The association between voter ID and electoral competition is negative, as expected, in models (1) and (2),

[36]We thank an anonymous reviewer for suggesting this possibility.

but the coefficients are not statistically significant and essentially zero once covariates and state and year fixed effects are added to the specification (model 2). Additionally, models (3) and (4) show a *positive* relationship between electoral competition and candidate extremity that is statistically significant initially and near zero after adding covariates and the two-way fixed effects.

In sum, electoral competition in state legislative districts does not appear to be a mechanism in our proposed causal process. Theoretically, Hassell (2023) shows that party elites are engaged in primaries across the whole spectrum of districts, including those in which the party has no chance to districts that are safe for the party. However, these elites are less likely to *coordinate* on a specific candidate in districts that are "safe" for their party. Instead, the level of intraparty elite disagreement increases with the electoral safety of the district. Hassell (2023) does not examine candidate ideology, so it is somewhat ambiguous how elite coordination (or lack thereof) affects candidate ideology across different districts. But we posit that primary winners in safe districts would most likely *not* be uniformly ideologically extreme given the lack of elite coordination.

A8.5 Party Tethers and Ideological Distribution of the Candidate Pool

We argue that parties use tethers to influence the ideological positions taken by their candidates in campaigns for state legislative office. In our conceptualization, party tethering is a multifaceted process that includes strategic candidate recruitment and selective candidate support in primary elections. Tethering starts with the informal party organization's efforts to shape the candidate pool. Bonica's (2016) DIME data allows us to estimate the ideological distribution of this pool and explore how that distribution changes after implementation of a voter ID law. Following Hall (2019), we create a measure of candidate pool polarization measured at the state-election level. Pooling across all candidates for state legislative positions within each state and election year, we measure the absolute distance between the CFscore of each candidate and the median candidate within the state, and then take the average of those distances to generate an overall polarization score for the candidate pool in that state during that election year. Considering a subset of states that passed and implemented voter ID laws within the timeframe of our study (see Figure 1 in the main text), we split state-year observations up into pre-implementation and post-implementation subsets. We then conducted a t-test to determine if there was a statistically significant difference in the mean candidate pool polarization score before and after implementation of a voter ID law.

We found a statistically significant ($p < 0.05$) difference between groups, with the candidate pool polarization score increasing by nearly 60% of a standard deviation after implementation. This result suggests that part of the polarization we observe in our main results may be driven by pre-election decisions by potential candidates about whether or not to run in the first place. However, this possibility does not necessarily contradict the theoretical logic that the implementation of a voter ID law increases polarization through its effect on uncertainty about the ideological position of the median voter. It could be the case that the polarizing effect of uncertainty on platform formation is already reflected in the CFscores of candidates who decide to run in a more uncertain environment.

The above analysis provides suggestive evidence that the implementation of a voter ID law is associated with increased polarization of the candidate pool. Next, we consider how party tethers can drive asymmetric polarization. While the deployment of a partisan tether is not a phenomenon we would expect to be able to observe directly, we are able to specify expectations about the observable implications of a tether being employed. One such implication is that a party's candidate pool should be more ideologically homogeneous after a tether has been deployed. Consistent with our expectation that state and local Republican parties are opportunistically deploying tethers in the wake of voter ID implementation, we find a marginally statistically significant ($p < 0.10$) decrease in the standard deviation of Republican candidates' CFscores after implementation of a voter ID law. This increase in ideological homogeneity of the Republican candidate pool is equivalent to roughly 26% of a standard deviation on the outcome variable. Conversely, we find a marginally statistically significant ($p < 0.10$) increase in the standard deviation of Democratic candidates' CFscores after implementation of a voter ID law. The Democratic candidate pool becomes more ideologically divided, with an effect size that is roughly 43% of a standard deviation. Finally, we conducted a t-test in which the outcome variable is the difference between the standard deviations of Republican candidate CFscores and Democratic candidate CFscores in each state-year. Once again, we are comparing state-years prior to voter ID law implementation with state-years after implementation. We find that the ideological cohesion gap between the parties (the extent to which the Republican candidate pool is more cohesive than the Democratic candidate pool) increases by nearly 44% of a standard deviation after the implementation of a voter ID law ($p < 0.05$).

A9 Budget Delay Models

Here we provide additional details on our analysis of budget delay presented in the main text. We discuss the covariates included in the models and report the full set of results.

A9.1 Covariates

We include two sets of covariates in these models. The first is the original set that we employ in our PanelMatch analyses in the main text. See Table A8 for complete descriptions of these variables. The one covariate we omit from this set is the one-year lag of the outcome, which is included in our PanelMatch analyses. This variable is recommended for use with PanelMatch (Imai et al. 2022), but is potentially problematic here. Lagged dependent variables are a source of bias in regression models with fixed effects unless the number of time points is large (Nickell 1981).

