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Right-to-Work Laws and State-Level Economic Outcomes: Evidence Using Synthetic Control Method

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Abstract

The effects of right-to-work laws on states, unions and workers are controversial and important policy questions. Empirical evidence on the effects is far from being conclusive in the previous literature, mainly because of the difficulties in identification. In this paper, we use a recently developed econometric methodology, so-called "synthetic control method", and exploit two most recent implementation of right-to-work laws in Idaho and Oklahoma- in 1985 and 2001 respectively- to evaluate the law's effects. Our results suggest that the right-to-work law passed in Oklahoma in 2001 only affected private unionization and coverage rates and, possibly to some extent, foreign direct investment. For the other outcome variables, manufacturing share of private non-farm employment, personal income per capita, private sector average wage rate, and manufacturing sector average wage rate, we did not find any discernable patterns after the right-to-work law was enacted. For Idaho, the second state that is the focus of our study- crucial especially for looking at the potential long-term effects of the law-, the right-to-work law passed in 1985 increased the manufacturing share of private non-farm employment while it had no impact on personal income per capita. For foreign direct investment, our results are inconclusive since pre-intervention fit of synthetic control for this variable is very poor.

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1 Introduction

A March 3, 2008 editorial in Wall Street Journal, citing a study by National Institute for Labor Relations Research , compared the economic performances of Texas and Ohio. The article stated that "in the previous decade while Texas added 1,615,000 new jobs Ohio lost 10,400 jobs" and offered right-to-work laws in Texas as one of the main determinants for the differences the economic performance of these two states, with the others being NAFTA and the absence of state income tax in Texas. Another Op-Ed in the same newspaper in December 2007 had also suggested right-to-work laws as one of the two most important policy variables attracting job and capital investment. Right-to-Work (RTW) laws the articles refer to are state statutes that prohibit labor unions and employers to enter into contracts that require all employees to be fee-paying members of a union. Therefore, by making these types of contracts illegal, being a member of a union would stop being a prerequisite for employment in many firms located in the states after the enactment of these laws. For example, 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, unions and workers are controversial policy questions. The opponents of the law, especially the unions, assert that RTW laws led to lower wages for both union and non-union workers, lower safety and health standards that protect workers on the job, and "free rider" problems, e.g. using union resources and having benefits union members enjoy by non-union workers without incurring any cost. Therefore, the opponents stress, by weakening unions and collective bargaining, these laws destroy job security protection that comes with a union contrac. On the other hand, the advocates of RTW laws emphasize the fact that RTW states enjoy a higher standard of living than do non-RTW states. Furthermore, RTW states experience faster growth in manufacturing and nonagricultural jobs, lower unemployment rates and fewer work stoppages.

Whether these arguments are correct or not is an empirical question. Holmes (1997) pointed out the fact that it is almost impossible to identify the separate impact of the RTW laws on state level economic outcomes since the RTW states also have other pro-business laws that affect the same economic outcomes as RTW laws potentially do. Furthermore, the time RTW laws have been enacted throughout the United States augments the difficulty of identification. After the first wave of implementation in 1940s and 1950s, there were a few number of states passed RTW laws with long intervals between each instance. For example, three states that enacted the laws most recently are Louisiana in 1976, Idaho in 1985, and Oklahoma in 2001. To deal with these issues, we propose using an econometric method that was introduced by Abadie and Gardeazabal (2003) and later extended in Abadie et al. (2010) help us identifying and evaluating the effect of RTW laws. While our main focus in this paper is the case of Oklahoma, we also examine the potential effect of the RTW law in Idaho for a selected number of outcomes. The novelty of this method is to allow researchers to evaluate case studies that cannot be evaluated using common econometric techniques because a very few number of groups (for example, a policy or legislative changes) in a sample period. As we will explain in more detail in section 3, the method is based on; first constructing a synthetic control group, which is similar to the group of interest (treatment group) in the case study in terms of the values of the outcome variable in pre-treatment years, using other groups in "control pool"; and, then, comparing the outcome variable for the synthetic control and treatments groups in the post-intervention years.

