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

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Selected Paper prepared for presentation at the 2015 Agricultural & Applied Economics Association and Western Agricultural Economics Association Annual Meeting
San Francisco, CA, July 26-28.

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Abstract

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

JEL Classification: J01, J08, J23, J38, J39, J51, L59
Keywords: Right to Work, Synthetic Control, unionization, inequality

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1. Introduction

Wilkinson and Pickett (2009) judge United States and the United Kingdom to be the most unequal among rich countries. In the U.S., 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%, 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.

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. One strand of literature debates whether labor regulations such as Right to Work (RTW) laws or relatively stagnant minimum wage regulations are possible contributors to increasing income inequality in the U.S. (Manzo and Bruno 2014). RTW laws/statutes remove union membership as a pre-requisite for employment as it makes it illegal for labor unions and employers to enter into contracts that require employees to be fee-paying members of a union.[1]

RTW in principle can accentuate inequality by suppressing wages of low income workers by constraining their bargaining powers. Decomposing wage variance, Western and Rosenfeld (2011) argue that between 1973 and 2007, unions' effect 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. Others argue that RTW

[1] The Taft-Harley Act (1947) allowed states to supersede union security provisions of the National Labor Relations Act by enacting RTW laws.

laws are in fact investment and employment friendly and have increased wages by 3-4 percent, particularly in non-union industries (Rinz 2012).[2]

To the best of our knowledge, only Nieswiadomy et al. (1991) assess the effects of RTW on income inequality using a Gini coefficient. We use Synthetic Control Method (SCM) for comparative case studies (Abadie et al. 2010) to estimate the counterfactual for Louisiana, Idaho, Texas and Oklahoma in the absence of RTW law; we find no significant impact of RTW on a comprehensive set of measures of inequality (e.g., gini coefficient, 50-10 ratio) in these states.

Our data covers a 50-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). 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 are the only states that enacted RTW between the 1970s and the 2000s, offering reasonable number of periods of both pre- and post-intervention information.

One of the 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

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

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.

Treating the RTW laws in different states as the exact same intervention is also likely to be incorrect. Texas, for instance; had a version of the RTW law passed in 1947 – modified in 1951 – that did not provide a comprehensive enforcement mechanism (Meyers 1955). The current language of the Texas law that we consider, took place in 1993 where enforcement mechanism was formalized. In Louisiana, a 1954 RTW law was repealed in 1956; the 1976 law was the one finally passed. Canak and Miller (1990) show that the composition of business support to RTW varies across states and over time.

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

2. Right-to-work Laws: A Review

The Taft-Harley 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 laws.[3] Currently 23 states have right-to-work laws. Of these, 12 states passed Right-to-work laws before 1950 and another six passed them prior to 1960. The two most recent states to adopt these laws are Oklahoma (2001) and Indiana (2012).

There is some evidence to suggest that RTW laws have had a negative impact on union organizing efforts. As per a report by the Congressional Research Service, union

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

membership rates are nearly three times lower in RTW states than in union security agreement states. Further, the same study shows that employment growth was higher in RTW states relative to non-RTW states over the period 2001-2011. Finally, wages were also higher in union security states than RTW states. Indeed, these differentials in aggregate outcomes between RTW and non-RTW states do not indicate causality since it is impossible to observe the counterfactual of how economic outcomes would differ in the absence of RTW laws. Establishing causality would require controlling for pre-existing trends and other factors that might influence economic outcomes independently.

While earlier studies (Lumsden and Petersen, 1975; Warren and Strauss, 1979; Wessels, 1981; and Eliwood and Fine, 1983) all find negative correlations between unionization and RTW laws, they tend to differ on the pathways leading to such an association. Each of these studies attempts, using different techniques and different data, to determine whether the negative correlation is caused by RTW laws or simply reflects tastes that result in enactment of RTW laws as well as less unionization. Warren and Strauss (1979) and Eliwood and Fine (1983) find that RTW laws have a real effect on the extent of unionization while the others find that RTW laws have no real effect and merely reflect pre—existing tastes. This finding is also supported by Farber (1983). Ellwood and Fine (1987) who look at changes in new union organizing efforts after the passage of RTW laws. The study finds 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 though these effects faded in subsequent years, they might have led to a permanent decline in unionization levels.

