

Right-to-Work Laws and State-Level Economic Outcomes: Evidence from the Case Studies of Idaho and Oklahoma Using Synthetic Control Method

*Ozkan Eren
University of Nevada, Las Vegas

†I. Serkan Ozbeklik
Claremont McKenna College

February 2011

Abstract

The role of right-to-work laws on state economies, labor organizations and employees are controversial and important policy questions. Empirical evidence is far from being conclusive predominantly due to identification issues. Using a recently developed econometric technique and exploiting the two most recent cases, -Idaho and Oklahoma- we examine the effectiveness of right-to-work laws on state-level outcomes. Our results indicate that the passage of right-to-work laws in Oklahoma affected union membership and coverage rates and, possibly to some extent, foreign direct investment. As for manufacturing employment, per capita income and average wage rates, we do not observe any impact. Our findings for Idaho, on the other hand, suggest that the laws increased the manufacturing employment, while it had no effect on per capita income and are inconclusive for foreign direct investment.

JEL: J51, J58, J21

Keywords: Right-to-Work Laws, Synthetic Control Method, Unionization, FDI, Manufacturing Employment, and Private Wages

* Assistant Professor of Economics, Department of Economics, College of Business, University of Nevada, Las Vegas, 4505 Maryland Parkway, Las Vegas, NV 89154-6005, U.S.A. Tel: 1-702-895-3653. Fax: 1-702-895-1354]
E-mail: ozkan.eren@unlv.edu.

† Corresponding Author; Assistant Professor of Economics, the Robert Day School of Economics and Finance, Claremont McKenna College, Claremont, CA 91711. Tel: 909-607-0721. Fax: 909-621-8249. E-mail: serkan.ozbeklik@cmc.edu; Visiting Assistant Professor of Economics, University of Maryland, College Park, MD, 20742 Tel: 301-405-3266. Fax: 301-405-3542.

1 Introduction

A March 3, 2008 editorial in *Wall Street Journal*, citing a study by the National Institute for Labor Relations Research, compared the economic performances of Texas and Ohio.¹ The article stated that “in the previous decade while Texas added 1,615,000 new jobs Ohio lost 10,400 jobs” and attributes the right-to-work (RTW) laws in Texas as one of the primary cause of the economic performance gap of these two states.² Another Op-Ed in the same newspaper in December 2007 had also suggested the RTW laws as one of the most crucial policy variables in attracting jobs and capital investments.³ The RTW laws the articles refer to are state statutes which prohibit labor unions and employers to enter into contracts that require all employees to be fee-paying members of a union. Thus, the enactment of these laws rules out union membership to be a prerequisite for employment within the firms. For instance, the Arizona’s RTW law reads “Right-to-work or employment without membership in labor organization: No person shall be denied the opportunity to obtain or retain employment because of non-membership in a labor organization, nor shall the State or any subdivision thereof, or any corporation, individual or association of any kind enter into any agreement, written or oral, which excludes any person from employment or continuation of employment because of non-membership in a labor organization (Article XXV, State of Arizona Constitution).”

The effects of RTW laws on states, labor organizations and employees are controversial policy issues. The opponents of the law, in particular the unions, assert that RTW laws lead to lower wages for both union and non-union workers, lower safety and health standards that protect workers on the job, and attract receiving union representation without incurring any cost for it. Therefore, by weakening the unions and the collective bargaining, the opponents claim these laws to be harmful for job security protection that comes via a union contract. The advocates, on the other hand, contend that these laws create jobs by attracting businesses, lead to higher wages, fewer work stoppages, improve union accountability and are morally right because they prevent individuals from having to support a cause in which they do not believe.

¹ “Texas vs. Ohio” *Wall Street Journal*, March 3, 2008.

² The others are the NAFTA and the absence of state income tax in Texas.

³ Lafer, A. and S. Moore. “The (Tax) War between the States” *Wall Street Journal*, December 10, 2007.

From a research perspective, the lack of consensus is predominantly driven by the difficulty of distinguishing the effects of RTW laws from state characteristics, as well as other state policies that are unrelated with these laws. Even in the absence of any causality, it is plausible to obtain significant correlations between RTW laws and state-level outcomes due to systematic differences across states. For instance, it is a well known fact that southern states have more hostile attitudes towards unions and thereby have lower unionization rates. Among other factors, it is also these attitudes that led to the passage of anti-union statutes such as RTW laws and the omission of these attitudes from the analysis may generate a spurious estimate of these laws on unionization rates. The inability to fully control for the systematic observed and unobserved differences across states may provide contaminated estimates and precludes making firm conclusions pertaining to the effects of RTW laws. Moreover, as pointed in Holmes (1998), the RTW laws proxies a pro-business environment. That is, RTW states are also the states that are known to have business friendly policies such as higher subsidies for new factories, low taxes on capital and weaker environmental/safety regulations. This proxy nature makes it almost impossible to identify the isolated impact of the RTW laws on some state level economic outcomes (for example, foreign direct investment). Finally, the timing of the RTW laws enactment throughout the United States augments the difficulty for identification. After the first wave of the implementation in 1940s and 1950s, there were a few numbers of states adopting the RTW laws with long intervals between each instance.

The purpose of this paper is to evaluate the economic effects of the RTW laws at the aggregate level by trying to address the aforementioned problems. To this end, we use the synthetic control method that was initially introduced in Abadie and Gardeazabal (2003) and then later extended in Abadie et al. (2010). The synthetic control method is an appealing data-driven procedure to examine the effects of policy interventions, in particular for case studies where only one (or a very few number) unit undergoes a treatment (for example, a policy or legislative change such as the RTW laws enactment) in a given sample period. The basic idea is to construct a weighted combination of control units based on the pre-intervention characteristics, which is expected to provide a better counterfactual than a single control unit for the treated unit(s). The synthetic control method generalizes the usual difference-in-difference approach by allowing for

the time-variant unobserved confounders. In this respect, the estimates of the policy intervention obtained from the synthetic control method are not only robust to time-invariant unobservables, but also unobservable confounders that vary over time.

We examine the impact of the RTW laws on several state level outcomes for two most recent cases: Idaho and Oklahoma. These two states adopted the RTW laws in 1985 and 2001, respectively. Each state has its own advantages and disadvantages in evaluating the RTW laws. For Oklahoma, while we have a long pre-intervention period, the post-intervention period is inherently limited and this examination may only reveal the short-run impacts. For Idaho, on the other hand, we have more than fifteen years of post-intervention data and this allows us to evaluate the potential effects of the RTW laws in the long-run. That being said, however, the lack of available data on pre-intervention period for some variables limits the number of outcomes that we can examine.

Using the synthetic control method and aggregate level data, we reach to the following empirical findings. The passage of the RTW laws in Oklahoma affected the private union membership and coverage rates, while the formal synthetic control significance tests do not lead to a firm conclusion with respect to total foreign direct investment. As for manufacturing employment rate, per capita income and average wage rates, we did not find any discernible patterns. The RTW laws for Idaho, on the other hand, increased the manufacturing employment, while providing qualitatively similar results for per capita income as in the case of Oklahoma. The impact of the RTW laws for foreign direct investment in Idaho is inconclusive due to lack of sufficient pre-intervention fit.

