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Abstract

There is an ongoing debate about the effect of changes in labor regulations such as Right-to-Work (RTW) laws on rising income inequality in the U.S. In this paper, we use a relatively new methodology, the Synthetic Control Method – which we argue is more suitable for analyzing this data – to examine the impact of a state's adoption of an RTW law on income inequality. We use a wide range of inequality measures for states that enacted their RTW laws between the 1960s and the 2000s. Unlike some earlier papers that suggest a negative link between the RTW laws and correlates of inequality such as wages, we find that RTW laws had no significant impact on income inequality in these states.

JEL Classification: J01, J08, J23, J38, J39, J51, L59
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1. Introduction

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 potential factors (Gordon and Dew-Becker 2008).[1] Our paper contributes to this debate by studying whether labor regulations such as Right-to-Work (RTW) laws are possible contributors to increasing income inequality in the U.S.

RTW statutes remove union membership as a prerequisite 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. In the media and public sphere – from policy beliefs generated by influential organizations and think tanks, articles in print media, reports in screen media to political documentaries – there is widespread belief that RTW laws have contributed to widening income inequality in the U.S. (Manzo and Bruno 2015).[2]

However, remarkably few papers have studied the direct link between RTW laws and income inequality, and none have done so using the Synthetic Control Method (SCM) approach, which offers a distinct advantage over traditional difference-in-difference models given the nature of the data. The existing literature presents some evidence of

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[1] 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%, they 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).

[2] 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 documentary 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).

economically significant impacts of unionization on wages (Nieswiadomy et al. 1991, Western and Rosenfeld 2011).[3] These studies take the negative association between unionization and lower wages as evidence that RTW laws have constrained organized labor and worsened income inequality. 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 (Holmes 1998).[4] Studies of the net impact of RTW laws on inequality, meanwhile, are surprisingly few.[5] This paper is an attempt to address the following question: does adopting an RTW law result in greater income inequality in a state?

Our data covers nearly a 50-year period (1964-2013). This is important since, by most measures, inequality in the United States started to rise in the 1980s (Meyer and

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[3] Freeman (1993) and Card (1992) estimate the union wage premium to be between 10 and 17 percent. Nieswiadomy et al. (1991), find union wages to be 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 non-union 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. On the other hand, 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 as well as state level characteristics – conclude 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.

[4] 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 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.

[5] While we find Nieswiadomy et al. (1991) to be the only study to look at the connection between RTW laws and inequality, a few studies look at the possible connections between inequality and unionization. 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. They instead find the largest contributor to be skill-biased technical change. 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. Reed (2003) differs from the conclusion in Gould and Shierholz (2011). 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.

Sullivan 2013, Frank 2014). Seventeen of the early adopter states instated their RTW laws in the 1940s and the 1950s (Wyoming, the eighteenth adopter, instituted its RTW law in 1963) and these states offered little pre-intervention information for us to use with our methodology. Meanwhile, Indiana, Michigan and Wisconsin passed their laws in 2011 or later and offered little post-intervention information. The four states that we examine – Idaho, Louisiana, Oklahoma and Texas – are the only states that enacted RTW laws over a period of five decades between the 1960s and the 2000s, thus offering a reasonable number of both pre- and post-intervention periods.

We conduct a comparative case study of each of the 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. We also look at some possible pathways through which these laws are commonly perceived to impact inequality, namely, investment, wages and salaries. Our finding of a lack of impact of RTW laws on inequality is further supported by findings of a lack of impact of the law on these variables.

In what follows, Section 2 and Section 3 describe the data and the estimation methodology, respectively. Section 4 reports and discusses the results and Section 5 concludes with the implications of the findings.

2. Data

To ensure that we cover different facets of – and different ways to look at – aggregate inequality, we use a wide range of inequality measures. To the best of our knowledge, only Nieswiadomy et al. (1991) assess the effects of RTW on income inequality;

they, however, use only the Gini coefficient. The reality of rising income inequality in the U.S. is that much of the increase can be explained by the upper end of the distribution (Lowell and Waller 2014). Thus, the Gini coefficient alone may not be sufficient to assess the dynamics of income inequality 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).[6]

From Frank (2009, 2014), we were able to obtain data on traditional measures, namely, the Gini coefficient, the Atkinson index, the relative mean deviation and Theil's entropy index, as well as top 1% income share and top 10% income share.[7] The data provided through Frank (2009, 2014) ends in 2012. From the Current Population Survey (CPS), we obtained household income measures of inequality in the form of the 50-10 Ratio, 90-10 Ratio and 90-50 Ratio. That data ends in 2013. The income shares and the

[6] In Nieswiodomy et al. (1991) the exogenous variable used in the 2SLS estimation is the wage rate; it is not clear if it can satisfy the exclusion restriction. The estimated effects of RTW laws are highly sensitive to model specification (Ellwood and Fine, 1987). 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.

[7] There is a 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. The literature also points to the role of unions in directing redistribution policies itself (Korpi 2006). 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.

household income ratios are widely used measures of inequality and have been used extensively to measure inequality in the U.S. states, for example, in Aghion et al. (2015).

The household income ratios as well as key predictor variables such as union memberships are not available before 1964. 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. Information on these variables is obtained from the Census Bureau, Bureau of Economic Analysis (BEA) and Uniform Crime Reporting (UCR) by FBI. Table 1 provides summary statistics of the outcome variables as well as predictor variables of the four treatment states and the 26 non-RTW states.

3. Estimation

In this section we first detail the advantages of the SCM approach in state-level policy evaluation. We then discuss why it is particularly appropriate to use the SCM approach to estimate the impact of the RTW law on a state's income inequality.

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

In program evaluation, researchers often select comparisons on the basis of subjective measures of similarity between the affected and the unaffected regions or states. SCM provides a comparison (or synthetic) state that is a combination of the control states. A data-driven procedure 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 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., 2014).[8]

Secondly, when aggregate data are employed (as the case is in this paper), uncertainty remains around 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 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.

