

Demobilizing Democrats and Labor Unions: Political Effects of Right to Work Laws*

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Abstract

Labor unions play a central role in the Democratic party coalition, providing candidates with voters, volunteers, and contributions. Has the recent decline of organized labor hurt Democrats? We use the enactment of right-to-work laws—which weaken unions by removing closed shop protections—to estimate the effect of unions on politics from 1980 to 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 right-to-work laws reduce Democratic presidential vote shares by 4-6 percentage points. We find similar effects in US Senate, US House, and gubernatorial races, as well as state legislative control. Turnout is also 2 to 3 percentage points lower in right-to-work counties after those laws pass. We explore mechanisms behind these effects, finding that right-to-work laws dampen labor campaign contributions to Democrats and that likely Democratic voters are less likely to be contacted to vote in right-to-work states. The weakening of unions also has large downstream effects both on who runs for office and state legislative policy. Fewer working class candidates serve in state legislatures and state policy moves in a more conservative direction following the passage of right-to-work laws.

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1 Introduction

The economic and political history of 20th century America strongly suggests that the labor movement, the electoral and political clout of the Democratic party, and economic inequality are all closely linked. As unions gained strength after the New Deal and the Second World War, income inequality sharply declined and the United States entered a period dubbed the “Great Compression” (Goldin and Margo 1992). But since the 1980s, inequality has steadily increased (Autor et al. 2008) and the political center has shifted rightward (McCarty et al. 2006; Hacker and Pierson 2010; Mann and Ornstein 2016; Hacker and Pierson 2005), also coinciding with a decline in the share of private-sector workers participating in labor unions (Farber and Western 2000; Hirsch et al. 2001). Are these three trends causally related? So far, economists and other social scientists have shed light mostly on the relationship between unions and inequality, arguing that unions can directly affect inequality by compressing the wage distribution within unionized firms and industries (Freeman 1980, 1982; Card 2001; Frandsen 2012; Western and Rosenfeld 2011).¹ In this research, we turn to the relationship between unions and politics and the potential downstream implications of politics for economic outcomes. The decline of the American labor movement may have directly increased economic disparities by limiting wage compression in the workplace, as others have suggested. But, as we show in this paper, diminished union clout may have also increased inequality indirectly by dampening the electoral prospects of Democratic candi-

¹Freeman and Medoff (1984) and Blanchflower and Bryson (2004) review the literature on the effects of unions on wages and labor markets. Using a regression discontinuity approach, comparing firms with NLRB certification vote shares close to the threshold, Dinardo and Lee (2004) find little evidence of a causal union wage premium or a union effect on employment in either direction. Evidence for the union effect on firms is also mixed. Using the NLRB regression discontinuity, Lee and Mas (2009) find little effect of close union wins relative to close union losses on firm stock market performance; however, using a unionization event study, the authors do find large negative effects of unionization. Unions may also play a role in determining the distribution of wages, both within unionized firms and across the economy. Western and Rosenfeld (2011) argue that unions reduced wage and income inequality because they institutionalized equity norms. According to Card (2001), the decline in unionization among men from 1973 to 1993 explains 15-20% of the increase in male wage inequality. Applying the NLRB RD technique to wage distributions, Frandsen (2012) finds that unions do increase wages at the bottom tail of the distribution.

dates who push for greater redistribution.² It is this relationship between unions and the political parties, especially the Democrats, that we focus on in our paper.

Although economists have largely focused on labor market outcomes when answering the question “What do labor unions do?” famously posed by Freeman and Medoff, there is good reason to consider the political implications and activities of unions as well. As UAW President Walter Reuther put it persuasively 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.”³ Indeed, in many developed, industrial countries, organized labor participates directly in party politics, forming its own parties and fielding its own candidates (Kitschelt 1994; Przeworski and Sprague 1986).⁴ Even in the United States, where no literal labor party exists (see eg Eidlin 2016), unions do not limit their activities to collective bargaining for their members. Unions also attempt to shape broader economic outcomes through policy and politics.⁵ In fact, as unions have lost power at the negotiating table, they may have shifted even more resources into politics (Dark 1999), especially since changes in public policy have been a major contributor to declines in U.S. unionization (Farber 2005; Hacker and Pierson 2010; Lichtenstein 2013).⁶ Politics are both a fundamental determinant of labor movement success—and its survival.⁷

One of the primary means through which American labor unions have sought to shape

²Kelly and Witko (2012) presents evidence of the pre- and post-fiscal policy effect of unions on inequality across U.S. states. Bradley et al. (2003); Huber and Stephens (2001); Korpi (1978) all explore the intersection of unions, the political power of the left, and redistribution cross-nationally. We contribute to this literature by pinning down the causal effect of unions on the political strength of the left party—Democrats—in the United States.

³Proceedings of the Constitutional Convention of the United Auto Workers, Vol. 22 (1970)

⁴See also the literature on power resource theory, (Bradley et al. 2003; Huber and Stephens 2001; Korpi 1978).

⁵Chang (2001), for instance, argues that the AFL-CIO merger in 1955 was motivated by both economics and politics—the combined union hoped to increase the lobbying power for all of organized labor. Schlozman (2013) notes that it was the creation of the CIO in 1935 that spurred American unions to begin taking national positions—and supporting New Deal Democrats—in the first place.

⁶Spinning Reuther’s quote for a new era of declining union power in bargaining, Dark (1999) suggests that “[w]hat cannot be won in the economic market can, perhaps, be won in the political market.”

⁷For instance, according to the head of the Amalgamated Transit Union, Republican control of the federal government after 2012, not any direct economic phenomena, could mean “there will be no such thing as a labor movement.” Larry Hanley, the ATU president, also admitted little faith in new pro-labor legislation in 2012 (Eidelson 2012).

politics 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)). 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 produced by Caughey and Warshaw (2016) (1b).⁸

[Figure 1 about here.]

Still, these bivariate relationships do not provide evidence of the *causal effect* of unions on the Democratic party’s strength and on state policy. 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 or have more union-supporting citizens.⁹ Our paper tackles this question, seeking to estimate the causal contributions of union strength to Democratic political power, and through it, the direction of state 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. Twelve years after the passage of the National Labor Relations Act in 1935 that

⁸These estimates of state policy liberalism come from dynamic latent-variable models applied to data on 148 state policies. Caughey and Warshaw (2016) describe their methodology in full detail. We find similar results using Grumbach (2017)’s alternative state policy score approach.

⁹Beland 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.

recognized the right of private-sector unions to collective bargain, Congress passed the Taft-Hartley Act, which greatly curtailed newly-established union rights. In one of Taft Hartley’s most important provisions, Congress granted states the ability to pass so-called RTW 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.¹⁰ After Congress approved Taft-Hartley, a number of state legislatures, largely in the South, quickly passed laws instituting RTW. Figure 2 summarizes the states with RTW laws in the United States as of 2016 and the years in which they were enacted.

[Figure 2 about here.]

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.¹¹ Opponents of RTW measures, for their part, 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.¹² Labor advocates are also quick to point out that RTW measures seem designed to weaken unions’ overall power, defunding their organizations of valuable revenues while forcing them to represent a broader pool of workers.

What are the actual 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

¹⁰Although several states had passed RTW laws before Congress enacted Taft-Hartley, their legality was in question until the passage of the law.

¹¹For one example, see: <http://www.heritage.org/jobs-and-labor/report/right-work-laws-myth-vs-fact>

¹²For one example, see: <https://www.bloomberg.com/view/articles/2012-12-12/the-conservative-case-for-right-to-work-laws>

their empirical specifications and state and year samples (Moore 1998).¹³

One major obstacle to the identification of the effects of RTW laws comes from the fact that states that pass such measures are often very different from non-RTW states on a number of important economic, social, and political characteristics that could themselves account for differences in future outcomes.¹⁴ Holmes (1998) proposed studying pairs of border counties where one county is in a RTW state and the other is not. The logic is that these counties should be more similar to one another than entire states. 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. We thus argue that any political differences that emerge after the passage of a RTW law in RTW counties are more likely to have been driven by the RTW laws themselves than differences that emerge between entire states. Accordingly, our estimates represent the reduced form effect of RTW laws on electoral and policy outcomes.¹⁵

To preview our results, we find strong causal evidence for the contribution of unions to

¹³In a sampling of the literature, Kalenkoski and Lacombe (2006) find small increases in the share of workers in manufacturing after the passage of RTW laws. Ellwood and Fine (1987) suggest that RTW laws are more “symbol” rather than “substance” but find some decrease in union organizing after passage. Farber (1984) also argues that RTW laws are more symbolic. Eren and Ozbeklik (2016) use a synthetic control method to compare Oklahoma with synthetic Oklahoma before and after the passage of the states 2001 RTW law. They find a reduction in private sector unionization rates but no effects on total employment or wages.

