What Do Right-to-Work Laws Do? Evidence from a Synthetic Control Method Analysis

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Abstract

Using the case study of Oklahoma and a recently developed econometric technique, we examine the impact of right-to-work (RTW) laws on state-level labor market outcomes. Our results show that the passage of RTW laws in Oklahoma decreased private sector unionization rates. Several other state outcomes including total employment rate and private sector average wages, on the other hand, were not affected by RTW laws. The findings for the private sector generally carry over to the manufacturing sector. © 2015 by the Association for Public Policy Analysis and Management.

INTRODUCTION

Recent enactments of right-to-work (RTW) laws in Indiana and Michigan have rekindled the debate over the role of RTW laws on state economies and labor organizations. Opponents of the laws assert that RTW laws lead to lower wages, lower safety and health standards that protect workers on the job, and cause workers to receive union representation without incurring the cost. In his December 2012 address to union workers in Michigan, President Obama stated: "...these so-called right-to-work laws, they don't have to do with economics, they have everything to do with politics" (Jackson, 2012). Advocates contend that these laws create jobs, lead to higher wages, improve union accountability, and are morally right because they do not compel individuals to support a cause in which they do not believe. For example, an editorial in The Wall Street Journal associates RTW laws with the massive job growth gap observed in the 1990s between Ohio and Texas (The Wall Street Journal, 2008).[1]

There were some nonnegligible differences between RTW and non-RTW states in terms of economic progress in the second half of the 20th century. For example, relative to non-RTW states, RTW states on average grew 1.5 percentage points faster over this time period. Focusing on more recent trends, we see that per capita income and private sector employment growth rates were higher in the RTW states in the last three decades (Table 1). We observe similar favorable statistics in terms of total unemployment and manufacturing employment growth rates as well. These

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[1] In the 1990s, Texas added more than one and a half million new jobs, while Ohio lost more than 10,000 jobs. In addition to RTW laws, the editorial attributes the job growth gap to NAFTA and the absence of state income tax in Texas.

Table 1. Average trends in various state-level outcomes.

|	RTW states (1)	Non-RTW states (2)
Per capita income growth rate	1.82	1.67
Employment growth rate	2.57	1.87
Unemployment rate	5.39	5.73
Manufacturing employment growth rate	−2.26	−3.12

Note: Average trends in state-level outcomes from 1980 to 2007.

remarkable differences across states have led researchers to investigate the economic effects of RTW laws.

From a research standpoint, the evidence regarding the role of RTW laws on state economies is mixed at best. Some studies find significant effects of RTW laws on various state outcomes, while others find no effect (see, e.g., Farber, 2005; Hirsch, 1980; Holmes, 1998; Lafer & Allegretto, 2011). The lack of consensus among previous studies is likely to be driven by challenges in identifying the effects of RTW laws. Even in the absence of a causal relation, it is possible to observe significant correlations between RTW laws and state outcomes due to systematic differences across states. For example, it is a well-known fact that some states have more hostile attitudes toward unions and, among many other factors, these state-specific attitudes may have facilitated the passage of RTW laws in these states. Omission of these attitudes in the estimation of the impact of RTW laws, say, on private sector unionization is likely to yield misleading results. Furthermore, the timing of RTW law enactments adds further challenges. After the first wave between the mid-1940s and mid-1950s, only a few states adopted RTW laws with long intervals between each instance. In this respect, the usual regression-based approach may not be the best fit in the current context.

The purpose of this article is to examine the impact of RTW laws on state-level labor market outcomes in Oklahoma, which passed an RTW amendment to the state constitution in 2001. The passage of RTW laws in Indiana and Michigan in 2012 set aside, Oklahoma is the most recent RTW state with comprehensive pre- and post-treatment data on our outcomes of interest. One drawback of a case study based on Oklahoma is that it is a relatively small state, and the private sector unionization rate was low compared to the rest of the nation, even before the passage of RTW laws. That being said, however, manufacturing sector unionization rates (an industry with traditionally high union density) prior to adoption of RTW laws were similar to the average of non-RTW states, while they were significantly higher than the average in RTW states. For example, in the year 1998, manufacturing sector unionization was slightly above 16 percent in Oklahoma, compared to an average of 17.5 percent in non-RTW states and an average of 10.3 percent in RTW states. Therefore, the study of Oklahoma may still help us to infer what other non-RTW states may experience with the passage of these laws.

To address the aforementioned identification problems, we use the synthetic control method, which was introduced in Abadie and Gardeazabal (2003) and later extended in Abadie, Diamond, and Hainmueller (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 very few) unit(s) undergoes a treatment in a given sample period. The basic idea is to construct a weighted combination of control units based on preintervention characteristics, which is expected to provide a better counterfactual than a single control unit (or a simple average of all control units) for the treated unit(s). The synthetic control method
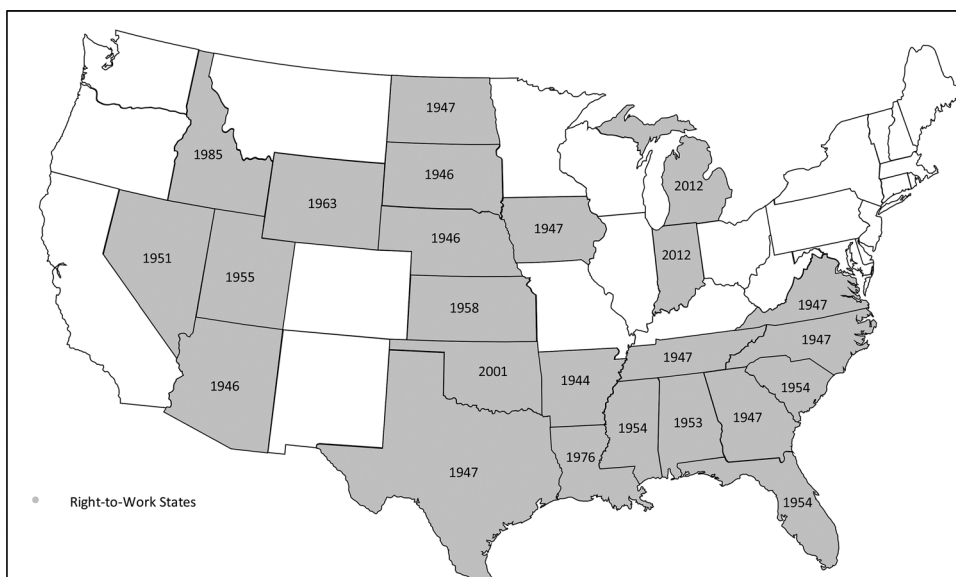
Figure 1. U.S. States by RTW Laws and Enactment Years—Recent Enactments (Indiana and Michigan) in 2012 are Included.

generalizes the usual difference-in-differences (DD) approach by allowing for time-variant unobserved confounders. In this respect, the estimates of the policy intervention obtained through the synthetic control method are robust not only to time invariant unobservables, but also to unobservable confounders that vary over time as long as these confounders vary in a similar way after the intervention.

Using the synthetic control method and aggregate level data from 1983 to 2007, we find that the passage of RTW laws in Oklahoma significantly decreased private sector unionization rates. On the other hand, at least in the short run, we do not find evidence that RTW laws had any impact on total employment rate or on average private sector wages. Further examination of the impact of RTW laws in the manufacturing sector alone does not alter our results. Several robustness checks support our findings.

BACKGROUND

Oklahoma and the RTW Laws

In 1935, with the passage of the Wagner Act, Congress granted organized labor statutory sanction to fire employees for refusal to join a union. The law gave rise to a movement to curb the additional power bestowed upon 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 RTW laws), a contract provision that requires all employees to join and pay dues to the union. By the time of the 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 and until the most recent cases of Indiana and Michigan in the year 2012, 22 states have passed RTW laws. Figure 1 shows these states and the years the laws were enacted.

Although the passage of RTW laws in Oklahoma is relatively recent, the debate has gone on for decades, with the first referendum occurring in 1964. An important

milestone in the history of the RTW laws in Oklahoma was the re-election of Governor Francis Keating in 1998. In his inaugural address, Keating, who was a strong supporter of RTW laws, set the passage of RTW laws as one of the central goals of his second term. After Republicans gained many seats in the House and Senate in 2000, both houses voted to submit RTW laws to a popular vote. In September 2001, Oklahomans passed an RTW amendment to the state constitution by a margin of 54 percent to 46 percent in a referendum, which made Oklahoma the 22nd RTW state. Almost immediately, several unions filed a federal lawsuit seeking to prevent the amendment's implementation. These attempts, however, did not overturn the amendment.

At the time Oklahoma passed the RTW laws, the state's economy was in relatively good shape. The unemployment rate in 2000 stood at 3 percent and it was not only well below the national average but also lower than 18 of the then 21 RTW states. Over the decade prior to RTW laws, partly owing to the state's concentration in the oil and gas industries, overall employment grew by 22 percent, which was also ahead of the national average. During the same time period, per capita income grew by 15 percent, compared to 16 percent growth for the country as a whole.

Previous Research

As noted, obtaining convincing estimates for the effects of RTW laws on state outcomes is a daunting task. The major obstacle stems from challenges in distinguishing the effect of RTW laws from other observable and unobservable state characteristics. The findings in the literature on various outcomes are mixed at best. Using aggregate level data and treating RTW laws as exogenously determined, Hirsch (1980) and Warren and Strauss (1979) obtain negative effects of RTW laws on private sector unionization rates. Studies that control for unobserved factors, on the other hand, usually find no significant effect of RTW laws on state-level private sector unionization (see, e.g., Farber, 1984; Lumsden & Peterson, 1975). Focusing on new union organizing activity, Ellwood and Fine (1987) show that the passage of RTW laws decreased new unionization activity in the short run, more than 45 percent in the first five years and around 30 percent in the second five years after the legislation. The adverse effects of RTW laws on union activity, however, dissipate after a decade or so.[2] Exploiting the passage of RTW laws in 1985 in Idaho, Dinlersoz and Hernández-Murillo (2002) observe large declines in the unionization rates after the passage of RTW laws. Turning to wage and employment effects, some studies find positive and significant effects, while others find no to negative effects of RTW laws (see, e.g., Moore & Newman, 1985, for a review; Dinlersoz & Hernández-Murillo, 2002; Farber, 2005; Kalenkoski & Lacombe, 2006; Reed, 2003).[3]

Among many others, one other notable study is Holmes (1998), which examines the location decision of manufacturing entrepreneurs. The author did not specifically explore the isolated effect of RTW laws, but instead used it to proxy for a state's business climate. As such, a state is described as pro-business if it is an RTW one and antibusiness otherwise. In order to circumvent the potential confounding effects arising from systematic differences across states, Holmes examines the change in manufacturing activity along the borders of adjacent states with different RTW laws. The intuition behind this estimation strategy is that the unobserved factors (i.e., attitudes toward unions, fertility of the soil) will be very similar within a given

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[2] Ellwood and Fine (1987) used new membership flow into unions, rather than the extent of unionization, as their measure of union activity.
[3] There are also a few studies examining the effects of RTW laws on other outcomes such as stock market movements (see, e.g., Abraham & Voos, 2000).

distance from the border. Holmes (1998) finds that when one crosses the border into a pro-business state, manufacturing employment on average increases roughly by one-third. However, it is important to note that this increase reflects several pro-business policies (i.e., low taxes, lax environmental regulations) adopted by RTW states and not just the law itself.

EMPIRICAL METHODOLOGY

In our discussion of the empirical methodology, we closely follow Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). In its simplest form, the idea of the synthetic control method is to construct a weighted combination of unexposed (control) units, which is expected to provide a better counterfactual for the (treatment) unit that is exposed to an intervention. To begin, consider that there are $S + 1$ states and suppose that only one state is continuously exposed to an intervention of interest after a time t. Let Y_{it}^N denote the outcome that would be observed for any state i ($i = 1, \ldots, S + 1$) at time t ($t = 1, \ldots, T$) in the absence of the intervention and T_0 be the number of pre-intervention periods such that $1 \le T_0 < T$. Let Y_{it}^E denote the outcome of i after being exposed to the intervention of interest in periods T_0 to T. This setup implies that the intervention has no effect before the implementation period; for $t \in \{1, \ldots, T_0\}$ and all $i \in \{1, \ldots, S + 1\}$, we have $Y_{it}^N = Y_{it}^E$.