Our focus is only to examine the cases of Idaho and Oklahoma since we do not have data for the most of the control or outcome variables of interest going back enough in time for the case of Louisiana (so in our framework treatment group is either Idaho or Oklahoma while the groups in "control pool" are the other states without RTW laws). Each state has its own advantages and disadvantages in evaluating RTW laws. For Oklahoma we do not have any data limitations on pre-treatment years while having only seven years of post treatment data. For Idaho, on the other hand, the amount of data we have for pre-treatment years limits the number of outcomes that can be examined. Since we have more than twenty years of post treatment data, however, it is feasible to evaluate the potential effects of the RTW law in the long-run.

Our results using synthetic control method suggest that RTW laws are neither as effective as their advocates claim nor are detrimental to the well being of workers as their opponents argue. In particular, we found that the RTW law passed in Oklahoma in 2001 only affected private unionization and coverage rates and foreign direct investment (FDI)-defined as the gross value of property, plant, and equipment owned of foreign-owned manufacturers. For the other outcome variables, manufacturing share of private non-farm employment, personal income per capita, private sector average wage rate, and manufacturing sector average wage rate, we did not find any discernable patterns after the RTW law was enacted. For Idaho, the second state that is the focus of our study-crucial especially for looking at the potential long-term effects of the law-, the RTW law passed in 1985 increased the manufacturing share of private non-farm employment while it had no impact on personal income per capita. For FDI, our results are inconclusive since pre-intervention fit of synthetic control for this variable is very poor.

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

With the passage of the 1935 National Labor Relations Act (NLRA), Congress, for the first time, granted organized labor statutory sanction to get workers fired for refusal to join a union. The reaction to this change in the law followed and gave rise to the movement to curb the additional power bestowed upon the unions at the state level. By the time of the Taft-Hartley Amendments to the 1935 NLRA in 1947, which affirmed states' right to so-called "Right-to-Work (RTW) laws", 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 RTW laws. Figure 1 plots these states and the years the laws were enacted. Note that with the exception of Idaho and Oklahoma, which passed these laws in 1985 and 2001 respectively, all other states enacted them between the mid 1940s and early 1970s. This timing made RTW laws harder to evaluate because of either data limitations (especially true for earlier implementations) and/or the absence of proper method of evaluation that can deal with problems generated by a few number of late adopter states and the extended period of time between the passage of bills in these states.

Past research pertaining to RTW laws primarily 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 (for example, state level data) and treating RTW laws as exogenously determined, Hirsch (1980), Warren and Strauss (1979) obtain a negative effect 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 RTW laws on state level unionization (Farber 1984 and Lumsden and Peterson 1975) with a prominent exception of Ellwood and Fine (1987). 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 other outcomes such as the extent of freeriding, wage rates and employment levels. Freeriders are those employees who are covered by collective bargaining agreements but are not union members and the major difference between members and covered nonmembers is the payment of union dues. Since employees, in RTW states, under the collective bargaining agreements are not required to join unions, RTW 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 is that freeriding is 6-10% higher in RTW states than in non-RTW states (Moore 1998). As for wage effects of RTW laws, the findings are also mixed. Some studies find significant positive significant effects, some find none and some find significant negative effects of RTW laws on average wages (see, for example, Garofalo and Malhotra 1992, Mishel 2001 and Greer 2004).

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 RTW laws, but rather used that to proxy for a state's business climate. That is, a state is described as probusiness if it is a RTW one and anti-business otherwise. In order to overcome the potential unobserved confounders due to systematic statewide differences, Holmes examines the change 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. On average, he finds that when one crosses the border into a probusiness state, the manufacturing employment, on average, increases approximately by one-third. It is important to emphasize that this increase reflects several probusiness policies adopted by RTW states not just the law itself.

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 a very few number of groups undergoes a treatment (for example, a policy or legislative change) in a 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 right-to-work laws) after a time t. Let Y_{it}^N denote the outcome that would be observed for any state i ($i = 1.....J+1$) at time t ($t = 1....T$) 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, which implicitly implies that the intervention has no effect before the implementation period; for $t \in \{1, ...T_0\}$ and all $i \in \{1, ...J+1\}$, 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 outcomes of the unexposed states are not affected by the intervention implemented.

