State-Based Heterogeneity in Right-to-Work's Effects

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Abstract

The role of unions in the US labor market has been a highly contested political issue, leading states to pass Right-to-Work (RTW) laws. As of 2023, 27 states in the US have active RTW laws, legislation that makes it illegal for unionized firms to require union membership as a condition of employment. I use synthetic control methods to estimate RTW's effects on a broad range of state outcomes that could be of interest to policymakers. I find evidence that RTW typically reduces the union coverage rate and average hourly wages, while it seems to have no generalizable effect on other important state-level variables such as total employment and the unemployment rate. The synthetic control methods help to uncover substantial heterogeneity in RTW's impacts on a state's union coverage rate, and this heterogeneity is likely due to union composition in the public sector and the size of a state's public sector at the time of enactment. It is also possible that differences in union organizing tactics after RTW enactment contribute to this heterogeneity. This paper examines five states that passed RTW after 2010 separately – Indiana (2012), Michigan (2013), Wisconsin (2015), West Virginia (2016), and Kentucky (2017). Additionally, estimated effects of RTW do not seem to be due solely to shifting union preferences or worker expectations. Synthetic control results for Missouri, which passed RTW in 2017 and then struck down the law before it could be enacted, are not similar to the results of the post-2010 enactment states. Missouri's results provide evidence that RTW has an independent effect on a state's union coverage rate and average hourly wages.

1 Introduction

Unions have been an important institution in the US labor market for over a century, but until 1935, union-employer relationships were largely unchecked by any legal framework, leading to a contentious relationship that included employers using strike breakers and using local police, state militia, and private agents to harass union leaders (Griffin et al., 1986). In 1935, Congress passed the National Labor Relations Act (also known as the Wagner Act), formally recognizing unions and establishing a legal framework to govern union activity. Unions have sought to increase worker bargaining power, leading to higher wages, increased fringe benefits, and protections against unfavorable management decisions for union members (Freeman and Medoff, 1984). However, these benefits can come at a cost to firms and non-union members due to unions' power over labor supply, including lower profits for firms and loss of employment for workers (Freeman and Medoff, 1984). With the perceived effects of unions benefiting union members and disadvantaging others, the presence and strength of organized labor has been a highly contested issue in US public policy. As union membership grew during the 1930s and early 1940s, political opposition to unions also grew, leading to the passage of the Taft-Hartley Act in 1947 (Callaway and Collins, 2018). The Taft-Hartley Act undermined a variety of union organizing tactics and allowed states to pass Right-to-Work (RTW) laws, state-level legislation that further restricts organizing practices. Though there has been some work on the economic implications of RTW, the legislation's true effects are still widely debated. This paper aims to evaluate RTW's effects on a broad range of state labor market variables that could be of interest to policymakers– the union coverage rate, union elections, wages, the wage distribution, the union wage premium, total employment, and the unemployment rate.

As of 2022, 27 states have active RTW legislation, with the most recent additions including Indiana (2012), Michigan (2013), Wisconsin (2015), West Virginia (2016), and Kentucky (2017). RTW is a state-level law that makes it illegal for unionized firms to require that employees be union members or pay union dues as a condition of employment. Proponents of

RTW claim that the legislation increases freedom for workers and creates a more favorable business environment that leads to job creation, while opponents of RTW claim that the legislation diminishes union membership and union power, resulting in lower wages, fewer health and safety protections, and fewer fringe benefits for workers (Eren and Ozbeklik, 2016). Table 1 shows that before the new cluster of post-2010 enactments, RTW states did experience a lower union coverage rate and lower hourly wages on average, providing some basis for the claim made by opponents. However, these average differences are not necessarily due to RTW. These differences could be due to other state characteristics that merely correlate with RTW passage, such as a state's industrial composition. This presents a challenge in estimating plausibly causal effects of RTW. Policymakers considering the legislation should rely on evidence that successfully accounts for other state characteristics and isolates the effects of RTW to inform their decision-making.

Because commonly used microdata (the Current Population Survey) did not track union status before 1973 and most current RTW states passed the law before 1973, a handful of recent enactments offers a new opportunity to estimate RTW's effects on the union coverage rate and other state-level outcomes. I examine five states that enacted RTW after 2010 – Indiana, Michigan, Wisconsin, West Virginia, and Kentucky. I use synthetic control methods to examine these states on a case-by-case basis and provide state-specific estimates on the effects of RTW. Synthetic control lends itself to measuring aggregate outcomes that cannot be investigated with worker-level microdata, specifically union elections and total employment. I choose to estimate state-specific effects of RTW rather than a single, pooled estimate because of potential heterogeneity. At the time of RTW passage, these states had different union coverage rates. Indiana and Wisconsin both exhibited union coverage rates within 0.5 percentage points of the national average in the year before each state's RTW enactment, while Michigan was almost 5 percentage points above the national average in the year before its RTW enactment (see Table 2). If the union coverage rate is thought of as a proxy for relative union power within a state, then we could expect to see heterogeneity in RTW's effects across these states. These states also differ across other observable characteristics that could influence union coverage rates and other outcomes of interest, such as average hourly wages. Some of these other observable characteristics include industrial composition and proportion of workers in the public sector. There is a large amount of existing literature documenting differences in union rates and wages across various industries and across the public and private sectors (e.g., Curme et al., 1990; Fortin et al., 2022; Krueger and Summers, 1986; Freeman, 1988). If RTW has a larger effect within a specific industry or within the public sector compared to the private sector, then we could expect to see heterogeneity in RTW's effects across states because of this as well.

I find evidence that RTW generally reduces a state's union coverage rate, though there is substantial heterogeneity in the magnitude of RTW's estimated effects across states. Wisconsin and West Virginia exhibit the largest estimated effects, while Michigan and Indiana exhibit much smaller estimated effects. For Kentucky, it seems as though RTW had little to no impact on the state's union coverage rate. This heterogeneity is likely due to two reasons. First, states with both higher public sector union coverage rates and larger public sectors generally produced larger estimated effects of RTW. My analyses indicate that RTW has a considerably larger effect on public sector union coverage rates compared to private sector union coverage rates for all states. RTW has larger estimated effects on a state's public sector union coverage rate when the public sector union coverage rate is higher at the time of enactment, but the legislation's effect on total union coverage rate is constrained by the relative size of the state's public sector. Second, the heterogeneity in RTW's effects across states could be due to differences in union organizing strategies after RTW is enacted. There is anecdotal evidence that some union organizations in Wisconsin chose not to attempt union re-certification after Act 10 (WI legislation that features RTW style clauses and only applies to the public sector), while some union organizations in Michigan took wage reductions in order to mitigate membership loss after RTW enactment.

I also find suggestive evidence that RTW generally reduces a state's average hourly wages and average hourly non-union wages, but it does not appear that RTW universally reduces average hourly union wages. The synthetic control analyses for average hourly union wages produces mixed results across the five states examined. Aside from a state's union coverage rate, average hourly wages, and average hourly non-union wages, I provide evidence that RTW does not have a universal effect on other important state-level variables. These other important state-level variables include total employment and the unemployment rate, which have often been included in policy discussions around RTW. When considering anticipated effects of RTW, policymakers should be cautious in predicting the legislation's effects on variables besides the union coverage rate, average hourly wages, and average hourly non-union wages. My analyses indicate that the magnitude and direction of RTW's effects on other state-level variables could vary between states, making the true effects on future enactment states difficult to anticipate.

It appears that RTW does have an independent effect on a state's union coverage rate and average hourly wage, and the estimated effects for Indiana, Michigan, Wisconsin, West Virginia, and Kentucky are not driven by shifts in other unobservable factors that could accompany RTW enactment, such as shifting union preferences among workers or changes in worker expectations after RTW is passed. Missouri provides a natural experiment to test whether the legislation does have an independent effect. The Missouri state legislature passed RTW in 2017, but it was struck down via a statewide ballot proposition before it could be enacted. I perform the same synthetic control analyses with Missouri, using 2017 as a placebo enactment date, and find that the Missouri results do not resemble my main results from the post-2010 enactment states. This indicates that estimated effects of RTW are likely due to the actual legislation and not other factors that could coincide with RTW passage.

This paper makes three important contributions to the RTW literature. First, it documents heterogeneity in RTW's effects on the union coverage rate across states and provides additional support that these effects are not due to other changes in public policy for most states. The synthetic control methods produce large estimated effects for Wisconsin and West Virginia's union coverage rate, while it produces much smaller estimated treatment effects for Indiana and Michigan's union coverage rates. Past literature has not investigated potential reasons why RTW has a larger impact on the union coverage rate in some states compared to others. The heterogeneity is likely due to public sector unionization rates and the relative size of the public sector at the time of passage, and differences in union organizing strategies after RTW enactment. Next, to the best of my knowledge, this is the first piece of research that documents a relatively large estimated effect of RTW on the average wages in Michigan, reducing average hourly wages by roughly 8.93% over Michigan's post-enactment period. Finally, I use Missouri to test whether estimated effects of RTW are merely a result of changes in union preferences or changing worker expectations. There is existing theoretical literature that predicts estimated effects of RTW are only due to changes in other unobservable factors that typically accompany RTW passage, rather than an independent effect of the legislation itself (Moore and Newman, 1985). However, empirical evidence for these theoretical predictions is sparse. Missouri provides evidence that estimated effects are not solely due to these unobservable factors and that RTW legislation has an independent effect on the union coverage rate and average hourly wages.

2 Background

2.1 Historical Background

In 1935, the US Congress passed the National Labor Relations Act (also known as the Wagner Act or NLRA), which codified workers' right to organize and bargain collectively; it also established the National Labor Relations Board to administer Congress's laws regarding union activity (Herrick, 1946). An important component of the NLRA was that it allowed *closed shop* arrangements and *union shop* arrangements, two levers for unionized firms to maintain union membership and organizational revenue, and curtail the potential for employees to free-ride on union benefits. A closed shop requires that employees must be union members as a condition of employment, while a union shop requires employees to be a union member, become a union member, or at least pay union dues if they are not a member (Cradden, 2005).

The legality of closed shops and union shops changed in 1947 when Congress passed the Taft-Hartley Act. The Taft-Hartley Act explicitly outlawed closed shops in the United States while also giving states the legal power to outlaw union shops if they chose to do so (Nadworny, 1963). Thus, by passing RTW laws, states could make it illegal for unionized firms to require union membership or require union dues as a condition of employment at a given firm. Three years prior to the passage of the Taft-Hartley Act, multiple states had already enacted RTW laws that made union shops illegal, but the legality of these state-level laws was ambiguous until 1947 (Nadworny, 1963). Most current RTW states enacted the legislation between 1944 and 1960, with these states being heavily concentrated in the South and Plains states (see Figure 1). The impact of RTW in these early adopter states and the national impact of the Taft-Hartley Act have not been studied because union status was not available in Current Population Survey data until 1973.

2.2 Political Salience

RTW has been a "profoundly partisan policy" (Fortin et al., 2022, p. 5). In all of the post-2010 enactment states except West Virginia, RTW enactment and passage occurred when the Republican party held a decisive majority in both chambers of the state legislature with a Republican governor.[1] Additionally, in Indiana, Michigan, Wisconsin, and Kentucky, RTW was passed shortly after Republicans took complete control from a divided government that had made it impossible to pass RTW due to Democratic opposition. Indiana, Michigan, and Wisconsin Republicans all took complete control in 2011, one year before RTW was enacted in Indiana, two years before it was enacted in Michigan, and four years before RTW

[1] In West Virginia, the state legislature overrode the governor's veto of RTW.

applied to the private sector in Wisconsin. Republicans typically need complete control to pass RTW as the Democratic Party has been closely aligned with organized labor since the New Deal Period, therefore Democrats typically oppose any efforts to pass RTW (Francia, 2010).

At the time of writing, RTW is still a salient policy that is highly contested in the US political arena. In the 2022 midterm elections, Democrats won complete control of Michigan and have so far signaled an effort to repeal RTW. Tennessee, a staunchly Republican state, voted to enshrine RTW in to the state constitution in 2022. Illinois, a staunchly Democratic state, voted to establish constitutional protections against RTW in 2022 as well (Gleason, 2022). While RTW passage is almost always preceded by Republicans taking complete control of the state government, Republican control is not a sufficient condition for RTW passage. In 2017, the Missouri state legislature passed RTW, but the policy was rejected by voters via a ballot referendum in 2018 before the legislation could be enacted. Although Republicans have maintained complete control of Missouri from 2017-2023, it is not a RTW state as of 2023.

2.3 Existing Literature

Moore and Newman (1985) provide a succinct overview of different theoretical predictions on the expected effects of RTW. These include the taste hypothesis, the free-rider hypothesis, and the bargaining power hypothesis. The taste hypothesis argues that RTW laws do not have an independent effect on levels of unionization, but merely reflect anti-union preferences among the labor force (Moore and Newman, 1985). The free-rider hypothesis contends that when RTW is passed, an opportunity to free-ride on union benefits and services arises, leading to a decrease in unionization rates after a state passes RTW. The bargaining power hypothesis argues that RTW diminishes union bargaining power, reducing the expected benefits of joining a union and decreasing unionization rates after a state passes RTW (Moore and Newman, 1985). These three hypotheses are not mutually exclusive. They could all be operating in a state at the time of RTW passage. Union preferences among workers could be declining while RTW introduces an additional shock that changes workers' calculus on whether or not to join or remain members of a union. Additionally, these three hypotheses could be operating to various degrees in different states, further necessitating separate estimated effects for separate states. While I do not seek to decompose the effects of RTW according to these three hypotheses, they are useful to acknowledge as a basis for ex-ante expectations on the effects of RTW in a given state.

My paper is most closely related to several applied papers since 2000 that examine RTW's effects on wages, union coverage rates, and employment levels. Chava et al. (2020) examine collective bargaining agreements and firm-level accounting data using a difference-in-differences design with Oklahoma, Indiana, Michigan, Wisconsin, and West Virginia. They find that passage of RTW was associated with an immediate reduction in union wage growth and an increase in firms' employment growth. Farber (2005) uses CPS data from 1983 to 2002 to evaluate two states separately – Idaho and Oklahoma. He utilizes a regression model with industry and state fixed effects to find evidence suggesting that the passage of RTW was associated with reduced non-union wages in Idaho, but not Oklahoma. He also finds no statistically significant association with changes in union wages. Farber motivates this analysis with the idea of a union threat effect, originally presented by Lewis (1963). The threat effect claims that non-union firms wish to remain non-union and thus attempt to mitigate desires by employees to unionize. To mitigate the threat of unionization, non-union firms can pay their employees more in order to mimic union wages. Similar to my paper, Eren and Ozbeklik (2016) also employ a synthetic control method to estimate the effects of RTW, but they only examine Oklahoma. They find evidence that in Oklahoma, RTW decreased private sector unionization rates but had no effect on the total employment rate and private sector average wages.

The most similar literature to this paper is Fortin et al. (2022), who examine the same set of states that I do – Indiana, Michigan, Wisconsin, West Virginia, and Kentucky. Using a pooled difference-in-differences model, and a differential exposure design based on industry unionization rates, they find evidence that RTW lowers wages and unionization rates. However, the precision of their difference-in-differences results are sensitive to the inclusion of the Wisconsin public sector, though all state-specific estimates are negative aside from West Virginia and Kentucky. The differential exposure design attributes a single estimate to RTW broadly and does not consider heterogeneous effects by state. Another contribution of Fortin et al. (2022) is that they identify how RTW effects could potentially vary between the public sector and private sector, and also how RTW effects could vary between industries due to different union coverage rates. Fortin et al. (2022)'s insights into differing effects among industries lead me to control for a state's industry composition in my analyses.

3 Methodology

In estimating RTW's effects on union coverage rates, average hourly wages, the union wage premium, the wage distribution, union elections, and measures of employment, I use a synthetic control method described by Abadie (2021). This method is designed to estimate treatment effects on an aggregate outcome of interest after a large, aggregate unit receives a unit-level intervention. This application estimates the effects of RTW for different outcome variables after a state enacts RTW. The setup is similar to a difference-in-differences approach but attempts to create a more valid counterfactual by weighting control units to best match the treated unit in the pre-treatment period. To do this, the synthetic control finds a convex combination of untreated units that best approximates the treated unit on some set of observable variables in the pre-treatment period. Synthetic control relies on the identifying assumption that there exists a convex combination of units in the group of untreated units that can accurately approximate the treated unit for an extended period of time and that this approximation would hold in the absence of treatment. The synthetic control also provides a more transparent counterfactual than usual difference-in-differences approaches. Whereas difference-in-differences approaches do not typically report individual observation weights and can feature extrapolation, where observation weights are assigned outside of $[0, 1]$, the synthetic control method creates weights that are all positive and sum to one, making them intuitively easier to understand.[2]

3.1 Setup

Following Abadie (2021), suppose we have a collection of $J + 1$ geographic units (states or counties), each designated by $j \in \{1, ..., J + 1\}$, and that we measure our outcome of interest for τ years, with t_0 representing the first treatment period for some continuous treatment. Then, we observe the outcome of interest for each unit at each year, denoted by Y_{jt}. Suppose we have a single treated state, $j = 1$, and a set of untreated units with $j \in \{2, ..., J + 1\}$. The set of untreated geographic units is referred to as the "donor pool." Let t, with $t_0 \leq t \leq \tau$, be the years in which the treatment state experiences treatment. Define Y_{1t}^I to be the outcome variable for state $j = 1$ and define Y_{1t}^N to be the outcome variable for state $j = 1$ in the absence of treatment. Then, the treatment effect for each year t is given by

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

Since we have observed data for Y_{1t}^I, the challenge in obtaining γ_{1t} arises with estimating a valid counterfactual in the absence of treatment, Y_{1t}^N. To recover $\widehat{\gamma_{1t}}$, the estimated treatment effect, synthetic control uses

$$\widehat{Y_{1t}^N} = \sum_{j=2}^{J+1} w_j Y_{jt}$$

where $W = (w_2, ..., w_{J+1})$ is a vector of weights such that $0 \leq w_j \leq 1$ and $w_2 + \cdots + w_{J+1} = 1$. Now, the only remaining piece is to find the optimal weighting vector $W^\star$ that provides the

[2]Goodman-Bacon (2021) also shows that the treatment effect parameter for two-way fixed effects difference-in-differences with variation in treatment timing is a variance-weighted average treatment effect on the treated. Goodman-Bacon recommends, "If treatment effects are likely to vary over time one should not use TWFEDD [two-way fixed effects difference-in-difference] to summarize the estimated effects" (2021, p. 272).

best counterfactual.