Prior to going further, it is crucial to note that the model relies on the assumption of no interference between units; 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 $\gamma_{it} = Y_{it}^E - Y_{it}^N$ and, borrowing from the common practice in the treatment literature, the observed outcome can be written as

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

where D_{it} is an indicator that takes the value of 1 if state i at time t is exposed to the intervention and zero otherwise. Let us define the only state that is continuously exposed to the intervention after T_0 ($1 \le T_0 < T$) as being the first state; then we have $D_{it} = 1$ if $i = 1$ and $t > T_0$ and zero otherwise. In this setup, we aim to estimate $(\gamma_{1T_0+1}, \ldots, \gamma_{1T})$ and the effect of the intervention for the first state for $t > T_0$ is

$$\gamma_{1t} = Y_{1t}^E - Y_{1t}^N = Y_{1t} - Y_{1t}^N.$$

Since Y_{1t}^E is observed, in order to estimate γ_{1t}, we need to estimate the counterfactual Y_{1t}^N. Following Abadie, Diamond, and Hainmueller (2010), suppose that Y_{1t}^N is given by a factor model

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

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, φ_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 $W = (\omega_2, \ldots, \omega_{S+1})$ with $\omega_S \ge 0$ for $s = 2, \ldots, S + 1$ and $\omega_2 + \cdots + \omega_{S+1} = 1$ and suppose there exists a vector $(\omega_2^*, \ldots, \omega_{S+1}^*)$ forming the synthetic control such that

$$\sum_{s=2}^{S+1} \omega_s^* Y_{s1} = Y_{11}, \ldots, \sum_{s=2}^{S+1} \omega_s^* Y_{sT_0} = Y_{1T_0}, \text{ and } \sum_{s=2}^{S+1} \omega_s^* Z_s = Z_1. \tag{2}$$

As long as the number of pre-intervention periods is large relative to the scale of transitory shocks and under the standard conditions, Abadie, Diamond, and Hainmueller (2010) show that

$$Y_{1t}^N - \sum_{s=2}^{S+1} \omega_s^* Y_{st} \approx 0. \tag{3}$$

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

$$\hat{\gamma}_{1t} = Y_{1t} - \sum_{s=2}^{S+1} \omega_s^* Y_{st} \quad \text{for } t \in \{T_0 + 1, \ldots, T\}.$$

It is important to note that equation (1) extends the usual DD approach as the model does not impose φ_t to be constant over time. As is well known, the traditional DD method allows for the presence of time-invariant unobserved confounders, and taking the time differences eliminates these unobservables. The synthetic control method, on the other hand, allows for the unobserved confounders to vary. As shown in Abadie, Diamond, and Hainmueller (2010), equation (1) implies that a synthetic control can fit Z_1 and a long set of pre-intervention outcomes $Y_{11}, \ldots, Y_{1T_0}$ only as long as it fits Z_1 and u_1.

The optimal weight vector W^* to construct the synthetic control is chosen to minimize the distance between the pre-intervention characteristics of the first state and the remaining control states.[4] To determine the significance of the estimated impact using the synthetic control method, Abadie, Diamond, and Hainmueller (2010) suggest the use of "placebo" or "falsification" tests. Similar to the classical permutation tests, the idea of the placebo test is to apply the synthetic control method to each control unit as if it 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.

Before describing the data and presenting the results, we must stress one potential limitation of our econometric approach. As noted, the synthetic control method hinges upon a data-driven procedure and counterfactuals used to construct the treated state absent of intervention can be outcome specific and time sensitive. Therefore, for the same treated unit, we may end up using different control states with different weights for each outcome of interest over the time period analyzed. Although it is unclear how this issue would affect the credibility of our results, we provide several sensitivity checks along with DD estimates below.

DATA

The data for this study are at the state level and come from several different sources covering a time span of 25 years from 1983 to 2007. Appendix A provides detailed information regarding these sources.[5] We choose 1983 as the initial year because it is the first year that the Current Population Survey (CPS) began collecting monthly

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[4] See Abadie, Diamond, and Hainmueller (2010) for a detailed discussion of the optimal weight vector selection.
[5] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

information for some of the important variables used in the analysis (e.g., union status).[6] To circumvent any potential confounding effects of the Great Recession, we restrict our analysis to prior to 2008.[7] Oklahoma adopted RTW laws in September 2001, leaving us with six years of post-intervention data. This period seems like a reasonable time span to observe the impact of the RTW laws on state-level outcomes, at least in the short run.[8]

For our main analysis, we restrict our effective sample to include only the pool of the 28 non-RTW states. This follows from the intuition inherent in the synthetic control method, which is to construct the best counterfactual from the pool consisting of unexposed units. Our state-level outcomes of interest are private and manufacturing sector unionization rates, employment–population ratio, manufacturing employment rate, and average hourly private and manufacturing sector wages.[9] The set of observed predictors (Z) includes the log of the state's population and total land square miles, log of per capita income, and the percentage of the state's labor force that is white, male, college graduates, and those residing in a metropolitan area. The observed predictors are averaged over the entire pre-intervention period; moreover, for all outcomes of interest, we augment the relevant lagged values biennially over the pre-intervention period. The average values of the covariates along with the lagged outcomes constitute our set of pre-intervention predictors for Oklahoma and all other states. The first two columns of Table 2 provide the mean of the selected pre-intervention predictors for Oklahoma and the non-RTW states.

Among all outcomes of interest, Figure 2 plots the raw data trends from 1983 to 2007 for manufacturing employment in Oklahoma and all other states disaggregated by RTW laws' status. We relegate all other raw data trends to Appendix B for the sake of brevity.[10] Overall, the trends in state outcome variables for Oklahoma largely track those of the rest of the nation. Even though economic variables are trending in the same direction, there are some important differences. For instance, we see that the downward trend in manufacturing employment observed in the last three decades is much flatter in Oklahoma (Figure 2). Given these differences in trends, imposing a common trend assumption and employing a simple DD approach may yield biased coefficient estimates.

EMPIRICAL RESULTS

Unionization

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: Colorado (72.2 percent), Vermont (23.6 percent), and New Mexico (4.2 percent). The rest of the non-RTW states are assigned zero weights.

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[6] Beginning 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.

[7] Several other papers employed a similar approach to minimize the confounding effects that may occur because of the Great Recession (see, e.g., Neumark & Muz, 2014).

[8] One other potential state for a case study would have been Idaho, which adopted RTW laws in 1985. However, the length of the preintervention years accompanied with the available data is likely to contradict with the assumptions of the synthetic control method and, therefore, we limit our attention only to Oklahoma.

[9] Wages are deflated by the year's consumer price index (2,000 = 100).

[10] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 2. Summary statistics: selected lagged outcome variables and preintervention characteristics.

Synthetic control predictors	Oklahoma (1)	Non-RTW states (2)	Synthetic control					
			Private sector unionization (3)	Manufacturing sector unionization (4)	Employment–population ratio (5)	Manufacturing employment (6)	Log of average private wages (7)	Log of average manufacturing wages (8)
Private sector unionization rate (2000)	4.40	10.53	5.77	…..	…..	…..	…..	…..
Private sector unionization rate (1992)	7.20	12.87	6.75	…..	…..	…..	…..	…..
Private sector unionization rate (1984)	8.50	16.96	9.00	…..	…..	…..	…..	…..
Manufacturing sector unionization rate (2000)	11.60	16.90	…..	13.17	…..	…..	…..	…..
Manufacturing sector unionization rate (1992)	17.50	21.85	…..	16.61	…..	…..	…..	…..
Manufacturing sector unionization rate (1984)	25.00	28.01	…..	24.25	…..	…..	…..	…..
Employment–population ratio (2000)	62.40	65.51	…..	…..	62.49	…..	…..	…..
Employment–population ratio (1992)	59.90	62.76	…..	…..	59.93	…..	…..	…..
Employment–population ratio (1984)	59.70	60.68	…..	…..	59.48	…..	…..	…..
Manufacturing employment rate (2000)	11.39	12.34	…..	…..	…..	11.28	…..	…..
Manufacturing employment rate (1992)	13.26	15.98	…..	…..	…..	13.24	…..	…..

Table 2. Continued.

| | | | Synthetic control | | | | | |
Synthetic control predictors	Oklahoma (1)	Non-RTW states (2)	Private sector unionization (3)	Manufacturing sector unionization (4)	Employment–population ratio (5)	Manufacturing employment (6)	Log of average private wages (7)	Log of average manufacturing wages (8)
Manufacturing employment rate (1984)	14.07	20.09	.....	.....	.....	14.04	.....	.....
Log of average private wages (2000)	2.65	2.83	.....	.....	.....	.....	2.64	.....
Log of average private wages (1992)	2.75	2.85	.....	.....	.....	.....	2.76	.....
Log of average private wages (1984)	2.96	2.96	.....	.....	.....	.....	2.92	.....
Log of average manufacturing wages (2000)	2.58	2.69	.....	.....	.....	.....	.....	2.58
Log of average manufacturing wages (1992)	2.64	2.68	.....	.....	.....	.....	.....	2.61
Log of average manufacturing wages (1984)	2.77	2.75	.....	.....	.....	.....	.....	2.76
Log of population	15.00	15.07	14.62	15.08	14.91	14.57	13.88	15
Log of total land square miles	11.14	10.31	10.99	9.41	11.28	11.12	11.79	11.01
Log of per capita income	9.99	10.22	10.16	10.19	10.03	10.00	10.02	10.19
White (%)	87.39	87.41	94.36	92.37	91.39	87.17	90.49	88.41
Male (%)	54.13	53.05	53.32	52.64	54.41	53.91	53.9	54.15
College graduate (%)	20.77	25.39	27.78	26.16	22.18	20.53	22.2	23.88
Living in metropolitan area (%)	59.17	68.50	65.63	77.67	60.26	46.45	33.33	73.75

Note: The variables are only a subset of those used in the analysis. The remainder are excluded in the interest of brevity. The full set of sample statistics are available upon request.
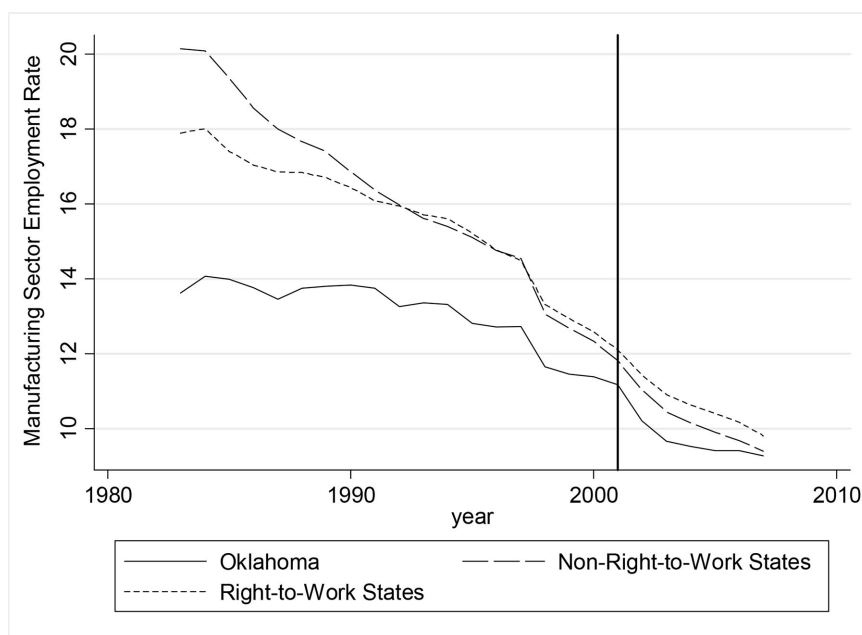
Notes: The panel contains the raw data trends (1983 to 2007) for Oklahoma and the average of RTW and non-RTW states.

Figure 2. Trends in Manufacturing Employment.

The third column of Table 2 displays the mean of the selected pre-intervention characteristics of synthetic Oklahoma. The average gap between the characteristics of Oklahoma and its synthetic counterpart is substantially smaller than it is between Oklahoma and control states (columns 1 through 3 of Table 2).

The top row of panel A of Figure 3 shows the private sector unionization rates from 1983 to 2007 for the actual and the synthetic Oklahoma and highlights several important points. First, consistent with the overall trend observed in the United States in the last three decades, private sector unionization rates have been declining in Oklahoma. Next, unionization rates for the synthetic Oklahoma track the trajectory closely for almost the entire pre-intervention period with a root mean squared prediction error (RMSPE) of 0.73.[11] Finally, right after the passage of the law, we observe a noticeable divergence between the two lines.[12] This finding provides tentative evidence of a negative impact of the RTW laws on private sector unionization. The average post-intervention gap is slightly above 1.35 percentage points. That is, if the state of Oklahoma had not adopted the RTW laws, private sector unionization rates would be 1.35 ($\bar{\alpha}$) percentage points larger. Taking Oklahoma's union membership rate of 4.4 percent ($Y_{OK,2000}$) in the year 2000 as our benchmark, the 1.35 percentage point gap corresponds to a roughly $\frac{\bar{\alpha}}{Y_{OK,2000}} = 30.6$ percent reduction in private sector unionization rates.

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[11] The preintervention 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 to 2001.
[12] Prior to the passage of RTW laws, we observe an initial decline in 1999 and a subsequent rebounding in 2000. The same pattern is observed for private sector unionization in Idaho. Dinlersoz and Hernández-Murillo (2002) attribute this pattern to evolving expectations about the eventual ruling of RTW laws.