¹⁴Zollo (2008) finds a negative relationship between RTW laws and presidential-level turnout at the county level during the 2000 election, which is consistent with our results. However, because that study only uses one year of data and examines all counties in the United States, we cannot be certain that there are not other state-level differences between RTW and non-RTW states that are confounding these estimates of the political effects of RTW laws.

¹⁵Holmes (1998) argues that RTW are just one of many pro-business policies a state might pass. If RTW laws are a common 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, ultimately, 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 are passed along other conservative wish list items that might reduce Democratic vote share or increase Republican vote share. We show later that RTW 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 other common conservative laws including those written and promoted by the American Legislative Exchange Council (Hertel-Fernandez 2014, 2017). 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.

Democratic political power—and for the demobilizing effects of RTW laws—examining state and federal elections from 1980 through 2016. After the passage of RTW laws, county-level Democratic vote shares in presidential elections fall by 4 to 6 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.¹⁶ 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 fall after RTW laws as well.¹⁷ 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.

We explore several mechanisms through which RTW laws and weakened unions might impair Democratic electoral performance, and 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 significantly 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 from national election surveys, we also find that Democratic would-be-voters and non-professional workers—the class of workers identified in surveys who are most likely to be potential union members—are less likely to report that they had been contacted about turning out to vote in states after the passage of RTW laws.

¹⁶We 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 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; the varying length of gubernatorial terms and varying timing of elections across states again reduces our sample of potential border-county pairs.

¹⁷Because 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 with their neighbors, before and after the passage of the laws. We expand on the identification assumptions in the results section.

Lastly, we consider the downstream consequences of weakened labor unions on state politics, and find that RTW laws have large effects on both who runs for office and the substance of state policy. We observe that in RTW states, state legislators are less likely to have a working-class background, drawing on biographical data from Carnes (2013). State legislative policy also shifts to the right after the passage of RTW laws, both on labor issues and other dimensions.¹⁸ To answer the question we posed at the start of our introduction, our analysis suggests that rightward shifts in American politics, rising inequality, and labor union strength are indeed all causally related. Changes in state labor policy that have weakened the labor movement have durably disadvantaged the Democratic party, shifting politics and policy to the right across the U.S. states and thus limiting possibilities for economic redistribution through the political system. Our paper thus contributes to the understanding of what labor unions do in politics by identifying the specific ways that the labor movement has built up its alliance with the Democratic party, and also how recent changes in state labor policy have had lasting ramifications for U.S. politics.

The rest of our paper 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. We conclude in the sixth section.

¹⁸As we explain below, we use two measures of state policy liberalism and find similar results in both cases. One measure comes from the work of Caughey and Warshaw (2016), who use Bayesian item response theory to estimate ideal points of state policy ideology based on a battery of various state policies. Another measure comes from Grumbach (2017), who sums up substantively important liberal and conservative policies to produce an index of state policy ideology.

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.

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 other characteristics of the counties themselves. In addition to Holmes (1998)’s work 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 very 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 attitudes; Clinton and Sances (2017)). 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 between a RTW and non-RTW state are attributable to the RTW laws, and not other characteristics of the two sets of counties.

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). Before 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 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 because unions may have mobilized their workers to support candidates from both parties. After 1980, on the other hand, ideological sorting between the two parties was well underway—and so it is more reasonable to focus on the electoral implications of labor strength for Democratic electoral victories (McCarty et al. 2006). As we show in our appendix, changing this starting point by one or two election cycles does not appreciably alter our results.

[Figure 3 about here.]

Figure 3 plots the counties that we identify as border pairs and that we will focus on in our analysis. These counties are those that 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. Southern and Southeastern counties are not included because RTW laws were always in place in these states, offering us no opportunities to observe control counties in this region. Within our sample, there are three types of border counties. One group of counties are those 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. 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. In Figure 4, we plot the number of border counties paired to a county with a different RTW status in each presidential election cycle.

[Figure 4 about here.]

Our border county approach is intended to compare like counties with like. Do border county pairs actually look similar to one another? Figure 5 answers this question by comparing 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. 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 comparison across many different variables with different scales, these differences in means are standardized by the standard deviation of each variable. Looking first at all counties, we see that there are 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 5 about here.]

The picture changes considerably when we restrict our attention to the border county pairs in the right-hand plot. Border county pairs are nearly identical on most demographic characteristics we examine. 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 outcomes that could be plausibly affected themselves by RTW laws and therefore might be considered to be part of the treatment (eg Moore 1998). In addition, the ways that 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.

So 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.¹⁹ It is hard, then, to reach a single conclusion about the directionality of the remaining small bias in this sample.

We also find no systematic evidence that counties that will eventually become RTW were trending differentially from their cross-state neighbors before passage of RTW. We plot these pre-RTW trends in Figure A.1.²⁰ Differential pre-RTW trends might have indicated that there are other factors explaining the passage of RTW laws—factors that could also help explain any changes in Democratic electoral performance. As the plots indicate, there is little evidence of statistically discernible trends in RTW counties on these variables before the passage of RTW one way or another.

The empirical approach we employ is relatively straightforward. The unit of analysis is the county-year from 1980 to 2016. We fit OLS models and 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:

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

where outcomes are either democratic vote share or turnout.²¹ We include county and year fixed effects and cluster standard errors by state because RTW laws are state level.

¹⁹Similarly, 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 lower levels of manufacturing push in the opposite direction.

²⁰We graph state border and year demeaned values of these variables against timing before the passage of RTW laws.

²¹We 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.

In our preferred specification, however, we zoom in to just counties on state borders and estimate:

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

which includes τ_{pt} , year by border pair fixed effects. Thus, only variation from county pairs with different RTW statuses identify the main RTW effect. Here we cluster two ways, by state and by county border pair.

If our theoretical expectations are correct 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, then we ought to 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 also observe a drop in turnout.

Our main results focus on presidential elections, though we follow up with similar findings for Senatorial, House and gubernatorial races.²² As we explain in the introduction, we prefer the presidential elections results for several reasons. One is that presidential elections have the virtue of comparing the performance of the same candidate across the entire country, holding constant the quality of those candidates (which would otherwise vary across US Senate, House, and gubernatorial races). Another is the problem of redistricting, which affects our US House results and is a potential confounder and an alternative explanation for any decline in electoral performance we identify. Lastly, the data for Senatorial and gubernatorial elections is much sparser given variation across the states (and thus county border pairs) in when elections are held. Notwithstanding these concerns, the fact that we find similar effect sizes across all of these different levels of government suggests that our RTW findings are not merely capturing the idiosyncrasies of campaigns for any one particular

²²Unfortunately state legislative districts do not always fall along county lines, complicating the estimation of county-level vote totals for these races. In addition, we were unable to identify consistent cross-walks for state legislative districts to counties over the period we are studying (1980-2016).

office and reflect a more general change following the passage of RTW.

3 Right-to-work Laws and Presidential Elections

Before we present our main empirical results, we first offer a graphical representation of our main findings in Figure 6, 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). This plot thus suggests that RTW laws may have a negative effect on Democratic electoral prospects—and one that warrants a closer inspection.

[Figure 6 about here.]

[Table 1 about here.]

We document the 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. Across all specifications we see consistent negative and significant correlations between the passage of RTW laws and Democratic electoral outcomes and presidential election turnout. 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 2 and 3. In all of the all county samples, we cluster standard errors at the state level. Examining just 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.

But do RTW law 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 statues to generate our estimated effect of RTW. We find RTW laws reduce democratic vote shares by 3.94 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 1.97 points.

RTW laws reduce democratic vote share by nearly 4 points and turnout by nearly 2 points. Are these large or small effects? We argue that they are quite meaningful in the context of tight presidential races, in which states can be won or lost on the basis of one or two percentage points. In 2016, Hillary Clinton lost Wisconsin, Michigan, and Pennsylvania in 2016 by less than a percentage point each. Table 1 thus provides compelling evidence that unions have provided essential resources for the election of Democratic presidential candidates, in part by turning out voters. RTW laws may have, in turn, demobilized potential Democratic supporters in presidential races.

[Figure 7 about here.]

