The Legislative Legacy of Strict Voter Identification Laws

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Abstract

Does the implementation of strict voter identification (ID) laws intensify conflict between legislative parties? Existing scholarship extensively studies these laws' influence on political engagement in the mass public, but their implications for elites in government remain an open question. We examine the consequences of the American states' modern wave of voter ID requirements for polarization in their legislatures. We theorize that these laws increase legislative candidates' 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 ID law implementation induced by the unpredictable timing of post-enactment legal challenges. Combining this variation with several methods for covariate adjustment isolates the unique effects of the laws on polarization. We find empirical support for our expectations; a law going in force widens interparty ideological distance. This divergence is driven primarily by Republican parties, does not appear in other contentious policies, and is not an artifact of the legislative agenda. Thus, we conclude that a crucial legacy of these laws is the amplification of political conflict. By creating conditions that stifle compromise and bipartisanship, strict voter ID requirements *indirectly* threaten the quality of American democracy.

Keywords: Strict voter identification laws; Party polarization; State legislatures

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On April 6, 2021, Wyoming Governor Mark Gordon signed House Bill 75 into law after it passed through the state's legislature with supermajority support. The bill, sponsored by a large contingent of Republican legislators, established a new requirement that all voters must present acceptable identification (ID) when voting in person. Republican officials touted the new law's expected improvements to election security, calling it a "victory for the citizens of Wyoming" (Fike 2021). However, one lawmaker, Senator Anthony Bouchard, focused more on the bill's ideological implications in a social media post:

> Governor Gordon has just signed into law the photo ID bill I sponsored along with my conservative colleagues in the Senate. So which #woke corporation will attack our state next? (quoted in Fike 2021).

While Bouchard's rhetoric could simply represent standard partisan taunting, the comments may also signal a real amplification of ideological division in the legislature. These bills are important to both parties and have garnered attention from lawmakers, news media, and scholars in recent years. Yet despite their prominence as a subject of controversy and acrimony in American political discourse, the discussion rarely turns back to their implications for the original sources of the laws: state legislatures. Could the implementation of a strict voter ID law actually *intensify* partisan conflict in future legislative sessions?

Scholars have traditionally studied the consequences of strict voter ID laws for the electorate, focusing on voter registration and turnout, voter fraud, and public opinion. They report that the laws' effects on mobilization and fraud are generally small, but also find evidence of partisan differences in public support. This apparent inconsistency—divergent views on laws that appear to exert minimal observable impacts—suggests the need to look beyond the mass public to fully understand the political ramifications of voter ID legislation. We do so here by assessing how these laws structure the prospects for the future of bipartisan cooperation in the legislature.

More specifically, we consider whether strict voter ID laws exacerbate polarization between elites in government. Using a spatial proximity model, we theorize that implementing a strict voter ID requirement expands state legislative candidates' 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 strict voter ID laws and party polarization in American state legislatures during 2003–2018. 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 are often the subject of lawsuits and other legal obstacles that can potentially delay when they are allowed to go in force. We argue that there is sufficient uncertainty in this regard such that the *length* of a legal delay is unpredictable. Thus, states that passed strict 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 strict voter ID law going into effect exerts a notable increase in legislative polarization that lasts several years. It is primarily a consequence of 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 strict voter ID requirements impact legislative politics. Two analyses of the causal mechanism suggest that the polarizing effect does not result from other contentious policies and that the replacement of relatively moderate incumbents by more extreme new lawmakers drives the effect. Furthermore, we find that our results *cannot* be explained by changes to the composition of the legislative agenda.

Our evidence demonstrates that the implications of strict 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 strict 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, has far-reaching implications; it hinders negotiation and efficient policymaking, polarizes the electorate, and may even encourage future legislation that restricts voter access. Thus, regardless of whether or not they impact voter turnout or political engagement, strict voter ID laws *indirectly* threaten the quality of the democratic process by inhibiting effective governance and the representation of citizen interests.

1 The Consequences of Strict Voter ID Laws

The extant research on strict 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 a high level of baseline support, but with clear differences between the parties.

Importantly, strict 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, Fording, and Schram 2011). A common claim is that strict 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, Lajevardi, and Nielson 2017; Kuk, Hajnal, and Lajevardi 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, Hersh, Meredith, Mummolo, and Nall 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 strict 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, Nuño, and Sanchez 2009; Fraga and Miller 2022).

An additional wave of research examines the development of public opinion toward strict 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 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 partisan differences in opinion, but overall diffuse support relative to other issues. For instance, Wilson and Brewer (2013) report that 62% of Democrats and 94% of Republicans in a 2012 national survey favored the laws. 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; Gronke, Hicks, McKee, Stewart, and Dunham 2019).

A more recent and growing 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, McKee, Sellers, and Smith 2015; Biggers and Hanmer 2017), electoral competition (Hicks, McKee, and Smith 2016), and state racial composition (Rocha and Matsubayashi 2014; McKee 2015) as key determinants of strict voter ID law adoption. However, while this research is located in 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 strict 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.

1.1 The Determinants of State Legislative Polarization

How might strict voter ID laws fit into the landscape of factors that influence party polarization in state legislatures? Despite arguments from reformers and commentators, polarization in the states is not simply a result of institutional design choices that can be easily changed if the parties become too ideologically distant. Several "usual suspects" that concern reformers—such as legislative professionalism, chamber size, redistricting, and primary elections—all exert minimal effects (see Masket 2019). The institutional factors that do matter operate in somewhat surprising ways. For instance, rather than reducing party conflict and improving representation, legislative term limits exacerbate polarization (Olson and Rogowski 2020).

State-level polarization largely stems from nationwide economic and political trends. Voorheis, McCarty, and Shor (2015), for instance, find that rising income inequality in the states has driven the parties apart (see also McCarty, Poole, and Rosenthal 2006). Grumbach (2020) shows that polarization increases as more donors to state legislative candidates are linked to interest groups. Ideological heterogeneity, uncertainty about average constituent preferences, and declining media coverage in state legislative districts can amplify the influence of party on representatives' decisionmaking (Snyder and Strömberg 2010; McCarty, Rodden, Shor, Tausanovitch, and Warshaw 2019). And the expansion of national issues into state politics further inflames partisan divisions (Grumbach 2022). In short, polarization in state legislatures is a dynamic process in which parties respond to each other and the political environment, pursue policy objectives, and advance electoral strategies. Below we theorize that strict voter ID laws fit into this narrative. By changing who can vote, these laws modify the incentives and beliefs of candidates with policy and electoral goals, ultimately widening divisions between legislative parties.

2 Strict Voter ID Laws and Legislative Polarization

Our theoretical framework originates in the unique nature of strict voter ID laws. Unlike abortion, gun control, or other potentially contentious policies, strict voter ID laws are written and implemented *specifically to alter the composition of the electorate*. We posit that this central objective changes legislative candidates' and parties' electoral strategies, ultimately shifting the parties in the legislature farther apart in ideological space. Our theory is based in McCarty and Meirowitz's (2007) spatial proximity models of political competition (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 model formally and derive our hypotheses from its equilibrium predictions (see Section A1).

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.[1]

With these assumptions in place, consider candidates' strategies when a strict 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

[1]Candidates may be more attuned to some voters, such as donors to their campaigns (Barber 2016). But they ultimately must win the median voter's support to win the election. Following Kousser, Lewis, and Masket (2007), we assume that there is no difference between candidates' chosen platforms and their voting behavior in office.

the median exactly, but can use these resources to form their beliefs about it. Candidates from both parties are then incentivized to move toward that point. In this scenario, winning candidates tend to be those with relatively centrist platforms, which generates 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 SI Section A1.1.1. In brief, in this scenario all actors involved believe that the new law has deactivated more Democratic than Republican voters (Grimmer and Yoder 2022).[2] 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 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 strict voter ID implementation. Irrespective of the law's actual effect on turnout, these candidates believe that the electorate as a whole has changed. 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 *increase* voter motivation. For example, the law could spike enthusiasm among Republicans and/or anger among Democrats (Valentino and Neuner 2017), increasing turnout. Indeed, in recent years the Democratic Party and its allied groups have used strict 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 strict voter ID laws may or may

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

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 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, Green, and Levy 2014). 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.

The main consequence of this increase in 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 SI Section A1.2 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 (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. Consequently, they choose relatively more extreme platforms, which are reflected in the voting behavior of those candidates who ultimately win office (see note 1). This logic predicts divergence between the two parties in the legislature, which we formalize in our first hypothesis.

H1 The implementation of a strict 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 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, Grofman, and Brunell 2014). 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 strict 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 strict voter ID laws if its leadership did not, on balance, believe that they benefited Republican candidates. 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 in districts, we contend that state Republican parties believe that they have, on average, come out ahead after implementing such a law.

As such, we expect that the implementation of a strict voter ID law leads Republican parties to adopt a tether and demand more extreme platforms from their candidates. Nationally, the Republican Party greatly values ideological purity and prefers to avoid allowing its candidates to drift to the middle (Merrill, Grofman, and Brunell 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.[3]

[3]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.

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). Furthermore, its strong opposition to these laws and efforts to pass expansive voting rights legislation to counteract them 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 strict 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 strict 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

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See SI Section A1.3 for more on this point.

enter the legislature compared to a baseline case with no strict voter ID law in force. These new representatives' roll call voting pulls the parties away from each other, increasing polarization.

3 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 strict 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; Hicks, McKee, and Smith 2016). We must carefully guard against attributing the political dynamics that led to the laws' adoption as part of their actual effects.

In what follows we describe our data and multifaceted research design intended to identify the effects of implementing strict voter ID laws 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. We look to other contentious policies for evidence of polarizing effects and examine the consequences of strict voter ID laws outside of the roll call record.

3.1 Data

We constructed state-year panel data for the period 2003–2018 to test our theory.[4] This timeframe covers state laws adopted after the passage of the federal Help America Vote Act (HAVA) in 2002. A few states had voter ID laws on the books that predated HAVA, but these laws were lax by modern standards. They generally only "requested" ID from voters and provided low-burden options for voters who could not fulfill the request (National Conference of State Legislatures 2021). As such, we follow previous studies and exclude these laws from our analyses (e.g., Hajnal, Lajevardi, and Nielson 2017; Grimmer et al. 2018; Cantoni and Pons 2021). We focus on identifying the effects of post-HAVA laws that have been the subject of political controversy in recent years.

[4]We omit only Nebraska because its nonpartisan legislature renders some of the variables we analyze undefined.

3.1.1 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 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.[5]

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.[6] 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, Lewis, and Masket 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. Previous research indicates that this framework for empirically observing polarization is reasonable (e.g., Olson and Rogowski 2020). In fact, two-

[5]Another option is Bonica's (2016) measures derived from campaign contributions. Results with those data point in the same substantive direction as what we report below, but with considerable uncertainty due to missingness for much of our sample.

[6]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 (Section A4).

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).

3.1.2 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 vote on a provisional ballot and also take additional steps after Election Day for it to be counted" (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 "strict" 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 some action after Election Day 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 SI Section A6.3 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.

3.1.3 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 (Section A3)—include variables that the literature suggests may be associated with the choice to adopt a strict voter ID law and could also plausibly correlate with our outcome variables.[7] 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) and Berry, Ringquist, Fording, and Hanson's (1998) measures of state governmental and citizen ideology.

We employ a parsimonious set of covariates due to a relatively small sample (see below). However, in the SI (Section A6.2) 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*

[7]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 (Section A10).

states' strict voter ID laws on legislative polarization in a state—which we measure with adoptions by geographic neighbors and policy "source" states (see Section A3 of the SI for complete details).

3.2 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 strict 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 strict 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 strict laws faced the uncertainty of a ballot initiative either before or after the legislature passed implementing legislation.[8]

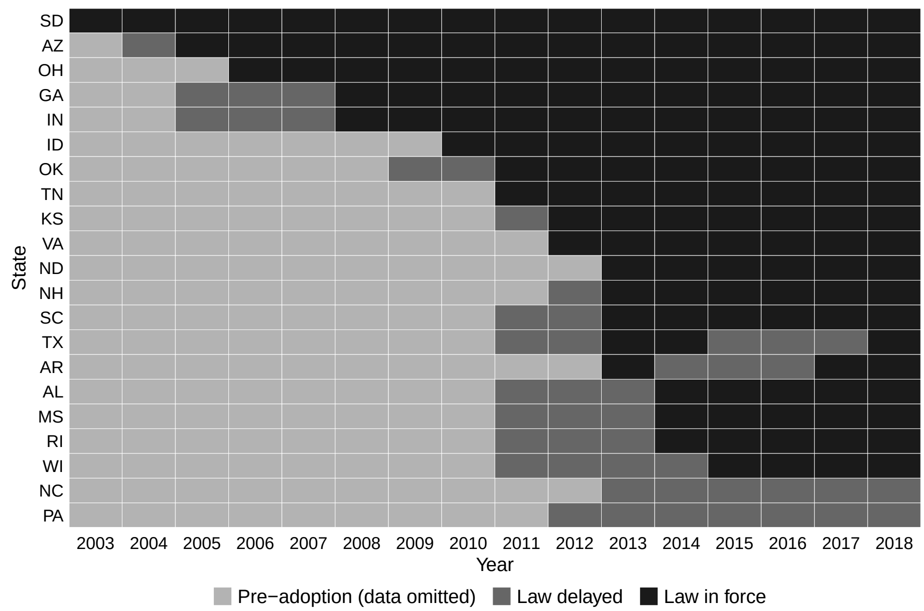
From an empirical standpoint, these delays represent the first component of our identification strategy. While strict voter ID laws are clearly not randomly assigned, we contend that the timing of their implementation in states that enact them is as-if random. When a governor signs a voter ID bill into 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 strict 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 strict 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 strict law (as defined above) beginning in the year in which the law was adopted (e.g., 2011 for Wiscon-

[8]In Section A2.1 of the SI we summarize the legal history and timing of all strict voter ID laws examined here.

sin). That is, in our main analyses the sample consists only of those state-years denoted in dark gray or black in Figure 1.

Figure 1: The Timing of Strict 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 strict voter ID laws.

