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POLITICAL EFFECTS OF RIGHT TO WORK LAWS

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ABSTRACT

Labor unions directly affect wages, employment, industrial structure, and inequality. But unions also influence the economy and labor market indirectly through their effects on politics, providing candidates with voters, volunteers, and contributions, and lobbying on public policy. We use the enactment of right-to-work laws---which weaken unions by removing agency shop protections---to estimate the effect of unions on politics and policy from 1980-2016. Comparing counties on either side of a state and right-to-work border to causally identify the effects of the state laws, we find that right-to-work laws reduce Democratic Presidential vote shares by 3.5 percentage points. We find similar effects in Senate, House, and Gubernatorial races, as well as on state legislative control. Turnout is also 2 percentage points lower in right-to-work counties after passage. Exploring the mechanisms, we find that right-to-work laws dampen organized labor contributions to Democrats and that potential Democratic voters are less likely to be contacted to vote. The weakening of unions also has large downstream effects: fewer working-class candidates serve in state legislatures and Congress, while state policy moves in a more conservative direction.

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1 Introduction

Freeman and Medoff (1984) famously asked, "What do labor unions do?" Scholars have largely focused on the direct effects of unions on labor market outcomes such as wages, employment, industrial structure, firm survival, and inequality.[1] But failing to account for the indirect effects of unions on markets through their political activities results in an incomplete understanding of the union effect. As United Auto Workers President Walter Reuther put it in 1970, "There's a direct relationship between the ballot box and the bread box, and what the union fights for and wins at the bargaining table can be taken away in the legislative halls."[2] In a similar vein, Clothing Workers President Sidney Hillman made a direct appeal to his members in 1936: "Now you may come and ask me: why should we be concerned about the law? Why shouldn't we rely on our own power? ... I say to you that if labor is to make real progress, labor must have some legislative support ... Labor must learn to use its economic and political power" (Eidlin 2018, p. 226).

As Reuther and Hillman both suggested, unions do not limit their activities to collective bargaining for their members; they also attempt to shape broader economic outcomes through policy and politics (Chang 2001; Rosenfeld 2014; Schlozman 2015). In a chapter in Freeman and Medoff (1984), the authors consider the political effects of unions and conclude that organized labor in the US has had a mixed record, but that unions have succeeded in pushing policies that "benefit workers as a whole rather than unionists alone (p. 22)." In recent decades, unions appear to have shifted even more resources into politics, especially since changes in public policy have been a major contributor to declines in U.S. unionization rates (Farber 2005; Hacker and Pierson 2010; Lichtenstein 2013; Rosenfeld 2014). With union bargaining power in decline, Dark (1999) suggests that "what cannot be won in the economic market can, perhaps, be won in the political market." The political process may determine whether the labor movement survives.

One of the primary means through which American labor unions have sought to shape policy is by forging an "enduring alliance" with the Democratic party (Dark 1999). That relationship has been well-documented in both the academic and popular press, and involves unions donating to Democratic candidates, launching grassroots mobilization in support of those candidates, shaping local, state, and national party platforms, and lobbying legislatures to pass pro-labor policies (for reviews, see Ahlquist 2017; Dark 1999; Greenstone 1969; Rosenfeld 2014). As a result of this longstanding relationship, we should expect that stronger labor unions would result in stronger Democratic electoral prospects and more liberal policies. Figure 1 illustrates bivariate support for both of these predictions, revealing a positive correlation between state-level union membership and Democratic Presidential vote shares from 1980 to 2016 (1a), as well as a very strong positive correlation between union membership and a summary measure of state policy ideological liberalism

[1] Freeman and Medoff (1984) and Blanchflower and Bryson (2004) review the literature on the effects of unions on wages and labor markets, yielding some answers to Freeman's question. Work exploiting close NLRB certification votes finds little evidence of a causal union wage premium or a union effect on employment (Dinardo and Lee 2004) or on firm stock market performance (Lee and Mas 2012). Unions may play a role in determining the distribution of wages, both within unionized firms and across the economy (Card 2001; Frandsen 2012; Wilmers 2017; Farber et al. 2018), possibly through institutional equity norms (Western and Rosenfeld 2011).

[2] Proceedings of the Constitutional Convention of the United Auto Workers, Vol. 22 (1970).

(1b).[3] At the individual level, Freeman (2003) finds a positive correlation between union membership and voter turnout, though this relationship is driven in part by selection into unionization and demographics (see also Leighley and Nagler 2007; Rosenfeld 2014).

[Figure 1 about here.]

The time series is also suggestive: as the share of private-sector workers represented by labor unions has declined (Farber and Western 2000; Hirsch et al. 2001), the electoral and political center has shifted rightward (Hacker and Pierson 2005, 2010; Mann and Ornstein 2012; McCarty et al. 2006). Still, these relationships do not provide evidence of the *causal* effect of unions on the Democratic party's strength or on the types of policies passed by state governments. The question remains whether U.S. states are more Democratic and liberal because they have strong unions and many union members, or whether those states have stronger unions because Democrats enact more union-friendly policies.[4]

In this paper, we tackle these questions, estimating the causal effects of union strength on Democratic political power and the direction of public policy. To estimate the effect of unions on politics and policy, we take advantage of the enactment of state-level "right-to-work" (RTW) laws, which directly affect the organizational clout of labor unions, and compare Democratic vote share and voter turnout in neighboring counties across RTW state borders from 1980 to 2016. The neighboring counties are politically, demographically, and economically similar before RTW laws pass, and we interpret differential changes in political outcomes in one neighbor county compared to another after RTW passes as the effect of RTW—and more generally of weaker unions—on political outcomes. We focus on these border counties as they are "policy-takers," treated by state-level laws but not necessarily driving policy-making in the state capital.

In 1947, twelve years after the passage of the National Labor Relations Act that recognized the right of private-sector unions to bargain collectively, Congress passed the Taft-Hartley Act, which greatly curtailed newly-established union rights.[5] In one of Taft Hartley's most important provisions, Congress granted states the ability to pass so-called right-to-work laws, which permit workers in a unionized business to opt out of paying dues to the union, even if those workers reap the benefits of collective bargaining and union representation.[6] After Congress approved Taft-Hartley,

[3]These estimates of state-year policy liberalism come from dynamic latent-variable models applied to data on 148 state policies—similar to the well-known DW-NOMINATE scores for Members of Congress (Caughey and Warshaw 2016). We find similar results using Grumbach (2017)'s alternative state policy score approach.

[4]Beland and Unel (2015), for instance, find no effect of Democratic governors on the change in unionization rates or union wages, exploiting close elections where Democratic candidates narrowly defeated their Republican opponents.

[5]Public sector unionization for federal government employees began in 1962; state and local employees, who make up the vast majority of public sector workers, are subject to state-level laws governing their organizing, bargaining, and dues collection rights that often vary by occupation and level of government.

[6]Proponents of RTW laws argue that workers should not be compelled to contribute to labor organizations they might oppose—and, more strategically, conservative activists have backed RTW measures as a means of weakening the strength of organized labor in general. Opponents of RTW measures argue that such laws permit free-riding, allowing workers to reap the benefits of a union (including collective bargaining and grievance protections) without supporting the union financially.

a number of state legislatures, largely in the South, quickly passed laws instituting RTW.[7] Figure 2 summarizes the states with RTW laws in the United States as of 2016 and the years in which they were enacted. As we discuss in detail later, RTW laws are all relatively comparable to one another in legal "bite" and substantive impact across states and have generally been interpreted by courts to apply with equal force to public and private sector unions alike.[8] That said, we are agnostic about whether the effect we identify runs mainly through public or private sector unions—and cleanly separating these effects is challenging given that private sector unions often provide political support for public sector unions and vice versa (Ahlquist 2012; Lichtenstein 2013).

[Figure 2 about here.]

What are the direct effects of RTW laws? As with the broader literature on labor unions, most research to date on RTW laws has focused on their contribution to labor market outcomes. These studies, focusing on the consequences of RTW laws for the union wage premium, manufacturing employment and wages, and union density, have produced a mixed picture at best, with scholars finding increases, decreases, and no effect at all depending on their empirical specifications and state and year samples (Moore 1998). One through-line seems to be that RTW laws weaken unions, either by reducing union organizing (Ellwood and Fine 1987) or density (Eren and Ozbeklik 2016) or labor's leverage more generally (Matsa 2010). Union revenues are also hit: Quinby (2017) shows the 2011 collective bargaining ban in Tennessee—conceptually similar to RTW targeting only teachers—reduced teacher union revenue by 25%. We see similar declines in teacher union revenue in Michigan and Wisconsin following the RTW laws we study. Interviews with labor leaders in recent RTW states echo this weakening (see Section 6). We expect this weakness to translate into political weakness, either directly or through a redirection of scarce resources.

One major obstacle to identifying the effects of RTW laws comes from the fact that states that pass such measures are often very different from non-RTW states across a number of important economic, social, and political dimensions that could themselves account for differences in future outcomes.[9] Holmes (1998) proposed studying pairs of border counties where one county is in a RTW state and the other is not. These border counties should be more similar to one another than entire border states are to one another. We adopt this approach in our paper and, as we will show, border counties are quite similar geographically, economically, socially, and politically in both trends and levels before RTW laws are passed.[10] We argue that any political differences that emerge after the passage of a RTW law in RTW counties are more likely to have been driven

[7] Although several states had passed RTW laws before Congress enacted Taft-Hartley, their legality was in question until the passage of the law.

[8] After our study period, *Janus* v. *AFSCME*, decided by the Supreme Court in June 2018, ruled unconstitutional agency fees for all public sector workers across all states, regardless of state right-to-work laws. Any *Janus* effects, while interesting, are beyond the scope of this study.

[9] For example, Zullo (2008) finds a negative correlation between RTW laws and Presidential-level turnout at the county level in the 2000 election. However, based only on a single year of data, disentangling causality from other confounding state-level differences between RTW and non-RTW states is difficult.

[10] Makridis (2018) also uses a county-border design to identify effects of RTW laws on employee sentiment measured in Gallup data.

by the RTW laws. Accordingly, our estimates represent the reduced form effect of RTW laws on electoral and policy outcomes.[11]

To preview our results, we find strong causal evidence for the contribution of unions to Democratic political power—and for the demobilizing effects of RTW laws—examining state and federal elections from 1980 through 2016.[12] After the passage of RTW laws, county-level Democratic vote shares in Presidential elections fall by 3.5 percentage points relative to bordering counties without RTW laws in place. Presidential-level turnout is also 2 to 3 percentage points lower in RTW counties compared to non-RTW bordering counties after the passage of RTW.[13] RTW laws generally reduce Democratic vote share and turnout in US Senate and House elections, as well as state Gubernatorial races. Democratic seat shares in state legislatures also fall after the passage of RTW laws.[14] These results are robust to a number of alternative specifications, including using different time periods, adding additional county-level controls, and excluding different regions of the country. In particular, our RTW effects are robust to controlling for the contemporaneous passage of strict voter ID laws and other conservative-aligned bills that might drive down Democratic vote share and turnout.

We explore several mechanisms through which RTW laws and weakened unions might impair Democratic electoral performance, and we show that in states with RTW laws, the total share of campaign contributions flowing from unions falls by about 1.25 percentage points following the passage of RTW laws. The share of overall contributions collected by Democratic candidates also falls following the enactment of RTW laws. Democrats thus appear unable to replace union funding from other sources, and they raise and spend less money after RTW laws pass. Drawing on data

[11] Holmes (1998) argues that RTW laws are one of many pro-business state policies. If RTW laws are a proxy for a suite of other new pro-business and anti-union policies, our estimates, like Holmes', represent the overall effect of such policies. However, we do not view this as a threat to our identification. Our interest is in the effect of unions on politics and policy, and any such policies that affect unions enable us to estimate just that effect. More problematic to our interpretation of our results would be if RTW laws were passed alongside other conservative wish list items that might reduce Democratic vote share or increase Republican vote share. We show later that RTW laws are not usually passed alongside restrictions on voting (namely, strict voter ID laws) that may disadvantage Democratic electoral prospects. Further, we show that our results are robust to controlling for voter ID and other common conservative laws. Finally, we also show that RTW laws do not have direct effects on the political participation of other members of the modern Democratic party coalition, including African Americans or younger voters, which we might expect to happen if our RTW effects merely reflected the coincidence of RTW laws with other anti-Democratic party legislation.

[12] Our analytical leverage comes from seven "switcher" states that went from non-RTW to RTW status from 1980 to 2016. These states are Texas, Oklahoma, Indiana, Wisconsin, West Virginia, Idaho, and Michigan. We code West Virginia as a switcher state because its law passed in 2016 before November's election even though the law was later enjoined by litigation. As our qualitative evidence suggests, however, unions still adjusted their behavior in response to the enjoined law in anticipation of a legal loss. Our results are robust to dropping WV from our analysis (Figure A.6).

[13] We consider our Presidential results to be our preferred specification. With the same candidates running in every state for President, the year fixed effects effectively control for candidate quality and relative positions on labor and economic issues. In contrast, Gubernatorial, Senate, and House races are all subject to issues of differential candidate quality and positions. House elections also introduce additional concerns related to redistricting and the staggered and longer terms of US Senators mean that our sample of potential border-county pairs is considerably reduced. A similar issue is at play with Gubernatorial elections.

[14] Because we measure state legislature seat share at the state level, we are unable to use the border RD method to estimate the effect of RTW laws on state legislatures. Instead, we run difference-in-difference specifications, comparing RTW states before and after the passage of the laws. We expand on the identification assumptions in the results section.

from national election surveys, we also find that Democratic would-be-voters (non-professional workers) are less likely to report that they have been contacted about turning out to vote in states after the passage of RTW laws.

Third, we consider the downstream consequences of weakened labor unions on state and federal politics and find that RTW laws have large effects on both who runs for office and the substance of state public policy. We observe that in RTW states, state legislators and U.S. Representatives are less likely to have a working-class background, drawing on biographical data from Carnes (2013) and McKibbin (1997). State legislative policy also shifts to the right after the passage of RTW laws, both on labor issues and along other dimensions as well as on specific state policy outcomes like prevailing wage and minimum wage laws.

We conclude with quantitative and qualitative evidence showing how RTW laws affect unions themselves. Specifically, we show how RTW laws force unions to reallocate resources from politics into membership recruitment and retention. By permitting workers to opt out of paying dues to unions that represent them, RTW laws push unions to work harder to retain the same level of revenue and resources. That effort, we show, is costly; it comes at the price of greater labor involvement in politics, helping to explain the RTW effects we identify elsewhere in the paper.

The decline of the American labor movement may have directly increased economic disparities by limiting wage compression in the workplace (Freeman 1980, 1982; Card 2001; Frandsen 2012; Western and Rosenfeld 2011). But diminished union clout may have also indirectly increased inequality by dampening the electoral prospects of Democratic candidates who push for greater economic redistribution.[15] The relationship between unions and inequality (Farber et al. 2018) need not run directly through the labor market; the political economy channel may also play an important role. In sum, changes in state labor policy that have weakened the labor movement may have durably disadvantaged the Democratic party, shifting politics and policy to the right across the U.S. and thus limiting possibilities for economic redistribution through the political system.

The political consequences of RTW laws were clearly on the minds of the lawmakers pushing to make RTW legal. Texas Senator Wilbert Lee O'Daniel, a staunchly conservative Southern Democrat and supporter of the Taft-Hartley amendments, made the connection between RTW and weakened union political clout explicit in remarks on the Senate floor in 1947:

> When Senators are talking about the closed shop [which would be outlawed in RTW states under Taft-Hartley], they are talking about the very heart and soul of the control of our American form of government, because it is the closed shop which siphons off from the taxpayers and the honest laboring people of the country, hundreds of millions of dollars. This is done for the specific purpose of defeating the reelection of any Member of Congress who opposes the labor-leader racketeers, and for the political purposes of using this money that is gained by virtue of the closed shop to elect to the Senate and to the House of Representatives men who will do the bidding of the labor leader racketeers ... The situation is a political one (quoted in Levinson and Sachs 2015, p. 435).

We find strong support for the intuition voiced by Taft-Hartley's conservative supporters in the

[15] Kelly and Witko (2012) present evidence of the pre- and post-fiscal policy effect of unions on inequality across U.S. states.

rest of our paper, which proceeds as follows. In the next section, we review the data and methods we use in our primary analysis. In the third section, we present the main results from the RTW state border discontinuity analysis, estimating the effect of RTW laws on vote share and turnout. In the fourth section, we explore two of the mechanisms through which RTW laws might operate in politics: fundraising and campaign mobilization. In the fifth section, we document downstream effects of RTW laws, showing how they decrease state policy liberalism and reduce the number of elected officials with working-class backgrounds. In the sixth section, we document the politics-focused and membership-focused spending trade-off unions face. The seventh section concludes the paper.

2 Research Design and Methodology

The central challenge to understanding the economic or political consequences of RTW laws is that there may be factors within states that *both* lead states to adopt RTW laws *and* affect outcomes of interest, here Democratic electoral prospects. For instance, public opinion in a state might shift against unions, and as a result a legislature and governor opposed to unions would gain power and then enact a RTW law. To account for this bias, our main empirical strategy involves looking at neighboring counties—the smallest geographic unit with available election and economic data—that straddle a state line separating a RTW state from a non-RTW state. In this section, we describe our sample, document the validity of the assumption that county border pairs are similar, and detail our empirical specification.

2.1 County Border Pair Sample, 1980 to 2016

We focus on the 1980 to 2016 period for our analysis because of the complicated relationship between unions and Democrats before this time (Greenstone 1969). Prior to 1980, the Democratic party coalition included many conservative Democrats, especially in the South, who vigorously opposed unions (Katznelson 2013). At the same time, the Republican party coalition included moderate and even liberal politicians who broadly supported union rights (Hacker and Pierson 2016; Anzia and Moe 2016). As a result, it is difficult to measure the electoral consequences of unions in a straightforward manner before 1980, as unions may have mobilized their workers to support candidates from both parties (but see Schickler 2016).[16] After 1980, however, ideological sorting between the two parties was well underway, leading us to focus on the electoral implications of labor strength for Democratic electoral victories from 1980 to 2016 (McCarty et al. 2006). As we will show in Figure 8, changing this starting point by one or two election cycles does not appreciably alter our results.

[Figure 3 about here.]

Our main sample for analysis will be the pairs of counties on either side of a RTW border between 1980 and 2016. Figure 3 plots the border pair counties. These counties are those that

[16] In addition, as we document in Figure 2, only two states went right-to-work between 1960 and 1980 (Wyoming in 1963 and Louisiana in 1976), so we have limited variation in RTW status before 1980.

border another state with a different RTW regime in place. As Figure 3 makes clear, our sample comes generally from Western, Midwestern, and Southwestern states. Northeastern counties are not included in the sample because RTW laws were never enacted in these states, offering us no opportunities to observe treatment counties in this region. Few Southern or Southeastern counties are included because RTW laws were always in place in these states during our sample period, offering us no opportunities to observe control counties in these regions. Within our sample, there are three types of border counties. One group comprises counties in which RTW was never in place; these counties are always control counties for our analysis. Another group of counties are those in which RTW was introduced during the period of our analysis (1980-2016). These counties are control counties for the period in which they did not have RTW and treatment counties for the period in which they did have RTW in place and provide the main empirical leverage for our analysis. These "switcher" counties come from Texas, Oklahoma, Indiana, Wisconsin, West Virginia, Idaho, and Michigan.[17] The final group of counties include those that always had RTW in place, and these are thus the treatment counties for the whole period in our sample. Our sample includes approximately 200 unique counties in all years, changing slightly as states enact RTW, and nearly 400 county border pairs, reflecting that many counties pair with more than one other county across the border (see Figure A.1 for the sample size trends in each Presidential election year).

Implicitly, our border pair design rests on the assumption that across-border neighbors are a reasonable control for the treated counties. One potential alternative to border pairs would be explicit matching of counties in RTW and non-RTW states, matching on pretreatment covariates or on our outcome variables. We opt for the border pair design because it makes identifying control counties straightforward; there are far fewer researcher-degrees-of-freedom in picking geographic neighbors compared to selecting the variables and matching function used in a matching specification.

2.2 How Similar Are Counties Across RTW Borders?

Counties paired across state borders ought to be much more similar than pairs of states—and therefore any changes in the differences we observe between these county pairs after RTW laws pass might be plausibly attributed to RTW laws and not to other characteristics of the counties themselves. In addition to Holmes (1998) on the economic consequences of RTW laws, similar methodologies have been employed to study the effects of minimum wage laws on wages and employment outcomes (border county pairs have similar labor markets, Dube et al. 2010) and the effects of Medicaid expansion on political participation (citizens in border county pairs have similar baseline political behaviors, Clinton and Sances 2018). The underlying assumption in our approach is that after controlling for year and border-pair fixed effects—which together net out any time-varying national shocks and time-invariant county-pair-specific characteristics—any political differences we observe between border county pairs across a RTW border are attributable to the RTW laws and not to other characteristics of the two sets of counties.

Do border county pairs actually look similar to one another? Figure 4 suggests this is the case.

[17] Our results are robust to dropping each state in turn. See Appendix Figure A.6.