2 Right-to-Work Laws: Background and Previous Research

With the passage of the 1935 Wagner Act, Congress, for the first time, granted organized labor statutory sanction to get employees fired for refusal to join a union. The reaction to this change in the law gave rise to a movement to curb the additional power bestowed upon the unions at the state level. The 1947 Taft-Hartley amendments to the 1935 Wagner Act granted states the power to ban the union shop (the

so-called Right-to-Work laws), a contract provision that requires all employees to join and pay dues to the union. By the time of the 1947 Taft-Hartley Act, five states had already passed such laws (Arkansas and Florida in 1944, and Arizona, Nebraska and South Dakota in 1946). Since then twenty two states has passed the RTW laws. Figure 1 depicts these states and the years the laws were enacted. Most of the RTW states are in the West and Southeast regions with the Northeast being the only region without a RTW state and the South with greatest concentration. Note also that with the exception of Idaho and Oklahoma, all other states adopted the RTW laws between the mid 1940s and early 1970s.

There are some remarkable facts about what has happened to the economic conditions in the RTW states in the second half of the last century. Relative to non-RTW states, the RTW states grew, on average, 1.5 percentage points faster. In 2000, per capita disposable income reached \$25,183 in the RTW states, while it was \$22,332 in the non-RTW states. Furthermore, between 1970 and 2000, the average annual private employment growth (manufacturing employment growth) was 0.09 (1.7) percentage points higher in the RTW states relative to non-RTW states. These non-negligible differences across the states have led researchers to investigate the economic effects of the RTW laws.

One strand of research pertaining to RTW laws focus on unionization. Several papers examine the impact of such laws on union density and the evidence from them are mixed. Using aggregate level data and treating RTW laws as exogenously determined, Hirsch (1980), Warren and Strauss (1979) obtain negative effects in the range of 3% to 5%. These studies, however, are potentially subject to biases with the most obvious candidate is the failure to account for underlying tastes and preferences. Studies that control for unobserved factors, on the other hand, usually find no significant effect of the RTW laws on state level unionization (see, for example, Farber 1984 and Lumsden and Peterson 1975) with the prominent exceptions of Ellwood and Fine (1987) and Ichniowski and Zax (1991). The evidence from micro studies are equally mixed (see, for example, Davis and Huston 1985, and Moore et al. 1986).

Another strand of the RTW literature examines outcomes such as the extent of freeriding, wage rates and employment levels. Freeriders are described as those employees who are covered by collective bargaining agreements, but are not union members. The major difference between members and covered nonmembers

is the payment of union dues. Since employees under the collective bargaining agreements are not required to join the unions in the RTW states, the laws may affect the extent of freeriding. The more free riders there are the less effective the union will be and could eventually be viewed as a candidate for decertification. The existing evidence states that the freeriding is 6% to 10% higher in RTW states than in non-RTW states (Moore 1998). As for wage effects of the RTW laws, the findings are also mixed. Some studies find significant positive effects while some others find none or negative significant effects of the RTW laws on average wages (see, for example, Garofalo and Malhotra 1992, Mishel 2001, Reed 2003 and Greer 2004). Using the variation across states in the adaptation of RTW laws, Farber (2005) examines the union and non-union earnings separately. The preferred estimates of the author does not indicate any effect of RTW laws on neither the union nor non-union earnings.

Among many others, one final notable study is Holmes (1998), who examines the location decision of manufacturing entrepreneurs. The author did not specifically explore the isolated effect of the RTW laws, but instead used that to proxy for a state's business climate. That is, a state is described as pro-business if it is a RTW one and anti-business otherwise. In order to overcome the potential unobserved confounders because of the systematic statewide differences, Holmes examines the change in manufacturing activity when one crosses the border from a RTW to a non-RTW state. The implicit assumption of the estimation strategy is that the unobserved factors such as the attitudes towards unions, the fertility of the soil will be very similar within 25 miles of the border. Holmes (1998) finds that when one crosses the border into a pro-business state, the manufacturing employment, on average, increases approximately by one-third. It is important to emphasize that this increase reflects several pro-business policies adopted by the RTW states not just the law itself.⁴

⁴Kalenkoski and Lacombe (2006) also find positive effects of the RTW laws on manufacturing employment rates.

3 Empirical Methodology

3.1 Synthetic Control Method

The synthetic control method developed in Abadie and Gardeazabal (2003) and later extended in Abadie et al. (2010) is an appealing data-driven procedure to examine the effects of policy interventions, in particular for case studies that can not be evaluated using common econometric techniques because only one (or a very few number of groups) undergoes a treatment in a given sample period. The main idea is to construct a weighted combination of unexposed (control) units, which is expected to provide a better counterfactual for the unit that is exposed (treated) to an intervention.

To begin with, consider that there are $J + 1$ states and, for simplicity, suppose that only one state is continuously exposed to an intervention of interest (for example, passage of the RTW laws) after a time t . Let Y_{it}^N denote the outcome that would be observed for any state i ($i = 1, \dots, J + 1$) at time t ($t = 1, \dots, T$) in the absence of the intervention and T_0 be the number of pre-intervention periods such that $1 \leq T_0 < T$. Let Y_{it}^I denote the outcome of i after being exposed to the intervention of interest in periods T_0 to T . This implicitly implies that the intervention has no effect before the implementation period; for $t \in \{1, \dots, T_0\}$ and all $i \in \{1, \dots, J + 1\}$, we have $Y_{it}^N = Y_{it}^I$. Before going any further, it is crucial to note that the model relies on the assumption of no interference between units. That is, we assume that the outcomes of the unexposed states are not affected by the intervention.

That being said, the effect of the intervention for state i at time t can be expressed by $\alpha_{it} = Y_{it}^I - Y_{it}^N$ and, borrowing from the common practice in the treatment literature, the observed outcome can be written as

$$Y_{it} = Y_{it}^N + \alpha_{it}D_{it}$$

where D_{it} is an indicator that takes the value of one if state i at time t is exposed to the intervention and, value of zero otherwise. Let us define the only state that is continuously exposed to the intervention after

T_0 ($1 \leq T_0 < T$) as being the first state, then we have

$$D_{it} = \begin{cases} 1 & \text{if } i = 1 \text{ and } t > T_0 \\ 0 & \text{otherwise} \end{cases}$$

In this simple setup, we aim to estimate $(\alpha_{1T_0+1}, \dots, \alpha_{1T})$ and the effect of the intervention for the first state for $t > T_0$ is

$$\alpha_{1t} = Y_{1t}^I - Y_{1t}^N = Y_{1t} - Y_{1t}^N$$

Since Y_{1t}^I is observed, in order to estimate α_{1t} , we need to estimate the counterfactual Y_{1t}^N . Following Abadie et al. (2010), suppose that Y_{1t}^N is given by a factor model

$$Y_{1t}^N = \delta_t + \theta_t Z_i + \lambda_t u_i + \epsilon_{it} \quad (1)$$

where δ_t is an unknown common factor with constant factor loadings, Z_i is a vector of observed covariates with the corresponding θ_t vector of unknown parameters, λ_t is a vector of unobserved common factors, u_i is a vector of unknown factor loadings and finally, ϵ_{it} are unobserved transitory shocks at the state level with zero mean. Now, consider a vector of weights $W = (w_2, \dots, w_{J+1})$ such that $w_j \geq 0$ for $j = 2, \dots, J+1$ and $w_2 + \dots + w_{J+1} = 1$. Each value of the vector W represents a potential synthetic control, which is nothing but a weighted average of the control states. The value of the outcome variable for each synthetic control indexed by W is

$$\sum_{j=2}^{J+1} w_j Y_{jt} = \delta_t + \theta_t \sum_{j=2}^{J+1} w_j Z_i + \lambda_t \sum_{j=2}^{J+1} w_j u_i + \sum_{j=2}^{J+1} w_j \epsilon_{it}$$

Suppose that there are $(w_2^*, \dots, w_{J+1}^*)$ such that

$$\sum_{j=2}^{J+1} w_j^* Y_{j1} = Y_{11}, \dots, \sum_{j=2}^{J+1} w_j^* Y_{jT_0} = Y_{1T_0}, \text{ and } \sum_{j=2}^{J+1} w_j^* Z_j = Z_1 \quad (2)$$

Under standard conditions and as long as the number of pre-intervention periods is large relative to the scale

of transitory shocks, Abadie et al. (2010) show that

$$Y_{1t}^N - \sum_{j=2}^{J+1} w_j^* Y_{jt} \approx 0. \quad (3)$$

Equation (3) implies that the outcome variable for the first state for any time t in the absence of the intervention can be approximated by a synthetic control state (a weighted average of the control states).

