Do Anti-Union Policies Increase Inequality? Evidence from State Adoption of Right-to-Work Laws

Vladimir Kogan1

Abstract
The distribution of income lies at the intersection of states and markets, both influencing and responding to government policy. Reflecting this reality, increasing research focuses on the political origins of inequality in the United States. However, the literature largely assumes—rather than tests—the political mechanisms thought to affect the income gap. This study provides a timely reassessment of one such mechanism. Leveraging variation in labor laws between states and differences in the timing of adoption of right-to-work (RTW) legislation, I examine one political mechanism blamed by many for contributing to inequality. Using a variety of panel designs, I find little evidence that RTW laws have been a major cause of growing income inequality, pointing to the importance of grounding theoretical arguments about the interrelationships between states and markets in a sound empirical reality.

Keywords
political economy, income inequality, right-to-work laws, unions, state politics

Introduction
Economic fundamentals can make or break political fortunes. But the flipside is also true: political processes shape the incentives and behaviors of market actors, affecting economic outcomes. The distribution of income in a society lies precisely at this

1Ohio State University, Columbus, OH, USA

Corresponding Author:
Vladimir Kogan, Department of Political Science, Ohio State University, 2140 Derby Hall, 154 N. Oval Mall, Columbus, OH 43210-1373, USA.
Email: kogan.18@osu.edu
intersection of states and markets, both influencing and being shaped by government policy. As Bartels (2008, 2–3) notes,

> While technological change, globalization, demographic shifts, and other economic and social forces have produced powerful pressures toward greater inequality in recent decades, politics and public policy can and do significantly reinforce or mitigate those pressures, depending on the political aims and priorities of elected officials.

The past decade has produced a flowering of research on the political origins of rising economic inequality, particularly in the U.S. context (e.g., Bartels 2008; Hacker and Pierson 2010; Kelly 2009). Much of this work documents empirical correlations between inequality and broad political variables, such as electoral rules (Iversen and Soskice 2006), partisan polarization (Enns et al. 2014; McCarty, Poole, and Rosenthal 2006), or aggregate policy liberalism (Kelly 2005). But the research provides almost no direct evidence for the hypothesized mechanisms through which these political factors are thought to ultimately shape labor market outcomes.

In this study, I provide causal evidence about the role played by one such mechanism: the enactment of anti-union government policies. A number of both popular and scholarly observers have identified the steady shrinkage of union rolls as a likely driver of rising inequality. Recent studies, utilizing both cross-national (Alderson and Nielsen 2002) and cross-state (Chintrakar 2011; Kelly and Witko 2012) comparisons, document a sizable correlation between declining union membership and growing income inequality. In one recent review, Manzo and Bruno (2014, ii) conclude that “the largest contributor to rising income inequality has been the gradual, long-term decline in labor union membership.” Many further link this decline to specific public policy choices. For example, legal scholar and former Department of Justice Attorney Michele Gilman (2014, 419) has written,

> The decline of American unions starting in the 1970s is traceable to several factors, including . . . congressional defeats of prounion reforms spurred by increased corporate donations to Congress; the appointment of Republican appointees to the National Labor Relations Board, which in turn issued decisions that limited union organizing; and the defeat of the 1981 air controllers strike by President Reagan,

echoing the arguments made by prominent political scientists including Jacob Hacker and Paul Pierson. In terms of strategies to reduce inequality, Paul Krugman (2007, 262) has argued that “the most important tool . . . is likely to be an end in the thirty-year tilt of government policy against unions.”

Although it is clear that the trends in union membership and income inequality have paralleled each other over time, there is little evidence to establish whether this correlation can be interpreted in a causal way. Providing a credible research design to identify the causal effect of one particular type of anti-union public policy—state right-to-work (RTW) laws—on the distribution of income is thus the purpose and ultimate contribution of this study.
I focus on RTW laws for several reasons. First, as I summarize below, there is strong evidence that the enactment of RTW laws in the states significantly weakened organized labor. Second, observers often point to these laws as a key driver of both union decline and rising inequality. For example, Hacker and Pierson (2010, 129) write that repealing such legislation represents the “Holy Grail” for organized labor. At a June 2015 congressional hearing, one witness—an academic economist—argued, “[R]ecent research has demonstrated that the decline of organized labor has contributed to the growth in inequality. . . . By significantly reducing union membership rates, policies like right-to-work laws contribute to income inequality both at home and globally.”

Third, variation in labor laws between U.S. states and differences in the timing of adoption provide a rare empirical opportunity to credibly identify the causal relationship between public policies toward organized labor and income inequality. Using new data on the distribution of income at the state level covering much of the twentieth century—including, crucially, years prior to the passage of the Taft-Hartley Act of 1947, which first authorized RTW laws—I employ a variety of panel methods to estimate the causal effect of these laws on multiple measures of income inequality.