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 a conventional asymptotic framework. The latter can seriously overstate the 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, 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

[8] 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.

matching on outcomes and characteristics, a synthetic control also matches on time-varying unobservables.[9]

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).

We present a more formal description of the Synthetic Control Method of Abadie et al. (2010, 2014) in the Appendix.

3.2. Appropriateness of SCM in Estimating the Impact of the RTW law on State's Inequality

In terms of the timing of adoption of the laws, while almost half the states in the U.S. currently have RTW laws, within the 50 year period between the 1960s and the 2000s, only 4 states (the ones we study) 'switched' from non-RTW status to RTW status. As a result, even though one can have a 50-year long panel for all U.S. states, the fact that only 4 states switched to RTW underscores the choice of SCM as the preferred method for assessing the impacts of the RTW laws.

With so few treatment units – as discussed in section 3.1 above – 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

[9] 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."

al., 2011, Bohn et al., 2014). If instead, a state-level difference-in-difference (DID) regression analysis were chosen, it would almost tantamount to a cross-section analysis since very few units would have treatment variation over time.

One of the important contributions of this paper is that by estimating RTW's impacts in each state individually, we accommodate for possible treatment heterogeneities. 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 Louisiana almost two decades before the passage of the North American Free Trade Agreement (NAFTA); Texas passed the law at about the same time as NAFTA was enacted; and Oklahoma introduced a RTW law a little less than a decade after NAFTA. 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.

Reflecting on another source of heterogeneity across states, Canak and Miller (1990) show that the composition of business support for RTW laws varied across states and over time. The variation in business support is important from the perspective of how businesses react to RTW in terms of bringing in investment and generating employment.

In a program evaluation context, one of the more serious issues is finding appropriate comparison or control states that can provide a reliable counterfactual for the treatment (or RTW) states. Not every non-RTW state would be a suitable candidate for a comparison unit for a treatment state. For instance, RTW states are often lower income states (Reed 2003). It is also unlikely that we can find a single non-RTW state that would have characteristics such as the size of labor force, industry makeup, taxation policies, and numerous other state-specific factors similar to those of a treatment state.

Under these circumstances, 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 and over a long period of time generates the weights. Our set of control units, or donor pool, consists of the 26 non-RTW states. We use an extensive set of predictor variables, as described in section 2, to obtain pre-intervention matching.

3.3. Implementing SCM

In the Appendix we describe some of the details of the process to obtain the optimal weights, $\mathbf{W}^*$. These weights are applied to calculate the weighted average of the donor pool, which is the synthetic control of a treatment unit. The post-intervention values of the synthetic serve as our counterfactual outcome for the treatment unit. 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 the 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 of 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 two more criteria. 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. And, finally, Abadie et al. (2010) produce a statistic that is obtained by dividing the rank of the post-pre MSPE ratio by one plus the size of the donor pool; this is the probability of obtaining a post-pre MSPE ratio as large as the treated if one were to assign the intervention at random in the data. We call this statistic 'donor probability' and report it for each estimate.

4. Results

We start with our main results where, using the donor pool that includes all 26 non-RTW states, we perform the SCM analysis with the main set of predictor variables listed in Table 1. Subsequently, as robustness checks, we conduct additional tests with different predictors and different donor pools (Tables 6-8). We also examine the impact of RTW on average 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).

We use two 'representative' measures of inequality – the top 1% income share and the 50-10 ratio – to describe the details of the results (Tables 2 and 3). The pictorial representations of the results detailed in Table 2 are presented in Figures 1-5. The SCM estimates of the remaining 7 inequality measures are presented in Table 4. Every specification and robustness check has been conducted, and reported, for all 9 measures (Tables 6-8).

4.1. The Main Results

In our main estimates we carry out SCM estimates where we include in the set of predictors the variable that can primarily be perceived to be directly related to incomes and redistributions. For example, Piketty et al. (2014) highlight factors such as tax rates as contributors to inequality that vary across states. We include in our main set of predictors the following variables: per capita income, measure of unemployment, effective minimum wage, poverty, medical benefits, state unemployment insurance compensation, supplemental nutrition assistance program (SNAP), taxes paid to state governments, current transfer receipts from federal, state and governments as well as businesses, and employer contributions to employee pension, among others.

In Figures 1-4, the left panels show the pre-intervention match and the post-intervention deviation between the synthetic and the actual. The right panels present the permutations/randomization tests where the post-intervention gap for the treatment state is the dark line whereas its placebo counterparts are the light lines. 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 the 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).

Table 2 reports the SCM estimates where, in panel A, we present the pre-intervention absolute prediction error to mean ratio (APEMR) and mean square prediction error (MSPE) show good pre-intervention fits.[10] Panel A also includes the statistical results of the permutations or randomization tests (p-value and rank of the post-pre MSPE ratio as well as 'donor probability'). As discussed in details in second 3.3, if the post-pre MSPE ratio for the exposed state is ranked first, then the treatment effect is significant (Abadie et al. 2010). The p-value represents another way to indicate statistical significance of the post-pre MSPE ratio. And finally, the donor probability is the probability of obtaining a post-pre MSPE ratio as large as the treated if one were to assign the intervention at random in the data.

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 and the 'donor probability' is high for each estimate. In the case of Oklahoma the rank is 2, the p-value is significant at 5% level, and the donor probability is a relatively low 7%. However, as we see in Tables 6-8, this marginally significant effect of RTW on the top 1% income share in Oklahoma is not

[10] From APEMR, for instance, we see that, across-the-board, the pre-intervention prediction error remains smaller than one tenth of the mean. The pre-intervention MSPE values are also small when we compare them to the variance of the respective outcome variable.

robust. Furthermore, as we see in the rest of Table 2 and Tables 6-8, RTW does not have even a marginally significant effect on any of the other 8 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 that order) are the biggest contributors in the construction of the synthetic control for Louisiana's Top 1% income share. Similarly, Kentucky, California, Minnesota, Delaware and Illinois (in that order) contributed the most in the construction of the synthetic control for Oklahoma's 50-10 ratio.

