Do Right to Work Laws Worsen Income Inequality? Evidence from the Last Five Decades

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Abstract

There is an ongoing debate about whether changes in labor regulations such as the Right to Work (RTW) laws are contributing to the rising trend of income inequality in the U.S. We adopt the Synthetic Control Method (SCM) for comparative case study to examine the impact of a state's adoption of an RTW law on its income inequality. We use a wide range of inequality measures for Idaho, Louisiana, Oklahoma and Texas, states that enacted their RTW laws between the 1960s and the 2000s. We find that the RTW laws did not impact income inequality in these states. This result is underpinned by additional findings of lack of impacts of these laws on unionization, wages and salaries, and investment.

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1. Introduction

Income inequality is widening in the United States. The share of pre-tax incomes earned by the top 1% rose from 9% in 1976 to 20% in 2011. Average real incomes for the bottom 90% dropped from $32,261 to $30,439 while, for the top 10%, it increased by more than 80% from $140,827 to $254,449 (Alvaredo et al. 2013). Data from the Congressional Budget Office (CBO) that accounts for taxes and transfers largely mirrors these trends (CBO 2014).

Rising inequality has engendered a debate about its determinants with studies identifying trade, immigration, skill biased technological change, female labor force participation and labor market regulations as factors (Gordon and Dew-Becker 2008). One strand of literature debates whether labor regulations such as Right to Work (RTW) laws are possible contributors to increasing income inequality in the U.S. (Manzo and Bruno 2015).[1] RTW statutes remove union membership as a pre-requisite for employment by making it illegal for labor unions and employers to enter into contracts that require employees to be fee-paying members of a union.[2]

[1] In the popular media and the public sphere the belief that the RTW laws contributed to increased inequality in the U.S. is quite widespread. See, for example, *Los Angeles Times* (http://www.latimes.com/business/hiltzik/la-fi-mh-imf-agrees-loss-of-union-power-20150325-column.html), *The Washington Post* (http://www.washingtonpost.com/news/wonkblog/wp/2015/02/10/should-you-join-a-union-the-research-says-yes/), Nicholas Kristof in *The New York Times* (http://www.nytimes.com/2015/02/19/opinion/nicholas-kristof-the-cost-of-a-decline-in-unions.html), or *Mother Jones* (http://www.motherjones.com/politics/2011/02/income-inequality-labor-union-decline). Former labor secretary Robert Reich has discussed this in many different media such as print, cable news and movies (http://robertreich.org/post/85532751265). An *International Monetary Fund* (IMF) report argues that declining unionization causing increased inequality is a world-wide phenomenon (http://www.imf.org/external/pubs/ft/fandd/2015/03/jaumotte.htm).

[2] The National Labor Relations Act (NLRA) establishes most private-sector workers' rights to unionize and collectively bargain over wages, benefits, and working conditions. Enacted in 1935, the NLRA also permits collective bargaining contracts between employers and labor organizations that require every individual covered by the collective bargaining contract to pay dues to the negotiating labor organization. These contract provisions are known as union security agreements. Since the NLRA was amended by the Taft-Hartley Act in 1947, individual states have been permitted to supersede the union security provisions of the NLRA by enacting laws that prohibit union security agreements. The Taft-Harley Act (1947) allowed states to supersede union security provisions of the National Labor Relations Act by enacting RTW laws (Collins 2014).

RTW, in principle, can suppress wages of low income workers by constraining their bargaining powers and thereby can accentuate inequality. There have been studies presenting evidence of economically significant impacts of these laws on wages (Nieswiadomy et al. 1991, Western and Rosenfeld 2011). At the same time, there are opposing studies arguing that RTW laws are in fact investment and employment friendly (Rinz 2012).[3] Studies of the net impact of the RTW laws on inequality, however, remain conspicuously absent. This paper is an attempt to address the following question: does an RTW law result in greater income inequality in the state?

Our data covers nearly a 60-year period (1964-2013). This is important since, by most measures, inequality in the U.S. started to increase in the 1980s (Meyer and Sullivan 2013, Frank 2014). Of the RTW states, 17 passed the law on or before 1963 offering little pre-intervention information while two (Indiana and Michigan) passed it in 2012 offering little post-intervention information. The four states that we examine – Idaho, Louisiana, Oklahoma and Texas – are the only states that enacted RTW between the 1970s and the 2000s, offering reasonable number of periods of both pre- and post-intervention.

We conduct a comparative case study of each four exposed states using the Synthetic Control Method (SCM) that is increasingly being used to evaluate the impacts of state-level policies (Abadie et al. 2010, Bohm et al. 2014, Maguire and Munasib, forthcoming). We find no significant impact of RTW on a comprehensive set of measures of inequality. In the process of assessing RTW's impact on inequality, we also look at some possible pathways through which these laws are commonly perceived to impact inequality, namely, unionization, investment, and

[3] http://www3.nd.edu/~krinz/Rinz_RTW.pdf.

wages and salaries. Our finding of a lack of impact of the RTW laws on inequality is further underpinned by additional findings of lack of impacts of the law on these variables.

To the best of our knowledge, only Nieswiadomy et al. (1991) assess the effects of RTW on income inequality using a Gini coefficient. Combining data from 50 states, the regression analysis in Nieswiodomy et al. (1991) implicitly assumes that the RTW laws across all states are identical, which we argue below is a strong assumption.

Moreover, the reality of rising income inequality in the US is that much of the increase can be explained by the upper end of the distribution (Lowell and Waller 2014). This implies that the Gini coefficient alone will not be sufficient to assess the dynamics of income inequality in the US or its states since it puts equal weight on all components of the income distribution. In contrast to Nieswiodomy et al. (1991), we look at a wide range of measures of inequality that put differential weights across groups. For example, while the Atkinson index puts greater weight on the lower end of the income distribution, measures such as 90-10 or 50-10 ratios look at different components of the income distribution (Atkinson and Piketty 2007).[4]

There are certain challenges in estimating the impact of RTW laws on inequality. The data on income distribution at the state level has been collected by the federal government only in the 1950s. The different income ratios that we use, such as 50-10, 90-10 ratios, are available only since 1964. This dictates our choice of time period, which is 1964-2013. In terms of the timing of adoption of the laws, while almost half the states in the US currently have RTW laws,

[4] The evidence in Nieswiodomy et al. (1991) of a positive and significant effect of RTW law on income inequality has a number of other limitations as well. The exogenous variable used in the 2SLS estimation is the wage rate; it is not clear if it can satisfy the criterion. The estimated effects of RTW laws are highly sensitive to model specification (Ellwood and Fine, 1987). Earlier, Farber (1984) argues that a convincing model of the simultaneous determination of RTW legislation and the evolution of unionization does not exist. Additionally, while the census data based estimates show a positive and significant effect (at 10 percent level) for 1970, there is no statistically significant effect using 1980 census data. Also, relying on cross sectional analysis, the results are extremely vulnerable to omitted variable problems.

within the 50 year period between the 1960s and the 2000s, only 4 states (the ones we study) 'switched' from non-RTW status to one with an RTW law. As a result, even though one can have a long panel of over 50 years for all US states, the fact that only 4 switched to RTW underscores the choice of SCM as the preferred method for assessing impacts of the RTW laws. If instead, difference-in-difference (DID) regression analysis were to be chosen, it would almost tantamount to a cross-section analysis since very few units would have treatment variation over time.

Furthermore, with so few treatment units, accurate inference is difficult, perhaps impossible, in a clustering framework (Donald and Lang, 2007, Buchmueller et al., 2011). SCM, on the other hand, is devised to address precisely these kinds of situations, and the method naturally renders itself to permutations or randomization tests for inference (Bertrand et al., 2004, Abadie et al., 2010, Buchmueller et al., 2011, Bohn et al., 2014).

One of the important contributions of this paper is that it estimates RTW's impacts in each state individually. Keele et al. (2013) argue that treatment heterogeneity in state policies needs to be taken seriously. The assumption of a uniform effect across states that essentially differ in history, population, and a host of observed and unobservable characteristics can be restrictive. For example, as RTW laws were being enacted at different times, the affected cohorts varied across states: the law was adopted in Idaho almost a decade before the passage of NAFTA, in Texas about the same time as NAFTA, and in Oklahoma a little less than a decade after NAFTA.

In terms of the design of the law itself, treating RTW in different states as the exact same intervention is also likely to be problematic. The constitutional amendment used to pass RTW in Oklahoma is broader in terms of coverage and includes all workers in the state (Collins 2014) whereas the RTW law in Michigan includes state and local workers except police, firefighters and certain other public safety occupations. Reflecting another source of heterogeneity across states, Canak and Miller (1990) show that the composition of business support to RTW varies across states and over time. The variation in business support is important from the perspective of how business reacts to RTW in terms of bringing in investment and generating employment. Attempting to estimate an average treatment effect can mask possible treatment heterogeneities.

Finally, there is the issue of finding comparison or control states that can provide a reliable counterfactual for the treatment states. This is especially important when the trends in unionization, wages and the passage of the RTW laws do not necessarily follow simple patterns. There does exist a strong negative correlation between the presence of RTW laws and union density. As per a report by the Congressional Research Service, union membership rates are nearly three times lower in RTW states than in union security agreement states and wages were higher in union security states than RTW states (Collins 2014). These can be potentially inequality raising factors. At the same time employment growth was higher in RTW states relative to non-RTW states over the period 2001-2011 which in principle is an inequality mitigating factor.

