The Modern Impact of Right-to-work Laws on Labor Market Outcomes: Evidence from the Midwest

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Abstract

This paper aims to study the impact on labor market outcomes of Right-to-Work laws that have passed since 2010 in Indiana, Michigan, and Wisconsin. Using synthetic control and difference-in-difference empirical strategies, the paper finds no significant aggregate effects, but that there are significant causal effects in the individual states from the implementation of the Right-to-Work policy.

1 Introduction

From the late 19th and 20th centuries onward, workers in the trades industries such as autoworkers, construction workers, metalworkers, and others were represented by labor unions and covered by union-negotiated collective bargaining agreements. In the 20th century, the labor union movement was thriving, as numerous laws such as the Wagner Act of 1935, the Norris-LaGuardia Act of 1932, the New Deal, the National Industrial Recovery Act of 1932-33 (Hannan and Freeman, 1988) strengthened the power of unions, by giving greater legal protection to union activities such as striking, organizing, and bargaining, while mandating union coverage and wages in federal contracts. Not only were private sector workers organizing, but increasingly, public sector workers were unionizing as well.

However, the story of this paper begins in 1947 with the passage of the Taft-Hartley Act. This legislation, among other things, modified the Wagner Act in a number of ways (National Labor Relations Board n.d). First, it banned employees from having to join a union as a condition of employment. Second, it made a number of changes meant to reduce union coercion and to ensure good-faith bargaining practices by the union. Third, and most interestingly, it contained a provision that allows for individual states to ban the so-called union shop and the agency shop. The union shop refers to the workplace where employees must join the relevant union within a certain period of their hire date, while the agency shop refers to the workplace where employees don't have to join the relevant union, but rather have to make monetary contributions to compensate the union for their bargaining and organizing activities.

This provision is the basis for Right-to-work laws. Right-to-work laws refer to pieces of legislation that "ban union-security agreements" (National Labor Relations Board n.d.) by giving employees at a workplace protected by a union-negotiated collective bargaining agreement the right to decide whether to join that union and pay dues to that union. This means that union membership or monetary contribution to the union is not a condition of employment at these workplaces. In other words, the law bans the agency shop and the union shop.

More recently, in 2012, the states of Indiana (Agarwal and Keatner, 2012) and Michigan (Egan and Gray, 2012) passed right-to-work legislation. In 2015, the state of Wisconsin (Davey, 2015) also passed right-to-work legislation. These three Rust Belt states have historically had strong union representation, but these laws were passed by Republican-controlled governments primarily as a political and economic measure to potentially increase the competitiveness of their states in attracting jobs.

Furthermore, in June 2018, the Supreme Court extended Right-to-work laws to cover all public sector employees in Janus vs. AFSCME (Supreme Court, 2018). The court ruled that collecting monetary contributions from employees who were not members of the union in workplaces covered by a union-negotiated collective bargaining agreement was illegal. As a result of this ruling, the agency shop is now outlawed in public sector workplaces, and public sector workplaces are covered by a nationwide Right-to-work labor law framework.

Given the increasing anxiety and insecurity felt by workers in traditionally unionized industries over automation, outsourcing, and job losses, and given that these recent policy changes have had time to take effect, I ask the following question: What did the passage of right to work laws do to incomes, poverty levels, and government public assistance program usage individually in Wisconsin, Michigan, and Indiana, and in aggregate across the Midwest?
I hypothesize that Right-to-work laws have reduced incomes and increased poverty levels and government public assistance program usage in aggregate across the Midwest. This hypothesis follows from the idea that these laws weaken union bargaining power, allowing employers to gain concessions in employee contracts that they otherwise would not have been able to. Using synthetic control and difference-in-differences estimators, I evaluate the impact of this law on

the individual states in the Midwest where it was implemented, and assess the impact of these laws passing in aggregate across the entire Midwest.

The rest of the paper is organized as follows. Section 2 provides the literature review. Section 3 discusses the data. Sections 4 to 7 discuss the methodology of the 4 estimation models that I employ. Section 8 provides a discussion of the results. Section 9 provides a discussion of robustness checks. Sections 10-13 provides result tables for each of the estimation strategies. Section 14 provides concluding remarks.

2 Literature Review

In other papers that have assessed the impact of Right-to-work laws, they have focused primarily on understanding the impacts of these laws on unions themselves. Ellwood and Fine (1987) showed that there is a reduction in union organizing activities after passage of right to work laws. Farber (1984) showed that interest for union representation is lower in right-to-work states, that the union relative wage premium is larger in right-to-work states, and that there is no difference in the supply of union jobs relative to demand. While these papers are not directly relevant, these papers serve as foundational assumptions.

Hogler, Shulman, and Weiler (2004) found that right-to-work laws have a significant, negative impact on union density independent of underlying attitudes towards unions. They specifically controlled for employer opposition to unions, political affiliation, and social capital in generating these estimates that confirm previous findings in the literature about right-to-work laws in a more robust manner. This is relevant to my paper, as negative impacts on union density from right-to-work laws could theoretically reduce wages and by extension affect poverty and welfare program usage, since weakened unions with less members would have less bargaining power in future collective bargaining agreements, meaning that their ability to ask for higher wages relative to non-union workers could decrease, reducing their economic wellbeing.

Budd and Na (2000) used CPS data from 1983-1993 on 19,102 private sector, full-time employees, who are covered by collective bargaining agreements and reside in right-to-work states, to answer the question of whether a union relative wage premium existed. They found that such a premium did exist and that unobservable characteristics, measurement error, and differences in job tenure did not explain the OLS estimated premium of 12% that they found. This finding is relevant to my paper as a foundational assumption, as this paper will newly assume that a union relative wage premium should exist in right-to-work states post-intervention, and any estimates that are derived will account for higher union wages relative to non-union wages.

This paper differs in that it focuses on assessing the economic and labor market impacts of Right-to-work laws.

To this end, some research has been done. Holmes (1998) found that there is an increase in manufacturing shares of total employment by one-third in a county when crossing borders from states without right to work laws (deemed as anti-business) to states with right to work laws (deemed as pro-business). This paper suggests that there is an increase in manufacturing activity when crossing borders between right-to-work and non-right-to-work states, indicating that there may be positive employment effects associated with right to work laws in the skilled and unskilled trades, which could affect wages, poverty, and welfare program usage, all outcomes that this paper is interested in investigating in the Midwest.

The most relevant paper that has discussed elements of this paper's specific question is Eren and Ozbeklik (2016). They used Oklahoma's passage of a right to work law in 2001 as a comparative case study to understand effects on labor market outcomes related to private sector and manufacturing sector unionization rates, average wages in manufacturing and private sectors, and employment-population ratio. Using a synthetic control method, they generated counterfactual estimates to compare labor market outcomes between Oklahoma and "synthetic" Oklahoma and found that passage of a right to work law in Oklahoma decreased unionization rates in the private sector and in manufacturing industries, while having no effect on employment-population ratio and average wages in the private sector and manufacturing sectors. My paper will build upon this paper in two ways. First, by estimating other labor market outcomes not covered in Eren and Ozbeklik (2016) such as incomes, poverty levels, and government public assistance program dependence rates. Second, my paper will aim to show that the trends that Eren and Ozbeklik (2016) found do not hold in states with higher private sector unionization rates. This was a specific concern and area for future research that Eren and Ozbeklik stated, as in the conclusion of their paper they indicated that their conclusions using Oklahoma as a comparative case study, which they described as a "relatively small state" with "low private sector unionization", may not hold in bigger, more unionized states such as Michigan, Indiana, and Wisconsin.