These weights, however, are more meaningful if we examine Table 3, which presents the pre-intervention characteristics matches between the actual and the synthetic outcomes. We find the characteristics matching between each synthetic and the actual to be very similar. Importantly, in terms of the crucial variable of per capita income (Reed 2003), for instance, we find a very close match between the actual and the synthetic outcomes for each state.

Table 4 presents the SCM estimates of the impacts of RTW on the rest of the seven measures of inequality.[11] In none of these seven measures for any of the four states do we obtain a significant impact of the RTW law: rank statistics are all greater than 1, no p-value is significant and all donor probabilities are large.

4.2. The 'Dosage' Test

Provided that the data permits, one useful way to verify the SCM results is to conduct a so-called 'dosage' test (Abadie et al. 2014, Mideksa 2013). The dosage test in this

[11] 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.

context would be to juxtapose the SCM estimate against the unionization rate of the treated unit. If we find certain systematic patterns of movement over time between the unionization rate and the gap between actual and synthetic outcomes (for instance, if we find that the rate of unionization and the gap are parallel during pre-intervention but post-intervention, as unionization rate declines, the gap increases), then our finding of a lack of impact of RTW on inequality is not supported by the dosage test.

Figure 5 presents the dosage tests for the top 1% income share and the 50-10 ratios (the rest of the pictures are available upon request). In the top right picture, for instance, we have the case of Louisiana where the line marked unionization is the rate of unionization in Louisiana, and the other line is the gap between the 50-10 ratio in the actual and the synthetic outcomes for Louisiana.[12] The common observation in Figure 5 is that the declining unionization post-intervention is not matched by an increasing (or decreasing) gap between actual and synthetic outcomes. In fact, overall, the gap does not seem to have any specific pattern of movement vis-à-vis unionization over time. The case of top 1% income share in Oklahoma shows a slight post-intervention uptick; this corresponds to the marginal significance of that particular measure, which is the only one out of the 36 measures of inequality that is marginally significant.

In this study we have emphasized the importance of individual case studies. To further this argument, we mention a few state specific factors that indicate, perhaps, the economic realities were simply not conducive for the RTW laws to have an impact on inequality. For instance, the manufacturing boom in Idaho post-RTW implementation was

[12] The case of Idaho is particularly interesting where the unionization rate fell from 23.1% in 1981 to 12.2% in 1985, 4years before RTW enactment (Collins 2014). This dramatic fall in unionization there 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.

driven by the high-tech industry which did not have significant unionization (Lafer and Allegretto 2011). In Oklahoma, employment is concentrated in oil and gas, government, and military services; the latter is unaffected by RTW (Lafer and Allegretto 2011).

4.3. The Possible Pathways

Investments and wages-salaries are the most talked about pathways through which an RTW law can, in principle, impact inequality. Table 5 presents the SCM estimates of the impact of the RTW law on average wages and salaries and per capita foreign direct investments (FDI). We find no significant impact on either of these variables: all rank statistics are greater than 1, and both the p-values and the donor probabilities are very large. These findings of no effects on possible pathways for affecting inequality essentially corroborate the findings of no significant effect of RTW on the inequality measures in each state.

4.4. Robustness

In this section we carry out a number of robustness checks by perturbing the set of predictors as well as the donor pool.

4.4.1. The Issue of Changes in Pre-intervention Unionization

It has been argued that the adoption of RTW legislation in a state may reflect its citizens' preference regarding unionization (Lumsden and Petersen 1975, Farber 1984). There has been an across-the-board decline in unionization over the last half-a-century.[13] It follows that the states that adopted RTW laws may have a preference for lower unionization exhibiting in faster than average pre-RTW decline in unionization. To account

[13] Various explanations have 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).

for this, we carry out a robustness check where we add pre-intervention rate of change in unionization to the set of predictors. This allows us to construct a synthetic that did not have RTW but exhibited movements in unionization similar to those in the treated state. The results are presented in Table 6. We find good pre-intervention fit but none of the post-pre MSPE ranks are even close to 1. In other words, findings are robust to this perturbation.

4.4.2. The Issue of Border States

It is possible that there are spillover effects in the preferences of the citizens and policies of the governments among neighboring states. There might also be some labor market linkages among these 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 SCM analyses with donor pools where neighboring states to the treatment states are expunged from the respective donor pool. Table 7 reports these results. Note that all the bordering states of Louisiana are already excluded in the main donor pool. Hence, this robustness test does not include Louisiana. Similar to the main estimates in Tables 2 and 4, we have excellent pre-intervention fits. In all measures for the three states, the estimates reinforce those of the main specification.

4.4.3. A Completely Different Set of Predictors

In our main specifications through Tables 2-7, we have used variables that are perceived to be related to incomes and redistributions. As a robustness check we re-run all the estimates using a completely different set of predictors that includes primarily demographic variables such as the total population growth, the non-White population growth, the proportion of adult population with high school, and the proportion of adult population with college. We have also added percent of rural population and crime rates. And finally, we have added average wages and salaries and per capita FDI, the presumed 'channels' through which the law is supposed to impact inequality. The results are reported in Table 8. We observe that even with this alternative set of predictors, while we get a good pre-intervention fit we do not find a significant impact of the RTW laws on any of the 9 inequality measures for any of the treatment states.

4.5 Discussion

The estimates of the union wage premium in Freeman (1993) and Card (1992) in the 10-17% range in the 1970s and the 1980s, the lack of impact of RTW on wages in Moore (1998), the 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. In contrast, we cover the entire time period of 5 decades between the 1960s and the 2000s.

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.

Last but not least, it is possible that for RTW to bear on inequality, the required threshold of unionization rate needs to be much higher than what we observe across U.S. states. Since the 1960s, the U.S. ranks last among the 21 top developed nations in both unionization rates and union coverage of the workforce (Visser 2006). Besides, while private sector unionization in the U.S. has fallen steadily, unionization among public sector workers has remained stable.

5. 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.