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 is

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

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 describe 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

$$D_{it} = \left\{ \begin{array}{ll} 1 \quad if \quad i = 1 \quad and \quad t > T_0 \\ 0 \qquad otherwise \end{array} \right\}$$

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

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

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 as

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

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 $W = (w_2, ..., w_{J+1})$ such that $w_J \geq 0$ for $j = 2, ..., J+1$ and $w_2 + ... + w_{J+1} = 1$. 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 $(w_2^*, ..., w_{J+1}^*)$ such that

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

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

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

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

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

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 $(Y_{11}, ...Y_{1T_0}, Z_1')$ belongs to the convex hull of $\{(Y_{21}, ...Y_{2T_0}, Z_2'), ...(Y_{J+11}, ...Y_{J+1T_0}, Z_{J+1}')\}$. If no such weights exist, however, the synthetic control state is selected so that equation (2) holds approximately. For the cases that even this approximation is not possible because $(Y_{11}, ...Y_{1T_0}, Z_1')$ falls far from the convex hull of $\{(Y_{21}, ...Y_{2T_0}, Z_2'), ...(Y_{J+11}, ...Y_{J+1T_0}, Z_{J+1}')\}$, 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

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

would provide an unbiased estimator of Y_{1t}^N. But of course, choosing the synthetic control in this manner is not feasible because $u_1 ... u_{J+1}$ are not observed. However, as shown in Abadie et al. (2010), equation (1) implies that a synthetic control can fit Z_1 a long set of pre-intervention outcomes $Y_{11}, ... Y_{1T_0}$ only as long as it fits Z_1 and u_1 and thus, equation (4) holds.

3.2 Implementation and Inference

The crucial aspect of the synthetic control model is the assignment of the weights to the control units. Let the $(T_0 \times 1)$ vector $K = (k_1, ..., k_{T_0})$ define a linear combination of pre-intervention outcomes: $\bar{Y}_i^K = \sum_{s=1}^{T_0} k_s Y_{is}$. For instance, if $k_1 = k_2 = ... = k_{T_0} = 1/T_0$, then the value of the outcome variable is the simple average of the pre-intervention outcomes; $\bar{Y}_i^K = \frac{1}{T_0} \sum_{s=1}^{T_0} k_s Y_{is}$. Consider M of such combinations denoted by the vectors $K_1, ... K_M$. Let $X_1 = (Z_1, \bar{Y}_1^{K_1} .... \bar{Y}_1^{K_M})$ 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 $X_0 = (Z_j, \bar{Y}_j^{K_1} .... \bar{Y}_j^{K_M})$. 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 $j = 2, ..., J + 1$ and $w_2 + ... + w_{J+1} = 1$ where V is a diagonal matrix with nonnegative components, which 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 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 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 had an impact, the actual estimate is expected to be large relative to the distribution of the placebo estimates.

4 Empirical Results

Below, we initially present the results for Oklahoma for all the outcome variables. For Idaho we are only able to present the results for the selected outcomes because of data limitations. 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 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 maximum likelihood approach. Conditional on V matrix, then, the W^* is set to produce the best counterfactual if Oklahoma or Idaho had not adopted RTW laws.

4.1 The Case of Oklahoma

4.1.1 Private Sector Unionization and Coverage Rates

The synthetic control that resembles Oklahoma the most, prior to the enactment of RTW laws, 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. Table A1 in the appendix displays the pre-intervention characteristics of actual Oklahoma, synthetic Oklahoma and all the other non-RTW states. Prior to the passage of the law, the average gap across characteristics between 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 unionization rates from 1983 to 2007 for the actual and synthetic Oklahoma and highlights several important points. First, consistent with the overall trend observed in the U.S. in the last three decades, the private sector unionization rates has been declining in Oklahoma as well. Next, even though it is not perfect, the unionization rate for the synthetic Oklahoma track the trajectory for almost the entire pre-intervention period with a mean squared prediction error (MSPE) of 0.58.[1] 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 RTW laws on private sector unionization 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 unionization rate would be 1% larger. Taking the average pre-treatment period of Oklahoma's unionization 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 pre-intervention period.