Our main RTW effects are robust to a number of other modeling strategies, as we show in Figure 7. The first row summarizes the RTW coefficients from our preferred specifications in Table 1 looking at border county pairs and adding 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 third row of the figure excludes southern states from our analysis and little changes. In the fourth row, we add in time-varying, county-level controls (summarized in Figure 5), and indicates very similar results to our preferred specification.²³

States that adopted RTW laws may have also adopted other legislation that would have

²³If anything the turnout estimates become more precise. The controls include the share of the population living in urban areas, white share of the population, native 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.

demobilized Democratic voters at the same time, either de facto or de jure. If, for instance, states adopted other policies at or around the same time as RTW laws that reduced the turnout of Democratic voters, we might be concerned that our results really reflect that alternative policy—and not RTW laws. This is especially true for the more recent enactments of RTW laws, which occurred after the GOP gained full control of state legislatures and governorships after 2010 and began enacting a raft of conservative policy priorities. 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 were designed to demobilize traditionally Democratic constituencies, like college students, minorities, and poorer voters (Bentele and O’Brien 2013; Berman 2015). We thus 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 second to last row of Figure 7 indicate that controlling for voter ID laws does not appreciably change our findings.²⁴ In the appendix, we also show, using individual-level survey data, that racial and ethnic minorities were 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.²⁵

Beyond strict voter ID laws, we also include another measure of conservative legislation intended to hobble Democrats. 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 the states and at its peak in the early 2000s counted between a third and a quarter of all

²⁴Our record of strict voter ID laws comes from the National Conference of State Legislatures: <http://www.ncsl.org/research/elections-and-campaigns/voter-id-history.aspx>

²⁵More 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.

state legislators as members. ALEC has promoted both RTW and voter ID laws, along with a slew of other measures intended to strengthen the political position of conservatives (Hertel-Fernandez 2014, 2017). Drawing on another dataset of enacted ALEC bills from 1996 to 2015 (Hertel-Fernandez 2017), we created a binary indicator of whether states enacted an ALEC bill in a given year (excluding ALEC bills related to labor unions). As the final row of Figure 7 indicates, the effect of RTW laws on Democratic vote share remains similarly sized as the other specifications. The turnout results, on the other hand, shrink a bit and are much less statistically precise, though the smaller sample size of this model (1996 to 2016, when we have ALEC data) may explain this.

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.

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? We focus on Presidential elections initially for three reasons. First, only with a national candidate are the year fixed effects (or the county border pair fixed effects) actually able to net out the effects of candidate quality or campaign-specific shocks; House, Senate, and gubernatorial elections all end at a state’s borders. Second, redistricting of house districts may complicate the comparison of county data over time—before and after redistricting counties include different mixes of candidates and districts. Third, because of the staggered nature of Senate elections and the variation across states in the timing of gubernatorial elections, not every county-border pair has a pair of elections in each year. 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 state gubernatorial, US House, and US Senate level. The results for Democratic vote share are less precise for the non-Presidential elections, though both the Senate and House effects (-4.4 and -4.1) are close in magnitude to the Presidential effect. The turnout

effect of RTW, though not significant for House elections, is similarly stable in magnitude, ranging from -1.97 to -2.49.

[Table 2 about here.]

4 Right-to-work Laws and State Legislative Elections

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 such levels. 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 legislature 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.

Examining Democratic legislative seat shares from 1980 to 2016 before and after RTW enactment and including state and year fixed effects, we see a very 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 6 to 13 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.]

5 Mechanisms for the Right-to-work Effect: Campaign Mobilization and Contributions

Why do RTW laws reduce Democratic vote share? What do unions do to drive 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, 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 GOTV contact—particularly among potential union members—and limit unions as a fund-raising source for Democrats.

5.1 Campaign Contact

To better capture one of the mechanisms through which RTW matters for elections, we turn to individual-level data from the American National Election Studies (ANES) time series cumulative file. The advantage of the ANES is that it offers high-quality representative surveys of Americans in election-years, potentially dating as far back as 1948. These surveys include a range of questions about Americans’ voting habits and overall participation in politics. Another advantage to the ANES data is that we can distinguish between different types of individuals, permitting us to consider how RTW laws might affect different individuals in varying ways. This permits us to conduct a series of placebo tests, as we expect that RTW laws ought to have the strongest effects on workers who would be most likely to be mobilized by unions—non-professional, non-managerial workers—and much smaller (if any effects) on professional and managerial workers.²⁶

²⁶In 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.

The disadvantage to the ANES, however, is that given sample sizes ranging from 1,000 to 2,000 each election year we cannot employ the same border county pair research design as we did earlier. Instead, we have to study the effect of RTW laws at the level of U.S. states. The trade-off we are making is thus between understanding the mechanisms driving the results we identified earlier and the opportunities for clean causal inference. We therefore emphasize that the individual-level results ought to be judged in concert with those from the county-level analysis.

The county-level analysis indicated that RTW laws reduced turnout, thus suggesting that weaker unions might mean lower turnout of reliably Democratic voters, but we could not test this mechanism directly in aggregate data. Why might RTW laws lead to lower turnout among non-professional workers? 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 1984 to 2012: “During the campaign this year, did anyone talk to you about registering to vote or getting out to vote?”

Our set-up is relatively straightforward: we estimate linear probability models of a respondent indicating get-out-the-vote contact in an election year. We estimate the effect of whether the state in which an ANES respondent resides had a RTW law in place. We then interact this variable with an indicator for whether a respondent was employed in occupations excluding managers or professional workers, a category we call *Non-Professional Workers*. These workers are the most likely potential union members we can identify in the ANES. We also add in state and year fixed effects.²⁷ We apply ANES survey weights and

²⁷In some models, we add in a full battery of individual control variables, which 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.

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—would 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 campaign as the outcome. As before, we also include state and year fixed effects, and the individual control variables in some models. 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 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.²⁸

5.2 Union and Campaign Fundraising

The 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 thus dampening Democratic vote shares. But by weakening union membership, do RTW laws have other consequences for the labor movements political activities? Next, we consider the effect of RTW laws on 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 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). If RTW laws reduce union membership rolls, and

²⁸We find no evidence that the non-turnout political activity of workers, non-professional or professional, changes after RTW laws. That is, with the standard ANES batteries of political participation questions as outcome variables, we found no interaction effects between RTW laws and non-professional worker status.

thus their budgets, then we should see that unions in RTW states spend less on elections. In this subsection, we find evidence for these negative effects of RTW laws on union campaign contributions.

Pooling available data on state and local campaign contributions from 1996 to 2016 from the Institute on Money in State Politics, Figure 8 indicates how the share of contributions from labor unions changes in never-RTW states and RTW states. Before the passage of RTW laws, never-RTW states look relatively similar to RTW states in the proportion of campaign contributions coming from the labor movement. However, following the passage of RTW laws, labor unions account for a much lower percentage of state and local contributions in RTW states compared to those states without RTW laws in place.

[Figure 8 about here.]

Estimating the difference in difference model displayed in Figure 8 in Table 5, we find that RTW laws reduce campaign contributions from unions, as measured as the share of total campaign spending. Again, we are limited by contribution data at the state, rather than county, level and unable to utilize our cross-RTW-state-line county-border set-up. However, the pretrends in 8 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). RTW laws reduce private sector union contributions by 1 to 2 percentage points. There may also be a negative effect on public sector unions, because total contributions from all unions falls by 2.5 to 3 points.

[Table 5 about here.]

The decline in labor contributions appears to have strongly disadvantaged Democrats. As we show in columns 5 and 6 of 5, the share of all state and local contributions flowing to

Democrats falls by more than 2.0 percentage points in RTW states following the enactment of RTW laws. It appears that Democrats are unable to replace the funding they are losing from labor unions following the passage of RTW laws, and that the balance of campaign funding tilts in favor of the Republican party.

6 The Downstream Political Consequences of Right-to-work 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 state legislators and the overall ideological liberalism of state policy. We find that working class candidates are less likely to hold elected state office and that state policy moves to the ideological right following the passage of RTW laws.

6.1 The Effect of RTW Laws on Who Serves in State 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 who came from working class or blue-collar occupations act differently from politicians who spend 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 are 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, and across the states, legislatures with a greater proportion of blue-collar workers serving in office 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 people, 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 there is data on the occupational characteristics of those lawmakers, was 3% (Carnes 2013). These rates varied, however, from 0% (in California) to 10% (in Alaska) in state legislatures in 2007.

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, on the other hand, 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 those workers through grassroots voter mobilization and campaign contributions. 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; 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 thus reduce the representation of the working class? Drawing on the 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. 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.]

In all, this analysis of working class political representation in Table 6 indicates that RTW laws do not only shape whether Democrats are elected to office—but also may well affect the backgrounds of elected politicians, which in turn has significant consequences for the economic policies and decisions pursued by those lawmakers. With fewer working class politicians in office, RTW states are less likely to pursue redistributive economic policies.

6.2 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. We hypothesize that this will be a 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 the 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, that should also move state policy to the right.

[Figure 9 about here.]

To assess this question, 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. This plot appears in Figure 9, and shows that the average level of state policy liberalism (examining all policies in the Caughey and Warshaw dataset) falls following the passage of RTW laws, indicating that state policies move in a sharp rightward direction. Of course, part of 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.²⁹

The size of the post-RTW rightward shift in state policy is sizable and substantively relevant. The difference implied by 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 2016.³⁰ 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 we should be careful in not interpreting these differences as causal effects, they are consistent with our theoretical expectation that state policy should shift rightward following the passage of RTW laws. 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.4 in the Appendix). These results emphasize that our findings are not simply an artifact of the Caughey and Warshaw (2016) data.