Finally, Curran (2019) used difference-in-differences estimation, with Wisconsin as the treatment and Minnesota as the control, to show that there were small declines in wages and income in Wisconsin, and an increase in employment in Wisconsin after Right-to-work was passed. This is a subset of the question that I am asking, however, my paper will correct a few short-comings of their empirical strategy. First, they use fixed effects, holding un-observable effects constant over time, which my synthetic control empirical strategy will not. Second, they only control for race, omitting a key control for gender which my model will include, which should be relevant in explaining differences in labor market outcomes. Third, the paper does not justify the parallel trends assumption for difference-in-differences estimates to be valid. As justification, the author provides 2010 Census data for Minnesota and Wisconsin relating to racial demographics, educational levels, and per-capita income. No time series trends between treatment and control are investigated or discussed, and differences in outcomes or characteristics over time between Minnesota and Wisconsin are never discussed or shown to hold constant over time visually. The synthetic control method described in Abadie et al. (2010) that I will employ in the paper will rigorously justify construction of control groups and solve the biased Wisconsin estimates in Curran (2019).

3 Data

The data that this paper will use is state-level panel data from 2001 to 2018. The District of Columbia and other U.S. territories were excluded from the data set due to missing and unreported data. The units of observation are states, with 1 observation for each of the 50 states for each of the 18 years. The number of observations is 900.

The data that comprises this dataset was aggregated together from numerous series from the Current Population Survey, United States Department of Agriculture, Bureau of Economic Analysis, Bureau of Labor Statistics, the St. Louis Federal Reserve's FRED database, and the Census Bureau.

The variables in the data are state poverty levels, percentage of a state's employment in manufacturing that year, percentage of a state having a bachelor's degree that year, number of females in a state for a given year, private sector unionization rates in a state for a given year, the percent of a state's private sector workers who are covered by a union-negotiated collective bargaining agreement that year, state per capita income for a given year, the log of state per capita income for a given year, employment to population ratio in a state for a given year, total civilian non-institutional labor force participation in a state for a given year, total land area in square miles for each state, state employment-population ratio for a given year, state unemployment rate for a given year, a 12 month seasonal average of the number of people in a state who've received benefits from the Supplemental Nutritional Assistance Program (SNAP), the log of the seasonal average of SNAP recipients that year in that state, a dummy to indicate whether or not Medicaid expansion provisions from the Affordable Care Act were implemented in that state for that year, the presidential state vote share of the Democratic candidate from the most recent presidential year election, and dummies to indicate whether or not the state received the right-to-work treatment during this time period of 2001-2018.

A table of summary statistics for the main variables in the data can be found in Table 1 on the next page.

Table 1: Summary Statistics for main variables in this dataset.

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
civilian labor force participation rate	900	65.488	4.225	52.900	62.675	68.400	75.300
unemployment rate	900	5.650	1.955	2.400	4.300	6.700	13.500
emp-population ratio	900	0.618	0.046	0.493	0.587	0.650	0.724
fem_pop_total	900	3,077,511.000	3,491,507.000	245,327.000	873,675.500	3,433,167.000	19,869,690.000
ba_or_higher	900	20.725	13.606	0.000	0.000	30.400	44.500
% of workers covered by union contract	900	12.063	5.419	2.600	7.300	15.725	27.700
union_members_percentage	900	10.712	5.416	1.600	6.000	14.500	26.700
land_sq_mi	900	71,272.480	86,698.670	1,034.000	34,577.500	81,869.250	570,665.000
annual per capita income	900	40,536.750	9,244.258	22,781.000	33,901.250	46,062.250	76,456.000
manufacturing employment rate	900	0.090	0.035	0.000	0.062	0.112	0.203
poverty rate	900	12.672	4.425	0.000	10.500	15.700	23.900
snap_users	900	675,696.900	789,338.900	22,538.750	162,127.600	849,977.900	4,417,772.000
log per capita income	900	10.585	0.224	10.034	10.431	10.738	11.244
log snap users	900	12.826	1.164	10.023	11.996	13.653	15.301
dem share	900	0.468	0.096	0.216	0.399	0.541	0.715

4 Methodology—Individual State Difference in Differences

The first empirical strategy is to use a one-way fixed effects Panel OLS model. This allows for the construction the counterfactual estimate for the impact of implementing a right to work law in three states: Michigan, Wisconsin, and Indiana. The model is specified as:

$$y_{it} = \beta X_{it} + \alpha_{it} Z_{it} + c_i + u_{it}$$

Where y_{it} is the dependent variable, either state-level poverty rate, state-level annual per capita income, or state-level SNAP program usage, X_{it} is the vector of independent variables, including employment-population ratio, total female population, rates of bachelor's degree or higher, percent of workers that are union members, manufacturing employment rate, percentage of voters who voted Democratic in the most recent presidential election, and a time dummy for whether or not the state had expanded Medicaid as provisioned by the Affordable Care Act. Z_{it} is the time dummy variable that indicates whether or not the state had right to work implemented at the time, c_i is a state specific fixed effect between the pairs of states that doesn't vary over time, and u_{it} is the error term.

This regression was run 4 times for each dependent variable to obtain necessary estimates of α for treatment and control groups before and after the policy change, forming the needed differences to compute the counterfactual estimates of the Right-to-work policy on the dependent variable.

There did not appear to be specification issues with this model. There is no obvious source of selection bias, since there is no biased sampling of which states receive the treatments since they were all applied independently of each other. Reverse causality should not be an issue, since the Right-to-work policies were passed in the three states for political and economic competitiveness reasons, not as anti-poverty, anti-welfare measures, or wage policies. This is confirmed by remarks issued by then Wisconsin Governor Scott Walker as he signed Right-to-work into law in Wisconsin, where the governor said "Passing right-to-work sends a powerful message around the country and around the world that Wisconsin is the right place because Wisconsin (is) yet again showing we are open for business" (Hall, 2015).

The model controls for various economic, political, and demographic differences between the groups, while capturing unmeasured or unobserved differences between the groups with the state specific fixed effect. Running this model with two-way fixed effects or time-fixed effects yielded numerical instabilities, and a Hausman test for random effects rejected the null hypothesis, confirming the choice to use a one-way fixed effect model.

This model was run for 3 pairs of states to generate counter-factual estimates: Michigan & Ohio, Wisconsin & Minnesota, and Indiana & Illinois.

5 Methodology—Difference in Differences with Multiple Treatment Periods

The second empirical strategy follows from Callaway and Sant'Anna (2018). Using a two-way fixed effects model:

$$y_{it} = \alpha_t + c_i + \beta D_{it} + \alpha X_i + u_{it}$$

where α_t is a time fixed effect, c_i is a state fixed effect, D_{it} is a treatment dummy indicating whether state i received the Right-to-work at time t, X_i is a vector of observed characteristics to serve as controls, and u_{it} is the error term, this paper uses Callaway and Sant'Anna's proposed framework and open-sourced software (Callaway and Sant'Anna 2019) to nonparametrically estimate group-time average treatment effects for multiple treated units and multiple treatment times for poverty rates, annual per capita income, and SNAP program usage in a state.

6 Methodology–Indiana Synthetic Control

Of the three treatment states, Indiana is the only one without a natural control state (see Robustness Checks section). Because of this, it is necessary to use a synthetic control estimation strategy to generate good counterfactual estimates for the effects of the intervention of passing Right-to-work in Indiana as this paper's third empirical strategy.