Let $X_1, \ldots, X_{J+1}$ be a vector of observable predictor variables for each unit j with X_1 representing the predictor variables for the treated state $j = 1$.[3] Predictor variables can include variables from a specific pre-treatment period and also include variables averaged over multiple periods of the pre-treatment. These variables are not time-indexed by default and the researcher must choose what variable/period combinations to match on. Let k be the number of predictor variables included in each vector X_j. Then, define X_0 to be the $k \times J$ matrix of predictor variables for untreated units $X_0 = [X_2 \cdots X_{J+1}]$. Synthetic control chooses $W^\star$ that minimizes

$$\sum_{m=1}^{k} v_m (X_{1m} - X_{0m}W)^2$$

where v_m is a weight that reflects the relative importance assigned to the m-th predictor variable.[4] In essence, the synthetic control chooses weights for untreated units in the donor pool to best match a set of defined variable/period combinations in the pre-treatment period, where these weights must be non-negative and sum to one so that the synthetic counterfactual is a convex combination of untreated units. Under the assumption that $\widehat{Y_{1t}^N}$ is a valid counterfactual for Y_{1t} in the absence of treatment, we should see that the weighted average of untreated units closely matches the treated state before treatment. That is,

$$Y_{1\tilde{t}}^I \approx \widehat{Y_{1\tilde{t}}^N} \qquad \text{for } 1 \leq \tilde{t} < t_0$$

This can only be accomplished if there exists a convex combination of untreated units

[3] Predictor variables are variables chosen by the researcher that help "predict" the outcome variable. Typically, predictor variables exhibit high correlation with the outcome variable or have some theoretical basis for inclusion. Attempting to match on these other additional variables helps us create a synthetic counterfactual that reflects multiple other unit characteristics aside from the outcome variable and attempts to alleviate potential omitted variable bias, borrowing from regression terminology. Abadie (2021, p. 401) mentions that this vector of predictor variables can also include pre-intervention values of Y_{jt} and this is usually implemented in practice to improve pre-intervention fit. When Y_{jt} is included as a predictor variable then $Y_{jt} \in X_j$.

[4] v_m is chosen to weight predictor variables based on their predictive power for the outcome variable. In my analyses, v_m is chosen to minimize $RMSPE_{pre}$, which is defined later in this section. For more information on predictor variable weights, see Abadie et al. (2010).

that can approximately match X_1, the vector of predictor variables for the treated state.

I use the permutation methods described in Abdaie et al. (2010) for statistical inference. Abadie et al. (2010) recommend using permutation methods for statistical inference with synthetic control by iteratively assigning placebo treatment status to untreated units in the donor pool. Then, a ratio of post-intervention fit to pre-intervention fit is computed for each placebo assignment and compared to the actual treatment unit. In this approach, the root mean squared prediction error (RMSPE) is computed for the pre-treatment period and post-treatment period where

$$RMSPE_{pre} = \sqrt{\frac{1}{(t_0 - 1)} \sum_{t=1}^{t_0 - 1} (\widehat{Y_{1t}^N} - Y_{1t}^I)^2}$$

and

$$RMSPE_{post} = \sqrt{\frac{1}{(\tau - (t_0 - 1))} \sum_{t=t_0}^{\tau} (\widehat{Y_{1t}^N} - Y_{1t}^I)^2}$$

t_0 is the first treatment period and τ is the final period with observations. Period indexing begins at 1 for the first period with observations, so, $1 \leq t_0 \leq \tau$.

Thus, the ratio for unit j is

$$r_j = \frac{RMSPE_{post}}{RMSPE_{pre}} \quad [5] \tag{1}$$

The basic idea is that larger estimated treatment effects are reflected as a larger $RMSPE_{post}$, thus larger estimated treatment effects would increase r_j. Simultaneously, a better pre-intervention fit in which the synthetic counterfactual closely approximates the treated unit in the pre-treatment period is reflected in a smaller $RMSPE_{pre}$, also increasing r_j. The ratios for the treatment state and placebo treatment states are ranked, with a large r_j in-

[5] $RMSPE_{pre}$ could be equal to 0, making r_j undefined. However, this does not usually occur in practice and it is not a concern in my analyses. To my knowledge, the existing synthetic control literature does not address this potential issue.

dicating that the estimated treatment effects seem too extreme to merely be a product of chance or the mechanics of the synthetic control method. I use Equation (1) as described in Abadie et al. (2010) as a measure analogous to an F-test for overall significance. I rely on rankings of r_j as my measure of statistical significance and refer to it as an "F-Test" in tables and the results section. An "F-Test" of less than 0.05 is statistically significant at the 5% level, indicating that the results are probably not driven by noisy estimates or an ill-fitting synthetic counterfactual. The "F-Test" is not a direct test for the quality of the synthetic counterfactual's pre-treatment fit, but ill-fitted counterfactuals are penalized compared to well-fitted counterfactuals. $RMPSE_{pre}$ could be considered a direct test of the quality of pre-treatment fit, but since the "F-Test" incorporates $RMSPE_{pre}$, I use only the "F-Test" for statistical inference.

3.2 Specification

For the primary results, I use a similar specification for all states and outcome variable combinations. Each outcome variable is examined using a separate analysis. For the union coverage rate, the synthetic counterfactual is created by matching only on the treatment state's union coverage rate during each pre-treatment period individually. For example, Indiana passed RTW in 2012. The predictor variables selected for Indiana's union coverage rate estimates are Indiana's observed union coverage rate in each year, 2000-2011.

In analyses where the union coverage rate is not the outcome of interest, predictor variables include the treatment state's selected outcome variable for each pre-treatment period until Period $Treatment - 2$ (2 years before RTW enactment), along with the union coverage rate of Period $Treatment - 1$ (1 year before RTW enactment). As another example, Indiana's mean wage analysis matches to Indiana's mean wage 2000-2010 for each year individually, and also matches to Indiana's union coverage rate in 2011. It is necessary to omit at least one pre-treatment period of the outcome variable in order to also match on the union coverage rate. If one pre-treatment period of the outcome variable is not omitted, then the union coverage rate would have a variable weight of 0 and the synthetic control method would not construct the synthetic counterfactual to minimize the difference between the treatment state's union coverage rate and the synthetic counterfactual's union coverage rate. By omitting one period of the selected outcome variable, I ensure that the synthetic counterfactual also approximates the treatment state's union coverage rate in the year immediately prior to RTW enactment. Since theoretical predictions of RTW's effects on other outcome variables, such as wages, consider RTW as a shock to union bargaining power, we want the synthetic control to also consider union bargaining power. It is plausible to expect that the level of union bargaining power within a state could influence the magnitude of effects. Taking the union coverage rate as a proxy for relative union power within a state, we want the synthetic counterfactual to exhibit a similar union coverage rate to the treatment state in the year immediately prior to RTW enactment. Additionally matching on trends in the union coverage rate would be ideal. However, introducing multiple periods of the union coverage rate as predictor variables generally limits the ability of the synthetic control to approximate its treatment state in the pre-treatment period. Thus, because we want the synthetic control to closely approximate the true RTW state at the time of enactment, I believe the benefits of matching on the level of union coverage in the year prior to RTW enactment outweigh the cost in pre-treatment fit introduced by explicitly matching on union coverage rate trends.

In all analyses, levels of other observable state characteristics are also computed for the synthetic counterfactual, although they are not initially included as predictor variables. These include the union coverage rate, mean hourly wage, mean hourly union wage, mean hourly non-union wage, standard deviation of wages, the union wage premium, percent of workers employed by the public sector, percent of workers who are white, percent of workers who are male, percent of workers who live in a metropolitan area, average education of workers, total employment in the state, and the unemployment rate of the state. In creating the synthetic control, we want the synthetic state to match the treatment state on other observables at the time of treatment. These other covariates are compared between the synthetic state and the treatment in the year immediately prior to RTW enactment. Substantial discrepancies between the synthetic state and its treatment state are addressed in robustness checks.

To account for differences in state industrial composition, the percent of workers employed in seven middle- and high-unionization industries are also computed for each state and compared to its respective synthetic counterfactual in the year immediately prior to enactment. It is well-documented that union membership varies greatly between industries (Curme et al., 1990). It could be that passage of RTW occurs at a similar time as a specific industry shock, leading us to perceive the effects of an industry shock as an effect of RTW. Including measures of industrial composition as predictor variables in the synthetic control specification generally reduces the synthetic counterfactual's ability to closely approximate the true treatment state in the pre-treatment period, leading to unreliable estimates. Therefore, I do not include industrial composition as predictor variables directly in the specification for main results. However, if there are large differences between a state's industrial composition of the seven middle- and high-unionization industries and that of its synthetic counterfactual, then this is addressed in robustness checks. The inspiration to only use middle- and high-unionization industries comes from Fortin et al. (2022), who use six middle- and high-unionization industries as controls in their difference-in-differences specification that examines RTW's impacts on the union coverage rate and wages. The industry categories that I use to compute industrial composition are: manufacturing, transportation and utilities, health, education, telecommunications, construction, and public administration. Figure 2 shows the union coverage rate for broad industry categories. The seven middle- and high-unionization industries are industries that did not exhibit a union coverage rate below 10% for all years 2000-2020.

While the synthetic control specification for each outcome variable is the same across all states, the donor pool from which the synthetic control is constructed may change. Some state/outcome variable combinations use entire states as the donor units, while others use individual counties as donor units. I choose between a donor pool of whole states and a donor pool of individual counties depending on which option provides a synthetic counterfactual that better approximates the treated state in the pre-treatment period. This is measured through $RMSPE_{pre}$. There are a limited number of counties identified through the CPS's Merged Outgoing Rotation Groups (my primary data source) and this does not include every county in the US. Thus, using counties as the donor pool does not always create a better pre-treatment fit between the synthetic counterfactual and its respective treatment state. For some state/outcome variable combinations, a donor pool consisting of states provides a lower $RMSPE_{pre}$, and for other state/outcome variable combinations, a donor pool consisting of individual counties provides a lower $RMSPE_{pre}$.

4 Data

4.1 Current Population Survey Merged Outgoing Rotation Groups

I rely on the Current Population Survey (CPS) Merged Outgoing Rotation Groups (MORGs) extracts from 2000 to 2020, available from the National Bureau of Economic Research, to estimate state-by-year and county-by-year union coverage rates, aggregate wage information, and other state and county characteristics including demographic information and industry composition.[6] I restrict my sample to non-agricultural, non-self-employed, currently employed wage and salary workers aged 16-65. For analyses using counties to construct the synthetic control, I drop counties with less than 25 wage observations or less than 50 total observations to limit precision deficiencies of county-level aggregate estimates. For synthetic control analyses using counties, only counties with more than 25 wage observations and more than 50 total observations for each year 2000-2020 are used.

[6] The NBER's extracts are available at `https://www.nber.org/research/data/current-population-survey-cps-merged-outgoing-rotation-group-earnings-data`

To maintain comparability with existing literature and avoid biases created by workers who do not know if they are technically a union member, I use union coverage rate as my measure of unionization for all analyses. The MORGs asks two union related questions: "On this job, is ... a member of a labor union or an employee association similar to a union?" and "On this job, is ... covered by a union or employee association contract?" I consider a worker covered by a union if they respond yes to either of these questions.

I use hourly wages deflated to 1999 dollars as my income measure. Wage cleaning follows Lemieux (2006) and uses directly reported hourly wage information from workers who are paid by the hour and a computed hourly wage for workers not paid by the hour, created by dividing weekly income by usual hours reported. Workers with allocated wage earnings are dropped from the sample because the CPS does not consider union status when imputing wages, resulting in potentially inaccurate wage observations (Hirsch and Schumacher, 2003). I use a Pareto distribution to estimate top-coded wage values to smooth the top of the income distribution (see Firpo et al., 2018 for more information). All estimates of state-by-year and county-by-year average wages are created by weighting individual wage observations with an hourly earnings weight. The hourly earnings weight is usual hours worked multiplied by CPS determined earnings weights. To estimate the union wage premium, I use a semi-log regression for each state-by-year and county-by-year similar to Hirsch and Macpherson (2003) where *union* is a dummy variable that takes a value of 1 if that individual is union covered and 0 otherwise:

$$\ln(wage) = \beta_0 + \beta_1 union + X + u$$ [7]

The MORGs also includes other demographic information including race, sex, metropolitan status, public sector employment status, and industry. Based on CPS industry classifications, I create dummy variables for 11 broad industry categories which include mining, oil

[7] X is a vector of control variables that includes education, sex, race, marital status, region, metropolitan status, potential experience interacted with sex, 11 industry and occupation categories, and dummies for public sector and part-time status.

and gas, construction, manufacturing, trade, transportation and utilities, FIRE (financial, insurance, and real estate), healthcare, education, telecommunications, and public administration. Based on these indicator variables for each individual worker, I use CPS observation weights to estimate percent white, percent male, percent metropolitan, percent public sector, and the proportion of workers employed in each broad industry category for each state and county by year.

4.2 National Labor Relations Board (NLRB)

After the National Labor Relations Act created the NLRB in 1935, all elections regarding union representation were conducted by the NLRB. Available on the NLRB's website are annual reports of all union elections in the US by year from 2001 to 2023, and these reports include the location of the firm holding the representation election, the election results, and the date the case was opened.[8] For election data after 2011, I track the number of union representation elections held for each state by year using these reports. For election data 2000-2011, I use a repository created by John-Paul Ferguson.[9] It is worth noting that union organizers may choose an alternative path to elections by persuading employers to voluntarily recognize a bargaining unit. Data on voluntary representation is not readily available by state from the NLRB, so I only use election filings as my measure of union organizing.

4.3 Bureau of Labor Statistics

I use the US Bureau of Labor Statistics's state-level estimates of total employment and unemployment rates as my measure of both variables. These are publicly available as time-series data from the Bureau of Labor Statistics's website.[10] Total employment estimates by

[8] NLRB election reports can be found at `https://www.nlrb.gov/reports/agency-performance/election-reports`

[9] John-Paul Ferguson's repository is available at `https://github.com/jpfergongithub/nlrb-cats`

[10] Time-series data on total employment by state can be found here. Time-series data on unemployment rate by state can be found here

state-by-year come directly from the Bureau of Labor Statistics and no additional data cleaning is performed. Unemployment rates are seasonally adjusted and provided on a monthly basis. I average over all twelve months in each year to compute a yearly average of the unemployment rate by state and use this measure in my analyses.

5 Results

In all analyses comparing the treatment state to its synthetic counterpart, the first treatment period is the year in which RTW was enacted because the aggregate observation for that year incorporates microdata after RTW was implemented, thus exposing this aggregate value to treatment. For the main figures (Figures 4-12), the difference between each state's outcome variable and its respective synthetic counterfactual's outcome variable is presented for all five states with Period 1 corresponding to the year in which RTW was enacted. Period 0 and negative period values correspond to the pre-treatment period. If the synthetic counterfactual can closely approximate the true state in the pre-treatment period, we should see a difference in outcome variables close to zero for negative period values and only see large positive or negative differences after Period 0. Point estimates for estimated treatment effects by period can be found in Appendix D. The donor pool, from which the synthetic counterfactual is created, consists of states that were not RTW states in 2010 for analyses using states as the donor units. The donor pool consists of individual counties that did not belong to RTW states for analyses using counties as the donor units. Additionally, other states that enacted RTW after 2010 are excluded from the donor pool, as well as their individual counties. I use states as the donor units if the $RMSPE_{pre}$ is lower using states and I use counties as the donor units if the $RMSPE_{pre}$ is lower using counties. A lower $RMSPE_{pre}$ means that the synthetic counterfactual approximates the true state in the pre-treatment period better, likely indicating that it serves as a more valid counterfactual in the post-treatment period.

As pointed out by other literature (e.g., Blanchflower and Bryson, 2004; Freeman, 1988), there is a large disparity between public sector and private sector union coverage rates (see Figure 3). With such a large difference in union coverage rates, union bargaining power could also differ between public and private sectors and lead to heterogeneity in the effects of RTW on coverage rates and wages. Therefore, for the union coverage rate and wages, I first examine pooled values with both sectors then move to separate examinations of the public and private sectors.

While Wisconsin enacted the typical RTW legislation in 2015 that outlaws all union shop arrangements, the state passed legislation that only applies to public sector workers and includes RTW provisions in 2011 (Act 10).[11] For the Wisconsin pooled analyses, I use 2011 as the initial treatment period. Then, for Wisconsin pooled analyses, the estimated treatment effects can be thought of as the result of RTW in 2015 in addition to Act 10 in 2011. Wisconsin's public sector specifications use 2011 as the initial treatment period and Wisconsin's non-public sector specifications use 2015 as the initial treatment period.

I include 2019 and 2020 in my main results despite the COVID-19 pandemic's affect on the labor market. Under the assumption that COVID-19 impacted the synthetic counterfactual in a similar manner to the true treatment state, 2019 and 2020 should still provide reliable estimates. This assumption seems reasonable given that many different observable state characteristics of the synthetic control closely resemble those of the true treatment state in the year prior to RTW enactment, including the state's industrial composition, union coverage rate, percent metropolitan, percent white, and average education. However, it is possible that these state characteristics could change between the treatment year and 2019-2020. If this is the case, then the estimates for 2019 and 2020 may not be accurate.

Appendix A provides synthetic control balance tables for my main results. These balance tables compare the treatment state's characteristics to those of its constructed synthetic

[11] Act 10 included RTW type provisions, where public sector unions could not require employees to pay union dues as a condition of employment. Additionally, Act 10 required public sector unions to hold certification elections every year in order to maintain their status as exclusive bargaining units. Act 10 also included extra measures that could potentially hinder union bargaining power, including restricting public sector unions to only bargain on "base wages" with potential raises capped by the CPI, and limiting collective bargaining agreement contract length to one year (Nack et al., 2020).

counterfactual in the last pre-treatment period (Period 0), including industrial composition. These balance tables are used to identify substantial differences between a state and its synthetic counterfactual in the year immediately prior to RTW enactment. Substantial differences are examined further in robustness checks.

5.1 Main Results

5.1.1 Union Coverage Rate

The strongest results come from each state's union coverage rate analyses. Figure 4 shows the results. All states exhibit negative estimated treatment effects on the pooled union coverage rate (public and non-public sectors together) after RTW is enacted, suggesting that RTW does reduce a state's union rate. But, the magnitudes of estimated effects vary. Looking at Panel (a), Wisconsin shows a large decline relative to its synthetic counterfactual once Act 10 is passed in Period 1, then shows another large decline beginning in Period 5, the year in which RTW was enacted, indicating that Wisconsin's pooled results are not solely due to Act 10. The only other state that exhibits a sustained decline of similar magnitude is West Virginia. All states are statistically significant at the 5% level except for Kentucky (see Table 3).

After Wisconsin, West Virginia experiences the largest estimated effect on its union coverage rate, then followed by Michigan and Indiana. West Virginia exhibits a similar magnitude of estimated treatment effects and additionally seems to follow a similar trend to Wisconsin. However, because there are only five treatment periods for West Virginia, the difference between West Virginia and its synthetic counterpart never reaches 5 percentage points, which Wisconsin achieves in Periods 6-9. The majority of estimated treatment effects for Michigan and Indiana are less than 1 percentage point for both states. Michigan's union coverage rate was about 3 percentage points less than its synthetic counterpart in Period 4, but this large difference attenuates after Period 4. Since this difference attenuates and does not seem to follow the general trend of Michigan's estimated treatment effects, it seems as though Michigan's Period 4 estimated treatment effect could be due to other confounding factors besides RTW. Although Kentucky shows a large difference in Period 4, it does not exhibit a sustained, negative trend like Wisconsin and West Virginia. This suggests that RTW had little effect on Kentucky's union coverage rate within the four post-enactment years examined.

Before RTW enactment, each treatment state is within two percentage points of its respective synthetic counterfactual throughout the pre-treatment period, indicating a plausible counterfactual. Additionally, the synthetic controls for Wisconsin, Indiana, and Michigan are all within five percentage points of their respective state's industry proportions besides manufacturing. West Virginia's synthetic control is within five percentage points of West Virginia's industry composition, though it is more than 30 points off from West Virginia's percent of workers in a metropolitan area. Appendix A provides balance tables that compare the treatment state and its synthetic counterfactual on a number of other variables. Manufacturing proportion, metropolitan status, and other state characteristics are explored more in robustness checks.