A closely related and explored outcome variable in the RTW literature is the private sector coverage rate. Consonant with its relation to unionization rate, the synthetic control for the private sector coverage rate is

[1] The 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.

constructed using the same states though with slightly different weights; Colorado (46.4%), followed 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 unionization rates, which is also observable in terms of magnitudes. The average post-intervention gap is just above 1% and the average pre-treatment period of Oklahoma coverage rate is $\bar{Y}_{pre-intervention}$ =7.5% and this corresponds to a roughly $\frac{\bar{\alpha}}{\bar{Y}_{pre-intervention}}$ =13.8% reduction with respect to 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" states that 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 RTW law, 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" suggest that RTW laws weaken the bargaining power of unions by reducing their membership and 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 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 unionization/coverage. The Freerider Hypothesis does not seem to hold as well. If the density of freeriders were to increase after the adaptation of RTW laws, we would expect the reduction in unionization rate to be larger than it is for coverage rates. Sobel (1995) estimates that not more than one-third of covered nonmembers are true freeriders and indicate that an elimination of RTW laws would have a modest effect on the extent of unionization. Our findings support this conclusion and leaves 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 significancy 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 RTW to one of the 26 states ,shifting Oklahoma to control pool, as if one of the control units adopted the law in 2001. We then estimate the effect of RTW for each placebo study. For comparison and rankability purposes, we compute the relative size of the average effect ($\bar{\alpha}$) to the average pre-treatment private sector unionization and coverage rate ($\bar{Y}_{pre-intervention}$). Apart from this, following Abadie et al. (2010), we can use the MSPE as a tool to compare Oklahoma with placebo studies. The basic idea is to examine the distribution of the ratios of post/pre-intervention MSPE, which provides a measure of affinity between each state and its synthetic counterpart before and after the intervention. If the effect of intervention were to be nonrandom, we would expect the post/pre-intervention MSPE ratio to be large relative to placebos studies.

Panel A of Table 1 presents the significancy results for the private sector unionization. The first row of the panel yields the average effect to average pre-intervention unionization 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 of Oklahoma is only 1/27=0.037, which is less that 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 RTW laws had a negative impact on the private sector unionization rates in Oklahoma. Nevertheless, in the second column, the post/pre-intervention MSPE ratios are given. The picture, however, is not clear here. Oklahoma is ranked the 12$^{\text{th}}$ with a MSPE ratio of 2.64, while the median across the rest of the states is 2.12 (second column).

Panel B of Table 1 presents the results for the private sector coverage. As expected, the ranking are very much in line with that of unionization. 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 14$^{\text{th}}$ using the post/pre-intervention MSPE (second row).

4.1.2 Foreign Direct Investment and Manufacturing Employment

As for the FDI, the counterfactual that resembles the most for Oklahoma is built using a weighted combination three states: Montana (66.9%), California (26.8%) and West Virginia (6.3%). Table A2 in the appendix displays the pre-intervention characteristics of actual Oklahoma, synthetic Oklahoma and all the other non-RTW states and we observe much more affinity for Oklahoma and its synthetic counterpart than the rest of the non-RTW states in terms of pre-intervention characteristics. The middle left panel of Figure 2 plots the log of FDI from 1983 to 2007 for the actual and synthetic Oklahoma. The pre-intervention MSPE is 0.018 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, 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 RTW laws increases the FDI by 23.9% over the six year period, on average.[2] This is a large effect.

Panel C of Table 1 presents the significancy results for FDI. Looking at the first column of the Panel and using the impact rank, we observe that Oklahoma is ranked the 6$^{\text{th}}$ with a value of 23.9%. The median effect for the placebo studies is 2.22%. In terms of MSPE ratio, Oklahoma is ranked the 16$^{\text{th}}$ with a MSPE ratio of 3.09, while the median across the remaining 26 state is 3.88 (second column). Out of the 5 states that are higher impact ranked than Oklahoma, California's MSPE ratio is less than one indicating a very poor pre-intervention MSPE and Indiana has a lower MSPE 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%) and Table A3 presents the pre-intervention characteristics as usual. The middle right panel of Figure 2 depicts the manufacturing 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 average effect to average pre-intervention manufacturing employment rates, Oklahoma is ranked the 12$^{\text{th}}$ with a value of 1.1% and is ranked 20$^{\text{th}}$ with respect to MSPE ratio. Based on these results, it is not likely that RTW laws have any impact on the manufacturing employment rate at least in the *short run.* For Idaho, as shown below, the story is different.