The synthetic control estimation strategy originated from Abadie et al. (2010). In this paper, they describe an algorithm for synthetic control to derive estimates for comparative case studies that removes bias in choosing control groups for comparative case studies. They proposed a data-driven process to generate a control using pre-intervention data to match the treatment group by constructing a weighted average of possible control units to generate a good match of the treatment group for the outcomes of interest and other observable characteristics, as measured by the root mean squared prediction error of the predicted synthetic control versus the treatment group.

Specifically, the Abadie et al. (2010) empirical strategy is defined as follows:

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

$$\alpha_{it} = Y_{it}^{I} - Y_{it}^{N}$$
$$Y_{it}^{N} = \alpha_t + \theta_t Z_i + \lambda_t \mu_i + u_{it}$$
$$\hat{\alpha}_{it} = Y_{it} + \sum_{j=i+1}^{J+1} w_j^* Y_{jt}$$

where Y_{it} is the observed outcome for unit i at time t, Y_{it}^{N} is the estimated outcome for unit i at time t if the unit is not exposed to an intervention, Y_{it}^{I} is the observed outcome for unit i at time t for units exposed to an intervention, α_{it} is the effect of the intervention for unit i at time t, D_{it} is the dummy indicating whether unit i at time t has been exposed to intervention, α_t is an unknown common factor with constant factor loadings across units, Z_i is a vector of observed characteristics not affected by the intervention, θ_t is a vector of unknown parameters, λ_t is a vector of unobserved common factors, μ_i is a vector of unknown factor loadings, u_{it} are the error terms, $\hat{\alpha}_{it}$ is the estimated effect of the intervention for unit i at time t, and w_j^* is the weight for control unit j, representing an entry in a (j X 1) vector of weights W that is estimated for the pre-treatment period for the control units to generate the synthetic control group using the root mean squared prediction error minimization procedure described above.

In this paper, the observed characteristics are employment-population ratio, total female population, rates of bachelor's degree or higher, percent of workers that are union members, manufacturing employment rate, percentage of voters who voted Democratic in the most recent presidential election, and a time dummy for whether or not the state had expanded Medicaid as provisioned by the Affordable Care Act. The intervention is the passage of a Right-to-work law. The outcomes are state-level poverty rate, state-level annual per capita income, or state-level SNAP program usage.

7 Methodology–Generalized Synthetic Control Estimates

The paper's fourth empirical strategy follows from Xu (2016). Xu proposes a generalized synthetic control method with multiple treated units and differing treatment periods with linear fixed effects. With the accompanying open source R library (Xu and Liu, 2019), the model uses Abadie et al (2010)'s scheme for constructing a synthetic control unit based on observed pre-treatment data, except that instead of using all possible factors, Xu (2016) employs dimensionality reduction to reduce the number of factors that are fitted into the model. To do this, a user chooses the number of factors desired, and then the best possible subset of all factors, smoothed across control units, that minimizes the root mean squared prediction error are selected through cross-validation. Once this is done, using an interactive fixed effects model that is estimated prior from latent factors in the control group data, the counterfactuals are computed based on the estimated latent factors and the estimated factor loadings found through cross-validation. The counterfactuals we get are generalized synthetic control estimates that allow for us to measure average treatment effects in aggregate, meaning that this method will allow for the estimation of the aggregate effect of passing Right-to-work laws in the Midwest on incomes, poverty levels, and SNAP program usage.

8 Discussion of Results

As shown in sections 11 and 13, the generalized synthetic control and difference-in-differences with multiple treatment periods estimation strategies both failed to return statistically significant estimates of the average treatment effect on poverty rates, annual per capita income, SNAP usage, Log per capita income, or Log SNAP usage. In essence, these models indicate that there was no significant effect on poverty rates, per capita income, or SNAP usage in American labor markets as a result of Right-to-work being implemented in Indiana, Michigan and Wisconsin.

This occurred for multiple reasons. First, in the difference-in-differences with multiple treatment periods estimation strategy, Callaway and Sant'Anna (2018) identify a conditional parallel trends assumption that is necessary for their model to generate valid causal estimates. They indicate that while this is "fundamentally untestable", it is necessary to strengthen the assumption by saying that conditional parallel trends holds not only in the pre-treatment period, but also in the post-treatment period. However, this does hold in our intervention because a Right-to-work law fundamentally changes the structure of a labor market by reducing union membership and weakening union bargaining power, while strengthening employer bargaining power, by reducing the financial resources that unions have to conduct their organizing and bargaining activities. Thus, when compared to non right-to-work states, conditional parallel trends should not hold post-treatment, meaning that this estimation strategy is not valid for our intervention. Second, the Xu (2016) generalized synthetic control method is based on an interactive fixed effects model from Bai (2009). This model builds upon a two-way fixed effects model by adding unit-specific intercepts with time-varying coefficients. This is inappropriate for my data, as there appear to be no significant time varying fixed effects factors between states. Furthermore, the Xu model imposes a fairly strict assumption that the error term of any unit at any time period is independent of treatment assignment, observed characteristics, and unobserved differences, time-varying and otherwise, between all units in all periods. This likely does not hold for my model, making the Xu (2016) strategy invalid, as there are a number of unobserved factors, particularly related to cultural and societal values, that could be correlated with my co-variates, especially relating to politics, education, and healthcare policy, all of which I explicitly control for.

On the individual state level, I find that there are generally significant effects of passing a right-to-work law in the treated states of Michigan, Indiana, and Wisconsin.

The Indiana difference-in-differences counter-factual estimates are derived by finding the difference between IL_POST and IL_PRE and IN_RTW_PRE and IN_RTW_POST in tables in Section 10. In Indiana, I find that the counterfactual estimate of the impact of Right-to-work on SNAP program users is a reduction of 1,863,008 people from SNAP program usage, and this is statistically significant at a p-value of 0.01. The counter-factual estimate of the impact of Right-to-work on annual per capita income is a reduction of $2019.47, and this is statistically significant at a p-value of 0.01. The counter-factual estimate of the impact of Right-to-work on poverty rate is a reduction of 2.466 percentage points, but this estimate is not statistically significant. These results are not causally valid, as parallel trends do not hold for Indiana and Illinois (see Section 9).

The Michigan difference-in-differences counter-factual estimates are derived by finding the difference between OH_RTW_pre and OH_RTW_post and MI_YES_RTW and MI_no_RTW in tables in Section 10. In Michigan, I find that the counter-factual estimate of the impact of Right-to-work on SNAP program users is a reduction of 409159.26 people from SNAP program usage, and this is partially statistically significant at a p-value of 0.01. The Ohio coefficients are statistically significant at a p-value of 0.01, while the Michigan coefficients are not significant. The counter-factual estimate of the impact of Right-to-work on annual per capita income is a reduction of $4807.916, and this is statistically significant at a p-value of 0.01. The counter-factual estimate of the impact of Right-to-work on poverty rate is a reduction of 7.342 percentage points, and this estimate is partially statistically significant at a p-value of 0.01. The Ohio coefficients are statistically significant at a p-value of 0.01, while the Michigan coefficients are not significant. These results are causally valid, as parallel trends do hold for Michigan and Ohio (see Section 9). This makes sense, as Michigan and Ohio are generally well matched states. They share a land border, history of industrially focused economies, a strong white working class culture, as well as significant cultural rivalries, especially between the University of Michigan and the Ohio State University.