Looking to the public sector union coverage rate, most states again experience a negative estimated effect over multiple years besides Indiana. The magnitudes of the estimated effect are larger than the pooled analysis and the non-public analyses, probably due to higher union coverage rates in the public sector before RTW. The estimated treatment effects of RTW on Indiana, Michigan, and West Virginia's public union coverage rates are much smaller in magnitude than the estimated effects for Wisconsin, likely indicating that Wisconsin's public sector estimates are an inflated upper-bound on the effects of RTW because of Act 10's additional provisions.[12] Although Indiana's public sector union coverage rate declined relative to its synthetic counterfactual in Periods 1-2, this decline is not sustained as in other states, creating doubts as to whether RTW actually reduced Indiana's public sector union coverage rate. Appendix C provides statistical significance of both the public sector and

[12] Act 10 also included other measures designed to limit union power besides the typical RTW provision. See Nack et al. (2022) for more.

non-public sector union coverage rate estimates.

Figure 4, Panel (c) provides the results for the non-public union coverage rate. Here, 2015 is the first treatment year for Wisconsin because that is when Wisconsin passed non-public sector RTW legislation. Indiana and Wisconsin are the only states that sustain a negative estimated effect for multiple continuous periods, though Indiana, Wisconsin, Michigan, and West Virginia are all statistically significant. Interestingly, the synthetic control analysis suggests that RTW increased Kentucky's non-public union coverage rate. However, most estimated treatment effects are less than 1 percentage point, and this small difference could be due to noisy estimates.

The synthetic control results suggest that RTW (and Act 10 in the case of Wisconsin) generally reduces the union coverage rate. RTW had a strong effect on Wisconsin and West Virginia, with smaller effects in Indiana and Michigan. In all states besides Indiana, the estimated treatment effect is larger for the public sector. In Wisconsin, Act 10 had a much larger impact on public sector union coverage with an average treatment effect of -19.31 percentage points, while RTW had a smaller effect in the non-public sector with an average treatment effect of -1.70 percentage points (averaged over all treatment periods). Michigan's decline also seems to be concentrated in the public sector as multiple periods in Michigan's non-public analysis predicts that RTW actually increased Michigan's non-public union coverage rate. Meanwhile, West Virginia and Indiana's declines in the union coverage rate are not as concentrated in the public sector.

When averaging the estimated treatment effect for each year over the number of post-treatment periods, my results are largely similar to the difference-in-differences results of Fortin et al. (2022). Using these averages as an average treatment effect for each state, I find that RTW reduced the union coverage rate by about 0.75 percentage points in Indiana, 0.67 percentage points in Michigan, 19.31 percentage points in the Wisconsin public sector, and 1.7 percentage points in the Wisconsin non-public sector. These estimates are all within 0.25 percentage points of Fortin et al.'s state-specific estimates (Fortin et al., 2022, Table

3), providing strong evidence from two different methodologies that these estimates are relatively accurate. The only large difference comes from West Virginia. I find that the average treatment effect on West Virginia is around -2.57 percentage points, while Fortin et al. (2022) find a positive estimate of about 0.7 percentage points. This difference is probably due to differences in the composition of West Virginia's counterfactual. Fortin et al. (2022) use all non-RTW states as the non-treated comparison group, while I use a composition of various counties that closely approximates West Virginia's state characteristics in the year immediately prior to RTW passage.

5.1.2 Average Hourly Wages

Besides the union coverage rate, I find evidence that RTW generally reduces a state's average hourly wage and average hourly non-union wage, though only Michigan's estimates are statistically significant at the 5% level. Figures 5 and 6 display the results and Table 3 displays the associated statistical significance. All states show larger estimated effects in the public sector average hourly wage than non-public sector average hourly wage. This could be due to higher union coverage rates in the public sector and larger effects of RTW on public sector union coverage rates compared to the non-public sector. Interestingly, Wisconsin, which saw the largest decline in its union coverage rate, shows the smallest decline in average hourly wage relative to its counterfactual.

Meanwhile, the average union wage results are mixed, with some states exhibiting positive estimated treatment effects and some states exhibiting negative estimated treatment effects. Figure 7 displays the results for average union hourly wages and Table 3 displays the corresponding statistical significance. The mixed results for average hourly union wages are interesting. Taking RTW as a shock to union bargaining power, it seems plausible to expect union wages to decrease. Perhaps RTW does not universally decrease union bargaining power and instead shifts bargaining power differently for states with different union environments. These results could also be explained by shifting union composition, where low-wage union workers choose to leave union membership after RTW is passed, raising the average union wage and lowering the average non-union wage. However, if this was the case, we would expect to see an increase in average union hourly wages similar to Wisconsin and Indiana for all states. It is possible that this response only occurred in Wisconsin and Indiana, though it seems unlikely.

Michigan is the only state that provides strong evidence that RTW had an actual effect on wages. The average hourly wage over the post-treatment period is about \$17.47 for synthetic Michigan, and \$15.91 for the true Michigan (1999 dollars). From a back-of-the-envelope calculation, this is a roughly 8.93% reduction in Michigan's average hourly wage over the period 2013-2020. RTW seems to have had a larger relative impact on union wages than non-union wages in Michigan. The estimated decrease in Michigan's average hourly union wage is about 7.12% averaged over the post-treatment period and the estimated decrease in Michigan's average hourly non-union wage is only about 4.24% averaged over the post-treatment period. This is expected since RTW could influence wages through a reduction to union bargaining power, leading to larger effects in the union sector. The negative estimated effects on Michigan's non-union wage provide some support for a union threat effect, where non-union firms increase wages to dissuade unionization. RTW could limit the perceived threat of unionization, leading non-union firms to adjust their wage schedules and decrease the average non-union wage. There is some concern with synthetic Michigan having a manufacturing proportion roughly 7 percentage points less than true Michigan in the year before RTW was enacted (see Appendix A). Since the true Michigan has a larger proportion of workers in manufacturing, a shock to the manufacturing sector in the same year as RTW enactment could be mistaken for an effect of RTW here. That is addressed in the robustness checks section.

5.2 Robustness Checks

For robustness checks, I focus on the four main results that provided the strongest evidence of RTW (and Act 10 in the case of Wisconsin) having an observable effect: the pooled union coverage rate in Wisconsin, Indiana, Michigan, and West Virginia, and the mean wage, union wage, and non-union wage in Michigan. All figures displaying the results for robustness checks can be found in Appendix B.

5.2.1 Covariate Matching

First, I return to the earlier issue of states not matching to other covariates in the year prior to RTW enactment. If the synthetic state is different from the treatment state on other observable state characteristics, then that could bias results. Therefore, we want the synthetic state to match the true treatment state on other observables in the year prior to RTW enactment, so that the synthetic state and treatment state look roughly similar at the time of treatment. I avoid adding other state characteristics as predictor variables in the main results because matching on too many predictor variables limits the ability to match to the outcome variable in the pre-treatment, reducing reliability in the estimated treatment effects. There can be large discrepancies between the synthetic control and its treatment state in other observable characteristics because of this. Thus, I use a new synthetic control specification that additionally matches on other predictor variables where such a discrepancy is alleviated in this new model. The details of this specification can be found in Appendix B.

All covariate balance tables from the main results can be found in Appendix A. Industry proportions are chosen as a robustness check if the initial result featured a synthetic control that was more than five percentage points away from any industry proportion of the treatment state in the year prior to enactment. Wage values are chosen if the synthetic control is more than $1.00 away from the treatment state in the year prior to enactment. The union wage premium is chosen as a robustness check if the synthetic control is more than 0.1 ln points away from the treatment state in the year prior to enactment. Though some point estimates are smaller in magnitude than the previous estimates, the covariate matching results are largely similar to the main results. The only cause for concern is Indiana's union coverage rate. In Indiana's specification that matches to manufacturing proportion, the synthetic control estimates three periods of positive effects on the union coverage rate. Thus, the earlier results for Indiana's union coverage rate should be taken with caution.

5.2.2 In-Time Placebo Treatment

Following Abadie et al. (2015), I repeat the four main analyses using a placebo intervention date before the true RTW implementation period. If the synthetic control provides an accurate counterfactual for the true treatment state, then we should see little divergence before the true RTW implementation period. However, if the main results were driven by the synthetic control method artificially enforcing a fit in the pre-treatment period, then the synthetic state may not be a valid counterfactual in the post-treatment period. To test for this, I restrict the period examined to only pre-treatment periods (only periods before RTW enactment). I then use three placebo intervention dates: two years before true enactment (T-2), four years before true enactment (T-4), and six years before true enactment (T-6). The in-time placebo tests provide an additional level of confidence in the main results, but they should not be viewed as entirely disconfirming if they create large estimated treatment effects. Other shocks to the respective treatment state could be occurring in the year chosen for placebo treatment.

The in-time placebo tests for Wisconsin's union coverage rate show no unusual divergence. The only large divergence for Michigan's union coverage rate in-time placebo tests occurs with a placebo treatment date of 2007, leading me to believe that the 2007 result is driven by the Great Recession disproportionately impacting Michigan in comparison to its synthetic counterfactual. Thus, Michigan's in-time placebo tests do not raise large concerns over previous findings. Both Indiana's and West Virginia's in-time placebos for the union coverage rate yield large, negative estimated treatment effects, raising some concern over the validity of earlier findings for these two states.

With Michigan's wage analysis, all three figures exhibit negative estimated treatment effects that are similar in magnitude to the main results. This raises large concerns over the validity of the main results on Michigan's average wage, average non-union wage, and average union wage. Thus, the earlier results are suggestive that RTW decreased Michigan's average wage, average non-union wage, and average union wage, but we cannot reject the possibility that these earlier findings are a result of over-fitting in the pre-intervention period. However, aside from T-6 in the mean wage and non-union wage in-time placebo results, a large divergence from Michigan's synthetic counterfactual occurs in the pre-treatment period, a feature that is not observed in the main results. This indicates that the synthetic counterfactuals for the in-time placebo are probably unable to provide a valid counterfactual in the placebo post-treatment periods.

5.2.3 Other Policy Shifts

Because RTW typically coincides with Republicans taking complete control of state government, it could be that Republicans pass other policies along with RTW that affect the union coverage rate and wages. Therefore, the results we are seeing could be due to other policies typically pursued by Republicans and not RTW.

Republicans in Wisconsin, Indiana, and Michigan all took complete control of their respective state government in 2011, the same year in which Republicans took complete control in Ohio, Pennsylvania, and Maine. However, Ohio, Pennsylvania, and Maine did not pass RTW. Assuming that Republicans in these three states passed similar policies to Republicans in Wisconsin, Indiana, and Michigan besides RTW, using these three states to construct the synthetic treatment state can plausibly isolate estimated treatment effects to only effects driven by RTW (and Act 10 with Wisconsin). I repeat the union coverage rate analysis for Wisconsin, Indiana, and Michigan using only Ohio, Pennsylvania, and Maine as potential donor states. Wisconsin and Indiana both show divergent trends from each state's respective counterfactual after RTW enactment, providing confidence that Wisconsin's and Indiana's earlier results were not driven by other Republican policy. However, Michigan seems to follow a similar trend to its synthetic control after RTW enactment when looking at the union coverage rate. Similarly Michigan's average hourly wage and average hourly non-union wage follow very similar trends to its respective synthetic counterparts in the post-treatment period. Thus, it is possible that the main results for Michigan's union coverage rate, average hourly wage, and average hourly non-union wage may have been driven by Republican policy initiatives besides RTW. The results for a Republican-controlled synthetic counterfactual for Michigan's average hourly union wage indicate that the main results for Michigan's average hourly union wage were not driven by other typical Republican policy.

West Virginia passed RTW in 2016 and its Democratic governor switched party affiliation in 2017, giving Republicans complete control of West Virginia in 2017, the same year in which Republicans also took complete control in Missouri and New Hampshire. While the Missouri state legislature passed RTW in 2017, the law was never enacted. New Hampshire did not implement RTW in any form. I repeat the union coverage rate analysis for West Virginia, using only Missouri and New Hampshire as potential donor states. The post-treatment trends between West Virginia's union coverage rate and that of the Missouri-New Hampshire counterfactual are not similar. It seems as though West Virginia's earlier results for its union coverage rate are driven by RTW and not other Republican legislation.

5.2.4 Dropping Border States

It is possible that the passage of RTW induces workers to migrate out of the enactment state and into border states. Perhaps, union workers leave the enactment state, lowering the union coverage rate in the enactment state, and enter the workforce in a border state, raising the union coverage rate in the border state. If the synthetic control includes one of these border states, then this could lead to a "double-counting" and bias the main results by overstating the true effect of RTW. I repeat the four main analyses, dropping border states that are non-RTW states from the donor pool. For Wisconsin, this includes Minnesota and Illinois. For Indiana, this includes Illinois and Ohio. For Michigan, this includes only Ohio. For West Virginia, this includes Ohio and Maryland. Dropping border states does not substantially change the results for the union coverage rate of any state or any of the Michigan average hourly wage estimates.

5.3 Additional Results

5.3.1 Std. Deviation of Wages and the Union Wage Premium

Due to the sizable volume of historical literature relating unions to reductions in wage inequality (e.g., Freeman and Medoff, 1984; Collins and Niemesh, 2019; Farber et al., 2021; Western and Rosenfeld, 2011), one might expect RTW to increase wage dispersion by reducing the union coverage rate within a state. I use the standard deviation of wages as my measure of wage dispersion. All states exhibit a predicted reduction in the standard deviation of wages, though no estimated differences are statistically significant. Figure 8 displays the results and Table 4 provides associated statistical significance. This provides suggestive evidence that RTW can decrease wage dispersion, but the magnitude of the effect is likely small if there is a true effect at all.

Turning to the union wage premium, some states showed negative effects and some states showed positive effects, and these predicted effects often returned to zero in the post-treatment period (see Figure 9). The only state that exhibits a sustained divergence from its synthetic counterfactual in the post-treatment period is Indiana, suggesting that RTW increased the union wage premium in Indiana. The union wage premium averaged over all post-treatment periods is about 0.2052 ln points for Indiana and about 0.1393 ln points for its synthetic counterpart. With a back-of-the-envelope calculation, this corresponds to union members being payed 22.78% more in Indiana 2012-2020 and 14.94% more in synthetic Indiana 2012-2020, about a 7.8 percentage point increase due to RTW. Only Wisconsin and

Indiana are statistically significant (see Table 4). The synthetic control predicts a positive effect on the union wage premium in Indiana for all years after enactment, while Wisconsin experiences multiple periods of positive effects then multiple periods of negative effects. While this is not impossible, it seems unlikely that RTW would first increase the union wage premium then decrease it in later years. However, this could be a result of changing union composition or subsequent union organizing efforts after Act 10 and RTW are enacted in Wisconsin that did not occur in Indiana. The union wage premium results show no broad trend across all states, but there is evidence of an effect in Indiana and Wisconsin.

5.3.2 Total Employment and Unemployment Rate

For both total employment and the unemployment rate, all synthetic counterfactuals are constructed using states as the donor units. Figure 10 provides the synthetic control results for total employment and Figure 11 provides the results for each state's unemployment rate. There are no broad trends that emerge from either of these figures, indicating that RTW likely does not have an independent effect on either of these variables. In both figures, there are multiple states with positive estimated effects and also multiple states with negative estimated effects. Indiana and Wisconsin both show a sustained divergence from their respective counterfactuals, though no state's estimates are statistically significant at the 5% level for total employment or the unemployment rate (see Table 4). Michigan's results suggest no effect on its unemployment rate. The difference between Michigan and its synthetic counterfactual is very small in the pre-treatment period and this continues throughout the entire post-treatment period. These results suggest that in general, RTW does not have an observable effect on a state's employment as some proponents of RTW claim.

5.3.3 Union Elections

Synthetic control methods also allow us to examine how the number of union certification elections in each state changes after RTW enactment, providing some insights into union response. I include two analyses of Wisconsin, one with 2011 as the treatment period (when Act 10 was enacted) and one with 2015 as the treatment period (when RTW was enacted). Here, the donor pool consists of non-RTW states for all states examined as the union election data is aggregated at the state level. Thus, counties are not available to create the synthetic counterfactual. Figure 12 displays the results. Indiana, West Virginia, and Kentucky seem to show that RTW had little effect on union elections after Period 1, as the estimated treatment effects seem to group around zero for multiple periods. Using 2015 as the first treatment period, Wisconsin exhibits negative estimated treatment effects for two periods before attenuating toward zero. While Wisconsin with 2011 as the first treatment period and Michigan seem to have a large negative effects for multiple years, the difference between each state and its synthetic counterfactual at the time of treatment cautions against drawing inferences from these results. However, all states saw a decrease relative to their synthetic control in Period 1, the year in which RTW (or Act 10) was enacted. This suggests that RTW potentially decreases union elections in the year of enactment, then future union organizing efforts look to hold more elections to account for the decline. Table 5 provides statistical significance associated with the results, no results are statistically significant at the 5% level.

5.4 A Brief Examination of Missouri – Does RTW Have a Truly Independent Effect?

Previous literature around RTW has proposed the "taste hypothesis," where "RTW laws exist only in states where anti-union sentiment among workers, employers, and the public is substantial" and where "RTW laws do not have an *independent* effect on the demand for, supply of, or extent of union membership, but simply represent underlying hostile attitudes toward unionism" (Moore and Newman, 1985, p. 574). Does RTW have an independent effect on a state's union coverage rate and other state-level variables, or, does RTW merely coincide with unobservable factors that also change state-level variables? Missouri offers a natural experiment to evaluate whether RTW has an independent effect on the union coverage rate and other state-level variables. If RTW does not have an independent effect, then the earlier estimated effects could be due to shifting worker expectations, shifting anti-union sentiment, or other unobservable factors, rather than the actual legislation. In 2017, the Missouri state legislature passed RTW and the legislation was then signed by the governor. But, before the legislation could take effect, voters defeated the law via a statewide ballot proposition in August, 2018.

I estimate a synthetic control for Missouri with each of the outcome variables previously examined, using the same specification from the main results section with 2017 as the treatment date. Assuming that RTW passage in Missouri coincided with other shifts in unobservable factors and assuming that RTW does not have an independent effect on various state-level variables, Missouri's analyses should produce similar results to that of other states that passed RTW. That is, Missouri should produce negative estimated treatment effects during the post-treatment period for its union coverage rate analysis and also produce negative estimated treatment effects for its average hourly wage. This could imply that earlier findings on RTW were not due to a true effect of RTW, but rather due to some other unobservable factor such as shifting union preferences. This is not the case. Appendix E shows the results and associated statistical significance.[13] Unlike the states where RTW actually took effect, the synthetic control for Missouri's union coverage rate predicts positive estimated treatment effects and the synthetic control for Missouri's average hourly wages does not produce negative estimated treatment effects sustained throughout the post-treatment period. Thus, Missouri's results are not consistent with the previous findings of RTW reducing a state's union coverage rate and the suggestive evidence of RTW reducing average hourly wages. Missouri's estimates for its union coverage rate are statistically significant at the 5% level, but no other estimates are.

The ballot initiative in 2018 could be considered a signal that anti-union sentiment in

[13] Missouri's synthetic control uses counties as donor units for all outcome variables besides total employment, the unemployment rate, and union certification elections. For total employment, the unemployment rate, and union certification elections, Missouri's synthetic control is constructed using states as the donor units.

Missouri is not substantial, even though the state government passed the law a year earlier. If that is the case, then the results for Missouri are not a true test for shifting anti-union sentiment. The results then could be considered a test of worker expectations after RTW is passed. Unlike other states examined, Missouri exhibits a larger union coverage rate than its synthetic counterpart in the post-treatment period and additionally shows no sustained effect on average hourly wages. Thus, it seems that RTW does have an independent effect on a state's union coverage rate and may have an independent effect on average hourly wages. The main results from Indiana, Michigan, Wisconsin, and West Virginia do not seem to be driven by shifts in unobservable factors that typically accompany RTW passage.