Providing clear evidence about the consequences of specific public policies is critical because building and testing theories about how the political system influences the income distribution requires, at base, a clear understanding of the mechanisms that are available to government actors. In the absence of credible evidence about these mechanisms, scholars risk placing too much emphasis on some political choices while overlooking others. For example, McCarty, Poole, and Rosenthal (2006, chap. 6) point to the stagnation of the federal minimum wage as a central reason for why income inequality rose so sharply in the second half of the twentieth century. Yet rigorous empirical evaluations of the relationship between wage floors and income distributions (Autor, Manning, and Smith 2016) suggest that the erosion of the minimum wage, while a contributing factor, has not been a primary cause of growing inequality—particularly in the upper tail of the income distribution, where inequality has increased the most. Although partisan polarization may have indeed contributed to this growth, preventing increases in the minimum wage was probably not the primary channel through which polarization has operated. By examining another oft-cited culprit—the adoption of anti-union laws—this study helps provide much needed empirical focus for the growing literature on the political causes of inequality.

To preview the results, I find little evidence that the adoption of RTW laws has been a central driver of inequality in the states. In the full panel, these null effects are estimated quite precisely. Focusing specifically on the three most recent adoptions using the “synthetic control” approach, I find evidence that these laws affected the distribution of income in only one of these three states. These findings provide an important empirical foundation against which to evaluate many of the broad claims made in the existing literature and speak to current arguments and debates in the American political economy. The findings can also help inform the strategies of activists who seek to use the political process to bring about more equitable economic outcomes. I return to these broader lessons in the concluding section of this article.
Passing over President Truman’s objections in 1947, the Taft-Hartley Act represented an important rollback of the pro-labor provisions put into place by the National Labor Relations Act a decade earlier. The Taft-Hartley Act outlawed “closed shops”—contractual requirements that employers hire only union members—and Section 14(b) of the law permitted individual states to go further by also banning “union shops,” a mandate that workers join a union as a condition of employment. A number of primarily Southern states did so shortly thereafter. Figure 1 identifies the states that have enacted such RTW legislation, with most of these adoptions coming in the two decades immediately following the passage of the 1947 law.

Theoretically, there is strong reason to expect that the adoption of RTW legislation should have reduced the economic clout of labor unions in the affected states. Because federal law continued to require that benefits won by union negotiators apply both to dues-paying members and nonmembers alike, allowing individual workers to opt out of membership while enjoying all of its benefits opened the door to “free riding” (e.g., Olson 1965). The absence of compelled membership should have also weakened the bargaining power of organized labor, reducing both the union wage premium in unionized firms and the threat of organization in other companies (e.g., Farber 2005). Testing these expectations empirically is difficult, however, because the earliest available data on union membership at the state level date back only to the mid-1960s, after most states with RTW laws had already adopted them.

Cross-sectionally, there is indeed a strong negative relationship between the presence of RTW laws and union density. However, there remains substantial debate...
among economists about what to make of this correlation (for an overview, see Moore 1998; Moore and Newman 1985). Although some scholars point to it as evidence that RTW laws weakened organized labor, others instead argue that states with weak unions to begin with—perhaps due to anti-union attitudes among the populace—were most likely to adopt such legislation in the first place (e.g., Lumsden and Petersen 1975). Both processes could explain the negative correlation between RTW adoption and union density in cross-sectional data.7

Despite these data limitations, the studies utilizing the best causal identification strategies provide strong evidence that the passage of RTW legislation produced important, lasting effects on the organizational capacity of labor unions. Such evidence takes primarily four forms. First, a number of studies have used sophisticated empirical approaches to estimate the effect of RTW laws on union membership while accounting for political determinants of adoption (e.g., Davis and Huston 1995; Ichniowski and Zax 1991; Zax and Ichniowski 1990). Even with such controls, the coefficient on RTW legislation remains negative and significant. In the most recent effort, Eren and Ozbeklik (2016) use the synthetic control method to analyze the impact of recently enacted RTW legislation in Oklahoma and find that it reduced private sector unionization by more than 20%. Second, panel data on union elections show that the passage of RTW laws produced a sizable reduction in union organizing activity in the states involved, an effect that persisted for an entire decade after adoption (Ellwood and Fine 1987). Third, event studies of stock valuations find that the passage of RTW legislation increased the market value of firms located in affected states, suggesting that investors expected these laws to significantly reduce labor costs (Abraham and Voos 2000). Fourth, a recent analysis leveraging exogenous change in federal law provided by the Taft-Hartley Act (Rinz 2015) shows that RTW laws reduced wage growth in highly unionized industries by roughly 10%. Finally, perhaps the best piece of evidence is the fact that business owners and corporate interests spent a great deal of time, resources, and energy in lobbying for the passage of RTW legislation (Dixon 2005).