The key consequence of this subsetting strategy is that the variation in our treatment variable comes *entirely* from the unpredictability of the implementation process rather than states' decisions to adopt a strict voter ID law or not. 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 strict voter ID law at the same time, but the legal process happened to resolve more quickly for South Carolina.

In the SI (Section A5), 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 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.

3.3 Additional Balancing and Estimation

A natural choice for modeling these panel data might be the two-way fixed effects estimator, which adjusts for state- and year-specific confounders. This method, which is a weighted version of the canonical differences-in-differences (DID) model, identifies the ATT under the assumption that the outcome trend would be the same in treated states in the absence of treatment (Imai and Kim 2021). While we contend that our subsetting approach strengthens the justification for this assumption, the clear partisan motivations underlying our selection problem—which likely vary over time within states—suggest the need for additional tools to address confounding.[9] Accordingly, we employ Imai, Kim, and Wang's (2022) PanelMatch estimator, which adapts the logic of matching methods specifically for panel data. We describe it briefly here; see Section A5.2 of the SI for details on our use of the method.

PanelMatch is a design-based approach that allows us to assess balance in our time-varying covariates and estimate treatment effects in the DID framework.[10] It is flexible in that it handles

[9]In addition, the inclusion of year fixed effects in our subset data is problematic because there are very few cases for some years, including just one observation in 2003 (see Figure 1).

[10]The method makes a few key assumptions (Imai, Kim, and Wang 2022, 10–13). 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. 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. Furthermore, we show in the SI (Section A6.2) that our results are robust to the inclusion of two common diffusion covariates: ID

treatment "reversals" more easily than does two-way fixed effects (Imai and Kim 2021; Imai, Kim, and Wang 2022), which Figure 1 shows is a relevant issue in our data. Furthermore, it permits estimation of short-term and long-term effects. This property is useful for understanding the "legacy" of strict voter ID laws on legislative politics beyond just the first year of implementation. Finally, PanelMatch is useful because it is more robust to model misspecification bias than two-way fixed effects (Imai, Kim, and Wang 2022).[11]

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, Kim, and Wang 2022). We are able to use two years of treatment history for this step.[12] 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). Then 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.[13] Here we estimate effects up to four years after treatment, which allows for the possibility of a delay in the effects of voter ID law implementation due to election cycles or other factors. Finally, the standard errors condition on the matching procedure

law adoptions by geographic neighbors and adoptions by policy "source" states. PanelMatch also inherits the parallel trends assumption as a DID estimator. While this assumption is difficult to test, especially in our subset data, we note that our subsetting strategy and further efforts to balance the data are intended to support the credibility of parallel trends (see the SI, Section A5.2).

[11]In Section A7 of the SI we consider an alternative estimation strategy using a weighting estimator with state fixed effects and time controls and find substantively similar results.

[12]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).

[13]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).

and assume independence across states, but not within them (Imai, Kim, and Wang 2022).

4 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 strict voter ID law going in force.[14] 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.

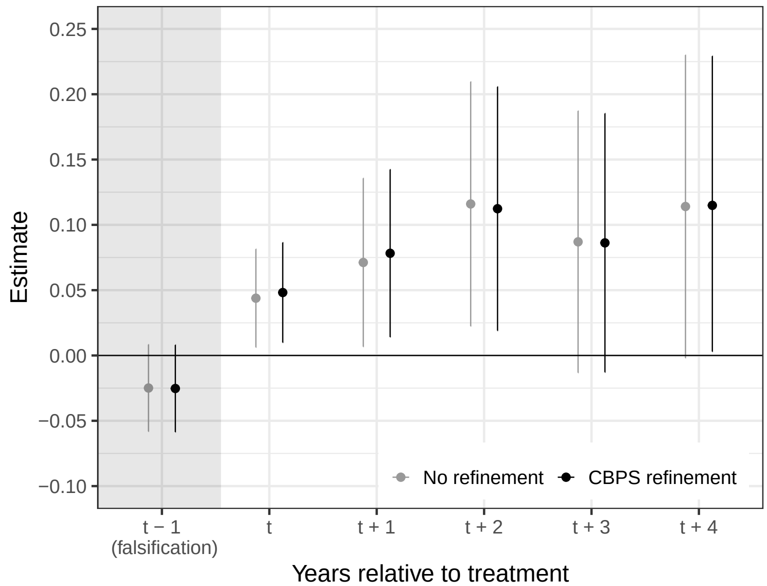
4.1 Strict Voter ID Laws Increase Polarization

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, strict 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 strict 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

[14]We omit the lagged outcome as a covariate in these estimates; including it would invalidate the falsification test (Imai, Kim, and Wang 2022).

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



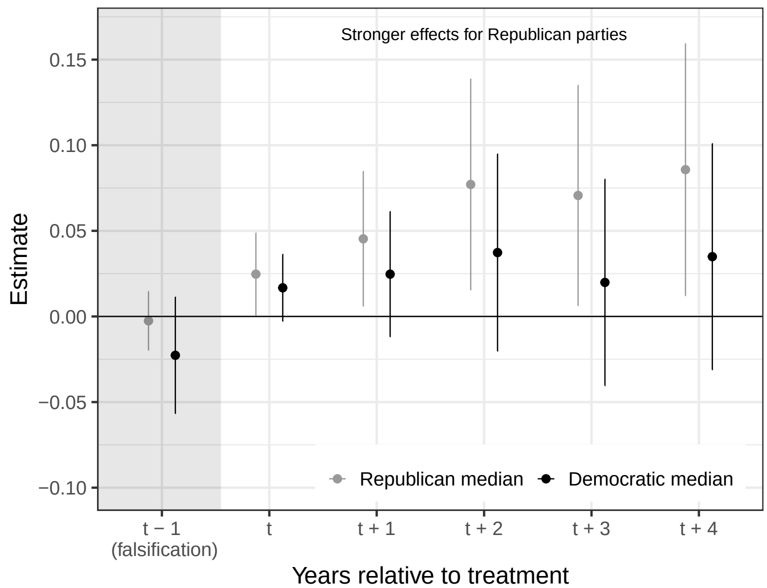
Note: The graph displays years relative to treatment (x-axis) against the estimated effects of strict voter ID laws going in force on the difference in party median ideal points in state legislatures (y-axis). Line segments indicate 95% confidence intervals. N = 194.

zero. The estimates remain approximately at that level after three and four years, although our statistical power declines at that point and some 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 we are examining 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 strict voter ID law is comparable in its amplification of ideological differences to the major 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 Strict 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 strict 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.[15] In terms of the treatment effects, the graph suggests that strict 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. However, the effects appear to be stronger for Republicans compared to Democrats. The estimates are all larger for

[15]Balance is not quite as strong compared to the polarization variable (see the SI, Section A5.4). However, the fact that these estimates still pass the falsification test bolsters our confidence in the inferences we can draw from the results.

Republicans, in line with H2, although the differences are not statistically distinguishable between parties. Thus, this result is suggestive of the asymmetric effect that our theory predicts, but caution in interpretation is warranted due to statistical uncertainty.

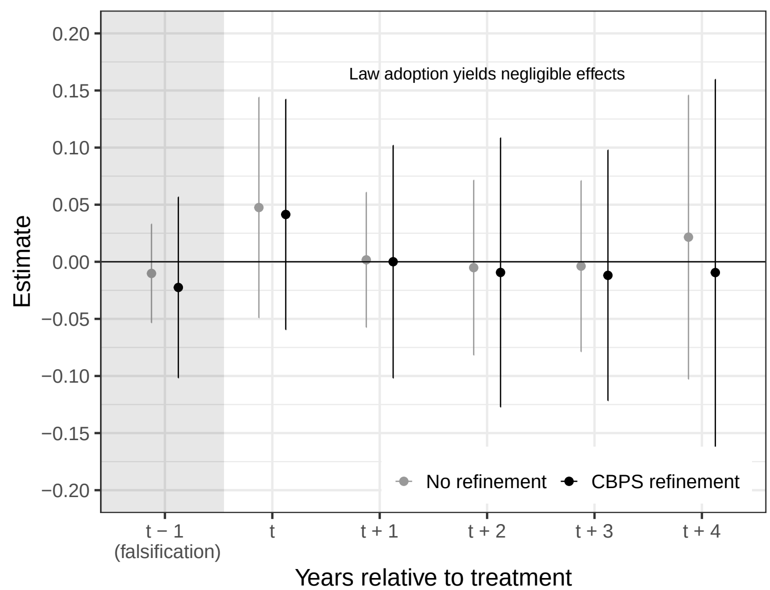
Overall, these main analyses display general empirical support for our theoretical framework. Strict voter ID laws polarize state legislatures, primarily by driving the average Republican party farther to the right. The effects start small, but grow in subsequent years after implementation of a law as relatively extreme candidates who campaigned under heightened uncertainty begin to replace relatively moderate incumbents (see SI Section A8.2 for additional analysis supporting this aspect of the mechanism). Additionally, we show in the SI that this process occurs in both chambers, but is mostly an upper chamber phenomenon (Section A4). We also demonstrate that the findings are robust to numerous other potential confounders, an alternative estimation strategy, and that the effects display some heterogeneity over time (see Sections A6 and A7).

4.2 Adoption as a Placebo Test

Our theoretical framework emphasizes the *implementation* of a strict 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 strict 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 states enacted the laws—ignoring implementation dates. We lose our data subset identification strategy here, which warrants appropriate caution. But our covariates and PanelMatch specification from 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 of the SI for details). The estimated treatment effects appear in Figure 4.

Figure 4: Estimated Effects of Strict 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 strict 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.

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 effects, the graph shows a small uptick in polarization in year *t* that is about the same magnitude as the initial effects of implementation (see Figure 2). However, the confidence intervals for these estimates include zero. Moreover, the estimated effects quickly move to near zero and stay there for several successive time periods. The confidence intervals are fairly wide, so large effects are at least *plausible* given the data. But overall the adoption of a strict 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 could actually affect the composition of the electorate and increase candidates' uncertainty about the median voter that we observe their polarizing influence.

4.3 Assessing the Mechanism with Other Contentious Policies

The placebo test above evaluates our theory by temporally removing the proposed mechanism from the analysis. Another means of achieving that same objective involves the consideration of other issues. Our theory implies that strict voter ID laws are unique, even among contentious policies over which state legislatures debate, because they directly target the composition of the electorate. Accordingly, policies that do not influence who can vote should not exert the same effects on polarization.

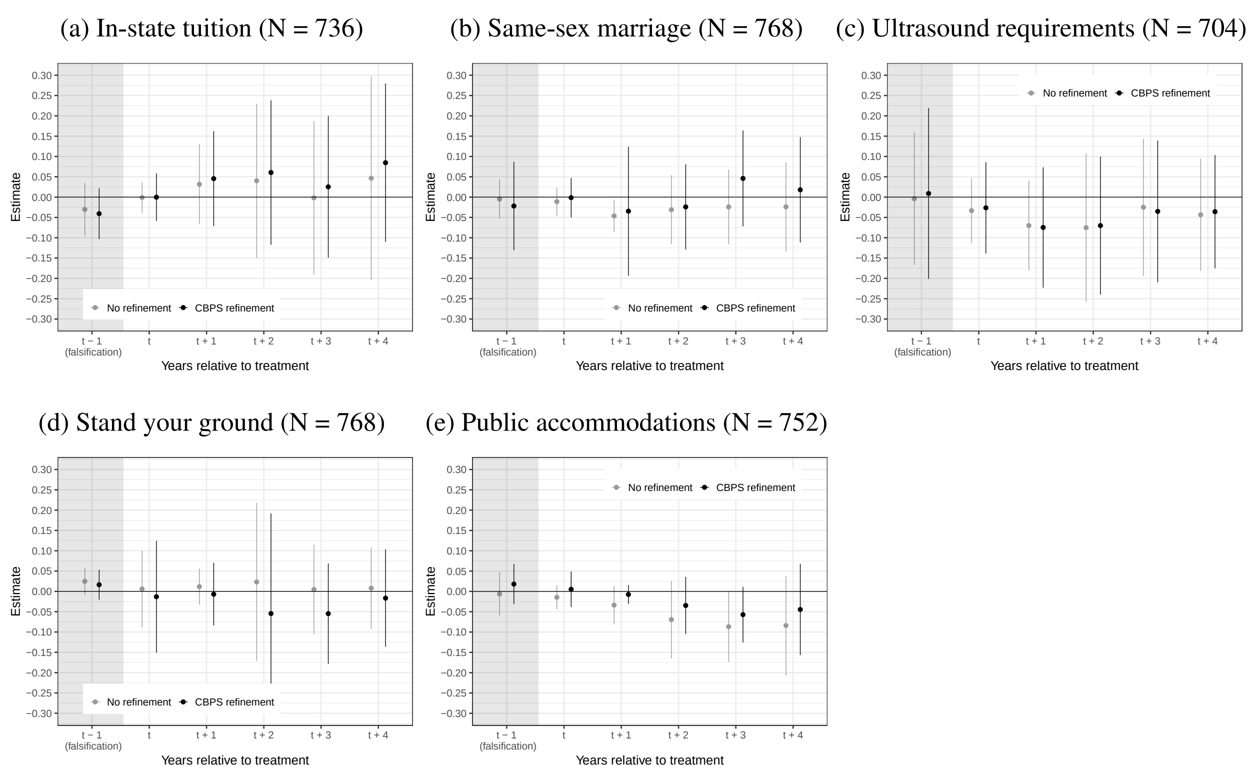
Specifically, we next examine whether several other contentious state-level policies that are unrelated to election law exert similar polarizing effects. We searched Boehmke et al.'s (2020) database for policies that diffused across the states during approximately the same time period and for which polling data indicate partisan opinion divergence in the mass public.[16] 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 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 strict voter ID laws. Thus, we consider a state treated 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 SI Section A8.1.2).

[16] See SI Section A8.1 for diffusion times and summaries of polling data on these issues.