Regarding the impact of RTW laws on wages, a summary of the empirical literature by Moore (1998) concludes that "RTW laws have no impact on union wages, nonunion wages, or average wages in either the public or private sector.[4] However, subsequent studies have challenged this conclusion. Gould and Shierholz (2011) for example using household survey data compare wages between RTW and union security states while controlling for personal (such as the lower share of workers in RTW states with college degrees) as well as state level characteristics (example -higher cost of living in union security states). The study concludes that the mean effect of working in a RTW state is a 3.2 percent reduction in wages and finds negative relationships between RTW and employer-provided benefits as well.

Another study by Reed (2003) challenges the conclusion in Gould and Shierholz (2011). Reed (2003) unlike other studies, controls for the states' varied economic conditions (such as each state's per capita income in 1945), prior to the initial wave of RTW laws after the passage of the Taft-Hartley Act. The study reasons that since RTW states were typically the lower-income states at the time of enacting RTW, comparisons between RTW and union security states should control for this initial condition. The study concludes that after controlling for income levels in 1945, RTW laws resulted in wages that were 6.7 percent higher than in union security states. The study also concludes that this effect was strongest in states with the lowest levels of income in 1945 and that states with higher initial incomes experienced weaker or perhaps even negative effects from RTW laws.

[4] William J. Moore, "The Determinants and Effects of Right-to-Work Laws: A Review of the Recent Literature," Journal of Labor Economics, Summer 1998, p. 460.

3. Synthetic Control Method (SCM) for Comparative Case Study

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

There are a number of advantages to using SCM in this study. 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 the set of all non-RTW states nor a single non-RTW state likely approximates the most relevant characteristics of an RTW (exposed) 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). In assessing studies that evaluate impacts of minimum wage legislations, Neumark et al. (2013) point out that there are underlying assumptions of similarities in a number of papers that adopted regression-based models. Unlike these *ad hoc* strategies with a presumption of affinity, SCM demonstrates affinities of the donor pool states with the exposed state.

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 our focus is one state at a time. 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 and can seriously overstate significance of the intervention (Donald and Lang, 2007, Buchmueller et al., 2011). Bertrand et al. (2004) also emphasize that regression-based difference-in-difference analyses tend to overstate the significance of the policy intervention in state-level policy analyses. 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, Abadie et al. (2010) argue 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. 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.[5]

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

[5] 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."

design while remaining blind to how each particular decision affects the conclusions of the study is a safeguard against actions motivated by a ‘desired’ finding (Rubin 2001).

2.2. 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 states 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 $i = 1,..., J+1$ and periods $t = 1,..., T$, 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.

Indexing the exposed state as state 1, we want to estimate $(\alpha_{1T_0+1},\ldots,\alpha_{1T})$. From equation (1) we note that $\alpha_{1t} = Y_{1t} - Y_{1t}^N$ for $t \in \{T_0+1,\ldots,T\}$, and while $\boldsymbol{Y}_{1t}$ is observed Y_{1t}^N is unobserved. We, therefore, need to estimate Y_{1t}^N.

Suppose Y_{it}^N is given by the model,

(2) $$Y_{it}^N = \delta_t + \boldsymbol{\theta}_t \mathbf{Z}_t + \boldsymbol{\lambda}_t \boldsymbol{\mu}_i + \varepsilon_{it},$$

where, δ_t is an unknown common factor constant across states, $\mathbf{Z}_t$ is a $(r \times 1)$ vector of observed covariates (not affected by the intervention), $\boldsymbol{\theta}_t$ is a $(1 \times r)$ vector of unknown parameters, $\boldsymbol{\lambda}_t$ is a $(1 \times F)$ vector of unobserved time-varying common factors, $\boldsymbol{\mu}_i$ is a $(F \times 1)$ vector of unknown unit specific factors, and ε_{it} are the unobserved transitory shocks at the state level with zero mean.