Taken together, the existing research provides overwhelming evidence that RTW laws weakened labor unions by reducing their membership rolls and organizing capacity, with consequences that reverberated through the labor market. These effects may have thus directly shaped inequality through their impact on worker pay. However, union strength may also affect the income distribution indirectly. Many observers, for example, point to shrinking union rolls as an explanation for the puzzling growth of inequality at the very top of the income distribution, with an increasing share of income accruing to just the top 1% of households. This phenomenon cannot be adequately explained by structural changes in the economy, such as an increasing premium to higher education or technological change—forces that have affected a much higher proportion of the population (Hacker and Pierson 2010).

Economists and sociologists (see, for example, Krugman 2007, 136–49; Western and Rosenfeld 2011) have both suggested that the growth of inequality at the very top of the income distribution may be related to declining labor strength because unions are thought to be key market actors that actively enforce the norms of equality and fairness in compensation. Historically, such norms restrained the compensation of
corporate executives who disproportionately make up the ranks of the top income holders, so the weakening of the labor movement may have contributed to the emergence of America’s “winner-take-all” economy by speeding the erosion of these norms. Fortunately, my empirical strategy can capture all such potential effects.

**Empirical Strategy**

Despite the extensive literature on the relationship between RTW legislation and union activity, there exist no rigorous analyses of how such laws have influenced income inequality. Indeed, to my knowledge, there are only a handful of studies on this question (Hanley 2010; Manzo and Bruno 2014; Nieswiadomy, Slottje, and Hayes 1991). All of these rely only on cross-sectional variation in labor laws, making them particularly vulnerable to problems of omitted variable bias and simultaneity. In short, these studies provide only evidence of correlations and cannot credibly speak to causal effects. Two of these studies also reach mixed conclusions: Nieswiadomy, Slottje, and Hayes (1991) find a significant positive relationship between RTW laws and inequality in 1970 but not in 1980; Hanley (2010) reports that inequality was higher in metro areas in states covered by RTW laws in 1970, but that inequality actually increased at a slower pace in these states in subsequent decades.

Until recently, the central challenge to estimating the effect of RTW laws on inequality has been measuring the outcome of interest over a sufficiently long period of time. The federal government began collecting data on the distribution of income at the state level only in the 1950s, and annual statistics are available only starting in the 1970s. As a result, it is difficult to empirically separate the effect of RTW legislation from forces shaping the initial decision to adopt such a law. If the distribution of income in a state affected its willingness to enact anti-union legislation, it is difficult to disentangle these two relationships using only data from the postadoption period.

To overcome such problems, this study makes use of new historical statistics on pretax income aggregated at the state level, systematically gathered for the first time by Frank (2014). Unlike traditional income data, which are based on survey self-reports, Frank’s measures are collected from annual Internal Revenue Service (IRS) publications, primarily the *Statistics of Income*, that report the number of tax returns in each income tax bracket at the state level. This is an important advantage, as survey-based measures regularly undersample the wealthiest respondents and are subject to severe top-coding. Unlike survey self-reports, IRS filings are subject to audit, increasing the incentive for truthful reporting, and utilize a standardized definition of income, which includes not only wages and salaries but also capital gains and self-employment earnings. While the IRS data are available back to 1916, very few households filed annual tax returns prior to the early 1940s. As a result, the analysis presented below covers the period from 1940 to 2012.

By measuring income inequality starting in 1940, before any state had adopted RTW legislation, I am able to identify the effect of these laws through a “difference-in-differences” (DiD) design (Angrist and Pischke 2009, 227–42). Using this approach, I examine how inequality changed in the affected states in the decades after the
enactment of RTW laws, relative to contemporaneous trends in states that had not (yet) adopted such legislation. More precisely, I model income inequality in state $s$ in year $t$ using the following equation:

$$\text{Inequality}_{st} = \beta \text{RTW Law}_{st} + \gamma \text{X}_{st} + \theta_s + \alpha_t + \varepsilon_{st}$$

To ensure that the results are not driven by the choice of dependent variables, I replicate the analysis using three standard measures of inequality widely used in the literature: (1) the share of income accrued to the top 1% of households; (2) the share held by the top 10%; and (3) the Gini coefficient. The first two measures focus specifically on inequality at the top of the income distribution, which is explained least well by apolitical, structural forces. By contrast, the Gini coefficient identifies the effects of these laws across the entire income distribution. In the online appendix, I also replicate the main results using three alternative measures of inequality that speak to potential effects at different points of the income distribution. The coefficient of interest, $\beta$, captures the effect of RTW legislation, a dummy variable coded as 1 for years when such a law was in force in state $s$. To identify the year of adoption, I relied primarily on the National Council of State Legislatures RTW database and Baird (1998). In a few cases where these sources provided different dates, I consulted a number of other state-specific sources. For states that have adopted both statutory and constitutional RTW provisions, I use the earliest date of enactment.