Hence, we can obtain an estimate of the effect of the intervention by

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt} \quad \text{for } t \in \{T_0 + 1, \dots, T\}$$

Before the discussion of the choice of the optimal weight vector W^* and the inference issues, a few comments are warranted regarding the estimation procedure. First, as noted in Abadie et al. (2010), equation (2) can hold exactly only if $(Y_{11}, \dots, Y_{1T_0}, Z'_1)$ belongs to the convex hull of

$\{(Y_{21}, \dots, Y_{2T_0}, Z'_2), \dots, (Y_{J+11}, \dots, Y_{J+1T_0}, Z'_{J+1})\}$. If no such weights exist, however, the synthetic control state

is selected so that equation (2) holds approximately. For cases when even this approximation is not possible because $(Y_{11}, \dots, Y_{1T_0}, Z'_1)$ falls far from the convex hull of $\{(Y_{21}, \dots, Y_{2T_0}, Z'_2), \dots, (Y_{J+11}, \dots, Y_{J+1T_0}, Z'_{J+1})\}$,

the researcher should refrain from using the synthetic control method. Fortunately, the magnitude of the discrepancy from the convex hull can be calculated and that one can decide whether the characteristics of

the exposed state are sufficiently matched by the synthetic control. Second and more importantly, equation

(1) extends the usual difference and difference approach as the model does not impose λ_t to be constant

over time. As it is well known, the traditional difference and difference method allows for the presence

of time-invariant unobserved confounders and taking time differences eliminates these unobservables. The

synthetic control method, on the other hand, allows for the unobserved confounders to vary and a synthetic

control such that

$$\sum_{j=2}^{J+1} w_j^* Z_j = Z_1 \quad \text{and} \quad \sum_{j=2}^{J+1} w_j^* u_j = u_1 \quad (4)$$

would provide an unbiased estimator of Y_{1t}^N . But of course, choosing the synthetic control in this manner is

not feasible because $u_1 \dots u_{J+1}$ are not observed. However, as shown in Abadie et al. (2010), equation (1) implies that a synthetic control can fit Z_1 and a long set of pre-intervention outcomes Y_{11}, \dots, Y_{1T_0} only as long as it fits Z_1 and u_1 and thus, equation (4) holds.

3.2 Implementation and Inference

The critical aspect of the synthetic control method is the assignment of the weights to the control units. Let the $(T_0 \times 1)$ vector $K = (k_1, \dots, k_{T_0})$ define a linear combination of pre-intervention outcomes: $\bar{Y}_i^K = \sum_{s=1}^{T_0} k_s Y_{is}$. For instance, if $k_1 = k_2 = \dots = k_{T_0} = 1/T_0$, then the value of the outcome variable is the simple average of the pre-intervention outcomes; $\bar{Y}_i^K = \frac{1}{T_0} \sum_{s=1}^{T_0} Y_{is}$. Consider M of such combinations denoted by the vectors K_1, \dots, K_M . Let $X_1 = (Z_1, \bar{Y}_1^{K_1}, \dots, \bar{Y}_1^{K_M})$ be a $(k \times 1)$ vector of pre-intervention characteristics for the first state and similarly, X_0 be a $(k \times J)$ matrix of the same characteristics for the remaining control states; for example the j -th column of X_0 is $X_0 = (Z_j, \bar{Y}_j^{K_1}, \dots, \bar{Y}_j^{K_M})$. The optimal weight vector W^* to construct the synthetic control is chosen to minimize the distance $(X_1 - X_0 W)' V (X_1 - X_0 W)$ subject to $w_j \geq 0$ for $j = 2, \dots, J+1$ and $w_2 + \dots + w_{J+1} = 1$ where V is a diagonal matrix with nonnegative components and it reflects the relative importance of the pre-intervention characteristics. Since W^* depends on V , the choice of V also matters. Among many others, the choice of V can be based on the researcher's subjective assessment. One other possibility is to choose V such that the mean squared prediction error of the outcome variable is minimized for the pre-intervention periods. Following Abadie and Gardeazabal (2003) and Abadie et al. (2010), we employ the latter approach.

In order to determine the significance of the estimated impact from the synthetic control method, Abadie et al. (2010) suggest the use “placebo” or “falsification” tests. Similar to the classical permutation tests, the idea of the placebo study is to apply the synthetic control method to each control units as if they were exposed to the intervention and compare the actual estimated effect with that of each control unit. Under the hypothesis that the intervention has an impact, the actual estimate is expected to be large relative to the distribution of the placebo estimates.

4 Data

The state level data that we use for our study comes from several different sources and Appendix A provides detailed information regarding these sources. For our primary focus, which is the case of Oklahoma, the data covers a time span of 25 years from 1983 to 2007. We choose the year 1983 as our starting point because it is the first year that Current Population Survey began collecting monthly information for some of our important outcome variables (union membership and collective bargaining status).⁵ As noted, Oklahoma adopted the RTW laws in September 2001, leaving us with a six year of post-intervention period and this seems like a reasonable time span to observe the impact of the RTW laws on the state economy at least in the short-run.

Since the intuition behind the synthetic control method is to construct the best counterfactual from the control pool in the absence of the intervention, we restrict our effective sample to include only the pool of 26 non-RTW states. Our state level outcomes of interest are private sector union membership rates, private sector union coverage rates-defined as those employees under the collective bargaining, log of foreign direct investment (FDI)-defined as the foreign owned gross value of the property, plant and equipment, fraction of labor force employed in manufacturing sector, log of per capita income and log of private sector average wage rates. The set of observed covariates (Z_i) are outcome specific and are displayed in Table 1. For instance, in the examination of private sector unionization rates, we control for the log of state's population, log of state's minimum wage, unemployment rate, percentage of the labor force that are white, male, college graduates and residing in a metropolitan area. Similarly, in the case of, say FDI, we include the log of state's population, log of state's land square mileage and road mileage, log of state's minimum wage, log of state's average local tax and per capita corporate tax, log of private sector wage rate, private sector unionization and state unemployment rates and the percentage of the labor force that are college graduates. Our choices for the set of controls are consistent with the relevant past literature (see, for example, Ellwood and Fine 1987, Keller and Levinson 2002 and List et al. 2003). The observed covariates are averaged over the entire

⁵Beginning in 1983, union status questions were added to the earnings supplement of the monthly CPS outgoing rotation group files. The monthly earnings supplement, absent the union status questions, began in January 1979.

pre-intervention period and moreover, for each outcome of interest, we augment its lagged value in 2000, 1995, 1990 and 1985. The average values along with the lagged outcomes constitute our X_1 and X_0 for Oklahoma and all the non-RTW states, respectively.