Figure 5 presents the estimates and 95% confidence intervals for each of the new policies. The graphs show essentially no evidence for polarizing effects. For some issues the point estimates suggest small positive effects on average (panel a, in-state tuition) while others indicate a decrease in polarization (panel c, ultrasound requirements). But virtually none of them 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 a). But others are small enough to suggest truly *negligible* treatment effects (e.g., some estimates in panels b, d, and e). Most importantly, the weight of the evidence in Figure 5 indicates a noticeably different pattern from that of strict voter ID laws (see Figure 2).

Figure 5: 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. Sample sizes for each policy are listed in parentheses.

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 directly affects who can vote. We contend that this targeting of voters produces the uncertainty among candidates necessary to increase polarization. Thus, we would only expect 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 the electorate. In the SI we conduct an additional mechanism test—the expectation that first-year legislators are the most ideologically extreme—and again find support for our theoretical framework.

4.4 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 1). 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 strict 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. Lacking a separate indicator of legislator ideology that is not based in votes (see note 5), we do not yet know if our results reflect a true increase in discord between the parties or if they 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, Phillips, and Muckler 2012). All states are legally required to adopt a new budget on regular schedules and failure to do so results in adverse consequences, such as government shutdowns. Budget delay yields considerable advantages in measuring the working relationship between parties in the legislature. Because the budget is a required agenda item and its due date is exogenous to the agenda, any delay in its passage is insulated from the institutional pressures that influence the bills that receive a floor vote (Kirkland and Phillips 2018). With such high stakes, passing a budget late is a clear signal that the parties in the legislature are unable to negotiate and compromise, which is one of the major normative concerns stemming from legislative polarization (Harden and Kirkland 2021). Thus, gridlock on one of the most important items on the agenda is useful here as an alternative indicator of conflict between the parties. Specifically, we test one final hypothesis.

H3 The implementation of a strict voter ID law increases the probability that the state budget is passed late.

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.[17] We also include state fixed effects and either year fixed effects or a time counter.[18] 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).[19] Table A7 in the SI reports coefficient estimates for the covariates.

[17]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 the start of the new fiscal year, state personal income, budget size, an indicator for a budget surplus, and legislative salary. See the SI (Section A9.1) for more details on these variables.

[18]The time counter is a necessary replacement for year fixed effects in the subset data models because some years contain only one or two states (see Figure 1). 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.

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

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

	Delay subset		Full data	
	(1)	(2)	(3)	(4)
Strict voter ID law implemented	0.112 (0.084)	0.125 (0.104)	0.101 (0.046)	0.127 (0.033)
Covariates		✓		✓
State Fixed Effects	✓	✓	✓	✓
Year Fixed Effects			✓	✓
Time Counter	✓	✓		
Adjusted R^2	0.105	0.130	0.281	0.297
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.

The treatment effects are positive across the four specifications, indicating that the implementation of a strict voter ID law increases the probability of a late budget by 10–13 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. 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 strict 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 strict voter ID laws polarize the parties. Importantly, this evidence is not subject to potential bias from the formation of the legislative agenda. When combined with our more conventional analyses of ideal point measures, the totality of our evidence lends strong empirical support to our theoretical expectations.

5 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 strict 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 newsworthy topic in modern political discourse. How, then, do these laws actually affect American politics?

In this paper we look beyond strict voter ID laws' effects on voters and find that they hold important implications for lawmaking in state legislatures. We theorize that implementing these laws introduces new uncertainty for legislative candidates regarding where in the policy space their districts' median voters are located. The added uncertainty leads candidates to place more weight on their own preferences and, consequently, move away from the ideological center. 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 strict 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 is most prominent among Republican parties and, as expected, only appears after implementation. Finally, the effects are unique to strict voter ID laws, driven by electoral replacement, and observable in negotiations over states' budgets, which are exogenous to the legislative agenda.

Our results hold notable implications for the study of strict voter ID laws. Voter turnout is perhaps the most important outcome variable in this literature because the laws directly target who can vote. So it is no surprise that scholars have focused their attention primarily on turnout and mass political engagement. 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 important to consider the broader consequences of these laws to provide a complete picture of the role they play in American politics. This paper is one of the first to assess their legislative implications.

Normatively, our findings demonstrate that even one policy can hold major implications for democratic politics. 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 bills proposed during the 2021 legislative sessions took place in several states that had already adopted strict voter ID laws (e.g., Georgia and Texas). Thus, a key legacy of strict 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 American politics. 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, strict 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 Strict Voter Identification Laws

Supporting Information

Contents

A1 Theoretical Model

Our theoretical model is an adaptation of a spatial model of electoral competition from McCarty and Meirowitz (2007, 103–107). The baseline version of our model—which we use to illustrate candidates' decisionmaking when a strict 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 a 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.

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 strict 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 $s_D^* = s_R^* = m$. If we assume the median voter is located at .5, we can write payoff functions for each candidate in the following way:[1]

$$EU_{D(s_D,\, s_R)} = \begin{cases} -s_D^2 \text{ if } s_D < s_R \text{ and } \frac{s_D+s_R}{2} < .5;\text{ or } s_D > s_R \text{ and } \frac{s_D+s_R}{2} > .5 \\ -.5s_D^2 - .5s_R^2 \text{ if } s_D = s_R \text{ and } \frac{s_D+s_R}{2} = .5 \\ -s_R^2 \text{ if } s_D < s_R \text{ and } \frac{s_D+s_R}{2} > .5;\text{ or } s_D > s_R \text{ and } \frac{s_D+s_R}{2} < .5 \end{cases}$$

$$EU_{R(s_D,\, s_R)} = \begin{cases} -(1-s_R)^2 \text{ if } s_D < s_R \text{ and } \frac{s_D+s_R}{2} > .5;\text{ or } s_D > s_R \text{ and } \frac{s_D+s_R}{2} < .5 \\ -.5(1-s_D)^2 - .5(1-s_R)^2 \text{ if } s_D = s_R \text{ and } \frac{s_D+s_R}{2} = .5 \\ -(1-s_D)^2 \text{ if } s_D < s_R \text{ and } \frac{s_D+s_R}{2} < .5;\text{ or } s_D > s_R \text{ and } \frac{s_D+s_R}{2} > .5. \end{cases}$$

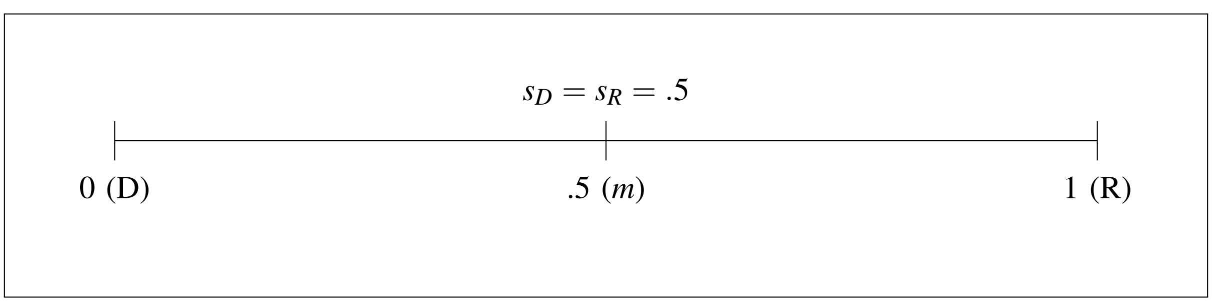
The Democratic candidate's best response (s_D^*) can be understood as a reaction to three possible categories of moves made by the Republican candidate. If the Republican candidate selects a platform that is less than the median ($s_R < .5$) the Democratic candidate cannot win the election by selecting a platform less than s_R, because the median voter would prefer the Republican candidate for all possible realizations of $s_D < s_R < .5$. Therefore, when $s_R < .5$ the Democratic candidate chooses a platform such that $s_D \geq .5 > s_R$. It is straightforward to demonstrate that this outcome would not constitute a Nash equilibrium, given the ideal points of the candidates (0 for the Democrat and 1 for the Republican).

[1]These payoff functions make an assumption that when the two candidates' platforms are identical (when $s_D = s_R$), elections are decided by a coin flip. This scenario is reflected in the middle outcome for each candidate, in which 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.

If, on the other hand, the Republican candidate selected a platform greater than the median ($s_R > .5$)—which would seem on the surface to be compatible with her policy preference—the Democratic candidate would select a platform such that $s_D \leq .5 < s_R$. This ordering is at least roughly consistent with the policy preferences of the candidates—but what would compel the Republican candidate to choose a platform greater than the median voter? It might seem that they would do so in pursuit of their ideal point of 1—but they would only be able to realize this outcome under the condition that they set a platform $s_R = 1$ *and* they won the election with that platform. Given a median of .5, with $s_R = 1$, the Democratic candidate would be able to win the election with any platform such that $0 < s_D < 1$. For example, a Democratic platform $s_D = .5$ would guarantee a victory for the Democratic candidate (in the case where $s_R = 1$), and therefore the Republican candidate would end up with a utility of $-(1-.5)^2 = -.25$. Because the Republican candidate could improve her utility by making a different choice of platform, this outcome would not constitute a Nash equilibrium.

The third possible scenario is one in which the Republican candidate chooses a platform identical to the location of the median voter, such that $s_R = .5$. In this case, it is clear that the Democratic candidate's best response is to choose a platform such that $s_D = s_R = .5$, because there is no platform that the median voter prefers to .5. This outcome, $s_D = s_R = .5$, constitutes the unique Nash equilibrium to this baseline version of the game. It shows that candidates converge toward the center in a scenario in which they are certain about the median voter's location. We illustrate this equilibrium in Figure A1.

Figure A1: Nash Equilibrium in the Baseline Model



A1.1.1 Rightward Shift Model

Because one potential outcome of implementing a strict voter ID law is that the eligible electorate becomes more conservative, it is worth pointing out that this convergence equilibrium would also apply to a more conservative median voter, $m > .5$. As long as the location of the median voter is known with certainty to both candidates, neither candidate 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.

Therefore, if a strict voter ID law simply makes the eligible electorate move right by deactivating relatively more Democratic voters on the left—and if all candidates agree on this point—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.

A1.2 Treatment Model with Uncertainty (H1)

In the main text we posit that implementing a strict 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:

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

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

We can eliminate the cases in which the Democratic candidate chooses a platform that is more conservative than the Republican's candidate ($s_D > s_R$), as such an outcome would never be incentive compatible given the candidates' policy preferences. We are therefore left with the cases in which $s_D < s_R$.

Differentiating the Democratic candidate's objective function with respect to s_D gives us:

$$\frac{s_R^2}{2} - \frac{3}{2}s_D^2 - s_D s_R.$$

Setting this equal to 0 and solving for s_D yields the Democratic candidate's optimal platform in terms of the Republican candidate's platform, s_R. We get two possible solutions:

$$s_D(s_R) = -s_R$$

$$s_D(s_R) = \frac{s_R}{3}.$$

Only the second solution makes sense for our purposes because $-s_R$ is outside of the interval $[0,1]$.

We go through the same process with the Republican candidate's objective function in the condition $s_D < s_R$. Differentiating this function with respect to s_R gives us:

$$2 + \frac{3s_R^2 - s_D^2}{2} - 4s_R + s_R s_D.$$

Setting this equal to 0 and solving for s_R gives us the Republican candidate's best response in terms of the Democratic candidate's platform, s_D. We again find two possible solutions:

$$s_R(s_D) = 2 - s_D$$

$$s_R(s_D) = \frac{s_D + 2}{3}.$$

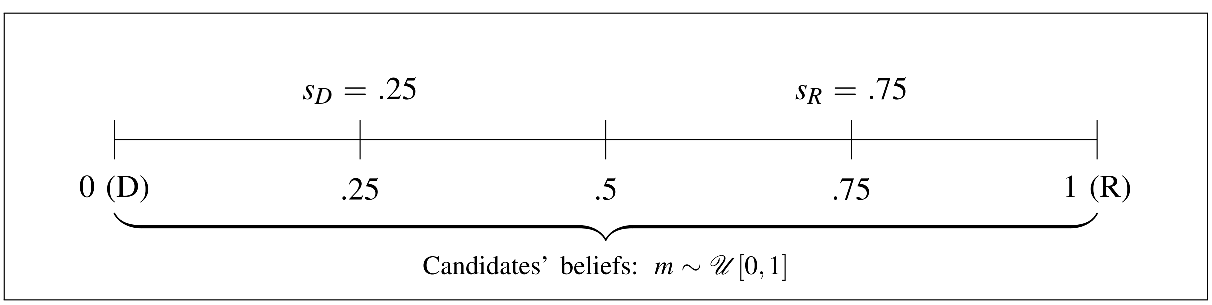
Given that only this second equation makes sense for our purposes (because $2 - s_D \geq 1 \ \forall$ possible values of s_D), we take $\frac{s_D+2}{3}$ as the optimal platform for the Republican candidate. The result is the following system of equations:

$$s_D^* = \frac{s_R^*}{3}$$

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

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 strict 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



To illustrate the logic of this equilibrium, we consider two possible ways that candidates could defect from this strategy ($s_D = .25$ and $s_R = .75$) and demonstrate why those defections would not constitute an equilibrium. We refer to the first category of defections as "symmetric defections," in which one candidate's platform movement will be mirrored by the other candidate, but the platforms will remain equidistant from the location of the median voter *in expectation*, which is .5. There are two subcategories of symmetric defections—polarizing defections, in which $s_D < .25$, $s_R > .75$, and $\frac{s_D+s_R}{2} = .5$; and converging defections, in which $.5 \geq s_D > .25$, $.5 \leq s_R < .75$, and $\frac{s_D+s_R}{2} = .5$. After showing why these defections do not constitute equilibria, we then move on to considering "asymmetric defections," in which candidates' moves are not constrained to be symmetric to their opponents' moves. We also demonstrate that these types of defections do not constitute equilibria.