Some studies take the association between unionization and lower wages as evidence that RTW laws have the effect of constraining organized labor. However, states with weak unions to begin with can quite possibly reflect the citizen's preference that makes them more likely to adopt RTW legislation in the first place (Lumsden and Petersen 1975).

Naturally, not all non-RTW states have the same characteristics; there is substantial heterogeneity among non-RTW states too. Differences in characteristics such as types and size of labor force, industry makeup, taxation policies, and numerous other state-specific factors imply that neither a single non-RTW state nor all non-RTW states make for a good counterfactual for an RTW state. SCM provides a systematic way to choose comparison units. In SCM, the counterfactual is the weighted average of the non-RTW states where the pre-intervention matching across a wide variety of characteristics over a long period of time generates the weights.

In what follows, section 2 presents a brief review of the Right-to-work laws in the United States. Section 3 and section 4 describe the estimation methodology and the data, respectively. Section 5 reports and discusses the results and section 6 concludes with the implications of the findings.

2. Right-to-work Laws and Pathways Affecting Inequality: A review

The Taft-Hartley Act in 1947 allowed states to supersede the union security provisions of the National Labor Relations Act by enacting laws that prohibit union security agreements. These laws are called Right-To-Work (RTW) laws.[5] Currently 24 states have RTW laws. Of these, 11 states passed the law before 1950 and another 6 passed them prior to 1960. The most recent states to adopt these laws are Oklahoma (2001), Indiana (2012) and Michigan (2012).

There is some evidence to suggest that RTW laws could have a negative impact on union organizing efforts. While earlier studies find a negative relationship between unionization and RTW laws, they tend to differ on the pathways leading to such an association (Lumsden and Petersen, 1975; Warren and Strauss, 1979; Wessels, 1981; and Ellwood and Fine, 1987). Each of these studies, using different techniques and different data, attempts to determine whether the negative correlation is caused by RTW laws or simply reflects tastes that result both in

[5] http://www.fas.org/sgp/crs/misc/R42575.pdf.

adoption of an RTW law as well as less unionization. While Warren and Strauss (1979) and Ellwood and Fine (1987) find that RTW laws have a real effect on the extent of unionization, others find that RTW laws have no real effect and merely reflect pre-existing tastes (Farber 1984).

Ellwood and Fine (1987) look at changes in new union organizing efforts (as opposed to unionization levels) after the passage of RTW laws and find that in the five years after states passed an RTW law, union organization fell by 28 percent and union organizing success fell by as much as 46 percent and, although these effects faded in subsequent years, they might have led to a permanent decline in unionization levels.

In terms of impacts on wages, Moore (1998), while summarizing the empirical literature, concludes that "RTW laws have no impact on union wages, nonunion wages, or average wages in either the public or private sector." However, subsequent studies have challenged this conclusion. Gould and Shierholz (2011) using household survey data compare wages between RTW and union security states while controlling for personal (such as the lower share of workers with college degrees in RTW states) as well as state level characteristics (e.g. higher cost of living in union security states). The study concludes that the mean effect of working in an RTW state is a 3.2 percent reduction in wages and in employer-provided benefits as well.

Reed (2003) differs from the conclusion in Gould and Shierholz (2011). Unlike other studies Reed (2003) controls for the states' initial conditions (such as per capita income in 1945) prior to the initial wave of RTW laws. This is crucial since RTW states are often lower income states. The results show that after controlling for income levels in 1945, RTW laws resulted in wages that were actually 6.7 percent higher and this effect was stronger in states with a lower income in 1945.

Unionization is one of the channels through which RTW laws can affect wages. Freeman (1993) and Card (1992) estimate the union wage premium to be between 10 and 17 percent. In the 1970s and 1980s, Card (1992) estimates that unions raised the wages of the bottom two quintiles by 23 to 32 percent, of the middle quintile by between 13 and 19 percent, and of the fourth quintile by 5 to 7 percent. Nieswiadomy et al. (1991), find that union wages are 10 to 20 percent higher than non-union wages in similar industries and occupations.

Decomposing wage variance, Western and Rosenfeld (2011) argue that between 1973 and 2007, unions' impact on union and nonunion wages explains a fifth to a third of the growth in inequality – an effect comparable to the growing stratification of wages by education. If unionization works to raise relative incomes of low and middle income workers, it can attenuate inequality.

However, in a recent review of research on determinants of inequality, Dew-Becker and Gordon (2008) ascribe a relatively small role to the decline of unionization towards the increase in inequality starting in the 1970s particularly for females.[6] They instead find the largest contributor to be skill-biased technical change. With computers and computing technologies in the workplace, there was an increasing wage premium for those who were able to use them. This mirrors the findings in Goldin and Katz (2007) who also associate the widened income inequality starting in the 1980s with an increased demand for skilled college graduates.

As another pathway for links between RTW and inequality, Holmes (1998) examines manufacturing employment in border counties of neighboring states where one state had RTW protections and the other did not. Holmes (1998) finds that manufacturing employment as a percentage of county population increased by one-third in the counties within the RTW states

[6] They similarly ascribe a small role to trade and immigration. They also contend the argument that minimum wages offer the single biggest explanation for rising inequality.

vis-à-vis non-RTW states. Hicks (2012), using a long panel of states between 1929 and 2005, suggests that while RTW laws do not explain the industrial structure across the U.S., after adjusting for inflation 7 out of 10 states saw manufacturing incomes increase by between 15 percent and 40 percent.

Though the evidence seems mixed, in principle RTW can aggravate inequality by suppressing wages of low income workers as their bargaining powers get diminished. At the same time, others argue that RTW laws are in fact investment and employment friendly and have increased wages by 3-4 percent, particularly in non-union industries (Rinz 2012). Overall, in the literature, how RTW laws affect inequality through its different pathways remains unclear.

One further point in this regard is about the widely held view that labor market institutions such as unions affect mostly low- and middle-income wage workers but are unlikely to have a direct impact on top income earners. However, Jaumotte and Buitron (2015) argue that with regard to unionization and/or union density, there is a basis for looking at measures of inequality that concerns top income earners as well. They argue that if de-unionization weakens earnings for middle- and low-income workers, this necessarily increases the income share of corporate managers and shareholders who fall in the upper end of the income distribution.

Weaker unions could further lead to higher top income shares by denting workers' influence on corporate decisions. Where unions are strong, firms tend to engage in consultations with workers that can influence the size and structure of top executive compensation (Lemieux et al. 2009, McCall and Percheski 2010). Volscho and Kelly (2012) show a negative effect of union density on top income shares for the United States.

3. Estimation

3.1. A Case Study Approach with Synthetic Control Method (*SCM*)

As discussed above, given the context, there are a number of advantages to using SCM. First, in program evaluation, researchers often select comparisons on the basis of subjective measures of similarity between the affected and the unaffected regions or states. But, neither every non-RTW states nor a single non-RTW state would likely approximate the most relevant characteristics of an RTW state.

SCM, in contrast, provides a comparison state (or synthetic) that is a combination of the control states – a data-driven procedure that calculates 'optimal' weights to be assigned to each state in the control group based on pre-intervention characteristics – thus making explicit the relative contribution of each control unit to the counterfactual of interest (Abadie and Gardeazabal 2003; Abadie et al., 2010). SCM provides a systematic way to choose comparison units where the researcher is forced to demonstrate the affinities between the affected and unaffected units using observed characteristics (Abadie et al., 2010; Abadie et al., 2014).[7]

Secondly, when aggregate data are employed (as the case is in this paper) the uncertainty remains about the ability of the control group to reproduce the counterfactual outcome that the affected unit would have exhibited in the absence of the intervention. This type of uncertainty is not reflected by the standard errors constructed with traditional inferential techniques for comparative case studies. As Buchmueller et al. (2011) explain, in a 'clustering' framework, inference is based on asymptotic assumptions that do not apply in our case as the focus is on one state at a time.

[7] Neumark et al. (2014), in the context of the impact of minimum wage legislations, point out that in several studies that adopted regression-based models, there were underlying assumptions of similarities across states (for example, categorization by region). Unlike the *ad hoc* strategies with a presumption of affinity, SCM demonstrates affinities of the donor pool states with the exposed state.

The comparison of a single state against all other states in the control group collapses the degrees of freedom and results in much larger sample variance compared to the one typically obtained under conventional asymptotic framework. The latter can seriously overstate significance of the intervention (Donald and Lang, 2007, Buchmueller et al., 2011). We, therefore, apply the permutations or randomization test (Bertrand et al., 2004, Abadie et al., 2010, Buchmueller et al., 2011, Bohn et al., 2014) that SCM readily provides.