In addition, all of the models include year ($\theta_s$) and state ($\alpha_t$) fixed effects. The year effects directly capture the impact of economic recessions and other macroeconomic trends or events that are common to all states. The state fixed-effects are necessary to account for any preexisting time-invariant differences between states (such as union strength) that may be correlated both with the adoption of RTW laws and with the distribution of income. All tables report heteroskedasticity robust standard errors that are clustered at the state level.

My preferred specifications include only the year and state fixed effects, although to test the robustness of the results, some models also include a vector of time-varying covariates, $X$. Some caution is warranted in interpreting these results, however, because many of the most relevant covariates are arguably “posttreatment”—in the sense that they are themselves potentially affected by RTW laws (see Angrist and Pischke 2009, 47–51; Rosenbaum 1984)—and are thus invalid controls. Fortunately, the inclusion of these covariates does not affect the key results in a substantive way.

One limitation of the DiD approach is that it requires assuming that the effect of RTW laws is constant over time, producing an identical change in inequality both immediately after adoption and over the long term. This assumption can be relaxed using the following event study model:

$$\text{Inequality}_{st} = \sum_{j=1}^{11} \pi_j 1(\tau_{st} = j) + \theta_s + \alpha_t + \varepsilon_{st}$$

where $\tau_{st}$ denotes the event year, with $\tau = 1$ capturing the effect of these laws in the year immediately following adoption; $\tau = 2$ two years after; and so on. To capture
long-term effects that occur more than one decade after adoption, \( \tau \) is set to 11 for all event-years greater than 10. The coefficients are measured relative to the years preceding adoption, and I interact the state fixed effects (\( \theta_s \)) with a time counter (\( t \)) to account for any preexisting state-specific trends. As is the case with the DiD approach, the identification in this model comes from variation in the timing of adoption between RTW states and the presence of states that do not adopt such a law during the sample period. Instead of a single coefficient, the event study approach allows me to estimate separate effects for each postadoption year, \( \tau \).

**Limitations**

Both panel approaches provide high internal validity, offering credible identification of the causal effect. As is often the case, however, these gains in internal validity must be weighed against more limited generalizability of the results. In particular, my approach estimates only the effect of RTW legislation on market inequality, without taking into account tax payments or government transfers. Nevertheless, the analysis provides important insights about how political processes shape inequality, as Enns et al. (2014, 292) ably explain,

But why analyze pretax, pretransfer income? One might suspect that limiting analysis to pretax/transfer inequality excludes a role for politics and policy. To the contrary, government has many ways to potentially condition the market. . . . Nearly every action that the government takes can change the market incentives of organizations and individuals. When economic actors respond to government-induced changes in incentives, the effects of these government actions will most often be observed in the market.

While my empirical strategy captures all labor influence channeled through “market conditioning” policies (Kelly 2005; 2009), the estimates do not include the potentially indirect effects that flow through state tax policies (e.g., earned-income tax credits), state welfare laws (e.g., generosity of Temporary Assistance for Needy Families [TANF] payments), or other redistributive programs (e.g., Haynes and Vidal 2015). Thus, I cannot examine other distributional consequences of labor union strength. It is possible that strong unions might influence state tax policies or the generosity of redistributive welfare programs by, for example, affecting the mobilization of low-income voters (Leighley and Nagler 2007) or the balance of partisan power in state legislatures. My estimates do not account for these effects, unless they directly shape the distribution of pretax income. This important limitation should be kept in mind when interpreting the results, a point I return to in the section “Discussion and Conclusion.”

A second concern is that the effect of union regulation can spill across state lines, potentially biasing toward null results. For example, the adoption of an RTW law by one state may weaken the bargaining position of organized labor in other nearby jurisdictions that have not enacted such legislation if employers use the threat of relocating to the RTW state to extract concessions from their employees. To address this worry, I
also examine results focusing only on the years from 1940 to 1970, a period during which high transportation and communication costs would have greatly reduced the credibility of relocation threats and thus extent of policy spillovers (see, for example, Newman 1984). Doing so does not affect the substantive conclusions.

**Results**

Before moving on to the regression models, Figure 2 plots three measures of inequality separately for states that have never enacted RTW legislation and those that adopted such a policy at any time during the study period. The figure contains three panels, corresponding to the three alternative measures of inequality noted above. Panel 2a tracks the share of income claimed by the wealthiest 1% of U.S. households, while Panel 2b reports the income share of the top 10%. Finally, Panel 2c tracks aggregate inequality, measured using the Gini coefficient.