We compare the differences in means between RTW and non-RTW counties in all counties (left-hand side) and between border county pairs, after accounting for state-border effects (right-hand side) for a variety of county characteristics available from the US Census. This figure shows the coefficient and associated standard errors of bivariate OLS regressions of various Census outcomes, brought forward from the most recent Census to each election year, on a RTW indicator. We focus on demographic characteristics, like race and education, that might shape political participation, as well as information on the labor markets in each county. To ease interpretation of the many comparisons across different variables with different scales, the covariates are standardized—divided by their standard deviation—before running the regressions. Looking first at all counties, we observe some large differences between counties in RTW states and counties in non-RTW states. Counties in RTW states are less urban, have much smaller white-only populations, much larger African-American populations, and much higher rates of poverty. Clearly, then, there may be underlying differences between RTW counties and non-RTW counties that would complicate a naive comparison across all counties.

[Figure 4 about here.]

However, we find that border county pairs are nearly identical on most characteristics we examine, shown in the right panel in Figure 4. There are four measures where there are differences between RTW and non-RTW counties: poverty, labor force participation, unemployment, and manufacturing. However, the differences are not economically large. Furthermore, these are labor market differences that are plausibly driven by the RTW treatment (Moore 1998). In addition, the ways in which RTW and non-RTW border county pairs differ do not point towards a clear bias one way or another for our results. RTW counties have lower unemployment, more employment in manufacturing, greater labor force participation, and less poverty compared to their neighboring non-RTW counties. In the overall sample, poverty, labor force non-participation, unemployment, and manufacturing are all correlated with higher Democratic vote shares. While we would expect RTW border counties to have lower Democratic vote shares based on their lower levels of poverty, unemployment, and labor force non-participation, we also would expect them to have greater support for Democratic candidates based on their higher levels of manufacturing employment.[18] It is difficult, then, to reach a single conclusion about the remaining small bias in this sample.

There is no systematic evidence that counties in states that eventually enacted RTW laws were trending differentially in economic or demographic variables from their cross-border neighbors before the passage of RTW. We plot these pre-RTW trends in Figure 5, reporting the decennial difference in each covariate for the treated counties less the decennial difference in their border pair neighbor control county in the two censuses before RTW passes in the treated county's state. Differential pre-RTW trends might have indicated that there are other factors explaining the passage

[18]Similarly, in the overall sample, poverty, labor force non-participation, manufacturing employment, and unemployment are all correlated with lower turnout. Therefore, the lower levels of poverty, unemployment, and labor force non-participation in RTW counties ought to push these counties towards having higher levels of turnout, while their higher levels of manufacturing push in the opposite direction.

of RTW laws—factors that could also help explain any changes in Democratic electoral performance. As the plots indicate, however, there is little evidence of statistically discernible trends in RTW counties on these variables before the passage of RTW one way or another. In the appendix, we show balance in border pair differences in levels in the census prior to RTW passage (Figure A.2).

[Figure 5 about here.]

Further, we find no evidence that the counties on the RTW side of the border were trending towards Republicans relative to their non-RTW paired county. In Figure 6, we plot the differences in Democratic vote share between the paired counties—treated county less control county—in the elections *before* RTW was passed in the treated county's state. The light lines are at the county-pair level and the darker lines are the state-level averages of the differences. Though the paired counties do not always vote for Democratic Presidential candidates in exactly the same share, there is no pattern in the levels or trends suggesting large, pre-existing political differences between the paired border counties. In Appendix Figure A.3, we show similar results for the difference in turnout across the eventual RTW border.

[Figure 6 about here.]

2.3 How Comparable Are Different RTW Statutes Across States?

Legally, RTW statutes in different states are comparable with one another. Although individual statutes do vary in their coverage of workers and in their penalties or remedies (for instance, invoking civil versus criminal law, or different fines or penalties), we argue that there are no significant differences in the legal "bite" of the laws across states. As one labor lawyer for the National Right-to-Work Committee, which promotes RTW laws, explained to one of us, despite the fact that state laws differ in their exact punishment of RTW violations, "an employer or union would have to have pretty incompetent counsel to agree to include mandatory fees in a RTW state." He was not aware of litigation, moreover, indicating that unions or employers were violating open-shop provisions in RTW states. Thus, RTW proposals generally should be comparably binding on unions regardless of their precise legislative language. In Appendix B, we provide the exact text of the RTW statutes in the states we study in this paper. In Table B.1, we compare the language used in the various RTW statutes in our sample using the Jaccard index, a common measure of textual overlap that runs from 0 (no overlap) to 1 (perfect overlap). As the appendix makes clear, some of the bills are much more similar to one another than others, especially the Indiana and Oklahoma laws, the Michigan and Oklahoma laws, and the Wisconsin and West Virginia laws.

2.4 Empirical Specification

The empirical approach we employ is relatively straightforward. The unit of analysis is the county-year for the 10 Presidential election years from 1980 to 2016. The main explanatory variable is a binary indicator (RTW) that captures whether a particular county in a given year had a RTW law in place. We begin investigating the effect of RTW laws on all counties:

$$Y_{cst} = \alpha + \beta RTW_{st} + \phi_c + \tau_t + \epsilon_{cst} \tag{1}$$

where outcomes are either two-party Democratic vote share or turnout.[19] We include county and year fixed effects and cluster standard errors by state because RTW laws are implemented at the state level. In our preferred specification, however, we zoom in on counties on state borders and estimate:

$$Y_{cspt} = \alpha + \beta RTW_{st} + \phi_c + \tau_{pt} + \epsilon_{cspt} \tag{2}$$

which includes τ_{pt}, year by border pair fixed effects. Thus, only the variation from county pairs with different RTW statuses identifies the main RTW effect, and this variation is driven by the seven switcher states that enacted RTW legislation between 1980 and 2016. Here we cluster two ways, by state and by county border pair.[20]

Our theoretical expectations are that RTW laws dampen the strength and mobilization of labor unions and thus the ability of unions to contribute resources to the Democratic party coalition. If true, then we should observe a drop in votes for Democratic candidates in RTW counties compared to their non-RTW counterparts. Similarly, to the extent that one of the valuable resources that unions offer to Democrats involves grassroots mobilization of voters, we ought to observe a drop in turnout as well.

Our main results focus on Presidential elections, though we follow up with similar findings for Senatorial, House, and Gubernatorial races.[21] As we explain in the introduction, we prefer the Presidential elections specification for three reasons. First, Presidential elections have the virtue of comparing the performance of the same candidates across the entire country, holding constant the quality of those candidates (which otherwise varies across Senate, House, and Gubernatorial races). Second, the Presidential election is not subject to redistricting, which could affect US House results and provide an alternative explanation for any change in electoral performance. Third, the data for Senatorial and Gubernatorial elections is much more sparse given variation across the states (and

[19] We compile the two-party Democratic share of the Presidential vote in each county from Congressional Quarterly elections data from 1980 to 2012 and the US Election Atlas from 2012 to 2016. We measure voter turnout as the total votes cast divided by the voting-age population in each county, drawing voting-age population data from the US Census. Unfortunately, the age divisions reported for counties before 1990 do not allow us to calculate a true VAP, so for 1980 to 1990 our VAP reflects the proportion of the population 20 years or older; our results remain similar excluding these years from the analysis.

[20] Counties enter the data in pairs, p, and we follow Dube et al. (2010) in stacking the data accordingly. For counties on a state border with multiple neighbors, the county will be included multiple times, one for each pair. The county border pair clustering accounts for the stacking. In Figure A.4, we show that our results are robust to variation in the construction of our sample. Specifically, we could have forced each county to only enter the data once, paired with only one other county. However, it is not obvious which pair for each county should be in the data and which pairs dropped. Rather than make this assumption, we bootstrap over 50,000 possible sample definitions, enforcing uniqueness such that each county only enters the data once. As we show in Figure A.4, the effect of RTW laws on Democratic vote share and on voter turnout are not sensitive to county border pair samples.

[21] Unfortunately, state legislative districts do not always fall along county lines, complicating the estimation of county-level vote totals for these races. In addition, we are unable to identify consistent cross-walks for state legislative districts to counties over the period we are studying (1980-2016).

thus county border pairs) in when elections are held. Notwithstanding these concerns, the fact that we find similar effect sizes across these different levels of government suggests that our RTW findings are not merely capturing the idiosyncrasies of campaigns for any one particular office and reflect a broader change following the passage of RTW.

While we lack county-level measures of union strength—either simple density (Hirsch et al. 2001) or more detailed measures of organizational or political power—the seven states that enacted RTW in our study period are a mix of union strongholds and anti-union states. The unionization rate in Michigan in the decade before RTW was nearly 20%, five points higher than the national average, and over 15% in Idaho, compared to union density in the single digits in Texas or Oklahoma before RTW. Our analysis thus identifies the estimated treatment effect for border counties in states with a wide range of pre-existing union strength.

3 Right-to-Work Laws and Elections

Using our county-border-pair design, we estimate a negative effect of RTW laws on Democratic vote share and turnout in Presidential elections. We also find decreases in Democratic candidate performance in other elections at the federal and state levels and in turnout for these elections. In this section, we detail our main empirical results.

Before presenting our border-pair regressions, we first offer a graphical representation of our main findings in Figure 7, indicating the change in Democratic electoral prospects in Presidential elections before and after the passage of RTW laws for all counties (in the left hand plot) and only border county pairs (in the right hand plot). As the figure shows, the pre-RTW trend in Democratic vote shares is quite similar in never-RTW and RTW counties alike, especially when restricting our focus to only border county pairs (in the right hand plot). But after RTW laws pass, as this plot suggests, Democratic electoral prospects decline—a suggestive pattern that warrants closer inspection.[22]

[Figure 7 about here.]

3.1 Right-to-Work Laws Reduce Democratic Vote Share and Turnout in Presidential Elections

We find consistent negative and significant relationships between the passage of RTW laws and Democratic electoral outcomes and Presidential election turnout across all specifications. We document these negative effects of RTW laws on Democratic vote share and turnout in Table 1, with vote share in the top panel and turnout in the bottom panel. We begin with simple correlations in the first column, reporting only the univariate regression of Democratic vote share on RTW laws on the sample of all counties in the US from 1980 to 2016. The coefficient is negative, but clearly there are many differences between states with and without RTW. The negative relationship remains as we add county and year fixed effects and county and census division by year fixed effects in columns

[22] The simple analysis in Figure 7 is unbalanced, as we only observe part of our sample 3 or 4 election cycles after RTW law treatment.

2 and 3. In all of the all-county samples, we cluster standard errors at the state level. Examining only counties on state borders in Table 1, columns 4 to 6, the estimated negative effects of RTW laws persist in specifications that mirror our estimates on the full county sample.

[Table 1 about here.]

But do RTW laws cause Democratic vote shares to fall? To make this stronger claim, we turn to our preferred specification in column 7 of Table 1. Here, we include county and year fixed effects, but we also include border pair by year fixed effects, using only the variation across a county-border-pair with different RTW statuses to generate our estimated effect of RTW. We find RTW laws reduce Democratic vote shares by 3.5 points.

We also find evidence that RTW laws reduce voter turnout at the county level in Panel B of Table 1. Focusing again on our preferred specification—border counties only with county and border pair by year fixed effects—we estimate RTW laws reduce turnout by 2 points.

RTW laws reduce Democratic vote share by 3.5 points and turnout by 2 points. Are these large or small effects? We argue that they are quite meaningful both in the context of tight Presidential races and relative to the literature on voter mobilization. In 2016, Hillary Clinton lost Wisconsin, Michigan, and Pennsylvania by less than a percentage point each. In addition, these are also meaningful effect sizes based on the large experimental literature on voter mobilization and contact. One meta-analysis of 71 canvassing experiments revealed an average causal effect of about 2.5 percentage points on turnout (looking at a complier-adjusted average), in line with our RTW estimate on turnout (Green et al. 2013).[23] Overall, some estimates indicate that during the 2012 Presidential campaign, turnout in highly campaign-targeted states increased by 7-8 percentage points, on average (Enos and Fowler 2016). And one analysis of the political effects of the expansion of health insurance to poor adults through Medicaid revealed a temporary increase of about 3 percentage points in turnout (Clinton and Sances 2018).

Our effects on partisan vote share are also comparable to other major interventions that reshape political participation. Hall (2016) estimated a large negative elasticity of corporate money bans in campaigns on Democratic electoral prospects; Democratic state legislative seat shares fell 1 percentage-point for every 2 percentage-point decline in the Democratic share of contributions. Enos (2016) estimated that the demolition of public housing in Chicago changed residential segregation patterns and the political behavior of white voters; when white Chicago voters were no longer living next to African American communities, Republican vote share fell by more than 5

[23] One question is how to reconcile the relatively large effects that we identify in our results with recent meta-analysis of political campaigning by Kalla and Broockman (2018) that finds minimal effects of campaigns on vote choice. It is important to acknowledge that our results are not experimental and are thus not directly comparable to those in this literature. That said, we believe that union political contact may not be like other more traditional campaign contact given the distinctive relationship labor organizations have to workers and the local communities in which they are embedded. Unions have enduring relationships with workers and their social networks that can reshape workers' political identities in profound ways (see especially Ahlquist and Levi 2013). And unions may also deploy social pressure to convince workers workers to turn out to vote and to vote for union-favored candidates (especially given voters' concerns about the secrecy of their ballots in social environments, e.g. Gerber et al. 2012). Ultimately, however, we leave this question of specific mechanisms and effect size for future work.

percentage points. And studying Tea Party rallies in 2009, Madestam et al. (2013) found that every 0.1 percentage point increase in the share of a House district protesting with the Tea Party raised Republican vote shares in the 2010 elections by 1.8 percentage points. Our estimated effect of RTW laws from Table 1 thus provide compelling evidence of the role unions have played in the election of Democratic Presidential candidates, in part by turning out voters. RTW laws have, in turn, demobilized potential Democratic supporters in Presidential races.

[Figure 8 about here.]

Our main RTW effects are robust to a number of alternative sample definitions and to the inclusion of time-varying county-level controls, as we show in Figure 8. The first row summarizes the RTW coefficients from our preferred specification in Table 1 (column 7) looking at border county pairs with border pair by year and county fixed effects. In the second row, we drop the 1980 and 1984 elections when—arguably—the parties were still in the process of realigning on support for and from unions. The results on Democratic vote share remain, though the turnout effects are only statistically significant at the 10% level. In the third row, we show that we can exclude the 2016 election and the negative effects of RTW laws on Democratic vote share remain evident. The fourth row of the figure excludes Southern states from our analysis, and little changes from the main baseline analysis. In the fifth row, we add in time-varying, county-level controls (summarized in Figure 4). The controls improve precision, at least in the voter turnout analysis, but generally yield similar results to our baseline specification.[24] In the sixth row, we include a control for economic attitudes in the mass public, derived from public opinion surveys (originally estimated by Caughey and Warshaw 2018). This control represents an estimated left-right policy preference among the population of each state, lagged four years before each Presidential election. The results are again substantively unchanged.

While states that adopted RTW laws may have also adopted other legislation to demobilize Democratic voters at the same time, either de facto or de jure, we find no evidence that these other laws affect the RTW effects we are focused on. The most recent enactments of RTW laws occurred after the GOP gained full control of state legislatures and governorships after 2010 and began enacting an array of conservative policy priorities. If states adopted these policies at or around the same time as RTW laws and if these policies reduce the turnout of Democratic voters, we would be concerned that our main results reflect policies other than RTW laws. We address this concern in two ways. First, we consider the enactment of strict voter ID laws, which a number of fully GOP-controlled states began enacting after 2006, and especially after 2010. These provisions require voters to present state-approved forms of identification in order to vote, and there is good evidence to suggest that these measures are designed to demobilize traditionally Democratic constituencies, like college students, minorities, and poorer voters (Bentele and O'Brien 2013; Berman 2015). We

[24]The controls include the share of the population living in urban areas, white share of the population, native-born share of the population, college educated share of the population, median family income, labor force participation, unemployment, manufacturing share of the labor force, transportation share of the labor force, and public administration share of the labor force.

estimate regressions that include an indicator for whether or not a state had a strict voter ID law in place alongside our RTW indicator. The results shown in the third to last row of Figure 8 indicate that controlling for voter ID laws does not appreciably change our findings.[25] In the appendix, we also show, using individual-level survey data, that racial and ethnic minorities are no less likely to report turning out to vote following the passage of RTW laws. If voter ID laws passed in the same years as RTW laws, then we might expect to see depressed turnout of racial and ethnic minorities following the passage of RTW. Yet this is not what we observe.[26]

Second, beyond strict voter ID laws, we also show that other conservative laws—often intended to hobble Democrats—do not diminish the RTW effect. The American Legislative Exchange Council, or ALEC, is an association of state lawmakers, conservative activists, and private-sector business representatives that formulates and distributes right-leaning, business-friendly policy proposals. Operating since 1973, ALEC has had great success in enacting many of its model bills across states; at its peak in the early 2000s, it counted between a third and a quarter of all state legislators as members. ALEC has promoted both RTW and voter ID laws, along with a range of other measures intended to strengthen the political position of conservatives (Hertel-Fernandez 2014, 2019). Drawing on an enacted ALEC bill dataset from 1996 to 2013 (Hertel-Fernandez 2019), we create a binary indicator of whether states enacted an ALEC bill in a given year (excluding ALEC bills related to labor unions). As the second to last row of Figure 8 indicates, the effect of RTW laws on Democratic vote share remains similarly sized to the other specifications. The turnout results, on the other hand, shrink a bit and are much less statistically precise, though this may be driven by the smaller sample size.

Together, the voter ID and ALEC controls make us more confident that there were not other changes, especially in recent years, that coincided with RTW law passage that could explain the decline in Democratic vote share and turnout that we observed in Presidential elections. In the appendix, we show that our results are robust to controlling for media market variation and commuting zone variation across county and state borders (Table A.5 and Table A.6), controlling for contemporaneous political power in each state (Figure A.5) and our stacked border pair sample (Figure A.4), as well as reweighting our stacked border counties so that each county has equal weight (Table A.4). In addition, we show that no single state in our sample of RTW-switchers is driving our results, dropping each state in turn and finding statistically indistinguishable effects of RTW on vote share and turnout (Table A.6).

Are our results driven by the effects of RTW laws on public sector unions, private sector unions, or both? As we detailed above, U.S. courts have generally interpreted right-to-work laws as applying to both government and private sector employees with equal force, even when right-to-work laws do not mention government workers specifically (see e.g. AFSCME v. Phoenix, 142 P.3d 234 (2006)). Right-to-work laws should thus generally affect both public and private sector political power. Two exceptions are states where public employee public bargaining is illegal—and therefore

[25] Our record of strict voter ID laws comes from the National Conference of State Legislatures.

[26] More generally, it is worth noting that many RTW states did not adopt strict voter ID laws and vice versa. In our dataset only 19% of state-year observations had both RTW and voter ID in place.

unions cannot bargain over agency fees or any other provisions—and states that have separately limited agency fees for public employees.

Controlling for the strength of teachers unions—a plausible proxy for the political power and legal rights of all public sector unions in a state—does not change our main findings. Measuring the collective bargaining rights of public employees across states is no easy task, so we focus specifically on teachers union rights. Not only are teachers one of the most politically powerful public unions (Moe 2011), but researchers have also assembled much more consistent and comparable state-by-state data on teachers unions' bargaining rights. We draw on the measures of collective bargaining permissiveness originally developed by Freeman and Ichniowski (1988) and later updated by Kim Rueben and Leslie Finger.[27] We include the index of teachers' collective bargaining rights as a set of dummy variables in our main specification. As the last row of Figure 8 indicates, our results do not appreciably change even accounting for the institutional bargaining strength of this important public sector union.

3.2 Effects of Right-to-Work Laws on Other Elections

Our results indicate that RTW laws lead to lower levels of Democratic votes and turnout in Presidential elections. What about other state and federal offices? As we explained earlier, we prefer the Presidential level results for both substantive and methodological reasons. However, these concerns aside, in the results presented in Table 2, we show similar negative effects of RTW laws on Democratic vote share and turnout at the state Gubernatorial, US House, and US Senate levels. The results for Democratic vote share are less precise for the non-Presidential elections, though both the Senate and Governor effects (-3.3 and -2.5) are close in magnitude to the Presidential effect. The negative effects of RTW on congressional elections may be more concentrated in the relatively low turnout and low information off-cycle, non-Presidential election years, but our results are too noisy for us to be confident about this finding. The turnout effect of RTW, though not significant for Senate or Gubernatorial elections, is similarly stable in magnitude, ranging from -1.1 to -2.5.

[Table 2 about here.]

We have shown that RTW laws dampen Democratic electoral prospects in federal elections as well as Gubernatorial elections. Do RTW laws also shape control of state legislatures? Unfortunately, we cannot answer this question with the same degree of causal credibility as in the preceding analyses: state legislative districts do not fall neatly along county border lines, and the vote totals are rarely reported at the county level.[28] This prevents us from applying the county-border-pair comparison as before. However, we can still exploit variation in the timing of RTW laws across states to examine their effects on statewide legislative control. If RTW laws indeed depress turnout among Democratic constituencies during elections, then we ought to see that the proportion of legislative seats held by Democratic politicians falls after the enactment of RTW policies.

[27] Updated series provided generously by Leslie Finger.

[28] State legislative district shapefiles are also not available for most of the years in our sample.