Additionally, recall that unlike the traditional regression-based difference-in-difference model that restricts the effects of the unobservable confounders to be time-invariant so that they can be eliminated by taking time differences, SCM allows the effects of such unobservables to vary with time Abadie et al (2010). In particular, Abadie et al. (2010) show that with a long pre-intervention matching on outcomes and characteristics a synthetic control also matches on time-varying unobservables.[8]

Finally, because the construction of a synthetic control does not require access to post-intervention outcomes, SCM allows us to decide on a study design without knowing its bearing on its findings (Abadie et al., 2010). The ability to make decisions on research design while remaining agnostic about how each particular decision affects the conclusions of the study is a safeguard against actions motivated by a 'desired' finding (Rubin 2001). Furthermore, in assessing RTW's impact on inequality, there are several possible time varying factors that could be confounding the estimates, for example, unobserved time and location specific preference for unionization.

[8] As Abadie et al. (2014) put it, "only units that are alike in both observed and unobserved determinants of the outcome variable as well as in the effect of those determinants on the outcome variable should produce similar trajectories of the outcome variable over extended periods of time."

3.2. The Synthetic Control

A typical SCM analysis is feasible when one or more states exposed to an intervention can be compared to other states that were not exposed to the same intervention. In this paper, an outcome is an inequality measure, an exposed state is an RTW state, the intervention is the passage of the RTW, and the donor pool (unexposed/control states) consists of states that did not have a similar law for the observed period.

The following exposition is based on Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2014). For states $i = 1, \ldots, J+1$ and periods $t = 1, \ldots, T$, suppose state i is exposed to the intervention (the RTW law) at $T_0 \in (1, T)$. The observed outcome for state i at time t is,

(1) $$Y_{it} = Y_{it}^{N} + \alpha_{it} S_{it},$$

where Y_{it}^{N} is the outcome for state i at time t in the absence of the intervention, the binary indicator variable S_{it} denotes the existence of the RTW law taking the value 1 if $i = 1$ and $t > T_0$, and α_{it} is the effect of the intervention for state i at time t. Thus, state i is exposed to the intervention in periods $T_0 + 1$ to T. We assume that the passage of the RTW law had no effect on the outcome in the exposed state before the implementation period.

Abadie et al. (2010) show that there exist $\mathbf{W}^{*} = (w_2^{*}, \ldots, w_{J+1}^{*})'$ such that, under standard conditions, we can use,

(2) $$\hat{\alpha}_{1t} = Y_{1t} - \sum\nolimits_{j=2}^{J+1} w_j^{*} Y_{jt}, \qquad t \in \{T_0 + 1, \ldots, T\},$$

as an estimator for α_{1t}. The term $\sum\nolimits_{j=2}^{J+1} w_j^{*} Y_{jt}$ on the right-hand-side of (3) is simply the weighted average of the observed outcome of the control (or donor pool) states for $t \in \{T_0 + 1, \ldots, T\}$ with weights $\mathbf{W}^{*}$. The procedure to obtain $\mathbf{W}^{*}$ can be found in Abadie et al. (2010).

3.3. Inference

Once an optimal weighting vector $\mathbf{W}^*$ is chosen, the "synthetic" of the exposed state is obtained by calculating the weighted average of the donor pool. The post-intervention values of the synthetic serve as our counterfactual outcome for the exposes state. We calculate the ratio of post-intervention to pre-intervention Mean Square Prediction Error (MSPE), denoted by Δ_{TR}. This ratio puts the magnitude of post intervention gap (between the actual and the synthetic outcome) in the context of the pre-intervention fit (between the actual and the synthetic outcome): the larger the ratio, the greater is the impact of the intervention.

To formally test the significance of the estimated impact, we apply the permutations test (Bertrand et al. 2002, Buchmueller et al. 2009, Abadie et al. 2010, Bohn et al. 2014). First, for each state in the donor pool, we carry out an SCM estimate as if the state had passed the RTW law the same year as the exposed state (i.e., apply a fictitious policy intervention). We can then calculate the post-pre MSPE ratio for each of these states. The distribution of these "placebo" post-pre MSPE ratios (Δ) then provides the equivalent of a sampling distribution for Δ_{TR}. The cumulative density function of the complete set of Δ estimates is given by $F(\Delta)$, which allows us to calculate the p-value of a one-tailed test of the significance of the magnitude of Δ_{TR} (Bohn et al. 2014, Munasib and Rickman 2015). Note that this answers the question, how often would we obtain an effect of the RTW law of a magnitude as large as that of the exposed state if we had chosen a state at random, which is the fundamental question of inference (Bertrand et al 2002; Buchmueller et al. 2009; Abadie et al. 2010).

Abadie et al. (2010) utilize the placebo tests for inference with a more straightforward criterion. They examine the ranking of the magnitude of the post-pre MSPE ratio of the exposed state vis-à-vis those of the placebos. If the exposed state is ranked first, then they consider it significant, the rationale being that for the treatment effect to be significant no placebo effect should be larger than the actual effect estimated for the exposed state. We adopt both these criteria and consider the impact of the RTW law to be significant if the post-pre RMSPE ratio of the exposed state is ranked first with a statistically significant p-value.

4. Data

We look at a comprehensive set of measures of income inequality in the four states as outcome variables that in principle could be affected by the passage of the RTW laws. To ensure that we cover different facets of – and different ways to look at – the aggregate inequality, we carry out our analyses using the following alternative aggregate inequality measures. From Frank (2009, 2014), we use the traditional measures, namely, Gini coefficient, Atkinson index, Relative mean deviation and Theil's entropy index, as well as top 1% income share and top 10% income share. From Census Population Survey (CPS) we obtain household income measures of inequality in the forms of 50-10 Ratio, 90-10 Ratio and 90-50 Ratio. The income shares and the household income ratios are widely used measures of inequality and have been extensively used to measure inequality in the U.S. states, for example, in Aghion et al. (2015).

In terms of predictor variables, we use an extensive set from historical data to obtain pre-intervention matching. Information on these variables is obtained from the Census, Bureau of Economic Analysis (BEA) and Uniform Crime Reporting (UCR) by FBI. As discussed above, the household income ratios as well as key predictor variables such as union memberships are not available before 1964; the terminal period for this data is 2013. The rest of the data, when available, is collected since 1964 to establish a period prior to the implementation of the RTW law in a state. The end date for the variables obtained from Frank (2009, 2014) is 2012.

Given the different timings for the implementation of the RTW law across states the pre intervention period is 1964-1975 for Louisiana, 1964-1984 for Idaho, 1964-1992 for Texas and 1964-2000 for Oklahoma. The main donor pool includes 26 states that never passed an RTW-type law. To account for possible spillover effects in preferences towards unionization and inequality, alternative donor pools were created that excluded states sharing border with the treatment state.

Among predictors, first, various demographic characteristics, population growth rates, non-white population growth rate, urbanization rate, rate of home ownership, percent Hispanic and age and education profiles of the population were used. Several labor market characteristics were also included, which are the wage and salary shares of agriculture, forestry and fishing, of manufacturing, and of retail trade. State specific minimum wage and public sector to private sector employment ratios were also included. The literature has shown that controlling for income levels prior to RTW is a crucial control in regression analysis; per capita GDP of the state was hence included as one of the predictors.

Most importantly, to account for preexisting preferences for unionization, we used as predictor state union membership growth. Since investment could have countervailing effects on inequality, growth in per capita foreign direct investment (FDI) in the state was also included. Finally, with a significant literature exploring the connection between the RTW laws and wages, we have also included growth in state's average wages and salaries as a predictor variable.[9]

Table 1 provides summary statistics of the outcome variables as well as predictor variables of the four treatment states and the 26 donor pool states. There are substantial

[9] Public to private employment and FDI data starts in 1977, hence pre-intervention FDI for Louisiana does not exist which enacted RTW in 1976.

differences across them rationalizing a case study approach. For example, while the 50-10 ratio was 4.84 in Louisiana, it was only 3.47 in Idaho (a difference of the magnitude of twice the sample standard deviation). A difference of almost a similar magnitude can be observed between the 90-10 ratio of Texas and that of Idaho. The states differ quite significantly in terms of several other characteristics vis-à-vis the average across the donor pool states implying that appropriate weighing of the control states would likely create a more accurate counterfactual for the treatment states.

5. Results

We start with our main results where we perform the SCM analysis with the predictor variables listed in table 1, and using the donor pool that includes all 26 non-RTW states. Subsequently, as robustness checks, we conduct additional tests with different predictors and different donor pools (Tables 6 and 7).

We describe in details the impact of RTW on two 'representative' measures of inequality: the top 1% income share and the 50-10 ratio. The SCM estimates of the remaining 7 inequality measures are summarized in Table 4 that further support the finding with the representative measures.

The details of the SCM estimation results for the two representative measures are presented in Tables 2 and 3. Table 2 reports a summary of the SCM estimates, the weights that the donor states contribute in the construction of the synthetic control, and the statistical results of the permutations or randomization tests (p-value and rank of the post-pre MSPE ratio). The pre-intervention characteristics matching are reported in Table 3. The pictorial depictions of the results detailed in Table 2 are presented in Figures 1-4.

Finally, we examine the impact of RTW on union membership, wages and salaries, and foreign direct investment as these are often hypothesized to be the main channels through which RTW could impact inequality (Table 5).