Our second set of covariates comes directly from Klarner et al. (2012), who develop and test a comprehensive theoretical account of delay in the state budgeting process. We omit their time-invariant covariates and temporal covariates that do not vary across states because those factors are subsumed by our fixed effects and time controls. We include all of their other covariates, which include temporal and cross-sectional variation. We briefly summarize these variables here; see Klarner et al. (2012, 998–1000) for full details.

- **Election year**: Indicator for a legislative and/or gubernatorial election year;
- **Divided government**: Indicator for years in which the Democratic and Republican parties share control of state government;
- **Session end vs. start of FY**: Calendar days between the start of a state's fiscal year and the last date the state constitution says the legislature can meet in regular session (a measure of the private costs of budget delay);

- **Personal income**: Annual growth rate in per-capita income over the prior 12 months;
- **Budget size**: Total expenditures in 2000 dollars;
- **Surplus**: Difference between total expenditures and revenue in the previous year;
- **Legislative salary**: Base salary (excluding per diem) in the legislature.

We include all of these covariates in a linear probability model of budget delay—an indicator for a budget passed after the first day of the new fiscal year.[37] The models estimated on the implementation delay subset data include state fixed effects and a linear time counter to control for temporal effects. We do not include year fixed effects in those models because some years include very few states, including only one state in 2003. The models estimated on the full data (all states, 2003–2018) include state and year fixed effects.

A9.2 Full Results

Table A15 reports the full model results: coefficient estimates and standard errors multiway clustered by state and year in parentheses. Recall that the outcome is binary, so the coefficients are interpretable on the probability scale. Two cases are dropped in the implementation delay subset (Indiana and Ohio in 2006) and 50 cases in the full data due to biennial budgeting in some states.

[37]The linear probability model allows us to avoid the well-known incidental parameters problem in nonlinear models with fixed effects (Greene 2004), but results are similar with logistic regression.

Table A15: Estimated Effects of Voter ID Law Implementation on Late Passage of State Budgets, 2003–2018 (Full Results)

	Delay subset		Full data	
	(1)	(2)	(3)	(4)
Strict voter ID law implemented	0.112 (0.084)	0.142 (0.111)	0.101* (0.046)	0.142* (0.039)
Election year		−0.003 (0.059)		0.072 (0.061)
Divided government		0.052 (0.248)		0.025 (0.045)
Session end vs. start of FY		−0.740 (1.615)		−0.010 (0.068)
Personal income		0.017 (0.045)		0.044 (0.037)
Budget size		−0.016 (0.112)		0.121* (0.060)
Surplus		−0.012 (0.033)		0.003 (0.031)
Legislative salary		−0.052 (0.101)		0.035 (0.090)
Republican governor		0.003 (0.365)		0.017 (0.070)
Electoral competition		0.002 (0.004)		0.001 (0.003)
Legislative party control		0.086 (0.134)		0.043 (0.022)
Governmental ideology		0.005 (0.013)		−0.001 (0.004)
Citizen ideology		−0.001 (0.005)		0.00002 (0.003)
Court strength		1.475 (1.162)		2.520 (1.072)
State Fixed Effects	✓	✓	✓	✓
Year Fixed Effects			✓	✓
Time Counter	✓	✓		
Adjusted R^2	0.105	0.131	0.281	0.308
N	192	192	734	734

Note: Cell entries report regression coefficients with standard errors multiway clustered by state and year in parentheses. The outcome is a binary indicator for a late state budget. Models (1) and (2) are estimated on the implementation delay subset data and models (3) and (4) are estimated with the full data. * $p < 0.05$ (two-tailed).