A1.2.1 Symmetric Defections

There are two components of the candidates' expected utilities—the probability of winning (or losing) the election, and the policy utility that a win (or a loss) would yield. Symmetric defections from the equilibrium presented above ($s_D = .25$ and $s_R = .75$) would maintain the same win/loss probabilities (.5) because the candidates' platforms would remain equidistant from the location of the median voter in expectation. Therefore, only the policy utilities would be affected by a symmetric defection. At first blush, it may seem intuitive that the Democratic and Republican candidates, with ideal points of 0 and 1, respectively, would prefer to set platforms closer to these ideal points, especially if they could do so without hurting their win probability. This possibility is what we would call a polarizing defection, as $s_D < .25$ and $s_R > .75$ would constitute an increase in polarization relative to the equilibrium presented above. However, any symmetric move that would increase polarization (e.g., $s_D = .2$ and $s_R = .8$) provides candidates with lower utility in expectation because the policy cost for either candidate of potentially losing the election outweighs the marginal policy gain of potentially winning the election. Compare the utility of the Democratic candidate under the equilibrium presented above versus the symmetric defection scenario:

$$EU_{D(s_D=.25,\, s_R=.75)} = -(.5(.25)^2) - (.5(.75)^2) = -0.3125$$

$$EU_{D(s_D=.2,\, s_R=.8)} = -(.5(.2)^2) - (.5(.8)^2) = -0.34$$

$$EU_{D(s_D=.25,\, s_R=.75)} > EU_{D(s_D=.2,\, s_R=.8)}$$

It is clear, then, that polarizing symmetric defections would not constitute equilibria, as they make at least one of the candidates worse off in expectation. A *converging* symmetric defection does not seem intuitive on its face—why would candidates move their platforms away from their ideal points without increasing their win probability? However, it actually does appear to be utility-increasing for at least one candidate at first blush. Consider the following comparison:

$$EU_{D(s_D=.25,\, s_R=.75)} = -(.5(.25)^2) - (.5(.75)^2) = -0.3125$$

$$EU_{D(s_D=.3,\, s_R=.7)} = -(.5(.3)^2) - (.5(.7)^2) = -0.29$$

$$EU_{D(s_D=.25,\, s_R=.75)} < EU_{D(s_D=.3,\, s_R=.7)}$$

We know from this comparison that this particular converging symmetric defection would make the Democratic candidate better off, but the next question is whether the Republican candidate would comply with such a strategy. Positing that the Republican candidate would defect from this strategy, we move into the realm of asymmetric defections from the equilibrium presented above.

A1.2.2 Asymmetric Defections

Asymmetric defections from the equilibrium presented above ($s_D = .25$ and $s_R = .75$) change both the probability portion of the candidates' expected utility, and the policy portion. Any defection from the equilibrium by definition requires a change in at least one candidate's platform, and if defections are asymmetric around the expected location of the median voter, that automatically gives an advantage to one candidate in terms of win probability. Directly above we have demonstrated that the Democratic candidate would be made better off if both candidates symmetrically moved their platforms closer to the location of the median voter (.5) *in expectation*.[2] What we

[2]It is also the case that this symmetric move would make the Republican candidate better off relative to the equilibrium of $s_D = .25$ and $s_R = .75$. The candidates are stuck in a prisoners' dilemma

are checking here is whether or not it is rational for the Republican candidate to make this symmetric move, relative to some other strategy (for example, retaining the platform $s_R = .75$). That comparison is shown here:

$$EU_{R_{(s_D=.3,\, s_R=.7)}} = -(.5(1-.3)^2) - (.5(1-.7)^2) = -0.29$$

$$EU_{R_{(s_D=.3,\, s_R=.75)}} = -(.525(1-.3)^2) - (.475(1-.75)^2) = -0.2869375$$

$$EU_{R_{(s_D=.3,\, s_R=.7)}} < EU_{R_{(s_D=.3,\, s_R=.75)}}$$

Therefore, if the Democratic candidate moderated their platform, the Republican candidate would be better off retaining a platform of $s_R = .75$—even though this asymmetric strategy decreases the Republican candidate's win probability from to .5 to .475. Of course, if the Republican candidate will not moderate their platform symmetrically, it also does not make sense for the Democratic candidate to moderate:

$$EU_{D_{(s_D=.3,\, s_R=.75)}} = -(.525(.3)^2) - (.475(.75)^2) = -0.3146875$$

$$EU_{D_{(s_D=.25,\, s_R=.75)}} = -(.5(.25)^2) - (.5(.75)^2) = -0.3125$$

$$EU_{D_{(s_D=.3,\, s_R=.75)}} < EU_{D_{(s_D=.25,\, s_R=.75)}}$$

A1.2.3 Logic of the Equilibrium

The above exercise is meant to illustrate *why* uncertainty about the location of the median voter leads to a specific degree of polarization relative to the baseline model in which candidates are certain about the location of the median voter. Section A1.1 demonstrates that giving the candidates polarized policy preferences is not enough to generate a prediction of polarization in

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scenario where cooperating on a converging strategy would be utility-improving for both, but each has an incentive to defect, and therefore cannot credibly commit to the cooperative converging strategy.

equilibrium. This point stems from the fact that any candidate that unilaterally decides to run on a platform that is not equal to the known location of the median voter (m) will guarantee themselves a loss. There is no tradeoff that exists between win probability and policy utility in this model, because winning or losing is deterministic—except for the case in which $s_D = s_R = m$, in which each candidate has a .5 probability of winning. Because no candidate (within the constraints of this model) can possibly prefer a guaranteed loss to a .5 probability of winning, in equilibrium both candidates set their platforms equal to m rather than their own ideal points.

However, when there is uncertainty about the location of the median voter, winning or losing is always probabilistic rather than deterministic. As long as the candidates set platforms that are equidistant to the location of the median voter *in expectation*, each candidate will have a .5 probability of winning. It is trivial to show that candidates would prefer to move their platforms towards their ideal points if they could do so without decreasing their probability of winning. But, as demonstrated above, this logic only goes so far. Past a certain point ($s_D = .25$, $s_R = .75$), the potential utility loss from losing an election to an opponent with a sufficiently extreme platform outweighs the potential utility gain from setting a platform closer to a candidate's own ideal point.

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 present a novel 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.

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, Grofman, and Brunell (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

[3]Technically, our tether is a simplified version of Merrill, Grofman, and Brunell's (2014). Their

(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 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

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, Grofman, and Brunell'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 strict voter ID law.

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:

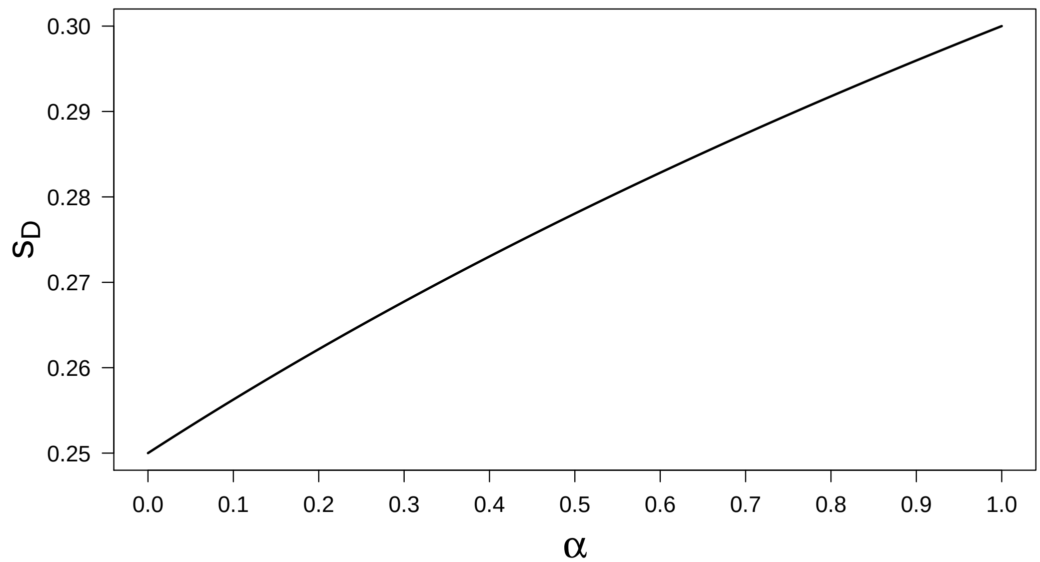
$$s_{R(\delta=1)}^* = t_R$$

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

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

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 was certain a tether was in place would not go all the way back to the median voter. Consider an extreme 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 strict voter ID law, state Republican parties believe the distribution of voters has shifted to the right. 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 would likely compel the Republicans to admit that the purpose of the law is electoral strategy—an attempt to deactivate Democratic voters and move the median voter to the right—rather than election security. 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 Background on States' Strict Voter ID Laws

Here we report additional information on strict voter ID laws in the states and our coding decisions.

A2.1 Implementation Histories

Table A1 provides brief summaries of states' strict 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 crux 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 strict 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 A1: Summaries of Strict 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 1, 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 1, 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 strict 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.

A2.2 Treatment Coding Notes

Table A2 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, Lajevardi, and Nielson (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 A2: 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
Georgia	Passed in 2005; Implemented in 2008 due to court challenges
Hawaii	ID required only if requested (non-photo)
Idaho	Photo ID required; Minor 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; 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; No major provisional ballot burden
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.

A3 Variable Summaries

Here we discuss the measurement and sources of the variables used in our analyses. Table A3 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 A6.2 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, Harden, and Boehmke (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 A3: 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	0.432
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	Strict voter ID law in force	NCSL	0.758	0.430
Adoption (full data)	Strict voter ID law adopted	NCSL	0.249	0.433
		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)	37.103	10.048
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	Conservatism of state's political leaders	Berry et al. (1998)	30.720	12.454
State citizen ideology	Conservatism of state's citizens	Berry et al. (1998)	42.515	12.187
		Additional Covariates		
Law severity	Indicator for both strict *and* photo requirements (see Section A6.1)	NCSL	0.320	0.468
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, Kreitzer, and Suo (2020)	0.670	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)	−0.249	1.231
Total bills introduced	Logged count of substantive (non-resolution) bills introduced in a legislative session	Council of State Governments (2018)	7.309	0.903
Neighbors adopting	Proportion of a state's geographic neighbors that are treated	NCSL	0.356	0.316
Sources adopting	Proportion of a state's policy sources that are treated	Desmarais, Harden, and Boehmke (2015)	0.399	0.253

Note: Cell entries report variable descriptions, data sources, and means and standard deviations (SD). See Section A6.2 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.

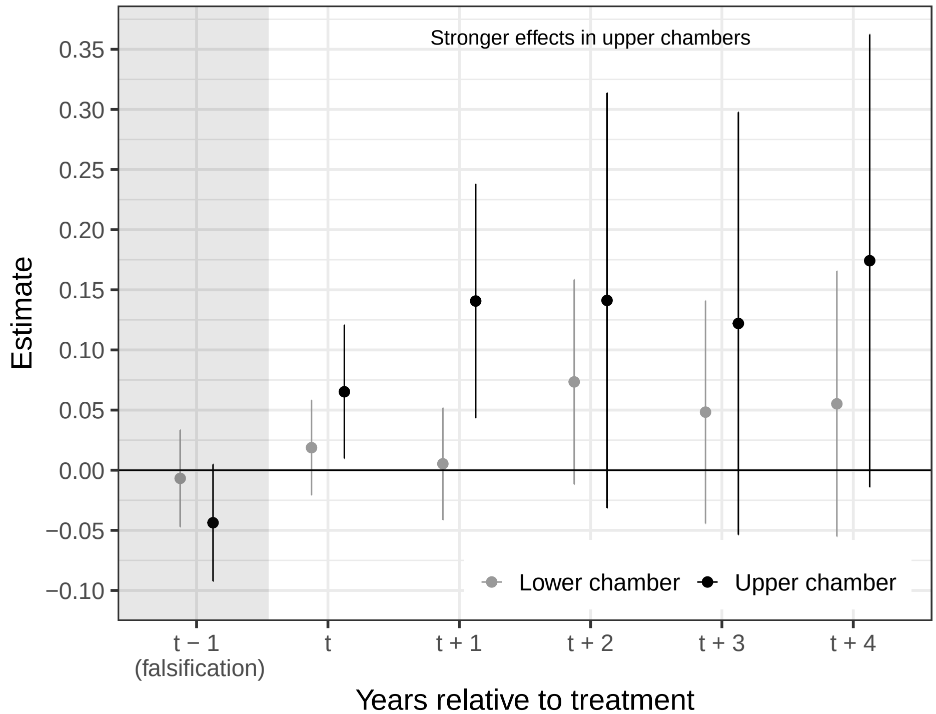
A4 Results by Chamber

In Figure A4 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 strict 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. 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 variation in polarization with these data (Shor and McCarty 2011). Upper chambers are smaller

Figure A4: Estimated Effects of Strict 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 strict 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.

than lower chambers, and thus the replacement of one member constitutes a larger change to the ideological distribution of the chamber.

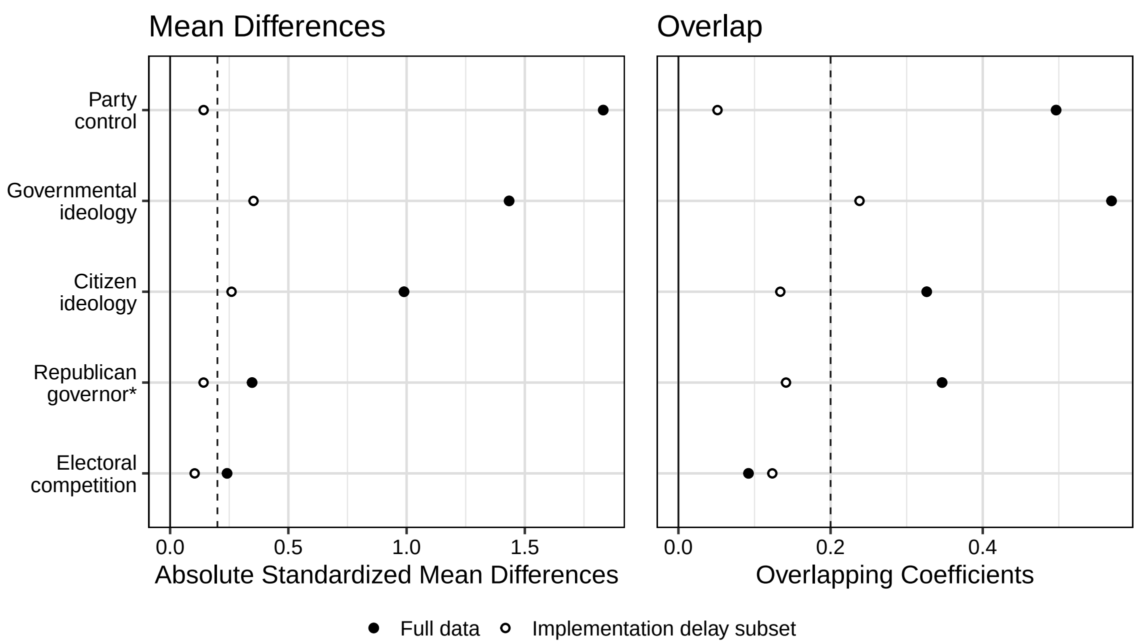
A5 Covariate Balance

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 strict voter ID laws and (2) our use of Panel-Match. 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 strict 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 A5 graphs two quantities: absolute standardized mean differences (left panel) and overlapping coefficients (right panel) in both samples.[5] 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, Rassen, Ackermann, Bartels, and Schneeweiss 2014). We set a standard target threshold of 0.20 to denote good balance in a covariate (Greifer 2021*a*).