4.1.3 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 RTW laws, an eyeballing of the figures do not reveal any discernible patterns and there are several post-intervention crossings between the two lines. Consonant with this, Oklahoma is the 8$^{\text{th}}$ (26$^{\text{th}}$) state in the impact (MSPE) ranking for per capita income with an average effect of 0.9% and is 18$^{\text{th}}$ (21$^{\text{th}}$) in the impact (MSPE) ranking for private sector average wage rate with an average effect of -0.8%.

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

Finally, we examine the effects of RTW laws on manufacturing unionization 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.

4.2 The Case of Idaho

We first present our estimation results for Idaho in Figure 3. Since our data on unionization and coverage rates and wages go back only to 1983, we do not use these outcome variables in our estimations for Idaho. Therefore, the state-level outcome variables we use in this case are personal income per capita, FDI, and fraction of workforce employed in manufacturing sector. We did not find any evidence that the RTW law enacted in Idaho in 1985 had an impact on FDI or per capita income using the synthetic control method (top left and bottom panel). The effect of this law on the share of manufacturing employment, however, seems to be significant-an average post-intervention gap of 2.5 percentage point or 17 percent of the average manufacturing share of non-farm private employment. Therefore, we will only focus on this outcome variable in the rest of discussion of the results for Idaho. Only five states contribute to the construction of synthetic Idaho: Montana (52.8%) and Maine (26.3%), Minnesota (13.2%), Michigan (7.5%), and New Mexico (0.2%). There are several patterns that are revealed in top right panel of Figure 3. First, the employment share of the manufacturing had been declining in Idaho as is the case for the rest of the United States. Second, the divergence between the actual Idaho and its synthetic counterpart starts a couple of years before the enactment of the RTW bill in 1985. We suspect that this is because of the "anticipation affect" that may have led employers to keep existing (or to create additional) manufacturing jobs they would have not otherwise done, even before the enactment of the law. Finally, the effect of the RTW law in Idaho on manufacturing share of the employment does not disappear over time, which gives our findings a greater credibility.

The impact we find for Idaho, the average post-intervention gap of 2.5 percentage points, is not small value as we have shown before. It is about 17 percent of the manufacturing share of the state workforce on average and about ten thousand manufacturing jobs saved or added per year over the sample period. But the question is whether it is large enough to be more than random. To test this we conduct same series of placebo studies and apply the synthetic control analysis to the non-RTW states in "control pool" for Idaho. The results from this analysis are displayed in Panel A of Table 2. Whe we compute the relative size of the post-intervention gap to average pre-pre-interventaion manufacturing share of non-farm private employment, Idaho ranked the second with 14.8%. Only the effect for Wisconsin seems to be higher with about 16.2%. The median value for the rest of the states in the donor pool is -1.4%. When we rank states according to the size the MSPE ratios, there are 6 states with larger MSPE ratio, with median ratio of 35 as opposed to 24 for Idaho, in the control pool consisting of 26 states. The median ratio for the rest of the control states is 3.7. Recall that from Figure 3, we failed to find any evidence on the impact of the RTW law in Idaho on FDI and per capita income. The panels B and C of Table 2 shows the corresponding values and rankings of Idaho for these outcome variables. In terms of the RMSE ratio, Idaho is the last for FDI with the value of 0.43 and ranked the 23rd for personal income per capita with the value of 1.20 (second columns of Panels B and C).

We believe the evidence we presented so far convincingly show that the implementation of the RTW law in Idaho had a positive impact on manufacturing share of non-farm private employment while we find no evidence of the law's impact on either per capita personal income or FDI. However, we must emphasize that our pre-treatment fit for FDI variable is very poor for Idaho and our finding for this outcome variable should be taken with caution at best.