Examining Democratic legislative seat shares from 1980 to 2016 before and after RTW enactment and including state and year fixed effects, we observe a strong correlation between the presence of laws hobbling labor unions and state legislative control. Our results, summarized in Table 3, suggest that Democrats control about 5 to 11 percentage points fewer seats in state legislatures following the enactment of RTW laws. These losses are felt by Democrats in both upper and lower chambers of state houses. While we reiterate that we cannot interpret these results in the same causal manner as the county-border pair models presented earlier, this difference in difference analysis strongly suggests that, in addition to disadvantaging Democratic candidates for federal office and state governorships, RTW laws appear to hamper Democratic aspirants for state legislatures as well.

[Table 3 about here.]

4 Mechanisms for the Right-to-Work Effect: Campaign Mobilization and Contributions

Why do RTW laws reduce Democratic vote share? What do unions do to direct voters to the polls and towards Democrats? In this section, we find support for the importance of unions as both a get-out-the-vote driver and a campaign funder to Democrats.

The advantage to the border-county analysis used in the previous section is that it enables us to credibly make causal inferences about the effect of RTW laws on election outcomes. However, data limits—little data on campaigns are collected at the county level—prevent us from applying it to reveal the mechanisms that drive the relationship between unions and election outcomes. Using a difference in difference analysis, comparing states before and after RTW laws are enacted, we can undertake this analysis of mechanisms. We find RTW laws reduce the share of voters receiving get-out-the-vote (GOTV) contact—particularly among potential union members—and limit unions as a fund-raising source for Democrats.

4.1 Campaign Contact

Following the passage of RTW laws, workers who would be most likely to be mobilized by unions—non-professional, non-managerial workers—are less likely to report being contacted by a GOTV effort. To explore this potential mechanism of our main RTW election effect, we turn to individual-level data from the American National Election Studies (ANES) time series cumulative file.

Though the design of the ANES survey prevents us from replicating our county-border design, the rich individual-level data allow us to measure the RTW effect for specific groups of potential voters who are more or less likely to be affected by the laws. The ANES is a series of representative surveys of Americans in election years, starting in 1948. These surveys include a range of questions about Americans' voting habits and overall participation in politics. In the ANES data, we can observe political participation at the individual level and study the effects of RTW laws on get-out-the-vote recruitment among non-professional, non-managerial workers.[29] The disadvantage to the ANES, however, is that sample sizes range from 1,000 to 2,000 in each election year. Thus, we cannot employ the same border county pair research design. Instead, we study RTW laws at the state level with a difference in difference analysis. The trade-off we make is between understanding the mechanisms driving the results we identified earlier and cleaner causal inference.

Why might RTW laws lead to lower turnout among non-professional workers? Our county-level analysis indicated that RTW laws reduced turnout, suggesting that weaker unions might mean lower turnout of reliably Democratic voters, but we could not test this mechanism directly in aggregate data. Individuals are more likely to participate in politics when they are asked to participate by someone else—and that includes voting in elections (Verba et al. 1995; Green and Gerber 2008). After the passage of RTW laws, unions may be less well positioned to mobilize workers to participate in politics, including elections. The ANES permits us to evaluate this question with the following item, asked from 1980 to 2012: "During the campaign this year, did anyone talk to you about registering to vote or getting out to vote?"

We estimate linear probability models of a respondent indicating get-out-the-vote contact in an election year:

$$GOTV_{ist} = \beta_0 + \beta_1 RTW_{st} + \beta_2 RTW_{st} \times NonProfessional_{ist} + \beta_3 NonProfessional_{ist} + \phi_s + \tau_t + \epsilon_{ist} \quad (3)$$

with an indicator for whether the ANES respondent resides in an RTW state, an indicator for whether a respondent was employed in occupations excluding managers or professional workers, a category we call *Non-Professional Workers*, and an interaction. We also include state and year fixed effects and a vector of individual controls.[30] The non-professional workers are the most likely potential union members we can identify in the ANES, since managers and professional workers are likely to be ineligible to form or join unions. It should therefore be the non-professional workers who would be most likely to be affected by the decline in unions. We apply ANES survey weights and cluster standard errors by state. In all, our data permits us to examine elections from 1980 to 2012.

[Table 4 about here.]

We find that RTW laws are associated with a reduction in the probability that non-professional workers—but not professional workers—report get-out-the-vote contact during the campaign. Table 4 presents the results of this analysis, with a binary indicator for GOTV contact during the last

[29] In the ANES from 1980 to 2012, the unionization rate among non-professional, non-managerial workers is 15.4%, compared to 12.9% among the balance of the sample.

[30] The full battery of individual control variables includes age and age squared, gender, education (high school or less, some college, or college or more; high school or less is the excluded category), indicators for race and ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and other; other is the excluded category), church attendance (in five categories of frequency), interest in political campaigns (in three categories), and a dummy variable for strong partisanship.

campaign as the outcome. In the model with individual controls, we find that RTW laws reduce the probability that a non-professional worker reported GOTV contact by 11 percentage points but had no discernible effect on professional and managerial workers. Table 4 thus presents strong evidence that RTW laws dampen turnout among rank-and-file workers by reducing the likelihood that they will be recruited into politics around elections.[31] We also find somewhat stronger effects of the RTW laws on GOTV in Presidential election years than in midterms, comparing columns 3 and 4 with columns 5 and 6.

Importantly, we find that RTW laws reduce voter turnout at the individual level among non-professional workers more than professional workers. We document these findings in Appendix A.8, showing that in both the ANES and in the much larger Current Population Survey, November Voting and Registration Supplement (N=100,000-150,000), respondents are less likely to report voting in the previous election if they are non-professional workers in RTW states. This effect contrasts with the minimal RTW effect on turnout of other likely Democratic-voting groups, like racial minorities and young voters, where we estimate small and statistically imprecise effects of RTW on turnout.

4.2 Union and Campaign Fundraising

Our analysis thus far has focused on voting and turnout, suggesting that following RTW laws unions might have fewer resources to invest in get-out-the-vote canvassing and dampening Democratic vote shares. In this section, we show that by weakening unions, RTW laws also limit unions' campaign fundraising clout.

Unions have long been one of the most important donors to political candidates in both federal and state races (Dark 1999). Indeed, the first major political action committee in all of American politics belonged to the precursor to the AFL-CIO, and it was that committee's heavy electoral involvement that in part inspired the business community to adopt its own strategy of campaign investment (Waterhouse 2013).

Pooling available data on state and local campaign contributions from 1996 to 2016 from the National Institute on Money in State Politics, we estimate a difference in difference analysis at the state and year level to measure the effect of RTW laws on union campaign contributions. We find that RTW laws reduce campaign contributions from unions, measured as the share of total campaign spending (Table 5). Once again, we are limited by contribution data at the state rather than county level and are unable to implement our cross-RTW-state-line county-border set-up. The pretrends—or lack thereof—we show in the Appendix (Figure A.7) give us some confidence that the difference in difference is still informative. Further, in the regression we are able to include state and year (or Census division by year) fixed effects to account for state-specific, time-invariant characteristics (like public attitudes), as well as election-specific, state-invariant shocks (like wave elections or on- and off-year cycles).

[Table 5 about here.]

[31] We find no evidence that the non-turnout political activity of workers, non-professional or professional, changes after RTW laws, using the standard ANES batteries of political participation questions as outcome variables.

RTW laws reduce private sector union contributions by 1 to 2 percentage points (Table 5). There may also be a negative effect on public sector unions, because total contributions from all unions falls by 2.5 to 3 points. The decline in labor contributions appears to have strongly disadvantaged Democrats. As we show in columns 5 and 6 of Table 5, the share of all state and local contributions flowing to Democrats falls in RTW states following the enactment of RTW laws, though the estimate is not significant adding Census division by year fixed effects. It appears that Democrats may be unable to replace the funding they lose from labor unions following the passage of RTW laws, and that the balance of campaign funding tilts in favor of the Republican party.

5 The Downstream Political Consequences of RTW Laws

RTW laws weaken unions' abilities to intervene in politics by turning out voters and contributing to candidates, thus lowering the electoral prospects for Democrats running for state and federal office in RTW states. But by durably weakening the relationship between labor unions and the Democratic party, are there other, long-term political consequences of state RTW laws? We test two such consequences in this section, looking at the socioeconomic backgrounds of legislators and the overall ideological liberalism of state policy. We find that working-class candidates are less likely to hold elected office and that state policy moves to the ideological right following the passage of RTW laws.

5.1 The Effect of RTW Laws on Who Serves in Legislatures

Why might RTW laws affect the class background of state legislatures, and why would that matter in the first place? There is increasing evidence, much of it from political scientist Nicholas Carnes, that politicians from working class or blue-collar occupations behave differently from politicians who spent their careers in white-collar jobs (Carnes 2013). Carnes shows that working class politicians, independent of party and ideology, are more likely to support redistributive economic policies than their peers from white-collar professions. Within Congress, for instance, the few working-class politicians who serve have been more likely to back progressive economic policies. At the state-level, legislatures with a greater proportion of one-time blue-collar workers are more likely to enact redistributive social programs and labor market regulations.

Working-class politicians are dramatically underrepresented at all levels of government, though there is considerable variation across states. Between 50% and 60% of Americans might count as working class, yet working-class lawmakers have made up only 2% or less of Congress throughout the twentieth century (Carnes 2013). The comparable figures for state legislatures in 2007, the last year for which we have data data, is 3% (Carnes 2013). These rates vary, however, from 0% (California) to 10% (Alaska).

Why might RTW laws reduce the number of working-class politicians? The barrier to working-class representation is not that voters dislike these candidates or that workers have fewer of the political skills necessary to run for office (Carnes 2013). Rather, traditional electoral "gatekeepers"—primarily local party leaders—simply do not encourage working-class politicians to run for office in the first place (Carnes 2016). A vibrant labor movement, however, might well encourage greater representation of the working class in political office. Unions might do this indirectly, by fostering ambition and political aspirations among working-class union members, or directly, by encouraging their members to run for office and then supporting them through grassroots voter mobilization and campaign financing. There is strong correlational evidence that workers are more likely to serve in elected office when unions representing them are larger and more encompassing: for instance, police officers are more likely to serve in state legislatures when police unions in that state are stronger, and construction workers are better represented in legislatures when construction and building trades unions are stronger in that state (Sojourner 2013). In addition, union density is positively related to the proportion of working-class members of state legislatures (Carnes and Hansen 2016).

By weakening union membership and political clout, do RTW laws effectively reduce the representation of the working class? Drawing on data first analyzed by Carnes and Hansen (2016), who examined the state-level correlates of working-class representation, we find states with RTW laws have lower shares of working-class state legislators. As we show in Table 6, states with RTW laws have 1 to 3 percentage points fewer working-class representatives.[32] Our unit of analysis is a state-year. Unfortunately, the occupational backgrounds of state lawmakers are only available for four years (1979, 1993, 1995, and 2007), and so we are more econometrically limited than in previous analyses. However, the correlation between RTW laws and lower shares of legislators with working class backgrounds is strong and negative throughout, whether we include year fixed effects, the many controls in the original Carnes and Hansen (2016) analysis, or state fixed effects. We cluster standard errors at the state level in all models.

[Table 6 about here.]

We also find evidence to suggest that RTW laws reduce working-class representation in Congress. Unfortunately, we are limited by the available data here as well. We use two different sources. From 1999 to 2008, Carnes coded the careers of all Members of Congress, indicating the proportion of their pre-Congress work spent in blue-collar occupations (Carnes 2013). Around 6% of Congress over this period spent any time in a blue-collar occupation before arriving in Washington. Over a slightly longer period from 1980 to 1996, Carroll McKibbin and the ICPSR staff compiled the jobs that Members of Congress held immediately before serving in Congress (McKibbin 1997). Less than 1% of Congress served in working-class occupations immediately before coming to Congress over this period.

We estimate difference in difference OLS regressions for both datasets, where the unit of analysis is a Member serving in a specific Congress and the main explanatory variable is a binary indicator for whether the state had a RTW law in place during the previous election year. We also add state and election year fixed effects and cluster our results by state. We focus only on the US House, given the rarity of working-class Senators in this period.

[32] The other categories in the data aside from workers include technical workers, business owners/executives, business employees, farm owners/managers, politicians, lawyers, and service-based professionals. See Carnes and Hansen (2016) for more information.

[Table 7 about here.]

As Table 7 indicates, we generally find that RTW laws are related to lower working-class representation in the US House, though the estimates are noisy and not always significant across different specifications that include Census region by year fixed effects. On average, RTW states are 3 percentage points less likely to elect a US Representative with any blue-collar work experiences than are non-RTW states in the Carnes data, following Members of Congress from 1999 to 2008 (Panel A; recall that slightly over 6 percent of the House had a working-class background over this period). Shares of years in blue-collar or working-class occupations also fall after RTW is passed (Panel B). Looking at the earlier period from 1980 to 1996, we find that RTW states similarly are less likely to elect blue-collar Representatives: RTW states were about 0.4 to 0.8 percentage points less likely to elect a Representative who had worked in a blue-collar job immediately before joining Congress (Panel C). While that may not seem like a large effect on its face, recall that only about 1% of the House had such work experience over this period. Though noisy, the effects of RTW laws on blue-collar backgrounds are negative for both Democrats and Republicans across most of our specifications.

Taken together, we find evidence at both the state and federal level that RTW laws—by weakening unions—diminished the representation of working-class Americans in elected office. With fewer working-class politicians in office, RTW states and the Congress as a whole may be less likely to pursue redistributive economic policies, a question we turn to in the following subsection.

5.2 RTW Laws Reduce State Policy Liberalism

By weakening the relationship between Democrats and unions, we anticipate that RTW laws will drive state policy—including but not restricted to labor policies—in a rightward direction. This rightward shift could be the product of the direct electoral effects of RTW laws: by favoring the election of GOP candidates to state legislatures and governorships, states with RTW laws in place will be more likely to have partially or fully Republican-controlled governments. But RTW laws should also move policy to the right even when states are fully or partially controlled by Democrats. With labor unions a less central member of the Democratic party coalition, we expect that Democrats will have less reason to pursue left-leaning economic policies favored by labor unions (e.g. Bawn et al. 2012). And to the extent that RTW laws make it harder for working-class state legislative candidates to win office, as we documented in the previous section, that should also move state policy to the right.

[Figure 9 about here.]

Using the Caughey and Warshaw (2016) measure of state policy ideology, we find that average state policy liberalism falls following the passage of RTW laws, indicating that state policies tend to move right, as we show in Figure 9. In that figure, we plot Caughey and Warshaw (2016)'s estimates of state policy liberalism, available from 1980 through 2014, against indicators for years before and after the passage of state RTW laws. These measures use a dynamic latent-variable model on

148 state-level policies to produce an estimate of the overall ideological tilt of state policies. The Caughey and Warshaw dataset includes RTW laws, so the passage of RTW laws could themselves be mechanically driving some of the effect we observe. The right plot of Figure 9 excludes state RTW laws from the estimation of state policy liberalism scores and shows a nearly identical pattern: after the passage of state RTW laws, state policy moves in a much more conservative direction.[33]

The size of the post-RTW rightward shift in state policy is sizable and substantively relevant. The difference implied by the right panel of Figure 9 is a shift of 1.49 units on the state policy liberalism scale, which is more than a standard deviation of change in state policy liberalism from 1980 to 2016. It also roughly corresponds to the average difference in state policy liberalism over this period between Connecticut and West Virginia—two states that have taken very different directions in governance over the past three decades. While our difference in differences analysis is not as strong as our county-border design and we are careful in interpreting these effects, they are consistent with our theoretical expectation that state policy should shift rightward following the passage of RTW laws (see Appendix Table A.9 for full regression specifications).

We also find similar effects using an alternative measure of state policy liberalism produced by Grumbach (2017), who uses an additive index of substantively important liberal and conservative policies. One advantage to this scale is that we can easily separate out social and economic policies. Doing so, we see that ideological liberalism of both social and economic policies falls after the passage of RTW laws (see Figure A.8 in the Appendix).

Lastly, we examined the effect of RTW laws on individual state policy outcomes that we think unions might plausibly shape, settling on three different sets of policies: state and local minimum wages; prevailing wage laws; and top income tax rates on individuals and corporations. We see prevailing wage laws and minimum wage laws as issues that are closer to unions' core political interests, while top-end tax rates are a more distant concern, especially for private-sector unions. Prevailing wage laws require government contractors to pay local labor market wage and benefit rates, thereby ensuring that union-won gains in working standards are not undercut by public works projects. Minimum wages serve a similar role for unions, making it harder for non-union employers to undercut unionized firms. In contrast, top income tax rates are only indirectly related to unions' political objectives.

Across state-year OLS regressions shown in Table 8, we see noisy but negative correlations between the passage of RTW laws and minimum wages as well as prevailing wages. In Panel A, we see that RTW states are less likely to have state-level minimum wage rates that exceed the federal level, as well as city-level minimum wages that exceed the state level, following the passage of RTW laws. The results in Panel B suggest a similar relationship between RTW laws and prevailing wage statutes, though again with a considerable degree of uncertainty in our coefficient estimates. Lastly, we find only weak and inconsistent results for tax rates. There is no clear relationship between the passage of RTW laws and top individual state income tax rates, and a positive correlation with

[33]This analysis also provides an important test for our main argument, showing that there are not clear pre-trends in policy liberalism *before* the passage of state RTW laws.

top corporate tax rates. In sum, we view the results in Table 8 as suggestive of the effect of RTW laws on policies of particular concern to the labor movement, though we reiterate that we are more confident in our results when looking at aggregate measures of state policy ideology in Figure 9.

[Table 8 about here.]

6 RTW and the Membership-Politics Tradeoff

The focus of our paper is the effect of RTW laws—and, through them, unions—on politics. In this final section, we present quantitative and qualitative evidence suggesting that RTW laws weaken unions' involvement in politics by shrinking union revenues and forcing unions to choose between membership recruitment and retention and broader political mobilization. Past research on RTW laws has failed to nail down a clear causal relationship between the passage of RTW laws and overall union membership. As Barry Eidlin sums up in his review of the literature, "the evidence shows that there is a clear relationship between low union density and RTW laws. However, it is unclear whether this is because RTW laws cause low union density, or because low-density states are more likely to adopt RTW laws" (Eidlin 2018, p. 90). Instead, we hypothesize that RTW laws force unions to reallocate scarce internal resources, including staff time and money, between politics and membership recruitment, as they can no longer count on a steady stream of dues from all workers they represent. Because RTW laws eliminate the requirement that workers pay dues to unions, even if that union represents the workers through collective bargaining agreements and grievance protections, unions have to expend additional effort to sign up workers as voluntary members to collect their dues and retain the same financial resources they possessed before RTW. RTW laws both make resources scarcer and push unions to direct spending away from politics.

To study the direct effects of RTW laws on union budgets—in particular, the relationship between individual unions' political spending and their efforts on membership recruitment and retention—we turn to data from LM-2 forms, filed by unions with the Department of Labor. The LM-2 forms represent a useful but somewhat limited source of information. Ideally, we would examine data on union operations before and after the passage of RTW laws. Unfortunately, publicly accessible data on unions is limited to the information disclosed on LM-2 forms, which all but the smallest private sector unions must file with the Department of Labor each year.[34] We focus on data on union political and membership representation spending available from 2006 to 2016.

How is spending on representational or political activities defined on the LM-2 forms? The Department of Labor considers representational spending to cover "direct and indirect disbursements ... associated with ... the negotiation of collective bargaining agreements and the administration and enforcement of the agreements made by the labor organization" as well as "disbursements associated with efforts to become the exclusive bargaining representative for any unit of employees, or to keep from losing a unit in a decertification election or to another labor organization, or to

[34] Public sector unions are not directly covered by this law, but if a union of any type represents private sector workers, their information appears in this database, including unions with mostly public sector members.

recruit new members." Political spending covers "direct and indirect disbursements ... associated with political disbursements or contributions in money" as well as "disbursements ... dealing with the executive and legislative branches of the Federal, state, and local governments and with independent agencies and staffs to advance the passage or defeat of existing or potential laws or the promulgation or any other action with respect to rules or regulations." We believe these two categories nicely capture spending related to core union representational and membership activities, as well as elections and lobbying. The remaining categories in the LM-2 filings include contributions, gifts, and grants unrelated to politics or representation, general overhead, union administration, and benefits.

We observe that spending on political involvement and membership services by unions are negatively correlated (Figure 10), aligning with our hypothesis about the resource trade-off between these two budget items of union effort. Private sector unions that expend more resources on representational activities—recruiting and servicing their members—tend to spend less on politics. That means that when unions in RTW states have to reallocate more effort to representational activities following the passage of the law, they are likely to spend less on the political activities—voter mobilization, candidate recruitment, lobbying, and campaign spending—that we summarized above.

[Figure 10 about here.]

We can see the effects of RTW laws on union revenue overall and political spending quite clearly in the experience of Michigan's unions. Because RTW passed in Michigan in 2012, we have sufficient LM-2 data both before and after the law to describe clear trends. Among eight large and politically important unions in Michigan—the UAW, SEIU, NEA, IUOE (Operating Engineers), CJA (Carpenters), IBT (Teamsters), AFT, and AFSCME—union revenue fell nearly 10% relative to 2011, before RTW. The declines were especially severe for the unions representing mostly or exclusively public sector workers. SEIU revenue fell by half in the few years after RTW and and then dropped to 14.5% of pre-RTW levels four years out, while revenue for AFSCME in Michigan decreased 40%. Michigan's teachers unions—the NEA and AFT—both saw revenue decline more than 10%.[35] These results echo similar declines in teachers union revenue after a collective bargaining ban in Tennessee (Quinby 2017).