5.1. The Main Results

In Figures 1-4, the left panel shows the pre-intervention match and the post-intervention deviation between the synthetic and the actual. The right panel presents the permutations/randomization test described above where the post-intervention gap for the treatment state is in dark line whereas its placebo counterparts are in light lines. Recall that this test answers the question, "How often would we obtain a gap as large as that of the exposed state if we had chosen a state at random?" We therefore apply the synthetic control method to each state in the donor pool (the placebos). The visual evidence in the figures clearly suggests a lack of causal impact of RTW on the top 1% income share as well as 50-10 ratio in household income in any of the four treatment states. Across all cases, the post-intervention gaps for the treatment states (the dark line) do not stand out from their respective placebo counterparts (light lines).

In Panel A of Table 2, the first four columns report the SCM estimates for the top 1% income share and the last four columns present the results for the 50-10 ratio in household income. The pre-intervention absolute prediction error to mean ratio (APEMR) and mean square prediction error (MSPE) show good pre-intervention fits. The p-value for post-pre MSPE is not significant for Louisiana, Idaho or Texas. The post-pre RMSPE ranks are not 1 for any of the four states. In case of Oklahoma the rank is 2 and the p-value is significant at 5% level. However, as we see in the rest of Table 2 as well as in Tables 4, 5, 6 and 7, this marginally significant effect of RTW in Oklahoma is not robust; RTW does not have a significant effect on any of the other inequality measures in Oklahoma.

Panel B of Table 2 presents the w-weights that describe the contributions of the different donor pool states in the synthetic. For instance, in the first column, we find that West Virginia, Kentucky, New York and Delaware (in order) are the biggest contributors in the construction of the synthetic for Louisiana's top 1% income share. Similarly, Kentucky, California, Minnesota, West Virginia, New York and Delaware (in order) contributed the most in the construction of the synthetic for Oklahoma's 50-10 ratio.

These weights, however, are more meaningful if we study Table 3. This table presents the pre-intervention characteristics matches between the actual and the synthetic based on the w-weights. With the exception of non-white population growth and the industry shares, matching in terms of most characteristics between the synthetic and actual is quite proximate. More importantly, in terms of the crucial variables such as growth in unionization, per capita FDI, and average wage-salary, as well as effective minimum wage rates, there is a very close match between the synthetic of each state and the actual.

It is quite possible that in terms of some measures like the two discussed above, inequality was unaffected but in terms of other measures RTW could have an effect. Table 4 presents the SCM estimates of the impacts of RTW on seven other popular measures of inequality.[10] In none of the seven measures in any of the four states do we obtain a significant impact of the RTW law: rank statistics are all different from 1 and no p-value is significant.

5.2. The Possible Pathways

As discussed above, while there can be several channels through which RTW laws could affect inequality. Unionization, investments, and wages and salaries are the ones most talked

[10] Pictures for the remaining 7 inequality measures also show the same pattern as those in Figures 1-4. These pictures and other details are available upon request.

about in the literature.[11] Hence, we look at the possible impacts of the RTW law on these channels.

The literature, among other things, also points to the role of unions in directing redistribution policies itself (Korpi 2006). Among the most cited work on the impacts of RTW laws on unionization is Ellwood and Fine (1987) who estimate a fixed effects model. Fixed-effects models can sometimes be used to overcome omitted-variable problems, for example, if the omitted variable is state specific. However, in contrast to the SCM employed in this paper, the standard fixed effects model assumes unobserved characteristics to be time invariant. Additionally, given that more than two decades passed since the paper, there clearly is a basis for taking a second look at the issue.

Table 5 presents the SCM estimates of the impact of RTW on the growth of unionization, per capita foreign direct investments (FDI), and average wages and salaries. We find no significant impact on either of these variables: none of the rank statistics take a value of 1 and all the p-values are very large. These findings of no effects on possible pathways for affecting inequality essentially ratify the findings of no significant effect of RTW on inequality in each state.

Unionization rates in RTW states are in fact less than half of what they are in union security states. It is possible that the states that enacted RTW have a preference for a smaller unionized labor force. The evidence on the relationship between preference for unionization and enactment of RTW law, however, is unclear. Collins (2014) shows that when Michigan enacted RTW law in 2012, the state's union membership rate of 16.6% was way above the

[11] RTW laws can affect non-wage compensation, unemployment, etc. They can also result in other business friendly or unfriendly policies, all of which can in turn impact inequality.

national average of 11.2%. On the other hand when Oklahoma passed its RTW law in 2000, unionization rate there was only 6.9% while the national average was 13%.

Yet, in both types of states, there has been a secular decline in union membership rate. Between 1983-2012, union membership rate fell in RTW states from 11.6% to 5.6% while in non-RTW union security states it fell from 24.3% to 15% (Hirsch and Macpherson 2014). In our estimates, we find that RTW was not a contributing factor in Louisiana, Idaho, Texas and Oklahoma. The case of Idaho is particularly interesting where the unionization rate fell from 23.1% in 1981 to 12.2% in 1985, i.e., 4 years before the RTW enactment (Collins 2014).

5.3. Robustness

To test the robustness of the results even further, we perform two more tests. In the main specification in Table 2, we tried to be comprehensive and included a wide range of characteristics in the set of predictors. In Table 6, we present a set of SCM estimates where we include in the set of predictors variables that can primarily be perceived to be directly related to income distribution. These include current transfer receipts of the individuals from the government, medical benefits, state unemployment insurance compensation, supplemental Nutrition Assistance Program (SNAP), current transfer receipts of the individuals from businesses and employer contribution for employee pension and insurance funds apart from several tax related variables.

The full list of this new set of predictors and the matching related statistics are presented in table 6. Note that the demographic and industry characteristics are no longer in the set of predictors. Again, the estimates overwhelmingly show a lack of any significant effect of the RTW law on inequality (measured in 9 different ways).

Finally, it is possible that there are spillover effects in the preferences of the citizens and policies of the government among neighboring states. There might also be some labor market linkages among the bordering states. A control (non-RTW) state that borders the treatment state can be viewed as contaminated because of spillovers across the state border. To rule out the possibility that this may have influenced our results, we perform an analysis using SCM with modified donor pools where neighboring states to our treatment states are expunged from the respective donor pool. Note that all the bordering states of Louisiana are already excluded in the main donor pool. Hence, this robustness test does not include Louisiana. Results are presented in Table 7. As in case of the main estimates in Tables 2 and 4, we have excellent pre-intervention fits. In all measures for the three states, the findings are robust to these modified donor pools.

5.4. Discussion

In a recent study, Jaumotte and Buitron (2015), using cross country data of advanced economies, show that the decline in unionization is related to the rise of the top income shares. There is also evidence that the broad extension of collective agreements to non-union members is associated with higher inequality, likely owing to higher unemployment.

In contrast with such a study, we do not find these kinds of effects for the US states that brought about significant changes in labor laws over the last 50 years. In Idaho, the dramatic decline in unionization and an upsurge in employment growth preceded RTW by several years (Lafer and Allegreto 2011). Unionization there, fell from 22% to 9% during 1981-84, coinciding with President Reagan's strike breaking in the PATCO showdown in 1981 and the decline in the well-organized timber industry. Further, the manufacturing boom post RTW was driven by the high-tech industry which did not have significant unionization (Lafer and Allegreto 2011).

In Oklahoma, employment is concentrated in oil and gas, government, and military services; the latter is unaffected by RTW. Aside from that, Oklahoma has attracted about 600 new companies with its policy of rebating payroll costs (Lafer and Allegreto 2011).

Recall that in the literature, the evidence on the effect of RTW on unionization from both macro as well as micro studies is itself mixed (Davis and Huston 1985, Moore et al. 1986). In our SCM estimates, we show in section 5.2 above that RTW laws in our treatment states did not impact unionization.

Other explanations have also been offered for the across-the-board decline in unionization. These include improvements in education levels, which reduce workers' incentives to organize unions by raising the outside option of skilled employees and inducing workers to move to less unionized sectors (Acemoglu et al 2002). At the same time, a rising share in the economy of less-unionized services sector would also reflect in the declining union density (Jaumotte and Buitron 2015).

Estimates of union wage premium in Freeman (1993) and Card (1992) in the 10-17% range in the 1970s and the 1980s, no impact of RTW on wages in Moore (1998), higher wages of 6-7% in RTW states in Reed (2003), and 3.2% lower wages in RTW states in Gould and Shierholz (2011) could differ simply because they cover different time periods. For some of these studies significant amount of time has passed and lot has changed over time which could alter the results if the same estimates were derived currently.

At the same time, note that union density does not necessarily equate with union strength. Bouis et al. (2012) find that increases in the excess coverage of collective bargaining – defined as the difference between the share of workers covered by collective agreements and the share of workers that are members of a union – lead to higher unemployment implying diminished strength.

The state level analysis where the donor pool comprises other states implies that the traditional explanations for the rise of inequality such as skill-biased technological change (SBTC) and globalization could not be confounding the results as they are not likely to differ significantly across states.[12] The states within the United States would have been affected by such forces in broadly similar ways making identification of RTW's impacts easier. Similarly, the recent changes in financial deregulation which some have argued as an emerging factor for rising inequality would vary little across states within U.S. Other factors such as tax rates, which Piketty et al. (2014) highlight as contributors to inequality and can in fact vary across states, have been accounted for in our estimation (Table 6).

It is also quite possible that for RTW to bear on inequality, the required threshold of unionization rate is much higher than what we observe across U.S. states. While the private sector unionization has fallen steadily unionization among public workers remained stable. However, overall, the U.S. ranks last among the 21 top developed nations in both unionization rates and union coverage of the workforce over the period 1960-2010.