Consider a $(J \times 1)$ vector of weights $\mathbf{W} = (w_2,\ldots,w_{J+1})'$ such that $\{w_j \geq 0 \mid j = 2,\ldots,J+1\}$ and $\sum_{j=2}^{J+1} w_j = 1$. Each value of the vector $\mathbf{W}$ represents a weighted average of the control states and, hence, a potential synthetic control. Abadie et al. (2010) show that, there exist $\mathbf{W}^* = (w_2^*,\ldots,w_{J+1}^*)'$ such that, $Y_{1t}^N = \sum_{j=2}^{J+1} w_j^* Y_{jt}$, $t = 1,\ldots T_0$, and $\mathbf{Z_1} = \sum_{j=2}^{J+1} w_j^* \mathbf{Z}_j$ (that is, pre-intervention matching with respect to the outcome variable as well as the covariates, henceforth referred to as predictors), then under standard conditions we can use,

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

as an estimator for α_{1t}. The term $\sum_{j=2}^{J+1} w_j^* Y_{jt}$ on the right-hand-side of (4) is simply the weighted average of the observed outcome of the control states for $t \in \{T_0+1,\ldots,T\}$ with weights $\mathbf{W}^*$.

Below we describe the procedure to obtain $\mathbf{W}^*$. Let $(T_0 \times 1)$ vector $\mathbf{K} = (k_1,\ldots,k_{T_0})'$ define a linear combination of pre-intervention outcomes $\widetilde{Y}_i^{\mathbf{K}} = \sum_{s=0}^{T_0} k_s Y_{is}$. Define $\mathbf{X}_1 = (\mathbf{Z}_1', \widetilde{Y}_1^{\mathbf{K}_1},\ldots,\widetilde{Y}_1^{\mathbf{K}_M})'$ as a $(k \times 1)$ vector of pre-intervention characteristics for the exposed state where $k = r + M$.[6] Similarly, define a $(k \times J)$ matrix $\mathbf{X}_0$ that contains the same variables for the unexposed states. The $j-\text{th}$ column of $\mathbf{X}_0$, thus, is $(\mathbf{Z}_j', \widetilde{Y}_j^{\mathbf{K}_1},\ldots,\widetilde{Y}_j^{\mathbf{K}_M})'$.

Let $\mathbf{V}$ be a $(k \times k)$ symmetric positive semidefinite matrix. Then,

(4) $$\mathbf{W}^* = \underset{\mathbf{W}}{\text{argmin}}\ (\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})'\mathbf{V}(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}) \ni \{w_j \geq 0 \mid j = 2,\ldots,J+1\} \text{ and } \sum_{j=2}^{J+1} w_j = 1.$$

Following Abadie et al. (2010), we choose $\mathbf{V}$ among positive definite and diagonal matrices such that the mean squared prediction error (MSPE) of the outcome variable is minimized for the pre-intervention periods.

As Abadie et al. (2010) argue, it is important to note that equation (2) generalization and that the traditional regression-based difference-in-difference model can be obtained if we impose that λ_t be constant for all t. Thus, 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, this model

[6] For example, if $M = 2$, $\mathbf{K}_1 = (1,0,\ldots,0)'$ and $\mathbf{K}_2 = (0,0,\ldots,1)'$ then $\mathbf{X}_1 = (\mathbf{Z}_1', Y_1, Y_{T_0})'$, that is the outcome values for the first year and the year before the passing of the RTW law are included in $\mathbf{X}_1$.

allows the effects of such unobservables to vary with time. In particular, Abadie et al. (2010) show that a synthetic control can fit $\mathbf{Z_1}$ and a long set of pre-intervention outcomes, $Y_{11},\dots,Y_{1T_0}$, only as long as it fits $\mathbf{Z_1}$ and μ_1 (unknown factors of the exposed unit).

2.3. 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. Following Abadie, Diamond, and Hainmueller (2010) we calculate the ratio of post-intervention to pre-intervention Mean Square Prediction Error or MSPE (the squared difference between the actual outcome and the synthetic outcome), denoted by Δ_{MA}. 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 this estimate, we apply the permutations test suggested by Bertrand et al. (2002), Buchmueller et al. (2009), Abadie et al. (2010), and 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_{MA}. 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_{MA} (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, Duflo, and Mullianathan 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 MSPE ratio of the exposed state is ranked first with a statistically significant p-value.