The Michigan unions also redirected spending away from politics after RTW. Political spending fell for all but one of the eight unions in the four years after RTW relative to spending the year before RTW passed.[36] This suggests that, facing strain from a RTW law, the unions reallocated newly scarce resources away from politics and to membership recruitment, retention, and servicing.

The membership-politics tradeoff hypothesis also finds support in interviews we have conducted with labor leaders in states that have passed RTW in recent years. In Indiana, for instance, the head

[35]UAW revenue was up slightly over this period but general recovery of the auto sector after the Great Recession likely played a role.

[36]Spending three and four years after RTW on politics grew substantially for the relatively small carpenter-representing CJA. However, in 2011, the year before RTW in Michigan, the CJA spent only $515,000 dollars on politics, dwarfed by spending by the UAW ($11.9 million), NEA ($7.8M), SEIU ($5.3M), and AFSCME ($3.9M).

of the AFL-CIO reported that as a result of the pressure from RTW, their unions "are trying to do more one-on-one education, not necessarily political [work] ... [We're] trying to put our money back into our membership." Similarly, he explained that "in the beginning ... we had to allocate a lot more resources and money into marketing and education [relative to other priorities] ... once [right-to-work] had passed." He also argued that unions in his state that did not engage in extensive membership recruitment and education efforts tended to tended to suffer the most after RTW: "some unions did not do that [membership education], and so they're playing catch up." Examples of the extra effort that unions allocated to membership retention include traditional educational activities, like explaining to members why strong unions are in their interest. But some unions were even more creative. One union, for instance, which represents grocery store workers, many of them younger adults, underscored how membership included discounts on things like cell phones and also partially subsidized college tuition. Kentucky's AFL-CIO head summarized the situation succinctly: "We've got a lot of folks doing a lot more internal organizing ... We're going to educate and inform our members and let them understand the importance of the union contract." These interviews also suggest an explanation for why we begin to observe RTW effects in the elections immediately following the passage of those laws: as soon as these laws pass the legislature, unions begin reallocating resources away from politics to member retention and education to ensure that the laws do not have an effect on the union's membership rolls, even before provisions in union contracts governing agency fees change.

In addition to these interviews, we have found evidence for unions' increased focus on membership recruitment and retention following the passage of RTW in the media. One account of Wisconsin locals in *Labor Notes*, for instance, described the post-RTW landscape in the state as follows:

> Two UE locals in Wisconsin recently bargained their first contracts under the state's 2015 right-to-work law. In each case local leaders understood that, in order to maintain membership under the new contract, they would have to put on a real fight that involved everyone, so that everyone in the shop understood the union's value. Local 1107 in Necedah responded to the threat of impending open shop by pulling out all the stops to reverse a divisive two-tier wage scale that had been imposed on members during the 2009 downturn in the auto-parts business. This meant months of preparation in the shop and at membership meetings, keeping members regularly informed of what was happening at the bargaining table. T-shirt days, creative protests involving penny jars and Legos, and rallies and marches in the shop and at the bargaining location united older and younger workers, many of whom had never participated in a rally before.[37]

Especially in recent decades, workers in RTW states are not left to discover that they can opt out of unions while still benefiting from unions' collective bargaining and grievance protections on their own. Many of the same conservative political groups that have helped to champion RTW laws have run publicity campaigns following the passage of RTW to let workers at unionized employers know their opt-out rights. Employee Freedom Week, for instance, is a national campaign intended

[37]Kissam, Johnathan. "Viewpoint: Unions Are Class Organizations, and Should Act Like It." *Labor Notes* April 27, 2018

"to inform union employees of the freedom they have regarding opting out of union membership and making the decision about union membership that's best for them," according to the group's website.[38] In-state advocacy groups have also made publicizing employee opt-out rights a priority, too. The Mackinac Center, a free-market think tank in Michigan that helped push for RTW in that state, now runs a website that provides easy-to-use legal documents and FAQs for workers looking for information about opting out of union membership and fees.[39] Given their heft in state politics, public employee unions are often a primary target for opt-out campaigns. Reporters at the *Guardian*, for example, uncovered documents from one conservative group arguing that "well run opt-out campaigns [following RTW campaigns] can cause public-sector unions to experience 5 to 20% declines in membership, costing hundreds of thousands or even millions of dollars in dues money. This can affect the resources and attention available for union leaders to devote to political action campaigns."[40] Accordingly, that group called for a large-scale effort in recent RTW states. It is clear that unions in both the public and private sectors face considerable pressure following the passage of RTW laws to maintain their membership and resources—both from members themselves and outside conservative groups.

In sum, the qualitative evidence suggests that as RTW laws squeeze union budgets, unions shift their focus away from politics into membership organizing and retention. This resource trade-off helps to explain the results that we have shown throughout this paper.

7 Conclusion

The study of labor unions by economists has a lengthy history, dating back at least to Adam Smith. Most of this literature has focused on the direct economic effects of unions on firm performance, workers' wages, and the distribution of income across businesses, industries, and society. Yet, as our opening quotes from 20th century labor leaders Walter Reuter and Sidney Hillman reveal, at least since the New Deal labor unions in the United States have not seen themselves as exclusively economic actors (see especially Lichtenstein 2013). Instead, most unions view political activities—through member education, elections, and lobbying—as a vital complement to their bread-and-butter collective bargaining. As a result, any accounting of the "union effect" is insufficient if it does not also capture the labor movement's political mobilization. As Acemoglu and Robinson (2013) lamented, "politics is largely absent from the scene (p. 173)" when economists consider market failures created by institutions like labor unions. Their focus on labor unions is notable; Acemoglu and Robinson (2013) stress how "unions do not just influence the way the labor market functions; they also have important implications for the political system ... Historically, unions have played a key role in the creation of democracy in many parts of the world [and] they have founded, funded, and supported political parties ... which have had large effects on public policy and on the extent of taxation and income redistribution, often balancing the political power of established business elites (p. 174)."

[38]http://employeefreedomweek.com/press-center/

[39]https://michiganunionoptout.com/

[40]Pilkington, Ed. "Exclusive: how rightwing groups wield secret 'toolkit' to plot against US unions." *The Guardian* May 15, 2018

In this paper, we have taken up the call in Acemoglu and Robinson (2013) to consider the political effects of unions and identified the contribution of contemporary American labor unions to elections and policymaking by leveraging the passage of RTW laws from 1980 to 2016. Comparing otherwise similar counties straddling state (and RTW) borders, we find that the passage of RTW laws led Democratic candidates up and down the ballot to receive fewer votes. In Presidential elections, Democratic vote share fell about 3.5 percentage points following the passage of RTW laws in the counties on the RTW side of the border. RTW laws also lower turnout in both federal and state races. Further survey-based analysis revealed that working class Americans (but not professional workers) were less likely to report get-out-the-vote contact in RTW states following the passage of RTW laws, suggesting that weakened unions have less capacity for turning out Democratic voters. And we showed that union fundraising for state and local races (and Democratic funding in general) falls sharply following the passage of RTW laws.

We also document how the effects of RTW laws—and through them, weakened unions—reverberate beyond election returns. Working-class candidates—the politicians most likely to be backed by the labor movement—are less likely to hold federal and state office following the passage of RTW laws. State policy as a whole, moreover, moves to the ideological right in RTW states following the passage of those laws. In sum, we show how beyond the bargaining table, unions affect inequality through the ballot box, through the politicians and policies they support. Moreover, our paper suggests that the capacity of unions to affect the labor market and the income distribution through this second channel may be waning as labor's strength—and political clout—diminishes in the face of unfavorable policies, such as RTW laws or the recent Supreme Court decision in *Janus*.

Aside from the theoretical contributions of the paper, our results also have bearing on current debates in U.S. politics. The anti-tax political activist Grover Norquist recently declared that while President Donald J. Trump may be historically unpopular, the GOP could still "win big" in 2020.[41] The secret to the Republican party's long-term success, Norquist argued, involved state-level initiatives to weaken the power of labor unions. As Norquist explained, if union reforms curtailing the power of labor unions to recruit and retain members—like RTW laws—"are enacted in a dozen more states, the modern Democratic Party will cease to be a competitive power in American politics." A weaker labor movement, he reasoned, would not just have economic consequences. It would also generate significant political repercussions, meaning that Democrats would have substantially less of a grassroots presence on the ground during elections and less money to invest in politics.

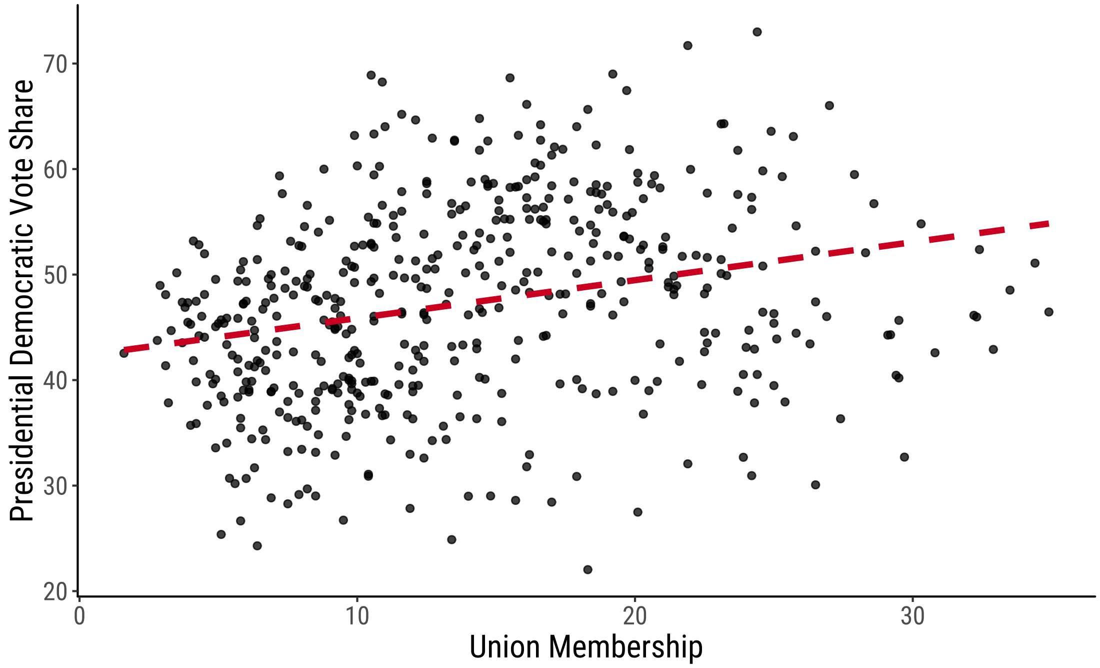
Norquist's theory is also shared by the state-level conservative activists who have been driving the recent push to enact additional RTW laws in newly GOP-controlled state governments—even as public opinion toward RTW laws is mixed.[42] Tracie Sharp runs a national network of state-level

[41]Norquist, Grover. "Why Republicans (and Trump) may still win big in 2020 despite Everything." *OZY* May 28, 2017.

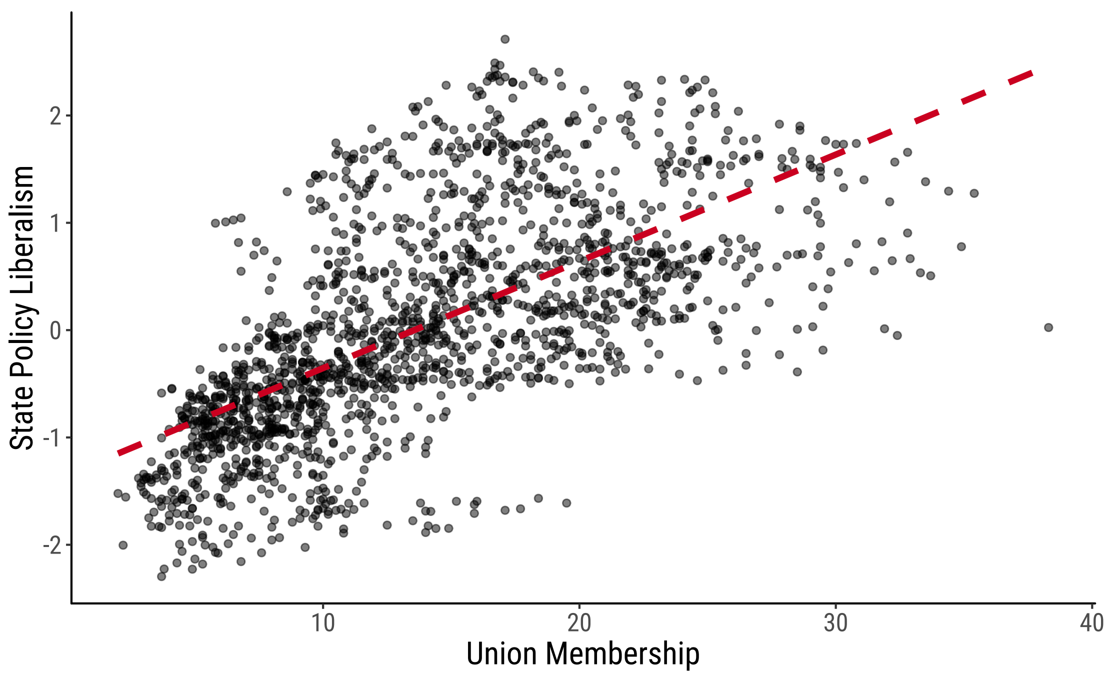
[42]In 2014, nearly half of American adults said that they had not heard of "right-to-work" or "open shop" laws. Question text: "Have you heard of state laws called right-to-work or 'open shop' laws?" Gallup Poll, August 2014. Extracted from the Roper Center Archive: USGALLUP.082814.R21.

conservative think-tanks that have championed the passage of RTW laws in recent years in states such as Michigan, Kentucky, Missouri, and Wisconsin (Hertel-Fernandez 2019). In an interview with the *Wall Street Journal*, Sharp explained why she was optimistic about the long-run effects of her network's push against the labor movement, explaining that "When you chip away at one of the [liberal] power sources that also does a lot of get-out-the-vote . . . I think that helps [conservative activists and GOP politicians]—for sure."[43] Internal documents from Sharp's organization provide an even clearer message: by passing RTW laws, the work of conservative organizations like hers was "permanently depriving the Left from access to millions of dollars . . . every election cycle." That meant dealing "a major blow to the Left's ability to control government at the state and national levels."[44] Our paper suggests that these arguments by conservative activists may well be accurate. RTW laws, among other measures intended to weaken the labor movement, may durably shift the balance of political power and drive rising inequality in the United States for years to come.

[43]Peterson, Kyle. "The Spoils of the Republican State Conquest." *The Wall Street Journal* December 9, 2016.
[44]Hertel-Fernandez (2019), chapter 6.



(a) State Democratic Vote Share and Union Membership



(b) State Policy Liberalism and Union Membership

Figure 1: States with higher union density are more Democratic and more liberal. We plot the positive correlation between state Democratic vote share for President and state union density, 1980-2016, and state policy liberalism and state union density, 1980-2014. Source of Democratic vote share is the U.S. Election Atlas. Union density from Hirsch et al. (2001), state policy liberalism from Caughey and Warshaw (2016).

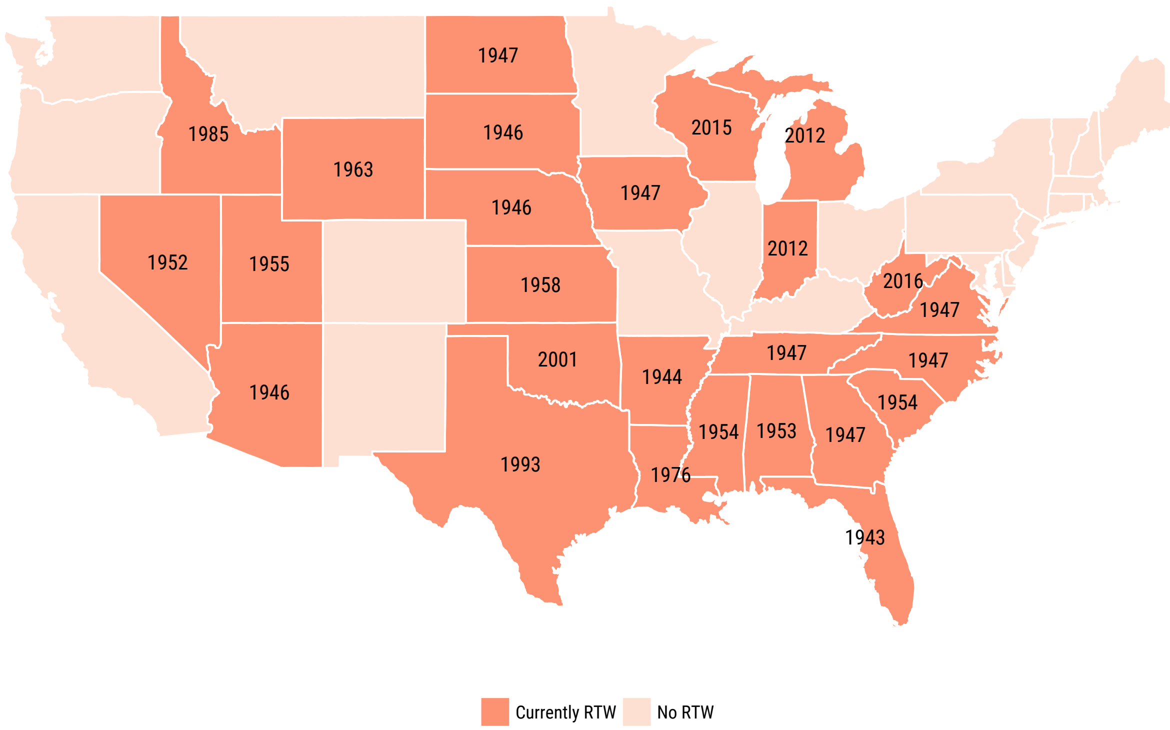


Figure 2: U.S. state right-to-work laws as of 2016. Years indicated on the map are the first year RTW was implemented in each state. Note that Indiana had RTW in place from 1957 to 1965 before passing RTW again in 2012. In 2017, after our study period, Kentucky and Missouri both passed RTW laws.

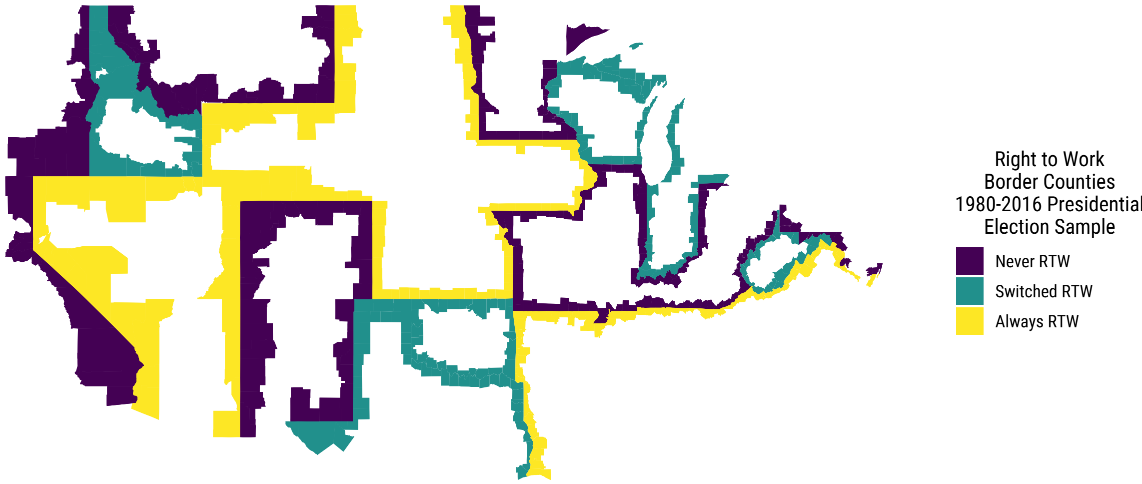


Figure 3: Border county pairs in our main specifications examining the effect of state right-to-work laws on Presidential vote shares, 1980-2016.