6. Conclusions

The findings in this paper do not speak in favor or against RTW adoption since we do not assess the welfare implications of the law which could have effects on different outcomes (for example, unemployment). This paper is specifically focused on the perceived connection between these laws and income inequality.

[12] Globalization has been argued to contribute to the decline in union density, as increased competition reduces rents that could be appropriated by unions. Skill biased technological change has also been put forward as a potential source of increased inequality since it can increase the outside option of skilled workers, and thereby weaken their incentives to join unions (Acemoglu et al 2002).

As more and more states adopt the Right to Work laws there is an ongoing debate whether these laws are contributing to the rising income inequality in the U.S. We adopt SCM for comparative case study to examine this issue at the state level. Specifically, we find that adoption of RTW laws in Louisiana, Idaho, Texas and Oklahoma – states that enacted their RTW laws between the 1960s and the 2000s – did not contribute to the worsening of income inequality in these states. We use a wide range of inequality measures. Our results are consistent across all measures. The finding is also robust across different specifications and choice of the control groups.

While our findings are specific to these four states they do have somewhat broader implications. It is important to reiterate that these four states, where we do not find any impact of RTW on inequality, are the only states that converted to RTW between 1964 and 2012. With the exception of Wyoming, the rest of the RTW states became so in the 1940s or the 1950s. However, inequality in the U.S. started to exacerbate in the mid-1980s (Frank 2014). If RTW were to bear on inequality, it would have to be that RTW started to have a causal effect on inequality in the states that enacted the law in the 1940s and the 1950s with a lag of more than 30 years.

Therefore, while the worsening inequality in the U.S. merits extensive exploration, RTW laws seem to be the unlikely place to look for an explanation. This is particularly important in light of the emerging literature and policy debates that argue that the tails of the income distribution are being affected by different labor market policies. The suppression of income growth in the middle and the lower part of the distribution is well documented and can originate from many different sources in an economy like the U.S. Our results suggest that perhaps more attention needs to be paid to the disproportionate leverage of various income groups in the financial market, disparities in the relative factor returns, and aspects of the labor market beyond collective bargaining.

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Figures

Figure 1: Synthetic Control Method (SCM) Estimates of the Right-to-work Law in Louisiana

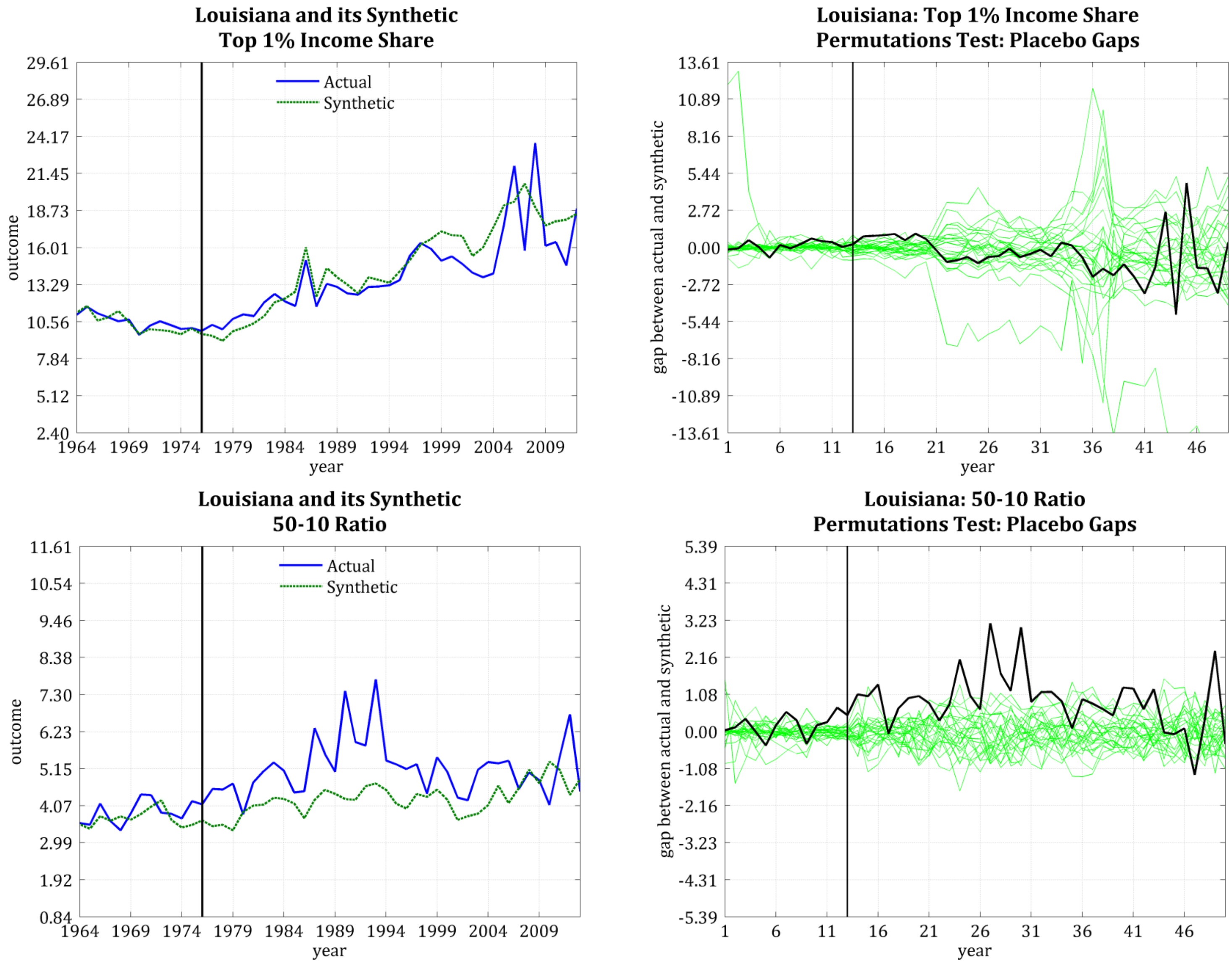


Figure 2: Synthetic Control Method (SCM) Estimates of the Right-to-work Law in Idaho

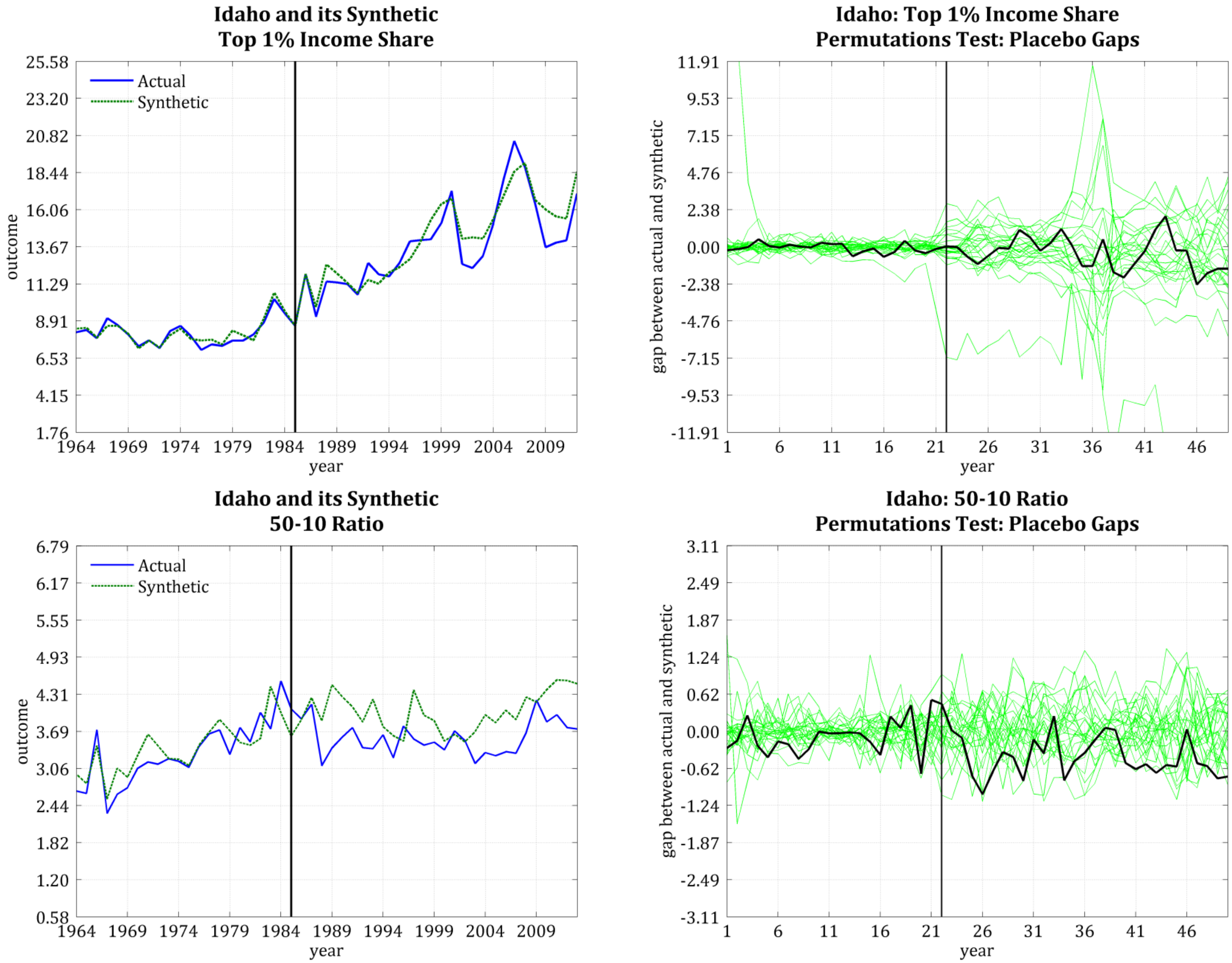


Figure 3: Synthetic Control Method (SCM) Estimates of the Right-to-work Law in Texas