Panel A. Private Sector Unionization.
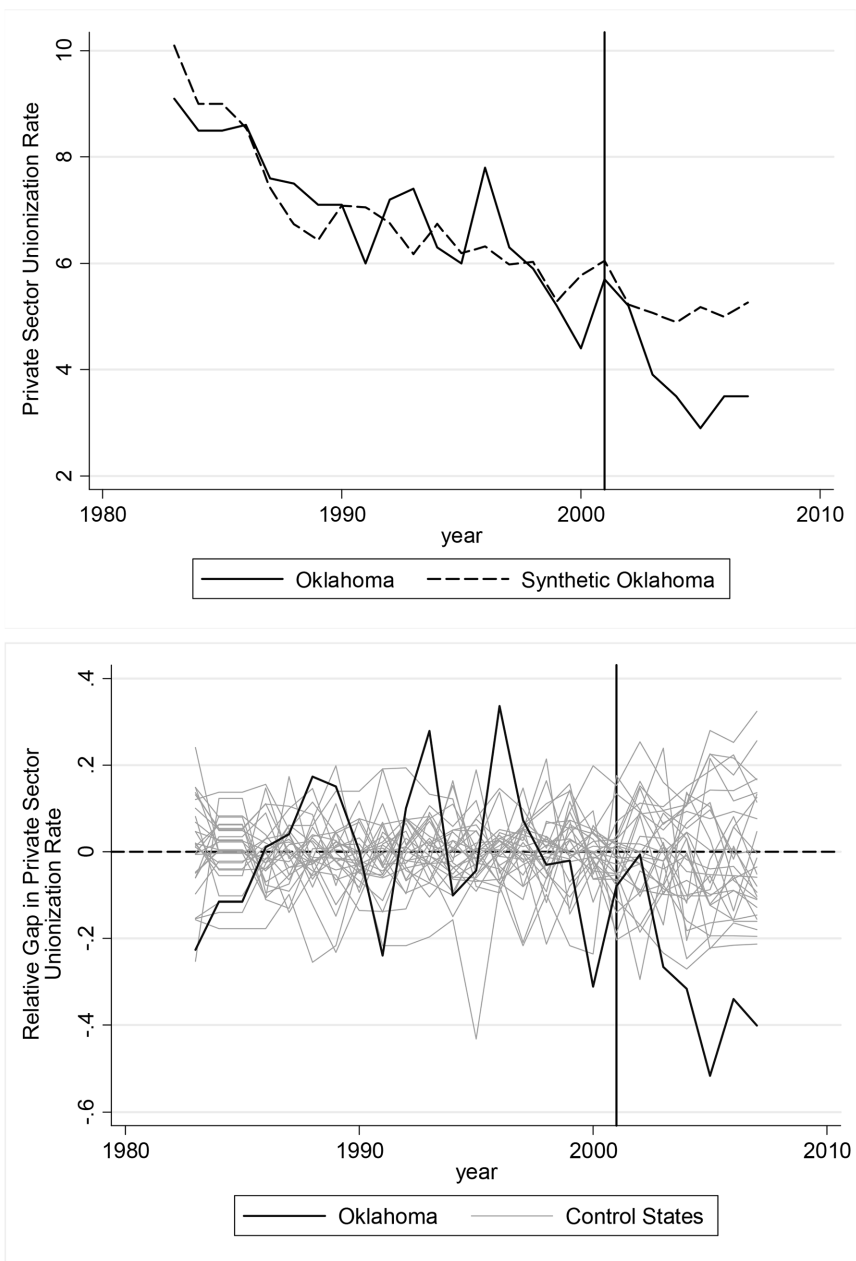
Notes: The top figures in each panel contain trends (1983 to 2007) for Oklahoma and synthetic Oklahoma while the bottom figures indicate the effects of RTW laws in Oklahoma and the placebo effects in all non-RTW states. Panels A–D express the effects relative to the corresponding outcome rates in the year 2000. The preintervention period is from 1983 to 2001. Wages are deflated by the year's consumer price index (2,000 = 100).

Figure 3. Trends in Various State Outcomes. Oklahoma vs. Synthetic Oklahoma.

Panel B. Manufacturing Unionization.
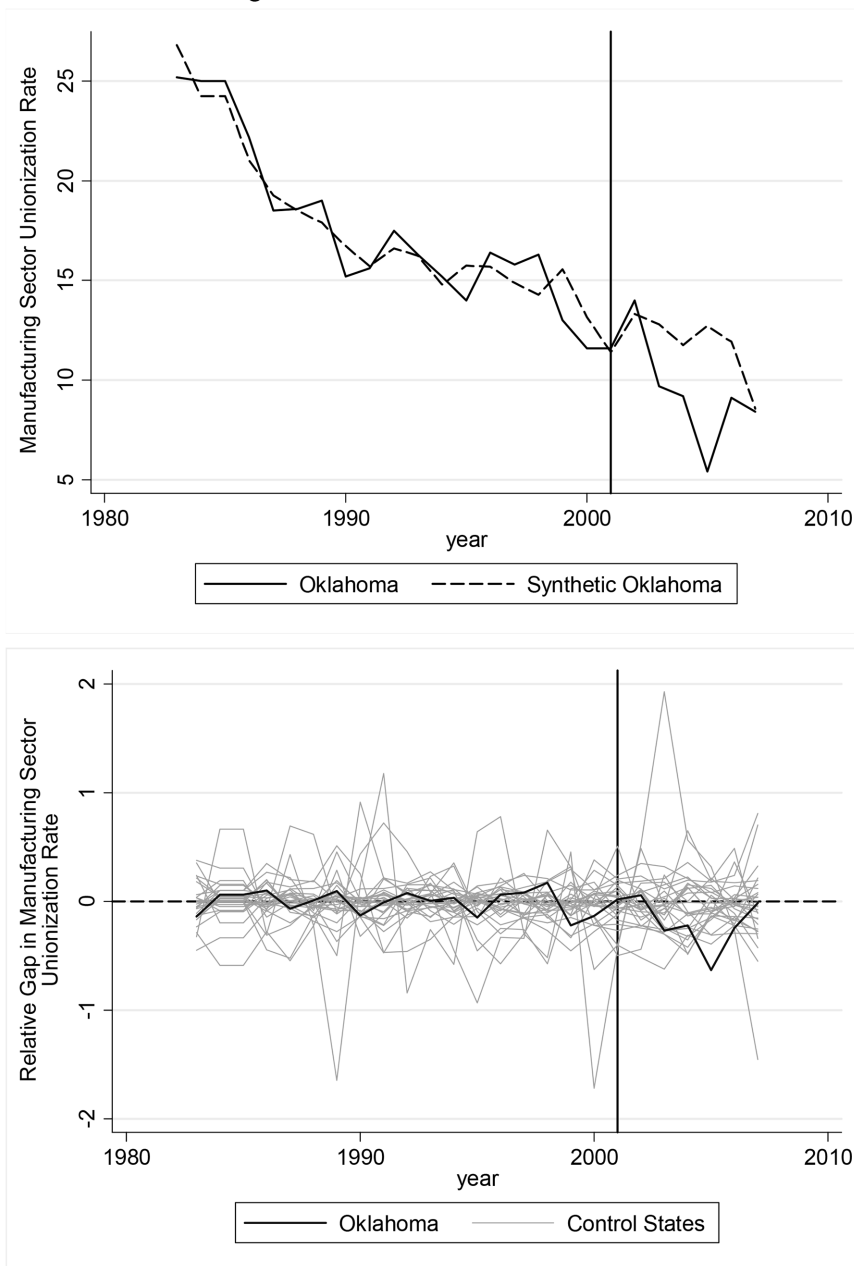
Figure 3. *Continued.*

Of course, a natural and an important question to ask at this point regards the precision of this estimate. To determine this, we conduct a series of placebo studies by iteratively applying the synthetic control method to all control states. In each iteration, we reassign the passage of the RTW laws to one of the 28 control states (shifting Oklahoma to the control pool) as if one of the control units had the intervention in 2001. We then estimate the effect of the RTW laws for each placebo

Panel C. Employment-Population Ratio.
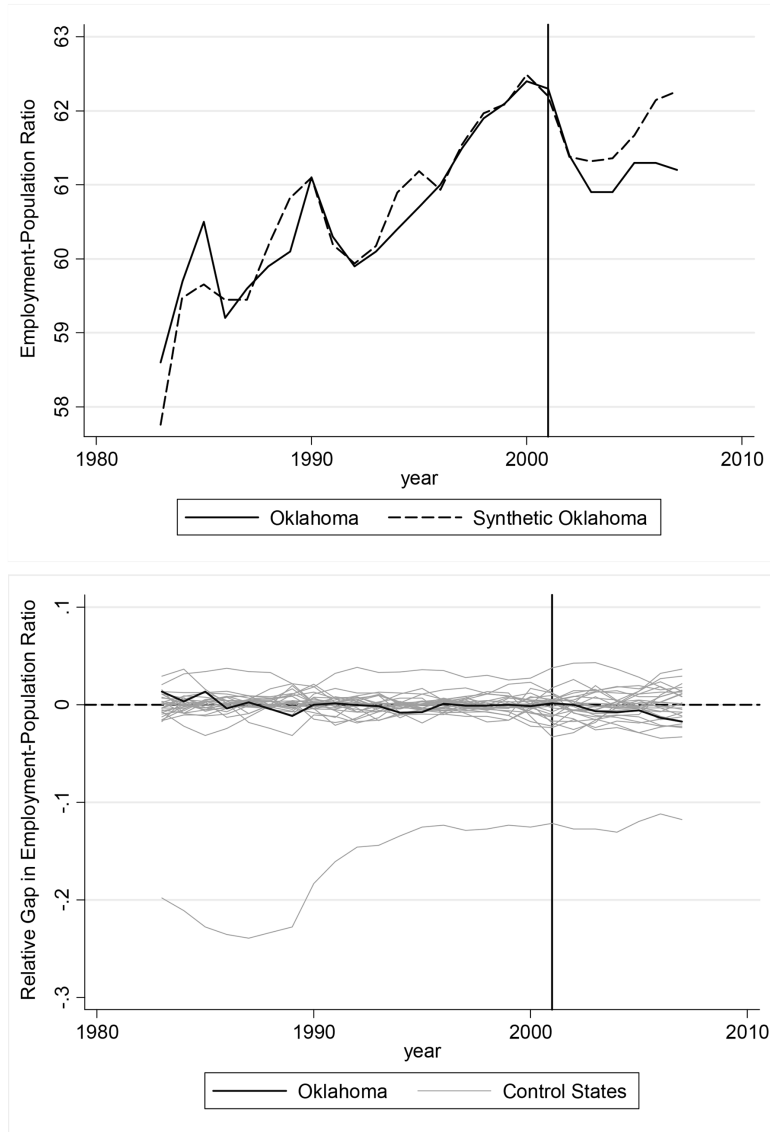
Figure 3. *Continued.*

study by taking the ratio between the post-intervention gap and the state's unionization rate in the year 2000 and we do this for each post-intervention year (i.e., $\frac{\alpha_{it}}{Y_{i,2000}}$, $i = 1, \ldots, S+1$ and $t = T_0 + 1, \ldots, T$). The bottom row of panel A in Figure 3 presents the results. The gray lines represent the effect of RTW laws on private sector unionization for each of the 28 states, while the superimposed black line denotes the effect for Oklahoma. As is visible from the figure, the effect line for Oklahoma is unusually large relative to the distribution of the lines for the control states, which

Panel D. Manufacturing Employment.
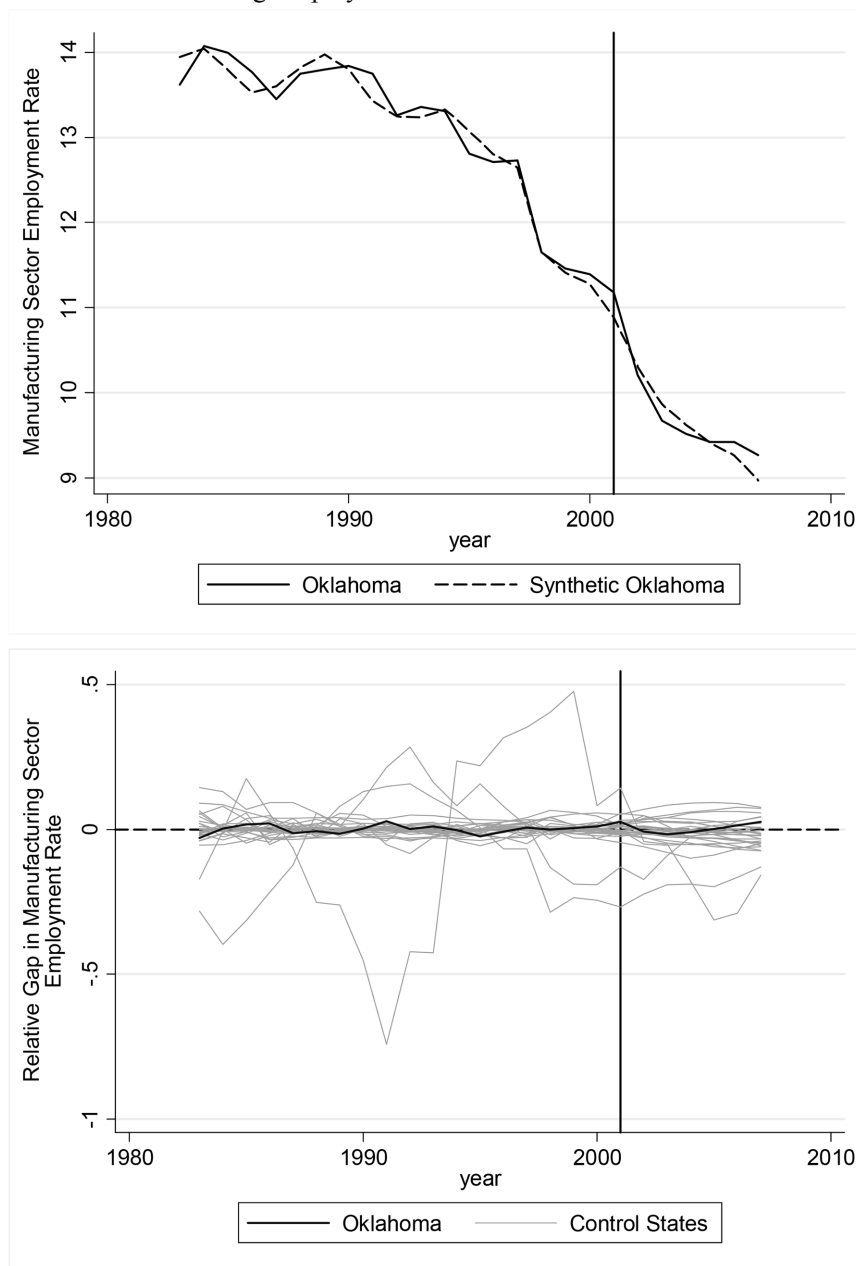
Figure 3. *Continued.*

is likely to indicate a "nonrandom" reduction in private sector unionization rates in Oklahoma after the adoption of RTW laws.[13]

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[13] It is important to note that we examine short-to-medium run effects of RTW laws on unionization. Ellwood and Fine (1987) provide evidence that unions can restore part of the decline resulting from RTW laws in the long run.