As more states adopt or consider Right-to-Work laws, there is an ongoing debate whether these laws are contributing to rising income inequality in the U.S. We adopted the Synthetic Control Method (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 their state's income inequality. 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 2010. Most of the RTW states implemented RTW laws in the 1940s or the 1950s. However, inequality in the U.S. started to exacerbate in the mid-1980s (Frank 2014). If RTW had an impact 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 do not seem to be the answer. 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 disparities in relative factor returns, and aspects of the labor market beyond collective bargaining.

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Appendix

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 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 $T_0 \in (1, T)$. For states $i = 1, ..., J+1$ and periods $t = 1, ..., T$, suppose state i is exposed to the intervention (RTW) at $T_0 \in (1, T)$. The observed outcome for state i at time t is,

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

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. We restrict the donor pool to states that did not enact an RTW law.

Indexing the exposed state as state 1, we want to estimate $(\alpha_{1T_0+1}, ..., \alpha_{1T})$. From equation (1) we note that $\alpha_{1t} = Y_{1t} - Y_{1t}^N$ for $t \in \{T_0 + 1, ..., T\}$, and while Y_{1t} is observed Y_{1t}^N is unobserved. Suppose Y_{it}^N is given by the model, $Y_{it}^N = \delta_t + \boldsymbol{\theta}_t \mathbf{Z}_t + \boldsymbol{\lambda}_t \boldsymbol{\mu}_i + \varepsilon_{it}$, where, δ_t is an unknown common factor constant across states, $\mathbf{Z}_j$ 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 $\mathbf{W} = (w_2, ..., w_{J+1})'$ such that $\{w_j \geq 0 \mid j = 2, ..., J+1\}$ 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 $\mathbf{W}^* = (w_2^*, ..., w_{J+1}^*)'$ such that, $Y_{1t}^N = \sum_{j=2}^{J+1} w_j^* Y_{jt}$, $t = 1, ..., T_0$, 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,

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

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

It is important to note, as Abadie, Diamond and Hainmueller (2010) show, the model for Y_{1t}^N above is a generalization and that the traditional regression-based difference-in-difference model can be obtained if we impose that $\boldsymbol{\lambda}_t$ 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 allows the effects of such unobservables to vary with time. In particular, Abadie, Diamond and Hainmueller (2010) show that a synthetic control can fit $\mathbf{Z_1}$ and a long set of pre-intervention outcomes, $Y_{11}, ..., Y_{1T_0}$, only as long as it fits $\mathbf{Z_1}$ and μ_1 (unknown factors of the exposed unit).