Examining the figures visually, particularly in the years prior to the passage of the Taft-Hartley Act, is useful for evaluating the plausibility of the parallel trends assumption necessary to identify the effect of RTW. If inequality in RTW and non-RTW states has followed divergent paths over time, the DiD model cannot recover meaningful causal effects. Fortunately, the data suggest this is not the case; during the length of the full period—including the years prior to the adoption of state RTW laws—the inequality trends in both sets of states have closely tracked each other. Across all states, the three measures of inequality reached their nadir in the years immediately following World War II and began increasing shortly thereafter, particularly in the 1970s. This upward trajectory accelerated significantly in the 1990s. During this period, inequality increased dramatically across all of the three measures, with the share of income going to the top 1% rising from approximately 8% in the mid-1970s to the nearly 20% by 2012.

**DiD**

Table 1 presents the DiD regression results, with the columns corresponding to each of the three dependent variables. Across all three measures, the coefficient on RTW laws is estimated to be quite close to 0 and nowhere near significant at conventional levels.

Table 2 includes a number of economic covariates collected from decennial censuses. Following Smith and Fridkin (2008), the census measures are linearly interpolated between census enumerations. In addition, the model controls for mass political preferences, using each state’s Democratic share of the two-party presidential vote. This measure is standardized across elections by subtracting the Democratic share of the national popular vote. As I note earlier, these variables may be directly affected by the activities of organized labor in each state, potentially introducing posttreatment bias. However, their inclusion does not substantively change the coefficient on RTW laws. Although the coefficient for the first column, corresponding to the income share of the top 1%, is marginally significant, it is modest in substantive terms.
Figure 2. State income inequality, 1940–2012.

*Note.* RTW = right-to-work.
### Table 1. Full Panel: Impact of Right-to-Work Laws on State Inequality.

<table>
<thead>
<tr>
<th>Variables</th>
<th>(1) Top 1% share</th>
<th>(2) Top 10% share</th>
<th>(3) Gini coefficient</th>
</tr>
</thead>
<tbody>
<tr>
<td>RTW Law</td>
<td>0.744</td>
<td>0.0171</td>
<td>0.00391</td>
</tr>
<tr>
<td></td>
<td>(0.500)</td>
<td>(0.543)</td>
<td>(0.00508)</td>
</tr>
<tr>
<td>Constant</td>
<td>11.34***</td>
<td>33.78****</td>
<td>0.418***</td>
</tr>
<tr>
<td></td>
<td>(0.212)</td>
<td>(0.331)</td>
<td>(0.00377)</td>
</tr>
<tr>
<td>Observations</td>
<td>3,685</td>
<td>3,685</td>
<td>3,685</td>
</tr>
<tr>
<td>R²</td>
<td>.808</td>
<td>.863</td>
<td>.922</td>
</tr>
<tr>
<td>Number of States</td>
<td>51</td>
<td>51</td>
<td>51</td>
</tr>
<tr>
<td>State FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Year FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
</tbody>
</table>

Note. Robust standard errors in parentheses. RTW = right-to-work; FE = fixed effects. *p < .1. **p < .05. ***p < .01.

### Table 2. Full Panel: Impact of Right-to-Work Laws on State Inequality, with Controls.

<table>
<thead>
<tr>
<th>Variables</th>
<th>(1) Top 1% share</th>
<th>(2) Top 10% share</th>
<th>(3) Gini coefficient</th>
</tr>
</thead>
<tbody>
<tr>
<td>RTW Law</td>
<td>0.612*</td>
<td>0.318</td>
<td>0.00266</td>
</tr>
<tr>
<td></td>
<td>(0.338)</td>
<td>(0.405)</td>
<td>(0.00450)</td>
</tr>
<tr>
<td>% Urban</td>
<td>−5.809</td>
<td>−1.149</td>
<td>−0.00451</td>
</tr>
<tr>
<td></td>
<td>(5.347)</td>
<td>(6.220)</td>
<td>(0.0561)</td>
</tr>
<tr>
<td>Dem. Presidential Vote Share</td>
<td>−0.0933</td>
<td>1.053</td>
<td>−0.00802</td>
</tr>
<tr>
<td></td>
<td>(1.763)</td>
<td>(2.132)</td>
<td>(0.0179)</td>
</tr>
<tr>
<td>Population (logged)</td>
<td>−0.318</td>
<td>0.168</td>
<td>−0.00287</td>
</tr>
<tr>
<td></td>
<td>(0.373)</td>
<td>(0.413)</td>
<td>(0.00360)</td>
</tr>
<tr>
<td>% Employed in Manufacturing</td>
<td>0.102*</td>
<td>0.0260</td>
<td>0.000546</td>
</tr>
<tr>
<td></td>
<td>(0.0516)</td>
<td>(0.0583)</td>
<td>(0.000526)</td>
</tr>
<tr>
<td>% Homeownership</td>
<td>−6.326**</td>
<td>−6.944</td>
<td>−0.101***</td>
</tr>
<tr>
<td></td>
<td>(3.053)</td>
<td>(4.782)</td>
<td>(0.0425)</td>
</tr>
<tr>
<td>Constant</td>
<td>20.34****</td>
<td>34.73***</td>
<td>0.527***</td>
</tr>
<tr>
<td></td>
<td>(6.452)</td>
<td>(7.130)</td>
<td>(0.0635)</td>
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<tr>
<td>Observations</td>
<td>3,640</td>
<td>3,640</td>
<td>3,640</td>
</tr>
<tr>
<td>Number of States</td>
<td>51</td>
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</tr>
<tr>
<td>State FE</td>
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<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Year FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
</tbody>
</table>

Note. Robust standard errors in parentheses. RTW = right-to-work; FE = fixed effects. *p < .1. **p < .05. ***p < .01.
Figure 3. Event study estimates of the impact of RTW law adoption.