²⁹This 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.

³⁰When we implement a difference in difference to estimate the effect of RTW laws on state policy liberalism (excluding RTW laws) we find a shift to the right of 0.30 points (standard error 0.12), clustering by state.

7 Conclusion

The anti-tax political activist Grover Norquist recently declared that while President Trump may be historically unpopular, the GOP could still “win big” in 2020.³¹ 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 it, if union reforms cutting 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, Norquist reasoned, would not just have economic consequences. It would also have 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 state-level conservative activists who have been driving the recent push to enact further RTW laws in newly GOP-controlled state governments. Tracie Sharp runs a national network of state-level 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 2017). 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.”³²

In this paper, we have tested the political arguments advanced by Norquist and Sharp, examining the short- and long-run political consequences of state RTW laws. Using a credible design for causal inference comparing otherwise similar counties straddling state borders, we find that the passage of RTW laws led Democratic candidates up and down the ballot to receive fewer votes. The effects were significant: in presidential elections, for instance, Demo-

³¹<http://www.ozy.com/politics-and-power/why-republicans-and-trump-may-still-win-big-in-2020-despite-e-78775>.

³²<https://www.wsj.com/articles/the-spoils-of-the-republican-state-conquest-1481326770>

cratic candidates received 4-6 percentage points fewer votes following the passage of RTW laws. RTW laws also lower turnout in both federal and state races as well. 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 of a 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. But we have not only examined the electoral consequences of state RTW laws. We also examined how, by weakening the relationship between unions and the Democrats, RTW laws may have fundamentally changed the political landscape across the U.S. states. Working class candidates—politicians most likely to be backed by the labor movement—are less likely to hold office in states following the passage of RTW laws. State policy as a whole, moreover, moved sharply to the ideological right in RTW states following the passage of those laws.

Beyond revealing the importance of state RTW laws for a broader set of political outcomes than has been previously appreciated, our paper makes three broader contributions to the study of labor unions, the historical development of the American political economy, and public policy. First, we shed new light on older debates about what unions do in the United States. While a long line of work has shown the ways that labor unions directly affect the wage and income distributions—by compressing wages in unionized firms and industries—we emphasize the political nature of labor organizations. Another important reason why unions affect inequality is not through their bargaining practices, but through the politicians and policies they support in the realm of politics. Yet the capacity of unions to affect the income distribution through this second channel is waning as labor’s strength—and political clout—diminishes in the face of unfavorable state policy, especially RTW laws.

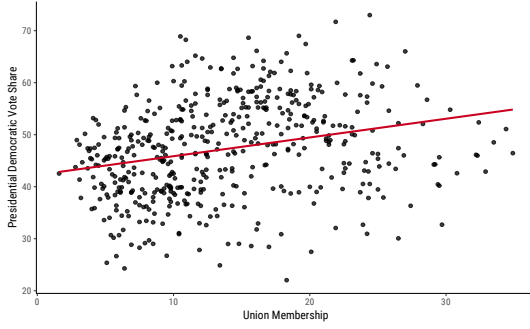
A second broader insight we reach in this paper is to develop in more detail the relationship between interest groups and political parties. Political parties can be thought of as coalitions of policy-minded activists and interest groups rather than office-seeking and office-

holding politicians (Bawn et al. 2012). Our work in this paper supports this perspective, tracing the specific resources that labor unions were able to offer to the Democratic party coalition—campaign financing and vote mobilization—and how changes in these resources can have large-scale consequences for the electoral fate of Democratic politicians, and the direction of policy.

Returning to the quotes from conservative activists that we offered up at the start of this section, our final conclusion underscores the path-dependent nature of political power in the United States—and more specifically, how public policy can be used as a tool by political winners to entrench their victories over time. Gaining control of state governments, conservative Republicans have pursued policies that not only fit well with their legislative agenda but that also fundamentally alter the balance of political power available to their opponents. RTW laws have durable consequences that last for years once in place, disadvantaging the political opponents of conservative activists in ways that may be hard to reverse, at least in the short-run. Given the increasing number of states enacting RTW laws in very recent years (see Figure 10), Norquist and Sharp may well be right that the GOP will lock-in many of the impressive gains that it has made in control of state government since 2010 (see Figure 11).

[Figure 10 about here.]

[Figure 11 about here.]

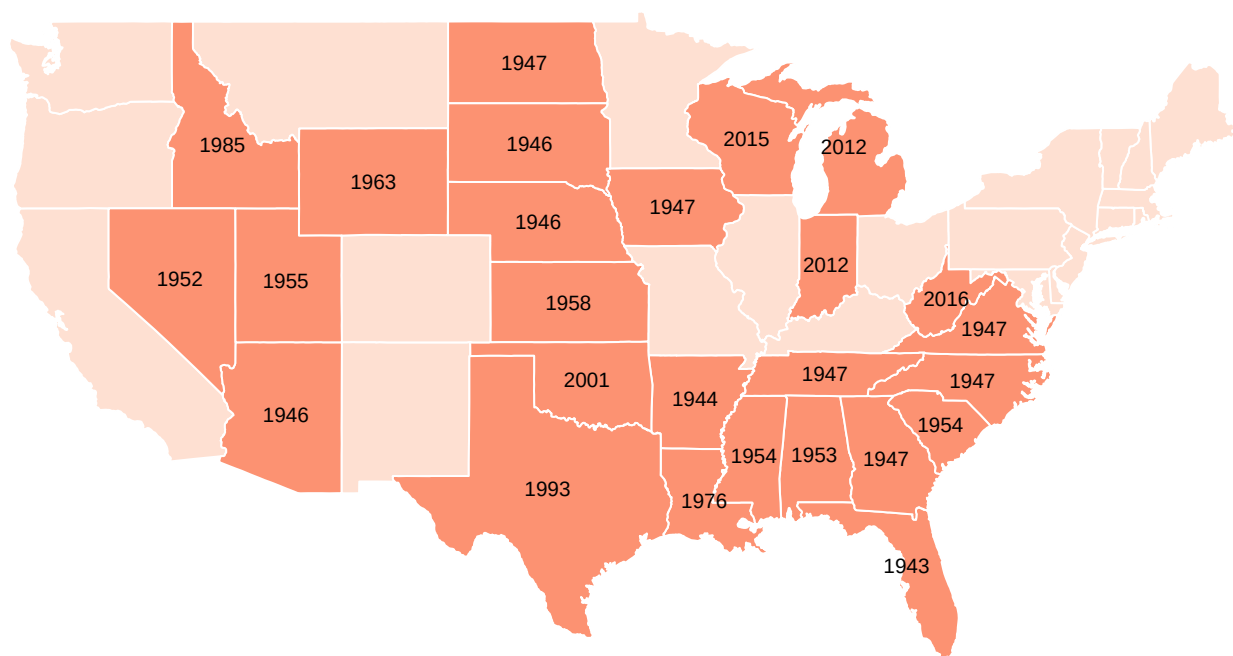


(a) State Democratic Vote Share and Union Membership



(b) State Policy Liberalism and Union Membership

Figure 1: States with more 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. Source of union density is Hirsch et al. (2001). Source of state policy liberalism is Caughey and Warshaw (2016).



Right to Work Status, 2016 No RTW Currently RTW

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

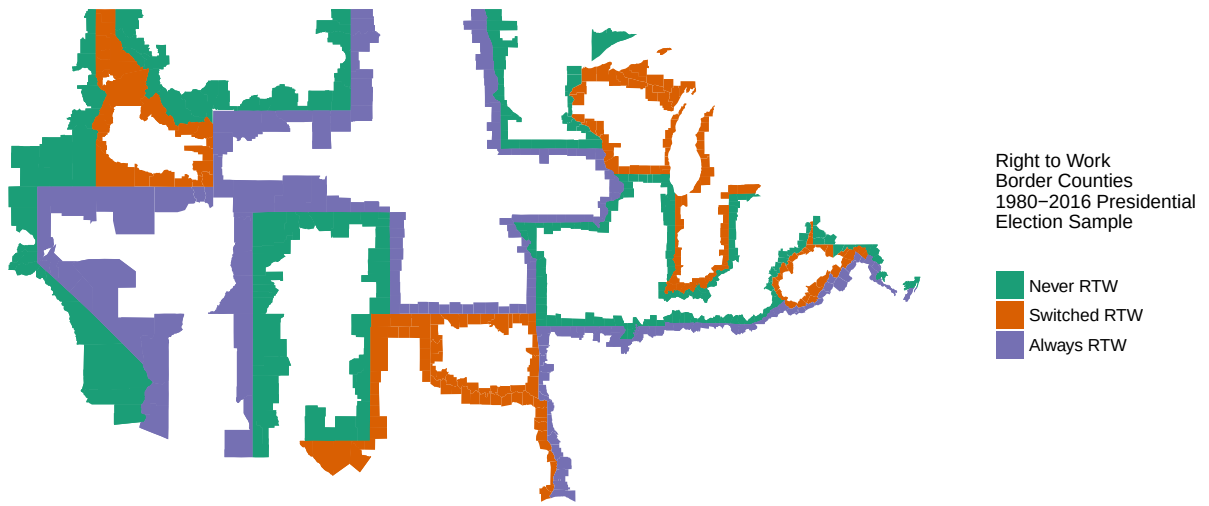


Figure 3: Border county pairs in our main specifications examining the effect of state right to work laws on presidential vote shares, 1980-2016.