The case of Oklahoma is likely to reveal potential short run, if any, impact of the RTW laws. However, it may be misleading to make a firm conclusion without further exploring the long-run consequences of the RTW laws enactment. To this end, we also examine the case of Idaho, which adopted the RTW laws in January 1985. The lack of detailed (industry-level) unionization data prior to 1983 leads to a very short pre-intervention span and thereby, restricts our ability to examine union related outcomes.⁶ For other outcomes, on the other hand, we can go back to 1977 resulting in a 9 years of pre-intervention data. Although we have 22 years of post-intervention data, from 1986 to 2007, we use only the period between 1986 and 2000. This is to prevent any interference the RTW law passed in Oklahoma in 2001 might have on our evaluation of the law in Idaho. Having fifteen years of post-intervention data seems feasible to evaluate the potential effects of the RTW laws in the long-run. With the exception of detailed unionization controls, the variables used for X_1 and X_0 are identical to those presented in Table 1; the covariates are averaged over the period of 1977 to 1985 and for each outcome, its lagged value in 1983, 1982, 1979 and 1977 are added.

5 Empirical Results

Below, we initially present the results for Oklahoma for all the outcome variables and for Idaho, we present the results for the selected outcomes. In all our estimations, the diagonal components of the V matrix are chosen to minimize the mean squared prediction error of the outcome variables before the passage of the RTW laws. To avoid any local minima, three different starting points are considered: regression based starting point, equal-weight starting point and starting point determined by the maximum likelihood approach.⁷ Conditional on V matrix, then, the W^* is set to produce the best counterfactual if Oklahoma or Idaho had not adopted the RTW laws.

⁶Recall that a synthetic control can fit Z_1 and a long set of pre-intervention outcomes Y_{11}, \dots, Y_{1T_0} only as long as it fits Z_1 and u_1 . Otherwise, equation (4) does not hold.

⁷The values of the V matrix for all the outcome variables are available upon request.

5.1 The Case of Oklahoma

5.1.1 Private Sector Union Membership and Coverage Rates

Prior to the enactment of the RTW laws, the synthetic control that resembles Oklahoma the most with respect to the predictors of private sector unionization is constructed using three states. The largest weight is attributable to Colorado (54.1%), followed by New Mexico (36.2%) and Vermont (9.8%). The rest of the states are assigned zero weights. The panel A of Table B1 in the Appendix B displays the mean of the selected pre-intervention characteristics of actual Oklahoma, synthetic Oklahoma and all the other non-RTW states. The average gap across the characteristics of Oklahoma and its synthetic counterpart is substantially smaller than it is between Oklahoma and other states. The top left corner of Figure 2 depicts the private sector union membership rates from 1983 to 2007 for the actual and the synthetic Oklahoma and it highlights several important points. First, consistent with the overall trend observed in the U.S. in the last three decades, the private sector union membership rates have been declining in Oklahoma. Next, even though it is not perfect, the membership rates for the synthetic Oklahoma track the trajectory for almost the entire pre-intervention period with a root mean squared prediction error (RMSPE) of 0.76.⁸ Finally, just after the passage of the law, we observe a noticeable divergence between the two lines; the synthetic Oklahoma continued its moderate downward trend, while Oklahoma experienced a sharp decline. This indicates a negative impact of the RTW laws on private sector union membership rates. The average post-intervention gap is just above 1%. That is, if the state of Oklahoma had not adopted the RTW laws, the private sector membership rates would be 1% larger. Taking the average pre-treatment period of Oklahoma’s union membership rate of $\bar{Y}_{pre-intervention} = 6.9\%$ as our benchmark, the $\bar{\alpha} = 1\%$ gap corresponds to a roughly $\frac{\bar{\alpha}}{\bar{Y}_{pre-intervention}} = 14.5\%$ reduction with respect to the pre-intervention period.

A closely related and explored outcome variable in the RTW literature is the private sector coverage rate. Consonant with its affinity to membership rates, the synthetic control for the private sector coverage rate is constructed using the same states, though with slightly different weights; Colorado (46.4%), followed

⁸The pre-intervention MSPE is the average of the squared discrepancies for the outcome variable (e.g., private sector unionization rate) between Oklahoma and its synthetic counterpart during 1983-2001.

by New Mexico (29.7%) and Vermont (23.8%). The top right corner of Figure 2 plots the private sector coverage rates for the entire period and is very similar to that of union membership rates, which is also observable in terms of the magnitudes. The average post-intervention gap is just above 1% and the average pre-treatment period of Oklahoma’s coverage rate is $\bar{Y}_{pre-intervention} = 7.5\%$, which corresponds to a roughly $\frac{\bar{\alpha}}{\bar{Y}_{pre-intervention}} = 13.8\%$ reduction with respect to the pre-intervention period.

Past research examining the role of RTW laws on union density evolves around three competing hypothesis (see, for example, Moore and Newman 1985 and Moore 1998). The “Taste Hypothesis” indicates that the RTW laws only exist in states where anti-union sentiment is strong. Since a populace with strong anti-union attitudes is likely to resist union-organizing attempts and is more likely to support the passage of the RTW laws, the estimate of RTW effect on union density is a by-product of omitted attitudes, tastes and preferences. If these unobservable factors are properly controlled in the econometric model, the nonzero effect will vanish. The “Freerider Hypothesis”, on the other hand, argues that the RTW laws increases union organizing and maintenance costs because union shops can not be used to curb freeriding. The reflection of freeriding to union members is by higher dues and thus, RTW laws are expected to reduce unionization. Finally, the “Bargaining Hypothesis” suggests that RTW laws weaken the bargaining power of unions by reducing their membership and the ability to conduct strikes. In the long run, the reduction in union benefits causes declines in the demand for union services and thereby lower unionization.

Our findings are likely to rule out the Taste Hypothesis. As described above, the synthetic control method estimates are robust to the presence of unobserved confounders even if they vary with time. If the unobservable factors such as attitudes towards unions were affecting and biasing the estimate of the RTW laws, we would not expect to see any effect once we remove them. However, eliminating the time variant and invariant confounders does not wash away the noticeable negative effect of RTW laws on membership/coverage. The Freerider Hypothesis does not seem to hold as well. If the density of freeriders were to increase after the adaptation of the RTW laws, we would expect the reduction in membership rates to be significantly larger than it is for coverage rates. Sobel’s (1995) findings indicate that no more than one-third of covered nonmembers are true freeriders and hence, an elimination of the RTW laws would have only a

modest effect on the extent of unionization. Our findings support this conclusion and leave us with the third hypothesis as a potential source of explanation.

Of course, a natural and an important question to ask at this point is the significance of our results. To determine this, we conduct a series of placebo studies by iteratively applying the synthetic control method to all non-RTW states. In each iteration, we reassign the passage of the RTW laws to one of the 26 states (shifting Oklahoma to the control pool) as if one of the control units adopted the law in 2001. We then estimate the effect of the RTW laws for each placebo study. For comparison and rankability purposes, we compute the size of the average effect ($\bar{\alpha}$) relative to the average pre-treatment private sector membership and coverage rate ($\bar{Y}_{pre-intervention}$). Apart from this, following Abadie et al. (2010), we can use the RMSPE as a tool to compare Oklahoma with placebo studies. The basic idea is to examine the distribution of the ratios of post/pre-intervention RMSPE, which provides a measure of affinity between each state and its synthetic counterpart before and after the intervention. Under the assumption that the pre-intervention is good, if the effect of the intervention were to be nonrandom, we would expect the post/pre-intervention RMSPE ratio to be large relative to the placebo studies.

Panel A of Table 2 presents the significance results for the private sector union membership. The first row of the panel yields the ratio of the average effect to the average pre-intervention membership rates. In absolute value, Oklahoma is ranked the first (-14.5%) with the median ratio of -1.5% across the placebo states. If one were to assign the intervention at random, the probability of estimating a gap of the magnitude as of Oklahoma is only $1/27=0.037$, which is less than the level of 5% used in conventional tests of statistical significance. We believe that this evidence is convincing enough to conclude that the adaptation of the RTW laws had a negative impact on the private sector union membership rates in Oklahoma. Nevertheless, in the second row, the post/pre-intervention RMSPE ratios are given. The picture, however, is not clear here. Oklahoma is ranked the 12th with a RMSPE ratio of 1.62, while the median across the rest of the states is 1.45 (second column).