3. Data

Inequality measures Gini coefficient, Atkinson index, relative mean deviation, Theil's entropy index, and top 1 and 10 percent income shares are from Frank (2009) and are available until 2005. Using the family income share information from Census Population Survey (CPS), we obtain 90-50, 90-10 and 50-10 ratios; these are available until 2011 (table 1). We use an extensive set of predictors – from the US census, Bureau of Economic Analysis (BEA), and Uniform Crime Reporting (UCR) by FBI – to obtain good pre-intervention matching.

4. Results and Discussion

Figure 1 summarizes the SCM estimates for top 1% income share: for each state, the left panel shows the pre-intervention match and the post-intervention deviation between the synthetic and the actual, the right panel shows that the post-intervention gap for the treatment state (the dark line) does not stand out from its placebo counterparts (light lines). Figure 2 shows the same pattern for 50-10 ratio.[7]

Complete SCM results for top 1% income share and 50-10 ratio are in Tables 2. We have also run the same estimates for the 7 remaining measures (Table 3). Of these 36 estimates, we find that, in only one case, top 1% income share in Oklahoma, there is a statistically significant impact (an increase). Note, in case of Oklahoma, unlike the family-income-based measures, only 4 years of post-intervention information is available for top 1% income share.

Unionization and investments are the main channels hypothesized for RTW to impact inequality. Table 4 presents the SCM estimates of the impact of RTW on unionization and foreign direct investments (FDI); we find no significant effect on either of these variables.

In the literature as well, the evidence on the effect of RTW on unionization is mixed. While Nieswiodomy et al. (1991) and Ellwood and Fine (1987) find significant effect on unionization, others like Farber (1984) find RTW as merely reflecting pre-existing tastes for unionization. The evidence from micro studies is also mixed (Davis and Huston 1985, Moore et al. 1986).

[7] Pictures for the remaining 7 inequality measures also show the same pattern and are available upon request.

Freeman (1993) and Card (1992) estimated union wage premium in the 10-17% range in the 1970s and 1980s while studies such as Moore (1998) find no impact of RTW on wages. More recently, Reed (2003) finds wages to be 6.7% higher in RTW states while Gould and Shierholz (2011) attribute 3.2% reduction in wages and lower employer-provided benefits to RTW.

In Idaho, a dramatic decline in unionization and an upsurge in employment growth preceded RTW by several years (Lafer and Allegreto 2011): unionization 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).

6. Conclusion

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. We use a wide range of inequality measures for Idaho, Louisiana, Oklahoma and Texas, states that enacted RTW between the 1960s and the 2000s. Our results suggest that RTW laws did not impact income inequality in these four states.

It is important to point out that these four states are also 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 40s or the 50s. However, inequality in the U.S. started to exacerbate in the mid-1980s. If RTW were to bear on inequality, it would have to be with a lag of more than 30 years in states that enacted RTW in the 40s and the 50s. While the worsening inequality in the U.S. merits extensive exploration RTW laws seem to be the unlikely place to look.