Figure A5: 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 graphs show substantial balance improvement for several key variables. Party control of the legislature moves from a mean difference of nearly two standard deviations to about 14% of a standard deviation. Governmental ideology and citizen ideology fall just outside the threshold of 0.20, but both of those variables demonstrate major imbalance reductions as well. The indicator for

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

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 but one of the overlapping coefficients fall below our threshold of 0.20, with governmental ideology missing it slightly. And in all but one case the quantity decreases (indicating more overlap between groups) from the full sample to the subset. The only exception is electoral competition, which is almost the same 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, Kim, and Wang 2022). Here, we set the method to look two years pre-treatment to create matched sets. As Table A4 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 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).[6] Table

[6]PanelMatch can also apply weights to the entire matched set using CBPS. This method pro-

A4 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.

Table A4: 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	

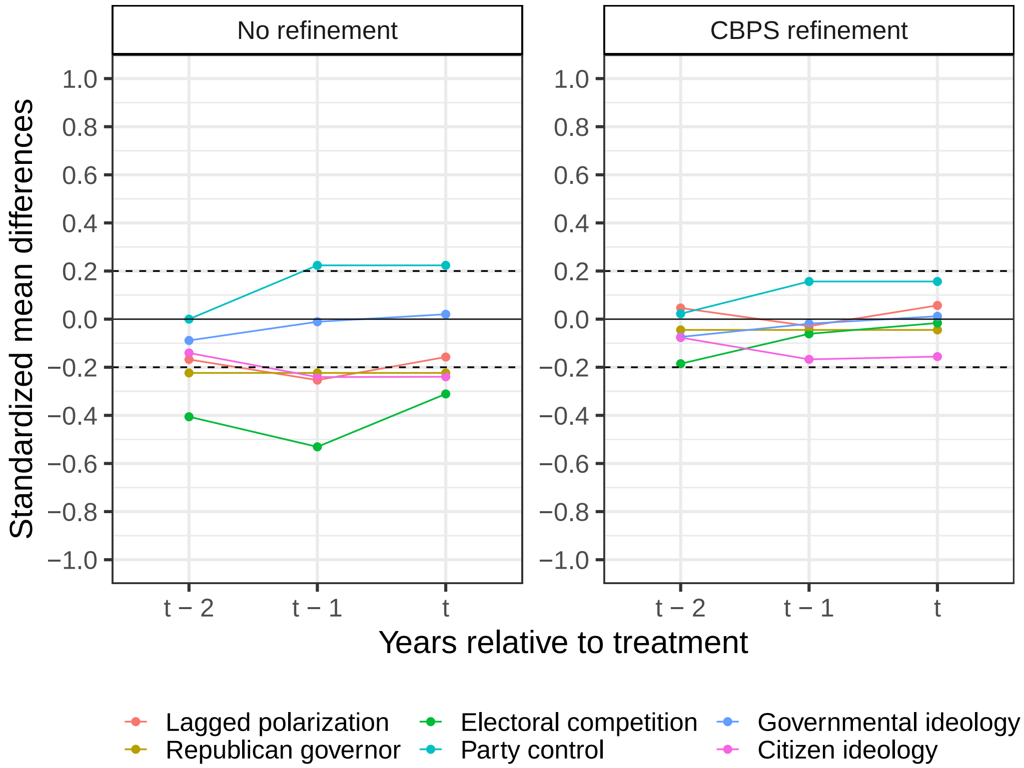
Note: Cell entries report the matched sets constructed by PanelMatch using a treatment history of two years and the refinement set within each matched set.

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 A6 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, Kim, and Wang (2022). Recall the threshold of 0.20 that we set previously, which is commonly used (Greifer 2021*a*). 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 for a given treated state.

Figure A6 shows good balance in the unrefined matched sets. Most of the covariates' mean

duces substantively identical results, but does not improve balance quite as much as matching with CBPS.

Figure A6: Covariate Balance in the Matched Sets



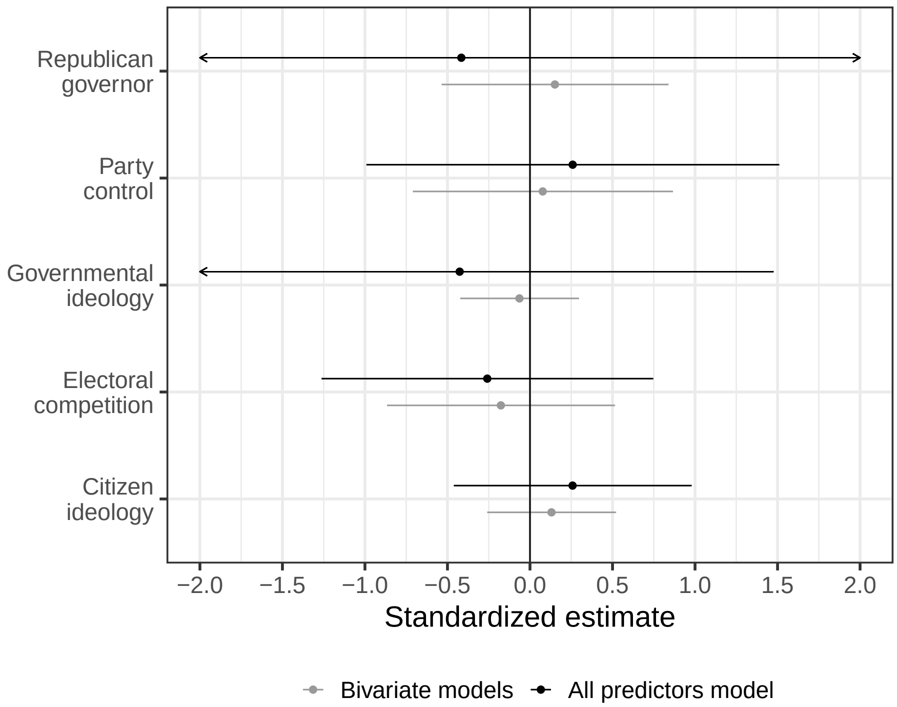
Note: The graphs present standardized mean differences in the covariates over time after generating matched sets with PanelMatch. The left panel gives results using all control states in a matched set and the right panel shows results after refining the matched sets with CBPS.

differences are within or near the $\pm$ 0.20 threshold, with electoral competition falling the furthest away. However, when we move to the refined matched sets, balance improves even more. All of the mean differences are less than 0.20 in absolute value, with lagged polarization showing especially small values. 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 strict voter ID law was more or less decided by a coin flip.

A5.3 Determinants of Delay Time

We argue that the time between passage of a strict 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.[7] Given this small sample, we estimate bivariate models with each predictor and a multiple regression model with all predictors. We also compute 95% confidence intervals with a nonparametric bootstrap rather than rely on the model-based estimates of uncertainty. The results appear as standardized coefficient estimates in Figure A7.[8]

Figure A7: Determinants of Time Between Passage and Implementation of Strict Voter ID Laws



Note: The graph presents standardized linear regression estimates and bootstrapped 95% confidence intervals from models of the number of years between passage and implementation of a strict voter ID law. Gray indicates estimates from separate bivariate regressions with each variable as the only predictor variable. Estimates in black come from one model with all predictors. N = 21.

The graph indicates that none of the covariates are strongly associated with the delay time between strict voter ID law enactment and implementation. All of the estimates are smaller than 0.50 in absolute value, indicating that a standard deviation increase in a predictor (or one-unit change in the case of the Republican governor variable) corresponds with less than one-half of a

[7]Delay time ranges from zero to seven with a mean of 2.24, a median of 2, and a standard deviation of 2.07.

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

standard deviation shift in delay time. In the bivariate regressions, which are much more efficiently estimated than the multiple regression model, the coefficients are all smaller than one-fourth of a standard deviation of the outcome in absolute value. None of the confidence intervals are bounded away from zero, although that finding is partially due to our small sample size.[9] We interpret these results as another piece of evidence in favor of our identification strategy.

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 A4 above). 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 above).

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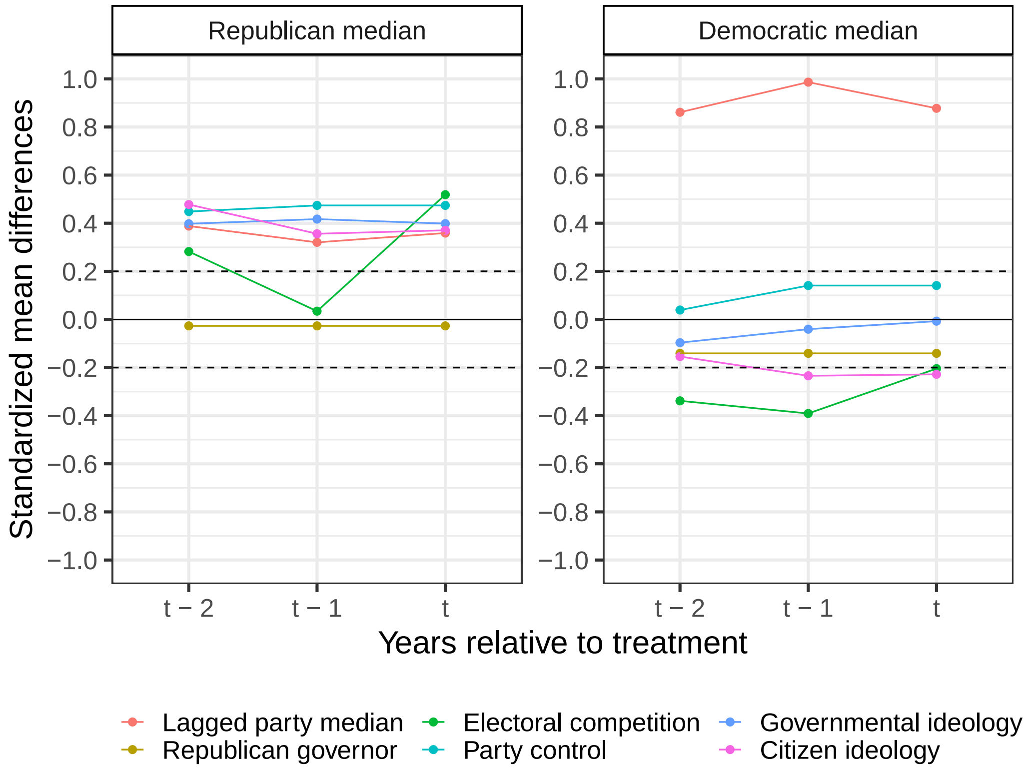
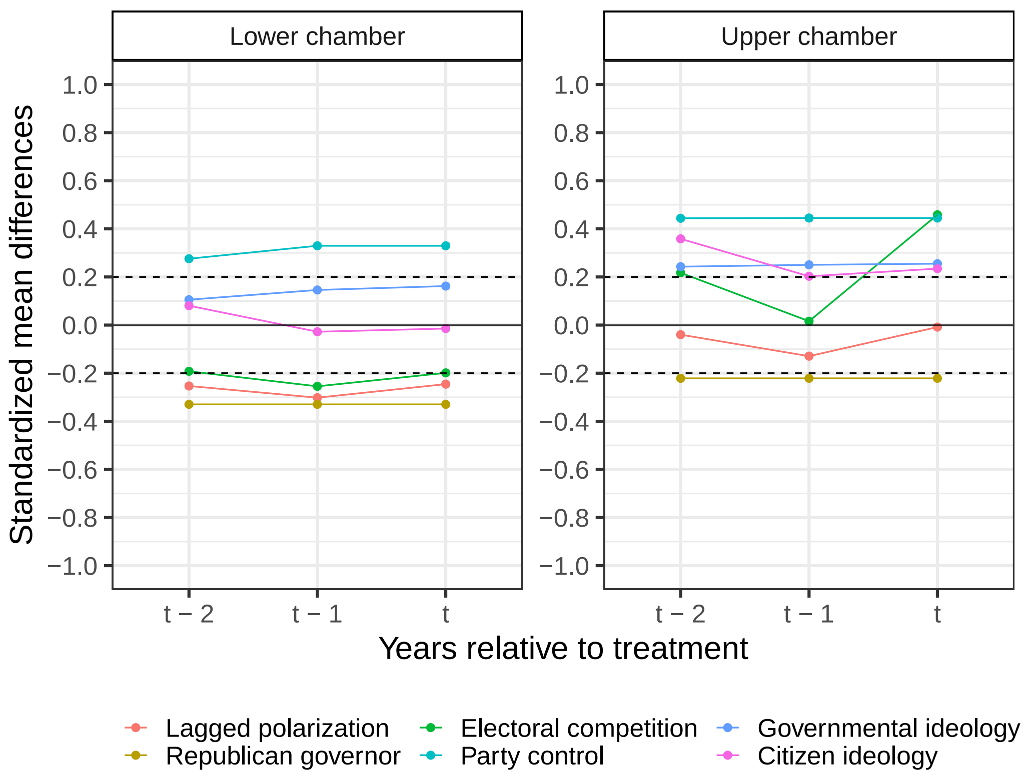
[9]Ideally, we would assess these estimates in the framework of an equivalence test, which could provide stronger evidence that the effect of a predictor on this outcome is negligible (Hartman and Hidalgo 2018). However, these data do not provide sufficient power for such tests, so we are limited to simply focusing on the coefficient estimate magnitudes. We contend that doing so is still informative despite the limitation.