6 Discussion – Differences in RTW's Effect on the Union Coverage Rate Across States

Why do Wisconsin and West Virginia show large, negative effects of RTW on their union coverage rate while Michigan and Indiana show effects that are smaller in magnitude? There are two likely answers. First, it appears that RTW has a larger effect on a state's union coverage rate if public sector unionization is high and the state has a relatively large public sector. Second, the differences in effect magnitude for the union coverage rate could be due to different strategies adopted by union organizations after RTW is enacted. These two explanations are not mutually exclusive and could be operating simultaneously. In the case of Wisconsin, Act 10 likely plays a large role in this since Act 10 also included other measures designed to limit union bargaining power in the public sector, such as limiting collective bargaining agreements to one year in length and requiring annual re-certification votes (Nack et al., 2020). But, West Virginia also shows larger effects of RTW, even though the state did not pass extra provisions included in Act 10.

Looking back at Figure 4, RTW had a much larger effect on the public sector. Thus, it seems as though the proportion of workers in the public sector can help explain the differences in RTW's effects on each state's total union coverage rate. Figure 13 displays each state's percentage of workers that belong to the public sector and also displays these percentages in the year immediately prior to RTW passage. West Virginia exhibits the largest percentage of public sector workers, followed by Kentucky and Wisconsin. Michigan and Indiana, the two states with small estimated treatment effects, had the smallest proportion of workers in the public sector in the year before RTW. If a larger effect of RTW is associated with a larger public sector, then Kentucky should also exhibit large estimated treatment effects, but it does not. Figure 14 is similar to the previous figure, but it displays the public sector union coverage rate. Kentucky's public sector union coverage is the lowest of the five states. Therefore, it appears that a combination of a larger public sector with a larger public sector union coverage rate leads to larger effects on the total union coverage rate. With a higher public sector union coverage rate at the time of enactment, RTW has a larger effect within the public sector, but RTW's effect on the total union coverage rate is ultimately constrained by the size of the public sector. At the time of Act 10 passage, Wisconsin had a large public union coverage rate and a moderate public sector relative to the other five states. At the time of RTW passage, West Virginia had a moderate public union coverage rate and a large public sector. Meanwhile, Indiana and Kentucky, states where RTW had little to no effects on the total union coverage rate, did not have large public sectors and additionally had low public sector union coverage rates. Although Michigan had the largest public union coverage rate, it also had one of the smallest public sectors.

Another factor that could help explain the differences in RTW's effect on the union coverage rate is the response of local unions. Nack et al. (2020) document that Service Employees International Union (SEIU) in Wisconsin completely abandoned the public sector after Act 10 and relinquished their bargaining units to other labor organizations. The American Federation of State, County, and Municipal Employees (AFSCME) Council 24 and AFT-Wisconsin, two of the states major public sector unions, elected to not try and re-certify many of their locals after Act 10 (Nack et al., 2020). In Michigan, there is some anecdotal evidence that public sector unions had a different reaction to the passage of RTW than their counterparts in Wisconsin. In a 2013 article, Jane Slaughter reports that the AFT in Michigan pushed to sign local contracts before RTW could take effect, locking in contracts that include a union shop provision while they still could. But, in the case of an AFT local in the suburb of Detroit, the rush to sign a contract also came with a 10% pay cut (Slaughter, 2013). Although this is only the response of one particular union, if many unions in Michigan adopted the same strategy as the AFT, then this could be a reason why RTW had a smaller effect on Michigan's union coverage rate. This could also help explain why there is suggestive evidence that Michigan's wages fell while there were little changes to its union coverage rate.

After RTW is enacted, different union organizations may respond to the new union environment differently. Due to geographic proximity and state-level organizational structures that foster close relationships between unions within a state, it is very possible that union response strategies are somewhat coordinated within a state. This is another potential explanation for why there are differences in RTW's estimated effects on a state's union coverage rate. Michigan's, Indiana's, and Kentucky's unions could have adopted organizing strategies that mitigated the loss of union membership after RTW enactment while Wisconsin's and West Virginia's unions did not.

7 Conclusion

Synthetic control methods provide a new examination of post-2010 RTW enactment states and additionally allow us to investigate RTW's effects on aggregate state-level variables such as total employment and the unemployment rate, two state-level measures that have been heavily featured in the policy discussion. In addition to synthetic control's ability to estimate RTW's effect on aggregate variables, I also choose to use this methodology because it possibly creates a more valid counterfactual for the enactment state than typical regression-based approaches. Whereas common regression-based methodological designs, such as difference-in-differences, typically use all non-RTW states as the comparison group, synthetic control creates a weighted average of non-RTW states such that the counterfactual closely resembles the enactment state on a number of important observable characteristics. These include industrial composition, size of the public sector, the union coverage rate, and average education at the time of RTW passage. Synthetic control methods provide evidence that RTW has a negative effect on important state-level variables that policymakers should consider – the union coverage rate, average hourly wages, and average hourly non-union wages. For the other state-level variables examined, it seems that RTW does not have an observable effect that generalizes to all enactment states. Evidence for a negative effect on the union coverage rate is fairly strong, while the evidence around RTW's effect on average hourly wages and average hourly non-union wages is only suggestive. Missouri's analyses provide evidence that the estimated effects for Indiana, Michigan, Wisconsin, West Virginia, and Kentucky are not merely because of shifts in unobservable factors that typically occur at the same time of RTW passage. It seems that RTW does have an independent effect on a state's union coverage rate, only if the law is enacted.

Considering RTW's estimated effects on the union coverage rate, there is substantial heterogeneity across states. RTW seems to have had the largest impact on Wisconsin's union coverage rate, then followed by West Virginia, then Michigan and Indiana. While Wisconsin's results are likely an inflated upper-bound on the effects of pure RTW-style policy, because Act 10 also featured other provisions that could limit union power, Wisconsin's union coverage rate results do not seem to be driven entirely by Act 10. Wisconsin still exhibited a reduction in its non-public sector union coverage rate relative to its synthetic counterfactual after RTW was passed for the non-public sector without the extra provisions of Act 10.

The heterogeneity in the magnitude of RTW's effect on the union coverage rate is likely due to the size of the state's public sector and its union coverage rate in the public sector. RTW disproportionately impacts the public sector. Based on the results, it seems likely that a larger public sector, paired with high rates of unionization in the public sector, lead to a larger reduction in the state's union coverage rate after RTW is enacted. Union organizing strategies aside from union elections, such as contract negotiation strategies, also seem to influence the effects of RTW on a state's union coverage rate. By utilizing various organizing strategies after RTW enactment, unions can perhaps mitigate the effects of RTW. The current literature documenting union tactics after RTW is sparse. Future investigation into how union organizing efforts respond to the passage of RTW could be a fruitful avenue of research and should examine other strategies aside from just union elections. Additionally, future research concerning RTW should consider separate estimated effects for different states and be cautious in attributing a single estimate to RTW broadly.

Although the Michigan wage results are only suggestive, the findings that RTW decreased Michigan's average hourly wage and average hourly non-union wage support the existence of a threat effect where non-union firms raise wages to dissuade the threat of unionization. RTW could send a strong signal that union power is diminishing. Thus, non-union firms perceive a lower threat of unionization and can lower wages. In Michigan, average non-union wages fell immediately after RTW enactment, reflecting a potential response by non-union firms predicted by the theoretical basis of a union threat effect.

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Tables

Table 1: RTW States vs. Non-RTW States 2010

	States			
	RTW	**Non-RTW**	**Difference**	**P-Values**
Union Coverage Rate	7.64%	17.98%	10.34 percentage points	0.00
Avg. Hourly Wage (2010 dollars)	\$19.36	\$22.38	\$3.02	0.00
Union Wage Premium	13.73%	17.31%	3.58 percentage points	0.00

WI, IN, MI, KY, and WV are included in non-RTW states here because none of them had implemented RTW in 2010. Estimates come from the CPS MORGs and the sample is limited to currently employed, non-agricultural, non self-employed workers, ages 16-65. The union coverage rate is estimated using a simple regression of a dummy variable for union coverage on a dummy variable for the individual living in a RTW state. This regression uses individual worker observations and CPS provided composite weights. Both the intercept and RTW dummy have p-values of 0.00, testing the two-sided alternate hypothesis. Wage estimates come from a similar regression with hourly wage as the dependent variable. Wage estimates use a weight created by multiplying a worker's usual weekly hours with the CPS provided earnings weight. Both the intercept and RTW dummy have p-values of 0.00, testing the two-sided alternative hypothesis. The union wage premium is estimated using a specification similar to Hirsch and Macpherson (2003) with a dummy variable for living in a RTW state, a dummy variable for union coverage, and an interaction term for $RTW \times UnionCovered$. Hirsch and Macpherson (2003)'s specification is described under the data section. $\log(wage)$ is the dependent variable so the percentages are computed by taking $100 \cdot \exp(\beta_{RTW \times Union} - 1)$ for RTW states and $100 \cdot \exp(\beta_{Union} - 1)$ for non-RTW states. Both beta coefficients have p-values of 0.00, testing the two-sided alternate hypothesis.

Table 2: Union Cov. Rates in Year Before RTW Enactment

State	Year Before	Year Enacted	Union Cov Rate	Rank	National Avg
Indiana	2011	2012	13.17%	25	13.50%
Michigan	2012	2013	18.03%	8	13.10%
Wisconsin	2014	2015	12.99%	22	12.95%
West Virginia	2015	2016	16.15%	11	13.09%
Kentucky	2016	2017	14.45%	19	12.82 %

Estimates come from the CPS MORGs and the sample is limited to currently employed, non-agricultural, non self-employed workers, ages 16-65. The union coverage rate is estimated using the CPS provided composite weights. Rank is a comparison to all 50 states and the District of Columbia in the year before RTW enactment. For example, Michigan's rank of 8 means it had the 8th highest union coverage out of all 50 states and the District of Columbia in 2012.

Table 3: Synthetic Control "F-Tests"

	Outcome Variable			
State	Union Cov. Rate	Mean Wage	Union Wage	Non-Union Wage
WI	0	0.0833	0.4583	0.1250
IN	0.0145	0.2463	0.1449	0.4202
MI	0	0.0145	0.0145	0
WV	0	0.3333	0.2029	0.4782
KY	0.5072	0.4927	0.3768	0.3478

"F-Tests" are the ranked ratios of Post-treatment RMSPE to Pre-treatment RMSPE. The numbers are rounded to four decimal places.

Table 4: Synthetic Control "F-Tests"

	Outcome Variable			
State	Wage SD	Wage Premium	Tot. Employment	Unemp. Rate
WI	0.2917	0.0417	0.1667	0.7083
IN	0.4583	0	0.1667	0.6667
MI	0.125	0.25	1	0.2083
WV	0.375	0.7083	0.4583	0.7917
KY	0.7083	0.0833	0.7083	0.2917

"F-Tests" are the ranked ratios of Post-treatment RMSPE to Pre-treatment RMSPE. The numbers are rounded to four decimal places.

Table 5: Synthetic Control "F-Tests"

	Outcome Variable
State	Union Elections
WI (2011)	0.917
WI (2015)	0.917
IN	0.5
MI	0.5
WV	0.625
KY	0.5417

"F-Tests" are the ranked ratios of Post-treatment RMSPE to Pre-treatment RMSPE. The numbers are rounded to four decimal places.

Figures

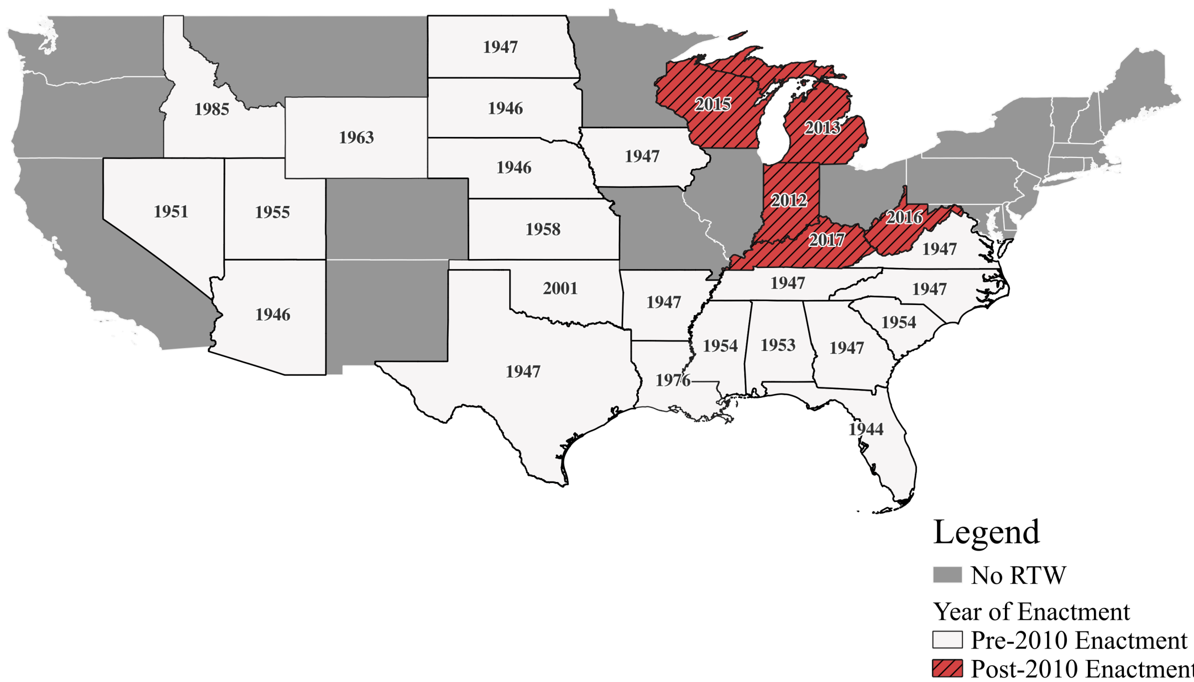


Figure 1: Current RTW States, Grouped by Enactment Year Relative to 2010

Alaska and Hawaii are omitted from the map. As of 2023, neither of these states has an active RTW law.

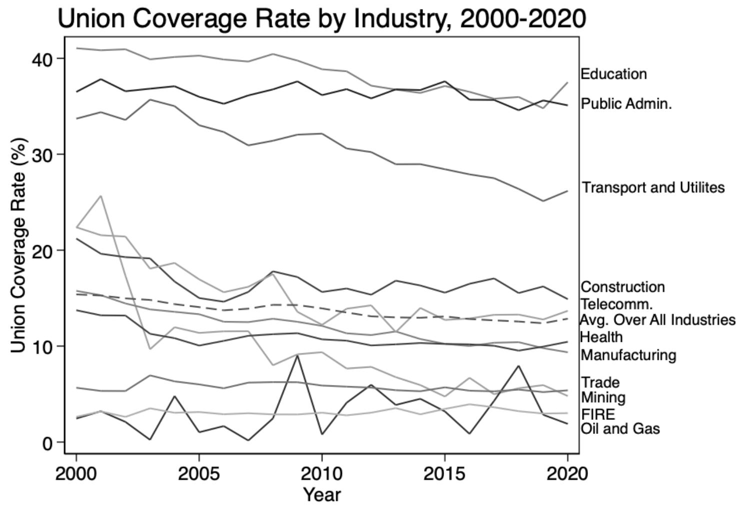


Figure 2: Union Coverage Rate by Industry

Estimates come from the CPS MORGs and the sample is limited to currently employed, non-agricultural, non self-employed workers, ages 16-65. The union coverage rate is estimated using the CPS provided composite weights. Individual industry codes are classified according to broader categories defined by the CPS.

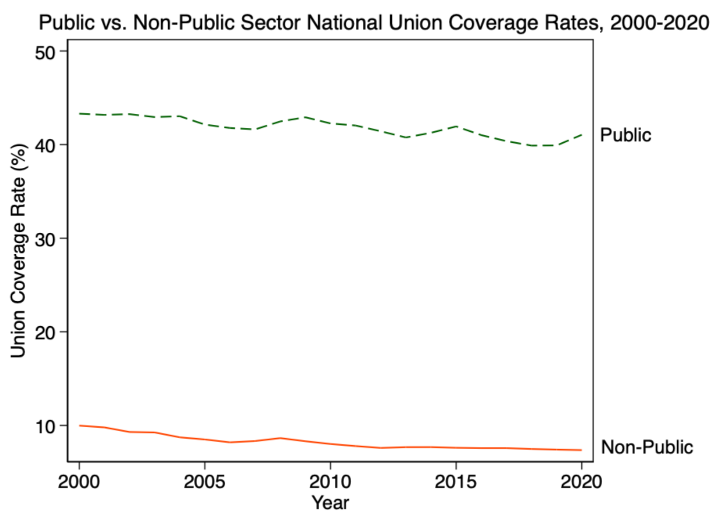
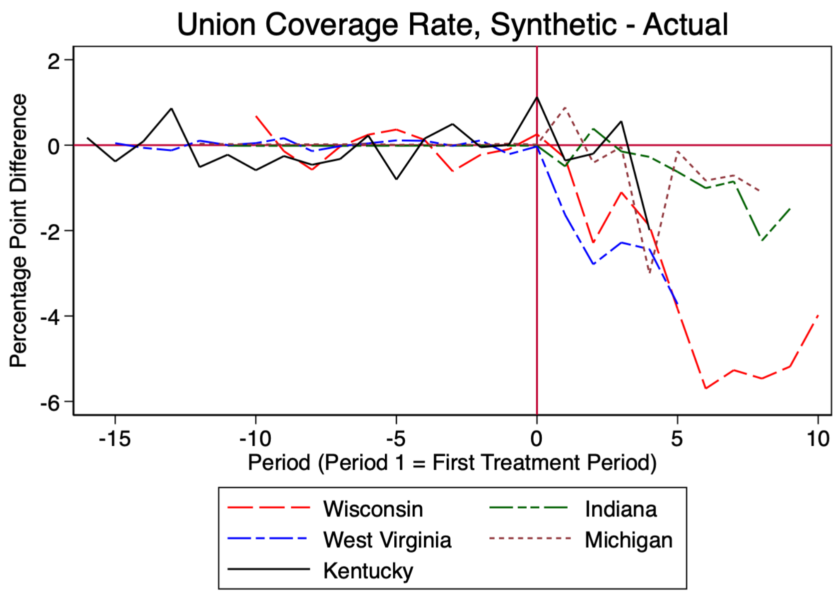
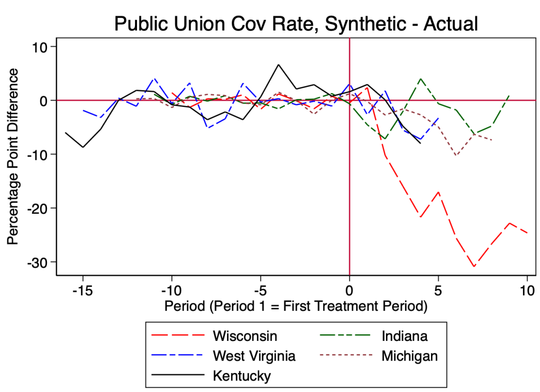


Figure 3: Public vs. Non-Public Union Coverage Rates for the Entire US

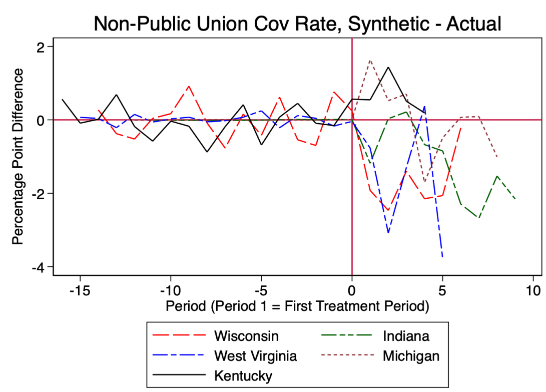
Estimates come from the CPS MORGs and the sample is limited to currently employed, non-agricultural, non self-employed workers, ages 16-65. The union coverage rate is estimated using the CPS provided composite weights. The public/non-public distinction comes from the CPS defined variable, “Class of worker.”