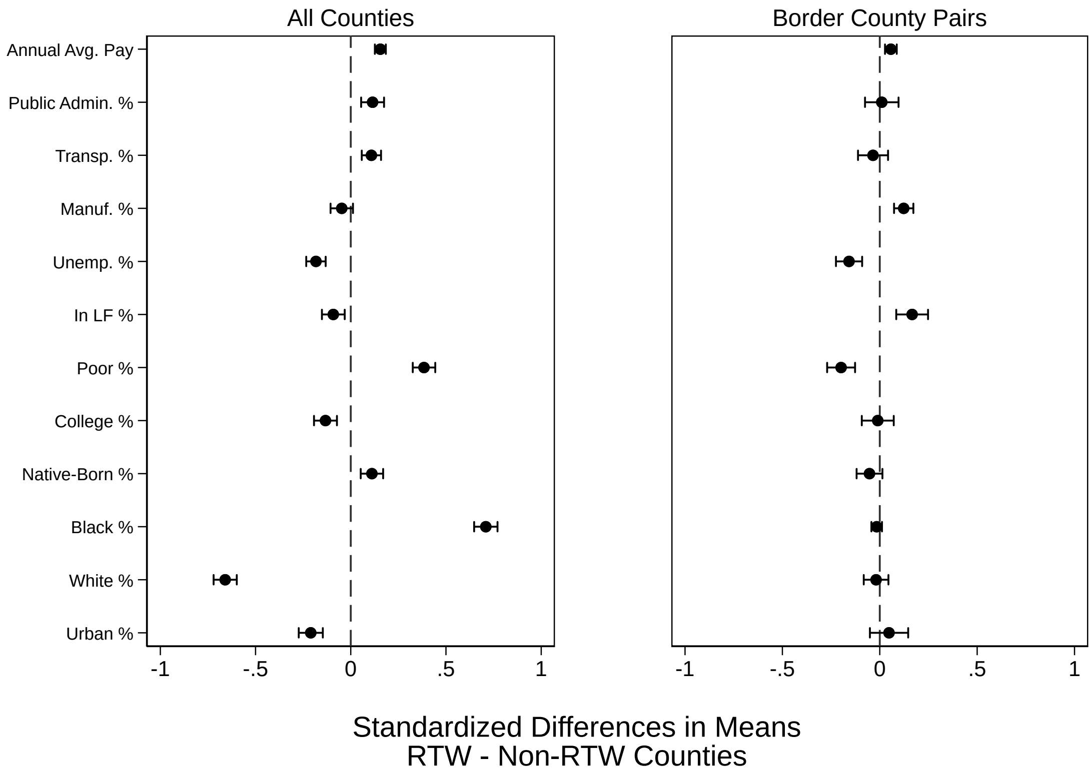


Figure 4: Balance on covariates between right-to-work and non-right-to-work counties, examining all counties (left plot) and only border county pairs (right plot). Full 1980-2016 sample. Lines indicate 95% confidence intervals. Standard errors clustered by county in the "All Counties" analysis and by border county pair in the "Border County Pairs" analysis. Border County Pairs analysis includes state border effects. Data from the US Census for 1980, 1990, 2000, and 2010. Intercensal values carried forward from previous census year. This figure shows the coefficient and associated standard errors of bivariate OLS regressions of various Census outcomes on a RTW indicator.

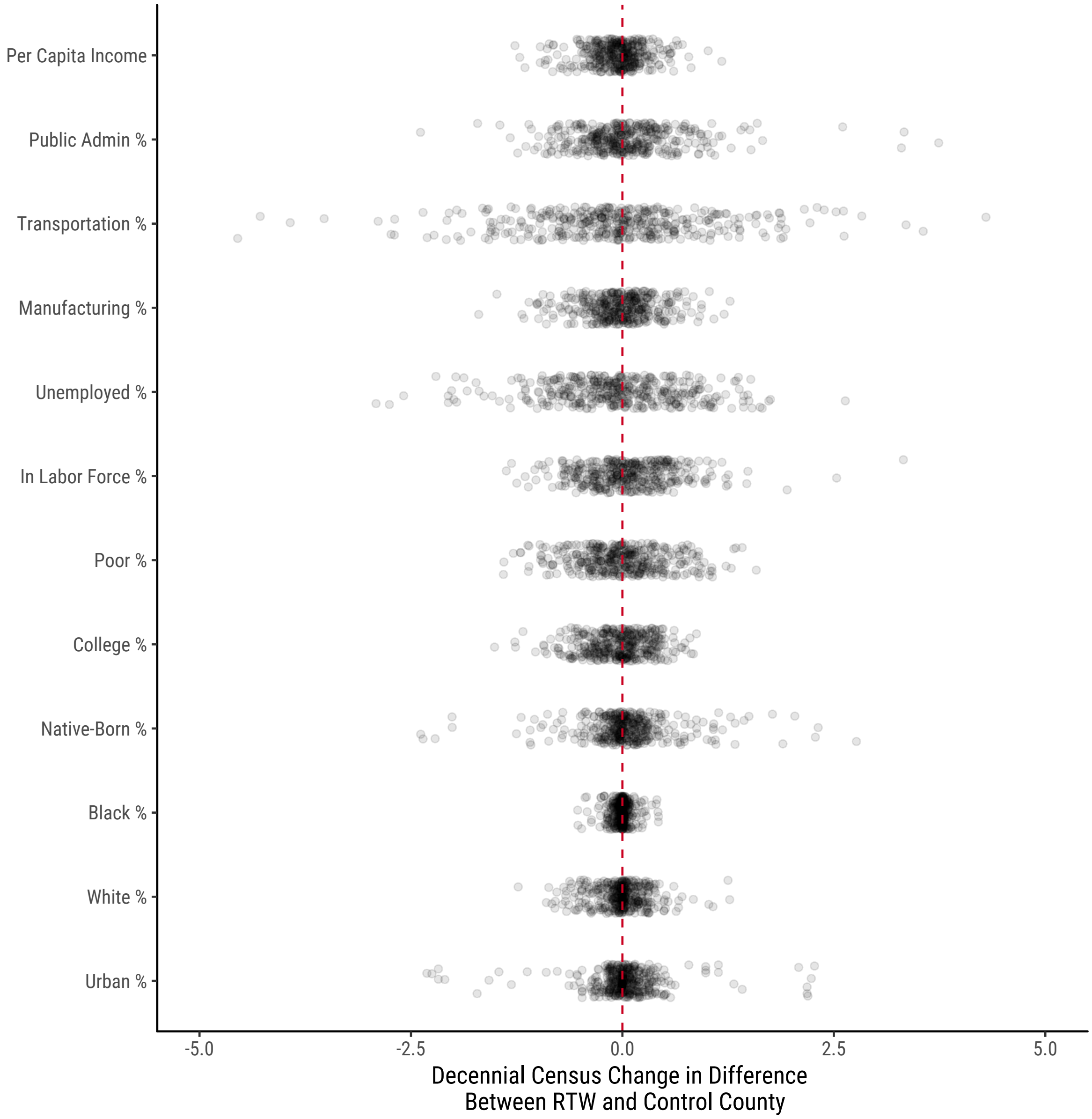


Figure 5: Balance on changes in covariates between right-to-work and non-right-to-work counties, comparing changes between the two decennial censuses taken before RTW was passed. Full 1980-2016 sample. Each point is the difference between a RTW county and a non-RTW county. Data from the US Census for 1980, 1990, 2000, and 2010.

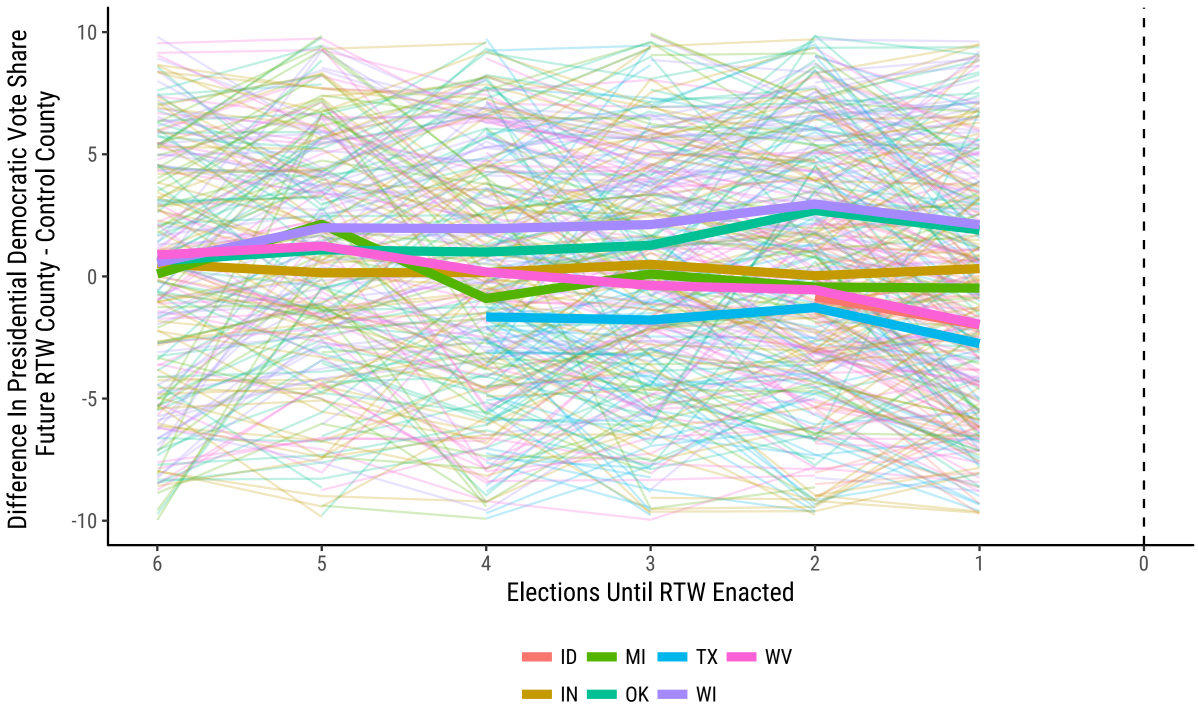


Figure 6: No Cross-RTW Border Trend in Democratic Vote Share. We plot the difference in Democratic vote share in Presidential elections between counties in states that will eventually enact RTW and their control—always RTW or never RTW—neighbors across the state border. The vertical dashed line indicates the first election cycle after RTW is enacted in the treatment counties. The light background lines are each county border pair, the darker lines represent simple state-level averages. In Figure A.3, we show similarly that turnout is not trending before RTW is enacted.

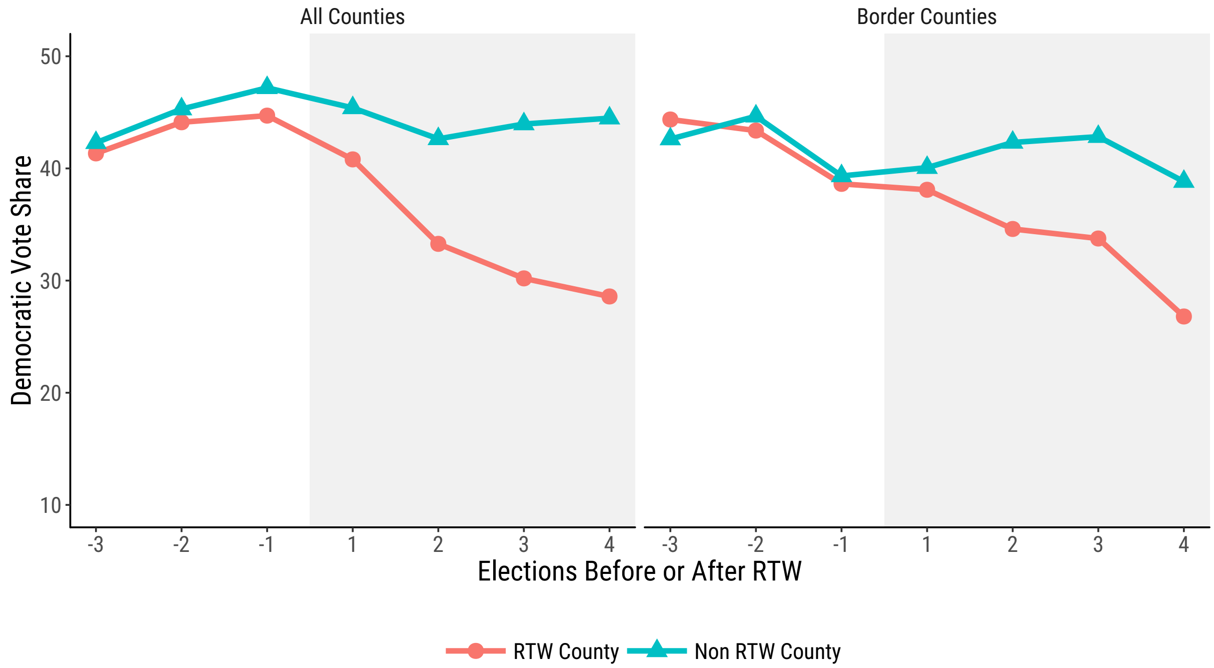


Figure 7: Difference-in-differences summary analysis of right-to-work laws and Democratic vote share, 1980-2016. Left plot examines all counties and right plot examines only border county pairs. Examining the border county sample, we see that Democratic vote share was very similar in the elections before RTW passed but diverges after RTW, beginning in the very first election after RTW and growing in magnitude over time, possibly as unions weaken or substitute out of political activity.

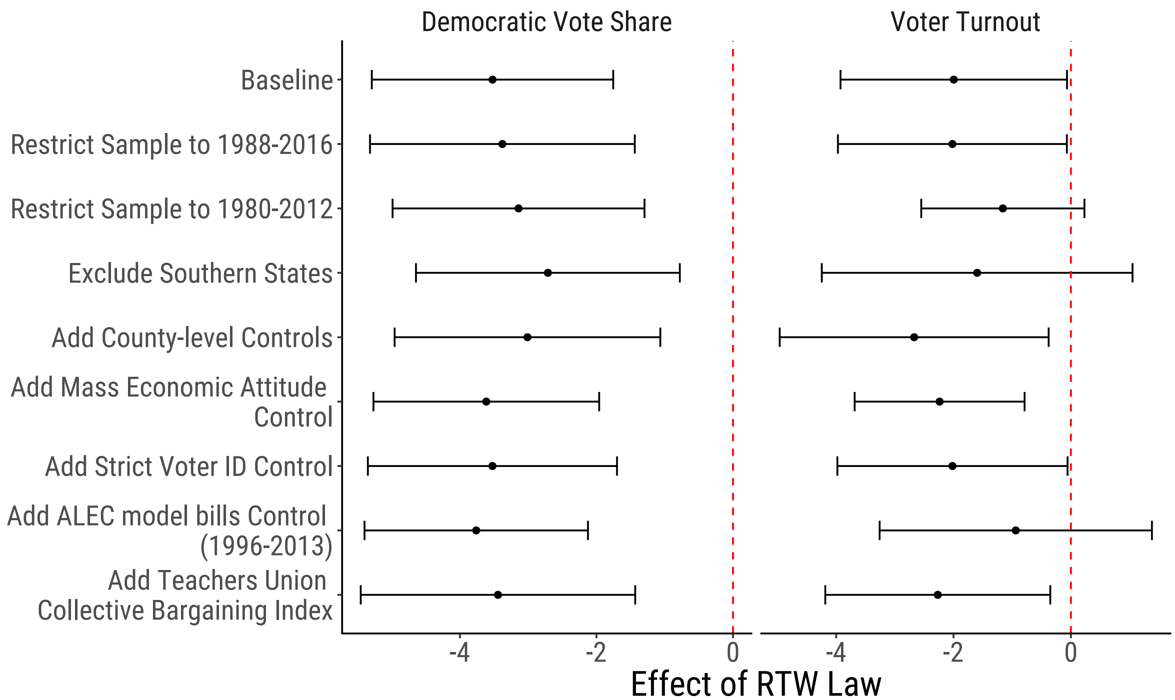
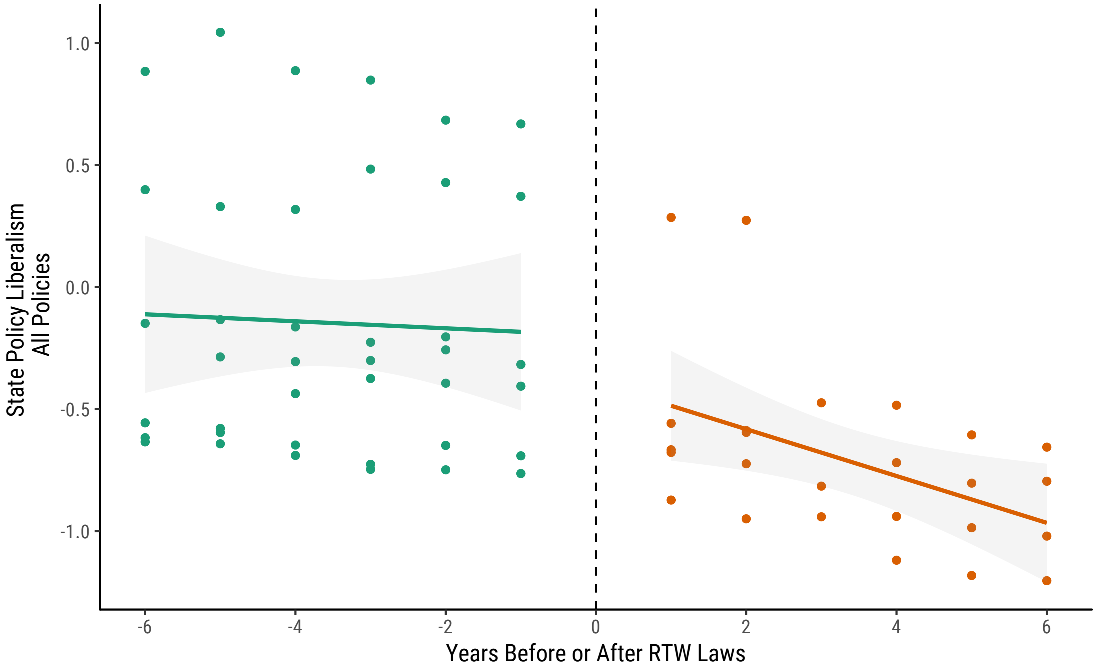
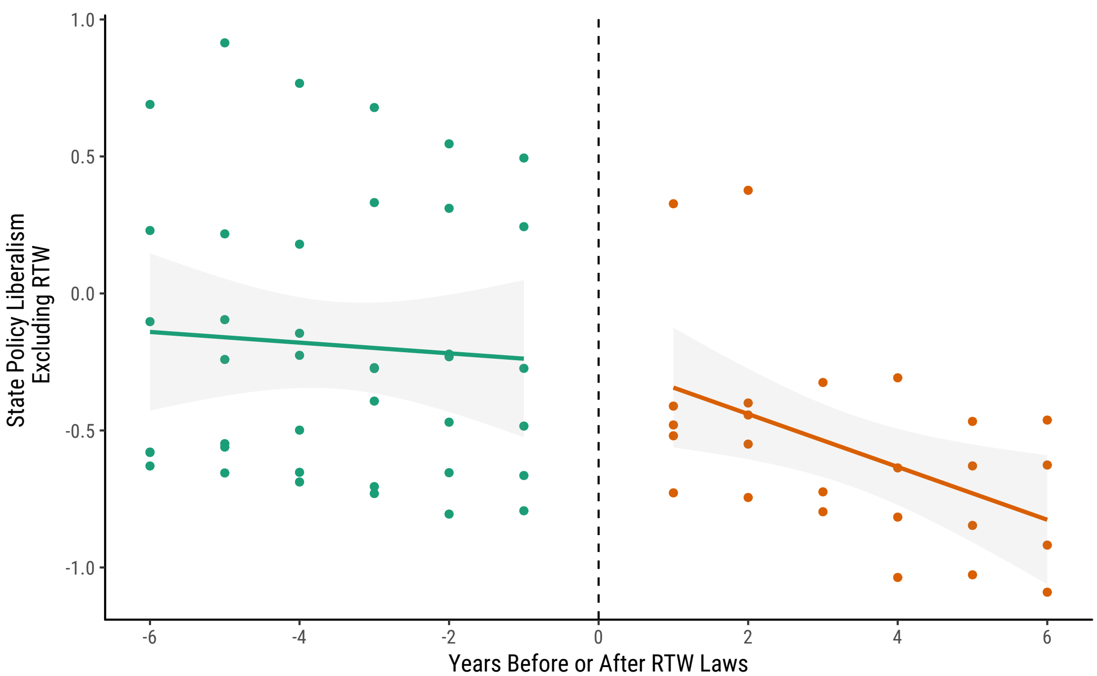


Figure 8: The effect of state right-to-work laws on Presidential elections, robustness checks. All models include county and border pair by year fixed effects. We cluster standard errors two ways, by border pairs and by state. The sample includes only counties on state borders. Both vote share and turnout measured on 0-100 scale.



(a) State Policy Liberalism, All



(b) State Policy Liberalism, Non-RTW

Figure 9: Relationship between state policy liberalism and state right-to-work laws, 1980-2014. Figure plots median state policy liberalism in states before and after the passage of right-to-work laws. Left plot includes all policies, right plot excludes right-to-work laws from the estimation of state policy liberalism. Gray shading indicates 95% confidence intervals. State policy liberalism from Caughey and Warshaw (2016).