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

(a) Party median ideal points



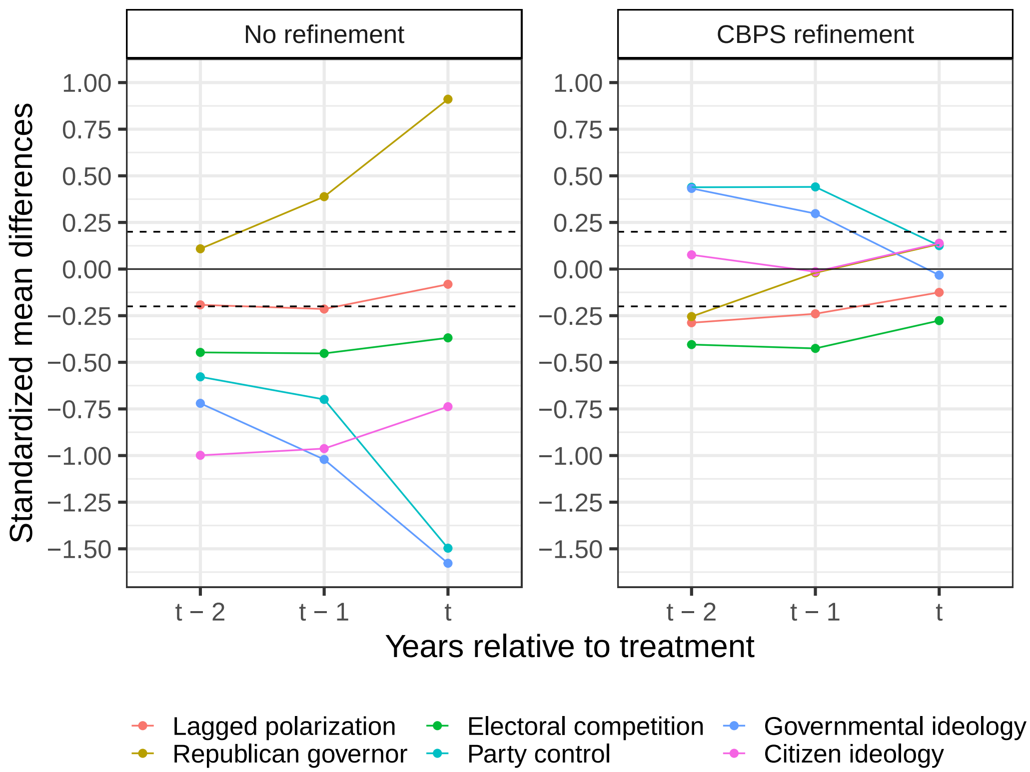
(b) Polarization by chamber



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 strict voter ID law *adoption* (the placebo test in the main text). The graphs show that balance improvement is quite poor without covariate refinement. However, it is quite strong after matching refinement with CBPS; the differences are within or close to $\pm$ 0.20. 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 strict 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 strict voter ID laws on polarization. First, we consider the possibility of heterogeneity in the laws themselves. Then we assess results after controlling for additional covariates.

A6.1 Law Severity

A potential threat to our design might emerge if the most severe voter ID laws took the longest to wind their way through the judicial system. In such a case, law severity might be correlated with the initial adoption decision and our outcomes. To consider this possibility, we grouped the states in our subset according to whether their initial laws included (1) both the NCSL's strict definition *and* a photo requirement (more severe) or (2) the NCSL's strict definition *or* a photo requirement (less severe).[10] The first group's average delay is two years with a standard deviation of 1.35. The less severe group's average delay is 1.13 years with a standard deviation of 1.13. Thus, more severe laws—those that combine both strict enforcement with a photo requirement—spend, on average, 10–11 more months in the judicial system compared to those with just one component.

This finding casts some doubt on the first part of our identification strategy. Thus, it is important to investigate whether our findings our robust to directly controlling for this law severity measure. We re-estimated the treatment effects with PanelMatch and included an indicator for the more severe category described above as a covariate.[11] Figure A10 graphs the implications for covariate balance as well as the treatment effects.

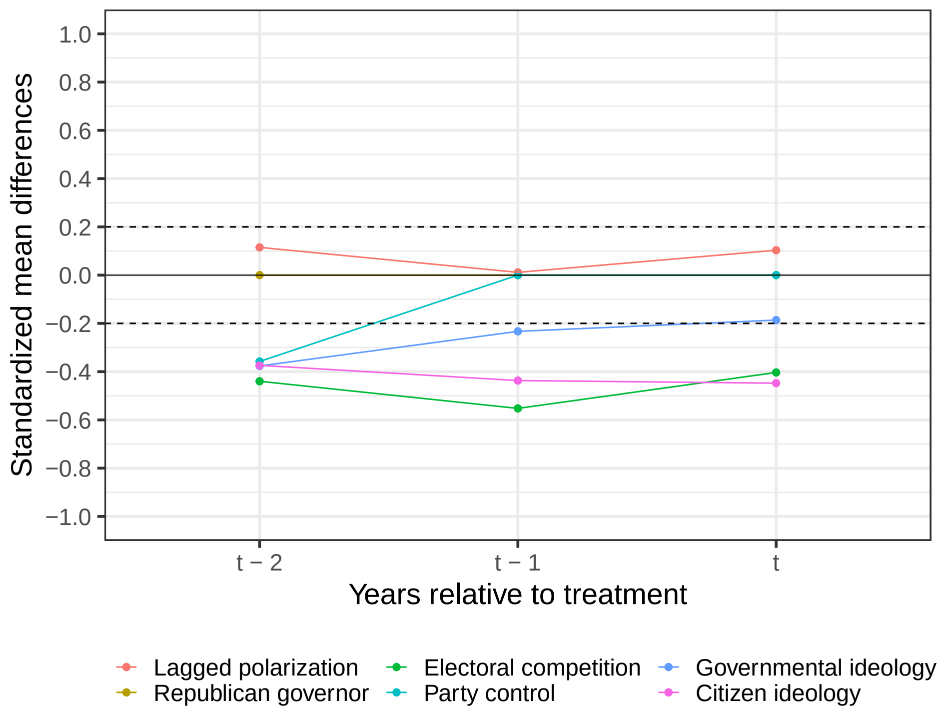
Panel (a) of Figure A10 indicates that balance weakens slightly on the other covariates after this addition. Thus, we should interpret these results somewhat more cautiously than the main set. Nonetheless, panel (b) shows that the point estimates are almost identical to the original set in the main text and the confidence intervals are quite similar as well. In short, this analysis indicates that our substantive conclusions are robust to variation in the severity of strict voter ID laws.

[10]We use the initial law only because some states either proposed or passed new laws in response to legal challenges to the originals.

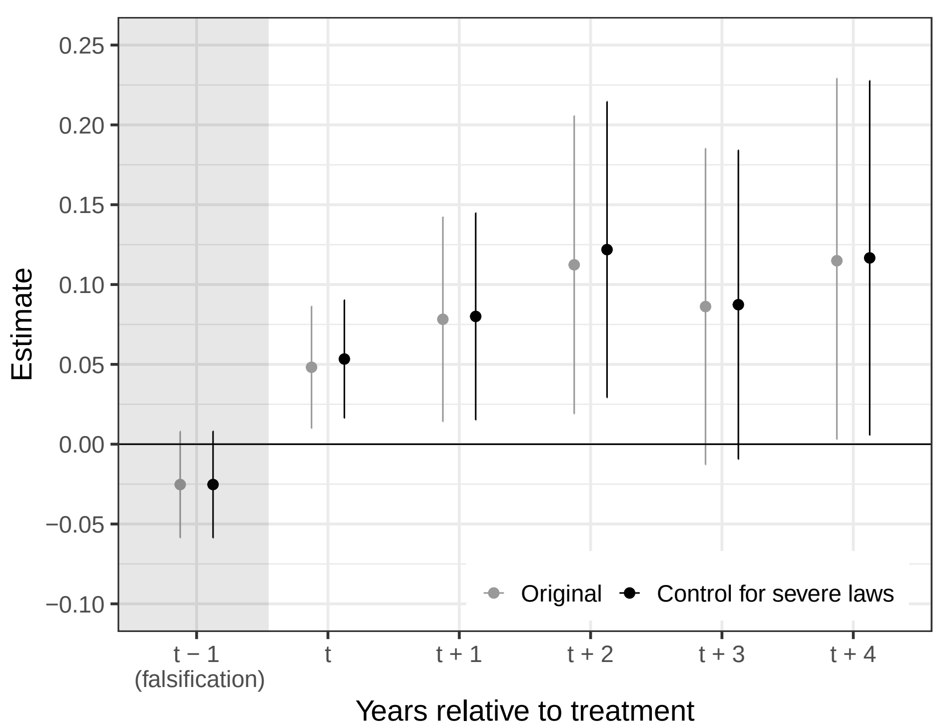
[11]This covariate is pre-treatment because the severity of the law is set before the law is implemented.

Figure A10: PanelMatch Results After Controlling for Law Severity

(a) Covariate balance



(b) Treatment effects



Note: The graphs present covariate balance (panel a) and treatment effect estimates on the difference in party median ideal points (panel b) after including a covariate measuring law severity. N = 194.

A6.2 Additional Covariates

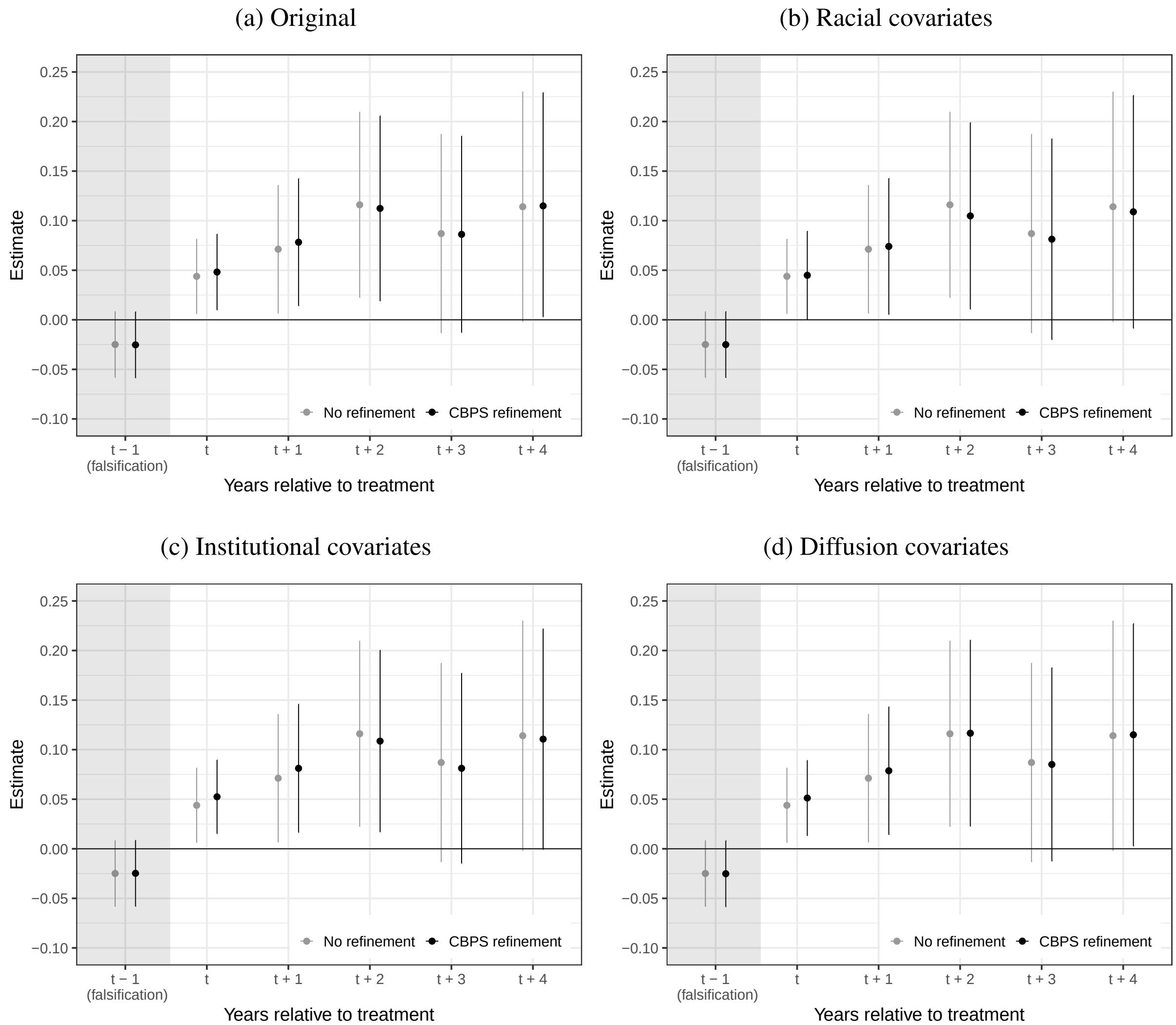
Here we present results with several other covariates that may plausibly correlate with both treatment status and the outcome, biasing our estimated treatment effects. Adding covariates is challenging in a small sample because the matching procedure demands more of a fixed amount of data. Thus, balance 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. The variables 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 strict voter ID law, (2) Percentage of source states adopting a strict voter ID law.

The racial variables capture the possibility that Republican lawmakers' motivation to enact strict 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, Kreitzer, and Suo (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 strict 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, Harden, and Boehmke 2015).

Figure A11: Estimated Effects with Additional 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.

Figure A11 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 results. Thus, we find that our conclusions are robust to these additional covariates.