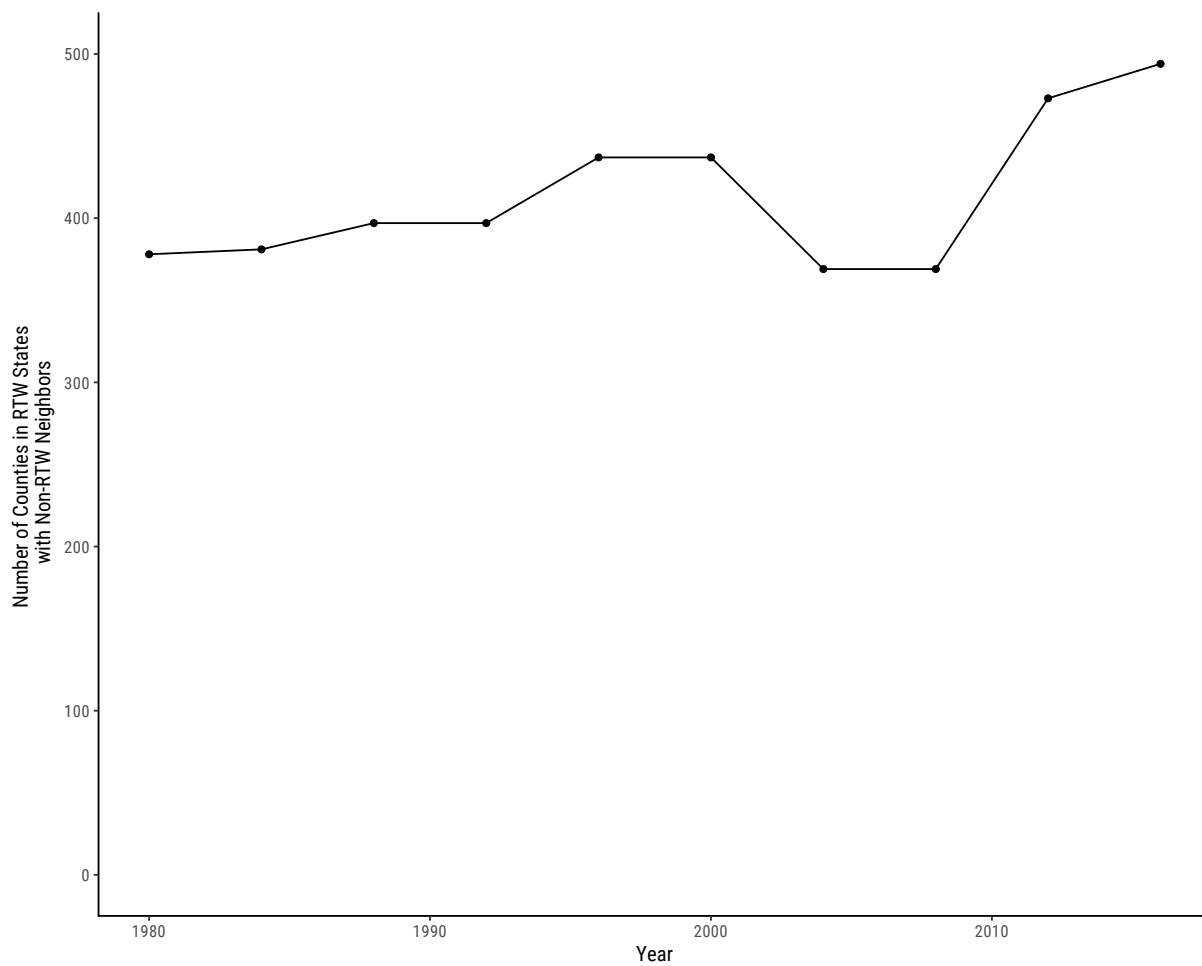


Figure 4: The number of border counties in our sample in each election cycle where state right to work laws differ on one side of the border to the other.

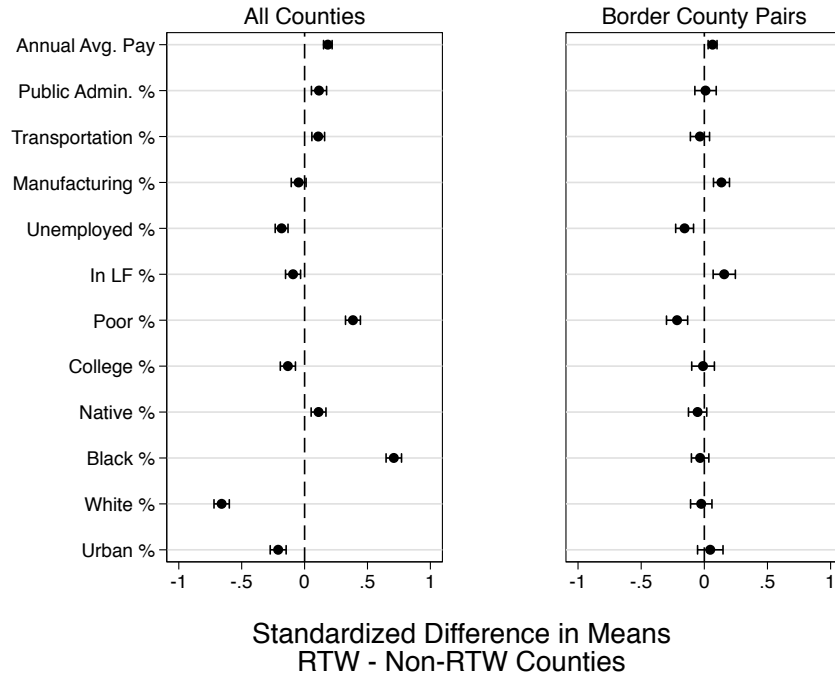


Figure 5: Balance on covariates between right-to-work and non-right-to-work counties, examining all counties (left plot) and only border county pairs (right plot). Full 1980-2016 sample. Lines indicate 95% confidence intervals. Standard errors clustered by county in the “All Counties” analysis and by 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.

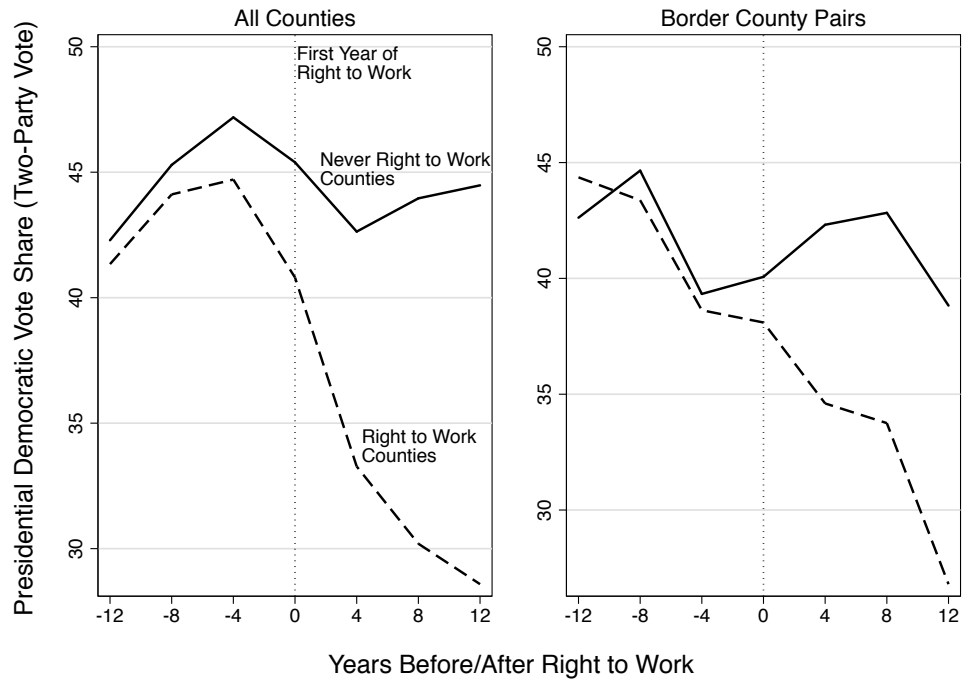


Figure 6: Difference-in-differences summary analysis of right-to-work laws and Democratic vote share, 1980-2016. Left plot examines all counties and right plot examines only border county pairs.