Panel B of Table 2 presents the results for the private sector coverage rates. As expected, the ranking is very much in line with that of membership rates. In terms of the average ratio ($\frac{\bar{\alpha}}{\bar{Y}_{pre-intervention}}$), in absolute

value, Oklahoma is ranked the first (-13.8%) with a median of 0.7% for the placebo states, while it is ranked 9th using the post/pre-intervention RMSPE.

5.1.2 Foreign Direct Investment and Manufacturing Employment

As for the FDI, the counterfactual that resembles the most of Oklahoma is built using a weighted combination of three states: Montana (66.9%), California (26.8%) and West Virginia (6.3%). The panel B of Table B1 in the appendix B displays the selected pre-intervention characteristics of actual Oklahoma, synthetic Oklahoma and all the other non-RTW states. We observe much more affinity for Oklahoma and its synthetic counterpart than the rest of the non-RTW states. The middle left panel of Figure 2 plots the log of FDI from 1983 to 2007 for the actual and the synthetic Oklahoma. The pre-intervention RMSPE is 0.13 indicating that the synthetic Oklahoma is able to provide a good fit for FDI. Just after the implementation of the law, we observe an initial divergence between the two lines then dying out and resulting an intersection in 2005 and a sharper divergence afterwards. The average intervention effect is 0.214 log points. That is, the adaptation of the RTW laws increases the FDI in Oklahoma by 23.9% over the six year period, on average.⁹

Panel C of Table 2 presents the significancy results for FDI. Looking at the first row of the Panel and using the impact rank, we observe that Oklahoma is ranked the 6th with a value of 23.9%. The median effect for the placebo studies is 2.2%. In terms of the RMSPE ratio, Oklahoma is ranked the 16th with a post/pre-intervention RMSPE ratio of 1.75, while the median across the remaining 26 state is 1.96 (second row). Out of the 5 states that are higher impact ranked than Oklahoma, California's RMSPE ratio is less than one indicating a very poor pre-intervention fit and Indiana has a lower RMSPE ratio. Taken altogether, we can not draw a firm conclusion from the significancy tests. However, if the effect is statistically meaningful, the impact of RTW laws on FDI is substantial.

The synthetic control for manufacturing employment is constructed using Kentucky (46.6%), New Mexico (27.8%), Montana (24.2%) and Minnesota (1.5%). The middle right panel of Figure 2 depicts the manu-

⁹Since FDI is defined in log form, a standardization is unnecessary. The percent wage differential is obtained as $(\exp(\bar{\alpha}) - 1) * 100$

facturing employment rate for the actual and synthetic Oklahoma. Manufacturing employment has been declining over almost the entire period of 1983 to 2007 with more pronounced declines in the last two decades and beginning with 2003, we observe a divergence between the two lines. As for the ratio of average effect to average pre-intervention manufacturing employment rates, Oklahoma is ranked the 12th with a value of 1.1% and is ranked 20th with respect to RMSPE ratio. Based on these results, it is not likely that RTW laws have any impact on the manufacturing employment rate. For Idaho, as shown below, the story is different.

5.1.3 Other State Level Outcomes

In addition to the outcome variables presented so far, we have examined the post-intervention evolution of per capita income and private sector average wage rate. The bottom panel of Figure 2 plots Oklahoma and its counterfactual for these outcome variables. After the passage of the RTW laws, an eyeballing of the figures does not reveal any discernible patterns and there are several post-intervention crossings between the two lines. Consonant with this, Oklahoma is the 8th (26th) state in the impact (RMSPE) ranking for per capita income with an average effect of 0.9% and is 18th (21th) in the impact (RMSPE) ranking for private sector average wage rate with an average effect of -0.8%.

Finally, we examine the effects of RTW laws on manufacturing union membership and coverage rates, as well as on manufacturing average wage rate. The estimations are qualitatively similar to those presented in the paper and are available upon request.

5.2 The Case of Idaho

As noted, due to data limitations, we exclude some of the state-level outcomes from the analysis in the case of Idaho. Figure 3 plots the actual Idaho and its synthetic control for FDI (top left panel), manufacturing employment rate (top right panel) and per capita income (bottom panel).¹⁰ Several post-intervention crossings, as well as the statistical examination (described below) rule out the possibility of any potential effect of the RTW laws on FDI and per capita income. Our findings for manufacturing employment, however,

¹⁰Idaho adopted the RTW laws in 1985 and therefore, Oklahoma is treated as a non-RTW state in the construction of the synthetic control.

suggest a significant effect of the law. Therefore, we mostly focus on our estimates for the manufacturing employment.

Five states contribute to the construction of synthetic Idaho: Montana (52.8%), Maine (26.3%), Minnesota (13.2%), Michigan (7.5%), and New Mexico (0.2%) and Table B2 in the appendix B presents the selected pre-intervention characteristics. Several patterns emerge from eyeballing the top right panel of Figure 3. Consonant with the entire country trend, manufacturing employment has been declining in Idaho. Next, with the implementation of the law, we observe a wide divergence between the two lines, indicating a positive effect of RTW laws on manufacturing employment. Moreover, the effect of the RTW laws in Idaho on manufacturing employment does not die out over time and even slightly more pronounced in the following decades, which provides some assurance for the long run effect of RTW laws, at least for the manufacturing employment.

In terms of the magnitude, the average post-intervention gap is 2.5% and the average pre-treatment period of Idaho's manufacturing employment rate is $\bar{Y}_{pre-intervention} = 17.1\%$, which corresponds to a $\frac{\bar{\alpha}}{\bar{Y}_{pre-intervention}} = 14.4\%$ increase with respect to the pre-intervention period (the pre-intervention RMSPE is 0.10). But, of course this quantity will be meaningful if it is large enough to be more than random. To this end, we conduct a series of placebo tests and the results from this exercise are displayed in Panel A of Table 3. As for the impact ranking, Idaho is ranked the 2nd and the only state that generates a slightly higher effect is for Wisconsin (14.8%) with the median value of 0.00% for all the control states. When we rank states according to the size of the RMSPE ratios, there are 5 states with larger RMSPE ratio with a median of 35.1 (third column) as opposed to 24.14 for Idaho and the median ratio for the entire control states is 6.24 (second column). Even though the results presented here are in line with Holmes (1998), there are important distinctive features of our findings. Instead of using the RTW laws as a proxy for all the state pro-business policies, we examine the isolated impact of these laws on manufacturing employment and moreover, our estimates reflect the effect for the entire state not just what one sees close to the border.

The inconsistency between the manufacturing employment estimates from Idaho and Oklahoma may have resulted from the different economic environments that existed in 1980s and in 2000s. A brief look at

the employment statistics in the U.S. in those two decades provides supportive, but tentative, evidence on our hypothesis. In both 1980s and 2000s, the manufacturing share of the total labor force declined, however, a closer look reveals different patterns in the labor markets of the RTW and non-RTW states. In 1980s, the RTW states experienced, on average, a significantly less pronounced decrease in the manufacturing employment rates (15% in the RTW states and 24% in non-RTW states). In the last decade, however, both the RTW and non-RWT states witnessed similar, and much sharper reductions in the manufacturing employment rates (29% in the RTW states and 32% in non-RTW states). These trends suggest that while having the RTW laws three decades ago may have potentially acted as a protective shield and mitigated the negative effects of economic environment, the same may not have been true at the time Oklahoma became a RTW state. Another potential explanation pertains to the length of post-intervention era. Even though we observe an immediate impact of RTW laws on manufacturing employment in Idaho, there may be some lag in the case of Oklahoma and indeed, the trends for actual and the synthetic Oklahoma drift apart beginning with 2003.