Note. RTW = right-to-work.
As robustness checks, the online appendix presents a variety of additional specifications. First, I limited the analysis to year 1970 and earlier, to address potential spillovers across state lines by focusing on a period before such spillovers were likely to occur. Second, I reestimated the main specification using three alternative measures of income inequality that can capture effects at different points of the income distribution. Third, I also included time trends, specified in various ways. Across all of these tests, the coefficient on RTW laws remains very close to 0 and insignificant.

**Event Study**

Figure 3 visually presents the results from the event study analysis, with separate panels corresponding to each dependent variable. The figure plots the estimated effect of RTW laws in each postadoption year along with the corresponding 95% confidence intervals in light gray. The results echo the findings from the pooled DiD models. Across each dependent variable, I find no evidence of an increase in inequality in the decade immediately after the laws were adopted nor in the years thereafter. Indeed, almost every coefficient is negative, corresponding to lower inequality; these negative effects are statistically significant for postadoption years 1 through 3 using the Gini coefficient measure.

**Heterogeneity over Time and Space**

The advantage of the panel results summarized above is that they leverage policy changes in all affected states, maximizing statistical power and, thus, precision of the estimates. One disadvantage, however, is that pooling in this way assumes that the effect of RTW laws is homogeneous among states and over time. As most of the legislative changes observed in the data occurred in the 1940s and 1950s, around the time of the Taft-Hartley Act, this may be problematic for generalizing from the results to make inferences about the consequences of anti-union policies in the modern era. To be sure, the results do capture the effect of these laws (i.e., weak unions) that persist over time, so they speak directly to the impact of union strength on inequality in the period since 1970. But it is possible that the suspected causes and consequences of economic inequality may have differed greatly in 1940s and 1990s America, and so averaging the effects of RTW laws across the full panel potentially ignores their growing importance in the latter decades.

I address this possibility in Table 3, which estimates a separate interaction between the presence of RTW laws and a dummy variable for the post-1970s era. This specification allows the effect of anti-union legislation to vary over time. In the online appendix, I also repeat this analysis by breaking the data down into alternative eras (i.e., post-1980 and post-1990). Across the board, there is no evidence that RTW laws influenced inequality in a statistically or substantively significant way either before or after 1970, or in any of the later decades.

Another concern is that early adopters of RTW legislation were simply different on important social and economic dimensions that also potentially depressed the laws’
One way to look for such heterogeneity is to estimate the effects of RTW legislation separately for the three states where these laws were adopted relatively recently, and sufficient years of postadoption inequality measures are available: Louisiana (1975), Idaho (1985), and Oklahoma (2001). As the number of late adopters is so small, there is insufficient statistical power to carry out a conventional panel analysis. Instead, I use the synthetic control method developed by Abadie, Diamond, and Hainmueller (2010) and Abadie, Diamond, and Hainmueller (2015). Using this method, I construct a counterfactual for each of the three RTW states by building a “synthetic control” based on a weighted average of states without such laws. The precise weights assigned to each of the control states in constructing these counterfactuals are set to best approximate the pretreatment trends in each of the RTW states. Additional details about the algorithm used to calculate the weights is provided in Abadie, Diamond, and Hainmueller (2015).

To build each counterfactual, I begin with a “donor pool” of all states without RTW legislation. I then calculate a vector of optimal weights, \( W \), assigned to the states in the donor pool to minimize the pretreatment differences between each treated state and its synthetic control. These weights are calculated using the dependent variables of interest—observed inequality in each state—and the other control variables included in Table 2 in all years prior to adoption. Using this method, I construct a counterfactual for each of the three RTW states by building a “synthetic control” based on a weighted average of states without such laws. The precise weights assigned to each of the control states in constructing these counterfactuals are set to best approximate the pretreatment trends in each of the RTW states. Additional details about the algorithm used to calculate the weights is provided in Abadie, Diamond, and Hainmueller (2015).