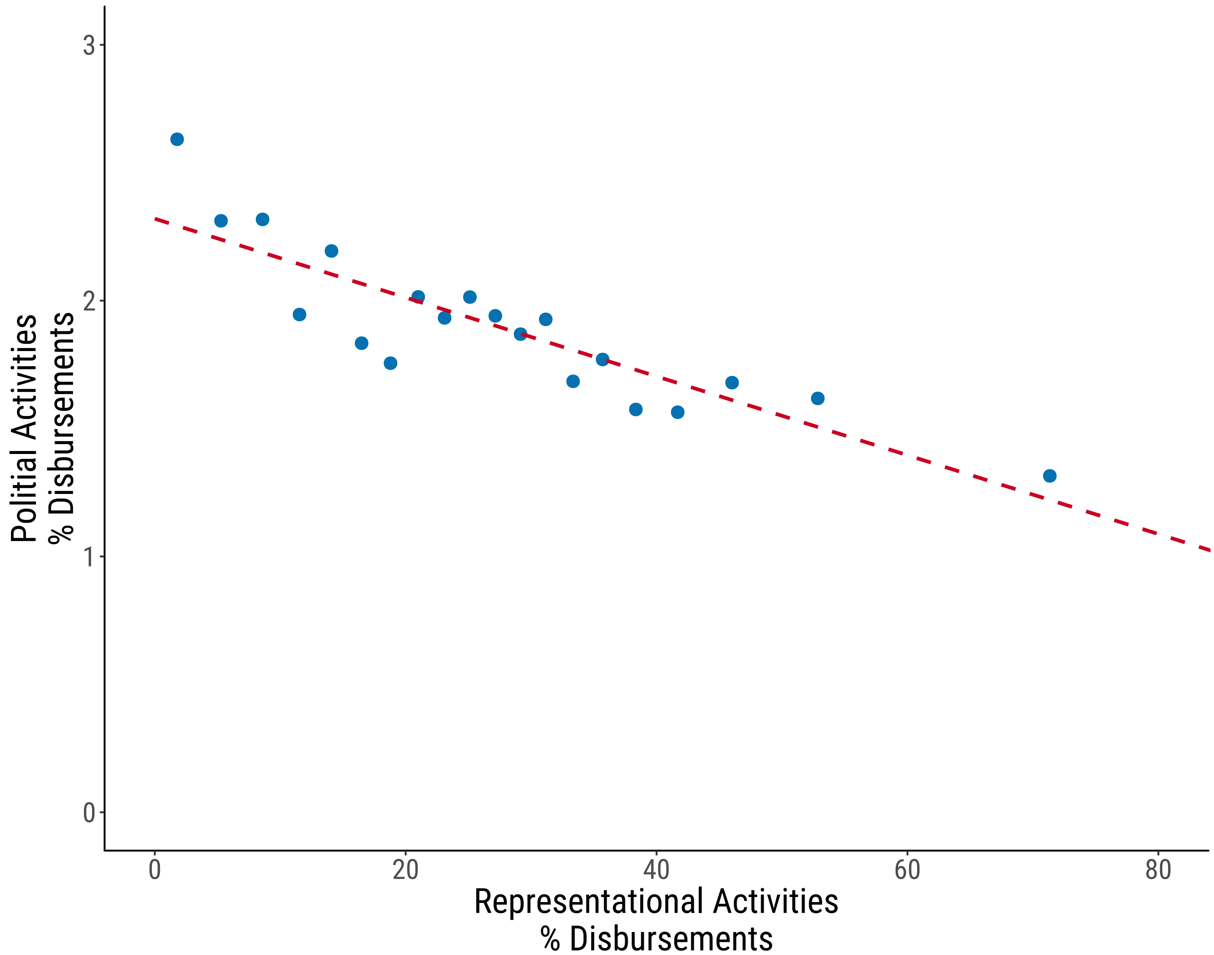


Figure 10: Binned scatterplot between union spending on political versus membership representational activities, both expressed as a share of total union disbursements, 2006-2016. With 20 bins, each point represents 5% of the data. Data from authors' analysis of LM-2 disclosure forms from the Department of Labor. The remaining categories in the LM-2 filings include contributions, gifts, and grants unrelated to politics or representation, general overhead, union administration, and benefits. We omit unions that reported spending nothing on representational activities.

Table 1: The Effect of State Right-to-Work Laws on Presidential Elections

	Panel A. Democratic Vote Share						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	−4.894	−6.277	−3.858	−5.579	−6.318	−5.093	−3.523
	(1.725)	(2.267)	(1.391)	(1.721)	(2.497)	(1.403)	(0.902)
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	Yes	No	No
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	30625	30625	30625	25494	25494	25494	25494
Adjusted R^2	0.03	0.77	0.83	0.04	0.80	0.86	0.92
Y Mean	41.70	41.70	41.70	41.80	41.80	41.80	41.80

	Panel B. Voter Turnout						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	−3.379	−3.989	−2.334	−2.957	−3.364	−2.662	−2.019
	(1.795)	(0.850)	(0.851)	(1.754)	(1.101)	(0.945)	(0.995)
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	Yes	No	No
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	30616	30616	30616	25483	25483	25483	25483
Adjusted R^2	0.03	0.77	0.81	0.02	0.78	0.81	0.83
Y Mean	57.04	57.04	57.04	56.88	56.88	56.88	56.88

Note: Standard errors clustered by state in the all county sample and clustered two-way by state and border-pair in the border county sample. Vote share outcomes are measured 0 to 100 percent. Following Dube et al. (2010), in the border sample we allow counties bordering multiple other counties to pair with each and stack the data accordingly, which is why the 1173 unique counties on a state border translate to more than 25,000 observations with 10 years of election data. The county border pair fixed effect (subsumed by the border pair by year fixed effect) identifies each separate pair, and we cluster at the border-pair level to account for repeated observations. In Table A.4, we reweight the analysis such that each county on a border weighs equally, whether or not the county pairs with 1, 2, or 3 or more counties across the state border. The results are robust to reweighting as well as to imposing any arbitrary selection of unique county pairs (Figure A.4).

Table 2: The Effect of State Right-to-Work Laws on Elections

	Panel A. Democratic Vote Share					
	Presidential	Senate	Governor	House of Representatives		
				All Years	On Cycle	Off Cycle
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−3.523	−3.331	−2.450	−5.414	−3.666	−8.395
	(0.902)	(3.802)	(3.441)	(4.189)	(4.358)	(4.971)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Border Pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25494	32445	23832	48629	25490	23139
Adjusted R^2	0.92	0.52	0.47	0.59	0.58	0.58
Y Mean	41.80	45.16	46.79	44.86	44.71	45.04
	Panel B. Voter Turnout					
	Presidential	Senate	Governor	House of Representatives		
				All Years	On Cycle	Off Cycle
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−2.019	−2.198	−1.120	−2.513	−2.435	−2.666
	(0.995)	(1.380)	(2.194)	(1.005)	(0.905)	(1.999)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Border Pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25483	32430	23821	48614	25486	23128
Adjusted R^2	0.83	0.81	0.86	0.77	0.69	0.70
Y Mean	56.88	48.69	45.53	48.18	55.32	40.31

Note: Sample limited to counties on state borders. Standard errors clustered two-way by state and border-pair. Vote share and turnout outcomes are measured 0 to 100 percent. Following Dube et al. (2010), we allow counties bordering multiple other counties to pair with each and we stack the data accordingly, which is why the 1173 unique counties on a state border translate to more than 25,000 observations with election data from 1980 to 2016 for the Presidential sample. The county border pair fixed effect (subsumed by the border pair by year fixed effect) identifies each separate pair, and we cluster at the border-pair level to account for repeated observations. In Table A.4, we reweight the analysis such that each county on a border weighs equally, whether or not the county pairs with 1, 2, or 3 or more counties across the state border. The results are robust to reweighting as well as to imposing any arbitrary selection of unique county pairs (Figure A.4).

Table 3: The Effect of State Right-to-Work Laws on Democratic State Legislative Seat Shares, 1980-2016

	Share in Both Houses		State Senate		State House	
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−11.009	−5.088	−14.921	−10.331	−10.057	−3.387
	(4.062)	(2.334)	(4.465)	(3.185)	(4.315)	(2.175)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes	No
Census Division × Year FE	No	Yes	No	Yes	No	Yes
Observations	1813	1813	1813	1813	1813	1813
Adjusted R^2	0.76	0.87	0.72	0.83	0.76	0.86
Y Mean	54.35	54.35	54.29	54.29	54.16	54.16

Note: Standard errors clustered by state. Seat share outcomes are measured 0 to 100 percent. Unicameral Nebraska is not included in our sample, leaving 49 states in 37 years for 1813 observations.

Table 4: The Effect of State Right-to-Work Laws on GOTV Contact, 1980-2012

	All Election Years		Presidential Election Year		Non-Presidential Election Year	
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−0.016	−0.002	−0.023	−0.024	−0.053	−0.014
	(0.044)	(0.043)	(0.051)	(0.051)	(0.021)	(0.029)
Non-Professional Worker	0.010	0.022	0.016	0.015	−0.013	0.021
	(0.012)	(0.016)	(0.017)	(0.023)	(0.014)	(0.018)
RTW × Non-Professional	−0.103	−0.112	−0.129	−0.123	−0.027	−0.053
	(0.031)	(0.031)	(0.038)	(0.038)	(0.022)	(0.024)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Observations	15,156	12,424	9,820	8,408	5,336	4,016
Adjusted R^2	0.037	0.054	0.022	0.044	0.061	0.066

Note: Standard errors clustered by state. Linear probability model where the outcome is whether or not the individual surveyed in the ANES reported being contacted about registering to vote or getting out to vote. Individual controls include age and age squared, gender, education (high school or less, some college, or college or more; high school or less is the excluded category), indicators for race and ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and other; other is the excluded category), church attendance (in five categories of frequency), interest in political campaigns (in three categories), a dummy variable for strong partisanship, and union membership. Data from National Election Studies.

Table 5: The Effect of State Right-to-Work Laws on State and Local Campaign Contributions, 1996-2016

	Share of Campaign Contributions from Unions				Party Share of Contributions	
	Private Sector Unions		All Unions		Share to Democrats	
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−1.255	−2.336	−2.526	−3.095	−7.437	0.909
	(0.449)	(1.235)	(0.627)	(1.614)	(3.432)	(4.764)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes	No
Census Division × Year FE	No	Yes	No	Yes	No	Yes
Observations	671	671	659	659	641	641
Adjusted R^2	0.51	0.59	0.47	0.58	0.45	0.55
Y Mean	2.85	2.85	5.21	5.21	47.14	47.14

Note: Standard errors clustered by state. Union campaign contribution shares and share of contributions by party measured on 0-100 scale. Contribution data from the National Institute on Money in State Politics.

Table 6: The Effect of State Right-to-Work Laws on State Legislator Class Background

	Working Class Share of State Legislature				
	(1)	(2)	(3)	(4)	(5)
Right to Work	−1.365	−1.307	−2.946	−2.935	−1.422
	(0.608)	(0.612)	(0.553)	(0.588)	(0.696)
Legislative Pay (in $10ks)			−0.793	−0.696	−0.159
			(0.174)	(0.167)	(0.461)
Session Length (Days)			−0.000	−0.001	0.010
			(0.007)	(0.007)	(0.007)
Staff Size			−0.068	0.014	0.133
			(0.063)	(0.067)	(0.155)
Term Limits				0.249	−0.208
				(0.732)	(1.195)
Top 1% Income Share				−0.069	−0.069
				(0.156)	(0.205)
Percent Black				−0.025	−0.102
				(0.028)	(0.446)
Percent Urban				−0.031	0.098
				(0.022)	(0.180)
Percent Poor				−0.063	0.290
				(0.065)	(0.192)
GOP Vote Share Average				0.044	0.128
				(0.035)	(0.048)
Per Capita Income				−0.052	−0.027
				(0.050)	(0.064)
Year FE	No	Yes	Yes	Yes	Yes
State FE	No	No	No	No	Yes
Observations	200	200	200	200	200
Adjusted R^2	0.04	0.10	0.37	0.38	0.55
Y Mean	4.44	4.44	4.44	4.44	4.44

Note: Standard errors clustered by state. State right-to-work laws and working-class legislators, 1979, 1993, 1995, and 2007. Working-class share of state legislature measured on 0-100 scale. Working-class data from Carnes and Hansen (2016).

Table 7: The Effect of State Right-to-Work Laws on U.S. House Legislator Class Background

	Panel A. Member of Congress Ever Held Working Class Occupation					
	All Members		Democrats		Republicans	
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−0.027	−0.030	−0.038	−0.026	−0.015	−0.031
	(0.006)	(0.024)	(0.009)	(0.011)	(0.007)	(0.033)
Year FE	Yes	No	Yes	No	Yes	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Census Division × Year FE	No	Yes	No	Yes	No	Yes
Observations	2208	2208	1077	1077	1125	1125
Adjusted R^2	0.12	0.11	0.10	0.07	0.21	0.19
Y Mean	0.06	0.06	0.10	0.10	0.03	0.03
	Panel B. Member of Congress Share Years in Working Class Occupation					
	All Members		Democrats		Republicans	
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−0.008	−0.007	−0.011	−0.005	−0.002	−0.009
	(0.002)	(0.007)	(0.004)	(0.003)	(0.001)	(0.009)
Year FE	Yes	No	Yes	No	Yes	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Census Division × Year FE	No	Yes	No	Yes	No	Yes
Observations	2208	2208	1077	1077	1125	1125
Adjusted R^2	0.08	0.07	0.08	0.06	0.12	0.10
Y Mean	0.02	0.02	0.03	0.03	0.00	0.00
	Panel C. Member of Congress Previous Job in Working Class					
	All Members		Democrats		Republicans	
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−0.004	−0.008	0.007	0.000	−0.024	−0.019
	(0.003)	(0.002)	(0.004)	(0.000)	(0.009)	(0.007)
Year FE	Yes	No	Yes	No	Yes	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Census Division × Year FE	No	Yes	No	Yes	No	Yes
Observations	3547	3547	2051	2051	1493	1493
Adjusted R^2	0.05	0.03	0.10	0.08	0.02	-0.00
Y Mean	0.01	0.01	0.01	0.01	0.01	0.01

Note: Standard errors clustered by state. Data in panels A and B are drawn from Carnes (2013): Carnes coded the careers of all Members of Congress, indicating whether they had ever held a blue-collar occupation (panel A) and the proportion of their pre-Congress work spent in blue-collar occupations (panel B). The data cover the Congresses from 1999 to 2008. Data in panel C covers 1980 to 1996 and is drawn from ICPSR data, compiled about the jobs that Members of Congress held immediately before serving in Congress.

Table 8: The Effect of State Right-to-Work Laws on Individual State Policies

	Panel A. Minimum Wage Laws, 1980-2016					
	State Greater Than Federal (0/1)		State to Federal Ratio		City Greater Than State (0/1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−0.083	−0.127	−0.035	−0.014	−0.051	−0.105
	(0.098)	(0.105)	(0.014)	(0.011)	(0.025)	(0.077)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Census Division × Year FE	No	Yes	No	Yes	No	Yes
Observations	1,850	1,850	1,850	1,850	650	650
Adjusted R^2	0.685	0.761	0.540	0.727	0.693	0.682

	Panel B. Prevailing Wage Laws and Top Tax Rates, 1980-2014					
			Top Income Tax Rate			
	Prevailing Wage Law (0/1)		Individuals		Corporations	
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−0.349	−0.219	0.0004	−0.012	0.076	0.047
	(0.200)	(0.246)	(0.011)	(0.010)	(0.038)	(0.032)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Census Division × Year FE	No	Yes	No	Yes	No	Yes
Observations	1,750	1,750	1,750	1,750	1,750	1,750
Adjusted R^2	0.922	0.923	0.900	0.903	0.923	0.928

Note: Standard errors clustered by state. Minimum wage data from Kavya Vaghul and Ben Zipperer's "Historical state and sub-state minimum wage data" (available from the Washington Center for Equitable Growth). Prevailing wage law and top tax rate data from Caughey and Warshaw (2016), as compiled by Jake Grumbach. City level minimum wage data only available from 2004 to 2016, shrinking the sample size in Panel A, columns 5 and 6. All measures in Panel B are only available through 2014, shrinking the sample size by 100 observations.

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A For Online Publication: Additional Results

A.1 Border County Pair Balance

In this section, we provide further evidence of the balance between border county pairs. Our sample varies slightly election year to election year as states enact RTW (Figure A.1). We show that there are no pre-treatment differences or trends in covariates and other key economic and demographic variables across border counties (Figure A.2), comparing the differences between border counties in the census before RTW is passed, similar to the comparison of trends between the two previous censuses before RTW is passed show in the main paper (Figure 5).

[Figure A.1 about here.]

[Figure A.2 about here.]

In the text, we showed the absence of pre-treatment trends in Presidential Democratic vote share in treatment border counties compared to their paired neighbors (Figure 6); here we show a similar lack of trends in Presidential election turnout (Figure A.3).

[Figure A.3 about here.]

A.2 Effect of RTW on Senate, House, and Gubernatorial Elections

Tables A.1, A.2, and A.3 provide results of the effect of RTW laws on Democratic vote share and turnout in US Senatorial, House, and Gubernatorial elections, respectively, using our main specifications. The point estimates are generally similar to those in Presidential elections.

[Table A.1 about here.]

[Table A.2 about here.]

[Table A.3 about here.]

A.3 RTW Effects: Border County Pairing Robustness

In our baseline border pair specification, we treat every county border pair as two observations, one for the county on one side of the border and another for the county on the other side of the border with a fixed effect for each pair. For example, Burnett County, WI and Pine County, MN, a pair, each enter the data paired with the other. However, many counties border more than one county in a neighboring state. For example, Pine County, MN also borders Douglas and Polk in WI. Of the counties with variation in RTW status on either side of the border, 1184 pair only once, 2006 pair twice, 730 pair 3 times, and 190 pair 4 or more times. We stack our data to include all of these pairs in the main analysis, following Dube et al. (2010). We cluster our standard errors to account for this.

An alternative specification would pair each county with another uniquely such that each county enters the data at most once and two counties in one state are never paired with the same county in another state. However, there are many alternative ways to choose which counties and county pairs to include or exclude in our sample. Rather than take a stand on which county neighbors are the "right" pairs, we pick randomly, bootstrapping over 50,000 random samples of unique county pairs. This generates 50,000 slightly different samples, such that in each a given county only appears once, paired to a neighbor which also only appears once.

Running our baseline specification (column 7 from Table 1) on each sample, we see that our results are robust to the choice of border pair samples. In Figure A.4, we plot a histogram of the estimated RTW effects in each sample (Figure A.4a for Democratic vote share, Figure A.4b for turnout, both for Presidential elections). In none of the 50,000 possible border pair samples we generate are the effects positive; in magnitudes, the weakest effect on Democratic vote share is -2 and on turnout is -1. Our baseline effects are not at the median of either distribution but are well within the bulk of the distributions.

[Figure A.4 about here.]

Our results are also robust to reweighting the data such that each county has equal weight. In our baseline specification, stacking the data leads to more weight being put on the counties with the most pairs across state borders. While the distribution of pairs puts most of the weight on counties with 1 or 2 pairs, there are a handful counties paired 3 or more times. We use a simple reweighting of the data such that each county has equal weight. In Table A.4, we replicate Table 2 and find very similar results in our preferred specification with the border pair by year fixed effect.

[Table A.4 about here.]

A.4 RTW Effects: Media Market Robustness

Local media markets play an important role in campaigns. Because election advertising is purchased at the media market level, potential voters in neighboring counties across a RTW border may be exposed to different levels of campaign spending if the counties are in different media markets. We draw data on designated media markets from Sood (2017).[45] Of the counties in our sample with a difference in RTW status across the state border for at least one election from 1980 and 2016, 348 of the border county pairs are in the same DMA, while 314 are in different DMAs. In Table A.5, we show that our baseline estimated effect of RTW laws is roughly the same as the RTW effect in the same DMA or the different DMA subsamples (columns 2 and 3). The RTW effect is also robust to including fixed effects at the DMA level (column 4).

[Table A.5 about here.]

[45] http://dx.doi.org/10.7910/DVN/IVXEHT

A.5 RTW Effects: Commuting Zone Robustness

While RTW laws stop at the state border, local labor markets do not necessarily follow the same geographic patterns. One concern with our border pair identification strategy might be that workers live in one state but work in another. In this case, the effects of RTW might bleed across state borders. This would likely lead us to underestimate the effects of RTW on elections if the control counties are also treated by weaker labor unions. But to address this concern more directly, we consider variation across our county border pairs in local labor markets.

The commuting zone (CZ) is one convenient definition of a local labor market. First used by Tolbert and Killian (1987) and Tolbert and Sizer (1996), commuting zones are built with county-level commuting patterns in the 1990 census. A commuting zone is a set of counties where residents are likely to live and work within the zone (and unlikely to live in the zone and work outside of it). Following Autor et al. (2013) and Feigenbaum and Hall (2015), we consider these commuting zones as local labor markets.

Of the counties in our sample with a difference in RTW status across the state border for at least one election from 1980 and 2016, 169 of the border county pairs are in the same CZ, while 493 are in different CZs. In Table A.6, we show that our baseline estimated effect of RTW laws is roughly the same as the RTW effect in the same CZ or the different CZ subsamples (columns 2 and 3). The effect, both statistically and economically, on turnout (Panel B) is weaker, but with only a quarter of the county border pairs in the same CZ across the border, this result may simply be driven by the small sample. The RTW effect is also robust to including fixed effects at the CZ level (column 4) for both vote share and turnout.

[Table A.6 about here.]

A.6 RTW Effects: Contemporaneous Political Variable Robustness

Our main results are robust to including controls for which party holds political power at the time of the election, as shown in Figure A.5. We can control for whether or not Democrats hold the governorship, the majority in the state upper house, the majority in the state lower house, or any combination. The effects of RTW laws are still negative and significant, reducing both Democratic vote share and voter turnout.

[Figure A.5 about here.]

A.7 RTW Effects: State-by-State Robustness

Seven states changed RTW status during our period, 1980-2016. In Figure A.6, we show that no one state is driving our results. To do this, we exclude each state, one at a time, and calculate the effect of RTW laws on vote share and turnout in the remaining sample. Though the turnout effects are noisier, both plots suggest that our main estimate is not driven by one state with a particularly powerful or anomalous RTW effect.