The panels B and C of Table 3 present the results for FDI and per capita income, respectively. Although the placebo tests are strong enough to invalidate any causal effect of RTW laws, there is one caveat to these results. The pre-intervention fit for the FDI is fairly poor in the case of Idaho (0.59); the second last among all the states. As noted in Section 3, the pre-treatment characteristics of the exposed unit must fall into the convex hull of the control units to achieve sufficient affinity between the treated unit and its counterfactual. Over the time period we exploit, Idaho is one of the states with the least FDI, which generates a serious problem in the construction of the synthetic control. Therefore, our finding for this outcome variable should be taken with caution at best.

5.3 Lower Bound Approximations and Interference Issues

Thus far, we overlook the fact that the pre-intervention trends across the actual and the synthetic states do not overlap perfectly. For instance, the manufacturing employment pre-intervention trends for Idaho almost perfectly overlap up until 1983 and just a few years before the RTW laws enactment, a divergence

occurs. Recall that the estimates from the previous sections will be valid under the assumption that the pre-intervention gaps on the outcome variables for the state of interest is zero- or more realistically, close enough to be zero. In order to account for these pre-intervention differences, we slightly modify the synthetic control method of Abadie et al. (2010). Specifically, we subtract the average pre-intervention gap from the average post-intervention gap. These modified estimates, therefore, can be thought of as approximations for the lower bound of the true effects.

Table 4 displays the lower bound estimates (second column) along with the actual estimates (first column) for the outcome variables for which we find a significant effect: private sector union membership, private sector coverage rate and FDI for Oklahoma, and manufacturing employment rate for Idaho. Our main results changed neither for Oklahoma nor for Idaho after the modification. For Oklahoma, the impact ratio is reduced from -14.5% to -13.7% for private unionization rate, from -13.8% to -13.1% for private coverage rates, and from 23.9% to 23.0% for FDI. Similarly for Idaho, it is reduced from 14.4% to 13.6%. These results suggest that even after correcting for the pre-intervention gaps across the actual and the synthetic outcomes, our estimates for the effects of the RTW laws still appears to be non-negligible.

Finally, as noted, the synthetic control method relies on the assumption of no interference between units. That is, the outcomes of the non-RTW states are not affected by the intervention. There are a variety of ways in which this assumption can be violated. In the case of union related variables, the anti-union sentiment created in Oklahoma could have spread to other non-RTW states. Moreover, fearing from a potential epidemic effect, national unions may react to the passage of the RTW laws by diverting resources to the fragile state (Oklahoma) from other non-RTW states. In both cases, interference would likely cause lower levels of membership or coverage rates in the control states, which will attenuate our estimates of the effect of RTW laws.¹¹ As for FDI or manufacturing employment, a reverse impact of interference may occur. The passage of RTW laws may attract investments into Idaho or Oklahoma from other non-RTW states, which would yield an upward biased estimate of the effect of RTW laws. However, given the large

¹¹It is also plausible that the national unions may divert resources away from Oklahoma to non-RTW states considered to be more fragile. This may lead to an increase in the membership or coverage rates of non-RTW states.

magnitudes of our estimates, this potential interference is not likely to account for all the effect.

6 Conclusion

The effects of RTW laws have been the focus of a controversial policy discussion with no consensus among policymakers or academics since the first wave of enactment in the mid-1940s. The lack of consensus in academia is predominantly driven by the difficulty of distinguishing the effects of RTW laws from state characteristics, as well as other state policies that are unrelated with these laws. Even in the absence of a causal relation, it is plausible to obtain significant correlations between RTW laws and state-level outcomes due to systematic differences across states. Furthermore, the enactment timing of the RTW laws throughout the United States augments the difficulty for identification. After the first wave of the implementation in 1940s and 1950s, there were a few numbers of states adopting RTW laws with long intervals between each instance.

Using the case studies of Idaho and Oklahoma, the most two recent states adopting the RTW laws, and the synthetic control method, we find that the RTW laws in Oklahoma affected the private union membership and coverage rates and possibly FDI. The findings from union related outcomes are likely to rule out the Taste Hypothesis and the Free Rider Hypothesis, while providing evidence in favor of the Bargaining Hypothesis. The RTW laws do not seem to yield any effect on manufacturing employment, per capita income and private sector average wages. The examination of Idaho reveals significant effects of the RTW laws on manufacturing employment. In the post-intervention era, the share of the manufacturing employment, on average, appears to be 2.5 percentage points higher, transferring into ten thousand manufacturing jobs saved or added per year. Finally, we do not observe any impact of the passage of RTW laws on per capita income and FDI in Idaho. However, the estimate for FDI should be taken with caution due to very poor pre-treatment fit.

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Table 1: The Set of Covariates Used to Construct the Synthetic Controls

	State-Level Outcomes					
	Private Sector Union Membership Rates	Private Sector Union Coverage Rates	Total FDI	Manufacturing Employment	Per Capita Income	Private Sector Average Wage Rate
Covariates						
Log of State's Population	*	*	*	*	*	*
Log of State's Road Mileage			*			
Log of State's Land Square Mileage			*			
Log of State's Minimum Wage	*	*	*	*	*	*
State's Average Local Tax			*			
Log of Per Capita Corporate Tax			*			
State's Unemployment Rate	*	*	*		*	*
Log of Average Private Sector Wage Rate	*	*	*			
Log of Average Manufacturing Wage Rate				*		
% of Whites in Labor Force	*	*				
% of Males in Labor Force	*	*				
% of College Graduates in Labor Force	*	*	*	*	*	*
% of Metropolitan Residing in Labor Force	*	*				
State's Total Unionization Rate					*	
State's Private Sector Unionization Rate			*			*
State's Manufacturing Unionization Rate				*		
Lagged Values of the Outcome	*	*	*	*	*	*

NOTES: See Appendix A for further definitions of the variables.

Table 2: The Effects of RTW Laws on State Level Outcomes: The Case of Oklahoma

	Oklahoma	Placebo Median All	Placebo Median States with Higher RMSPE	Placebo Median States with Lower RMSPE
Panel A: Private Sector Union Membership Rates				
Average Effect/Pre-Intervention Average (Rank)	-14.5% (1st)	-1.55%	-5.1%	-0.5%
RMSPE Ratio (Rank)	1.62 (12th)	1.45	3.05	1.01
Panel B: Private Sector Union Coverage Rates				
Average Effect/Pre-Intervention Average (Rank)	-13.8% (1st)	0.75%	1.10%	0.00%
RMSPE Ratio (Rank)	1.60 (9th)	1.26	3.01	1.04
Panel C: Log of FDI				
Average Effect/Pre-Intervention Average (Rank)	23.9% (6th)	2.20%	-0.99%	3.14%
RMSPE Ratio (Rank)	1.75 (16th)	1.96	2.75	1.33
Panel D: Manufacturing Employment Rate				
Average Effect/Pre-Intervention Average (Rank)	1.10% (12th)	0.70%	1.50%	0.00%
RMSPE Ratio (Rank)	1.12 (20th)	2.03	2.79	0.60

NOTES: The placebo state values are based on 26 non-RTW states. The RMSPE Ratio is the post/pre-intervention RMSPE. See text for further details.