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<table>
<thead>
<tr>
<th>Variables</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Top 1% share</td>
<td>Top 10% share</td>
<td>Gini coefficient</td>
</tr>
<tr>
<td>RTW Law</td>
<td>0.263</td>
<td>0.212</td>
<td>0.00428</td>
</tr>
<tr>
<td></td>
<td>(0.359)</td>
<td>(0.404)</td>
<td>(0.00361)</td>
</tr>
<tr>
<td>RTW Law × Post-1970</td>
<td>0.744</td>
<td>−0.301</td>
<td>−0.000582</td>
</tr>
<tr>
<td></td>
<td>(0.484)</td>
<td>(0.592)</td>
<td>(0.00510)</td>
</tr>
<tr>
<td>Constant</td>
<td>11.34***</td>
<td>33.77***</td>
<td>0.418***</td>
</tr>
<tr>
<td></td>
<td>(0.211)</td>
<td>(0.331)</td>
<td>(0.00378)</td>
</tr>
<tr>
<td>Observations</td>
<td>3.685</td>
<td>3.685</td>
<td>3.685</td>
</tr>
<tr>
<td>( R^2 )</td>
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<td>.863</td>
<td>.922</td>
</tr>
<tr>
<td>Number of States</td>
<td>51</td>
<td>51</td>
<td>51</td>
</tr>
<tr>
<td>State FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Year FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
</tbody>
</table>

Note. Robust standard errors in parentheses. RTW = right-to-work; FE = fixed effects.
* \( p < .1 \). ** \( p < .05 \). *** \( p < .01 \).
Figure 4. Synthetic control analysis: Difference between each state and its synthetic control one decade postadoption.
Note. RTW = right-to-work.
directly comparable with the coefficients from the pooled event study model, I include these coefficients in light gray to facilitate visual comparison between the estimates produced by these two alternative approaches.21

Overall, there is some evidence that the RTW legislation increased inequality in the case of Oklahoma, where the gap between the state and its counterfactual appears to grow in the years after adoption using the income share of the top 1% and 10% of taxpayers. There are no consistent differences in the posttreatment period for either Idaho or Louisiana, however, or for Oklahoma using the Gini coefficients. Although the synthetic control effects are less precisely estimated, the substantive magnitudes of the effects are similar to those obtained in the pooled event study model. Together, the evidence suggests that RTW legislation has not been a central driver of the growing inequality observed over the past half a century.

Discussion and Conclusion

Although a number of academic and popular commentators blame anti-union public policies for causing, or at least exacerbating, economic inequality, such arguments have rarely confronted direct empirical testing. This study carries out just such a test, examining the relationship between state adoption of RTW legislation and the distribution of market income. It finds no evidence that such legislation has been a major cause of the growing inequality recorded in the second half of the twentieth century.

What might account for these largely null results? One possible explanation—that RTW legislation did not actually affect union activity or the bargaining power of organized labor—seems clearly inconsistent with the available evidence. Overall, there is a variety of evidence showing that the passage of such laws weakened labor unions, reducing the number of successful union elections held, the average size of newly organized workforces, and—most tellingly—the size of the union wage premium.

Another explanation is that weak enforcement of existing labor laws at the federal level undermined labor influence across the board (e.g., Hacker and Pierson 2010), including in states with stronger union protections. The absence of national enforcement would reduce the impact of between-state statutory differences. However, although this explanation may be consistent with experience in more recent decades, it cannot explain the trends in the pre-1970 period, when presidential appointees on the National Labor Relations Board aggressively enforced federal labor laws, allowing significantly more scope for state policy to influence economic outcomes. Nevertheless, I find no significant differences in inequality between states with and without RTW laws on the books during these earlier years.

Instead, the most likely explanation appears to be that the economic and social forces most responsible for rising market inequality in the United States in recent decades, including globalization, industry deregulation, and evolving norms of executive compensation, have been sufficiently strong to overcome even the resistance of strong unions, affecting organized and nonunionized firms alike.

However, special care should be taken to note what the analysis does not show. One should not conclude, on the basis of the evidence presented here, that unions do not
matter for inequality or that active union representation does not provide other important benefits for workers, including better working conditions, more generous non-wage benefits, and greater political resources for the election of labor-friendly political candidates. Although it suggests that unions’ strength does not appear to affect market inequality, organized labor may still play an important role in building political support for government redistribution to offset the effects of such inequality. Similarly, care needs to be taken when making out-of-sample projections from the analysis presented here: although RTW legislation does not appear to have increased inequality in the states that have adopted such laws during the twentieth century, the analysis does not speak to the possibility that new, more stringent legislation adopted in other states since then might produce larger distributional consequences. Indeed, many of the early adopters—including the three states examined in the synthetic control analysis—had much lower rates of unionization than the average state at the time these laws were passed, so it is possible that the RTW laws adopted in recent years in more heavily unionized states (e.g., Michigan, Wisconsin) might produce larger effects. Unfortunately, too little time has passed to credibly evaluate the impact of these more recent adoptions.