[Figure A.6 about here.]

A.8 RTW Effects on Individual Turnout: ANES and CPS Placebo Tests

States might enact RTW laws at the same time as other restrictive voting laws that also disadvantage liberal constituencies. These laws—especially voter ID laws—are often thought to disadvantage younger and minority voters. If passed at the same time as RTW laws, we might expect to see a decline in youth and minority turnout following the passage of RTW, an effect we might falsely attribute to the RTW laws rather than the other restrictive laws passed at the same time. We use the ANES data to examine the effects of RTW laws on other groups but find no disproportionate decline in either youth or minority turnout following the passage of RTW laws (Table A.7). We also turn to CPS data, 1980 to 2014, and replicate our ANES findings (Table A.8).

In these models, we regress individual voter turnout from the ANES on a RTW indicator and interactions of RTW with various demographic indicators. We again add in a full battery of individual control variables, which include gender, education (high school or less, some college, or college or more; high school or less is the excluded category), church attendance (in five categories of frequency), interest in political campaigns (in three categories), and a dummy variable for strong partisanship.[46] In the CPS results, we have a smaller set of controls, omitting the church attendance control, interest in political campaigns, and partisanship, but a much larger sample.

[Table A.7 about here.]

[Table A.8 about here.]

A.9 Pre- and Post-Trends in Union Contributions Before and After Right-to-Work Law Passage

In Figure A.7, we show a difference in difference style analysis of union campaign contributions before and after the passage of RTW laws. This figure shows few pre-trend differences before the passage of RTW laws and clear separation thereafter.

[Figure A.7 about here.]

A.10 Alternative Measures of State Policy Liberalism

In Figure A.8, we show the graphical difference in state policy liberalism before and after the passage of RTW laws using an alternative measure of state policy liberalism produced by Grumbach (2017) instead of Caughey and Warshaw (2016). One advantage to the Grumbach measure is that we can separate social and economic policies. In both cases, we see more conservative policy in states following the enactment of RTW laws.

[46] In the African American and Hispanic turnout models (Columns 3 through 6 of Table A.7), we include age and age squared. In the African American turnout model, we include a control for Hispanic. In the Hispanic turnout model, we include a control for African-American. In the youth turnout model (Columns 7 and 8 of Table A.7), we include a control for those over 50 years old, and we include the same indicators for race and ethnicity that we use in our paper (non-Hispanic white, non-Hispanic black, and Hispanic other).

[Figure A.8 about here.]

[Table A.9 about here.]

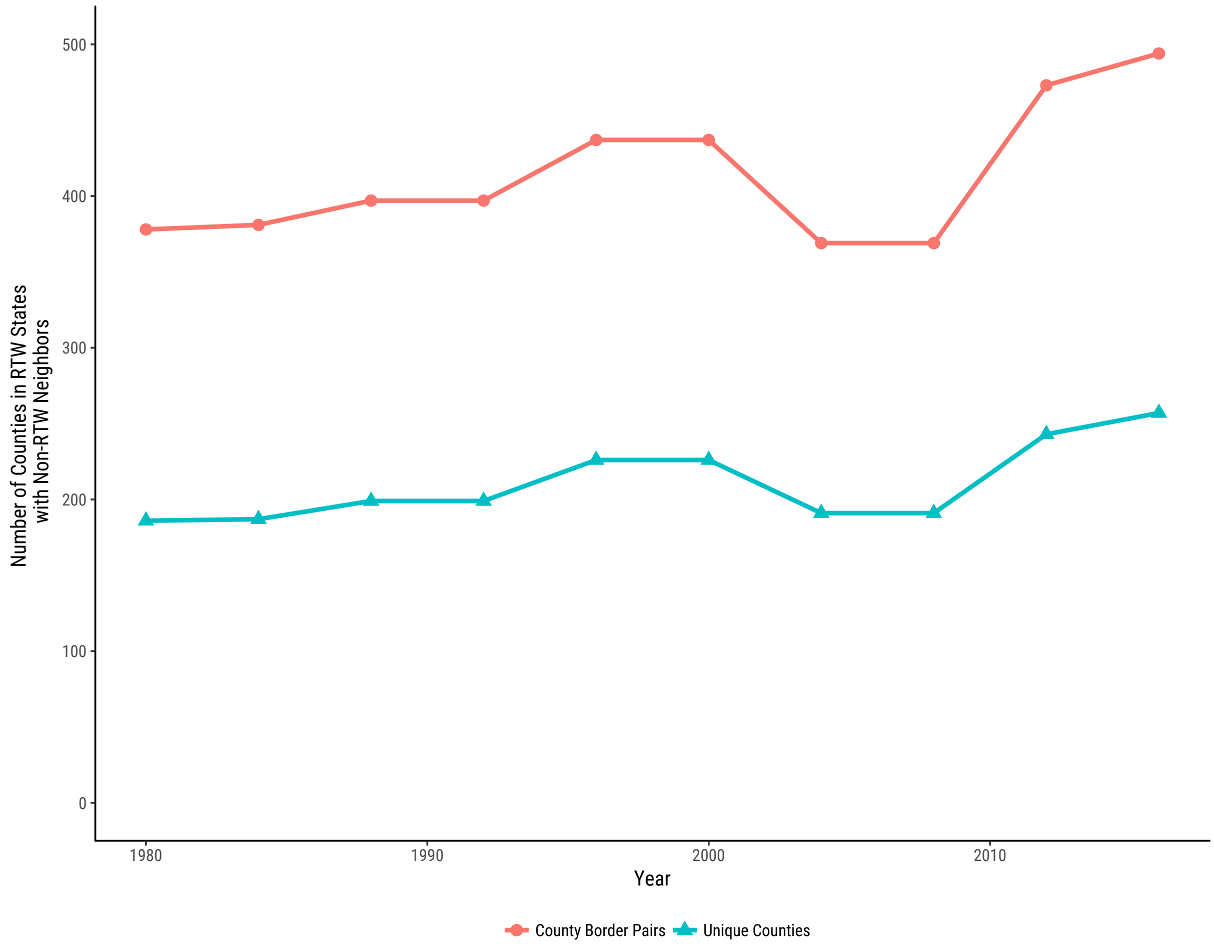


Figure A.1: The number of border counties in our sample in each election cycle where state right-to-work laws differ on one side of the border to the other.

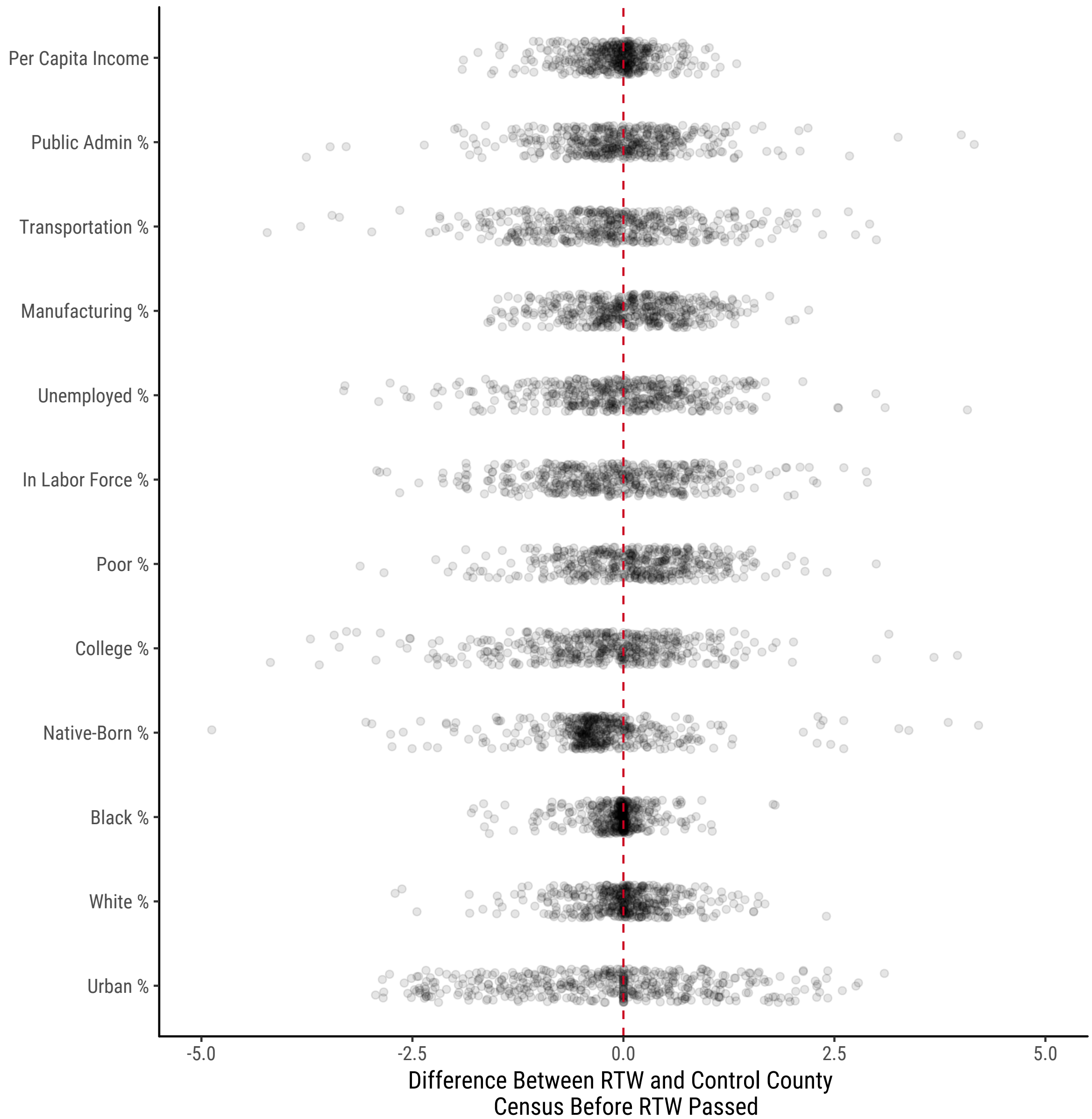


Figure A.2: Balance in covariates between right-to-work and non-right-to-work counties, comparing covariates in the decennial census taken before RTW was passed. Full 1980-2016 sample. Each point is the difference between a RTW county and a non-RTW county. Data from the US Census for 1980, 1990, 2000, and 2010.

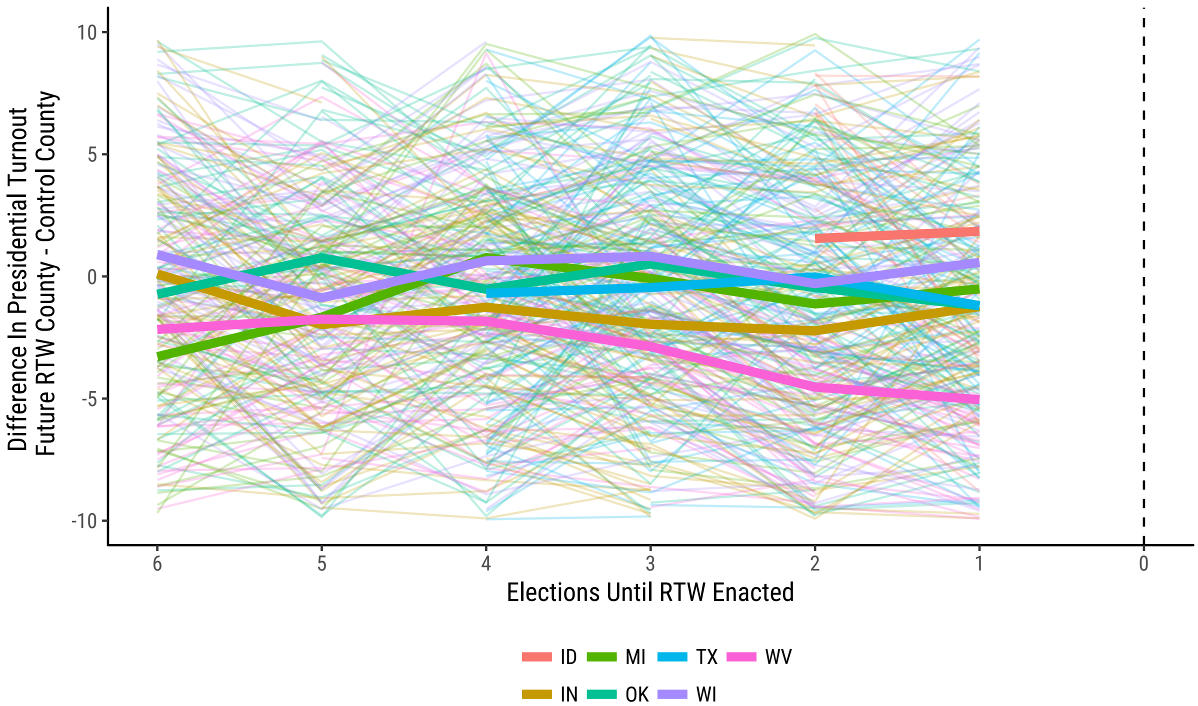
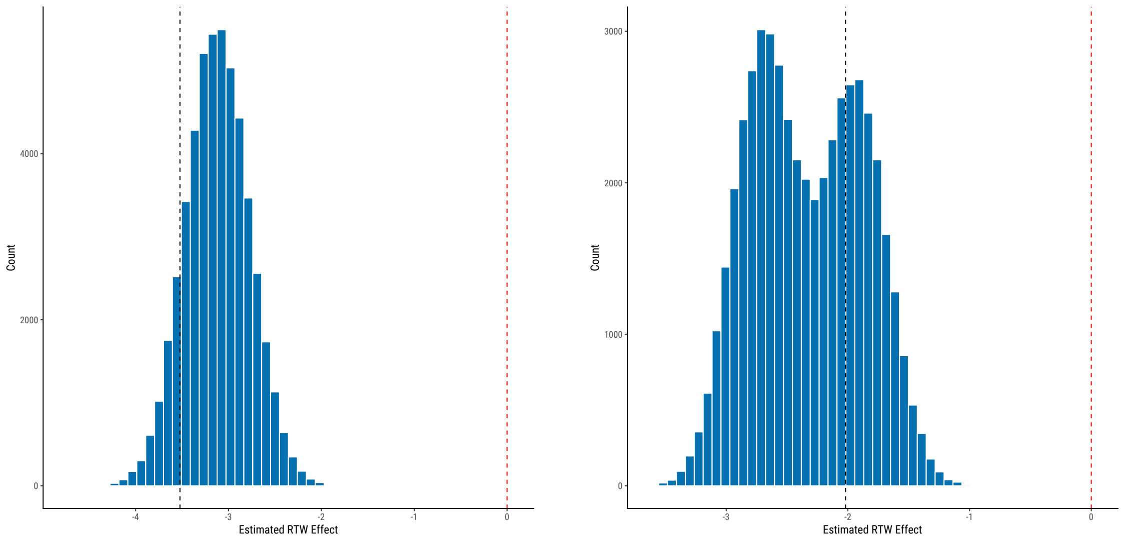


Figure A.3: No Cross-RTW Border Trend in Turnout Rates



(a) RTW Effect on Democratic Vote Share

(b) RTW Effect on Turnout

Figure A.4: Robustness of estimated RTW effect in 50,000 bootstrapped random samples of unique county pairs. In every sample, a given county only appears once, paired to a neighbor which only appears once. Estimates are from our baseline specification (column 7 from Table 1) on each sample, Presidential Democratic vote share or Presidential election turnout as the outcomes. The dashed black lines indicate the baseline results with all county border pairs included, stacking and allowing for counties to be paired to multiple pairs.

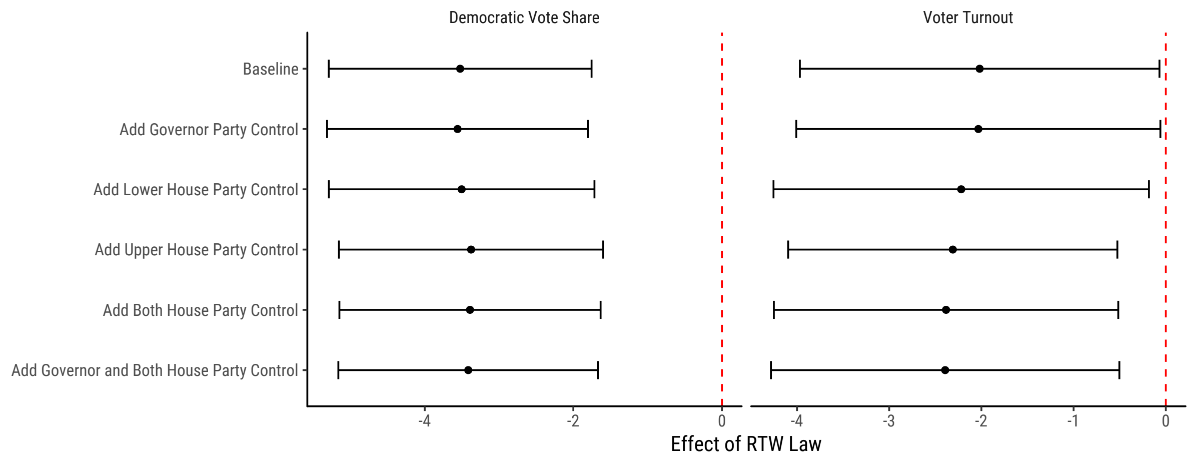


Figure A.5: The effect of state right-to-work laws on Presidential elections, contemporary political power robustness checks. All models include county and border pair by year fixed effects. We cluster standard errors two ways, by border pairs and by state. The sample includes only counties on state borders. Both vote share and turnout measured on 0-100 scale.

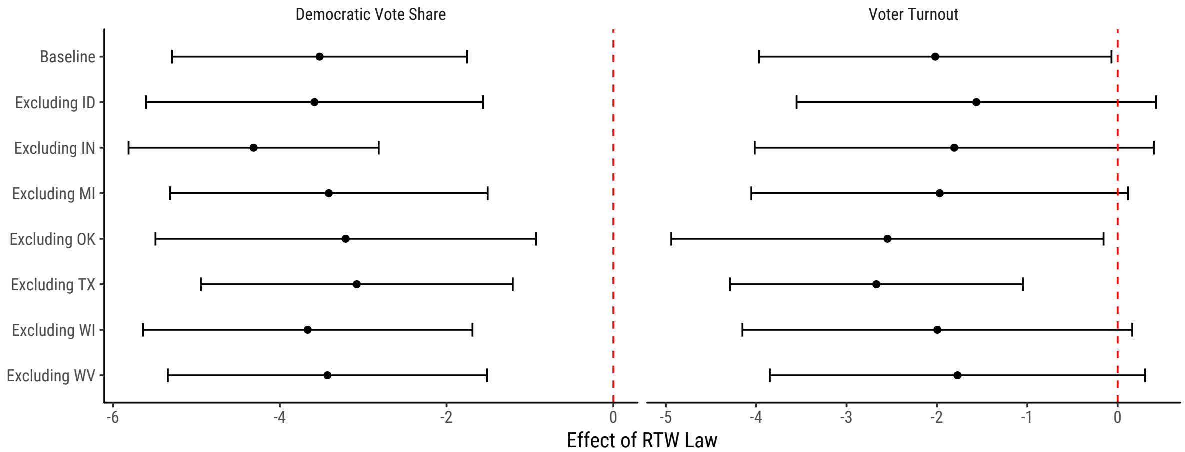


Figure A.6: The effect of state right-to-work laws on Presidential elections, excluding each state in our sample one by one. All models include county and border pair by year fixed effects. We cluster standard errors two ways, by border pairs and by state. The sample includes only counties on state borders. Both vote share and turnout measured on 0-100 scale.