Table 3: The Effects of RTW Laws on State Level Outcomes: The Case of Idaho

	Idaho	Placebo Median All	Placebo Median States with Higher RMSPE	Placebo Median States with Lower RMSPE
Panel A: Manufacturing Employment Rate				
Average Effect/Pre-Intervention Average (Rank)	14.35% (2nd)	0.00%	10.75%	-1.51%
RMSPE Ratio (Rank)	24.14 (6th)	6.24	35.1	3.73
Panel B: Log of Foreign Direct Investment				
Average Effect/Pre-Intervention Average (Rank)	44.87% (12th)	10.82%	10.82%	NA
RMSPE Ratio (Rank)	0.43 (27th)	7.68	7.68	NA
Panel C: Log of Per Capita Income				
Average Effect/Pre-Intervention Average (Rank)	0.76% (11th)	-0.40%	-0.49%	-0.31%
RMSPE Ratio (Rank)	1.20 (23rd)	3.80	5.75	3.73

NOTES: The placebo state values are based on 27 non-RTW states including Oklahoma. The RMSPE Ratio is the post/pre-intervention RMSPE. See text for further details.

Table 4: Lower Bound Approximations

	Actual Estimates	Lower Bound Estimates
Oklahoma		
Private Union Membership Rate	-14.5%	-13.7%
Private Coverage Rate	-13.8%	-13.1%
FDI	23.9%	23.0%
Idaho		
Manufacturing Employment Rate	14.4%	13.6%

NOTES: The actual estimates represent average effect/pre-intervention average, while the lower bound estimates represent (average effect-pre-intervention gap)/pre-intervention average. See text for further details.

Figure 1: US States by Right-to-Work (RTW) Laws and Enactment Year

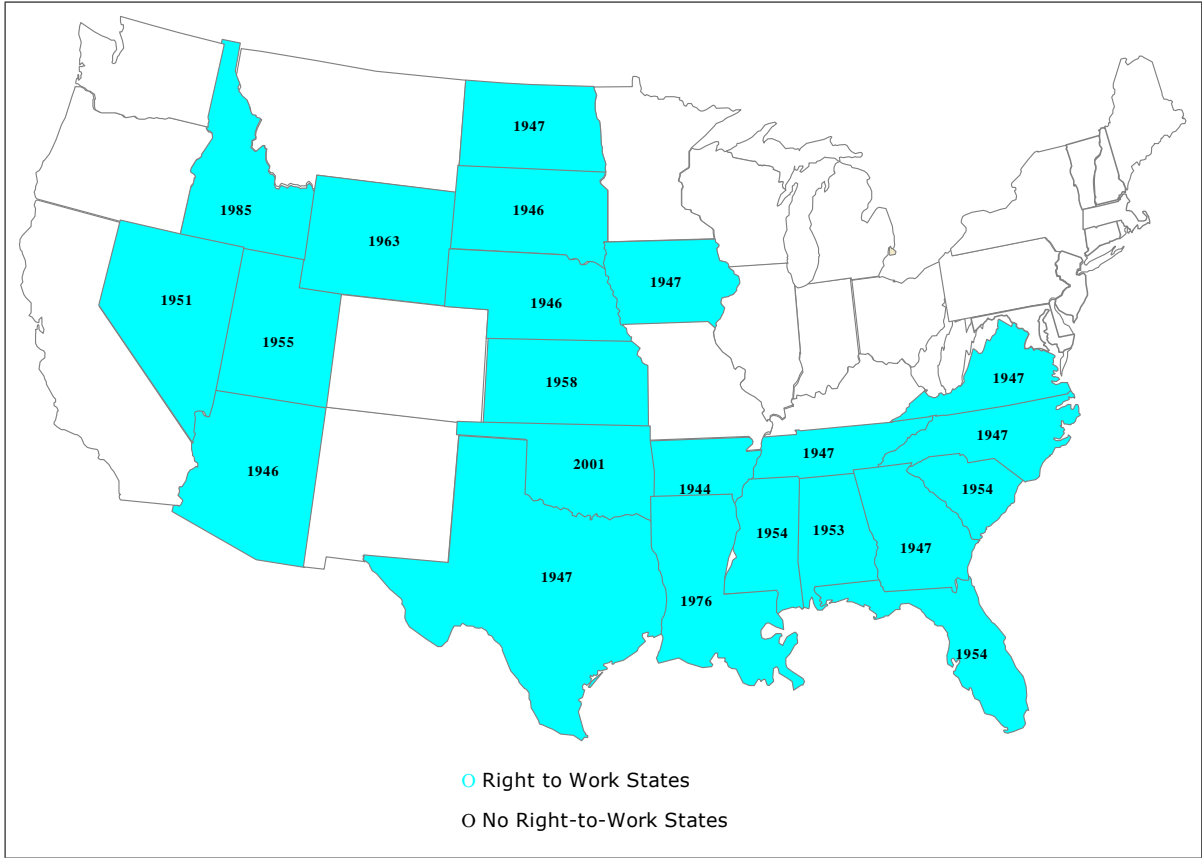
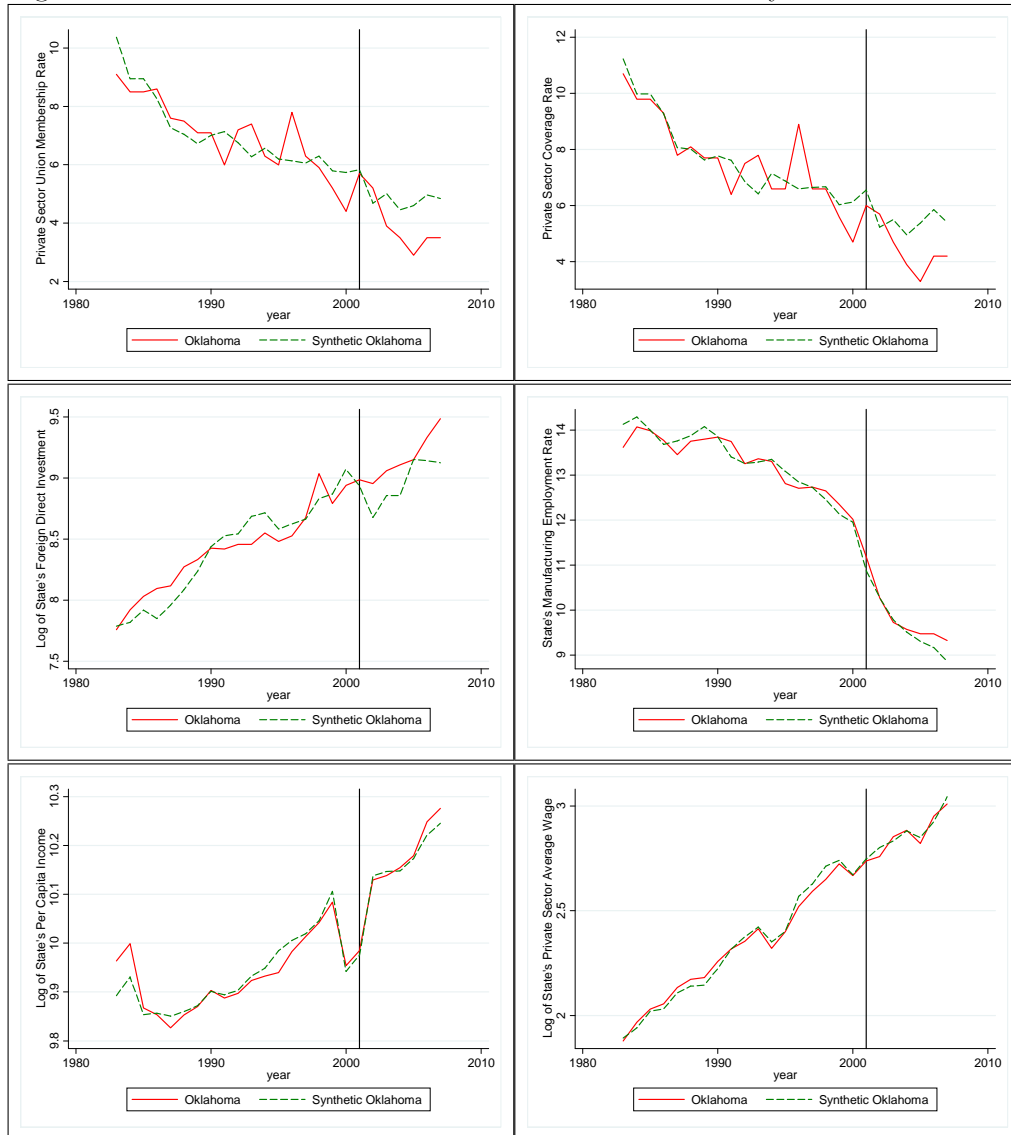
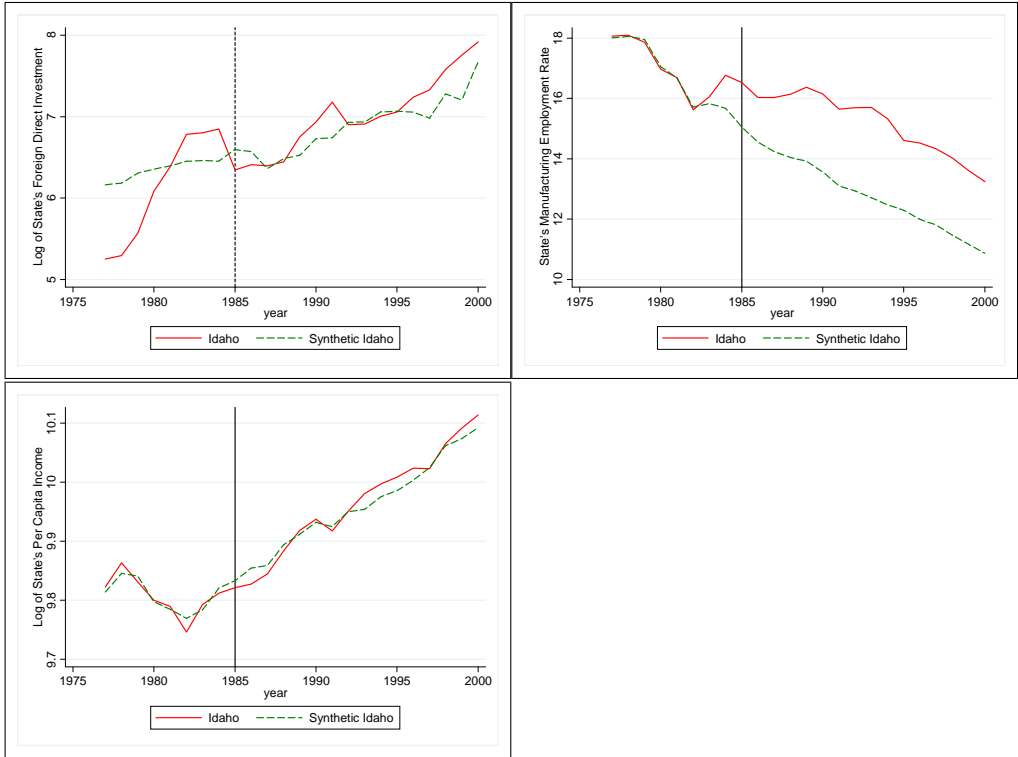