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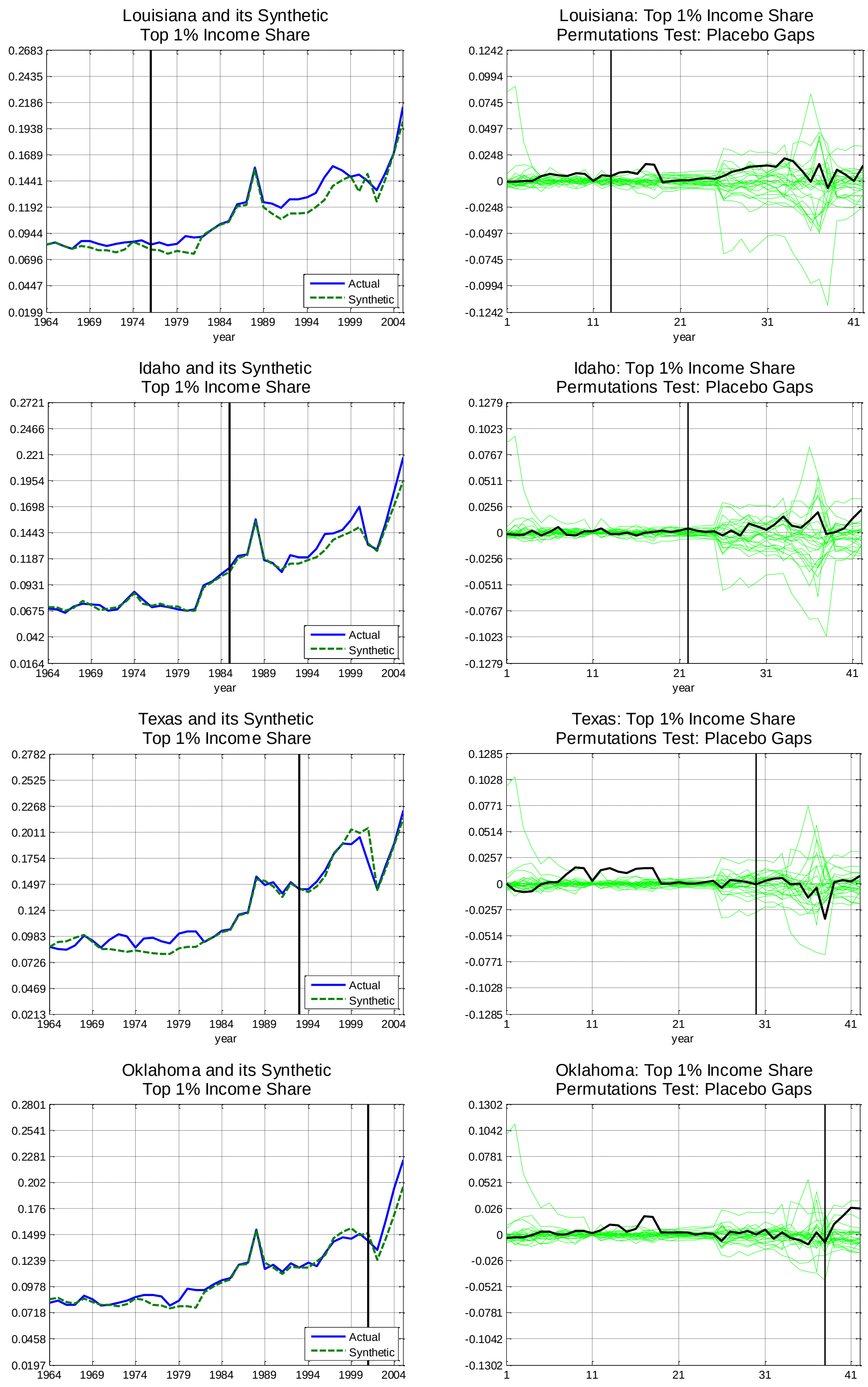
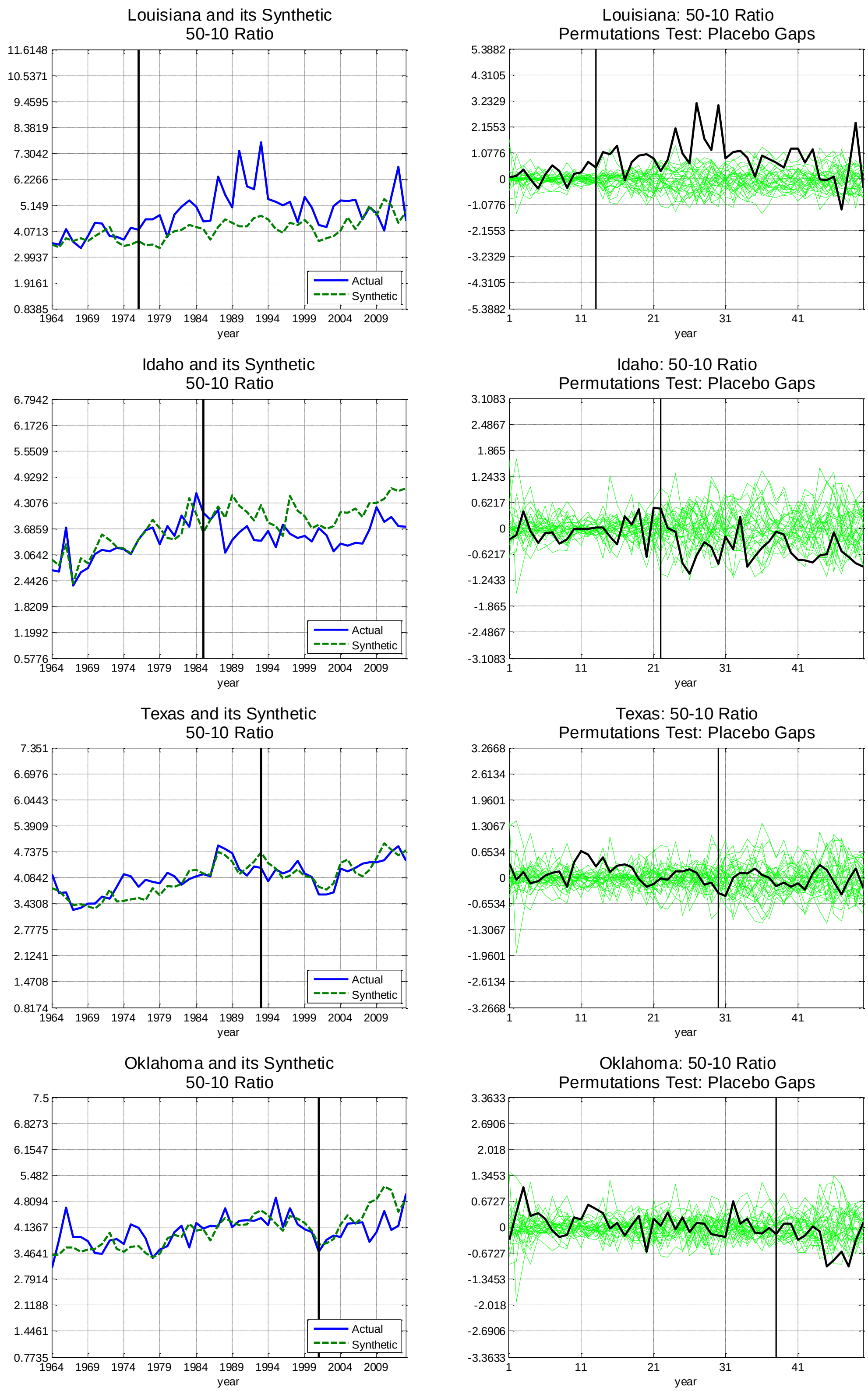
Figures