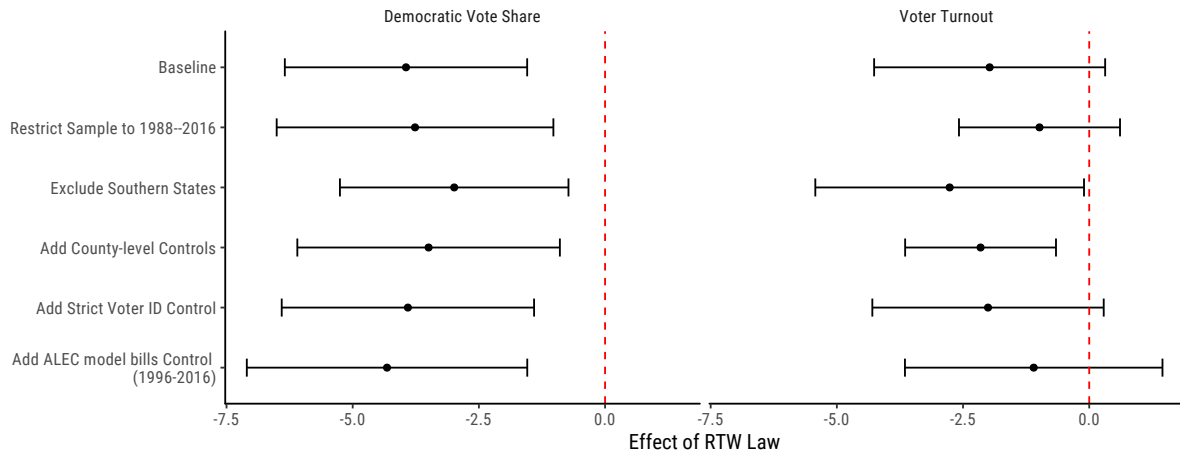


Figure 7: The effect of state right-to-work laws on presidential elections, robustness checks. All models include county and border pair by year fixed effects. We cluster standard errors two ways, by border pairs and by state. The sample includes only counties on state borders. Both vote share and turnout measured on 0-100 scale.



Figure 8: 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.

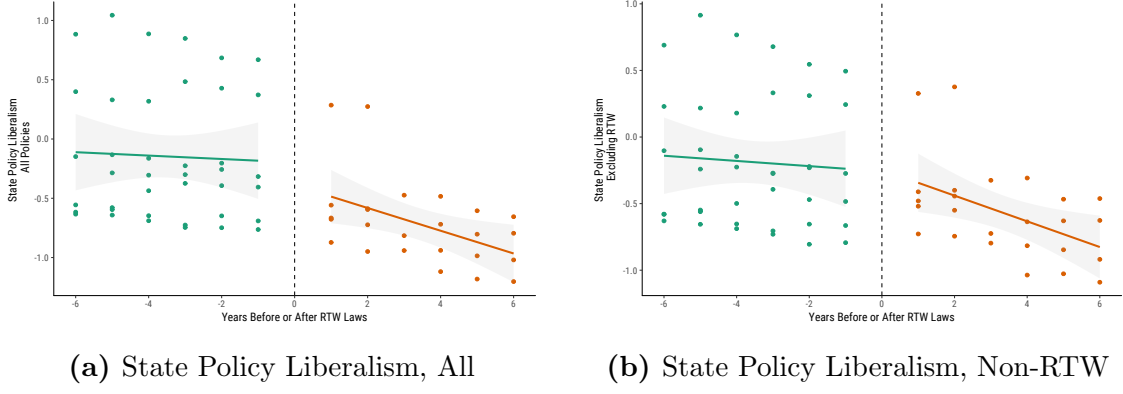


Figure 9: Relationship between state policy liberalism and state right-to-work laws, 1980-2014. Figure plots median state policy liberalism in states before and after the passage of right-to-work laws. Left plot includes all policies, right plot excludes right-to-work laws from the estimation of state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

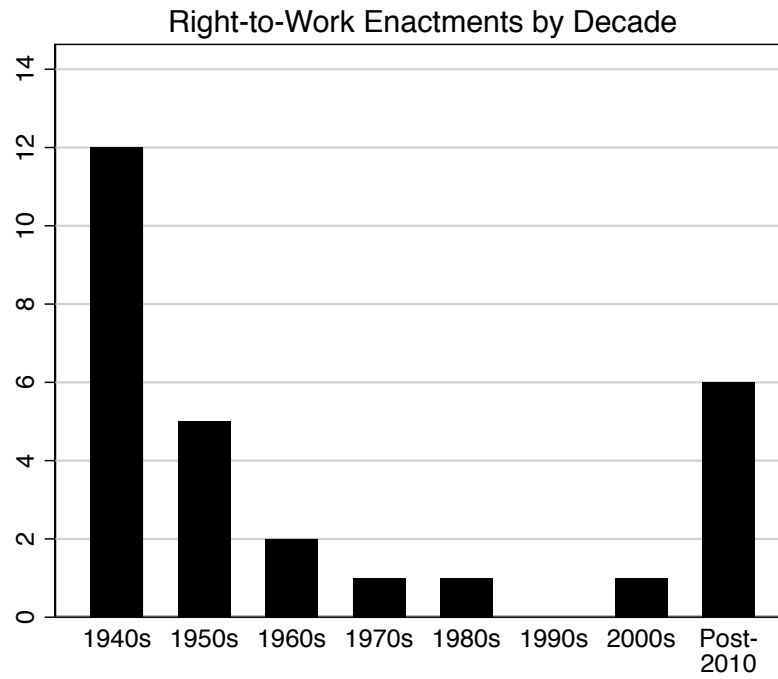


Figure 10: States enacting right-to-work laws by decade.

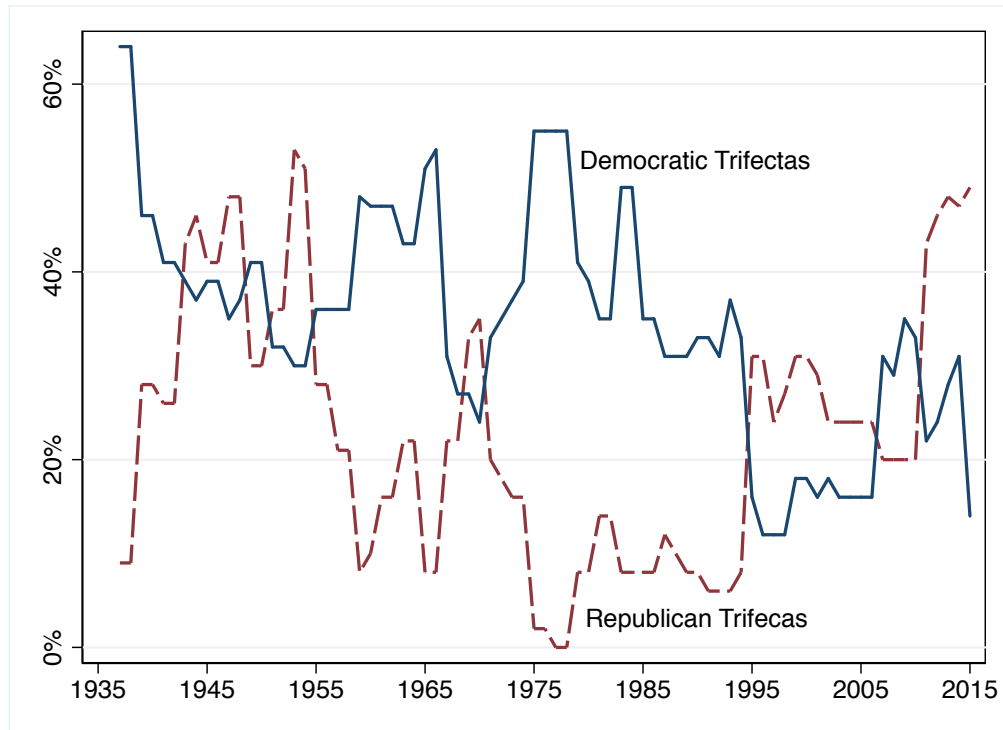


Figure 11: States under full Democratic or Republican control (legislative branch and executive). Data from Carl Klarner's Database of State Partisan Control

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*** (1.725)	-6.277*** (2.267)	-3.858*** (1.391)	-5.579*** (1.716)	-6.318** (2.488)	-5.093*** (1.407)	-3.944*** (1.225)
Constant	44.276*** (0.822)			44.641*** (0.948)			
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	Yes
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	30,625	30,625	30,625	25,494	25,494	25,494	25,494
Adjusted R ²	0.032	0.770	0.831	0.040	0.800	0.855	0.900
	Panel B. Voter Turnout						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	-3.341* (1.791)	-4.010*** (0.840)	-2.319** (0.946)	-2.925* (1.751)	-3.455*** (1.116)	-2.663*** (1.028)	-1.973* (1.168)
Constant	58.849*** (1.190)			58.403*** (1.182)			
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	Yes
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	30,601	30,601	30,601	25,464	25,464	25,464	25,464
Adjusted R ²	0.026	0.771	0.806	0.021	0.779	0.808	0.809

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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 we stack the data accordingly which is why the 1173 unique counties on a state border translate to more than 25 thousand 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.