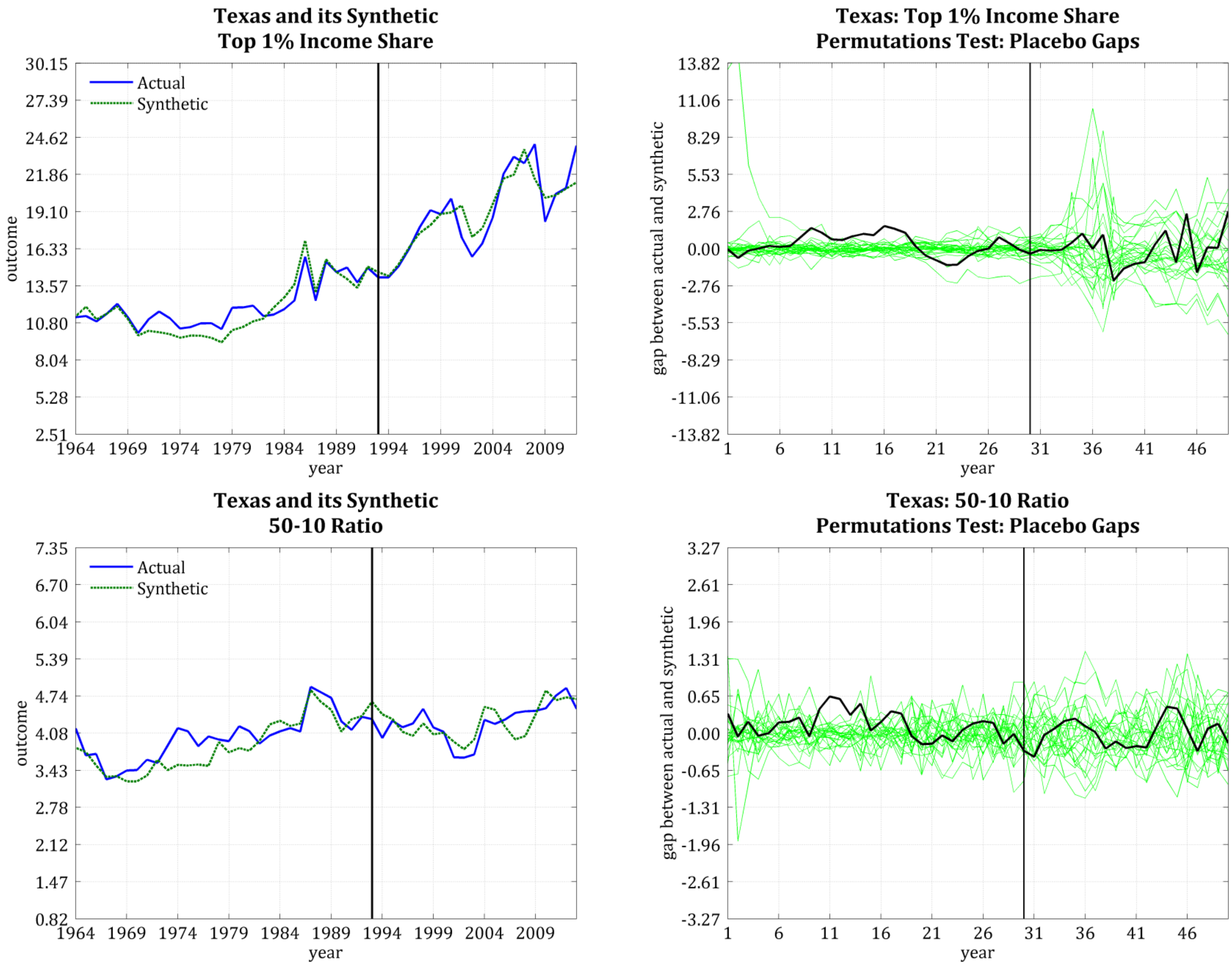
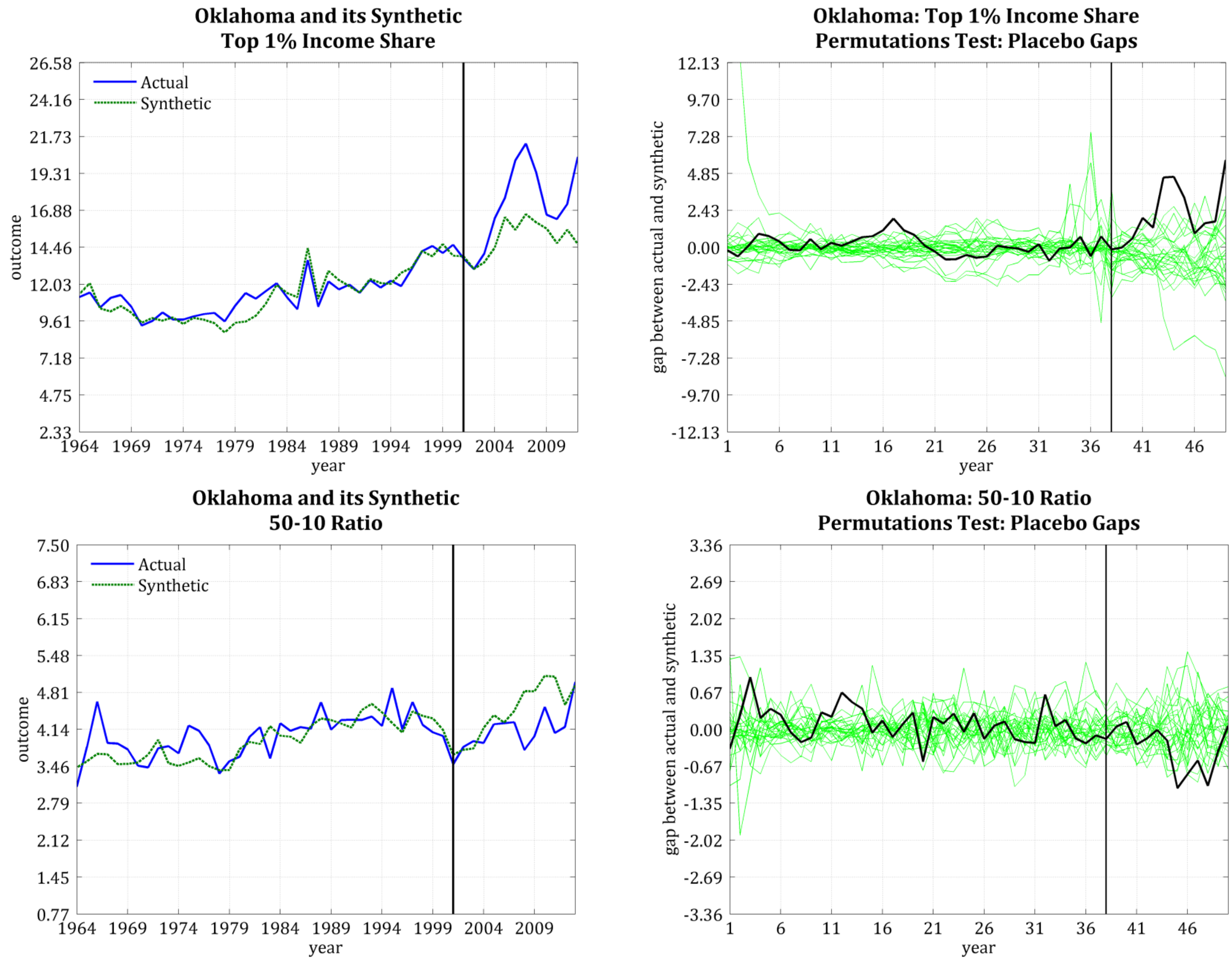


Figure 4: Synthetic Control Method (SCM) Estimates of the Right-to-work Law in Oklahoma