Figure 1: Synthetic Control Estimates of RTW on Top 1% Income Share



Note: Years of RTW are - Louisiana 1976, Idaho 1985, Texas 1993, Oklahoma 2001.

Figure 2: Synthetic Control Estimates of RTW on 50-10 Ratio



Note: Years of RTW are - Louisiana 1976, Idaho 1985, Texas 1993, Oklahoma 2001.

Tables

Table 1: Summary Statistics

	Donor pool (26 states)				Mean			
	mean	sd	min	max	Louisiana	Idaho	Texas	Oklahoma
Inequality: Gini coefficient (1964-2005)	0.51	0.05	0.41	0.67	0.53	0.53	0.55	0.53
Inequality: Atkinson index (1964-2005)	0.21	0.04	0.12	0.38	0.21	0.20	0.23	0.21
Inequality: Theil's entropy index (1964-2005)	0.55	0.21	0.13	1.41	0.54	0.48	0.64	0.52
Inequality: relative mean deviation (1964-2005)	0.72	0.07	0.56	0.95	0.75	0.74	0.78	0.74
Top one percent income share (1964-2005)	0.11	0.04	0.06	0.28	0.11	0.11	0.13	0.11
Top decile income share (1964-2005)	0.35	0.05	0.27	0.53	0.36	0.34	0.38	0.36
Family income: 90-50 ratio (1964-2013)	2.27	0.29	1.65	3.31	2.59	2.17	2.53	2.38
Family income: 90-10 ratio (1964-2013)	9.01	2.39	3.79	17.00	12.63	7.54	10.50	9.72
Family income: 50-10 ratio (1964-2013)	3.92	0.64	1.71	6.00	4.84	3.47	4.12	4.06
Per capita FDI (2005 $) (1977-2007)	2660.06	1721.12	226.77	12024.66	5548.81	1129.80	3995.15	2194.22
% union member (1964-2013)	20.88	8.04	6.20	44.80	10.97	13.51	8.51	10.97
Population growth rate (census)	10.19	8.26	-8.01	30.93	8.38	18.61	21.52	10.45
Non-white population growth rate (census)	56.14	44.54	-21.63	235.56	11.59	96.98	52.58	39.40
Percent White (includes White-Hispanic)	87.92	8.66	59.55	99.60	67.56	94.75	77.92	83.32
Percent (non-White) Hispanic	2.66	4.05	0.05	21.52	0.71	2.96	8.33	2.41
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
Population age>25 with high school (%)	71.66	13.19	31.96	91.80	62.26	75.14	65.43	69.32
Percent urban population	70.49	15.60	32.13	94.44	68.95	57.98	80.53	67.08
Percent homeowners	66.20	5.62	47.30	75.20	65.6	71.15	63.42	69.10
Percent below poverty	11.66	3.66	6.40	22.90	23.17	12.77	17.43	16.73
Per capita income (2005 dollars)	19067.69	5226.32	10091.81	33124.50	14879.93	15678.40	17415.92	16181.19
State effective minimum wage (current dollars)	4.16	1.91	1.60	9.19	3.95	3.95	3.95	3.95