Figures

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

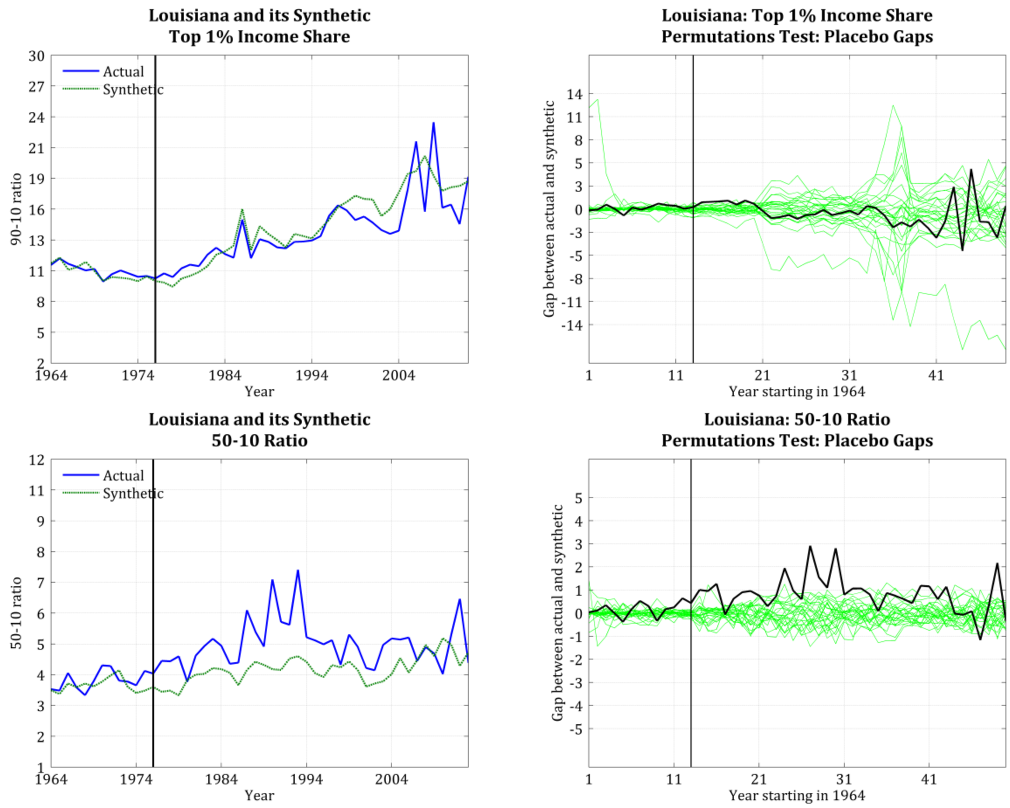


Figure 2: Synthetic Control Method Estimates of the Right-to-work Law in Idaho

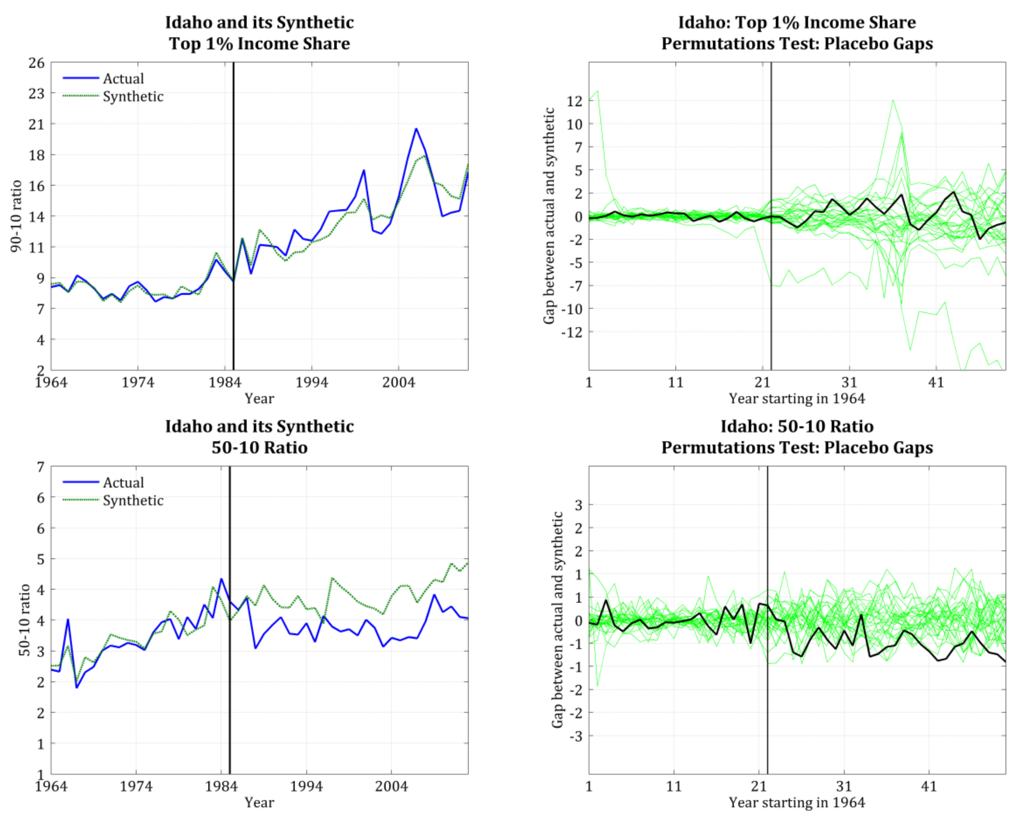


Figure 3: Synthetic Control Method Estimates of the Right-to-Work Law in Texas

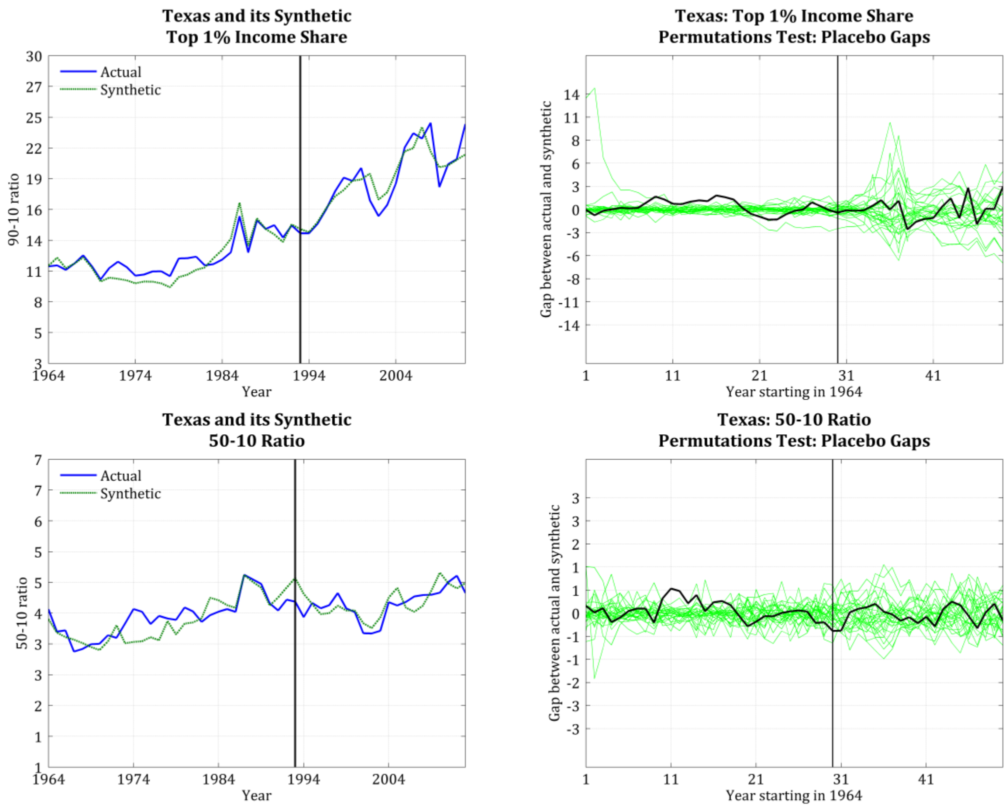


Figure 4: Synthetic Control Method Estimates of the Right-to-Work Law in Oklahoma