Panel E. Private Sector Average Wages.
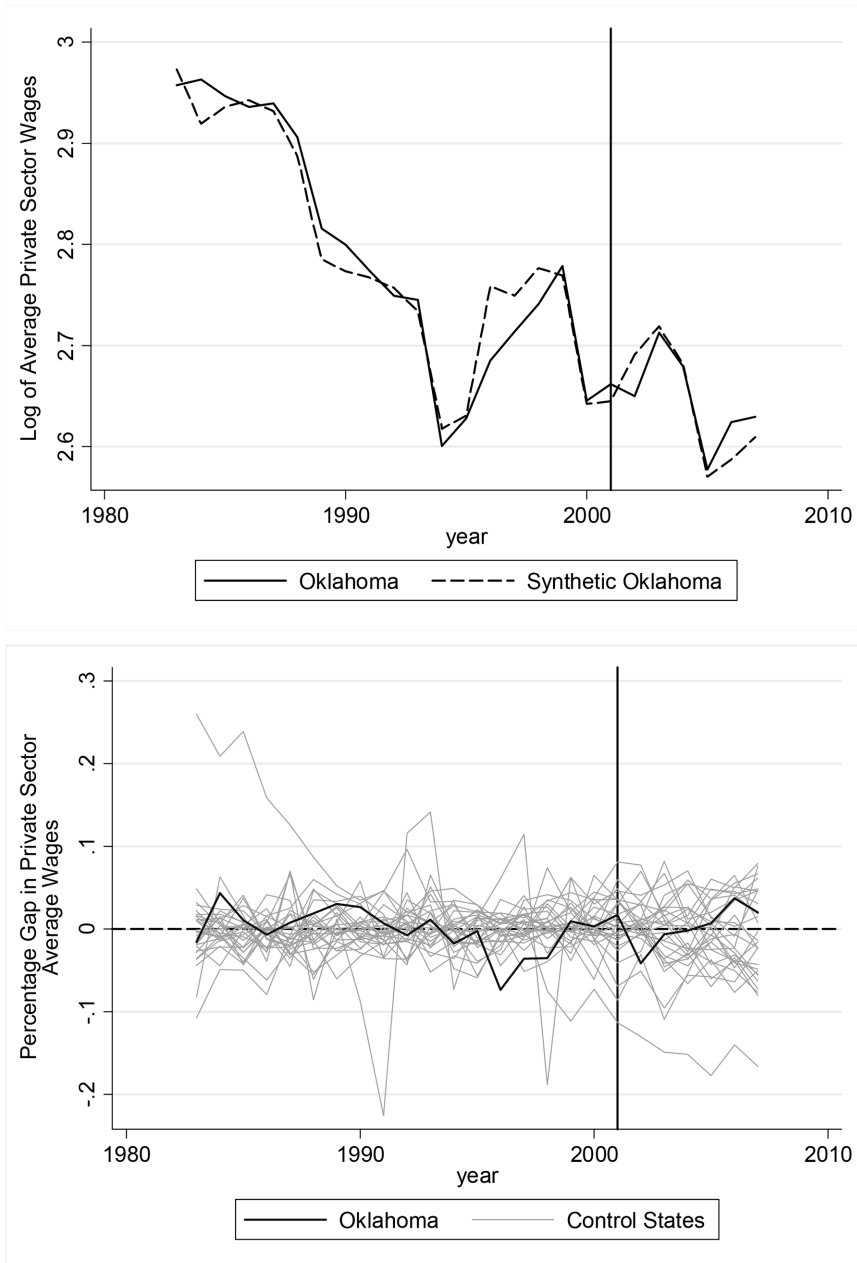
Figure 3. *Continued.*

Turning to the manufacturing sector unionization, the synthetic Oklahoma is constructed using six states with Massachusetts (36.7 percent), Montana (29.7 percent), Rhode Island (21.1 percent), and New Mexico (7.5 percent) being the states with the four largest weights. The RMSPE between Oklahoma and its synthetic counterfactual is equal to 1.23. Looking at the top row of panel B of Figure 3, we see that the

Panel F. Manufacturing Average Wages.
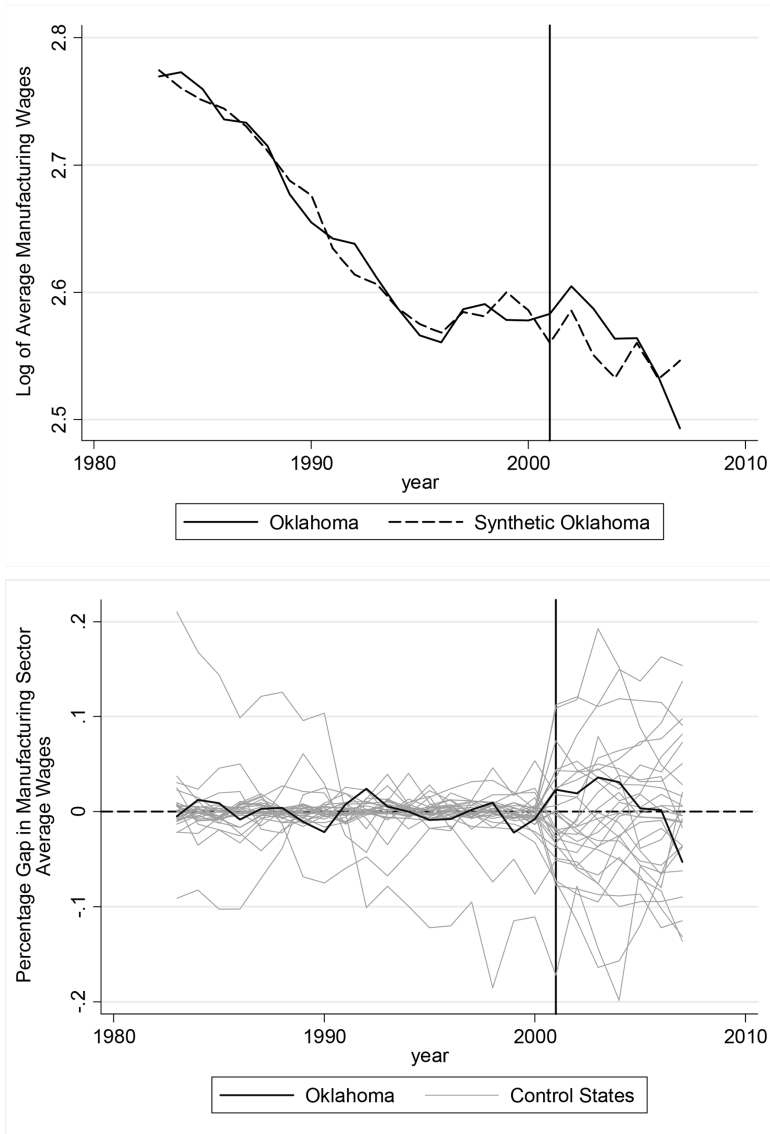
Figure 3. *Continued.*

trends from private sector unionization carry over here. Specifically, the passage of RTW laws in Oklahoma indicates a roughly $\frac{\tilde{\alpha}}{\overline{Y}_{OK,2000}} = 21.8$ percent reduction in manufacturing sector unionization rates.

Compared with placebo studies, the Oklahoma effect line seems to plunge right after the enactment of RTW laws (bottom row of panel B in Figure 3). This sudden (and the largest among all lines) drop in the effect line, however, does not appear to sustain its downward trend. As such, we observe a bounce back of the line to its synthetic control level.

Employment

The next set of results pertains to state-level employment outcomes. Focusing first on employment–population ratio, the counterfactual that most resembles Oklahoma is built using a weighted combination of New Mexico (50.7 percent), Kentucky (16.3 percent), Colorado (14 percent), California (13.7 percent), and Rhode Island (5.3 percent) with a pre-intervention RMSPE of 0.38. The rest of the states are assigned zero weights. Column 5 of Table 2 shows the selected mean of the pre-intervention characteristics of synthetic Oklahoma. We observe much more affinity between Oklahoma and its synthetic counterpart relative to Oklahoma and all other non-RTW states (columns 1, 2, and 5 of Table 2). Looking at the bottom row of panel C in Figure 3, the Oklahoma effect line ($\frac{\hat{\alpha}}{Y_{OK,2000}}$) is not unusually large relative to the placebo lines, which suggests that the passage of RTW laws had no effect on Oklahoma's employment rate.[14]

As for manufacturing employment, the synthetic Oklahoma is constructed using four states: Kentucky (50.1 percent), New Mexico (30.5 percent), Alaska (12.1 percent), and Hawaii (7.3 percent) with a pre-intervention RMSPE of 0.16. Column 6 of Table 2 provides the selected pre-intervention characteristics for synthetic Oklahoma. Looking at the bottom column of panel D of Figure 3, we do not observe any discernible pattern for the effect line of Oklahoma compared to the rest of the placebo lines. This indicates no effect of RTW laws on manufacturing employment in Oklahoma.

Wages

Our final set of evidence pertains to log average hourly wages. The synthetic control that resembles Oklahoma the most with respect to the predictors of private sector average wages is constructed using six states with the states with the four largest weights being Montana (45.5 percent), Alaska (19.6 percent), New Mexico (13.4 percent), and West Virginia (12.6 percent). The top row of panel E in Figure 3 plots the log of average hourly wages for the entire period for the actual and synthetic Oklahoma; the pre-intervention RMSPE is 0.02. The two lines almost perfectly overlap prior to the adoption of RTW laws and they begin to diverge with the law. After an initial divergence, however, the gap narrows and the lines cross roughly three years after the implementation of the laws. Looking at the Oklahoma effect line, we once again do not observe any unusual divergence from the placebo lines. This evidence is likely to rule out any effect of the RTW laws on private sector average wages in Oklahoma.[15]

Similar to private sector wages, we do not find any impact of RTW laws on average manufacturing wages in Oklahoma (panel F in Figure 3). While wages decline after 2003 in Oklahoma relative to its synthetic control, the impact does not seem large enough to provide any evidence.

Robustness Checks and Discussion

We undertook several sensitivity checks to examine the validity of our synthetic control estimates. First, we extended our control pool to include all states, instead

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[14] We also examine the impact of RTW laws on the state's labor force participation rate and unemployment rate. The findings from this exercise do not indicate any discernible effect of RTW laws. These additional results are available upon request.

[15] Since wages are expressed in log form, we do not need to take the ratio of the postintervention gap relative to a benchmark period.

of using only non-RTW states. Note that the states with positive weights change (potential limitation discussed earlier) when we add RTW states to our control pool. However, the synthetic control method estimates using all states produce similar findings. Figure C1 in Appendix C displays these results.[16] Second, with the exception of wages, we reported the effect estimates with respect to their values in the year 2000. As an alternative, we calculated the effect estimates using the ratio between the post-intervention gap and the average of the relevant outcome over the entire pre-intervention period (i.e., $\frac{\alpha_{it}}{\overline{Y}_{i,\text{pre}-\text{intervention}}}$, $i = 1, \ldots, S+1$ and $t = T_0 + 1, \ldots, T$). The results are qualitatively similar to those presented in the article. Third, we extended our set of predictors to include variables such as states' minimum wage, per capita corporate tax, total road mileage, and gross product. We also dropped all additional predictors and ran the specifications with only lagged outcome values. The results remain intact. Next, we implemented a year-by-year matching to construct the synthetic controls. Specifically, rather than augmenting the relevant lagged outcomes biennially over the pre-intervention period into each synthetic control specification, we added all lagged outcomes from 1983 to 2001. The results are virtually identical to those presented in the article.

Fourth, rather than using the actual treatment, we reassigned RTW law enactment to a placebo pre-intervention year and limit our analysis to the 1983 to 2000 period. A large post-intervention estimate would raise concerns regarding the causality of the synthetic control estimates. Figure C2 in Appendix C presents the results using 1993 as our placebo year of intervention.[17] Looking at all top rows of the panels in Figure C2, we do not observe any unusual divergence of the Oklahoma effect line from the placebo lines. Fifth, recall that we limit our post-intervention analysis to the 2002 to 2007 period to circumvent any potential confounding effect of the Great Recession. Extending our time span to include the Great Recession years and beyond (e.g., the 2002 to 2010 period) does not largely alter the observed trends with two plausible exceptions in the manufacturing sector: (1) trends in manufacturing unionization rates continue to drift apart after bouncing back to its synthetic control level in 2007 (panel A of Figure C3 in Appendix C), and (2) beginning with 2004, we observe a noticeable divergence in the manufacturing wage trends of Oklahoma and its synthetic counterpart (panel B of Figure C3 in Appendix C).[18] The two lines continue to drift apart during the Great Recession years, reaching a gap of roughly 20 percent in 2010. However, it is not clear whether these extended trends are solely attributable to RTW laws. Imposing a common trend assumption for Oklahoma and its synthetic control during the Great Recession may be too restrictive. All additional results are available upon request.