Tables

Table 1: Summary Statistics

	Donor pool (26 states)				Mean			
	mean	sd	min	max	Louisiana	Idaho	Texas	Oklahoma
Family income: 50-10 ratio (1964-2013)	3.92	0.64	1.71	6.00	4.84	3.47	4.12	4.04
Family income: 90-10 ratio (1964-2013)	9.01	2.39	3.79	17.00	12.63	7.54	10.50	9.65
Family income: 90-50 ratio (1964-2013)	2.27	0.29	1.65	3.31	2.59	2.17	2.53	2.38
Inequality: Gini coefficient (1964-2012)	0.51	0.05	0.41	0.67	0.53	0.53	0.55	0.53
Inequality: Atkinson index (1964-2012)	0.22	0.04	0.15	0.39	0.22	0.20	0.24	0.21
Inequality: Theil's entropy index (1964-2012)	0.55	0.20	0.29	1.39	0.54	0.48	0.64	0.52
Inequality: relative mean deviation (1964-2012)	0.72	0.07	0.56	0.95	0.75	0.74	0.78	0.74
Top one percent income share (1964-2012)	12.25	4.03	6.73	31.33	12.44	10.47	13.72	11.85
Top decile income share (1964-2012)	37.11	5.16	28.05	58.84	38.59	33.73	39.01	37.01
Average wage and salary growth (1969-2013)	1.23	0.22	0.97	1.88	1.26	1.20	1.34	1.24
Union membership growth (1964-2013)	0.67	0.21	0.22	1.35	0.61	0.54	0.63	0.69
Per capita FDI growth (1977-2007)	4.46	2.96	0.80	19.10	2.66	5.63	2.95	2.99
Population growth rate (census)	0.10	0.08	-0.08	0.31	0.08	0.19	0.22	0.10
Non-white population growth rate (census)	0.56	0.45	-0.22	2.36	0.12	0.97	0.53	0.39
Percent White (includes White-Hispanic)	0.88	0.09	0.60	1.00	0.68	0.95	0.78	0.83
Percent (non-White) Hispanic	0.03	0.04	0.00	0.22	0.01	0.03	0.08	0.02
Population age>25 with high school (%)	0.72	0.13	0.32	0.92	0.62	0.75	0.65	0.69
Log per capita income (2005 dollars)	9.82	0.27	9.22	10.41	9.58	9.64	9.74	9.67
Wage-salary share: agri, forestry, fishing (%)	0.36	0.18	0.09	1.05	0.34	0.88	0.42	0.33
Wage-salary share: manufacturing (%)	26.23	9.41	6.25	49.14	16.33	19.41	18.72	17.53
Wage-salary share: retail trade (%)	10.22	1.13	6.95	13.29	10.19	11.77	10.69	10.60
Percent urban population	0.70	0.16	0.32	0.94	0.69	0.58	0.81	0.67
Percent homeowners	0.66	0.06	0.47	0.75	0.66	0.71	0.63	0.69
Public to private total employment ratio	0.20	0.06	0.13	0.44	0.23	0.23	0.20	0.25
State effective minimum wage (current dollars)	4.16	1.91	1.60	9.19	3.95	3.95	3.95	3.95
Proportion of population in poverty	0.12	0.03	0.06	0.31	0.22	0.13	0.17	0.16

Notes: (a) Maximum time period is 1964-2013. Numbers of observations are not same across variables. The period of availability is described in parenthesis. (b) 26 states in the donor pool. DC is excluded. Alaska and Hawaii are not RTW states, but they have missing data and hence not in the donor pool. (c) Indiana and Michigan included in the donor pool because they became RTW states in 2012.

Table 2: Synthetic Control Method (SCM) Estimates of the Impact of the Right to Work Laws on Inequality

	Top 1% income share				50-10 Ratio			
	LA	ID	TX	OK	LA	ID	TX	OK
Panel A: Estimation Statistics								
Pre-intervention APEMR	0.03	0.03	0.06	0.04	0.08	0.08	0.05	0.07
Pre-intervention MSPE	0.15	0.09	0.71	0.40	0.12	0.10	0.08	0.11
Post-pre MSPE ratio	21.40	14.16	2.45	22.12	12.15	3.11	0.78	2.79
Post-intervention gap	-0.55	-0.40	0.03	2.38	0.90	-0.44	0.00	-0.36
P-value: Post-pre MSPE ratio	0.70	0.63	0.89	0.04	0.26	0.41	0.85	0.22
Post-pre MSPE ratio rank	20	18	25	2	8	12	24	7
Panel B: Donor Pool w-weights								
California	0.00	0.00	0.00	0.00	0.00	0.00	0.24	0.20
Colorado	0.00	0.00	0.00	0.00	0.00	0.90	0.00	0.00
Connecticut	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Delaware	0.04	0.00	0.02	0.10	0.00	0.10	0.00	0.08
Illinois	0.00	0.00	0.00	0.00	0.00	0.00	0.40	0.00
Indiana	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Kentucky	0.30	0.00	0.00	0.00	0.55	0.00	0.00	0.38
Maine	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Maryland	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Massachusetts	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Michigan	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Minnesota	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.17
Missouri	0.00	0.00	0.00	0.00	0.00	0.00	0.36	0.00
Montana	0.00	0.64	0.00	0.00	0.00	0.00	0.00	0.00
New Hampshire	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
New Jersey	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
New Mexico	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
New York	0.23	0.00	0.42	0.04	0.00	0.00	0.00	0.08
Ohio	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Oregon	0.00	0.00	0.00	0.00	0.29	0.00	0.00	0.00
Pennsylvania	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Rhode Island	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Vermont	0.00	0.16	0.00	0.00	0.00	0.00	0.00	0.00
Washington	0.00	0.20	0.00	0.00	0.00	0.00	0.00	0.00
West Virginia	0.42	0.00	0.56	0.85	0.17	0.00	0.00	0.09
Wisconsin	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes: (a) Pre-intervention periods: Louisiana (LA) 1964-1975, Idaho (ID) 1964-1984, Texas (TX) 1964-1992, Oklahoma (OK) 1964-2000. Pre-intervention outcome variables are for each states are for the respective pre-intervention periods. (b) APEMR refers to absolute prediction error to mean ratio, MSPE refers to mean square prediction error. (c) Donor pool states with w-weight<0.01 are reported as zeroes. (d) Except for the pre-intervention outcome, the set of predictors is the same in each estimate (see Table 3 for details).

Table 3: Pre-intervention Characteristics Comparison (Top 1% Income Share and 50-10 Ratio SCM Estimates)

	Louisiana			Idaho			Texas			Oklahoma		
	(1)	(2)	Actual	(1)	(2)	Actual	(1)	(2)	Actual	(1)	(2)	Actual
Population growth	0.02	0.07	0.12	0.12	0.27	0.20	0.03	0.10	0.22	0.04	0.12	0.14
Non-white population growth	0.09	0.19	0.05	0.67	1.44	1.24	0.19	0.52	0.64	-0.01	0.49	0.40
Proportion White	0.92	0.94	0.70	0.95	0.91	0.97	0.90	0.85	0.83	0.94	0.90	0.87
Proportion Hispanic	0.00	0.00	0.00	0.00	0.03	0.01	0.01	0.02	0.04	0.00	0.01	0.01
% population 25+ with high school	0.44	0.45	0.42	0.66	0.69	0.65	0.52	0.59	0.54	0.49	0.55	0.57
Per capita income	9.35	9.33	9.22	9.53	9.66	9.46	9.52	9.67	9.55	9.43	9.55	9.51
Average wage-salary growth	1.04	1.04	1.03	1.05	1.09	1.06	1.07	1.03	1.11	1.09	1.05	1.11
Share of agriculture	0.15	0.23	0.32	0.38	0.30	0.68	0.15	0.29	0.39	0.14	0.31	0.30
Share of manufacturing	28.86	28.62	18.57	17.58	19.49	19.35	26.03	28.74	20.65	27.78	27.53	18.13
Share of retail	9.49	10.52	10.46	11.99	11.35	12.21	8.91	10.40	11.04	9.00	10.18	10.80
Proportion urban	0.55	0.54	0.66	0.54	0.79	0.54	0.58	0.80	0.80	0.43	0.65	0.68
Homeownership rate	0.63	0.67	0.63	0.67	0.64	0.71	0.62	0.62	0.65	0.70	0.65	0.70
Union membership growth	0.95	0.93	0.97	0.80	0.83	0.83	0.92	0.86	0.88	0.90	0.87	0.91
Public to private employment	--	--	--	0.27	0.22	0.25	0.25	0.20	0.18	0.25	0.22	0.23
Effective minimum wage	1.63	1.60	1.60	2.30	2.30	2.30	2.32	2.31	2.30	2.30	2.31	2.30
Per capita FDI growth	--	--	--	2.11	2.20	2.84	2.26	1.68	1.89	2.49	1.95	2.03
Proportion poverty	0.20	0.20	0.27	0.13	0.12	0.13	0.17	0.12	0.18	0.19	0.16	0.17

Notes: (a) The underlying estimates are reported in Table 2. (b) Public to private employment and FDI data starts in 1977, hence pre-intervention FDI for Louisiana does not exist which enacted RTW in 1976. (c) Column (1) is the synthetic for top 1% income share, column (2) is the synthetic for 50-10 ratio.