A10 Multiple Imputation Diagnostics

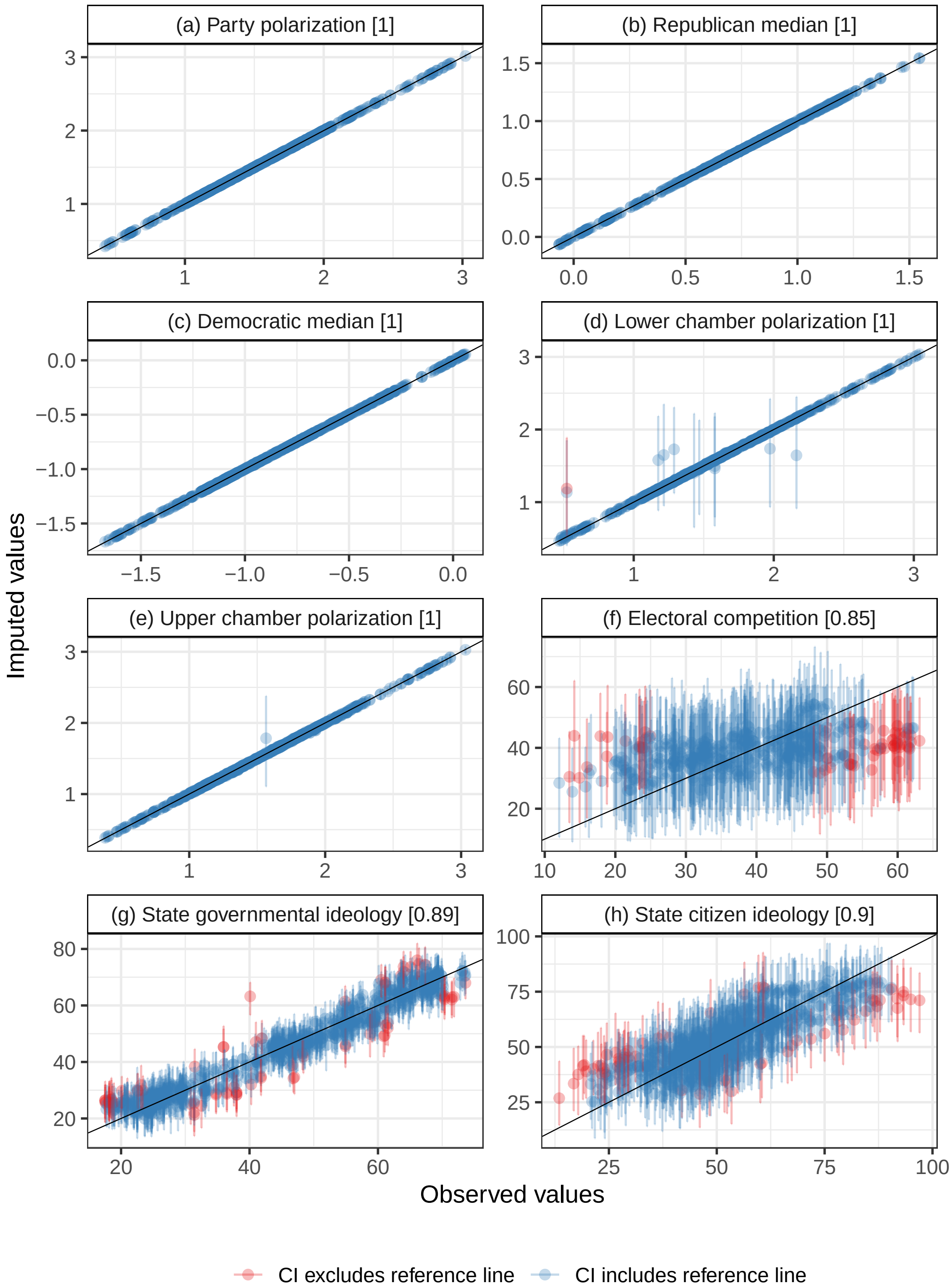
Our data include some missingness. We used multiple imputation with Amelia II (Honaker et al. 2011) to fill in missing values, producing five complete datasets for each outcome. The estimates we report throughout this research combine analyses with all five complete datasets and include appropriate adjustments to measures of uncertainty (Blackwell et al. 2017). Imputation has its own problems, which may even make listwise deletion preferable (see Arel-Bundock and Pelc 2018). Thus, it is important to assess the quality of the imputed data.

A10.1 Overimputation

Overimputation is a diagnostic tool that conducts imputation of the observed (i.e., non-missing) data, then compares the imputed to the actual values of those data. Figure A25 presents overimputation results for the variables used in the data presented in the main text. The observed values of the non-missing data are plotted on the x-axes and imputed values (averaged over the five datasets) are plotted on the y-axes. The vertical line segments indicate 95% confidence intervals for the imputations and the solid line serves as a reference point for "perfect" imputation. In an ideal scenario the points would fall along the reference line. More realistically, favorable evidence for the imputation procedure would exist if (approximately) 95% of the confidence intervals include the reference line. The colors classify each point based on this criterion: blue indicates points for which the confidence interval includes the reference line and red indicates points that do not. The values in square brackets next to each label refer to the actual coverage level for that variable.

The graphs in Figure A25 generally shows good coverage of the reference line. The clouds of points trend upward, and most of the points are blue. The ideal point variables (panels a–e) show near perfect results because polarization is a linear combination of the party medians. The coverage rates for the other variables are slightly less than, but close to, the target of 0.95. Electoral competition is the worst performer at 0.85, although an alternative measure (a folded Ranney index) shows better coverage and does not change our substantive conclusions. Thus, the imputation results fall short of ideal, but are nonetheless reasonable.