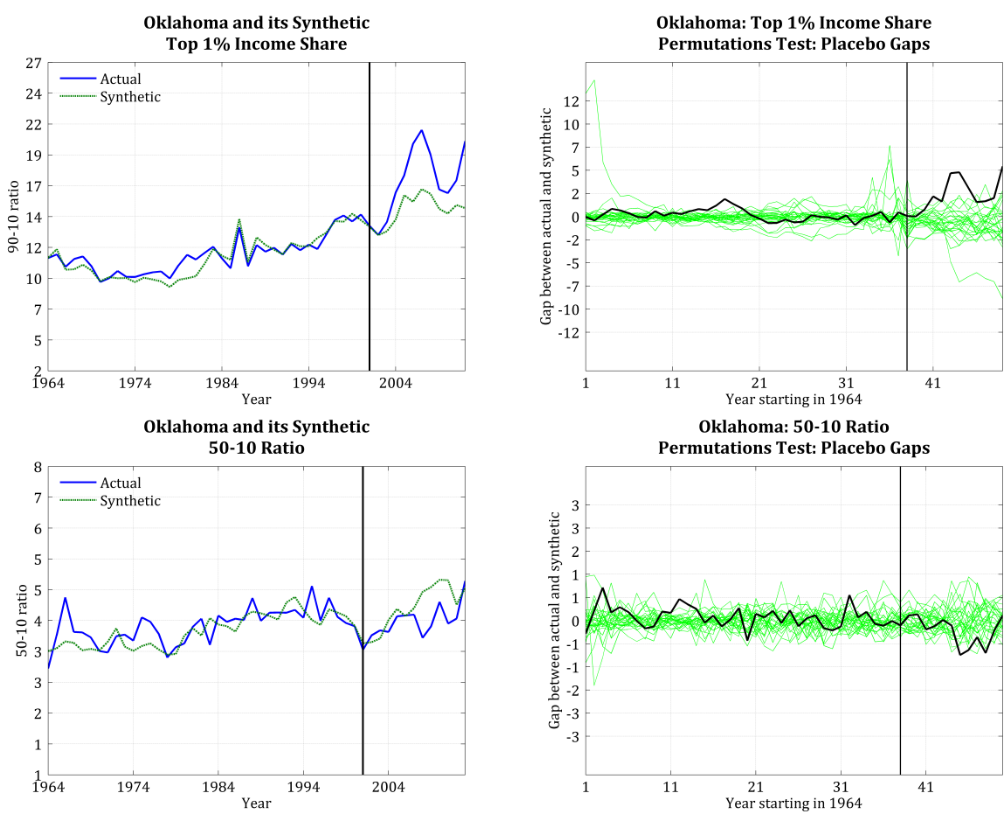
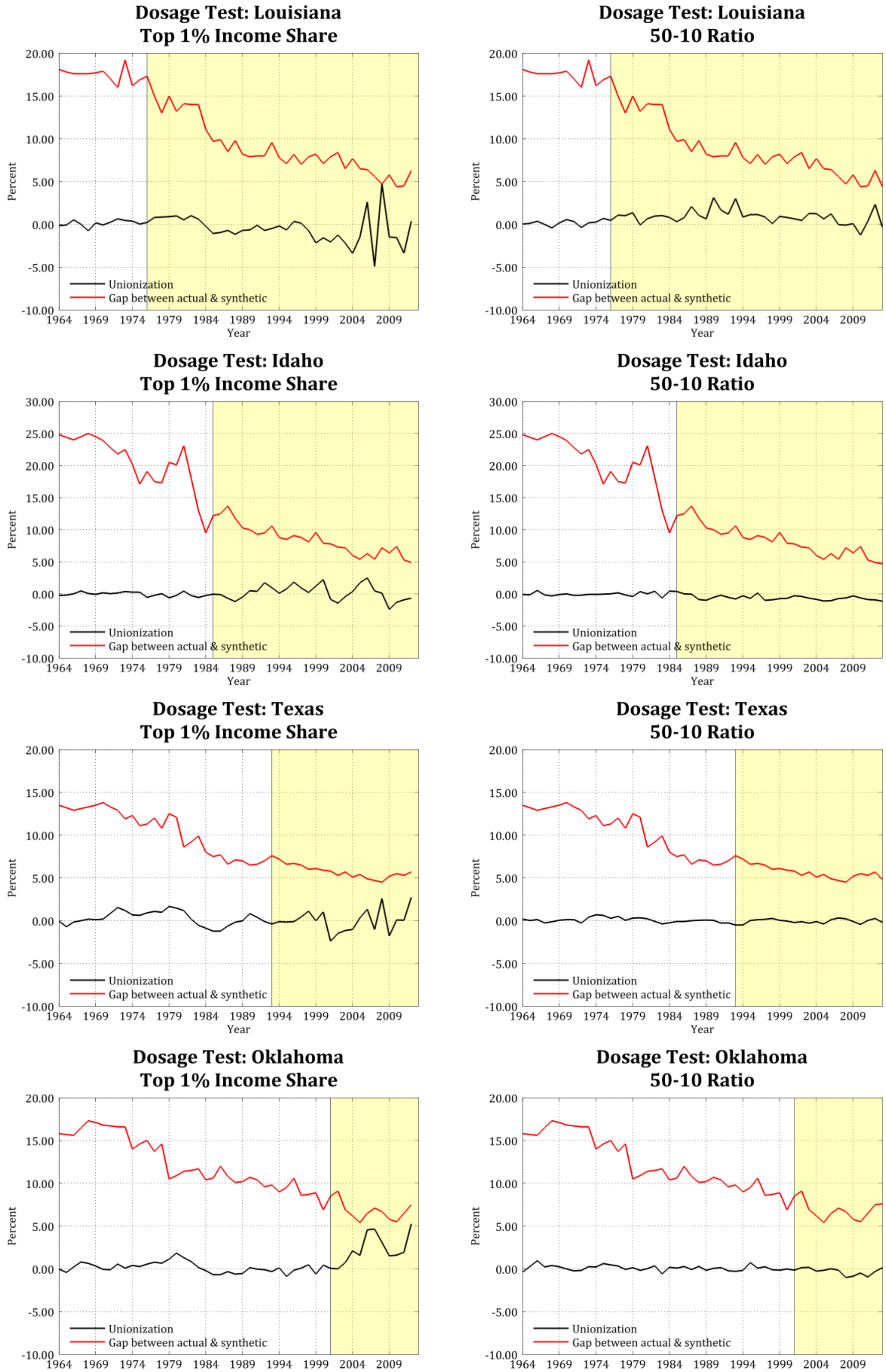


Figure 5: Dosage Test - Impact of RTW Laws on Inequality Measures: Top 1% Income Share and 50-10 Ratio