5 Conclusion

The effects of RTW laws have been the focus of a controversial policy discussion since the first wave of states enacted these laws in the mid-1940s. In this paper, we evaluated the impact of these laws on several economic outcomes in Idaho and Oklahoma, two states that enacted the laws the last. In doing so, we used a novel estimation methodology recently introduced by Abadie et al. (2010), which allowed us to identify, under certain assumptions, the potential effects in a single state. Our results suggest that RTW laws are neither as effective as their advocates claim nor appear to be detrimental to the well being of workers as their opponents declare. For Oklahoma, in particular, we found that the RTW law passed in Oklahoma in 2001 only affected private unionization and coverage rates and, to an extent possibly, FDI. Our results on private unionization and coverage could rule out the "Taste Hypothesis" and the "Free Rider Hypothesis" while providing evidence in favor of the "Bargaining Hypothesis", which suggests that RTW laws weaken the bargaining power of unions by reducing their membership and ability to conduct strikes. For the other outcome variables- manufacturing share of private non-farm employment, personal income per capita, private sector average wage rate, and manufacturing sector average wage rate- we did not find any discernable after the RTW law was enacted.

While we did not find any evidence that the RTW law enacted in Idaho in 1985 had an impact on personal income per capita or FDI, the effect of this law on manufacturing employment seems to be significant. Between 1986 and 2000, the share of the manufacturing employment, on average, appears to be 2.5 percentage points higher, which means about ten thousand manufacturing jobs saved or added per year over the sample period, because of the implementation of the RTW law in Idaho. Recall that our results suggested that the implementation of the RTW law in Oklahoma had a potentially positive impact on FDI. We are, however, not surprised with this difference in our findings for the two states. The pre-treatment fit of FDI variable for Idaho is too poor to say anything conclusive about the effect of this variable. This is most likely for the fact that Idaho is one of the states with least FDI over time period we exploit to construct our synthetic control. This generates a significant problem for the synthetic control method because the method minimizes the distance between a convex combination of the outcome from different states and actual pre-treatment outcome in choosing the best counterfactual synthetic state.

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Table 1: Effects of RTW Laws on State Level Outcomes: The Case of Oklahoma

		Placebo Median	Placebo Median	Placebo Median
Panel A: Private Sector Unionization Rate	Oklahoma	All	States with Higher MSPE	States with Lower MSPE
Average Effect/Pre-Intervention Average (Rank)	-14.5% (1st)	-1.55%	-6.0%	-0.5%
RMSE Ratio (Rank)	2.64 (12th)	2.12	9.33	1.02
Panel B: Private Sector Coverage Rate				
Average Effect/Pre-Intervention Average (Rank)	-13.8% (1st)	0.75%	1.1%	0%
RMSE Ratio (Rank)	2.57 (9th)	1.60	9.32	1.09
Panel C: Log of Foreign Direct Investment				
Average Effect (Rank)	23.9% (6th)	2.2%	-0.99%	3.14%
RMSE Ratio (Rank)	3.09 (16th)	3.88	7.60	1.77
Panel D: Manufacturing Employment Rate				
Average Effect/Pre-Intervention Average (Rank)	1.1% (12th)	0.7%	1.5%	0%
RMSE Ratio (Rank)	1.27 (20th)	4.14	7.79	0.36

NOTES: The placebo state values are based on 26 non-RTW states.

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

Panel A: Manufacturing Employment Rate	Idaho	Placebo Median All	Placebo Median States with Higher MSPE	Placebo Median States with Lower MSPE
Average Effect/Pre-Intervention Average (Rank)	14.78% (2nd)	-0.72%	11.89%	-1.73%
RMSE Ratio (Rank)	24.14 (6th)	6.24	35.1	3.73
Panel B: Log of Foreign Direct Investment				
Average Effect (Rank)	39.45% (12th)	8.13%	8.13%	N/A
RMSE Ratio (Rank)	0.43 (27th)	7.68	7.68	N/A
Panel C: Log of Per Capita Income				
Average Effect (Rank)	0.77% (19th)	0.03%	1.52%	-9.37%
RMSE Ratio (Rank)	1.20 (23rd)	3.80	5.75	1.10

NOTES: The placebo state values are based on 27 non-RTW states.