Figure A25: Overimputation Results

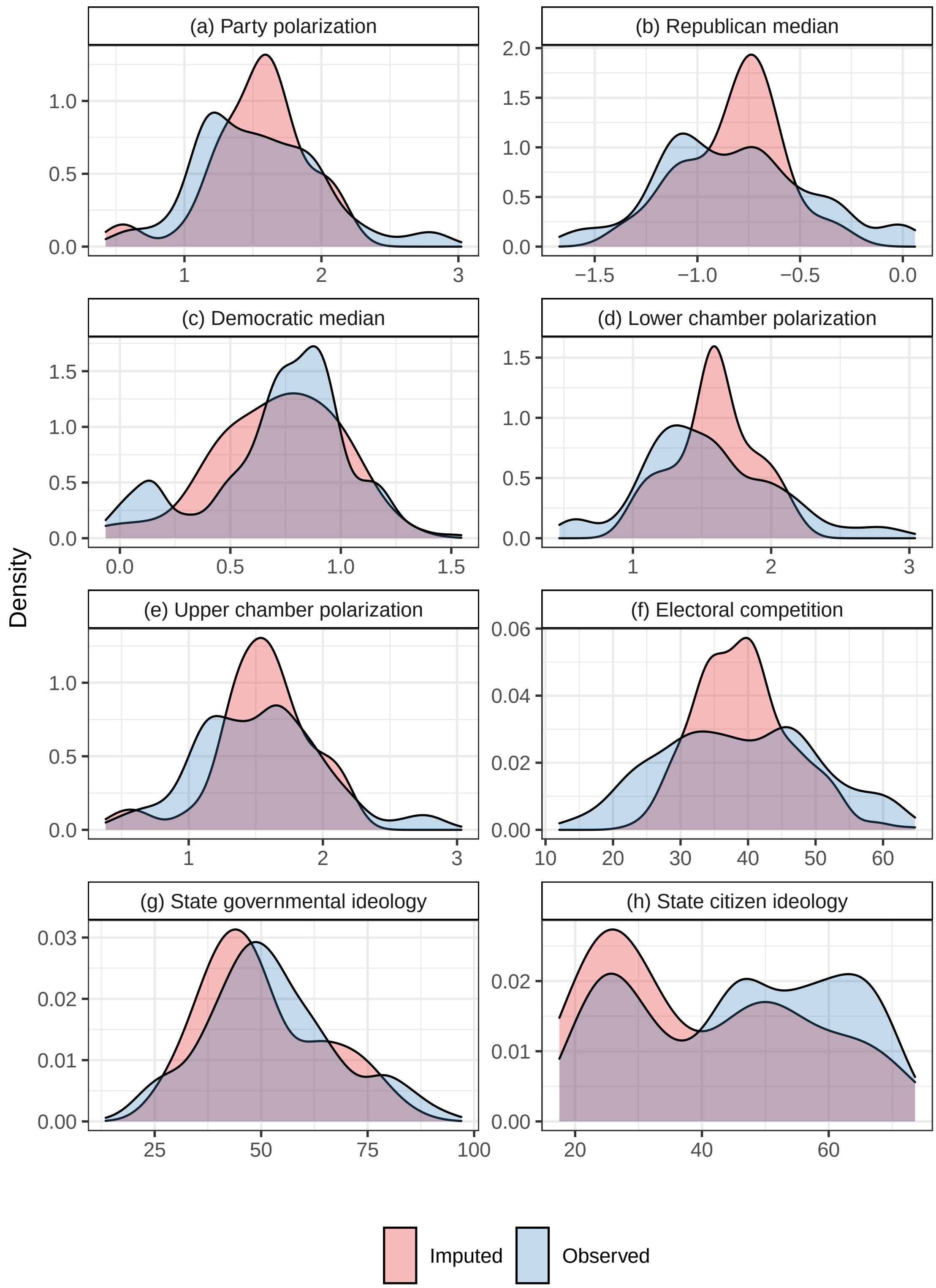


Note: The graphs present observed values of each variable on the x-axes against mean imputations of those values on the y-axes. Line segments indicate 95% confidence intervals. The solid line serves as a reference point for perfect imputation. The values in square brackets next to each variable label refer to the actual coverage level for that variable.

A10.2 Density Plots

Figure A26 presents density plots of the observed (blue) and imputed (red) values (averaged across the five datasets) of each variable. These graphs indicate considerable overlap between the two groups. This finding provides further evidence that the imputation procedure produced reasonable values for the missing data.

Figure A26: Observed and Imputed Densities



Note: The graphs present density plots of the observed and mean imputed values for each variable.

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