The Wisconsin difference-in-differences counter-factual estimates are derived by finding the difference between MN_PRE_2015 and MN_POST_2015 and WI_YES_RTW and WI_NO_RTW in tables in Section 10. In Wisconsin, I find that the counterfactual estimate of the impact of Right-to-work on SNAP program users is an increase of 468376.12 people from SNAP program usage, and this is partially statistically significant at a p-value of 0.01. The Wisconsin coefficients are statistically significant at a p-value of 0.01, while the Minnesota coefficients are not significant. The counter-factual estimate of the impact of Right-to-work on annual per capita income is an increase of $740.764, and this is statistically significant at a p-value of 0.01. The counter-factual estimate of the impact of Right-to-work on poverty rate is an increase of 7.864 percentage points, and this estimate is partially statistically significant at a p-value of 0.01. The Wisconsin coefficients are statistically significant at a p-value of 0.01, while the Minnesota coefficients are not significant. These results are causally valid, as parallel trends do hold for Wisconsin and Minnesota (see Section 9). They share a land border, history of agriculturally focused economies, a strong white working class culture from Northern Europe, as well as significant cultural rivalries, especially between the University of Minnesota and the University of Wisconsin and other Wisconsin and Minnesota sports teams.

To generate valid estimates for Indiana, we turn to Abadie et al (2010)'s synthetic control strategy. In this specification, it was necessary to take the logs of annual per capita income and SNAP usage to solve numerical instability issues with the pre-treatment fitting procedure and the root mean squared minimization problem.

For poverty rates, the average treatment effect suggested by this model is no effect. For SNAP usage, the average treatment effect is a 0.15 percent reduction in SNAP program usage. For per capita income, the average treatment effect suggested by this model is an increase of 0.01 percent in per capita income.

Other statistics, such as the state weights selected by the algorithm, the predictor balance, and overall plots between the treatment and synthetic control are reported in Section 12. In section 9, robustness checks for sensitivity analysis, where we test the validity of our causal estimates by reporting results for the causal estimates obtained by removing the two biggest weighted states in donor pool: Ohio and Iowa. This sensitivity analysis shows that there is no significant change in the findings, meaning that the model is generally robust. Placebo tests are also included in section 9, and indicate that with a simulation of other units who are exposed to the treatment when they actually didn't receive it, our intervention is an outlier for all three outcomes, indicating that there is strong evidence against a placebo effect and that the effects of the intervention are actually tied to the right-to-work intervention itself in Indiana.

9 Robustness Checks

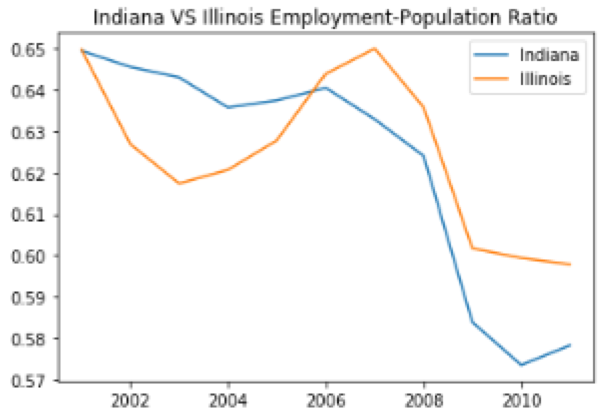

Figure 1: Parallel trends check for Indiana and Illinois.

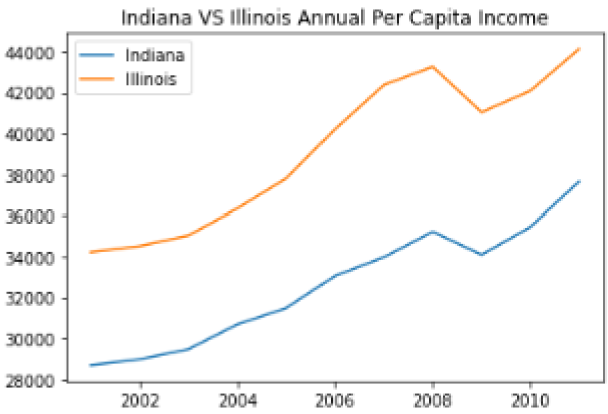

Figure 2: Parallel trends check for Indiana and Illinois.

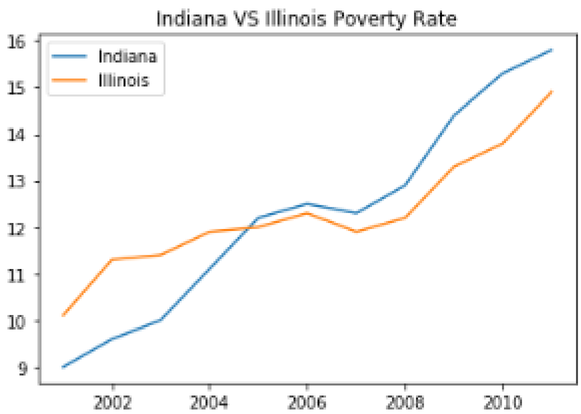

Figure 3: Parallel trends check for Indiana and Illinois.

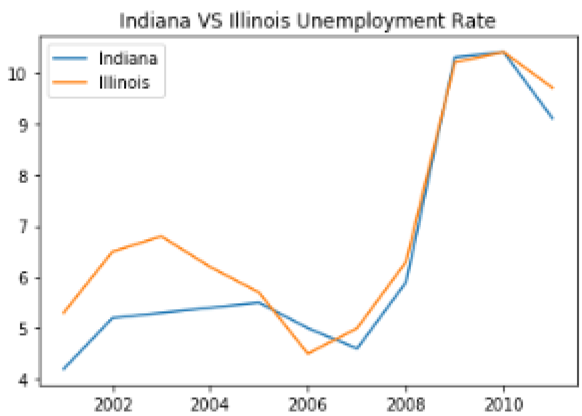

Figure 4: Parallel trends check for Indiana and Illinois.

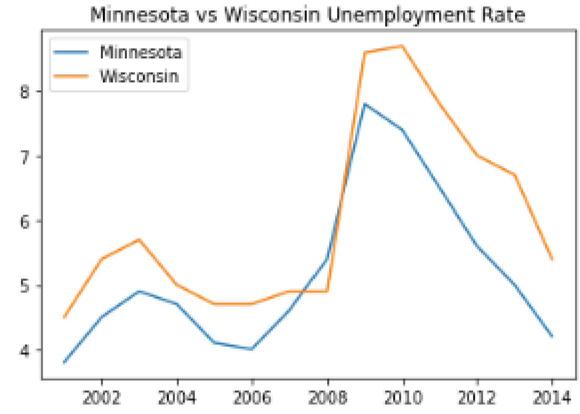

Figure 5: Parallel trends check for Minnesota and Wisconsin.

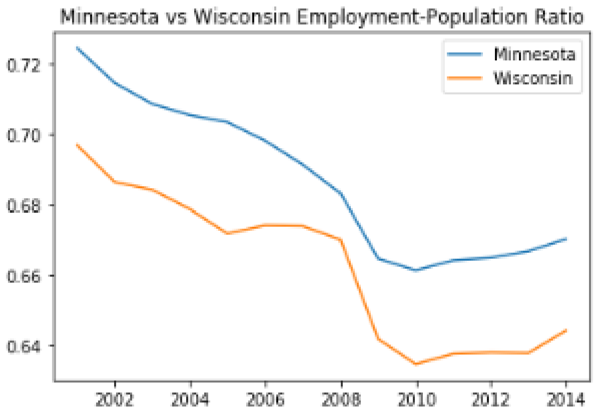

Figure 6: Parallel trends check for Minnesota and Wisconsin.