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

	Panel A. Democratic Vote Share			
	Presidential	Senate	Governor	House of Representatives
	(1)	(2)	(3)	(4)
Right to Work	-3.944*** (1.225)	-4.396 (3.972)	-0.523 (3.021)	-4.081* (2.426)
County FE	Yes	Yes	Yes	Yes
Border Pair × Year FE	Yes	Yes	Yes	Yes
Observations	25,494	32,314	33,530	23,832
Adjusted R ²	0.900	0.517	0.648	0.471
	Panel B. Voter Turnout			
	Presidential	Senate	Governor	House of Representatives
	(1)	(2)	(3)	(4)
Right to Work	-1.973* (1.168)	-2.440* (1.423)	-2.488** (0.972)	-2.332 (2.549)
County FE	Yes	Yes	Yes	Yes
Border Pair × Year FE	Yes	Yes	Yes	Yes
Observations	25,464	32,293	43,848	23,823
Adjusted R ²	0.809	0.814	0.787	0.865

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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 20 thousand 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 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	-13.067*** (4.203)	-6.008** (2.692)	-18.754*** (4.917)	-13.349*** (3.929)	-12.124*** (4.560)	-3.699*** (1.073)
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	793	793	701	701	755	755
Adjusted R ²	0.772	0.861	0.758	0.809	0.803	0.871

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Standard errors clustered by state. Seat share outcomes are measured 0 to 100 percent. Unicameral Nebraska is not included in our sample.

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 (0.044)	-0.002 (0.043)	-0.023 (0.051)	-0.024 (0.051)	-0.053** (0.021)	-0.014 (0.029)
Non-Professional Worker	0.010 (0.012)	0.022 (0.016)	0.016 (0.017)	0.015 (0.023)	-0.013 (0.014)	0.021 (0.018)
RTW $\times$ Non-Professional	-0.103*** (0.031)	-0.112*** (0.031)	-0.129*** (0.038)	-0.123*** (0.038)	-0.027 (0.022)	-0.053** (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

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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 Union 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*** (0.449)	-2.336* (1.235)	-2.526*** (0.627)	-3.095* (1.614)	-22.177** (9.372)	-19.984*** (5.803)
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	741	741
Adjusted R ²	0.506	0.591	0.473	0.585	0.397	0.488

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Standard errors clustered by state. Union campaign contribution shares and share of contributions by party measured on 0-100 scale.

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

	Working Class Share of State Legislature					
	(1)	(2)	(3)	(4)	(5)	(6)
Right to Work	-1.365** (0.608)	-1.307** (0.612)	-2.946*** (0.553)	-2.946*** (0.553)	-2.935*** (0.588)	-1.422** (0.696)
Legislative Pay (in \$10ks)				-0.793*** (0.174)	-0.696*** (0.167)	-0.159 (0.461)
Session Length (Days)				-0.0004 (0.007)	-0.001 (0.007)	0.010 (0.007)
Staff Size				-0.068 (0.063)	0.014 (0.067)	0.133 (0.155)
Term Limits						-0.208 (1.195)
Top 1% Income Share						-0.069 (0.205)
Percent Black						-0.102 (0.446)
Percent Urban						0.098 (0.180)
Percent Poor						0.290 (0.192)
GOP Vote Share Average						0.128*** (0.048)
Per Capita Income						-0.027 (0.064)
Constant	5.009*** (0.524)					
Year FE	No	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	Yes
Observations	200	200	200	200	200	200
Adjusted R ²	0.040	0.103	0.366	0.366	0.383	0.549

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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).

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A Supplementary Materials

A.1 Border County Pair Balance

In this section, we provide further graphical evidence of the balance between border county pairs, including the absence of pre-treatment trends in treatment border counties (Figure A.1); the absence of pre-treatment trends in presidential Democratic vote share in treatment border counties (Figure A.2); and balance in changes in key economic variables (with 4- and 8-year differences) between treatment and control border counties (Figure A.3).

[Figure A.1 about here.]

[Figure A.2 about here.]

[Figure A.3 about here.]

A.2 Additional Border County Results

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 Subgroup Effects: 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 they were 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.

In these models, we use the same set-up as for the presidential turnout ANES results described in the body of the paper. 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.³³

[Table A.4 about here.]

A.4 Alternative Measures of State Policy Liberalism

In Figure A.4, 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.

[Figure A.4 about here.]

[Table A.5 about here.]

³³In the black and Hispanic turnout models (Columns 3 through 6 of Table A.4), we include age and age squared. In the black turnout model, we include a control for Hispanic. In the Hispanic turnout model, we include a control for black. In the youth turnout model (Columns 7 and 8 of Table A.4), 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).

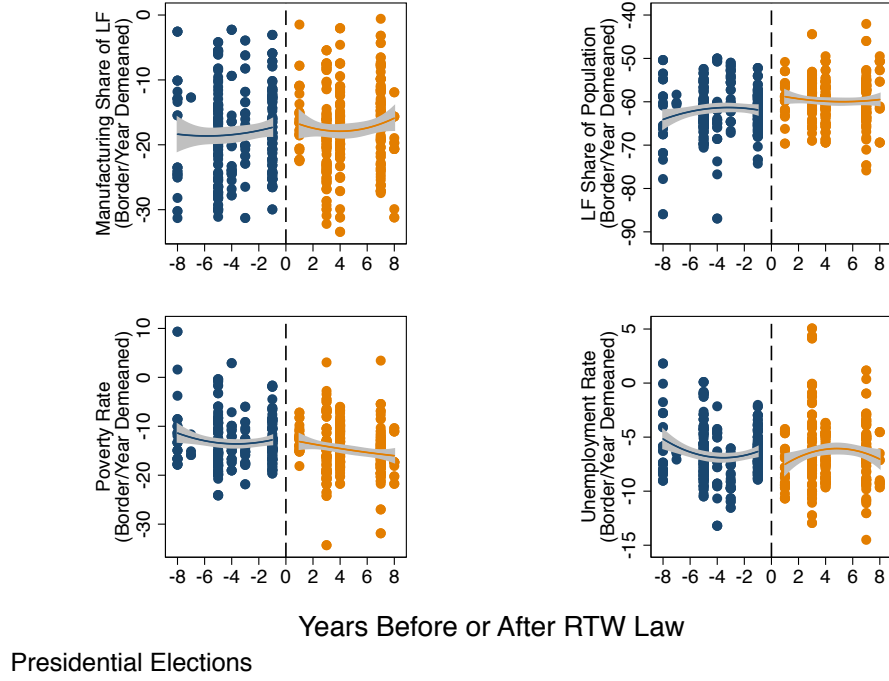


Figure A.1: Balance in pre-trends in covariates examining border county pairs. Full 1980-2016 sample. Quadratic regression lines of best fit shown. Shading indicate 95% confidence intervals. Standard errors clustered by border county pairs. Values state border and year demeaned. Data from the US Census for 1980, 1990, 2000, and 2010. Intercensal values carried forward from previous census year.

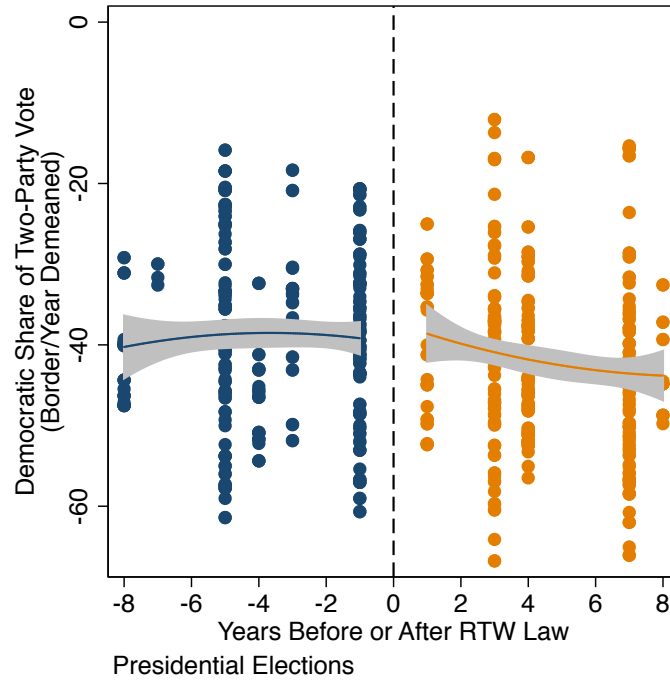


Figure A.2: Balance in pre-trends in Democratic presidential vote share examining border county pairs. Full 1980-2016 sample. Shading indicate 95% confidence intervals. Standard errors clustered by border county pairs. Values state border and year demeaned. Data from the US Election Atlas.