Finally, as an alternative to the synthetic control method, we employ a DD model to estimate the impact of RTW laws in Oklahoma. Specifically, we run the following regression equation

$$Y_{st} = \beta_0 + \beta_1 D_{st} + X'_{st}\beta_2 + \lambda_s + \gamma_t + \lambda_s t + u_{st} \tag{4}$$

where Y_{st} is any state-level outcome for state s in year t, D_{st} is an indicator variable that takes the value of 1 for post-intervention Oklahoma, λ_s and γ_t are state and year fixed effects, respectively, $\lambda_s t$ is the set of state-specific linear time trends, and u_{st} is

[16] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[17] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.
[18] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

the error term. Note that we include the state-specific linear time trends in equation (4) to capture any pre-intervention trend differences observed in Figure 2. The coefficient estimate β_1 is the DD estimate and Table 3 presents the results. It appears that the passage of RTW laws in Oklahoma significantly decreased unionization rates (columns 1 and 2 of Table 3). The effect sizes are similar to those obtained from the synthetic control method. As for other state outcome variables, the estimated effects are not different from zero. Overall, our DD estimates with state-specific linear trends are consistent with the synthetic control method estimates.

As noted earlier, the synthetic control method relies on the assumption of no interference between units. This implies no effect of Oklahoma's RTW laws on control states' outcomes. There are not many routes by which this assumption can be violated. One potential scenario is that the passage of RTW laws may have increased the antiunion sentiment in other states or that national unions fearing a potential epidemic effect may have reacted to the adoption of RTW laws by diverting resources to Oklahoma from other states. In both cases, interference is likely to lead to lower unionization rates in the control states and this would imply an attenuation bias in our unionization estimates. The passage of RTW laws may have also attracted investment to Oklahoma from other states. This would then suggest that the employment and manufacturing employment gaps were upward biased (assuming a positive impact of RTW laws), while wage gaps can also be biased because of a lower labor demand in the control states. These potential biases, however, are unlikely to rule out no effect of RTW laws.[19]

The extent of unionization in Oklahoma or declining union power observed over the last decades may explain why the passage of RTW laws had no effect on several state-level outcomes. Oklahoma was not a highly unionized state to begin with. In the year 2000, as noted, the private sector unionization rate was 4.4 percent. Further declines in union density arising from RTW law enactment may have a negligible effect on the state's overall economy. Given the relatively higher pre-intervention unionization rates in the manufacturing sector in Oklahoma, however, the extent of unionization may not alone explain our findings. As such, among several other factors, skill-biased technological changes, shifts in employment from industries and occupations where union density has been traditionally high, and international trade have all led to weaker unions, which, in turn, may have diminished the influence of RTW laws.

CONCLUSION

Since the first wave of enactments in the mid-1940s, RTW laws have been at the center of policy discussions. Recent passage of the laws in Indiana and Michigan has also rekindled the debate over the role of RTW laws in state economies. There is, however, still no clear consensus among policymakers and researchers. This lack of consensus among researchers predominantly stems from challenges in distinguishing the effect of the laws from other observable and unobservable state characteristics. Even in the absence of a causal relation, it is possible to obtain significant correlations between RTW laws and state outcomes.

Using the case study of Oklahoma and a recently developed econometric technique, this article examines the impact of RTW laws on various state-level labor

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[19] To examine the validity of no interference assumption further, we analyze the change in firm behavior by looking at the trends in firm birth and foreign direct investment. Noting that any interference would indicate an upward bias in the impact of RTW laws on firm birth or foreign direct investment, the Oklahoma effect lines are not unusually large for either of the state-level outcomes. These results are available upon request.

Table 3. Differences-in-differences estimates of Oklahoma's right-to-work laws.

	Private sector unionization	Manufacturing sector unionization	Employment–population ratio	Manufacturing employment	Log of average private wages	Log of average manufacturing wages
	(1)	(2)	(3)	(4)	(5)	(6)
			Coefficients (standard errors)			
RTW Laws–Oklahoma	−2.033[**]	−1.990[*]	−0.311	−0.278	−0.004	−0.015
(N = 725)	(0.385)	(1.017)	(0.428)	(0.225)	(0.009)	(0.016)

Notes: Standard errors are clustered at the state level. Specifications control for state's population and total land square miles, per capita income, the percentage of the state's labor force that are white, male college graduates and those residing in a metropolitan area, state and year fixed effects, and a linear trend. The sample is restricted to Oklahoma and all non-RTW states. *N* represents the sample size. See text for further details.
*significant at 5%; **significant at 1%.

market outcomes. Our results indicate that the passage of RTW laws in Oklahoma significantly decreased private sector unionization rates. State outcomes, including total employment rate and private sector average wages, on the other hand, were not affected by RTW laws, at least in the short run. The findings for the private sector generally extend to the manufacturing sector.

Before concluding, there is at least one caveat to keep in mind. As noted, Oklahoma is a relatively small state and even before the passage of RTW laws, private sector unionization was low, compared to the rest of the nation. Considering the most recent states that passed RTW laws (Indiana and Michigan) and other larger non-RTW states with high private sector union densities (i.e., Wisconsin), where the debate over RTW laws is currently intense, the adoption of the laws may have different effects. Nevertheless, the findings of the article may still shed light on the effects of RTW policies on local labor market outcomes.

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APPENDIX A: DATA SOURCES

Unionization Rates

Union membership as a percentage of the private sector civilian nonfarm labor force. Manufacturing unionization rate is defined similarly. Source: Union Membership and Coverage Database available at www.unionstats.com. See Hirsch and Macpherson (2003) for a general description of the database.

Employment–Population Ratio

Total employment as a percentage of the civilian population. Source: Bureau of Labor Statistics, Local Area Unemployment Statistics.

Manufacturing Employment Rate

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

Private Sector Average Wage Rates

All values are deflated by the year's average consumer price index (2,000 = 100). Source: Union Membership and Earnings Data Book, Bureau of National Affairs. The database is constructed by Barry Hirsch and David Macpherson (2003).

Manufacturing Sector Average Wage Rates

All values are deflated by the year's average consumer price index (2,000 = 100). Source: Bureau of Labor Statistics, Current Employment Statistics—State and Metro Area Estimates.

State Population

Source: Bureau of Census, Bureau of Economic Analysis.

Land Square Miles

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

Per Capita State Income Data

All values are deflated by the year's average consumer price index (2,000 = 100). Source: Bureau of Economic Analysis, U.S. Department of Commerce, Regional Economic Measurement Division, Regional Economic Information System CD.

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: STATE OUTCOME TRENDS

Panel A. Private Sector Unionization.

**Panel B.** Manufacturing Sector Unionization.
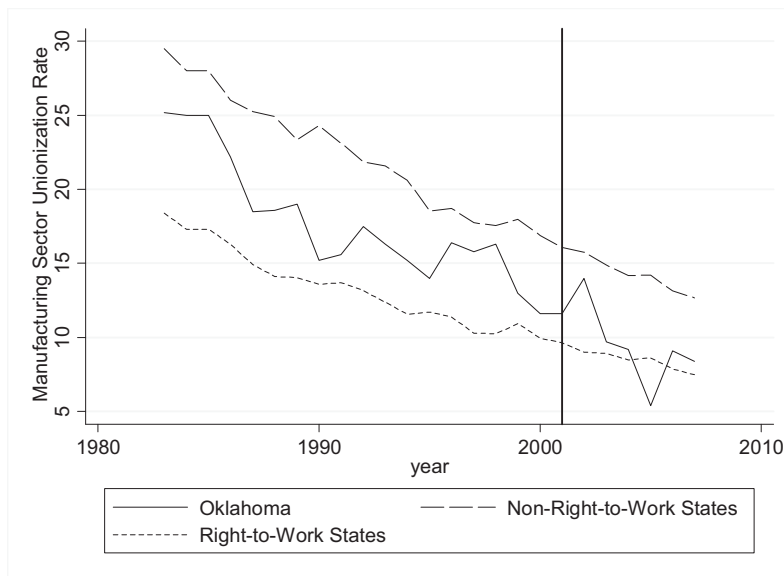
Notes: The panels contain the raw data trends (1983 to 2007) for Oklahoma and the average of RTW and non-RTW states. Wages are deflated by the year's consumer price index (2,000 = 100).
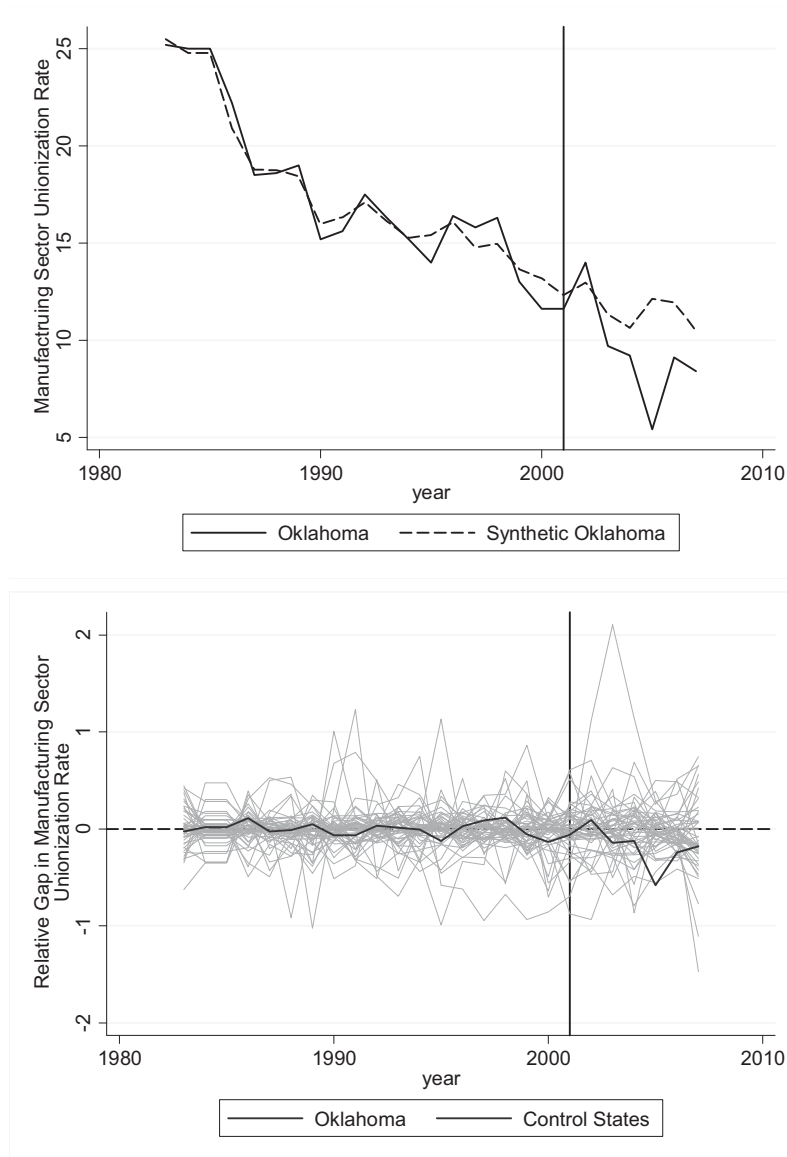
Figure B1. Trends in State Outcomes.

Panel C. Employment-Population Ratio.

Panel D. Private Sector Average Wages.

Figure B1. *Continued.*

Panel E. Manufacturing Average Wages.

Figure B1. Trends in State Outcomes.