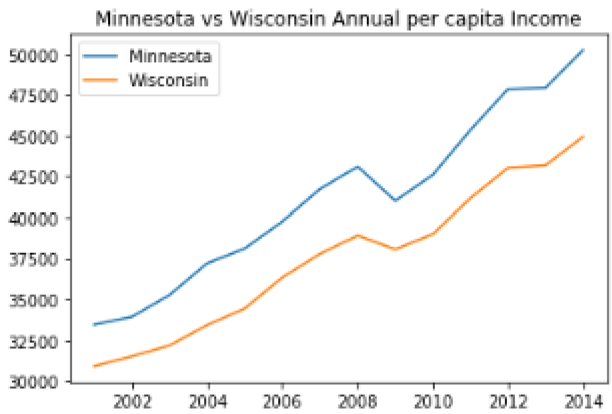

Figure 7: Parallel trends check for Minnesota and Wisconsin.

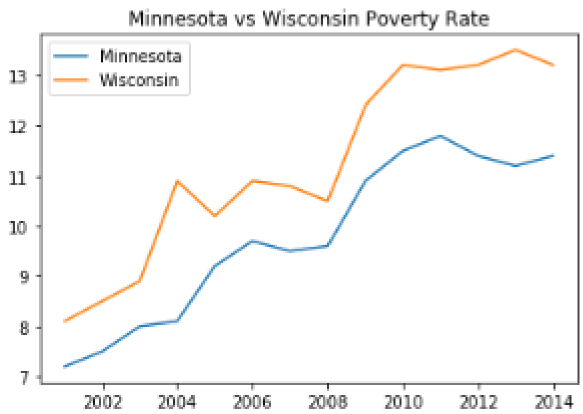

Figure 8: Parallel trends check for Minnesota and Wisconsin.

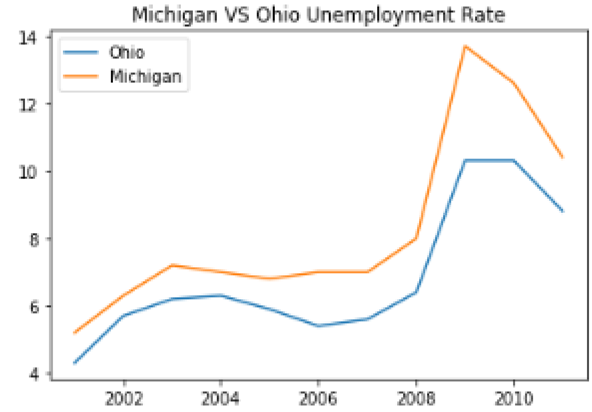

Figure 9: Parallel trends check for Michigan and Ohio.

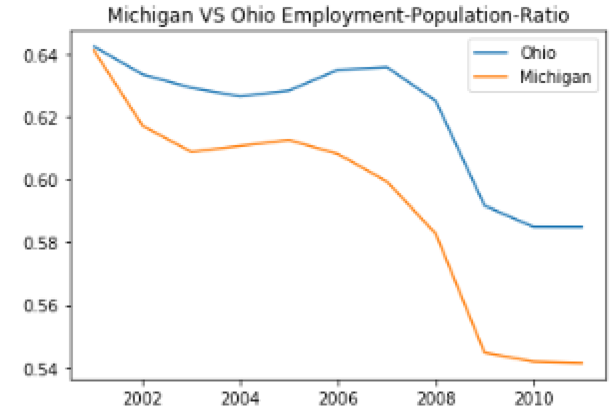

Figure 10: Parallel trends check for Michigan and Ohio.

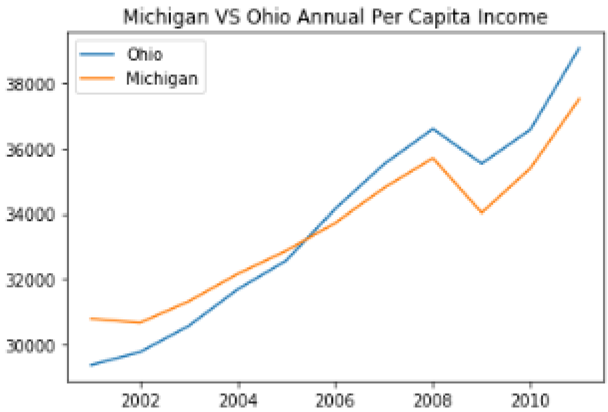

Figure 11: Parallel trends check for Michigan and Ohio.

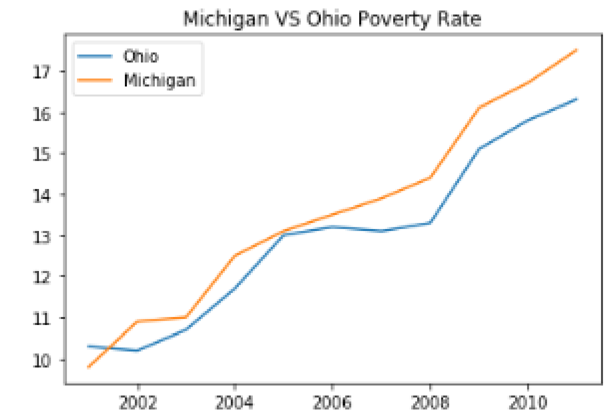

Figure 12: Parallel trends check for Michigan and Ohio.

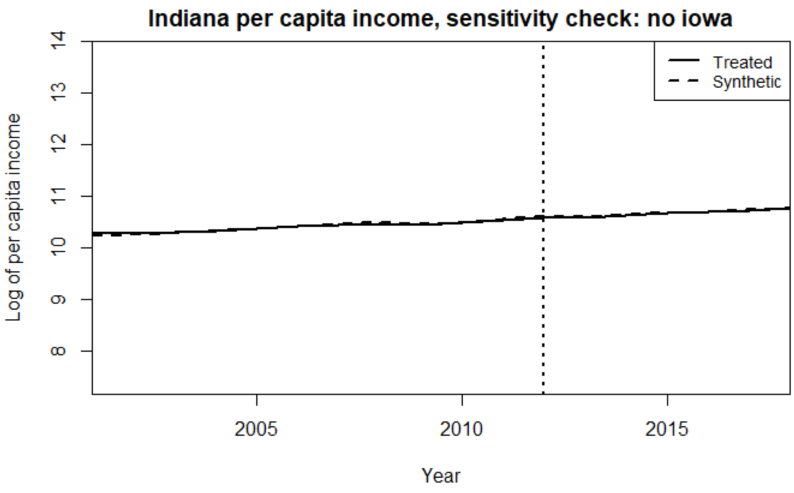

Figure 13: Sensitivity Check for the Indiana Synthetic Control Result.

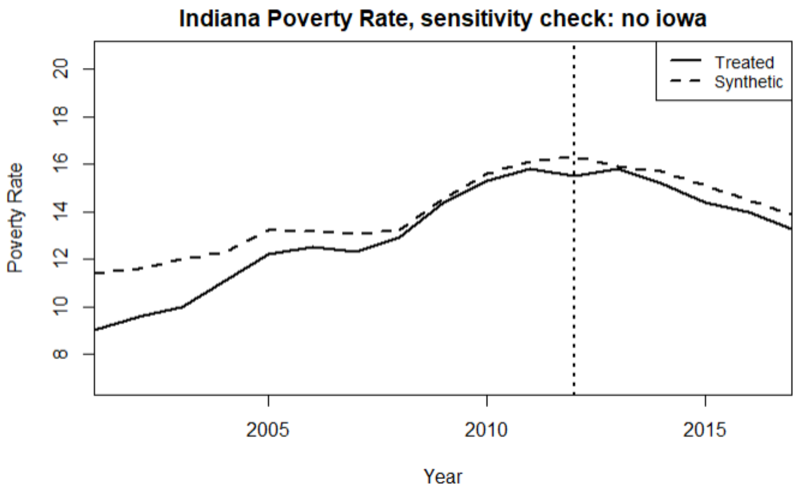

Figure 14: Sensitivity Check for the Indiana Synthetic Control Result.