A6.3 Separate Estimates for Strict and Photo Laws

In the main text we define strict 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 some action after Election Day to officially count their votes;
- The primary acceptable identification documents are those with a photo of the voter.

We contend that this definition best connects to our theoretical framework because either of these conditions could deter voters, thus 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 A12 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).

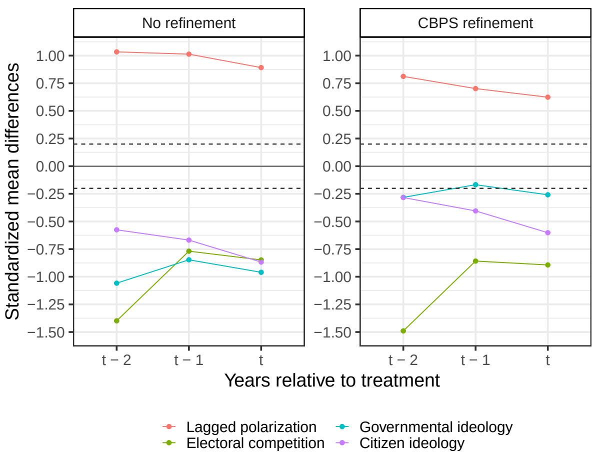
The first point to note is that separating these two conditions holds empirical consequences. We are demanding more from the same data and have less treatment variation in each analysis. The result is a weakening of covariate balance and statistical power. In the top panels, balance is suboptimal and does not improve much with CBPS refinement. In fact, in the case of strict laws the partisan control 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 do show positive effects, as we find in our main results. Thus, there is a baseline level of robustness from these analyses. Nonetheless, there are two points to highlight. First, the confidence intervals are quite wide and do not permit us to statistically distinguish the estimated effects from zero. Second, 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 (though still positive).

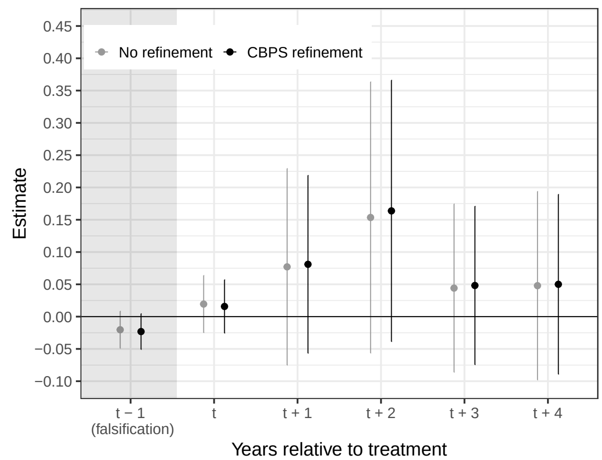
Overall, these analyses highlight an empirical case for bundling the strict and photo conditions into one treatment. Doing so offers substantial covariate balance and statistical power improvements. However, they also show that the general pattern we report in this research still emerges from separate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

Figure A12: Estimated Effects with Strict Laws and Photo Laws

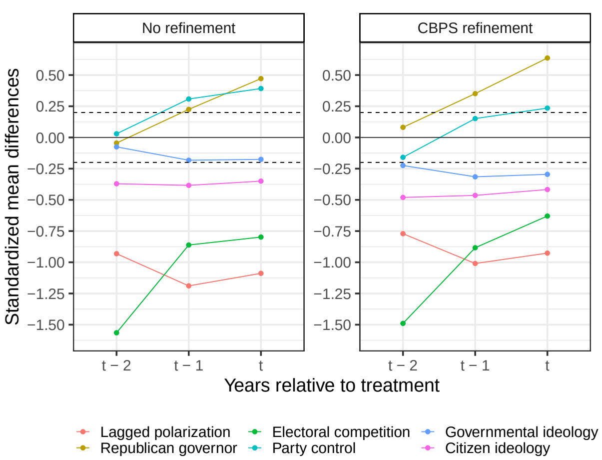
(a) Balance: Strict laws



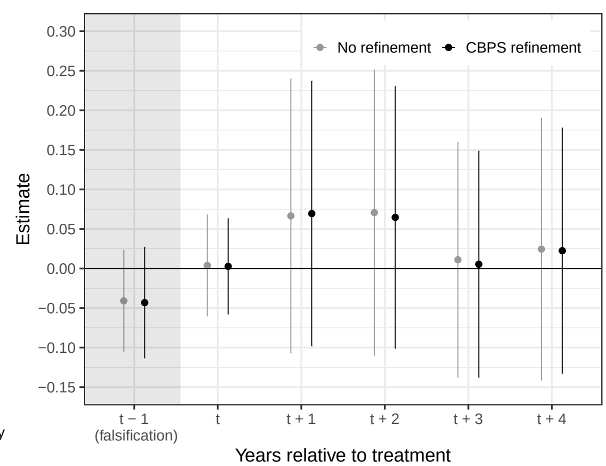
(b) Estimates: Strict laws



(c) Balance: Photo-only laws



(d) Estimates: Photo-only 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.

A7 Weighting

Our main estimation strategy involves "pruning" the data to identify the effects of strict voter ID laws. Here we employ weighting as an alternative approach.[12] 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.[13] We also use this estimation strategy to consider the possibility of temporal heterogeneity in our treatment effect.

We utilize Huling and Mak's (2020) 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 2020), 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*).[14] 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.

[12]We focus only on an alternative test of H1 here to save space. Analogous tests of H2 produce similar results (see the replication materials).

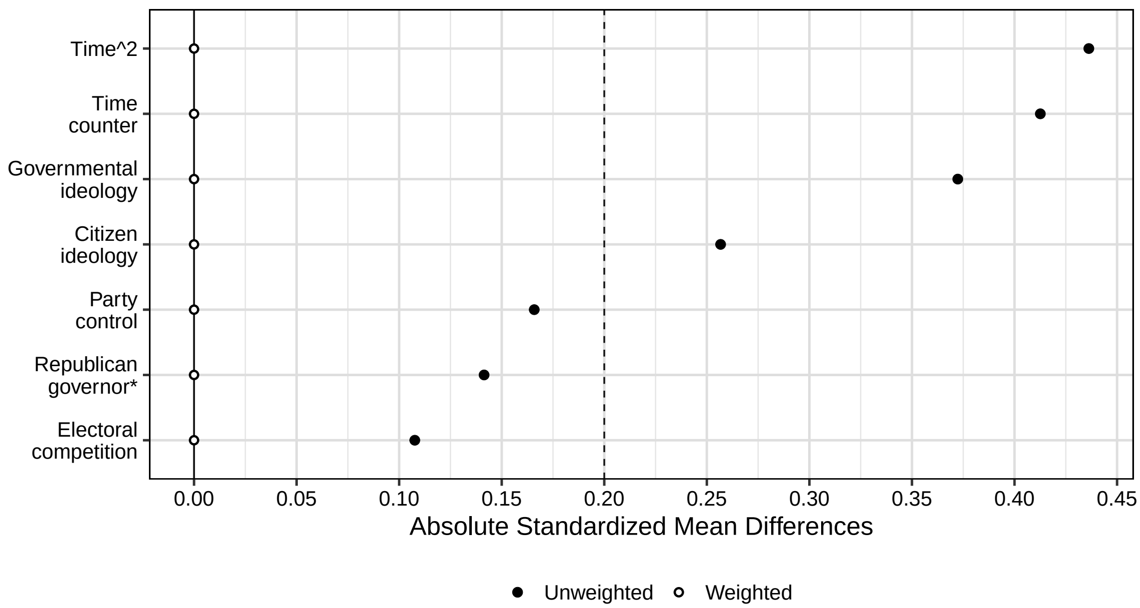
[13]We still restrict the sample to only the implementation delay subset, as described in the main text. However, results are substantively similar if we use the full sample of data.

[14]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).

A7.1 Balance

We begin by assessing our weighting procedure's effects on covariate balance.[15] Figure A13 reports absolute standardized mean differences between the treated and control cases before and after weighting. Imbalance in the unweighted data is not extremely severe; indeed, as we show in Figure A6, the subsetting process significantly improves balance. However, while there are three covariates within the threshold of 0.20, 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.[16]

Figure A13: Covariate Balance After Weighting with Energy Balancing



Note: The graph presents absolute standardized mean differences before and after weighting with Huling and Mak's (2020) energy balancing algorithm.

[15]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 A6.2 above.

[16]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*).

A7.2 Estimated Effects

Table A5 reports regression estimates of strict 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).[17] 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 A5: Treatment Effect Estimates with Energy Balancing Weights

	2003–2018		2003–2010		2011–2018	
Estimate	0.087	0.101	0.186	0.212	0.071	0.051
SE	0.028	0.042	0.094	0.110	0.021	0.034
95% Lower	0.031	0.008	−0.012	−0.034	0.030	−0.021
95% Upper	0.142	0.193	0.383	0.458	0.111	0.123
Weights		✓		✓		✓
State Fixed Effects	✓	✓	✓	✓	✓	✓
N	194	194	35	35	159	159
Adjusted R^2	0.92	0.95	0.90	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. The largest effects appear for laws implemented during 2003–2010, although our statistical power is lower in those models. The effects of later laws (post-2010) are still positive, but smaller in magnitude. In short, this analysis shows the robustness of our findings with a different estimation strategy and provides suggestive evidence that earlier laws may have been more polarizing than later strict voter ID requirements.

[17] Results are substantively unchanged if we include the covariates described in the main text in the modeling stage.

A8 Mechanism Tests

Here we report additional information about the test of the mechanism presented in the main text as well as a second mechanism 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. 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.[18] Table A6 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.[19]

[18]We omit states that adopted these policies prior to 2003 from the analyses. Including them does not change our substantive conclusions (see the replication materials).

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

Table A6: 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).

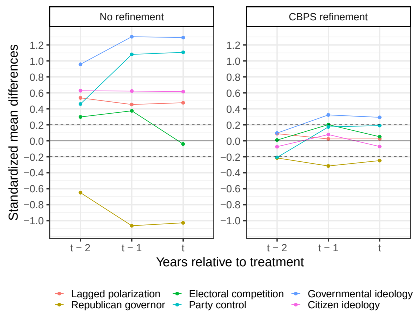
A8.1.2 Covariate Balance

Figure A14 summarizes covariate balance for the analyses presented in the main text mechanism test (Figure 5). 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 strict 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 5.

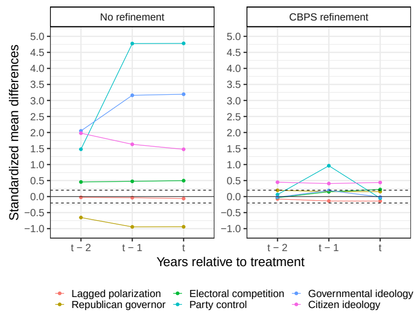
Most importantly, the balance after CBPS refinement is quite strong. The right panels in Figure A14 show that the standardized mean differences between treated and untreated cases with respect to lagged polarization, electoral competition, partisan control of the executive and legislative branches, and state ideology are consistently within or near the threshold of 0.20. Thus, despite the fact that we are unable to leverage plausibly exogenous variation due to legal delays, Figure A14 gives us some confidence that the results we report in Figure 5 are not spurious associations

Figure A14: Covariate Balance in the Matched Sets with Other Policies

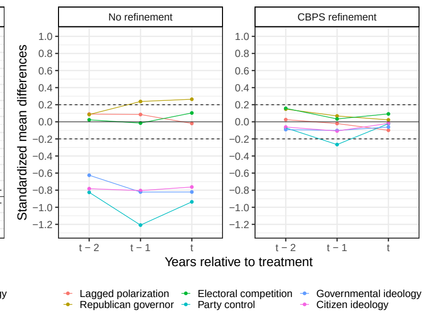
(a) In-state tuition (N = 736)



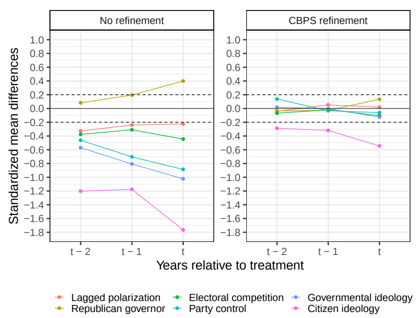
(b) Same-sex marriage (N = 768)



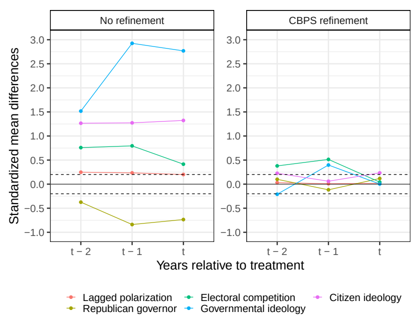
(c) Ultrasound requirements (N = 704)



(d) Stand your ground (N = 768)



(e) Public accommodations (N = 752)



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.

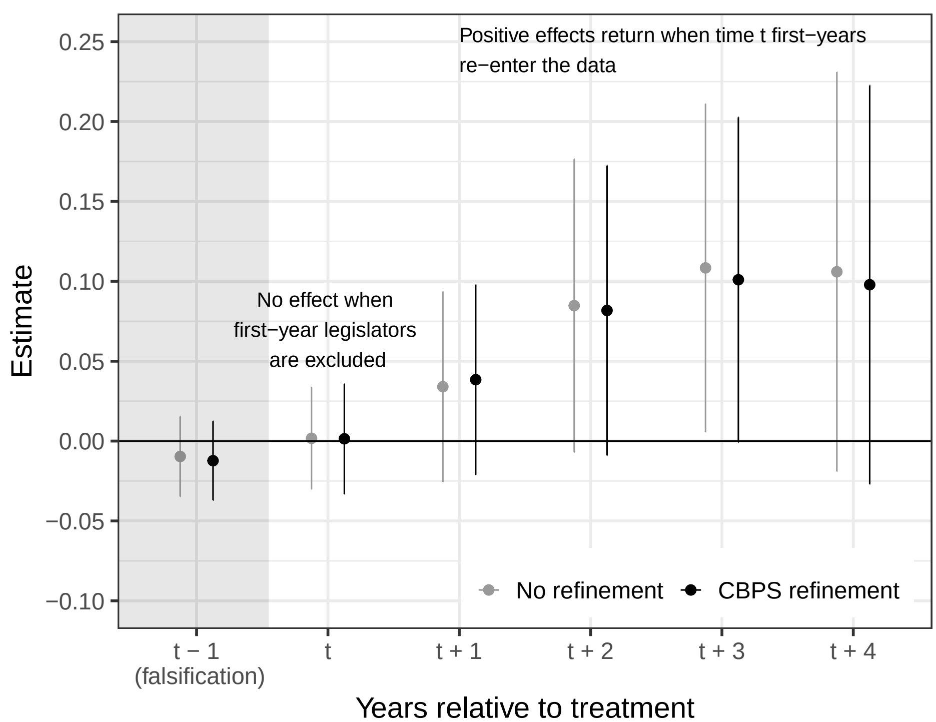
driven by the political factors that lead states to adopt these policies.