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
Unionization rate (1964-2013)	20.88	8.04	6.20	44.80	10.97	13.51	8.51	10.97
Main set of predictors								
Log per capita income (2005 dollars)	9.82	0.27	9.22	10.41	9.58	9.64	9.74	9.67
Employment to population ratio	0.54	0.07	0.37	0.68	0.48	0.53	0.53	0.52
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
Current transfer receipts from governments	8.06	0.58	6.48	9.10	7.99	7.86	7.79	8.03
Medical benefits	6.74	1.14	0.96	8.44	6.70	6.37	6.48	6.72
State unemployment insurance compensation	4.84	0.68	2.66	6.74	4.38	4.78	4.12	4.12
Supplemental Nutrition Assistance Program (SNAP)	3.90	1.33	0.00	5.73	4.70	3.56	4.00	3.68
Receipts from state and local governments	2.50	1.09	0.00	3.48	2.50	2.50	2.50	2.50
Current transfer receipts from businesses	4.16	0.47	2.97	5.00	4.15	4.13	4.15	4.13
Personal current taxes to State governments	6.17	0.98	3.12	7.66	5.52	6.38	3.88	6.13
Employer contributions employee pension, etc.	7.57	0.54	6.08	8.51	7.38	7.28	7.47	7.37
Alternative set of predictors								
Growth rate: population	0.10	0.08	-0.08	0.31	0.08	0.19	0.22	0.10
Growth rate: Non-White population	0.56	0.45	-0.22	2.36	0.12	0.97	0.53	0.39
Proportion population 25 plus with high school	0.72	0.13	0.32	0.92	0.62	0.75	0.65	0.69
Proportion population 25 plus with college	0.19	0.07	0.06	0.39	0.15	0.17	0.18	0.16
Average wage and salary growth	1.23	0.22	0.97	1.88	1.26	1.20	1.34	1.24
Percent rural population	0.29	0.16	0.06	0.68	0.31	0.42	0.19	0.33
Log overall crime rate per 100,000	8.27	0.37	6.71	8.97	8.43	8.12	8.54	8.36
PC FDI growth	4.46	2.96	0.80	19.10	2.66	5.63	2.95	2.99

Notes: (a) Maximum time period is 1964-2013. Number 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. (d) Unionization rate refers to % non-agri w-s employees members of collective bargaining. All monetary variables are in real per capita terms. (e) In order to preserved maximum number of observations, wage-salary and FDI growths are calculated as ratio to the first observed year.

Table 2: Synthetic Control Method Estimates of the Impact of Right to Work Laws on Two Inequality Measures

	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	0.07	0.06	0.06
Pre-intervention MSPE	0.15	0.10	0.71	0.38	0.12	0.08	0.08	0.11
Post-intervention gap	-0.55	0.22	0.03	2.46	0.90	-0.62	-0.04	-0.30
post/pre MSPE ratio rank	20	20	25	2	8	7	25	7
P-value: Post-pre MSPE ratio	0.70	0.70	0.89	0.04	0.26	0.22	0.89	0.22
Donor probability	0.74	0.74	0.93	0.07	0.30	0.26	0.93	0.26
Panel B: Donor Pool w-weights								
California	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.23
Colorado	0.00	0.00	0.00	0.00	0.00	0.62	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	0.08	0.00	0.01	0.00	0.10
Illinois	0.00	0.00	0.00	0.00	0.00	0.00	0.39	0.08
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	0.20	0.44
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	0.15
Missouri	0.00	0.00	0.00	0.26	0.00	0.00	0.35	0.00
Montana	0.00	0.74	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	0.05	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	0.00	0.00	0.00	0.00	0.00
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	0.37	0.00	0.00
Vermont	0.00	0.26	0.00	0.00	0.00	0.00	0.00	0.00
Washington	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
West Virginia	0.42	0.00	0.56	0.65	0.17	0.00	0.00	0.00
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. 'Donor probability' is the probability of obtaining a post-pre MSPE ratio as large as the treated if one were to assign the intervention at random in the data. (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		
	Synthetic			Synthetic			Synthetic			Synthetic		
	top 1% share	50-10 ratio	Actual	top 1% share	50-10 ratio	Actual	top 1% share	50-10 ratio	Actual	top 1% share	50-10 ratio	Actual
Per capita income	9.35	9.33	9.22	9.49	9.63	9.46	9.52	9.59	9.55	9.45	9.56	9.51
Employed to population ratio	0.42	0.43	0.40	0.48	0.51	0.47	0.43	0.48	0.49	0.43	0.47	0.48
Effective minimum wage	1.63	1.60	1.60	2.30	2.30	2.30	2.32	2.30	2.30	2.30	2.31	2.30
Proportion poverty	0.20	0.20	0.27	0.13	0.11	0.13	0.17	0.14	0.18	0.18	0.16	0.17
Transfers from government	7.37	7.28	7.09	7.48	7.48	7.34	7.73	7.50	7.21	7.61	7.51	7.52
Medical benefits	5.12	4.95	4.91	5.64	5.75	5.35	5.80	5.61	5.38	5.39	5.67	5.81
State unemployment insurance	4.13	4.07	4.01	4.51	4.35	4.55	4.66	4.45	3.51	4.43	4.55	3.88
SNAP	2.98	3.02	3.31	2.99	3.17	2.50	3.55	3.21	2.86	3.65	3.30	2.16
Receipts from state-local govt.	0.73	0.73	0.73	1.75	1.74	1.74	1.75	1.75	1.74	1.75	1.74	1.74
Transfers from businesses	3.76	3.71	3.70	3.92	3.97	3.92	4.00	4.01	3.96	3.97	3.97	3.92
State personal tax	5.50	5.51	4.62	5.96	5.67	5.96	5.92	5.45	3.78	5.57	5.86	5.47
Employer benefit contribution	6.75	6.69	6.60	6.77	7.12	6.78	6.97	7.01	7.02	6.85	7.03	6.92

Note: The underlying estimates are reported in Table 2.