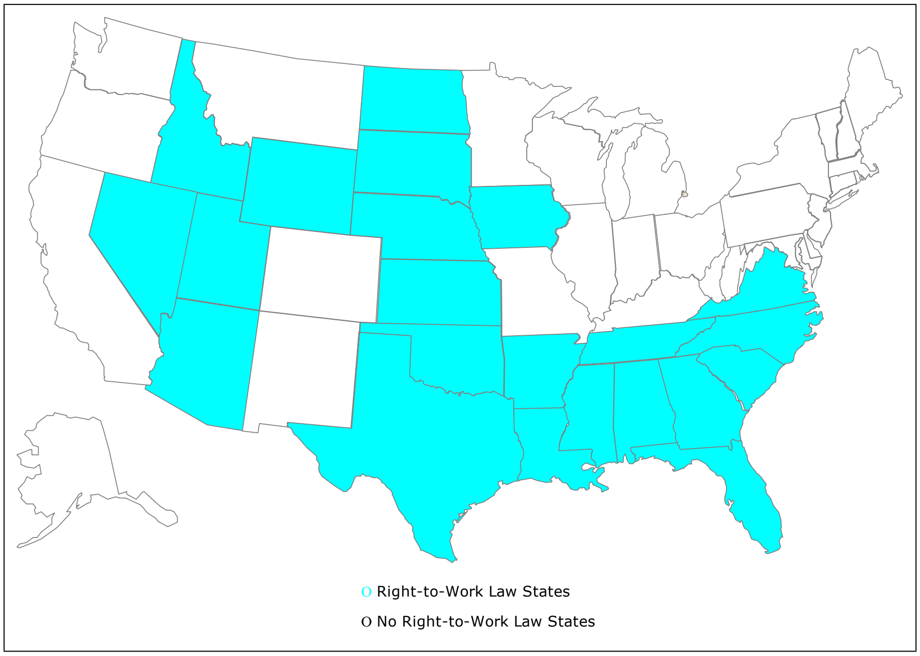

Figure 1: US States by Right-to-Work (RTW) Laws

Note: Oklahoma's RTW legislation was enacted in 2001; Idaho's in 1985 and all other states' laws were enacted in late 1940s or early 1950s.