Figure 2: Trends in State Level Outcomes: Oklahoma vs. Synthetic Oklahoma



NOTES: The control group consists of 26 non-RTW states.

Figure 3: Trends in State Level Outcomes: Idaho vs. Synthetic Idaho



NOTES: The control group consists of 27 non-RTW states including Oklahoma.

Appendix A: Data Sources

Union Membership and Coverage Rates

Union Membership (Union Coverage status) as a percentage of the civilian labor force. Source: *Union Membership and Coverage Database* available at *www.unionstats.com*, see Hirsch and Macpherson (2003) for a general description of the database.

Foreign Owned Gross Value of Property, Plant and Equipment

All values are deflated by the year's average consumer price index (2000=100). Source: U.S. Department of Commerce, Bureau of Economic Analysis, *Foreign Direct Investment in the United States*.

Manufacturing Employment Rate

Manufacturing Employment as a percentage of the civilian labor force. Source: Bureau of Economic Analysis, U.S. Department of Commerce, Regional Economic Measurement Division, *Regional Economic Information System CD*.

State Income Data

All values are deflated by the year's average consumer price index (2000=100). Source: Bureau of Economic Analysis, U.S. Department of Commerce, Regional Economic Measurement Division, *Regional Economic Information System CD*.

Average Wage Rates

Source: *Union Membership and Earnings Data Book*, available from *Bureau of National Affairs*. The database is constructed by Barry Hirsch (Andrew Young School of Policy Studies, Georgia State University) and David Macpherson (Department of Economics, Trinity University).

State Minimum Wages

Source: U.S. Department of Labor, *U.S. Employment Standards Administration*.

Unemployment Rate

Total Unemployment as a percentage of the civilian labor force. Source: Bureau of Labor Statistics, *Local Area Unemployment Statistics*

Road Mileage

Sum of urban and rural highway mileage. Source: U.S. Department of Transportation, *Federal Highway Administration*.

Land Square Miles

Source: Bureau of Census, *State and County Quick Facts*.

State Population

Source: Bureau of Census, *Bureau of Economic Analysis*

State Local Tax and Per Capita Corporate Tax

The state local tax burden is an estimate of the combined state local tax burden shouldered by the residents as percentage of their income. Per capita corporate tax is the per capita tax revenue collected from the corporations. Source: *The Book of the States*, various years.

Gender, Racial, Residential and Educational Statistics

Percentage of the civilian labor force that are male, white, college graduate and those residing in metropolitan area. Source: *Current Population March Supplement*.

Appendix B: Summary Statistics

Table B1: Summary Statistics of Pre-Intervention Characteristics

	Oklahoma		Non-RTW States
	Actual	Synthetic	Mean
	Mean		Mean
Panel A: Private Sector Union Membership Rates			
Log of State's Population	14.996	14.619	15.168
State's Unemployment Rate	5.816	5.825	5.708
% of Males in Labor Force	53.868	53.766	53.177
Private Sector Union Membership Rates in 1995	6.001	6.198	11.503
Private Sector Union Membership Rates in 1985	8.500	8.915	16.773
Panel B: Log of Foreign Direct Investment			
Log of State's Road Mileage	11.624	11.353	10.895
Log of State's Minimum Wage	1.386	1.367	1.487
Log of Per Capita Corporate Tax	3.721	4.528	4.212
Log of Average Private Sector Wage Rate	2.313	2.305	2.591
% of College Graduates in Labor Force	18.921	21.935	24.118
Log of Foreign Direct Investment in 2000	8.94	9.072	9.617
Log of Foreign Direct Investment in 1990	8.425	8.438	8.572

NOTES: The variables are only a subset of those used in the paper. The full set of summary statistics are available upon request.

Table B2: Summary Statistics of Pre-Intervention Characteristics

	Idaho		Non-RTW States
	Actual	Synthetic	Mean
	Mean		Mean
Manufacturing Employment Rates			
State's Unemployment Rate	7.450	7.250	7.655
Log of Average Manufacturing Wage Rate	3.450	2.992	3.173
Manufacturing Employment Rate in 1983	16.052	15.844	21.211
Manufacturing Employment Rate in 1977	18.068	18.018	24.697

NOTES: The variables are only a subset of those used in the paper. The full set of summary statistics are available upon request.