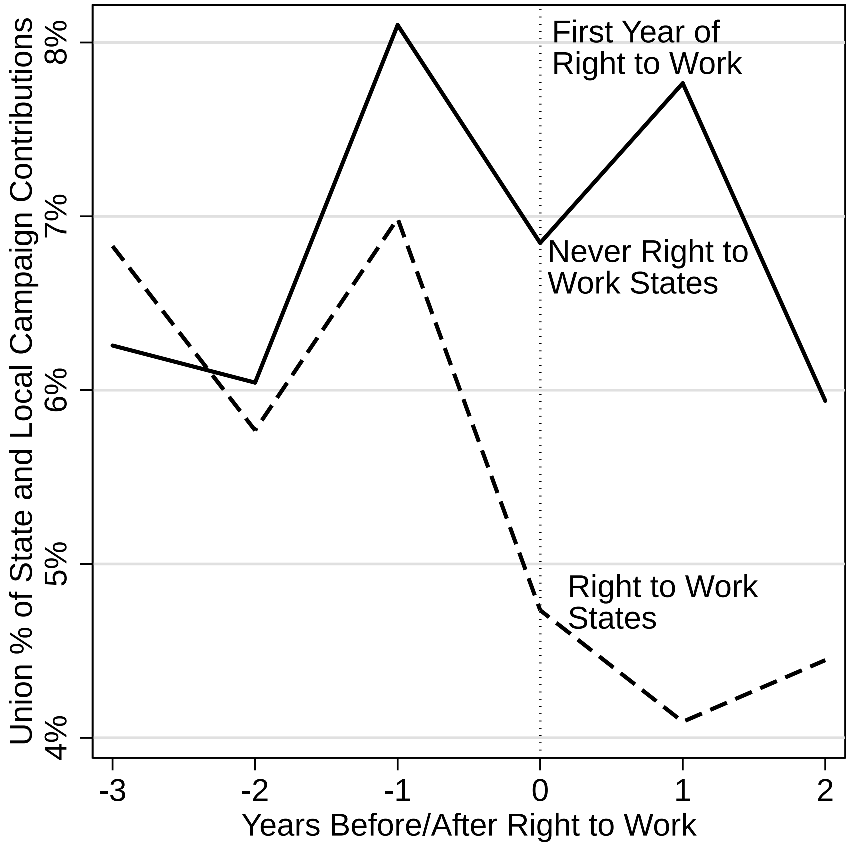
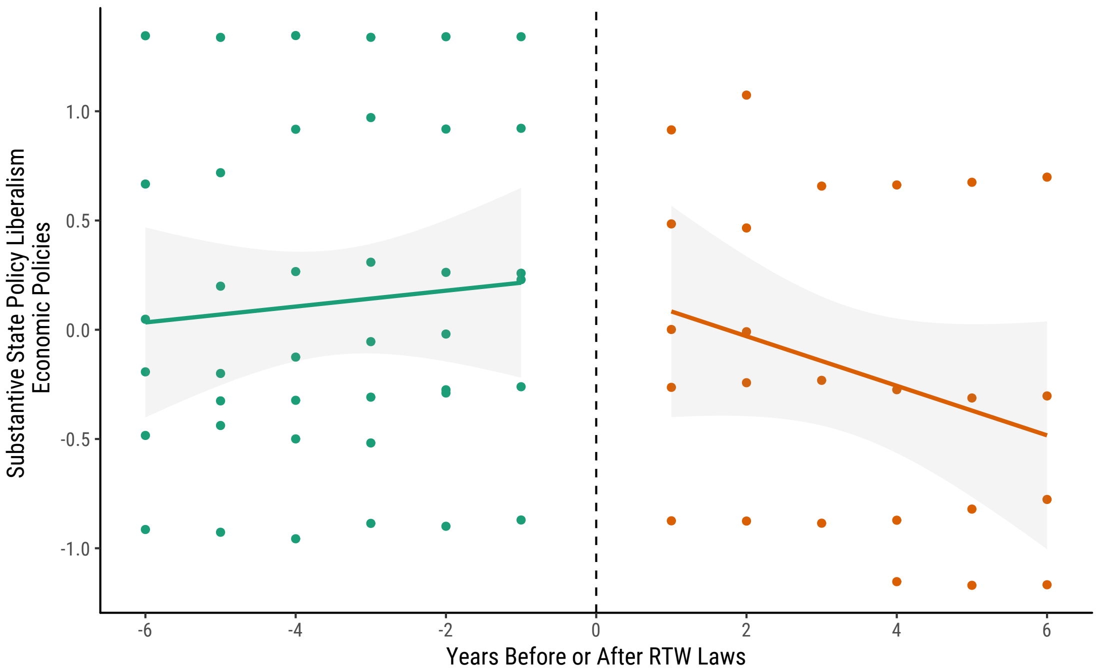
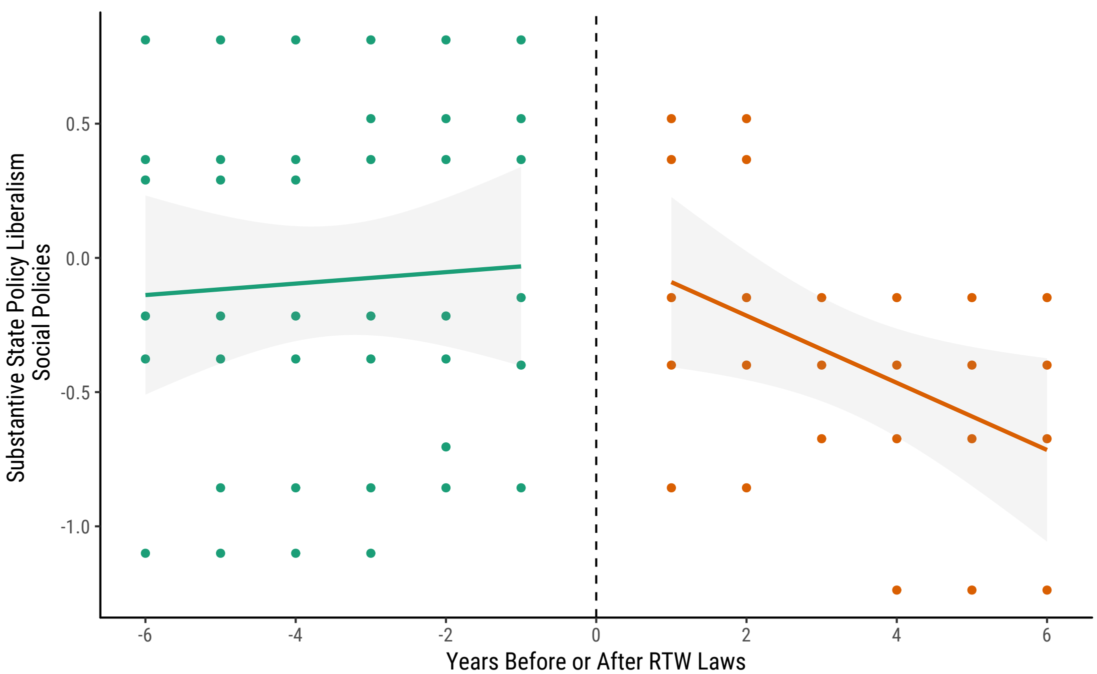


Figure A.7: Difference-in-differences summary analysis of right-to-work laws and labor campaign contributions, 1996-2016. Campaign contribution data from the National Institute on Money in State Politics.



(a) State Economic Liberalism



(b) State Social Liberalism

Figure A.8: Relationship between state policy liberalism and state right-to-work laws, 1980-2014. Figure plots average state policy liberalism in states before and after the passage of right-to-work laws. Left plot includes economic policies, right plot includes social policies. Gray shading indicates 95% confidence intervals. State policy liberalism from Grumbach (2017).

Table A.1: The Effect of State Right-to-Work Laws on Senate Elections

	Panel A. Democratic Vote Share						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	−5.620	−6.393	6.218	−6.543	−6.479	1.133	−3.331
	(2.855)	(4.156)	(3.485)	(2.709)	(4.395)	(4.009)	(3.802)
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	Yes	No	No
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	38876	38876	38876	32445	32445	32445	32445
Adjusted R^2	0.02	0.45	0.58	0.03	0.48	0.60	0.52
Y Mean	45.19	45.19	45.19	45.16	45.16	45.16	45.16

	Panel B. Voter Turnout						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	−3.847	−3.655	−3.691	−2.998	−3.039	−4.219	−2.198
	(2.167)	(1.121)	(1.171)	(2.159)	(1.278)	(1.015)	(1.380)
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	Yes	No	No
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	38864	38864	38864	32430	32430	32430	32430
Adjusted R^2	0.02	0.76	0.80	0.01	0.80	0.84	0.81
Y Mean	49.07	49.07	49.07	48.69	48.69	48.69	48.69

Note: Standard errors clustered by state in the all county sample and clustered two-way by state and border-pair in the border county sample. Vote share outcomes are measured 0 to 100 percent. Following Dube et al. (2010), in the border sample we allow counties bordering multiple other counties to pair with each and stack the data accordingly, which is why the 1173 unique counties on a state border translate to more than 32,000 observations with election data from 1980 to 2016. The county border pair fixed effect (subsumed by the border pair by year fixed effect) identifies each separate pair, and we cluster at the border-pair level to account for repeated observations.

Table A.2: The Effect of State Right-to-Work Laws on House Elections

	Panel A. Democratic Vote Share						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	−4.769	−13.149	−0.550	−5.681	−12.700	−2.584	−5.414
	(3.564)	(5.593)	(4.060)	(2.987)	(6.038)	(3.514)	(4.189)
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	Yes	No	No
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	58009	58009	58009	48629	48629	48629	48629
Adjusted R^2	0.01	0.28	0.32	0.01	0.51	0.58	0.59
Y Mean	45.18	45.18	45.18	44.86	44.86	44.86	44.86
	Panel B. Voter Turnout						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	−3.030	−2.907	−2.139	−2.899	−2.910	−3.416	−2.513
	(2.224)	(1.369)	(1.462)	(2.216)	(1.298)	(1.488)	(1.005)
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	Yes	No	No
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	58004	58004	58004	48614	48614	48614	48614
Adjusted R^2	0.00	0.43	0.44	0.01	0.75	0.79	0.77
Y Mean	48.63	48.63	48.63	48.18	48.18	48.18	48.18

Note: Standard errors clustered by state in the all county sample and clustered two-way by state and border-pair in the border county sample. Vote share outcomes are measured 0 to 100 percent. Following Dube et al. (2010), in the border sample we allow counties bordering multiple other counties to pair with each and stack the data accordingly, which is why the 1173 unique counties on a state border translate to more than 32,000 observations with election data from 1980 to 2016. The county border pair fixed effect (subsumed by the border pair by year fixed effect) identifies each separate pair, and we cluster at the border-pair level to account for repeated observations.

Table A.3: The Effect of State Right-to-Work Laws on Gubernatorial Elections

	Panel A. Democratic Vote Share						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	−2.455	−1.999	−0.133	−2.860	−2.463	−1.709	−2.450
	(2.201)	(2.582)	(4.382)	(1.888)	(2.081)	(3.582)	(3.441)
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	Yes	No	No
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	28342	28342	28342	23832	23832	23832	23832
Adjusted R^2	0.01	0.49	0.64	0.01	0.53	0.66	0.47
Y Mean	46.35	46.35	46.35	46.79	46.79	46.79	46.79
	Panel B. Voter Turnout						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	−3.583	−2.609	−0.757	−2.970	−2.346	−1.237	−1.120
	(2.996)	(0.788)	(1.202)	(2.901)	(0.875)	(1.075)	(2.194)
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	No	No	Yes	No	No
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	28335	28335	28335	23821	23821	23821	23821
Adjusted R^2	0.02	0.84	0.87	0.01	0.85	0.88	0.86
Y Mean	45.67	45.67	45.67	45.53	45.53	45.53	45.53

Note: Standard errors clustered by state in the all county sample and clustered two-way by state and border-pair in the border county sample. Vote share outcomes are measured 0 to 100 percent. Following Dube et al. (2010), in the border sample we allow counties bordering multiple other counties to pair with each and stack the data accordingly, which is why the 1173 unique counties on a state border translate to more than 32,000 observations with election data from 1980 to 2016. The county border pair fixed effect (subsumed by the border pair by year fixed effect) identifies each separate pair, and we cluster at the border-pair level to account for repeated observations.

Table A.4: The Effect of State Right-to-Work Laws on Elections

	Panel A. Democratic Vote Share					
	Presidential	Senate	Governor	House of Representatives		
				All Years	On Cycle	Off Cycle
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−3.184	−2.457	−1.840	−4.395	−2.686	−7.203
	(0.955)	(4.269)	(3.692)	(4.573)	(4.570)	(5.675)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Border Pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25494	32445	23832	48629	25490	23139
Adjusted R^2	0.93	0.57	0.52	0.64	0.63	0.63
Y Mean	41.80	45.16	46.79	44.86	44.71	45.04
	Panel B. Voter Turnout					
	Presidential	Senate	Governor	House of Representatives		
				All Years	On Cycle	Off Cycle
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	−2.211	−2.370	−1.578	−2.724	−2.613	−2.932
	(0.865)	(1.323)	(2.361)	(1.037)	(0.885)	(2.097)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Border Pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25483	32430	23821	48614	25486	23128
Adjusted R^2	0.86	0.84	0.88	0.83	0.81	0.76
Y Mean	56.88	48.69	45.53	48.18	55.32	40.31

Note: Sample limited to counties on state borders. Standard errors clustered two-way by state and border-pair. Vote share and turnout outcomes are measured 0 to 100 percent. Following Dube et al. (2010), we allow counties bordering multiple other counties to pair with each and stack the data accordingly, which is why the 1173 unique counties on a state border translate to more than 25,000 observations with election data from 1980 to 2016 for the Presidential sample. However, we reweight the data such that each county weights equally, regardless of how many counties it pairs with across a state border. The county border pair fixed effect (subsumed by the border pair by year fixed effect) identifies each separate pair, and we cluster at the border-pair level to account for repeated observations.

Table A.5: The Effect of State Right-to-Work Laws on Presidential Elections: Media Market Robustness

	Panel A. Democratic Vote Share			
	Baseline	Same DMA	Different DMA	DMA FE
	(1)	(2)	(3)	(4)
Right to Work	−3.523	−3.898	−3.156	−3.523
	(0.902)	(1.304)	(1.002)	(0.908)
County FE	Yes	Yes	Yes	Yes
Border Pair × Year FE	Yes	Yes	Yes	Yes
Designated Media Area FE	No	No	No	Yes
Observations	25,494	13,945	11,549	25,494
Adjusted R^2	0.922	0.918	0.925	0.921
	Panel B. Voter Turnout			
	Baseline	Same DMA	Different DMA	DMA FE
	(1)	(2)	(3)	(4)
Right to Work	−2.019	−2.668	−1.384	−2.019
	(0.995)	(0.881)	(1.650)	(1.002)
County FE	Yes	Yes	Yes	Yes
Border Pair × Year FE	Yes	Yes	Yes	Yes
Designated Media Area FE	No	No	No	Yes
Observations	25,483	13,942	11,541	25,483
Adjusted R^2	0.827	0.853	0.799	0.825

Note: Sample limited to counties on state borders. Standard errors clustered two-way by state and border-pair. Vote share and turnout outcomes are measured 0 to 100 percent. Following Dube et al. (2010), we allow counties bordering multiple other counties to pair with each and stack the data accordingly, which is why the 1173 unique counties on a state border translate to more than 25,000 observations with election data from 1980 to 2016 for the Presidential sample. The county border pair fixed effect (subsumed by the border pair by year fixed effect) identifies each separate pair, and we cluster at the border-pair level to account for repeated observations.

Table A.6: The Effect of State Right-to-Work Laws on Presidential Elections: Commuting Zone Robustness

	Panel A. Democratic Vote Share			
	Baseline	Same CZ	Different CZ	CZ FE
	(1)	(2)	(3)	(4)
Right to Work	−3.523	−4.317	−3.281	−3.523
	(0.902)	(0.989)	(1.089)	(0.920)
County FE	Yes	Yes	Yes	Yes
Border Pair × Year FE	Yes	Yes	Yes	Yes
Commuting Zone FE	No	No	No	Yes
Observations	25,494	6,513	18,981	25,494
Adjusted R^2	0.922	0.925	0.921	0.919
	Panel B. Voter Turnout			
	Baseline	Same CZ	Different CZ	CZ FE
	(1)	(2)	(3)	(4)
Right to Work	−2.019	−0.555	−2.467	−2.019
	(0.995)	(1.104)	(1.152)	(1.015)
County FE	Yes	Yes	Yes	Yes
Border Pair × Year FE	Yes	Yes	Yes	Yes
Commuting Zone FE	No	No	No	Yes
Observations	25,483	6,508	18,975	25,483
Adjusted R^2	0.827	0.857	0.818	0.820

Note: Sample limited to counties on state borders. Standard errors clustered two-way by state and border-pair. Vote share and turnout outcomes are measured 0 to 100 percent. Following Dube et al. (2010), we allow counties bordering multiple other counties to pair with each and stack the data accordingly, which is why the 1173 unique counties on a state border translate to more than 25,000 observations with election data from 1980 to 2016 for the Presidential sample. The county border pair fixed effect (subsumed by the border pair by year fixed effect) identifies each separate pair, and we cluster at the border-pair level to account for repeated observations.

Table A.7: The Effect of State Right-to-Work Laws on Individual Voter Turnout, ANES 1980-2012

	Presidential Election							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Right to Work	0.029	0.020	0.013	0.010	0.001	0.007	0.001	−0.0004
	(0.011)	(0.010)	(0.010)	(0.006)	(0.011)	(0.010)	(0.010)	(0.008)
Non-Professional Worker	−0.102	0.0003						
	(0.010)	(0.010)						
RTW × Non-Professional	−0.070	−0.040						
	(0.019)	(0.017)						
Black			−0.035	−0.012				
			(0.013)	(0.013)				
RTW × Black			−0.027	−0.025				
			(0.019)	(0.017)				
Hispanic					−0.104	−0.018		
					(0.012)	(0.011)		
RTW × Hispanic					0.007	−0.010		
					(0.034)	(0.028)		
Young (under 25)							−0.260	−0.206
							(0.014)	(0.014)
RTW × Young							0.022	0.022
							(0.023)	(0.026)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	18,140	14,846	29,810	24,445	29,810	24,432	29,837	24,359
Adjusted R^2	0.514	0.583	0.507	0.589	0.509	0.589	0.534	0.584

Note: Standard errors clustered by state. Linear probability model. Individual controls include age and age squared, gender, education (high school or less, some college, or college or more; high school or less is the excluded category), indicators for race and ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and other; other is the excluded category), church attendance (in five categories of frequency), interest in political campaigns (in three categories), and a dummy variable for strong partisanship. Data from National Election Studies

Table A.8: The Effect of State Right-to-Work Laws on Individual Voter Turnout, CPS 1980-2016

	Presidential Election							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Right to Work	−0.020	−0.015	−0.025	−0.021	−0.021	−0.016	−0.025	−0.019
	(0.011)	(0.008)	(0.010)	(0.008)	(0.012)	(0.009)	(0.009)	(0.008)
Non-Professional Worker	−0.131	−0.019						
	(0.004)	(0.002)						
RTW × Non-Professional	−0.016	−0.012						
	(0.006)	(0.004)						
Black			−0.013	0.067				
			(0.013)	(0.014)				
RTW × Black			−0.0003	0.015				
			(0.017)	(0.015)				
Hispanic					−0.171	−0.028		
					(0.007)	(0.005)		
RTW × Hispanic					0.004	−0.024		
					(0.030)	(0.012)		
Young (under 25)							−0.267	−0.244
							(0.003)	(0.008)
RTW × Young							−0.003	0.003
							(0.007)	(0.010)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1,145,656	992,672	1,684,666	1,457,339	1,669,549	1,446,494	1,684,666	1,446,494
Adjusted R^2	0.066	0.197	0.041	0.188	0.048	0.186	0.074	0.143

Note: Standard errors clustered by state. Linear probability model. Individual controls include age and age squared, gender, education (high school or less, some college, or college or more; high school or less is the excluded category), and indicators for race and ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and other; other is the excluded category). Data from the CPS (Flood et al. 2017).

Table A.9: The Effect of State Right-to-Work Laws on State Policy Liberalism, 1980-2014

	Panel A. Caughey-Warshaw State Policy Liberalism Scores			
	All Policies		Excluding RTW	
	(1)	(2)	(3)	(4)
Right to Work	−0.435	−0.365	−0.295	−0.230
	(0.122)	(0.118)	(0.125)	(0.127)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Census Division × Year FE	No	Yes	No	Yes
Observations	1,750	1,750	1,750	1,750
Adjusted R^2	0.950	0.959	0.939	0.951
	Panel B. Grumbach State Policy Liberalism			
	Economic Policies		Social Policies	
	(1)	(2)	(3)	(4)
Right to Work	−0.001	−0.005	−0.330	−0.155
	(0.079)	(0.089)	(0.117)	(0.136)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
Census Division × Year FE	No	Yes	No	Yes
Observations	1,750	1,750	1,750	1,750
Adjusted R^2	0.945	0.950	0.884	0.916

Note: Standard errors clustered by state. Relationship between state policy liberalism and state right-to-work laws, 1980-2014. Panel A draws state policy liberalism from Caughey and Warshaw (2016), Panel B draws state policy liberalism from Grumbach (2017).

B For Online Publication: Legal Language in RTW Statutes

In the linked appendix, we present the exact legal language for RTW statutes in Idaho, Oklahoma, Indiana, Michigan, Wisconsin, and West Virginia:

`https://www.dropbox.com/s/apg4zh1a1g3j3kx/bill_text.pdf`.

B.1 Jaccard Pairwise Similarity Indices

We use Jaccard index scores to measure the textual similarity of the RTW statutes in each of the states in our sample. The pairwise scores are in Table B.1. The Jaccard index is a common measure of textual overlap that runs from 0 (no overlap) to 1 (perfect overlap). Some of the bills are much more similar to one another than others, especially the Indiana and Oklahoma laws, the Michigan and Oklahoma laws, and the Wisconsin and West Virginia laws.

[Table B.1 about here.]

Table B.1: Pairwise Jaccard Index Scores of RTW Statute Textual Similarity

State 1	State 2	Jaccard Index
TX	WV	0.014
TX	WI	0.024
MI	TX	0.032
IN	WV	0.036
MI	WV	0.038
ID	WV	0.04
IN	TX	0.049
ID	TX	0.051
OK	WV	0.062
IN	WI	0.067
ID	WI	0.07
OK	TX	0.074
ID	IN	0.089
ID	MI	0.091
IN	MI	0.101
OK	WI	0.113
MI	WI	0.144
WI	WV	0.168
MI	OK	0.223
ID	OK	0.328
IN	OK	0.34