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

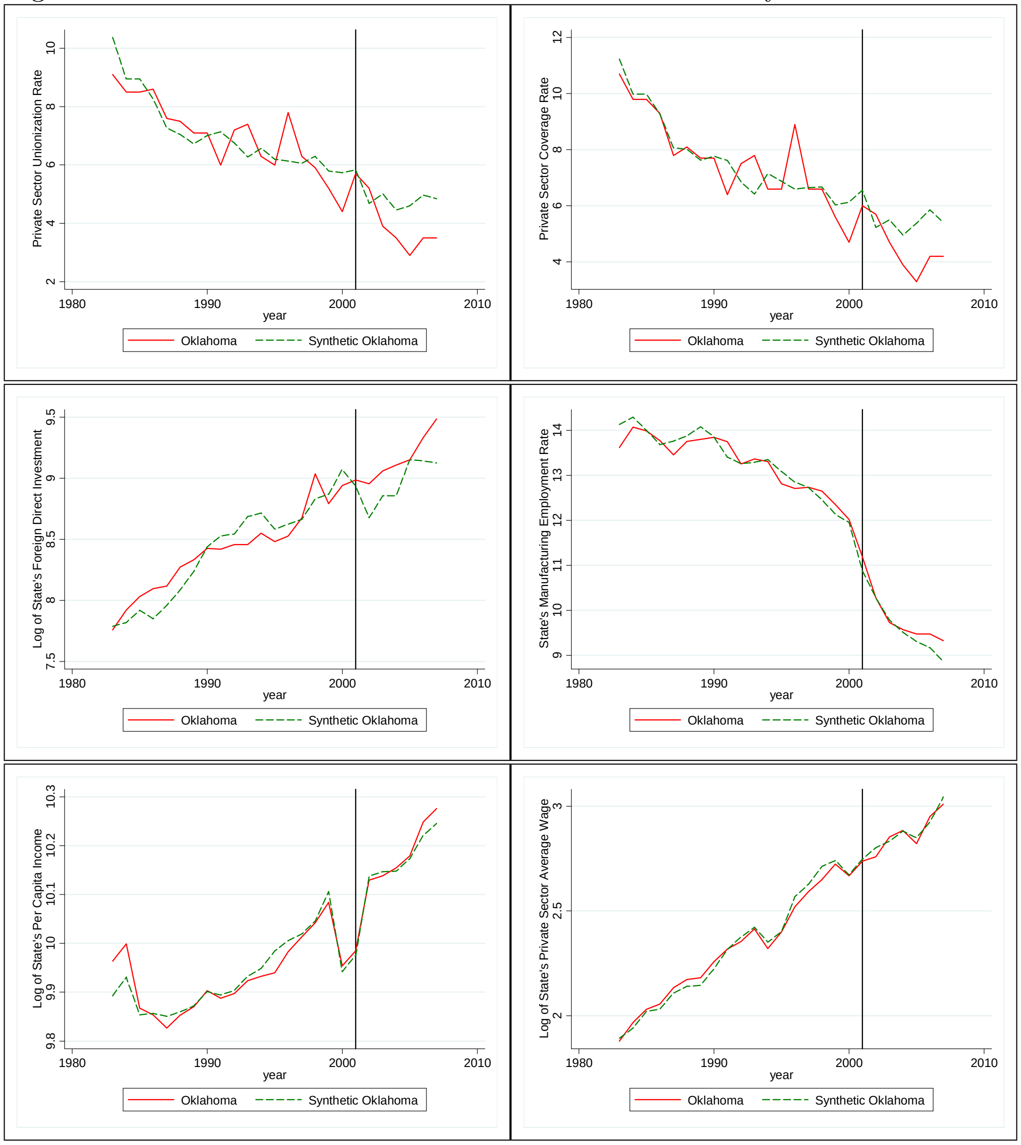

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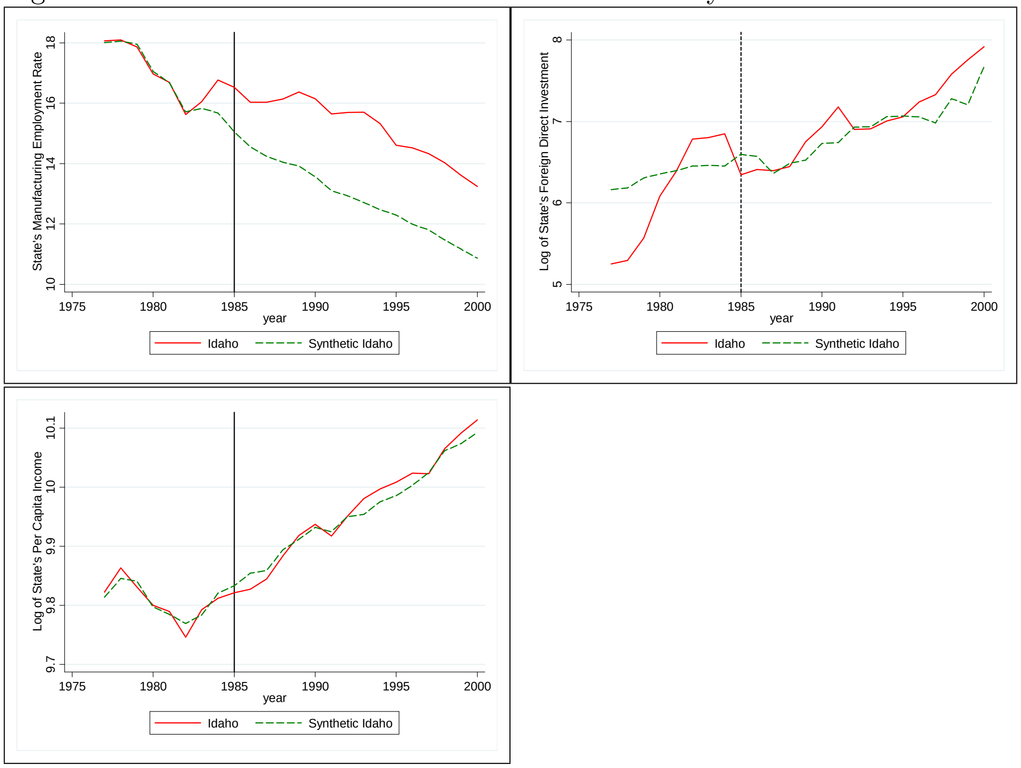
Figure 3: Trends in State Level Outcomes: Idaho vs. Synthetic Idaho



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