Notes: (a) Maximum time period is 1964-2013. Number of observations 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 Estimates of the Impact of Right to Work Law on Inequality

	Top 1% income share				50-10 Ratio			
	LA	ID	TX	OK	LA	ID	TX	OK
Panel A: Estimation Statistics								
Abs. prediction error to mean ratio	0.041	0.025	0.059	0.038	0.077	0.069	0.054	0.064
Pre-intervention MSPE	0.000	0.000	0.000	0.000	0.124	0.087	0.071	0.108
Post-intervention gap	0.008	0.006	-0.002	0.020	0.896	-0.523	-0.046	-0.350
Post-pre MSPE ratio	5.511	18.125	1.736	13.754	12.128	4.385	0.685	2.720
P-value: Post-pre MSPE ratio	0.704	0.519	0.926	0.037	0.222	0.259	0.889	0.222
post/pre MSPE ratio rank	20	15	26	2	7	8	25	7
Panel B: Donor Pool w-weights								
California	0.000	0.000	0.104	0.000	0.000	0.000	0.214	0.310
Colorado	0.000	0.000	0.000	0.072	0.000	0.854	0.000	0.000
Connecticut	0.000	0.000	0.000	0.077	0.000	0.000	0.000	0.000
Delaware	0.058	0.000	0.000	0.062	0.000	0.000	0.000	0.118
Illinois	0.000	0.000	0.000	0.000	0.000	0.000	0.318	0.075
Kentucky	0.380	0.000	0.159	0.459	0.532	0.000	0.159	0.403
Missouri	0.000	0.000	0.000	0.000	0.000	0.000	0.309	0.000
Montana	0.000	0.326	0.000	0.001	0.000	0.000	0.000	0.000
New Mexico	0.487	0.134	0.247	0.302	0.000	0.000	0.000	0.000
New York	0.076	0.000	0.490	0.027	0.000	0.000	0.000	0.000
Oregon	0.000	0.540	0.000	0.000	0.269	0.000	0.000	0.000
Rhode Island	0.000	0.000	0.000	0.000	0.000	0.146	0.000	0.000
West Virginia	0.000	0.000	0.000	0.000	0.198	0.000	0.000	0.095

Panel C: List of Predictors

Population growth	Share of retail trade	Per capita income
Growth of non-White population	% population 25+ high school	% union membership
Percent White	Percent urban population	State effective minimum wage
Percent Non-White Hispanic	Percent homeowners	Per capita FDI
Share of agri-forestry-fishing	Percent below poverty	Pre-intervention outcome
Share of manufacturing		

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) Donor pool states with w-weight<0.0001 are: Indiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, New Hampshire, New Jersey, Ohio, Pennsylvania, Vermont, Washington, Wisconsin. (c) Except of the pre-intervention outcome, the set of predictors is the same in each estimate. (d) In case of Louisiana, the intervention predates the availability of FDI data. FDI therefore is not one of the predictors for Louisiana outcomes. (e) According to the absolute prediction errors to mean ratios, the pre-intervention matching is very good for each estimate including those for Louisiana. It has to be noted, however, that for Louisiana there are many more years of post-intervention comparison vis-à-vis 12 years of pre-intervention matching.