(a) Public and Non-Public Sectors

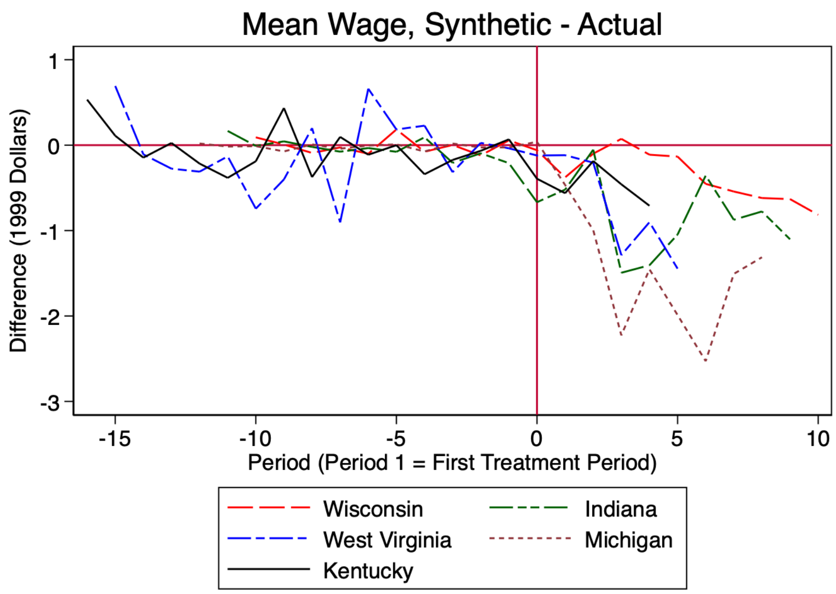


(b) Public Sector Only

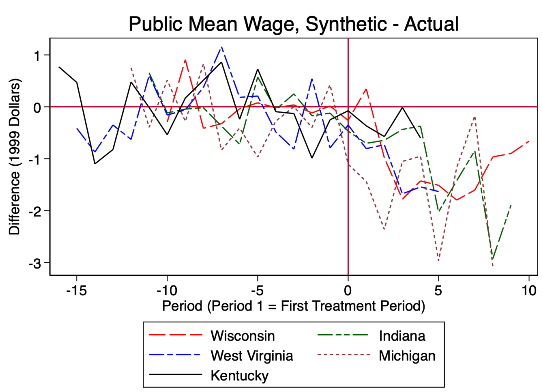


(c) Non-Public Sector Only

Figure 4: Synthetic Control Results for Union Coverage Rate



(a) Public and Non-Public Sectors



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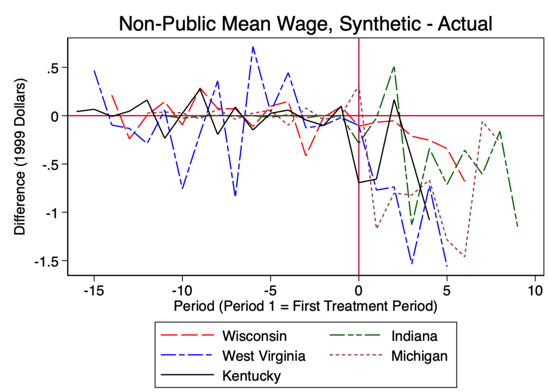
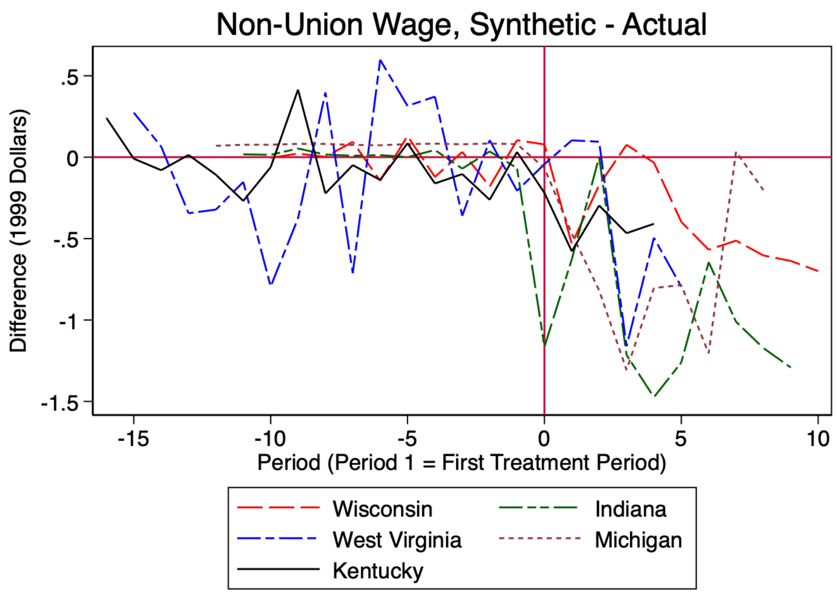


(c) Non-Public Sector Only

Figure 5: Synthetic Control Results for Average Hourly Wages (1999 dollars)



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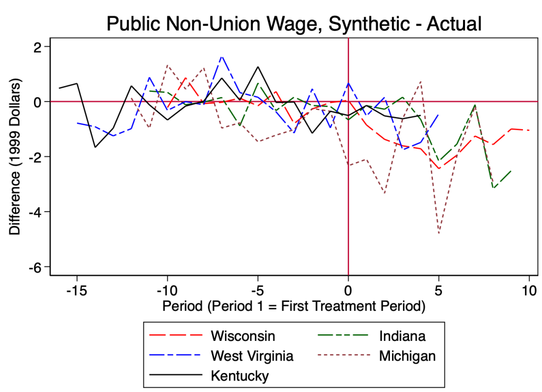

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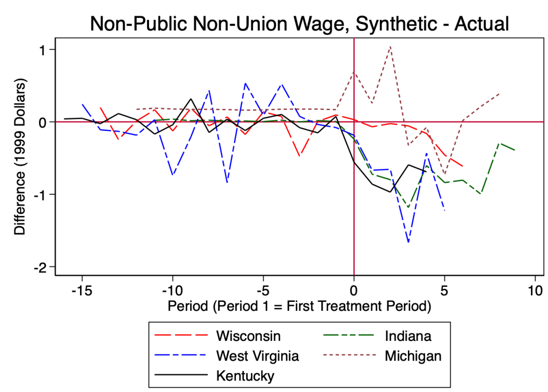
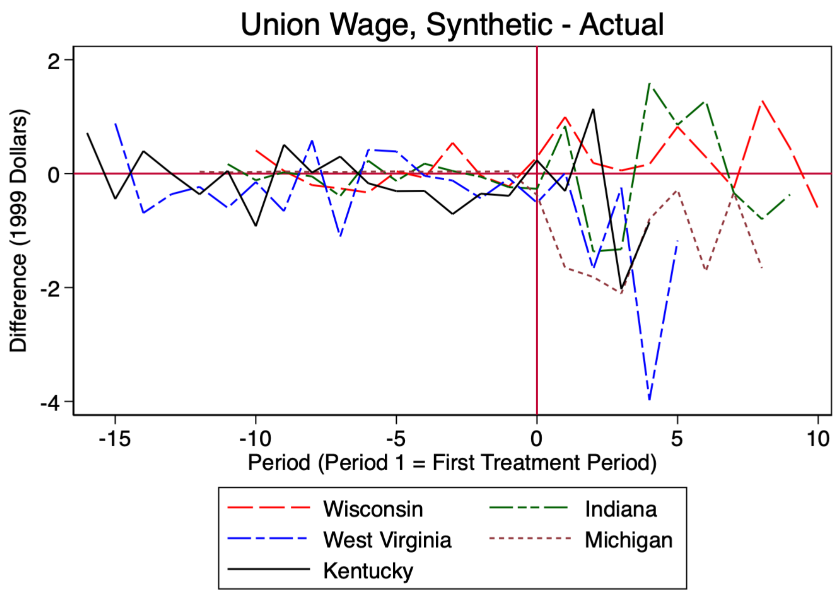
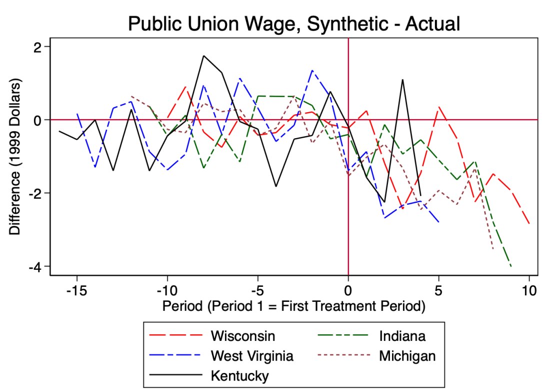


(c) Non-Public Sector Only

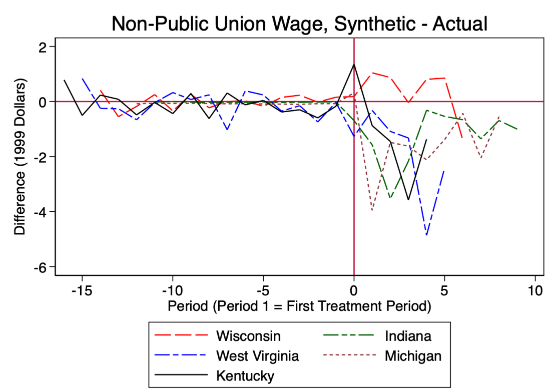
Figure 6: Synthetic Control Results for Average Hourly Non-Union Wages (1999 dollars)



(a) Public and Non-Public Sectors



(b) Public Sector Only



(c) Non-Public Sector Only

Figure 7: Synthetic Control Results for Average Hourly Union Wages (1999 dollars)

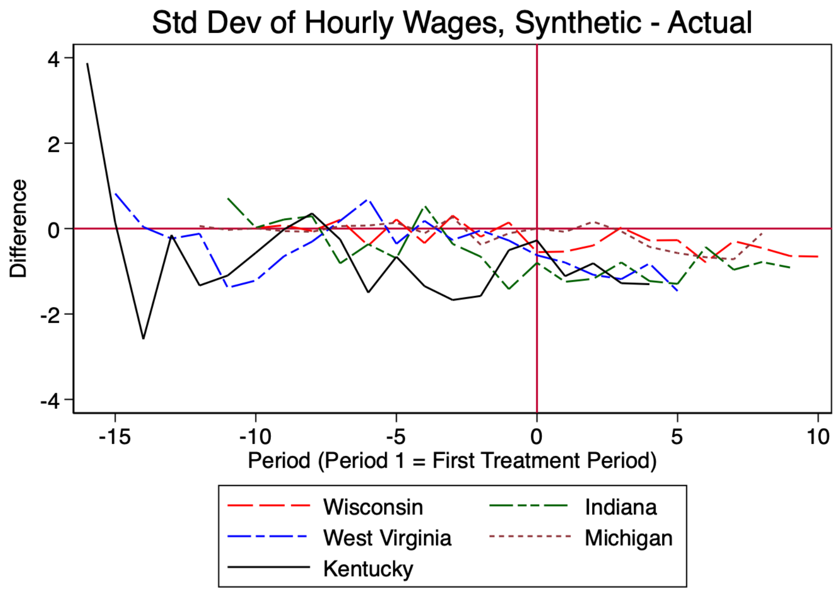


Figure 8: Synthetic Control Results for Standard Deviation of Hourly Wages

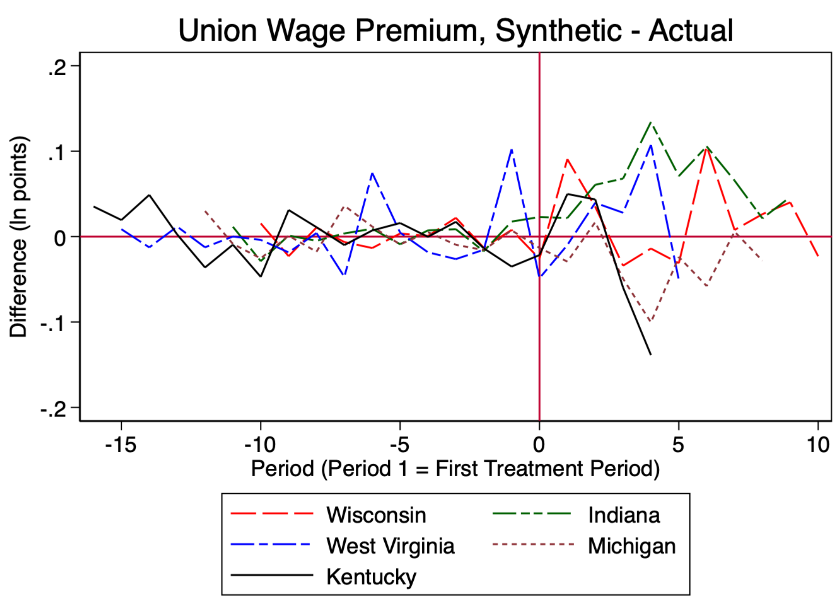


Figure 9: Synthetic Control Results for the Union Wage Premium

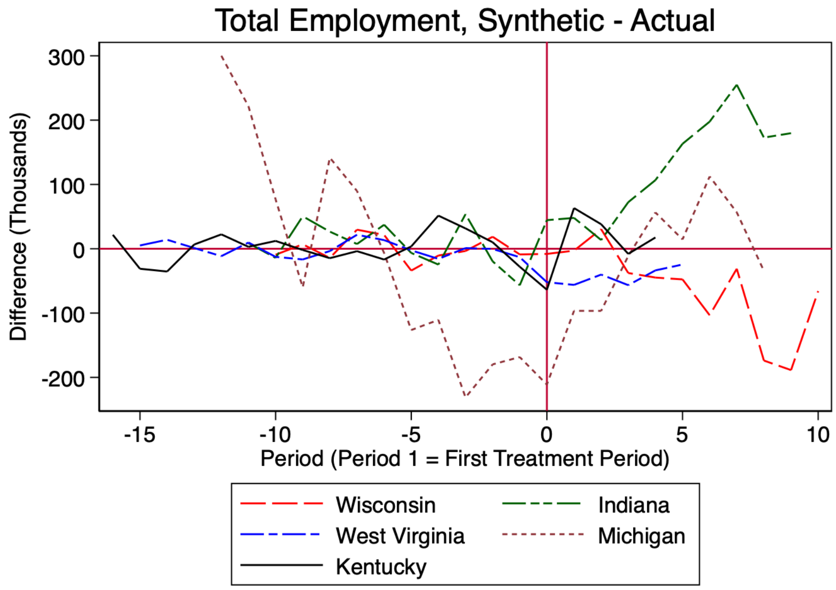


Figure 10: Synthetic Control Results for Total Employment

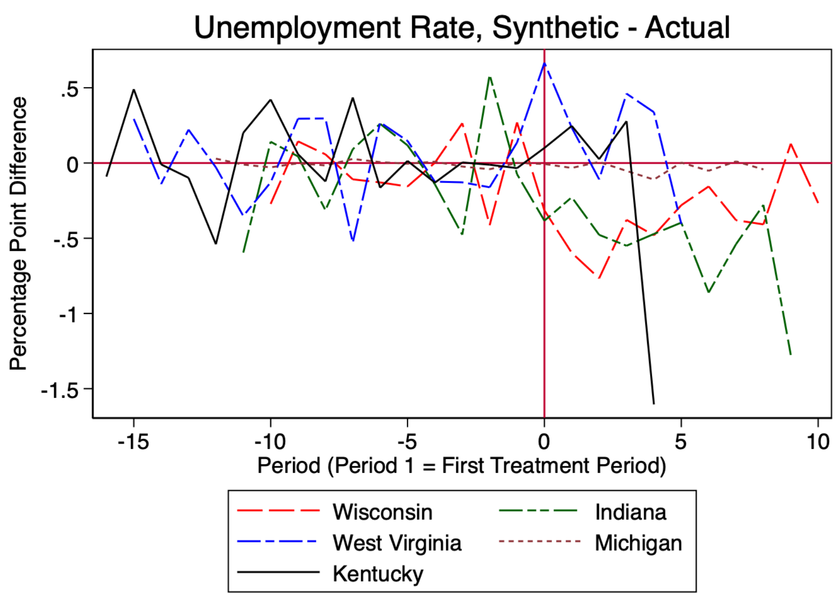


Figure 11: Synthetic Control Results for the Unemployment Rate

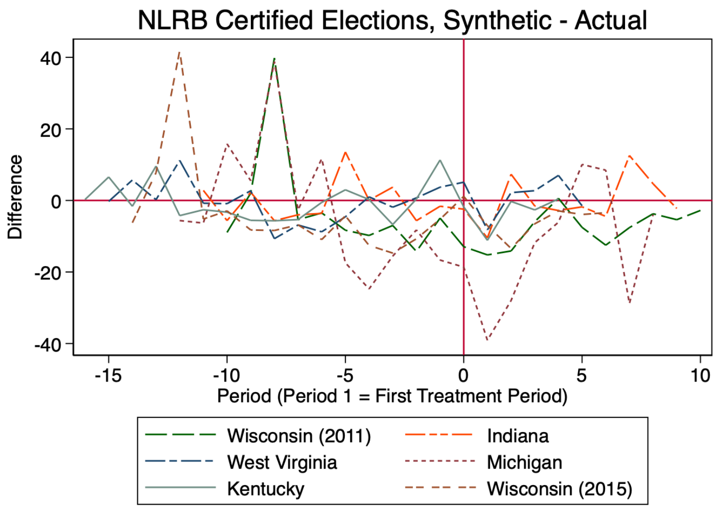
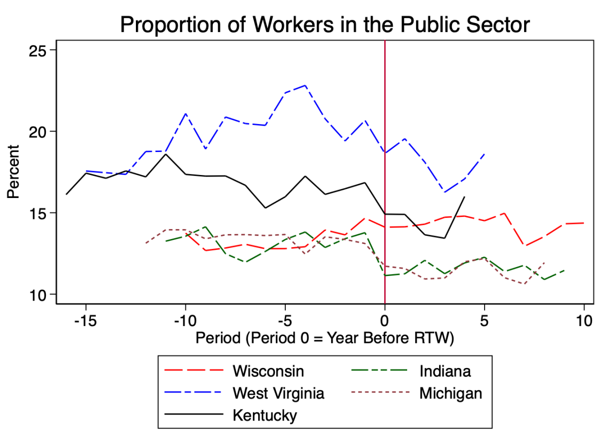
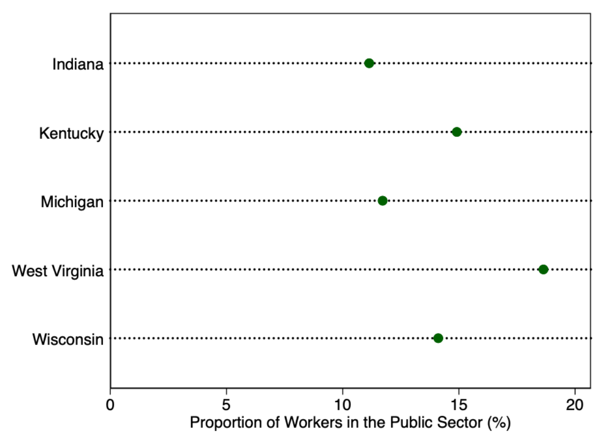