Finally, it should be emphasized that the dependent variables examined in this study—with the exception of the Gini coefficient—focus on income at the upper tail of the distribution, which has driven the increase in inequality in the United States. It is possible that RTW laws may have produced substantively important effects on other economic outcomes that are less connected to inequality—such as median income, unemployment rates, or service sector wages, which I have not examined here. Such possibilities should be explored in future research.

Nevertheless, the evidence presented in this study does raise doubts about the likely efficacy of proposed reforms to strengthen unions by, for example, allowing card-check unionization in lieu of secret elections, in slowing or reversing the recent trends toward greater market inequality. Similarly, the results suggest that further erosion of union power, through efforts to restrict the collection of union agency fees for public-sector employees (e.g., Friedrichs v. California Teachers Association, No. 14-915), will likely produce negligible impacts on market inequality. Such changes might, however, contribute to the rollback of other redistributive policies by reducing the political and electoral success of Democratic politicians supported by organized labor, increasing inequality through other channels.

The null results also speak to the importance of complementing the rich and growing theoretical literature on income inequality with rigorous empirical evidence. Identifying the precise mechanisms through which political decisions shape economic opportunity is important, because doing so sheds light on the most appropriate and effective political strategies and institutional reforms that can help level the economic playing field. For example, while the adoption of regressive tax policy can be reversed only through effective electoral counter-mobilization, policy drift may potentially be addressed through politically less onerous steps, such as the introduction of inflation-indexing for existing redistributive programs. Although a number of published studies have documented correlations between income inequality and political phenomenon,
including polarization (McCarty, Poole, and Rosenthal 2006) and aggregate policy output (Kelly 2005), rarely do such studies provide convincing evidence linking observed economic outcomes to specific political decisions. Yet such evidence is sorely needed, both to advance our understanding of how political processes affect the economic well-being of individuals and to draw the right conclusions about how political action can and should be used to shape and discipline market forces.

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Notes
1. To be sure, not all studies find a statistical relationship between inequality and union density. See, for example, Checchi et al. (2008).
3. The National Labor Relations Act (NLRA) is also commonly known as the Wagner Act.
4. Several states adopted laws banning union shops several years earlier, but these were unenforceable prior to the passage of the Taft-Hartley Act.
5. The online appendix also lists the first year the law was in force in each state.
6. It is no coincidence that the third section of Mancur Olson’s influential book examines the collective action problem from the point of view of unions and dedicates a number of pages to state right-to-work (RTW) laws.
7. A number of studies have attempted to overcome this problem using simultaneous equation modeling. These efforts have not borne much fruit, however. As Farber (1984, 323) concluded, “Unfortunately, a convincing model of the simultaneous determination of RTW legislation and the historical evolution of the extent of unionization, required to find appropriate identifying restrictions, does not exist.”
8. Unlike similar data at the national level collected by Piketty and Saez (2003), however, it is not possible to disaggregate the income by source.
9. It should be emphasized that this approach identifies the effect of RTW laws on inequality over the full sample period, 1940 to 2012, and not simply in the years immediately after their adoption.
10. For states that adopted RTW prior to the Taft-Hartley Act, I use 1948 as the first year when such laws are in effect, following Rinz (2015). However, the results are substantively identical if one instead codes the laws as taking effect when they are first passed.
11. The legislation is assumed to take effect the year following adoption.
12. In almost every instance, the discrepancy occurred because the National Council of State Legislatures (NCSL) database lists the date of the most recent iteration of the RTW law, while Baird reports the first year such a law was adopted.
13. To remove $\alpha_t$ from the estimation, all of the variables are demeaned and ordinary least
squares (OLS) regressions are estimated based on the transformed data.  

Ideally, one would also test for these effects empirically. Unfortunately, sufficiently detailed historical data on state fiscal and tax policy do not exist.  

More than half of this small group is made up of corporate CEOs and other top-level executives (Hacker and Pierson 2010).  

I include all available measures that are available for the full length of the panel.  

As with the census measures, the standardized presidential vote is linearly interpolated between election years.  

Note that the dummy itself is perfectly collinear with the year fixed effects, so is not estimated separately.  

A number of other states have adopted RTW laws since 2010, but that leaves too few post-adoption years to credibly estimate their effect.  

The weights used for each state-dependent variable combination and the covariate balance comparisons between each treatment state and its synthetic are provided in the online appendix.  

Note that, unlike the event study coefficients, the synthetic control estimates do not begin at 0 in the year of RTW adoption. This reflects the fact that the synthetic counterfactual, while closely tracking the trends in each treatment state, were not exactly identical to them in the preadoption period. It is important to account for these pretreatment differences when calculating the net effect of treatment.

References


**Author Biography**

**Vladimir Kogan** is an assistant professor in the Department of Political Science at the Ohio State University. His research examines state and local politics in the United States, with a focus on education policy and political reform. His work has been published or is forthcoming in *American Journal of Political Science, Journal of Politics*, and *Journal of Policy Analysis and Management*. 