Notes: The panels contain the raw data trends (1983 to 2007) for Oklahoma and the average of RTW and non-RTW states. Wages are deflated by the year's consumer price index (2000=100).
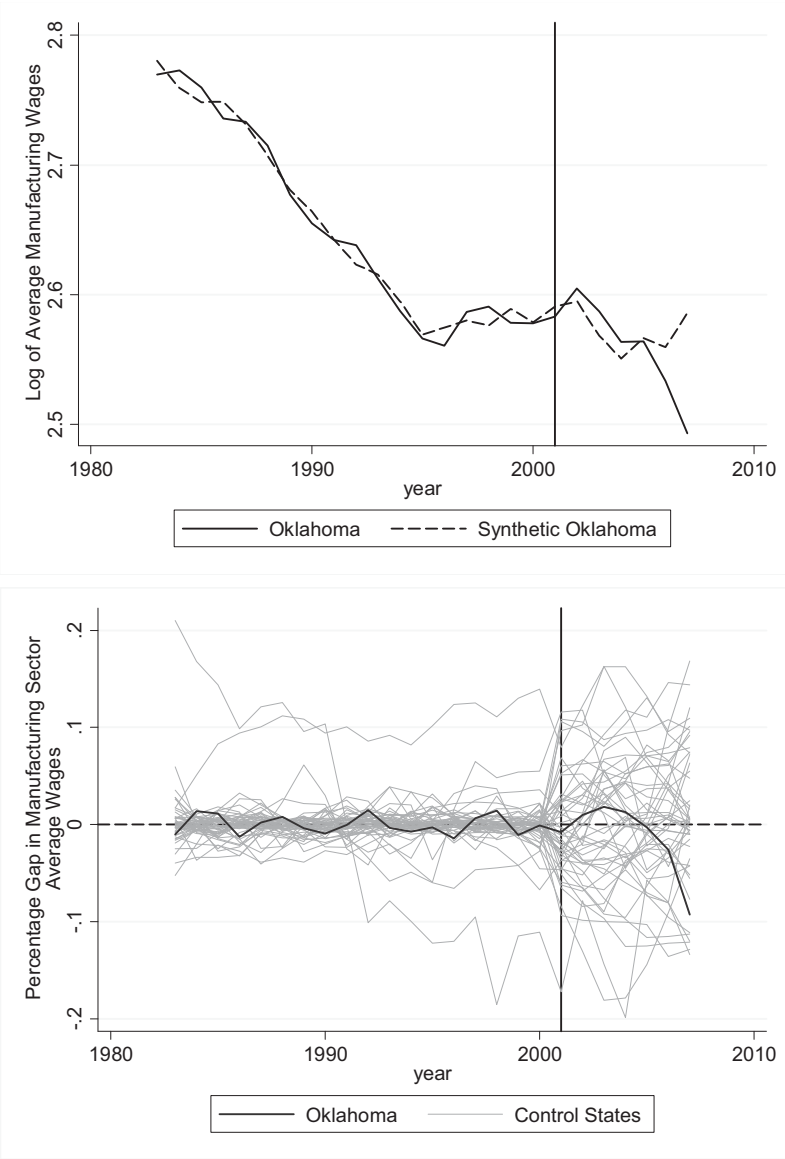
Figure B1. *Continued.*
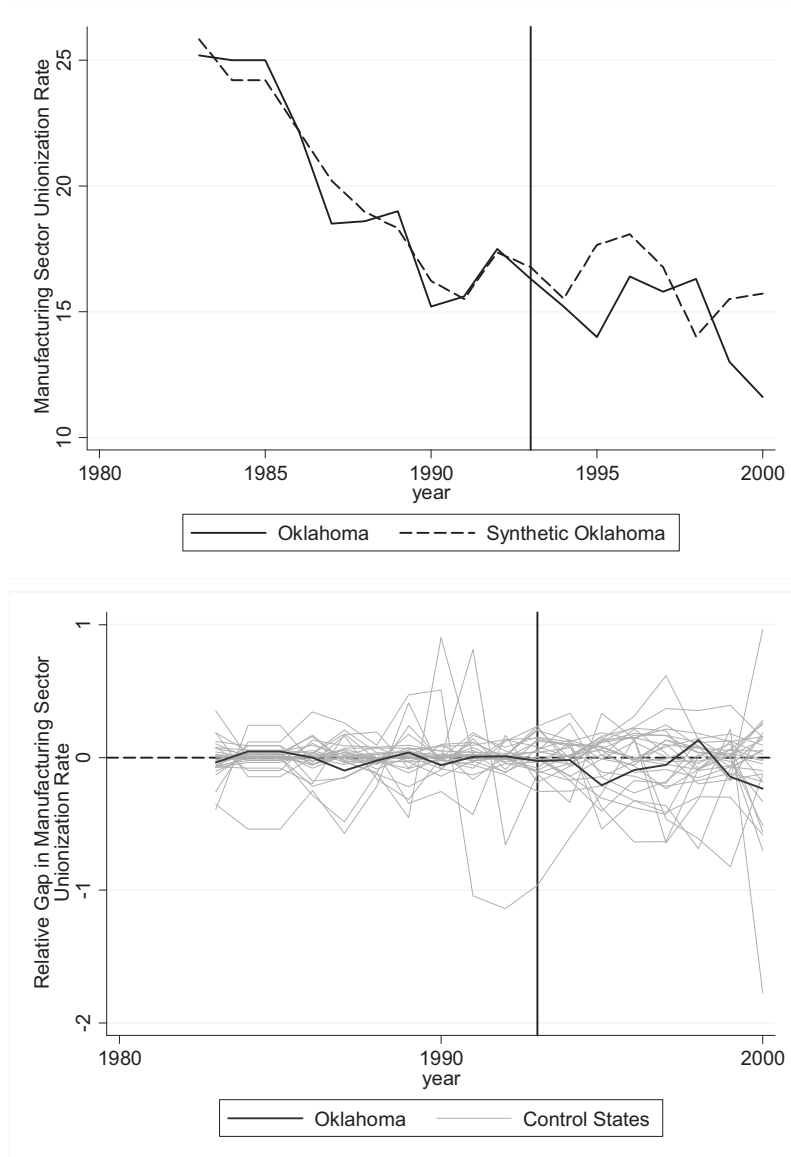
APPENDIX C: ADDITIONAL ESTIMATIONS

Panel A. Private Sector Unionization.
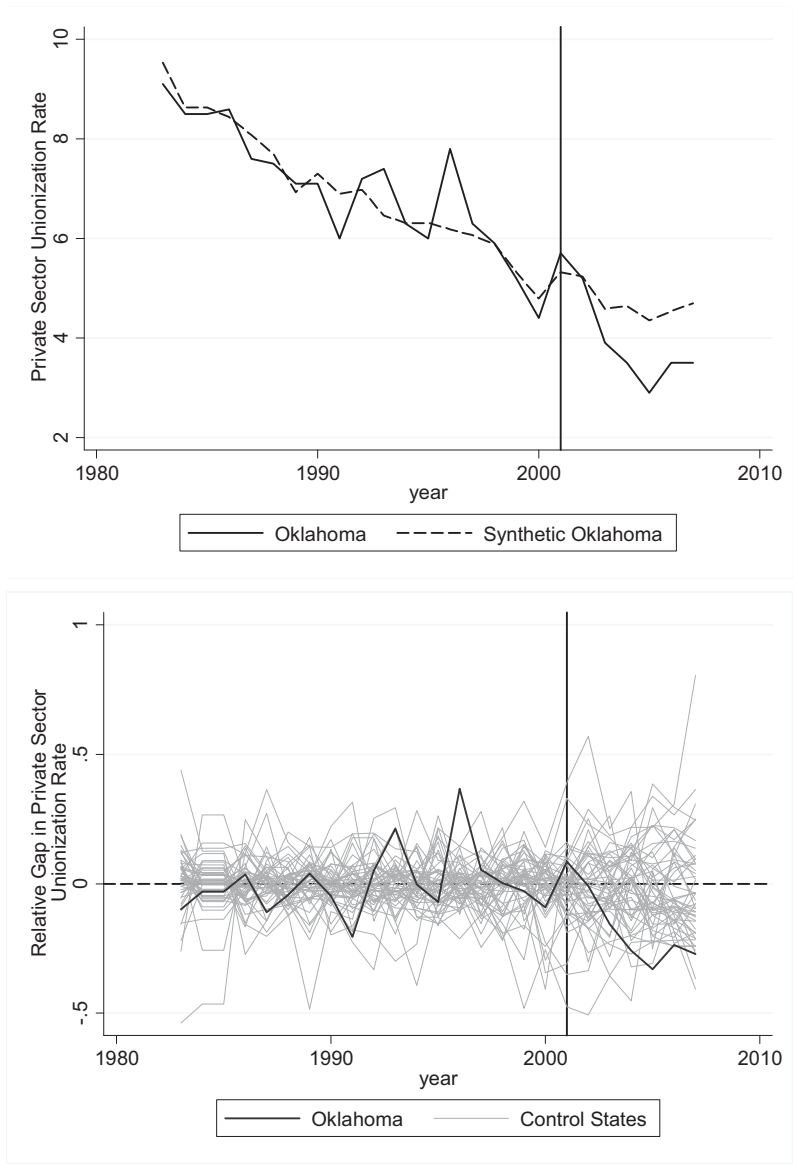
Notes: The top figures in each panel contain trends (1983 to 2007) for Oklahoma and synthetic Oklahoma while bottom figures indicate the effects of RTW laws in Oklahoma and the placebo effects in all states. Panels A–D express the effects relative to the corresponding outcome rates in the year 2000. The preintervention period is from 1983 to 2001. Wages are deflated by the year's consumer price index (2,000 = 100).

Figure C1. Trends in Various State Outcomes. Oklahoma vs. Synthetic Oklahoma.

Panel B. Manufacturing Unionization.