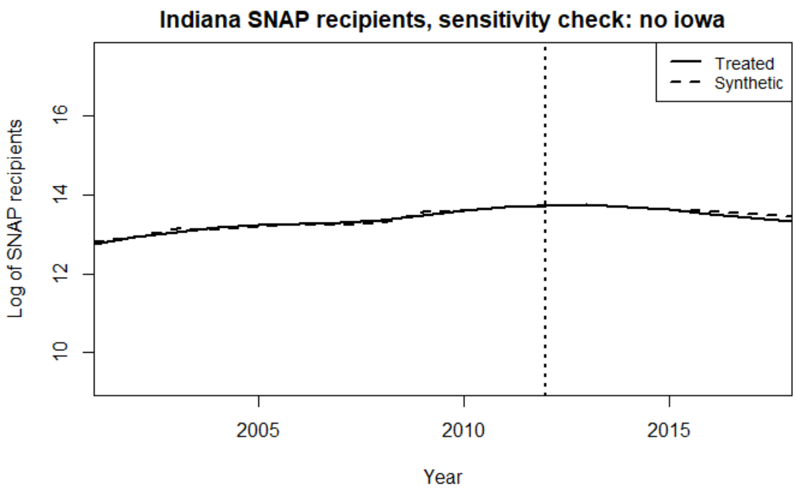

Figure 15: Sensitivity Check for the Indiana Synthetic Control Result.

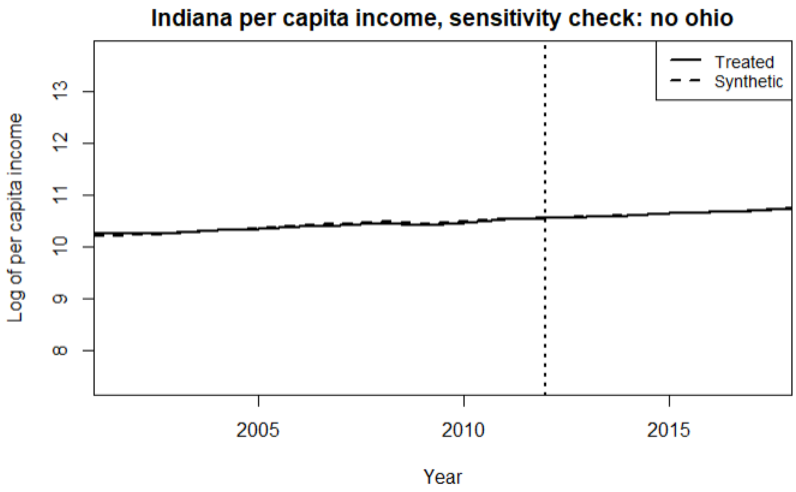

Figure 16: Sensitivity Check for the Indiana Synthetic Control Result.

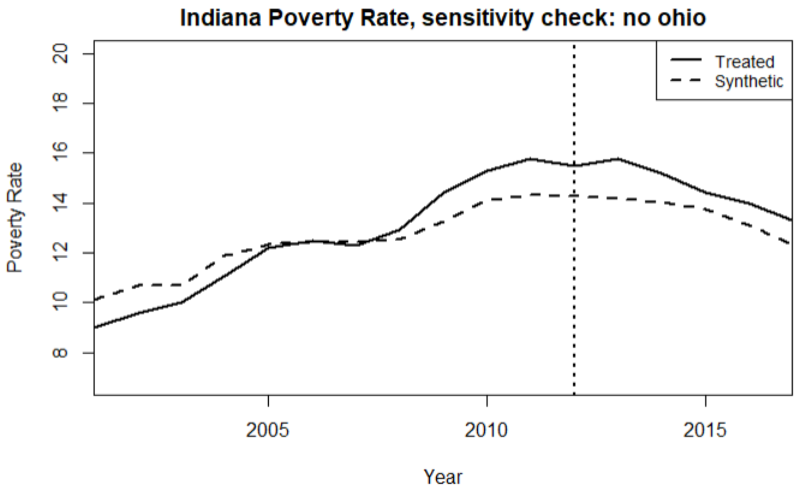

Figure 17: Sensitivity Check for the Indiana Synthetic Control Result.

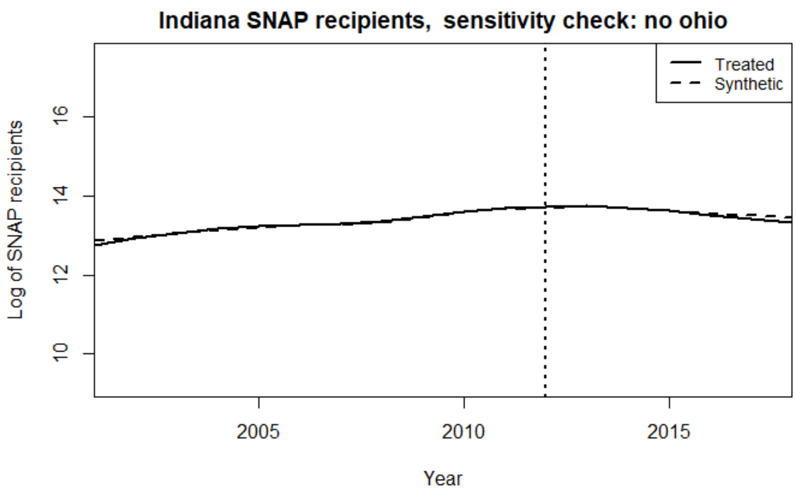

Figure 18: Sensitivity Check for the Indiana Synthetic Control Result.

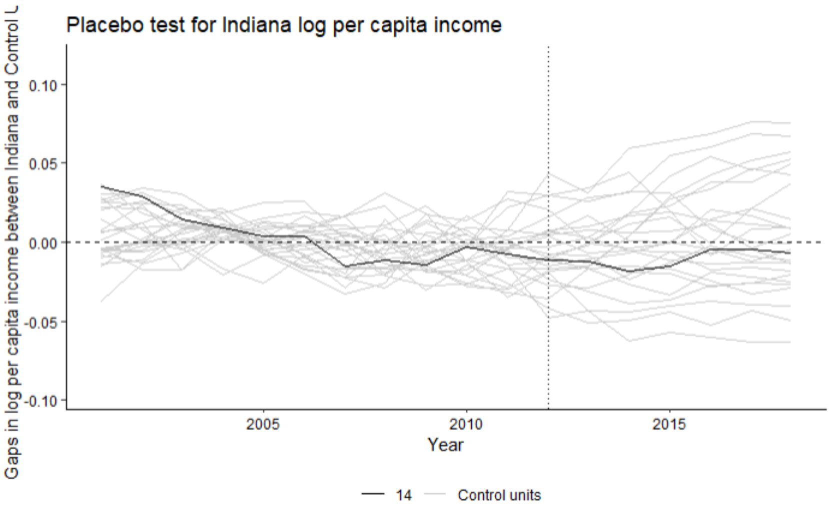

Figure 19: Placebo Test for the Indiana Synthetic Control Result.