Figure 12: Synthetic Control Results for Union Elections

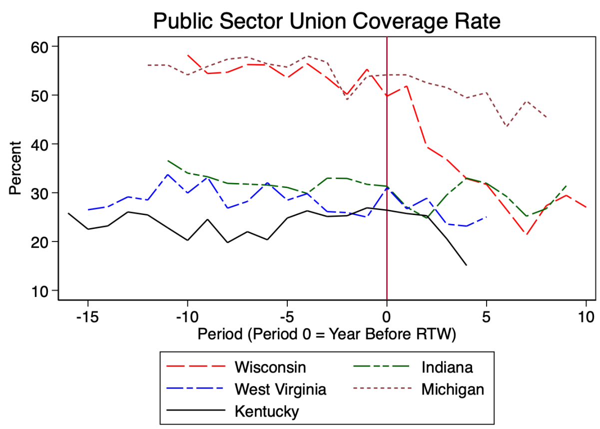


(a) 2000-2020 in Relation to RTW Enactment Year

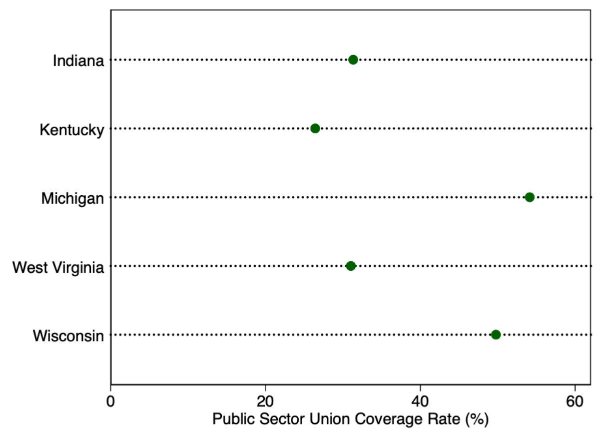


(b) Year Before RTW Enactment

Figure 13: Proportion of Workers in Public Sector by Treatment State



(a) 2000-2020 in Relation to RTW Enactment Year



(b) Year Before RTW Enactment

Figure 14: Public Sector Union Coverage Rate by Treatment State

Appendix A Synthetic Control Balance Tables and Donor Weights

Table A.1: WI Pooled Union Cov. Rate Balance Table

	Treated	Synthetic
Manufacturing Prop. (2010)	17.623%	11.5485%
Transport. and Util. Prop. (2010)	04.45871%	05.21098%
Health Prop. (2010)	12.38085%	12.20769%
Educ Prop. (2010)	10.30267%	09.82107%
Telecomm Prop.(2010)	01.19265%	01.6424%
Construction Prop. (2010)	04.91499%	05.72572%
Public Admin. Prop. (2010)	03.6946%	05.22513%
% Public Sector (2010)	14.11212%	14.92507%
% White (2010)	90.91278%	80.21979%
% Metro (2010)	76.80713%	89.73951%
% Male (2010)	50.20221%	50.55528%
Mean Wage (2010)	$15.37706	$17.38947
Wage Premium (2010)	.1060885	.1605565
Union Wage (2010)	$17.17721	$19.41014
Non-Union Wage (2010)	$15.00452	$16.9729
Unemployment Rate (2010)	8.5%	9.894183%
Total Employment (2010)	2508000	2224466
Avg. Educ. (2010)	13.89695	13.72995

The Treated column displays the levels of each respective variable for the enactment state in the year immediately prior to RTW enactment. The Synthetic column displays the levels of each respective variable for the constructed synthetic counterfactual in the same year. Total Employment and the Unemployment Rate are not included for synthetic counterfactuals constructed using counties as the donor units.

Table A.2: WI Pooled Union Cov. Rate Synthetic Control Composition

State	Weight
DE	0.309
NJ	0.26
OH	0.219
RI	0.211
VT	0.001

This table reports the weights assigned to different donor units to create the synthetic counterfactual. For analyses using states as the donor units, the weights are reported directly from the synthetic control. For analyses using individual counties as the donor units, the weights are given by state that the county belongs to. If multiple counties belong to the same state, their weights are summed. All weights sum to 1, but the reported weights are rounded to three decimal places.

Table A.3: IN Pooled Union Cov. Rate Balance Table

	Treated	Synthetic
Manufacturing Prop. (2011)	19.59691%	09.06452%
Transport. and Util. Prop. (2011)	05.20858%	05.49559%
Health Prop. (2011)	11.55514%	10.35852%
Educ Prop. (2011)	08.6332%	09.00508%
Telecomm Prop.(2011)	01.00743%	01.53829%
Construction Prop. (2011)	06.04012%	05.72731%
Public Admin. Prop. (2011)	04.01066%	07.39747%
% Public Sector (2011)	11.1408%	16.95044%
% White (2011)	90.26142%	82.51225%
% Metro (2011)	72.31595%	100%
% Female (2011)	47.65956%	48.79653%
Mean Wage (2011)	\$13.92454	\$16.59619
Wage Premium (2011)	.1594791	.105213
Union Wage (2011)	\$16.63034	\$16.99663
Non-Union Wage (2011)	\$13.46069	\$16.55325
Avg. Educ. (2011)	13.48736	14.10336

The Treated column displays the levels of each respective variable for the enactment state in the year immediately prior to RTW enactment. The Synthetic column displays the levels of each respective variable for the constructed synthetic counterfactual in the same year. Total Employment and the Unemployment Rate are not included for synthetic counterfactuals constructed using counties as the donor units.

Table A.4: IN Pooled Union Cov. Rate Synthetic Control Composition

State	Weight
PA	0.105
OR	0.037
NY	0.001
NM	0.036
NJ	0.154
MD	0.021
DE	0.205
CO	0.253
CA	0.189

This table reports the weights assigned to different donor units to create the synthetic counterfactual. For analyses using states as the donor units, the weights are reported directly from the synthetic control. For analyses using individual counties as the donor units, the weights are given by state that the county belongs to. If multiple counties belong to the same state, their weights are summed. All weights sum to 1, but the reported weights are rounded to three decimal places.

Table A.5: MI Pooled Union Cov. Rate Balance Table

	Treated	Synthetic
Manufacturing Prop. (2012)	18.85219%	08.5201%
Transport. and Util. Prop. (2012)	04.40536%	05.65248%
Health Prop. (2012)	13.47632%	10.33264%
Educ Prop. (2012)	09.77125%	08.86022%
Telecomm Prop.(2012)	01.16916%	01.43017%
Construction Prop. (2012)	04.18247%	05.46461%
Public Admin. Prop. (2012)	03.14612%	05.52926%
% Public Sector (2012)	11.7181%	14.23942%
% White (2012)	83.34221%	74.2998%
% Metro (2012)	86.36155%	98.8%
% Female (2012)	48.37928%	47.62031%
Mean Wage (2012)	\$15.27737	\$17.23941
Wage Premium (2012)	.1113984	.2120005
Union Wage (2012)	\$16.97137	\$19.44893
Non-Union Wage (2012)	\$14.87333	\$16.63966
Avg. Educ. (2012)	13.98439	14.04654

The Treated column displays the levels of each respective variable for the enactment state in the year immediately prior to RTW enactment. The Synthetic column displays the levels of each respective variable for the constructed synthetic counterfactual in the same year. Total Employment and the Unemployment Rate are not included for synthetic counterfactuals constructed using counties as the donor units.

Table A.6: MI Pooled Union Cov. Rate Synthetic Control Composition

State	Weight
CA	0.186
CO	0.022
DE	0.012
DC	0.006
HI	0.011
ME	0.008
MD	0.009
MN	0.062
MO	0.095
NJ	0.386
NM	0.005
NY	0.12
OH	0.007
OR	0.011
PA	0.06

This table reports the weights assigned to different donor units to create the synthetic counterfactual. For analyses using states as the donor units, the weights are reported directly from the synthetic control. For analyses using individual counties as the donor units, the weights are given by state that the county belongs to. If multiple counties belong to the same state, their weights are summed. All weights sum to 1, but the reported weights are rounded to three decimal places.

Table A.7: WV Pooled Union Cov. Rate Balance Table

	Treated	Synthetic
Manufacturing Prop. (2015)	11.94242%	10.93708%
Transport. and Util. Prop. (2015)	05.32906%	03.7666%
Health Prop. (2015)	14.37788%	12.01648%
Educ Prop. (2015)	10.44408%	11.46833%
Telecomm Prop.(2015)	0.87958%	02.33436%
Construction Prop. (2015)	05.82162%	05.45703%
Public Admin. Prop. (2015)	06.46588%	03.85211%
% Public Sector (2015)	18.63852%	14.16373%
% White (2015)	93.8306%	82.63279%
% Metro (2015)	61.22732%	99.9%
% Female (2015)	47.81954%	48.06712%
Mean Wage (2015)	$13.81152	$18.32453
Wage Premium (2015)	.1323565	.0157218
Union Wage (2015)	$15.50562	$20.80842
Non-Union Wage (2015)	$13.46204	$17.91628
Avg. Educ. (2015)	12.59534	12.77921

The Treated column displays the levels of each respective variable for the enactment state in the year immediately prior to RTW enactment. The Synthetic column displays the levels of each respective variable for the constructed synthetic counterfactual in the same year. Total Employment and the Unemployment Rate are not included for synthetic counterfactuals constructed using counties as the donor units.

Table A.8: WV Pooled Union Cov. Rate Synthetic Control Composition

State	Weight
MO	0.038
NJ	0.258
NY	0.198
PA	0.185
CA	0.085
CO	0.235

This table reports the weights assigned to different donor units to create the synthetic counterfactual. For analyses using states as the donor units, the weights are reported directly from the synthetic control. For analyses using individual counties as the donor units, the weights are given by state that the county belongs to. If multiple counties belong to the same state, their weights are summed. All weights sum to 1, but the reported weights are rounded to three decimal places.

Table A.9: MI Pooled Mean Wage Balance Table

	Treated	Synthetic
Union Cov. Rate (2012)	15.77456%	15.25059%
Manufacturing Prop. (2012)	18.85219%	11.17003%
Transport. and Util. Prop. (2012)	4.40536%	4.7066%
Health Prop. (2012)	13.47632%	14.26535%
Educ Prop. (2012)	9.77125%	8.38096%
Telecomm Prop.(2012)	1.16916%	1.05554%
Construction Prop. (2012)	4.18247%	5.15395%
Public Admin. Prop. (2012)	3.14612%	5.23358%
% Public Sector (2012)	11.7181%	13.89069%
% White (2012)	83.34221%	84.124%
% Metro (2012)	86.36155%	91.1%
% Female (2012)	48.37928%	51.66866%
Wage Premium (2012)	.1113984	.2219837
Union Wage (2012)	$16.97137	$18.09077
Non-Union Wage (2012)	$14.87333	$14.4653
Avg. Educ. (2012)	13.98439	13.55528

The Treated column displays the levels of each respective variable for the enactment state in the year immediately prior to RTW enactment. The Synthetic column displays the levels of each respective variable for the constructed synthetic counterfactual in the same year. Total Employment and the Unemployment Rate are not included for synthetic counterfactuals constructed using counties as the donor units.

Table A.10: MI Pooled Mean Wage Synthetic Control Composition

State	Weight
ME	0.09
MN	0.007
NJ	0.009
NY	0.03
OH	0.134
OR	0.08
PA	0.428
CA	0.191
CO	0.032

This table reports the weights assigned to different donor units to create the synthetic counterfactual. For analyses using states as the donor units, the weights are reported directly from the synthetic control. For analyses using individual counties as the donor units, the weights are given by state that the county belongs to. If multiple counties belong to the same state, their weights are summed. All weights sum to 1, but the reported weights are rounded to three decimal places.

Table A.11: MI Pooled Union Wage Balance Table

	Treated	Synthetic
Union Cov. Rate (2012)	15.77456%	12.8305%
Manufacturing Prop. (2012)	18.85219%	11.04658%
Transport. and Util. Prop. (2012)	4.40536%	4.7436%
Health Prop. (2012)	13.47632%	12.2361%
Educ Prop. (2012)	9.77125%	8.76374%
Telecomm Prop.(2012)	1.16916%	1.20785%
Construction Prop. (2012)	4.18247%	4.46312%
Public Admin. Prop. (2012)	3.14612%	4.8376%
% Public Sector (2012)	11.7181%	13.67144%
% White (2012)	83.34221%	81.68268%
% Metro (2012)	86.36155%	97%
% Female (2012)	48.37928%	48.20694%
Mean Wage (2012)	$15.27737	$16.10176
Wage Premium (2012)	.1113984	.0985254
Non-Union Wage (2012)	$14.87333	$15.80782
Avg. Educ. (2012)	13.98439	13.96882

The Treated column displays the levels of each respective variable for the enactment state in the year immediately prior to RTW enactment. The Synthetic column displays the levels of each respective variable for the constructed synthetic counterfactual in the same year. Total Employment and the Unemployment Rate are not included for synthetic counterfactuals constructed using counties as the donor units.

Table A.12: MI Pooled Union Wage Synthetic Control Composition

State	Weight
DE	0.044
DC	0.002
HI	0.005
ME	0.011
MD	0.002
MN	0.037
MO	0.015
NJ	0.14
NM	0.006
NY	0.126
OH	0.034
OR	0.075
PA	0.236
CA	0.196
CO	0.069

This table reports the weights assigned to different donor units to create the synthetic counterfactual. For analyses using states as the donor units, the weights are reported directly from the synthetic control. For analyses using individual counties as the donor units, the weights are given by state that the county belongs to. If multiple counties belong to the same state, their weights are summed. All weights sum to 1, but the reported weights are rounded to three decimal places.

Table A.13: MI Pooled Non-Union Wage Balance Table

	Treated	Synthetic
Union Cov. Rate (2012)	15.77456%	15.69087%
Manufacturing Prop. (2012)	18.85219%	11.26432%
Transport. and Util. Prop. (2012)	04.40536%	05.26593%
Health Prop. (2012)	13.47632%	13.29684%
Educ Prop. (2012)	9.77125%	7.85422%
Telecomm Prop.(2012)	1.16916%	1.05462%
Construction Prop. (2012)	4.18247%	5.5142%
Public Admin. Prop. (2012)	3.14612%	6.61347%
% Public Sector (2012)	11.7181%	15.59465%
% White (2012)	83.34221%	80.07124%
% Metro (2012)	86.36155%	99.4%
% Female (2012)	48.37928%	48.59408%
Mean Wage (2012)	$15.27737	$15.76999
Wage Premium (2012)	.1113984	.2100401
Union Wage (2012)	$16.97137	$18.60815
Avg. Educ. (2012)	13.98439	13.64043

The Treated column displays the levels of each respective variable for the enactment state in the year immediately prior to RTW enactment. The Synthetic column displays the levels of each respective variable for the constructed synthetic counterfactual in the same year. Total Employment and the Unemployment Rate are not included for synthetic counterfactuals constructed using counties as the donor units.

Table A.14: MI Pooled Non-Union Wage Synthetic Control Composition

State	Weight
DE	0.074
HI	0.001
ME	0.001
MN	0.006
MO	0.001
NJ	0.017
NY	0.18
OH	0.165
OR	0.001
PA	0.202
CA	0.344
CO	0.004

This table reports the weights assigned to different donor units to create the synthetic counterfactual. For analyses using states as the donor units, the weights are reported directly from the synthetic control. For analyses using individual counties as the donor units, the weights are given by state that the county belongs to. If multiple counties belong to the same state, their weights are summed. All weights sum to 1, but the reported weights are rounded to three decimal places.

Appendix B Robustness Checks

B.1 Covariate Matching Details

For the covariate matching specification, the synthetic control is matched on the outcome variable in 2000, the outcome variable in the year immediately prior to RTW enactment, and the outcome variable averaged over the pre-treatment period. Additionally, the extra predictor variable or variables that we want to address are included in the same timing scheme. For example, the main analysis of Michigan's union coverage rate created a synthetic control where its manufacturing proportion is about seven percentage points less than Michigan. So, for this robustness check, we will match on union rate (2000), union rate (2012), union rate (averaged 2000-2012), manufacturing (2000), manufacturing (2012), and manufacturing (averaged 2000-2012). Since adding extra predictor variables can increase the $RMSPE_{pre}$, evaluating statistical significance with these models is less important than ensuring that there is no evidence that seems contrary to the main results.