Table 4: Synthetic Control Method (SCM) of the Impact of the Right to Work Laws on Various Inequality Measures

	Gini	Atkinson	Theil	Rel mean deviation	Top 10% share	90-10 Ratio	90-50 Ratio
Louisiana							
Pre-intervention APEMR	0.01	0.03	0.03	0.01	0.01	0.10	0.07
Pre-intervention MSPE	0.00	0.00	0.00	0.00	0.37	1.18	0.04
Post-pre MSPE ratio	11.82	5.83	35.89	10.70	12.44	12.06	1.55
Post-intervention gap	0.03	0.02	0.06	0.04	-0.48	2.96	0.17
P-value: Post-pre MSPE ratio	0.15	0.48	0.33	0.11	0.63	0.37	0.93
Post-pre MSPE ratio rank	5	14	10	4	18	11	26
Idaho							
Pre-intervention APEMR	0.02	0.04	0.04	0.02	0.03	0.07	0.04
Pre-intervention MSPE	0.00	0.00	0.00	0.00	1.46	0.44	0.01
Post-pre MSPE ratio	0.95	2.25	5.51	0.24	1.89	15.20	1.51
Post-intervention gap	0.00	-0.01	-0.01	0.00	-0.95	-2.34	0.02
P-value: Post-pre MSPE ratio	0.89	0.67	0.89	0.93	0.93	0.15	0.89
Post-pre MSPE ratio rank	25	19	25	26	26	5	25
Texas							
Pre-intervention APEMR	0.02	0.03	0.06	0.03	0.02	0.08	0.04
Pre-intervention MSPE	0.00	0.00	0.00	0.00	0.59	0.96	0.01
Post-pre MSPE ratio	0.75	1.37	4.82	1.07	5.45	1.39	1.08
Post-intervention gap	0.01	0.00	-0.06	0.02	0.22	-0.20	0.07
P-value: Post-pre MSPE ratio	0.89	0.81	0.74	0.59	0.63	0.93	0.93
Post-pre MSPE ratio rank	25	23	21	17	18	26	26
Oklahoma							
Pre-intervention APEMR	0.01	0.03	0.05	0.01	0.04	0.07	0.03
Pre-intervention MSPE	0.00	0.00	0.00	0.00	2.71	0.63	0.01
Post-pre MSPE ratio	0.34	1.83	8.14	1.35	0.64	5.01	1.24
Post-intervention gap	0.00	0.01	0.08	0.01	-0.71	-1.30	-0.02
P-value: Post-pre MSPE ratio	0.96	0.52	0.30	0.22	0.93	0.15	0.74
Post-pre MSPE ratio rank	27	15	9	7	26	5	21

Notes: (a) Pre-intervention periods: Louisiana (1964-1975), Idaho (1964-1984), Texas (1964-1992), Oklahoma (1964-2000). Pre-intervention outcome variables are for each states are for the respective pre-intervention periods. (b) APEMR refers to absolute prediction error to mean ratio, MSPE refers to mean square prediction error. (c) Donor pool is the same as that in Table 2. (d) Set of predictors is the same Table 3.

Table 5: Synthetic Control Method (SCM) Estimates of the Impact of Right to Work Law on Union Membership, Per capita FDI and Average Wages and Salaries

	Union membership growth	Per capita FDI growth	Average wages-salary growth
Louisiana			
Pre-intervention APEMR	0.02	--	0.00
Pre-intervention MSPE	0.00	--	0.00
Post-pre MSPE ratio	26.04	--	241.43
Post-intervention gap	-0.15	--	0.07
P-value: Post-pre MSPE ratio	0.26	--	0.26
Post-pre MSPE ratio rank	8	--	8
Idaho			
Pre-intervention APEMR	0.05	0.14	0.01
Pre-intervention MSPE	0.00	0.23	0.00
Post-pre MSPE ratio	5.56	53.24	109.90
Post-intervention gap	-0.13	2.81	0.08
P-value: Post-pre MSPE ratio	0.22	0.85	0.22
Post-pre MSPE ratio rank	7	24	7
Texas			
Pre-intervention APEMR	0.05	0.04	0.04
Pre-intervention MSPE	0.00	0.01	0.00
Post-pre MSPE ratio	0.47	48.87	1.56
Post-intervention gap	0.02	-0.46	0.06
P-value: Post-pre MSPE ratio	0.93	0.56	0.93
Post-pre MSPE ratio rank	26	16	26
Oklahoma			
Pre-intervention APEMR	0.07	0.09	0.02
Pre-intervention MSPE	0.01	0.15	0.00
Post-pre MSPE ratio	1.11	4.34	1.56
Post-intervention gap	-0.04	0.75	0.03
P-value: Post-pre MSPE ratio	0.63	0.59	0.85
Post-pre MSPE ratio rank	18	17	24

Notes: (a) Pre-intervention periods: Louisiana (1964-1975), Idaho (1964-1984), Texas (1964-1992), Oklahoma (1964-2000). Pre-intervention outcome variables are for each states are for the respective pre-intervention periods. (b) APEMR refers to absolute prediction error to mean ratio, MSPE refers to mean square prediction error. (c) Donor pool is the same as that in Table 2. (e) Set of predictors is the same Table 3. (d) FDI data starts in 1977, hence pre-intervention FDI for Louisiana does not exist which enacted RTW in 1976.

Table 6: SCM of the Impact of the Right to Work Laws on Various Inequality Measures (Alternative Set of Predictors)

	Gini	Atkinson	Theil	Rel mean deviation	Top 1% share	Top 10% share	50-10 Ratio	90-10 Ratio	90-50 Ratio
Louisiana									
Pre-intervention APEMR	0.01	0.03	0.03	0.01	0.03	0.01	0.08	0.10	0.07
P-value: Post-pre MSPE ratio	0.26	0.30	0.19	0.15	0.70	0.63	0.26	0.33	0.93
Post-pre MSPE ratio rank	8	9	6	5	20	18	8	10	26
Idaho									
Pre-intervention APEMR	0.02	0.03	0.04	0.02	0.03	0.03	0.07	0.07	0.04
P-value: Post-pre MSPE ratio	0.85	0.93	0.56	0.85	0.70	0.93	0.11	0.19	0.96
Post-pre MSPE ratio rank	24	26	16	24	20	26	4	6	27
Texas									
Pre-intervention APEMR	0.02	0.03	0.06	0.03	0.06	0.02	0.05	0.08	0.04
P-value: Post-pre MSPE ratio	0.85	0.63	0.56	0.56	0.89	0.52	0.89	0.85	0.96
Post-pre MSPE ratio rank	24	18	16	16	25	15	25	24	27
Oklahoma									
Pre-intervention APEMR	0.01	0.03	0.05	0.01	0.04	0.04	0.06	0.07	0.03
P-value: Post-pre MSPE ratio	0.78	0.52	0.30	0.37	0.04	0.96	0.22	0.15	0.74
Post-pre MSPE ratio rank	22	15	9	11	2	27	7	5	21

List of predictors

In per capita logarithm: Income, current transfer receipts of individuals from governments, medical benefits, state unemployment insurance compensation, supplemental Nutrition Assistance Program (SNAP), receipts from state and local governments, current transfer receipts of individuals from businesses, personal current taxes to the state government, state and local property taxes, employer contributions for employee pension and insurance funds.

As ratios, percentages or levels: Employed to population ratio, public to private employment, effective minimum wage, proportion poverty.

Notes: (a) Pre-intervention periods: Louisiana (1964-1975), Idaho (1964-1984), Texas (1964-1992), Oklahoma (1964-2000). (b) Pre-intervention outcome variables for each state correspond to the respective pre-intervention periods. Public to private employment starts in 1977, hence not available for Louisiana SCM. (c) APEMR refers to absolute prediction error to mean ratio, MSPE refers to mean square prediction error. (c) Donor pool is the same as that in Table 2.

Table 7: SCM of the Impact of the Right to Work Laws on Various Inequality Measures (Excluding the Border States from the Donor pool)

	Gini	Atkinson	Theil	Rel mean deviation	Top 1% share	Top 10% share	50-10 Ratio	90-10 Ratio	90-50 Ratio
Idaho									
Pre-intervention APEMR	0.02	0.04	0.04	0.02	0.07	0.03	0.07	0.07	0.04
P-value: Post-pre MSPE ratio	0.38	0.92	0.50	0.58	0.75	0.88	0.33	0.17	0.92
Post-pre MSPE ratio rank	10	23	13	15	19	22	9	5	23
Texas									
Pre-intervention APEMR	0.03	0.03	0.06	0.03	0.06	0.02	0.05	0.08	0.04
P-value: Post-pre MSPE ratio	0.96	0.64	0.88	0.96	0.88	0.48	0.88	0.92	0.96
Post-pre MSPE ratio rank	25	17	23	25	23	13	23	24	25
Oklahoma									
Pre-intervention APEMR	0.02	0.03	0.05	0.01	0.04	0.05	0.07	0.07	0.04
P-value: Post-pre MSPE ratio	0.96	0.33	0.25	0.46	0.08	0.88	0.29	0.17	0.88
Post-pre MSPE ratio rank	24	9	7	12	3	22	8	5	22

Notes: (a) All the bordering states of Louisianan are already excluded in the main donor pool. (b) In case of Texas, Colorado is excluded. Although Colorado technically does not border Texas the two states are separated by only a 35-mile-stip of the Oklahoma panhandle. (c) Pre-intervention periods: Idaho (1964-1984), Texas (1964-1992), Oklahoma (1964-2000). APEMR refers to absolute prediction error to mean ratio, MSPE refers to mean square prediction error. (d) Pre-intervention fit refers to pre-intervention absolute prediction error to mean ratio. (e) Set of predictors is the same Table 3.