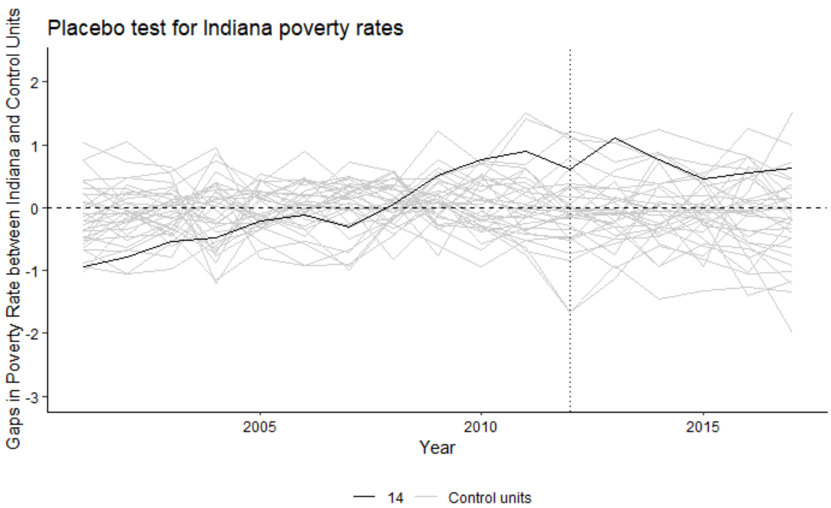

Figure 20: Placebo Test for the Indiana Synthetic Control Result.

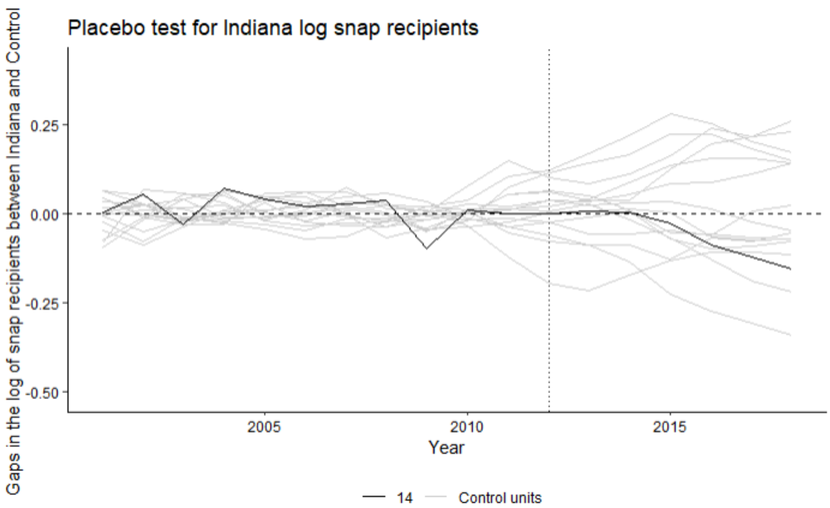

Figure 21: Placebo Test for the Indiana Synthetic Control Result.