Table 4: Synthetic Control Method of the Impact of 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-intervention gap	0.03	0.02	0.10	0.04	-0.51	2.96	0.16
Post-pre MSPE ratio rank	7	7	5	5	18	12	26
P-value: Post-pre MSPE ratio	0.22	0.22	0.15	0.15	0.63	0.41	0.93
Donor probability	0.26	0.26	0.19	0.19	0.67	0.44	0.96
Idaho							
Pre-intervention APEMR	0.02	0.03	0.04	0.02	0.03	0.07	0.04
Pre-intervention MSPE	0.00	0.00	0.00	0.00	1.46	0.45	0.01
Post-intervention gap	0.00	0.00	0.03	0.00	-0.95	-2.40	0.04
Post-pre MSPE ratio rank	23	25	13	25	27	5	24
P-value: Post-pre MSPE ratio	0.81	0.89	0.44	0.89	0.96	0.15	0.85
Donor probability	0.85	0.93	0.48	0.93	1.00	0.19	0.89
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	0.92	0.01
Post-intervention gap	0.01	0.00	-0.10	0.03	0.33	0.10	0.05
Post-pre MSPE ratio rank	23	25	15	15	17	26	25
P-value: Post-pre MSPE ratio	0.81	0.89	0.52	0.52	0.59	0.93	0.89
Donor probability	0.85	0.93	0.56	0.56	0.63	0.96	0.93
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	2.67	0.63	0.01
Post-intervention gap	0.00	0.01	0.09	0.01	-0.71	-1.30	-0.02
Post-pre MSPE ratio rank	24	13	10	8	26	4	21
P-value: Post-pre MSPE ratio	0.85	0.44	0.33	0.26	0.93	0.11	0.74
Donor probability	0.89	0.48	0.37	0.30	0.96	0.15	0.78

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. 'Donor probability' is the probability of obtaining a post-pre MSPE ratio as large as the treated if one were to assign the intervention at random in the data. (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 Laws on Per capita FDI and Average Wages and Salaries

	Average wage and salary growth				PC FDI growth		
	Louisiana	Idaho	Texas	Oklahoma	Idaho	Texas	Oklahoma
Pre-intervention APEMR	0.00	0.01	0.03	0.03	0.11	0.03	0.09
Pre-intervention MSPE	0.00	0.00	0.00	0.00	0.17	0.01	0.12
Post-intervention gap	0.09	0.10	-0.01	0.02	1.81	-0.82	0.67
Post-pre MSPE ratio rank	7	7	27	24	25	10	18
P-value: Post-pre MSPE ratio	0.22	0.22	0.96	0.85	0.89	0.33	0.63
Donor probability	0.26	0.26	1.00	0.89	0.93	0.37	0.67

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 Right to Work Laws on Various Inequality Measures (Matching on Pre-intervention Unionization)

	Louisiana		Idaho		Texas		Oklahoma	
	APEMR	Rank	APEMR	Rank	APEMR	Rank	APEMR	Rank
Gini	0.01	5	0.02	23	0.02	22	0.01	24
Atkinson	0.03	10	0.03	26	0.03	16	0.03	17
Theil	0.02	6	0.04	13	0.06	15	0.05	9
Rel mean dev	0.01	5	0.02	24	0.03	13	0.01	7
Top 1% share	0.03	20	0.03	21	0.06	25	0.04	4
Top 10% share	0.01	19	0.03	26	0.02	17	0.04	27
50-10 Ratio	0.08	8	0.07	7	0.05	25	0.06	6
90-10 Ratio	0.10	10	0.07	5	0.08	26	0.07	5
90-50 Ratio	0.07	26	0.04	26	0.04	26	0.03	21

Notes: (a) Pre-intervention change in unionization is added to the set of predictors used in the main estimates in Table 2. This is a rate of change measured by dividing each year's value by the value of the first year observed (see Munasib and Rickman 2015). (b) Donor pool is the same as that in Table 2. (c) Pre-intervention periods: Louisiana (1964-1975), Idaho (1964-1984), Texas (1964-1992), Oklahoma (1964-2000). (d) APEMR refers to absolute prediction error to mean ratio. The 'Rank' refers to Post-pre MSPE ratio rank where MSPE = mean square prediction error. Rel mean dev refers to relative mean deviation.

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

	Idaho		Texas		Oklahoma	
	APEMR	Rank	APEMR	Rank	APEMR	Rank
Gini	0.03	22	0.03	24	0.01	16
Atkinson	0.03	21	0.03	19	0.03	7
Theil	0.04	10	0.06	19	0.05	9
Rel mean dev	0.03	22	0.03	20	0.01	15
Top 1% share	0.07	20	0.06	23	0.04	3
Top 10% share	0.03	21	0.02	14	0.05	21
50-10 Ratio	0.06	7	0.06	22	0.06	7
90-10 Ratio	0.07	5	0.08	24	0.07	6
90-50 Ratio	0.04	22	0.05	25	0.04	23

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). (d) Set of predictors is the same Table 2. (d) APEMR refers to absolute prediction error to mean ratio. The 'Rank' refers to Post-pre MSPE ratio rank where MSPE = mean square prediction error. Rel mean dev refers to relative mean deviation.

Table 8: SCM of the Impact of the Right to Work Laws on Various Inequality Measures (Alternative Set of Predictors)

	Louisiana		Idaho		Texas		Oklahoma	
	APEMR	Rank	APEMR	Rank	APEMR	Rank	APEMR	Rank
Gini	0.01	7	0.02	25	0.02	21	0.01	23
Atkinson	0.03	14	0.03	24	0.03	10	0.03	16
Theil	0.03	12	0.04	24	0.05	20	0.05	7
Rel mean dev	0.01	9	0.02	25	0.03	14	0.01	10
Top 1% share	0.03	20	0.03	19	0.06	25	0.04	4
Top 10% share	0.01	19	0.03	26	0.02	17	0.04	27
50-10 Ratio	0.08	9	0.07	13	0.05	25	0.06	7
90-10 Ratio	0.10	11	0.07	6	0.08	27	0.07	4
90-50 Ratio	0.07	25	0.04	25	0.04	27	0.03	21

List of predictors

Population growth rate: population, Non-White population growth rate, proportion population 25 plus with high school, proportion population 25 plus with college, percent rural population, log overall crime rate per 100,000, average wage and salary, per capita FDI.

Notes: (a) Pre-intervention periods: Idaho (1964-1984), Texas (1964-1992), Oklahoma (1964-2000). (b) APEMR refers to absolute prediction error to mean ratio. The 'Rank' refers to Post-pre MSPE ratio rank where MSPE = mean square prediction error. Rel mean dev refers to relative mean deviation. (c) (c) Donor pool is the same as that in Table 2.