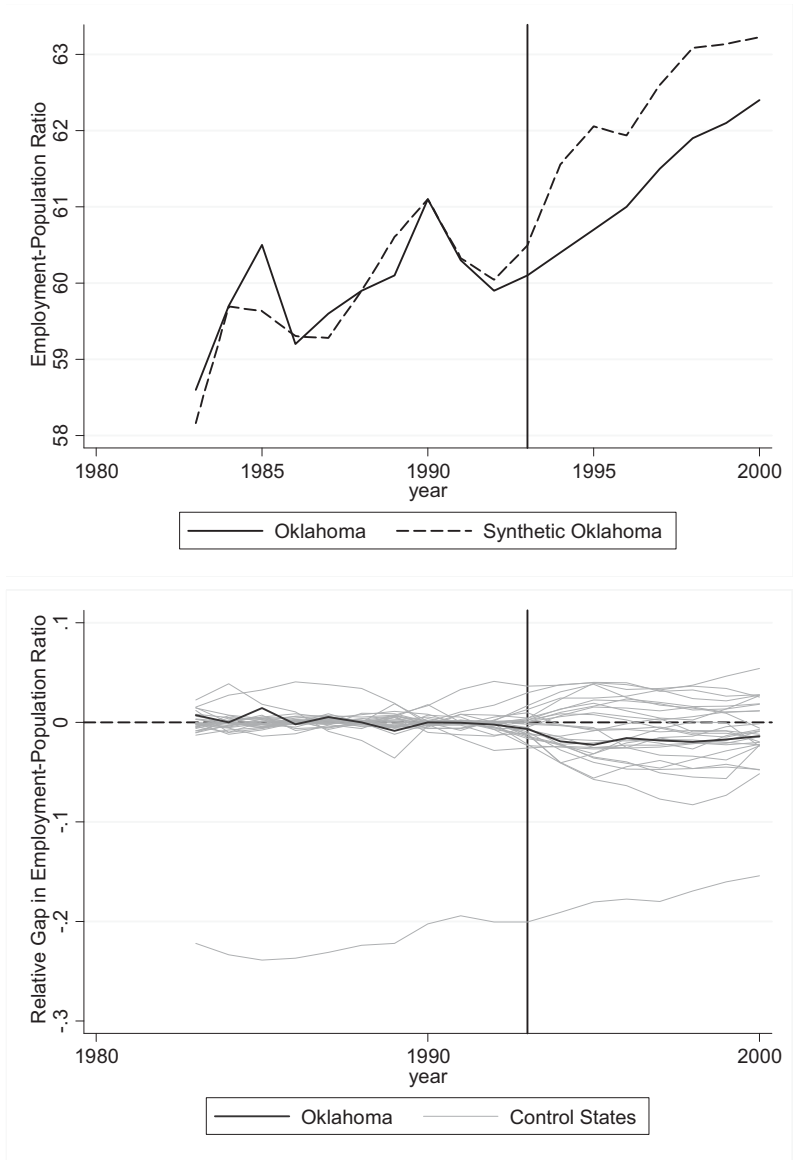**Figure C1.** *Continued.*

Panel C. Employment-Population Ratio.
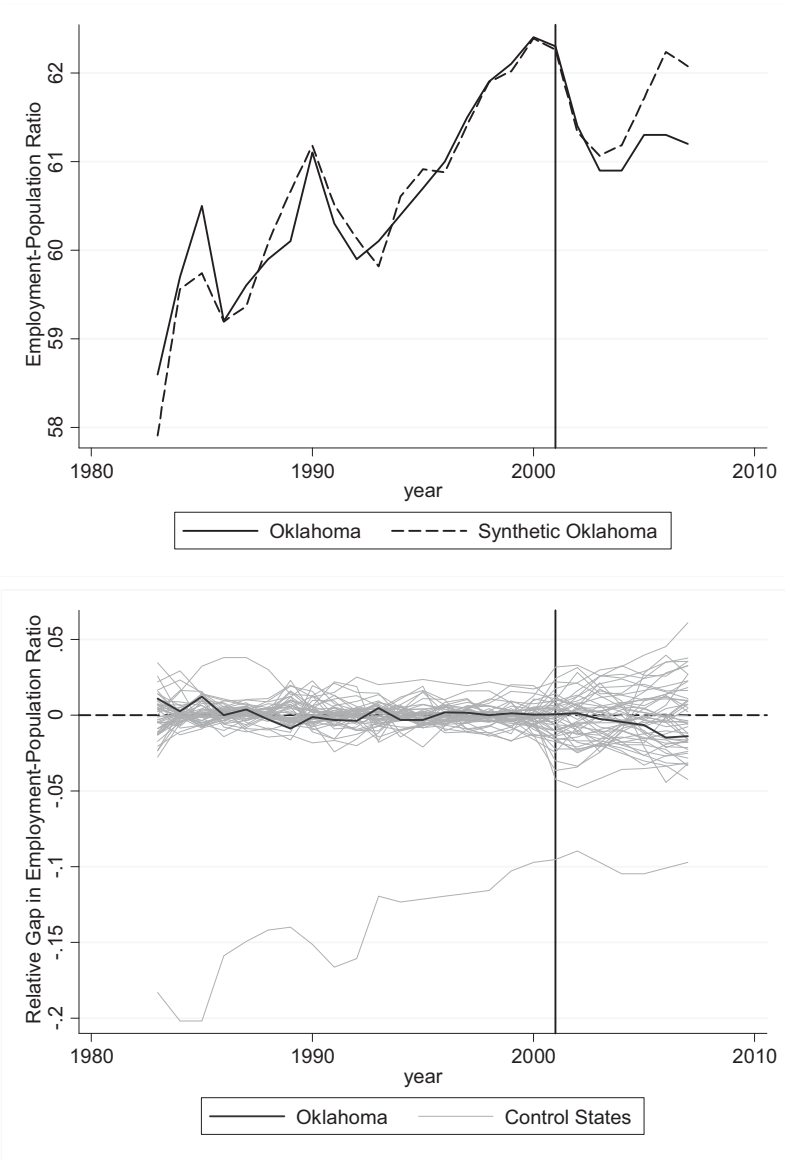
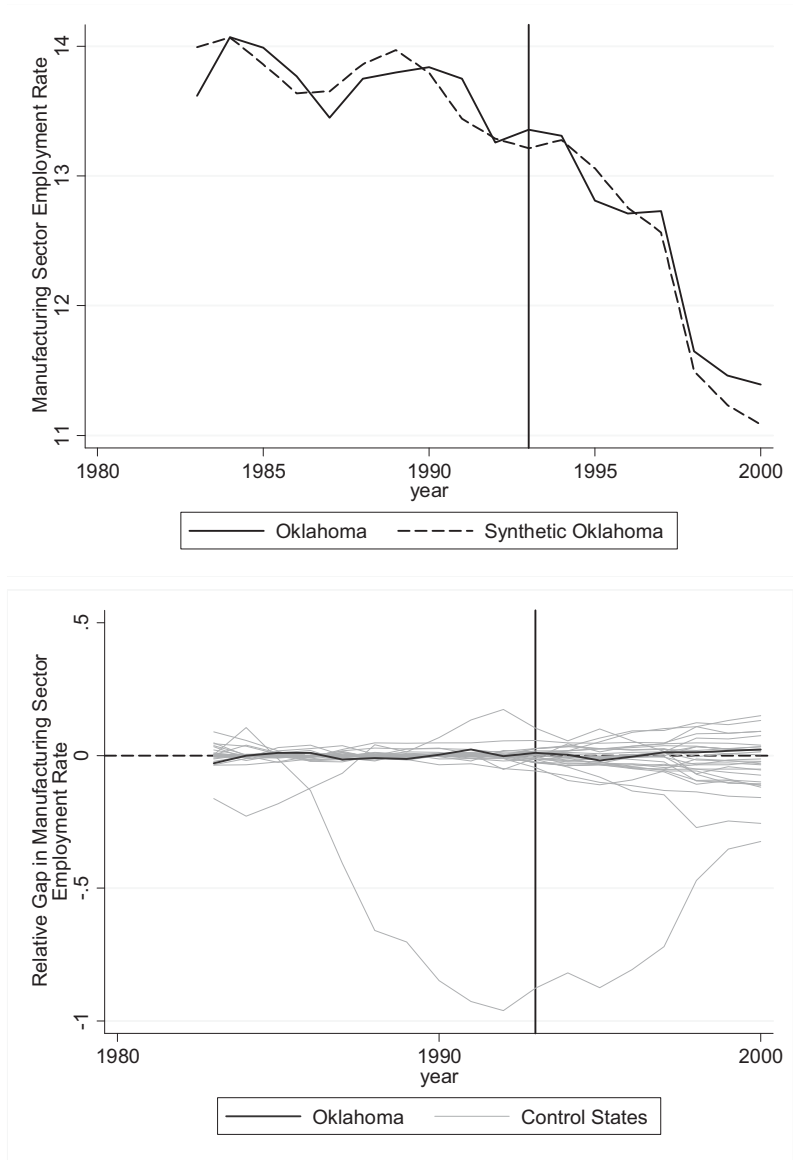**Figure C1.** *Continued.*

Panel D. Manufacturing Employment.
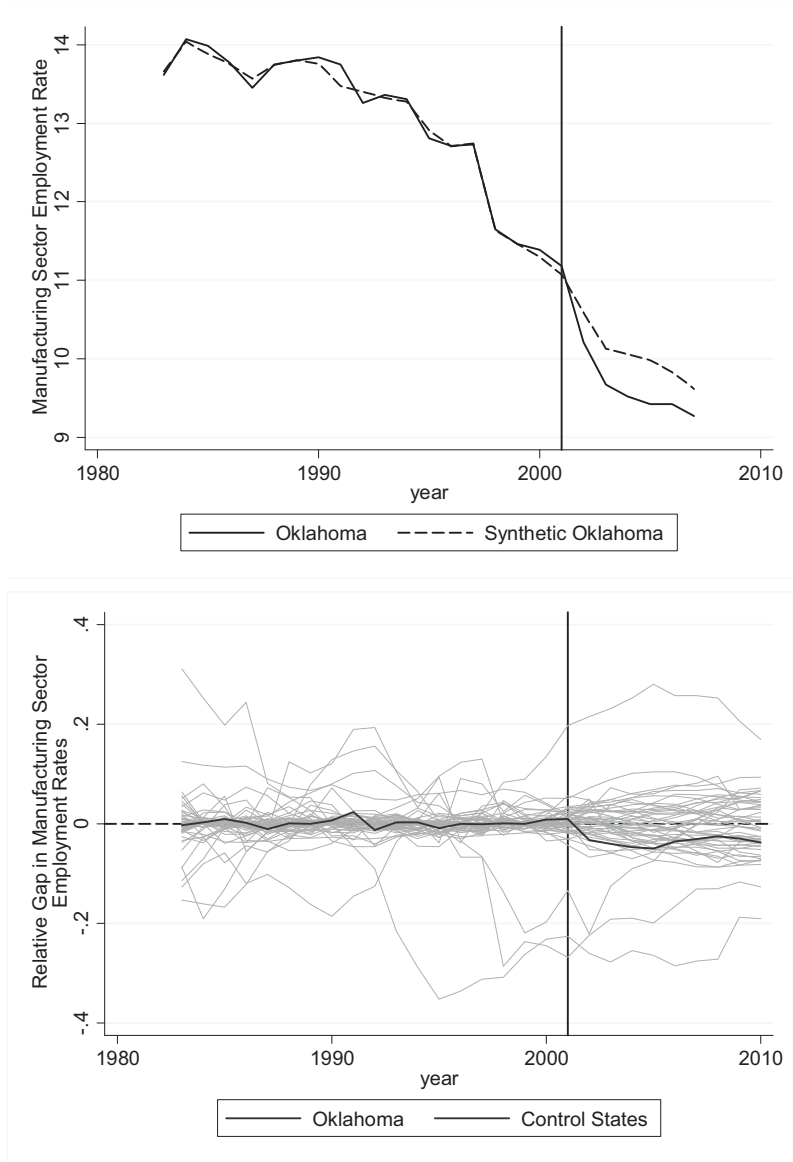
Figure C1. *Continued.*

Panel E. Private Sector Average Wages.
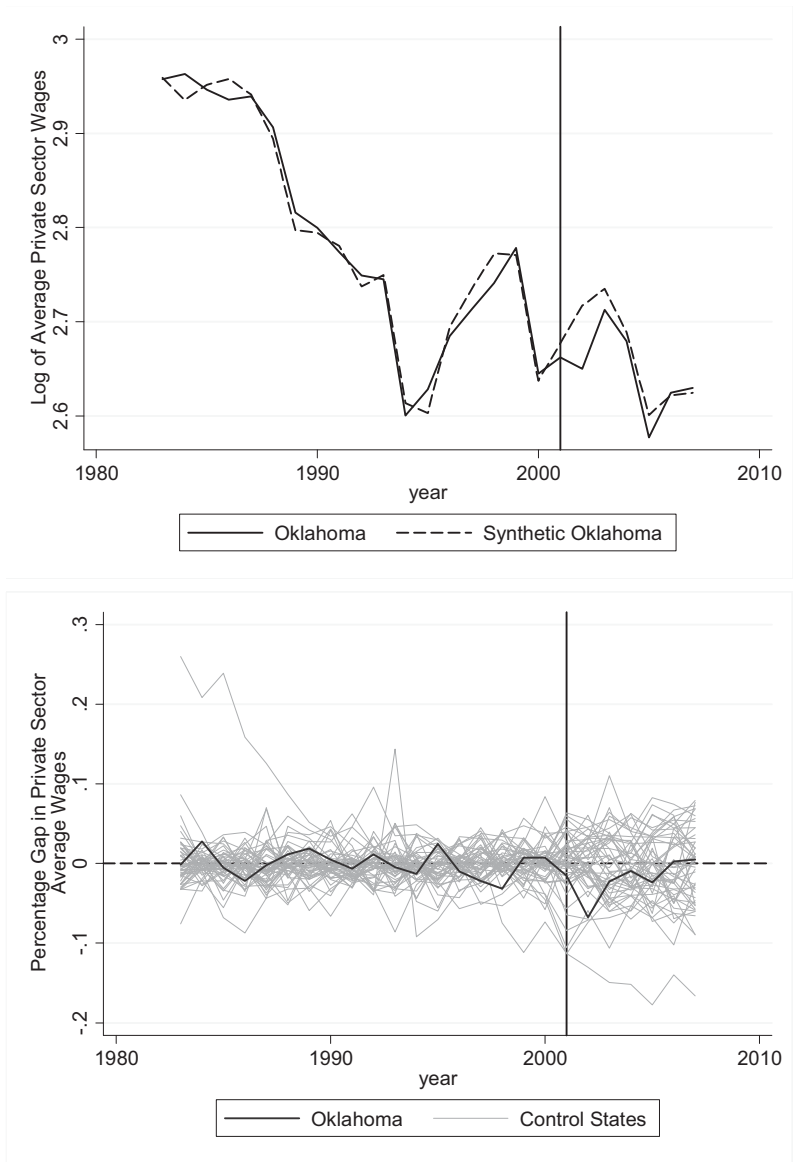
Figure C1. *Continued.*

Panel F. Manufacturing Average Wages.

Figure C1. *Continued.*

Panel A. Private Sector Unionization.
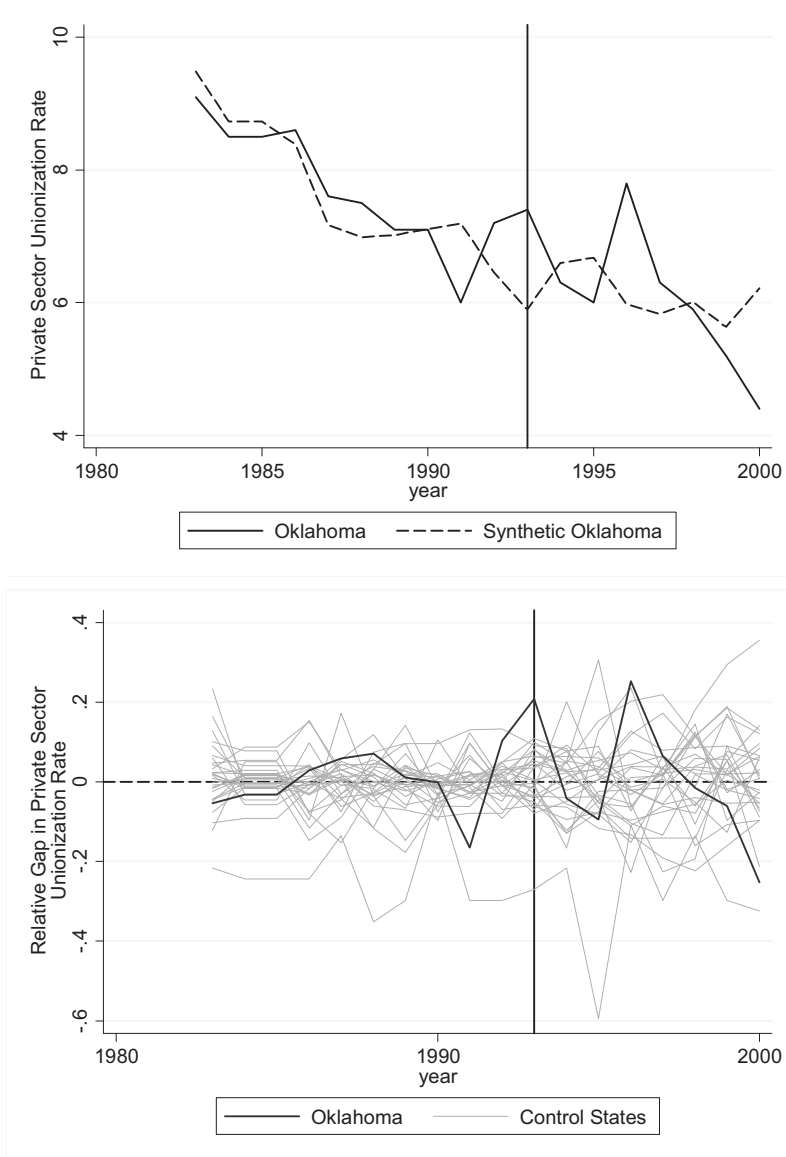
Notes: The top figures in each panel contain trends (1983 to 2000) for Oklahoma and synthetic Oklahoma while the bottom figures indicate the effects of RTW laws in Oklahoma and the placebo effects in all non-RTW states. Panels A–D express the effects relative to the corresponding outcome rates in the year 1992. The preintervention period is from 1983 to 1992. Wages are deflated by the year's consumer price index (2,000 = 100).