B.2 Robustness Checks Figures

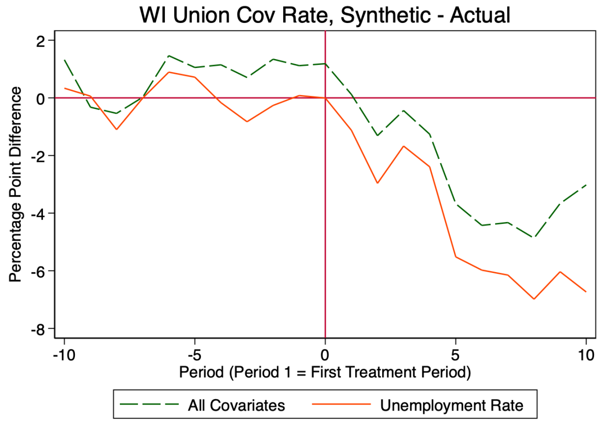


Figure B.1: WI Union Rate Covariates

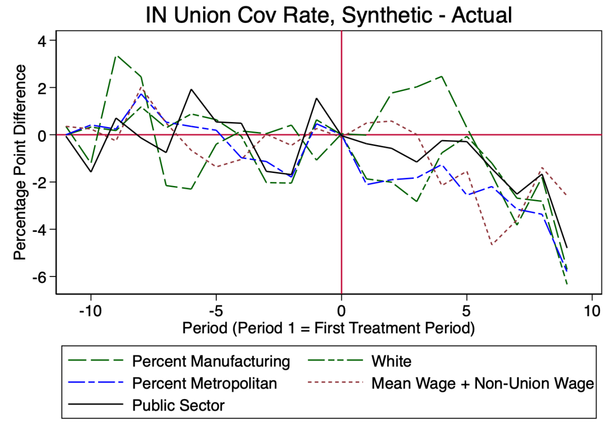


Figure B.2: IN Union Rate Covariates

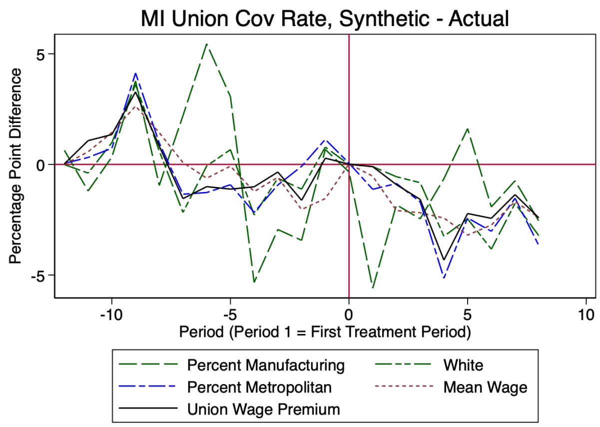


Figure B.3: MI Union Rate Covariates

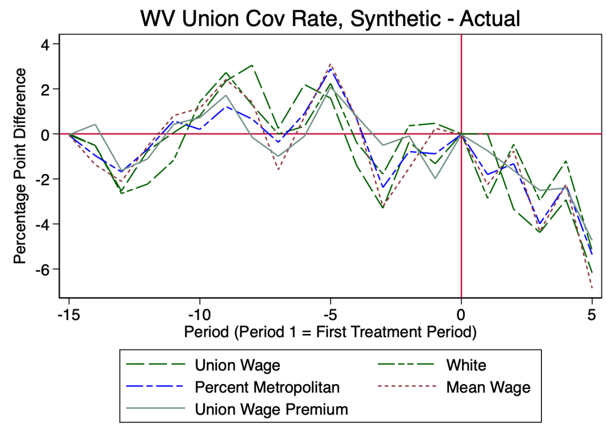


Figure B.4: WV Union Rate Covariates

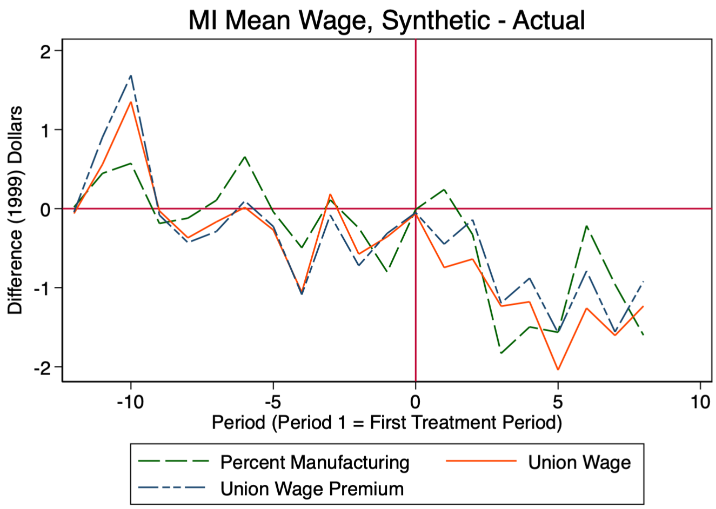


Figure B.5: MI Mean Wage Covariates

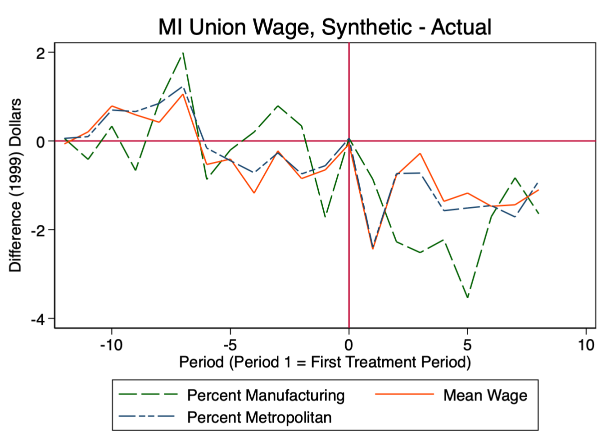


Figure B.6: MI Union Wage Covariates

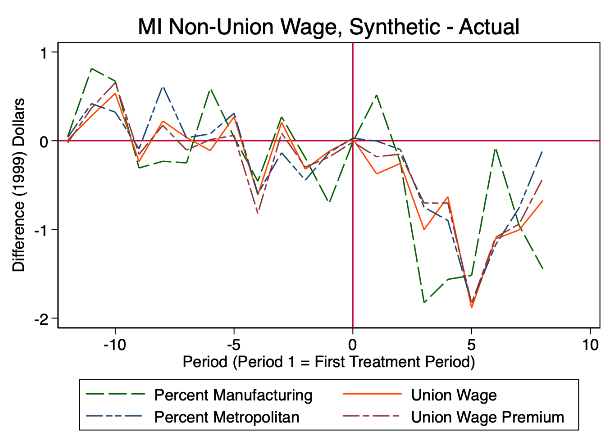


Figure B.7: MI Non-Union Wage Covariates

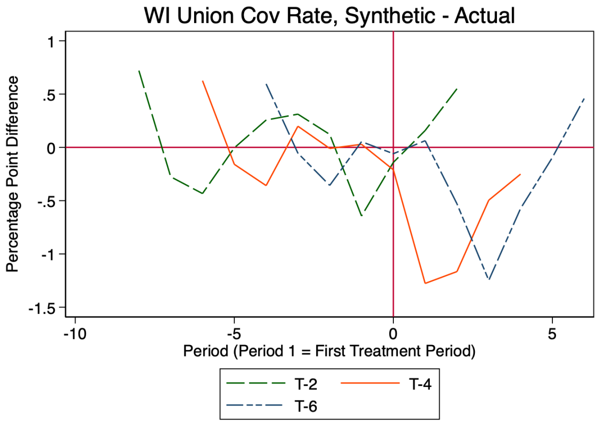


Figure B.8: WI Union Rate In-Time Placebos

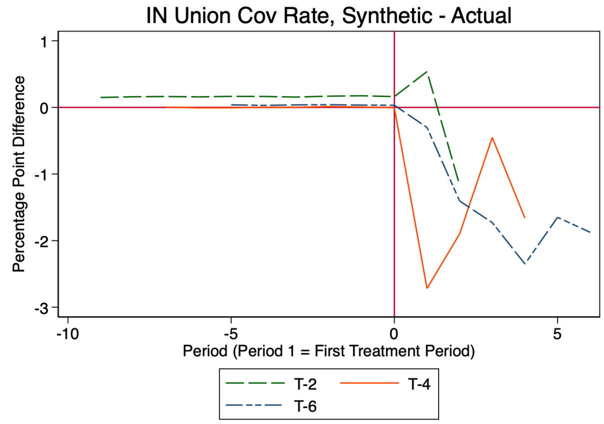


Figure B.9: IN Union Rate In-Time Placebos

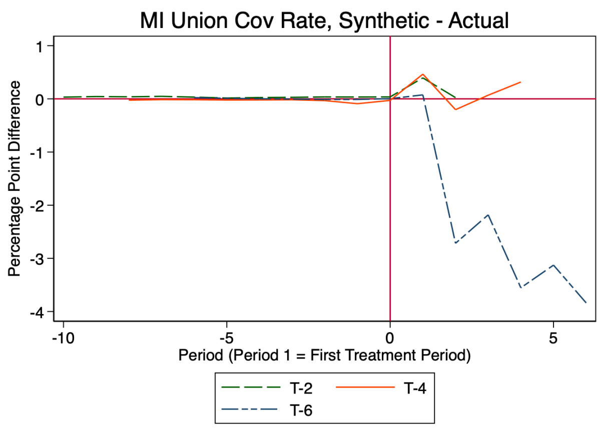


Figure B.10: MI Union Rate In-Time Placebos

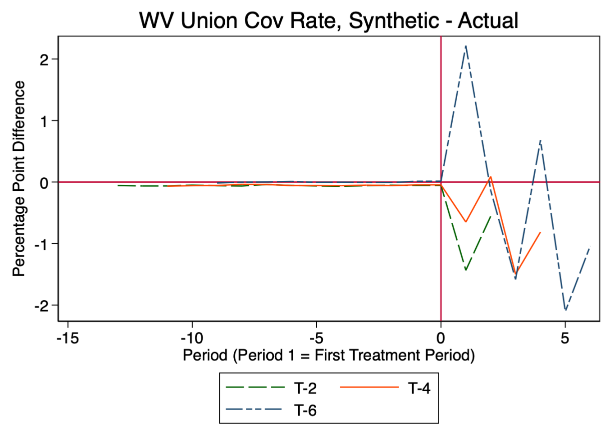


Figure B.11: WV Union Rate In-Time Placebos

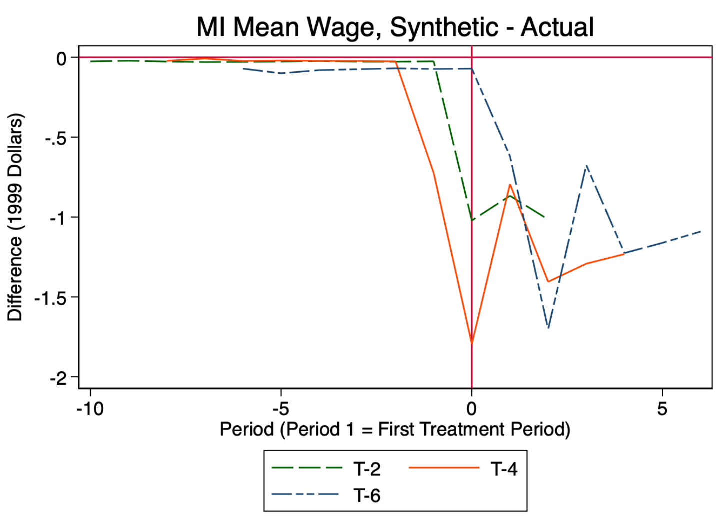


Figure B.12: MI Mean Wage In-Time Placebos

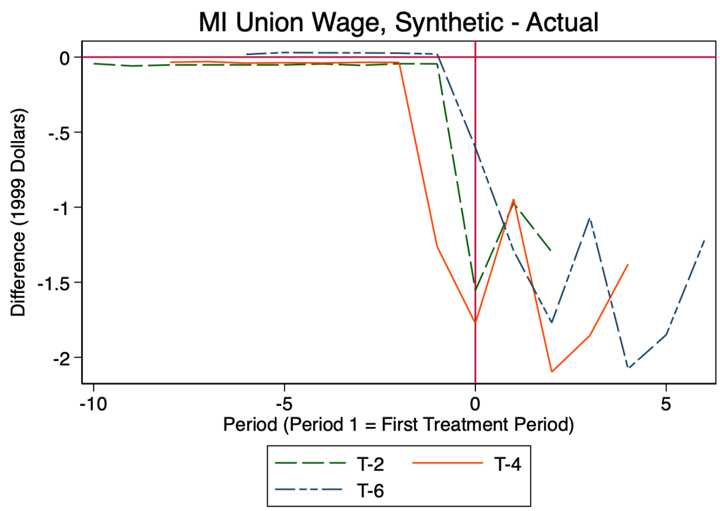


Figure B.13: MI Union Wage In-Time Placebos

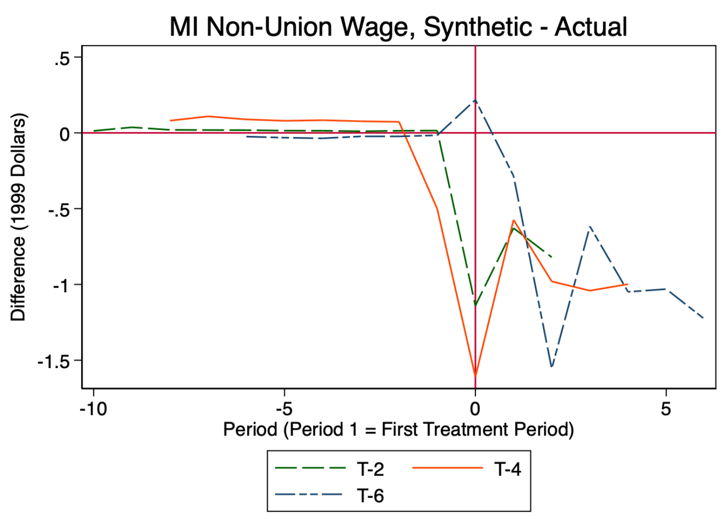


Figure B.14: MI Non-Union Wage In-Time Placebos

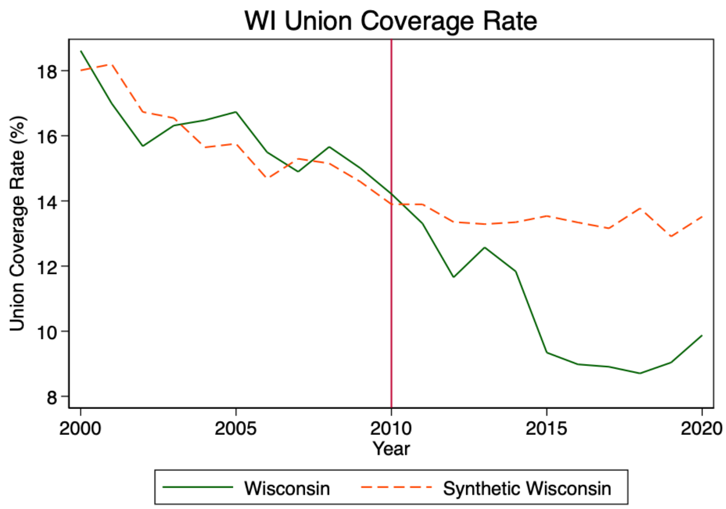


Figure B.15: WI Union Rate: OH, PA, ME Donor Pool

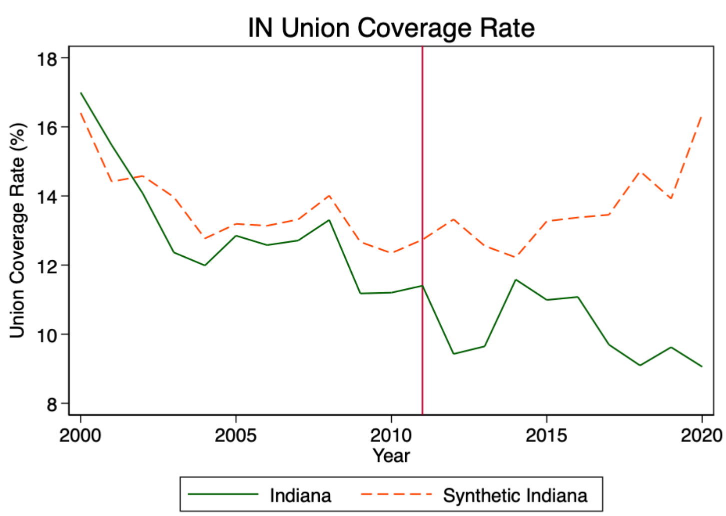


Figure B.16: IN Union Rate: OH, PA, ME Donor Pool

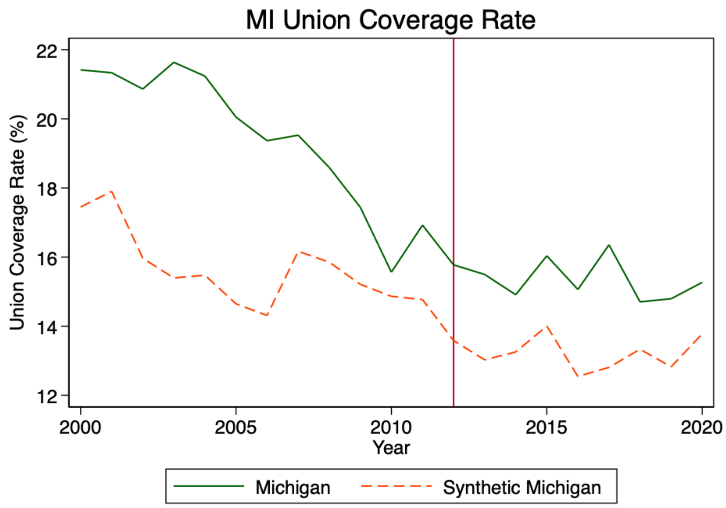


Figure B.17: MI Union Rate: OH, PA, ME Donor Pool

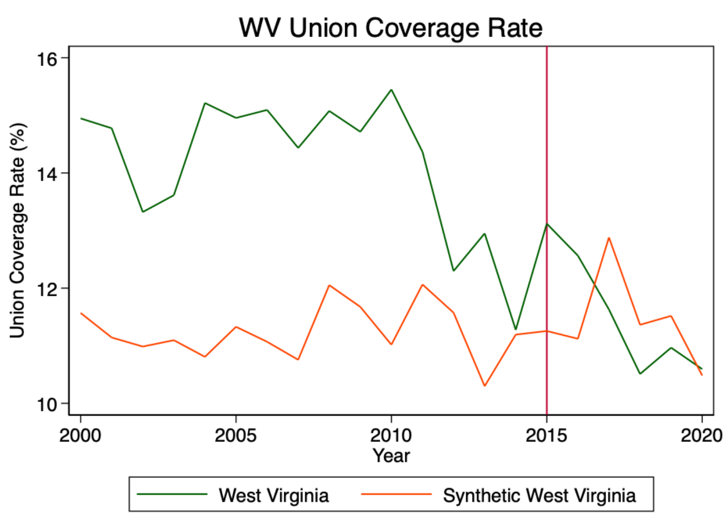


Figure B.18: WV Union Rate: MO and NH Donor Pool

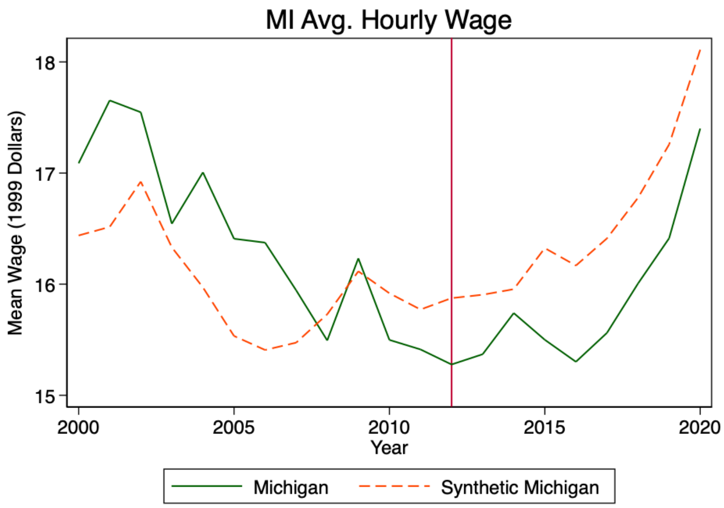


Figure B.19: MI Mean Wage: OH, PA, ME Donor Pool

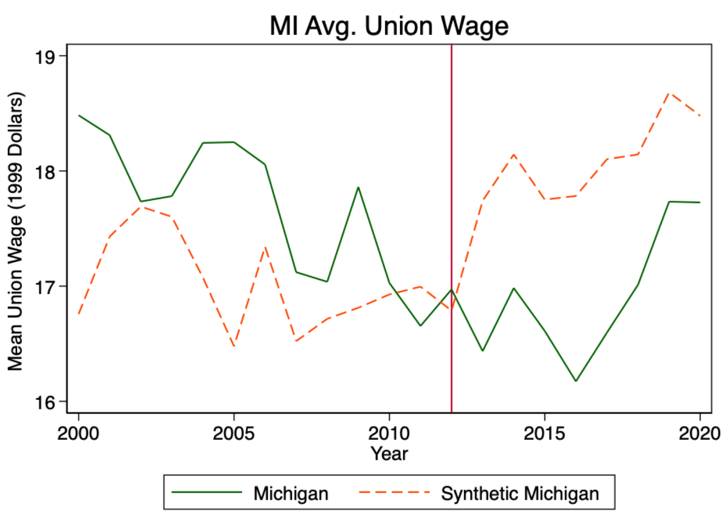


Figure B.20: MI Union Wage: OH, PA, ME Donor Pool

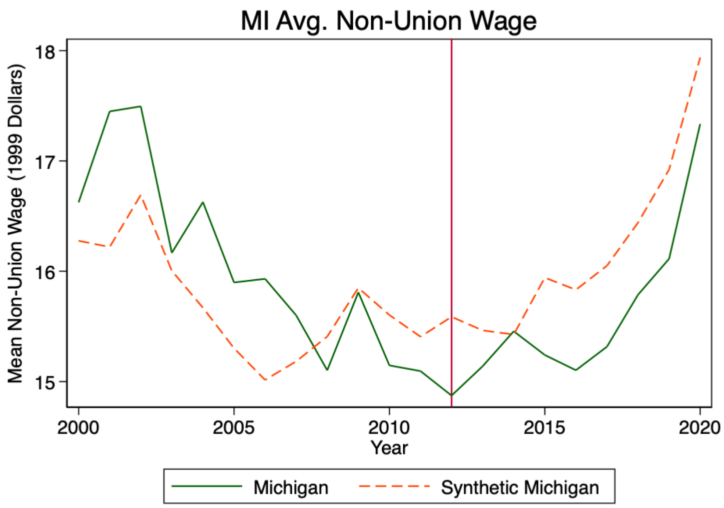


Figure B.21: MI Non-Union Wage: OH, PA, ME Donor Pool

Appendix C Public/Non-Public “F-Tests”

Table C.1: Synthetic Control “F-Tests”

	Outcome Variable	
State	Public Union Cov. Rate	Non-Public Union Cov. Rate
WI	0	0.0417
IN	0	0.0149
MI	0	0.0299
WV	0.5	0.0299
KY	0.4375	0.6119

“F-Tests” are the ranked ratios of Post-treatment RMSPE to Pre-treatment RMSPE. The numbers are rounded to four decimal places.