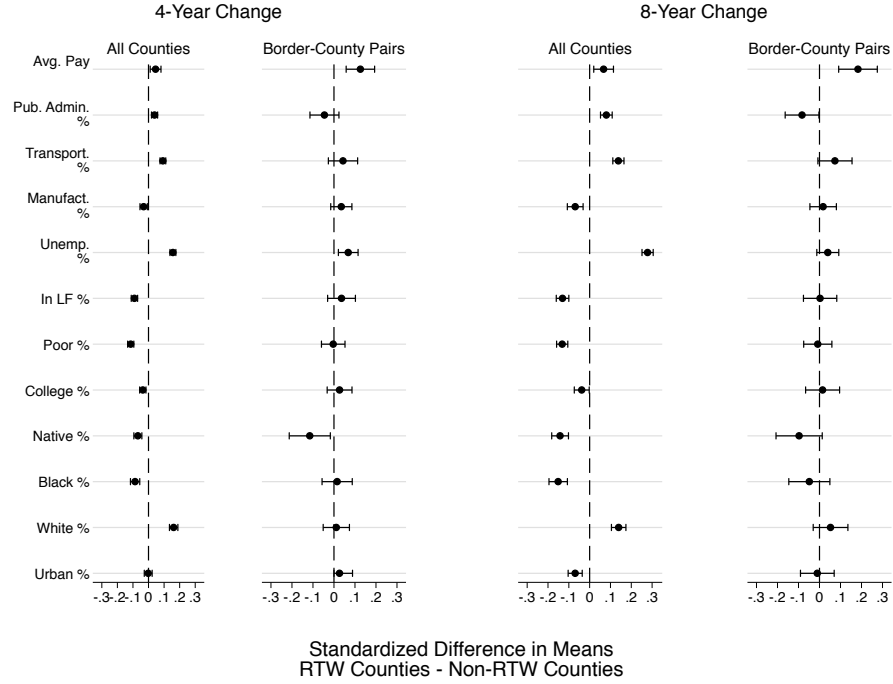
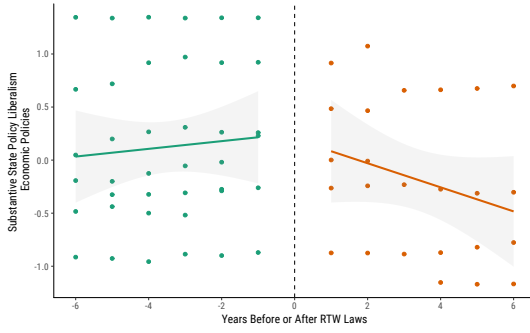
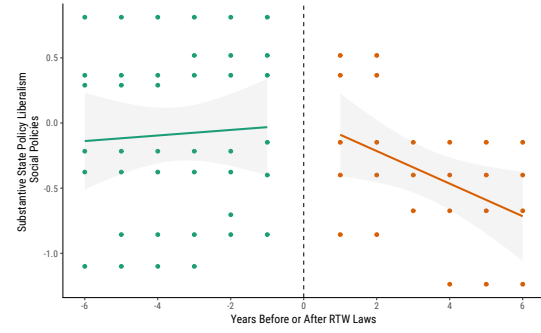


Figure A.3: Balance on changes in covariates between right-to-work and non-right-to-work counties (either 4 or 8 year changes), examining all counties (left plot) and only border county pairs (right plot). Full 1980-2016 sample. Lines indicate 95% confidence intervals. Standard errors clustered by county 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.



(a) State Economic Liberalism



(b) State Social Liberalism

Figure A.4: 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. Source of state policy liberalism is 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** (2.855)	-6.393 (4.156)	6.218* (3.485)	-6.434** (2.702)	-6.134 (4.342)	1.339 (4.062)	-4.396 (3.972)
Constant	48.173*** (1.318)			48.488*** (1.429)			
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	Yes
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	38,876	38,876	38,876	32,314	32,314	32,314	32,314
Adjusted R ²	0.022	0.453	0.581	0.029	0.482	0.599	0.517

	Panel B. Voter Turnout						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	-3.840* (2.167)	-3.659*** (1.124)	-3.626*** (1.190)	-2.857 (2.157)	-2.884** (1.244)	-3.991*** (1.015)	-2.440* (1.423)
Constant	51.127*** (1.330)			50.232*** (1.361)			
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	Yes
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	38,858	38,858	38,858	32,293	32,293	32,293	32,293
Adjusted R ²	0.020	0.757	0.800	0.012	0.795	0.837	0.814

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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 we stack the data accordingly which is why the 1173 unique counties on a state border translate to more than 32 thousand 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	-5.388* (2.865)	-4.211 (3.210)	1.327 (3.738)	-7.808*** (2.863)	-5.417 (3.544)	-0.810 (4.372)	-0.523 (3.021)
Constant	43.483*** (2.059)			45.534*** (1.986)			
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	Yes
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	41,406	41,406	41,406	33,530	33,530	33,530	33,530
Adjusted R ²	0.013	0.567	0.605	0.029	0.599	0.640	0.648
	Panel B. Voter Turnout						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	-2.187 (2.301)	-5.969*** (1.953)	-2.800* (1.682)	-2.668 (2.002)	-4.467*** (1.132)	-3.298** (1.339)	-2.488** (0.972)
Constant	48.121*** (1.656)			50.481*** (1.327)			
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	Yes
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	43,953	43,953	43,953	43,848	43,848	43,848	43,848
Adjusted R ²	0.005	0.766	0.806	0.008	0.767	0.801	0.787

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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 we stack the data accordingly which is why the 1173 unique counties on a state border translate to more than 32 thousand 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 (2.201)	-1.999 (2.582)	-0.133 (4.382)	-2.860 (1.883)	-2.463 (2.077)	-1.709 (3.581)	-4.081* (2.426)
Constant	47.625*** (1.215)			48.215*** (1.127)			
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	Yes
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	28,342	28,342	28,342	23,832	23,832	23,832	23,832
Adjusted R ²	0.006	0.493	0.643	0.008	0.527	0.664	0.471
	Panel B. Voter Turnout						
	All Counties			Border Counties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Right to Work	-3.576 (3.000)	-2.701*** (0.778)	-0.914 (1.297)	-2.958 (2.905)	-2.548*** (0.818)	-1.443 (1.150)	-2.332 (2.549)
Constant	47.549*** (2.171)			47.020*** (2.048)			
County FE	No	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	Yes
Census Division × Year FE	No	No	Yes	No	No	Yes	No
Border Pair × Year FE	No	No	No	No	No	No	Yes
Observations	28,334	28,334	28,334	23,823	23,823	23,823	23,823
Adjusted R ²	0.019	0.838	0.871	0.013	0.853	0.884	0.865

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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 we stack the data accordingly which is why the 1173 unique counties on a state border translate to more than 32 thousand 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 Individual Voter Turnout, 1980-2012

	Presidential Election							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Right to Work	0.029*** (0.011)	0.020* (0.010)	0.013 (0.010)	0.009 (0.006)	0.001 (0.011)	0.008 (0.010)	0.001 (0.010)	0.002 (0.008)
Non-Professional Worker	-0.102*** (0.010)	0.0003 (0.010)						
RTW $\times$ Non-Professional	-0.070*** (0.019)	-0.040** (0.017)						
Black			-0.035*** (0.013)	0.033** (0.016)				
RTW $\times$ Black			-0.027 (0.019)	-0.024 (0.017)				
Hispanic					-0.104*** (0.012)	0.026* (0.014)		
RTW $\times$ Hispanic					0.007 (0.034)	-0.011 (0.029)		
Young (under 25)							-0.260*** (0.014)	-0.085*** (0.017)
RTW $\times$ Young							0.022 (0.023)	0.022 (0.025)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	18,140	14,846	29,810	24,359	29,810	24,359	29,837	24,359
Adjusted R ²	0.514	0.583	0.507	0.590	0.509	0.590	0.534	0.591

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Standard errors clustered by state. Linear probability model. Individual controls include BLAH. Data from National Election Studies

Table A.5: 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*** (0.122)	-0.365*** (0.118)	-0.295** (0.125)	-0.230* (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 (0.079)	-0.005 (0.089)	-0.330*** (0.117)	-0.155 (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

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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).