Figure C2. Placebo Estimates (Placebo Intervention 1993). Oklahoma vs. Synthetic Oklahoma.

Panel B. Manufacturing Unionization.

Figure C2. *Continued.*

Panel C. Employment-Population Ratio.

Figure C2. *Continued.*

Panel D. Manufacturing Employment.

Figure C2. *Continued.*

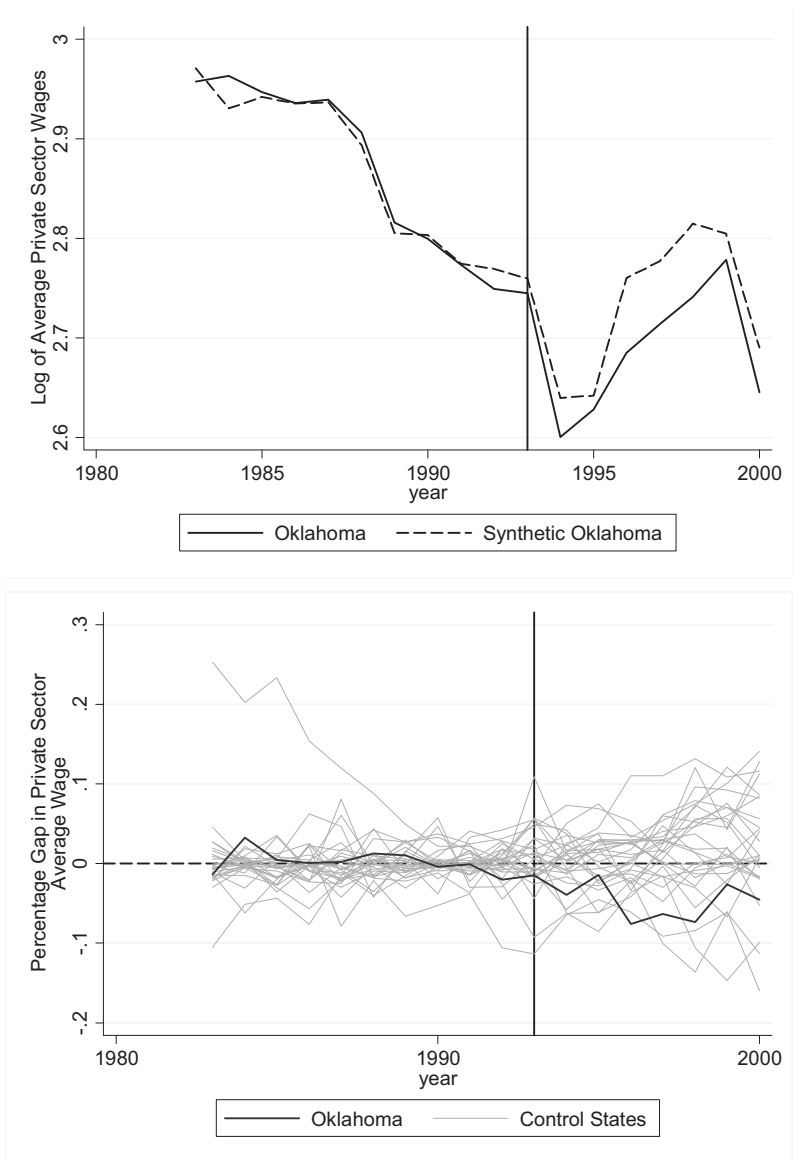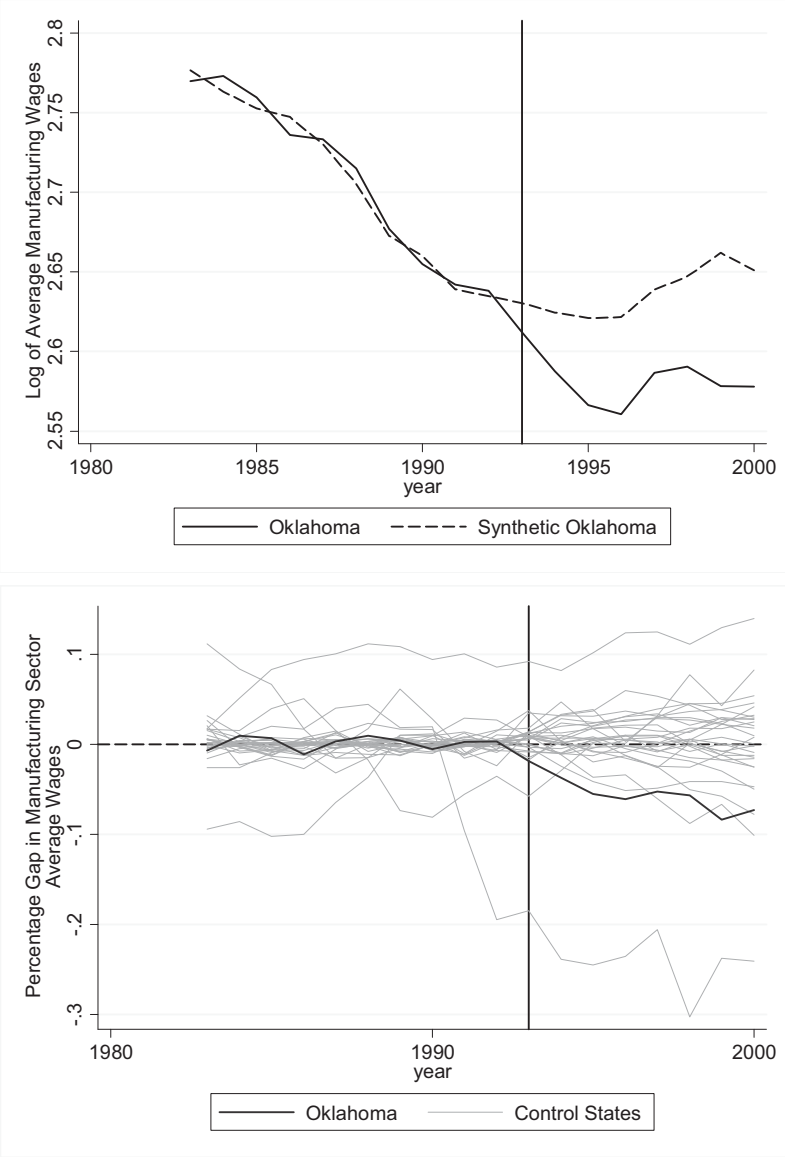**Panel E.** Private Sector Average Wages.

Figure C2. *Continued.*

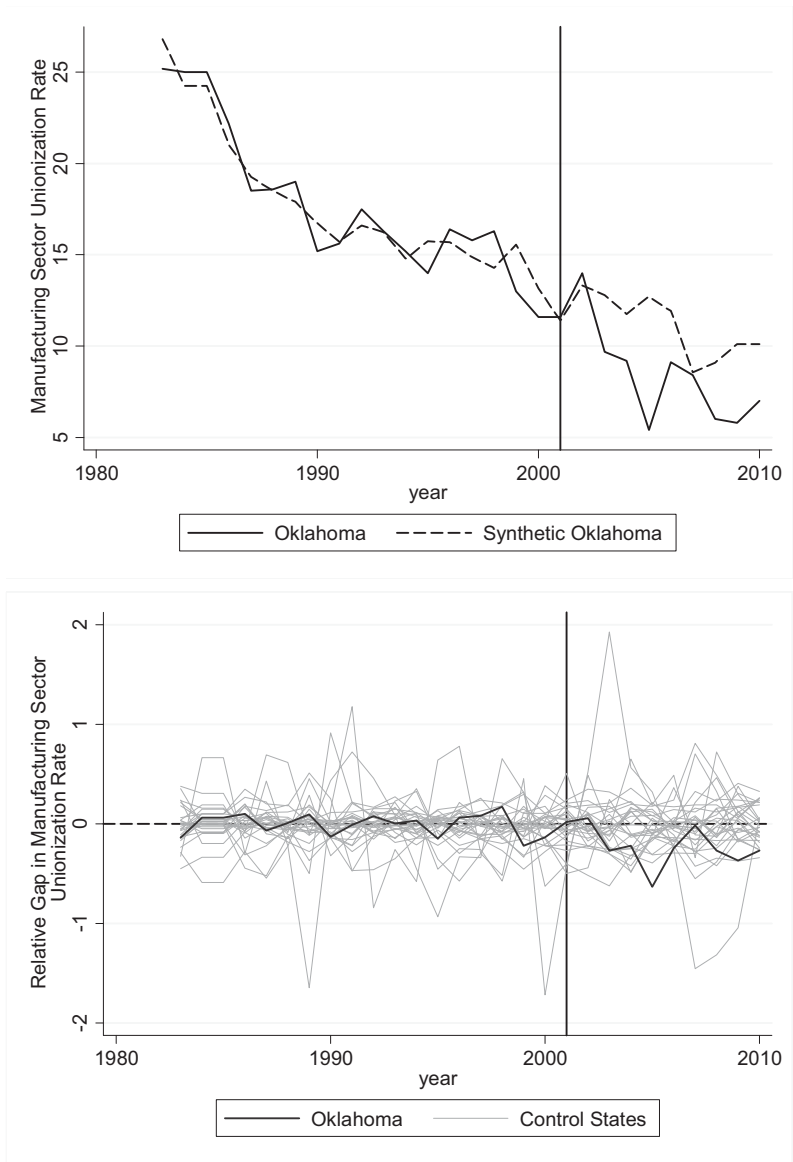**Panel F.** Manufacturing Average Wages.

Figure C2. *Continued.*

Panel A. Manufacturing Unionization.

Notes: The top figures in each panel contain trends (1983 to 2010) for Oklahoma and synthetic Oklahoma while the bottom figures indicate the effects of RTW laws in Oklahoma and the placebo effects in all non-RTW states. Panel A expresses the effects relative to the corresponding outcome rates in the year 2000. The preintervention period is from 1983 to 2001. Wages are deflated by the year's consumer price index (2,000 = 100).

Figure C3. Trends in Various State Outcomes. Oklahoma vs. Synthetic Oklahoma.
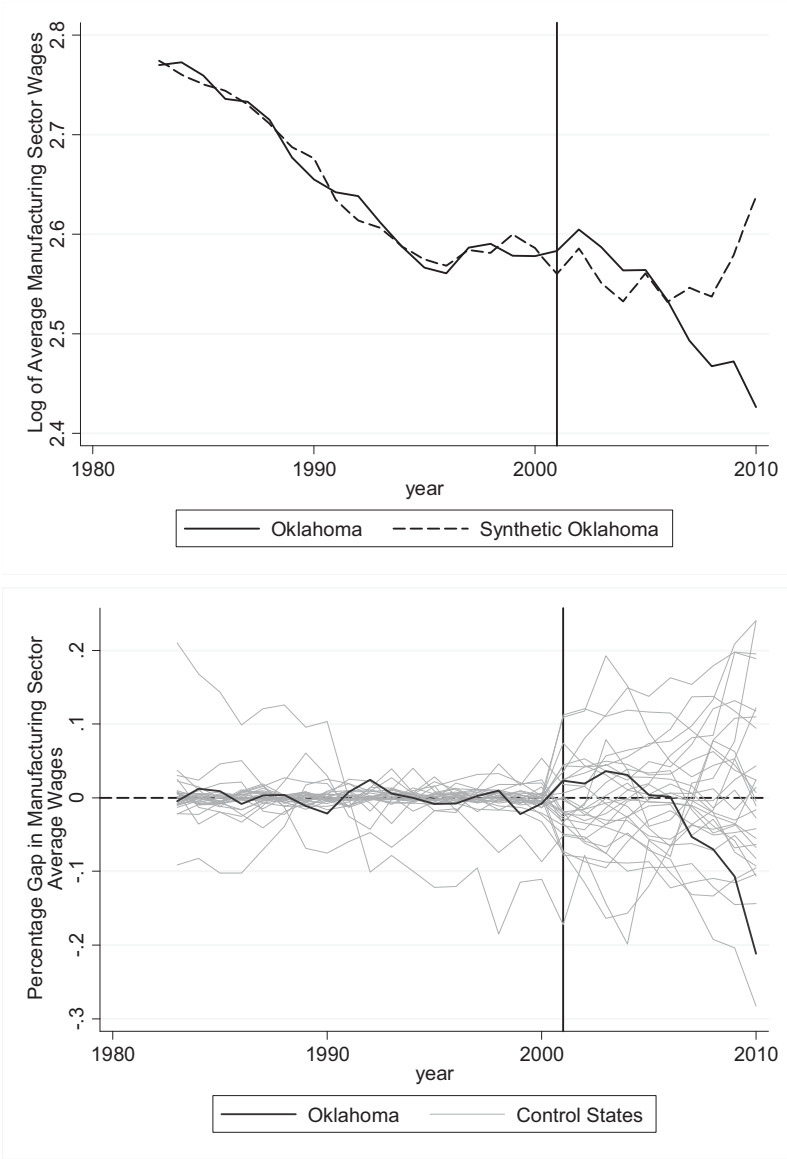
Panel B. Manufacturing Average Wages.

Figure C3. *Continued.*