Table C.2: Synthetic Control “F-Tests”

	Outcome Variable	
State	Public Mean Wage	Non-Public Mean Wage
WI	0.2083	0.6250
IN	0.1667	0.0896
MI	0.1875	0.0746
WV	0.2917	0.3134
KY	0.9583	0.2687

“F-Tests” are the ranked ratios of Post-treatment RMSPE to Pre-treatment RMSPE. The numbers are rounded to four decimal places.

Table C.3: Synthetic Control “F-Tests”

	Outcome Variable	
State	Public Union Wage	Non-Public Union Wage
WI	0.1250	0
IN	0.2500	0.0896
MI	0.0625	0.0149
WV	0.2083	0.1940
KY	0.1875	0.2537

“F-Tests” are the ranked ratios of Post-treatment RMSPE to Pre-treatment RMSPE. The numbers are rounded to four decimal places.

Table C.4: Synthetic Control “F-Tests”

	Outcome Variable	
State	Public Non-Union Wage	Non-Public Non-Union Wage
WI	0.1667	0.5417
IN	0.1667	0.0746
MI	0.1250	0.3731
WV	0.8333	0.3582
KY	1.0000	0.1493

“F-Tests” are the ranked ratios of Post-treatment RMSPE to Pre-treatment RMSPE. The numbers are rounded to four decimal places.

Appendix D Point-Estimate Tables

Table D.1: Union Cov. Rate Estimated Treatment Effects

	Estimated Effect by State (Percentage Points)			
Period	WI	IN	MI	WV
1	-0.30	-0.49	0.88	-1.63
2	-2.29	0.39	-0.41	-2.79
3	-1.10	-0.15	-0.04	-2.28
4	-1.89	-0.27	-3.01	-2.43
5	-3.83	-0.62	-0.14	-3.72
6	-5.70	-1.01	-0.83	.
7	-5.27	-0.85	-0.71	.
8	-5.46	-2.23	-1.10	.
9	-5.18	-1.49	.	.
10	-3.98	.	.	.

Period 1 corresponds to the year of enactment. Estimates are rounded to two decimal places.

Table D.2: Public Union Cov. Rate Estimated Treatment Effects

	Estimated Effect by State (Percentage Points)		
Period	WI	IN	MI
1	2.36	-4.55	-0.22
2	-10.21	-7.16	-2.73
3	-15.96	-2.08	-1.60
4	-21.69	4.02	-2.70
5	-17.02	-0.64	-4.95
6	-25.59	-1.80	-10.33
7	-30.90	-6.20	-6.31
8	-26.62	-4.77	-7.37
9	-22.82	1.04	.
10	-24.63	.	.

Period 1 corresponds to the year of enactment. Estimates are rounded to two decimal places.

Table D.3: Non-Public Sector Union Cov. Rate Estimated Treatment Effects

	Estimated Effect by State (Percentage Points)			
Period	WI	IN	MI	WV
1	-1.92	-1.18	1.64	-0.77
2	-2.47	0.03	0.52	-3.08
3	-1.40	0.22	0.70327	-1.29
4	-2.15	-0.67	-1.71	0.37
5	-2.06	-0.85	-0.49	-3.77
6	-0.22	-2.30	0.07	.
7	.	-2.68	0.09	.
8	.	-1.53	-1.01	.
9	.	-2.16	.	.

Period 1 corresponds to the year of enactment. Estimates are rounded to two decimal places. 2015 is the first treatment period for Wisconsin because that is the year in which Wisconsin passed RTW that cover's the non-public sector.

Table D.4: Mean Wage Estimated Treatment Effects

Estimated Effect by State (1999 Dollars)	
Period	MI
1	-0.47
2	-1.00
3	-2.23
4	-1.45
5	-1.99
6	-2.53
7	-1.51
8	-1.31

Period 1 corresponds to the year of enactment. Estimates are average hourly wages in 1999 dollars, rounded to two decimal places.

Table D.5: Union Wage Estimated Treatment Effects

Estimated Effect by State (1999 Dollars)	
Period	MI
1	-1.65
2	-1.81
3	-2.11
4	-0.80
5	-0.28
6	-1.72
7	-0.33
8	-1.66

Period 1 corresponds to the year of enactment. Estimates are average hourly wages in 1999 dollars, rounded to two decimal places.

Table D.6: Non-Public Sector Union Wage Estimated Treatment Effects

Estimated Effect by State (1999 Dollars)		
Period	WI	MI
1	1.04	-3.95
2	0.87	-1.49
3	-0.04	-1.62
4	0.81	-2.12
5	0.85	-1.38
6	-1.39	-0.43
7	.	-2.05
8	.	-0.55

Period 1 corresponds to the year of enactment. Estimates are average hourly wages in 1999 dollars, rounded to two decimal places.

Table D.7: Non-Union Wage Estimated Treatment Effects

Estimated Effect by State (1999 Dollars)	
Period	MI
1	-0.47
2	-0.82
3	-1.31
4	-0.80
5	-0.79
6	-1.20
7	0.03
8	-0.20

Period 1 corresponds to the year of enactment. Estimates are average hourly wages in 1999 dollars, rounded to two decimal places.

Table D.8: Union Wage Premium Estimated Treatment Effects

Estimated Effect by State (ln points)		
Period	WI	IN
1	0.0906	0.0217
2	0.0349	0.0606
3	-0.0341	0.0679
4	-0.0140	0.1341
5	-0.0311	0.0708
6	0.1059	0.1056
7	0.0078	0.0655
8	0.0262	0.0213
9	0.0400	0.0464
10	-0.0230	.

Period 1 corresponds to the year of enactment. Estimates are rounded to four decimal places.

Appendix E Missouri Results

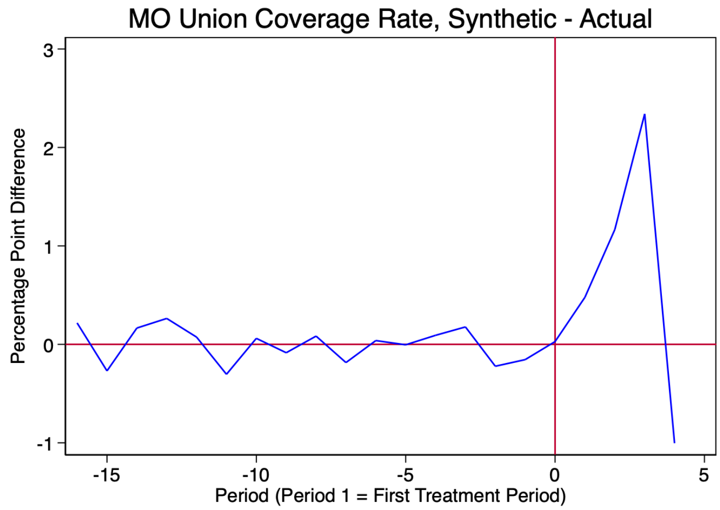


Figure E.1: Synthetic Control Results for MO Union Coverage Rate

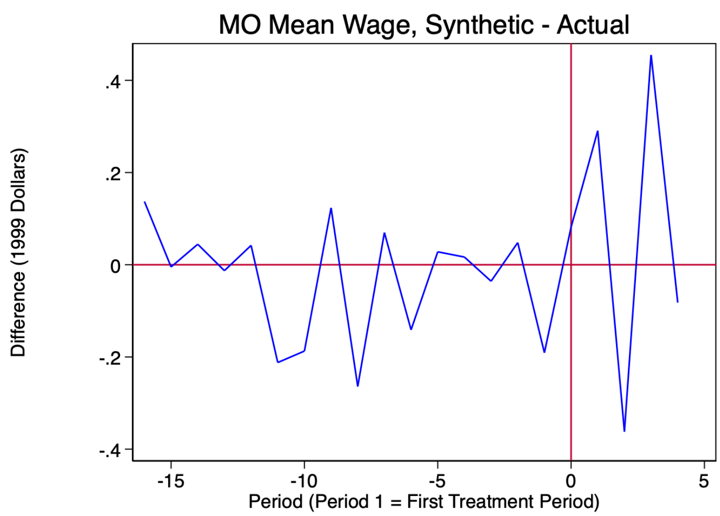


Figure E.2: Synthetic Control Results for MO Mean Wage

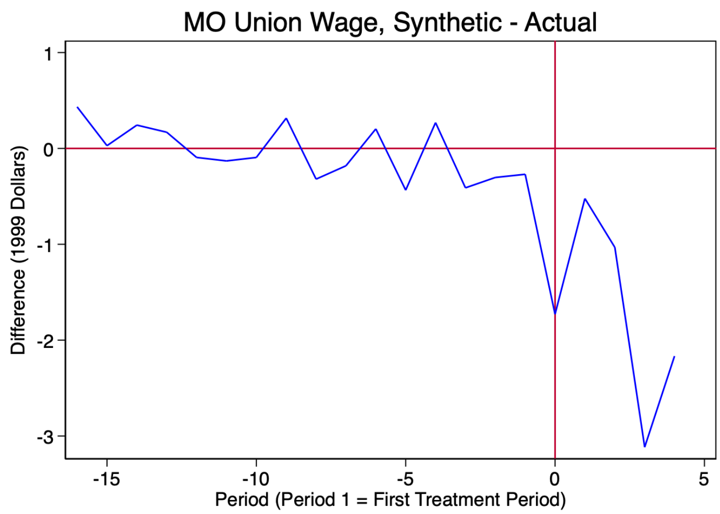


Figure E.3: Synthetic Control Results for MO Union Wage

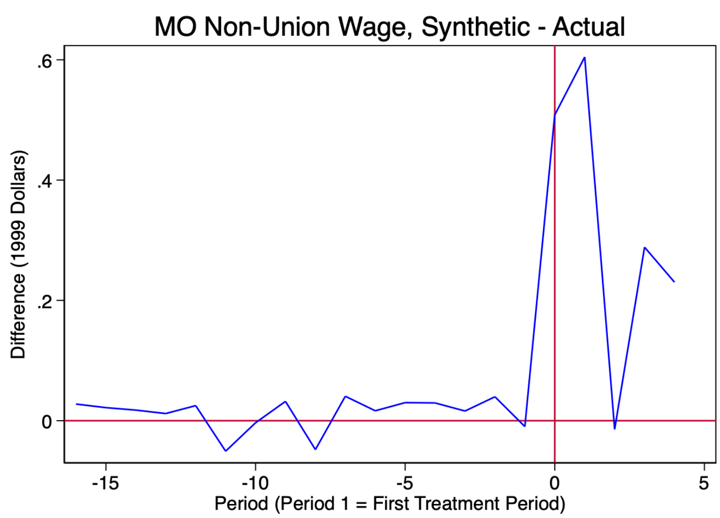


Figure E.4: Synthetic Control Results for MO Non-Union Wage

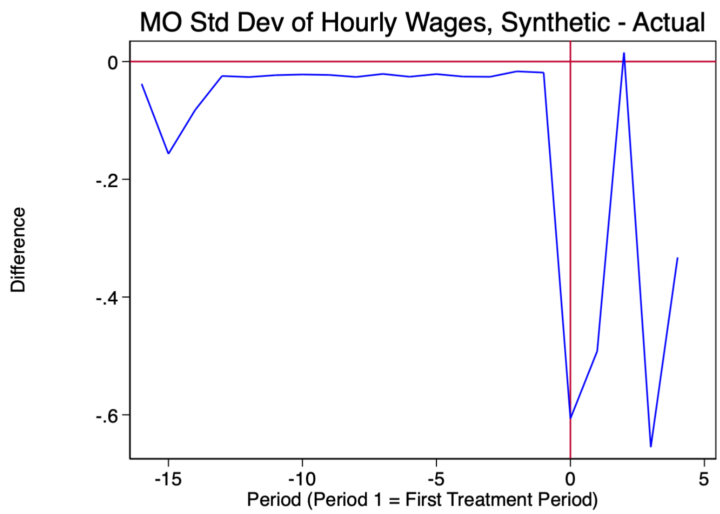


Figure E.5: Synthetic Control Results for MO Wage SD

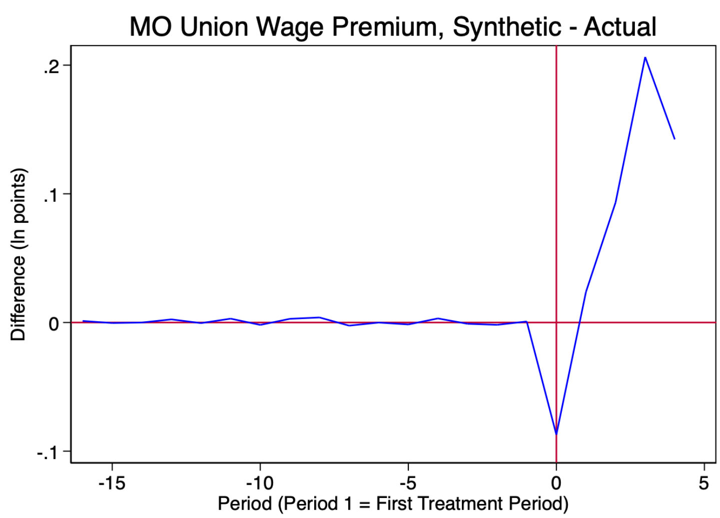


Figure E.6: Synthetic Control Results for MO Union Wage Premium

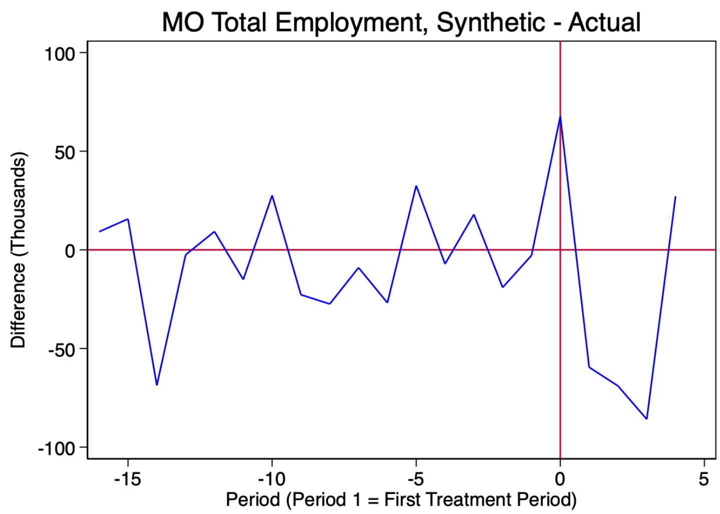


Figure E.7: Synthetic Control Results for MO Total Employment

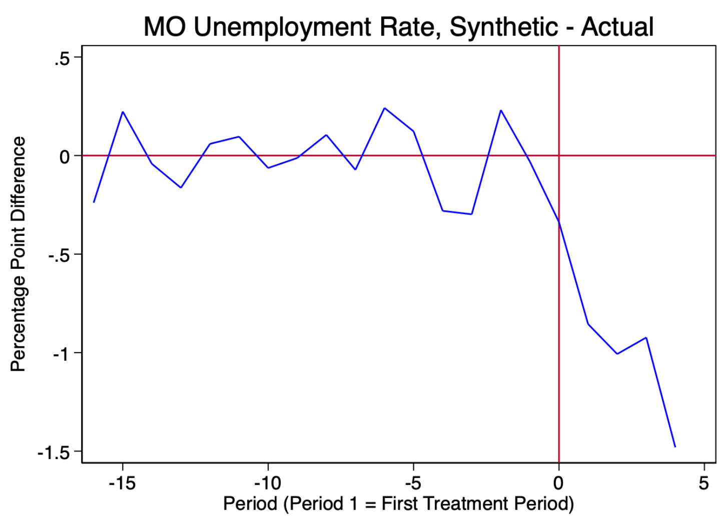


Figure E.8: Synthetic Control Results for MO Unemployment Rate

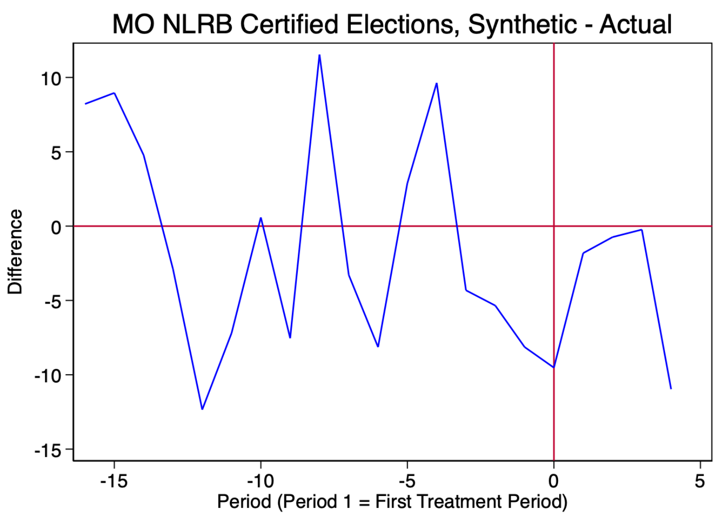


Figure E.9: Synthetic Control Results for MO Union Elections

Table E.1: Synthetic Control “F-Tests”

	Outcome Variable			
State	Union Cov. Rate	Mean Wage	Union Wage	Non-Union Wage
MO	0.0299	0.2836	0.1493	0.2537

“F-Tests” are the ranked ratios of Post-treatment RMSPE to Pre-treatment RMSPE. The numbers are rounded to four decimal places.

Table E.2: Synthetic Control “F-Tests”

	Outcome Variable				
State	Wage SD	Wage Prem.	Tot. Employment	Unemp. Rate	Elections
MO	0.1641	0.5522	0.4783	0.0870	0.6957

“F-Tests” are the ranked ratios of Post-treatment RMSPE to Pre-treatment RMSPE. The numbers are rounded to four decimal places.