Table 3: Synthetic Control Estimates of the Impact of Right to Work Law on Various Inequality Measures

	Gini	Atkinson	Theil	Rel Mean Dev	Top 10% share	90-10 Ratio	90-50 Ratio
Louisiana							
Abs. prediction error to mean ratio	0.016	0.035	0.028	0.013	0.010	0.103	0.069
Pre-intervention MSPE	0.000	0.000	0.000	0.000	0.000	1.178	0.042
Post-intervention gap	0.027	0.015	0.044	0.030	0.017	2.959	0.168
Post-pre MSPE ratio	10.458	4.708	19.305	6.706	22.066	12.060	1.548
P-value: Post-pre MSPE ratio	0.185	0.481	0.370	0.148	0.148	0.333	0.926
post/pre MSPE ratio rank	6	14	11	5	5	10	26
Idaho							
Abs. prediction error to mean ratio	0.017	0.035	0.052	0.022	0.020	0.068	0.043
Pre-intervention MSPE	0.000	0.000	0.001	0.000	0.000	0.543	0.011
Post-intervention gap	-0.001	-0.012	-0.038	-0.001	0.000	-2.087	0.019
Post-pre MSPE ratio	0.107	3.331	7.097	0.098	0.747	10.057	1.614
P-value: Post-pre MSPE ratio	0.963	0.556	0.704	0.926	0.926	0.222	0.889
post/pre MSPE ratio rank	27	16	20	26	26	7	25
Texas							
Abs. prediction error to mean ratio	0.025	0.029	0.057	0.027	0.030	0.083	0.036
Pre-intervention MSPE	0.000	0.000	0.001	0.001	0.000	0.956	0.012
Post-intervention gap	0.011	-0.007	-0.075	0.021	-0.012	-0.196	0.065
Post-pre MSPE ratio	1.002	1.803	5.805	0.935	1.581	1.385	1.130
P-value: Post-pre MSPE ratio	0.815	0.704	0.481	0.667	0.667	0.926	0.926
post/pre MSPE ratio rank	23	20	14	19	19	26	26
Oklahoma							
Abs. prediction error to mean ratio	0.011	0.029	0.053	0.012	0.022	0.074	0.034
Pre-intervention MSPE	0.000	0.000	0.001	0.000	0.000	0.634	0.011
Post-intervention gap	-0.002	-0.004	0.020	0.003	-0.003	-1.331	-0.018
Post-pre MSPE ratio	0.411	0.318	0.494	0.422	0.267	5.250	1.235
P-value: Post-pre MSPE ratio	0.815	0.630	0.778	0.481	0.704	0.111	0.778
post/pre MSPE ratio rank	23	18	22	14	20	4	22

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) Donor pool is the same as that in Table 2. (c) Set of predictors is the same Table 2. (d) In case of Louisiana, the intervention predates the availability of FDI data. FDI therefore is not one of the predictors for Louisiana outcomes.

Table 4: Synthetic Control Estimates of the Impact of Right to Work Law on FDI and Union Membership

	Louisiana	Idaho		Texas		Oklahoma	
	Union membership	PC FDI	Union membership	PC FDI	Union membership	PC FDI	Union membership
Abs. prediction error to mean ratio	0.023	0.033	0.057	0.015	0.215	0.015	0.118
Pre-intervention MSPE	0.264	0.051	2.453	0.030	9.508	0.021	4.514
Post-intervention gap	-2.283	-0.605	-3.453	0.279	-2.295	-0.064	-1.749
Post-pre MSPE ratio	26.539	7.450	5.525	2.965	0.619	0.623	1.045
P-value: Post-pre MSPE ratio	0.185	0.667	0.296	0.630	0.815	0.926	0.519
post/pre MSPE ratio rank	6	19	9	18	23	26	15

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) Donor pool is the same as that in Table 2 with the following modification: When FDI is the outcome variables, FDI is excluded from the common set. FDI as pre-intervention outcome is included. Similarly for union member as the outcome variable. (c) In case of Louisiana, the intervention predates the availability of FDI data. Therefore, there is no SCM for FDI for Louisiana. Also, FDI is not one of the predictors for Louisiana union membership SCM.