A8.2 Removing First-year Legislators

Our proposed theoretical mechanism is that strict voter ID laws alter legislative candidates' uncertainty about the median voter's location, which moves their campaign platforms away from center. While we cannot measure their perceptions directly, 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. 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 A15 reports PanelMatch results with this alternative version of the outcome.

Figure A15: Estimated Effects of Strict Voter ID Laws' Implementation on Party Polarization in State Legislatures without First-year Members, 2003–2018

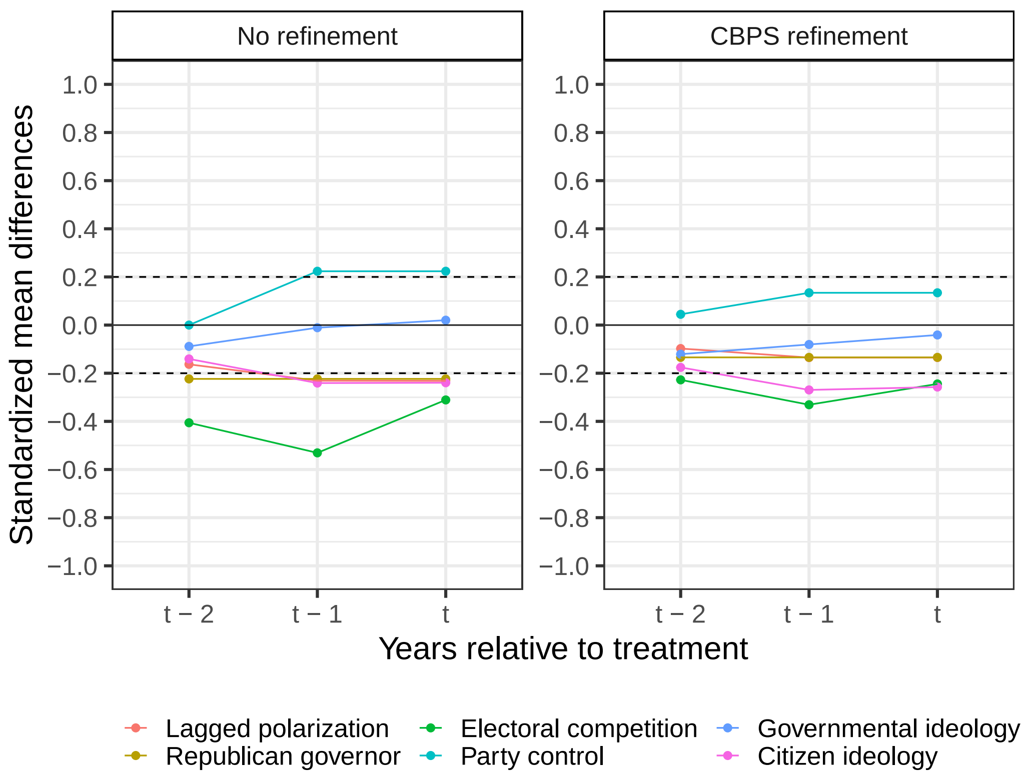


Note: The graph displays years relative to treatment (x-axis) against the estimated effects of strict voter ID laws going in force on the difference in party median ideal points in state legislatures with first-year members omitted (y-axis). Line segments indicate 95% confidence intervals. N = 194.

The falsification test again indicates no anticipatory effect, providing support for the research design. Most importantly, the estimated effect at time *t* in Figure A15 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 (Figure 2). In other words, when first-year lawmakers are excluded (time t), strict voter ID laws appear to exert no effect on party 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 A16 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 A16: 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.

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 A3 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, Kim, and Wang 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.[20] 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 A7 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.

[20]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 A7: Estimated Effects of Strict 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)	0.125 (0.104)	0.101 (0.046)	0.127 (0.033)
Election year		−0.005 (0.057)		(0.071) (0.056)
Divided government		0.041 (0.205)		0.054 (0.042)
Session end vs. start of FY		−0.429 (1.521)		0.036 (0.037)
Personal income		0.018 (0.039)		0.045 (0.038)
Budget size		−0.030 (0.086)		0.133 (0.062)
Surplus		−0.024 (0.032)		0.009 (0.033)
Legislative salary		−0.070 (0.081)		0.003 (0.096)
Republican governor		−0.066 (0.248)		0.026 (0.076)
Electoral competition		0.001 (0.004)		0.001 (0.002)
Legislative party control		0.107 (0.108)		0.044 (0.021)
Governmental ideology		0.002 (0.006)		−0.001 (0.004)
Citizen ideology		−0.001 (0.004)		−0.0002 (0.003)
State Fixed Effects	✓	✓	✓	✓
Year Fixed Effects			✓	✓
Time Counter	✓	✓		
Adjusted R^2	0.105	0.130	0.281	0.297
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.

A10 Multiple Imputation Diagnostics

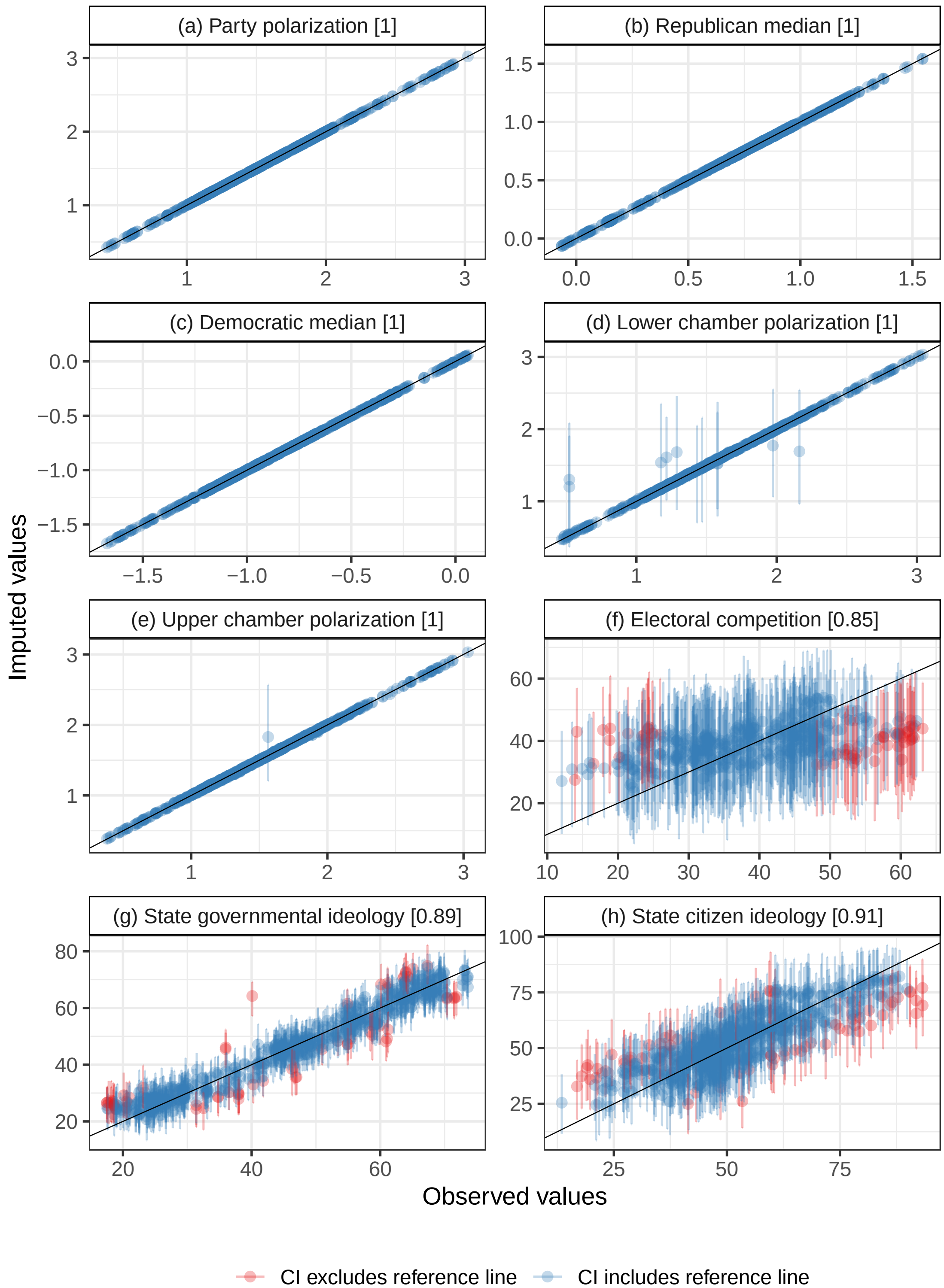
Our data include some missingness. We used multiple imputation with Amelia II (Honaker, King, and Blackwell 2011) to fill in missing values, producing five complete datasets for each outcome. The estimates we report throughout this research combine analyses with all five complete datasets and include appropriate adjustments to measures of uncertainty (Blackwell, Honaker, and King 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 A17 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 A17 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 A17: Overimputation Results

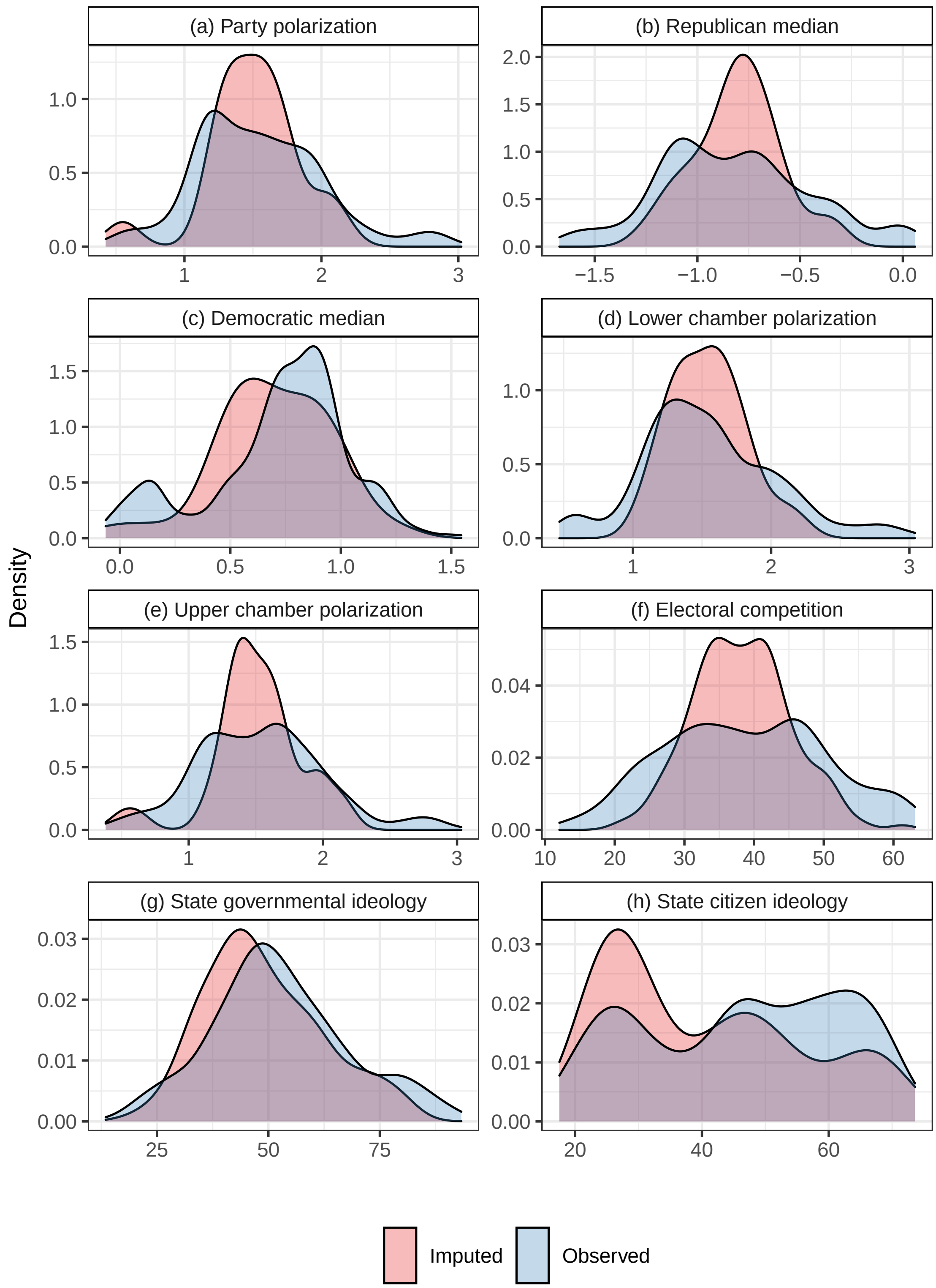


Note: The graphs present observed values of each variable on the x-axes against mean imputations of those values on the y-axes. Line segments indicate 95% confidence intervals. The solid line serves as a reference point for perfect imputation. The values in square brackets next to each variable label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

A10.2 Density Plots

Figure A18 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 A18: Observed and Imputed Densities



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

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