10 Results—-Individual State Difference in Differences

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Illinois/Indiana SNAP Panel OLS Model Result Table
===============================================================================================
                                                    Dependent variable:
                         ----------------------------------------------------------------------
                                                        snap_users
```

	HC1 robust SE (1)	HC1 robust SE (2)	HC1 robust SE (3)	HC1 robust SE (4)
epr	-12,689,034.000***	-12,689,034.000***	-3,552,254.000***	-3,552,254.000***
	(83,894.260)	(83,894.260)	(1,130,897.000)	(1,130,897.000)
fem_pop_total	0.980	0.980	0.808***	0.808***
	(1.187)	(1.187)	(0.097)	(0.097)
ba_or_higher	18,123.840***	18,123.840***	7,648.863***	7,648.863***
	(2,333.018)	(2,333.018)	(736.011)	(736.011)
union_members_percentage	-77,422.380***	-77,422.380***	-23,876.960***	-23,876.960***
	(2,854.947)	(2,854.947)	(8,824.330)	(8,824.330)
mer	19,836,860.000***	19,836,860.000***	3,799,372.000	3,799,372.000
	(2,186,414.000)	(2,186,414.000)	(4,560,601.000)	(4,560,601.000)
dem_share	-1,081,315.000	-1,081,315.000	964,206.900*	964,206.900*
	(679,017.200)	(679,017.200)	(556,951.100)	(556,951.100)
medicaid_expansion	261,561.400***	261,561.400***	-32,742.950	-32,742.950
	(34,061.280)	(34,061.280)	(28,320.660)	(28,320.660)
IN_RTW_PRE	423,761.100**			
	(213,880.000)			
IN_RTW_POST		-423,761.100**		
		(213,880.000)		
IL_PRE			-507,743.100***	
			(62,286.260)	
IL_POST				507,743.100***
				(62,286.260)
Observations	36	36	36	36
F Statistic (df = 8; 26)	17.524***	17.524***	34.509***	34.509***

Note: *p<0.1; **p<0.05; ***p<0.01

Illinois/Indiana per capita income Panel OLS Model Result Table

	Dependent variable:			
	annual_pci			
	HC1 robust SE (1)	HC1 robust SE (2)	HC1 robust SE (3)	HC1 robust SE (4)
epr	-13,181.930	-13,181.930	12,958.290	12,958.290
	(8,617.013)	(8,617.013)	(20,414.220)	(20,414.220)
fem_pop_total	-0.008	-0.008	0.012	0.012
	(0.007)	(0.007)	(0.016)	(0.016)
ba_or_higher	180.382***	180.382***	107.469***	107.469***
	(2.596)	(2.596)	(8.115)	(8.115)
union_members_percentage	-602.202***	-602.202***	-656.463***	-656.463***
	(72.384)	(72.384)	(131.527)	(131.527)
mer	-102,297.600**	-102,297.600**	-87,236.150	-87,236.150
	(42,847.230)	(42,847.230)	(124,579.400)	(124,579.400)
dem_share	-15,109.270**	-15,109.270**	-11,371.680***	-11,371.680***
	(6,104.784)	(6,104.784)	(3,510.029)	(3,510.029)
medicaid_expansion	6,084.252***	6,084.252***	4,983.148***	4,983.148***
	(445.112)	(445.112)	(311.199)	(311.199)
IN_RTW_PRE	-1,649.354***			
	(505.677)			
IN_RTW_POST		1,649.354***		
		(505.677)		
IL_PRE			-2,659.088***	
			(661.743)	
IL_POST				2,659.088***
				(661.743)
Observations	36	36	36	36
F Statistic (df = 8; 26)	53.825***	53.825***	61.922***	61.922***

Note: *p<0.1; **p<0.05; ***p<0.01

Illinois/Indiana Poverty Rate Panel OLS Model Result Table

	Dependent variable:

	poverty_rate			
	HC1 robust SE	HC1 robust SE	HC1 robust SE	HC1 robust SE
	(1)	(2)	(3)	(4)
epr	-78.541*	-78.541*	-67.391	-67.391
	(42.750)	(42.750)	(70.015)	(70.015)
fem_pop_total	0.00002	0.00002	0.00002	0.00002
	(0.00003)	(0.00003)	(0.00002)	(0.00002)
ba_or_higher	0.036	0.036	0.026	0.026
	(0.072)	(0.072)	(0.088)	(0.088)
union_members_percentage	0.668**	0.668**	0.746***	0.746***
	(0.273)	(0.273)	(0.047)	(0.047)
mer	46.443	46.443	23.320	23.320
	(34.646)	(34.646)	(29.870)	(29.870)
dem_share	-6.091*	-6.091*	-3.472	-3.472
	(3.453)	(3.453)	(11.750)	(11.750)
medicaid_expansion	-2.571***	-2.571***	-2.915***	-2.915***
	(0.133)	(0.133)	(0.934)	(0.934)
IN_RTW_PRE	0.684			
	(2.383)			
IN_RTW_POST		-0.684		
		(2.383)		
IL_PRE			-0.549	
			(1.109)	
IL_POST				0.549
				(1.109)
Observations	36	36	36	36
F Statistic (df = 8; 26)	2.186*	2.186*	2.186*	2.186*

Note: *p<0.1; **p<0.05; ***p<0.01

Michigan/Ohio Poverty Rate Panel OLS Model Result Table

	Dependent variable:			
	poverty_rate			
	HC1 robust SE	HC1 robust SE	HC1 robust SE	HC1 robust SE
	(1)	(2)	(3)	(4)
epr	-8.453	-8.453	17.324	17.324
	(5.317)	(5.317)	(29.256)	(29.256)
fem_pop_total	-0.00001	-0.00001	-0.00003***	-0.00003***
	(0.00001)	(0.00001)	(0.00001)	(0.00001)
ba_or_higher	-0.012	-0.012	-0.035*	-0.035*
	(0.034)	(0.034)	(0.019)	(0.019)
union_members_percentage	0.242	0.242	-0.068	-0.068
	(0.796)	(0.796)	(0.127)	(0.127)
mer	-68.594**	-68.594**	-88.462**	-88.462**
	(27.214)	(27.214)	(41.183)	(41.183)
dem_share	60.092***	60.092***	54.563***	54.563***
	(3.423)	(3.423)	(13.194)	(13.194)
medicaid_expansion	-0.565***	-0.565***	-1.155***	-1.155***
	(0.216)	(0.216)	(0.020)	(0.020)
MI_YES_RTW	1.719			
	(2.554)			
MI_no_RTW		-1.719		
		(2.554)		
OH_RTW_pre			-1.952***	
			(0.504)	
OH_RTW_post				1.952***
				(0.504)
Observations	36	36	36	36
F Statistic (df = 8; 26)	2.502**	2.502**	2.544**	2.544**

Note: *p<0.1; **p<0.05; ***p<0.01

Michigan/Ohio per capita Income Panel OLS Model Result Table

Dependent variable:

	annual_pci			
	HC1 robust SE	HC1 robust SE	HC1 robust SE	HC1 robust SE
	(1)	(2)	(3)	(4)
epr	-50,929.910***	-50,929.910***	-32,028.150***	-32,028.150***
	(9,701.494)	(9,701.494)	(8,440.372)	(8,440.372)
fem_pop_total	0.048***	0.048***	0.033***	0.033***
	(0.010)	(0.010)	(0.011)	(0.011)
ba_or_higher	93.370***	93.370***	77.941***	77.941***
	(16.244)	(16.244)	(7.544)	(7.544)
union_members_percentage	-1,163.926***	-1,163.926***	-1,343.864***	-1,343.864***
	(285.201)	(285.201)	(135.952)	(135.952)
mer	29,338.730	29,338.730	12,579.370	12,579.370
	(25,989.870)	(25,989.870)	(13,219.220)	(13,219.220)
dem_share	-32,623.420***	-32,623.420***	-35,919.260***	-35,919.260***
	(1,061.584)	(1,061.584)	(3,002.830)	(3,002.830)
medicaid_expansion	2,925.865***	2,925.865***	2,521.529***	2,521.529***
	(543.923)	(543.923)	(365.213)	(365.213)
MI_YES_RTW	1,010.067***			
	(186.383)			
MI_no_RTW		-1,010.067***		
		(186.383)		
OH_RTW_pre			-1,393.891***	
			(404.456)	
OH_RTW_post				1,393.891***
				(404.456)
Observations	36	36	36	36
F Statistic (df = 8; 26)	70.507***	70.507***	72.880***	72.880***

Note: *p<0.1; **p<0.05; ***p<0.01

Michigan/Ohio SNAP Panel OLS Model Result Table

	Dependent variable:			
	snap_users			
	HC1 robust SE	HC1 robust SE	HC1 robust SE	HC1 robust SE
	(1)	(2)	(3)	(4)
epr	-11,322,580.000***	-11,322,580.000***	-9,061,483.000***	-9,061,483.000***
	(637,016.000)	(637,016.000)	(335,164.200)	(335,164.200)
fem_pop_total	3.050***	3.050***	2.050***	2.050***
	(0.061)	(0.061)	(0.055)	(0.055)
ba_or_higher	1,526.485	1,526.485	749.275	749.275
	(1,425.434)	(1,425.434)	(639.189)	(639.189)
mer	6,389,188.000***	6,389,188.000***	2,753,331.000***	2,753,331.000***
	(2,413,517.000)	(2,413,517.000)	(705,472.500)	(705,472.500)
union_members_percentage	-110,839.200***	-110,839.200***	-96,764.340***	-96,764.340***
	(20,862.040)	(20,862.040)	(457.982)	(457.982)
dem_share	1,291,177.000**	1,291,177.000**	1,461,619.000**	1,461,619.000**
	(531,348.600)	(531,348.600)	(661,751.300)	(661,751.300)
medicaid_expansion	-147,413.900***	-147,413.900***	-174,526.000***	-174,526.000***
	(33,812.970)	(33,812.970)	(41,574.000)	(41,574.000)
MI_no_RTW	65,525.130			
	(67,175.170)			
MI_YES_RTW		-65,525.130		
		(67,175.170)		
OH_RTW_pre			-139,054.500***	
			(9,828.149)	
OH_RTW_post				139,054.500***
				(9,828.149)
Observations	36	36	36	36
F Statistic (df = 8; 26)	63.613***	63.613***	69.686***	69.686***

Note: *p<0.1; **p<0.05; ***p<0.01

Minnesota/Wisconsin SNAP Panel OLS Model Result Table

	Dependent variable:			
	snap_users			
	HC1 robust SE	HC1 robust SE	HC1 robust SE	HC1 robust SE
	(1)	(2)	(3)	(4)
epr	-3,960,576.000***	-3,960,576.000***	-7,168,305.000***	-7,168,305.000***
	(1,491,383.000)	(1,491,383.000)	(2,733,204.000)	(2,733,204.000)
fem_pop_total	-0.206	-0.206	-0.254	-0.254
	(0.263)	(0.263)	(0.617)	(0.617)
ba_or_higher	-3,402.140***	-3,402.140***	-2,815.520***	-2,815.520***
	(472.638)	(472.638)	(930.317)	(930.317)
union_members_percentage	-78,743.150***	-78,743.150***	-46,579.170***	-46,579.170***
	(11,742.270)	(11,742.270)	(7,642.405)	(7,642.405)
mer	-2,856,955.000***	-2,856,955.000***	686,545.900	686,545.900
	(1,000,743.000)	(1,000,743.000)	(1,537,636.000)	(1,537,636.000)
dem_share	439,354.200	439,354.200	553,739.900*	553,739.900*
	(539,747.600)	(539,747.600)	(310,094.100)	(310,094.100)
medicaid_expansion	35,401.510	35,401.510	56,162.540***	56,162.540***
	(38,140.820)	(38,140.820)	(11,625.050)	(11,625.050)
WI_YES_RTW	-271,633.400***			
	(18,661.270)			
WI_NO_RTW		271,633.400***		
		(18,661.270)		
MN_PRE_2015			-37,445.340	
			(62,927.290)	
MN_POST_2015				37,445.340
				(62,927.290)
Observations	36	36	36	36
R2	0.913	0.913	0.888	0.888
Adjusted R2	0.883	0.883	0.849	0.849
F Statistic (df = 8; 26)	34.104***	34.104***	25.645***	25.645***

Note: *p<0.1; **p<0.05; ***p<0.01

Minnesota/Wisconsin Poverty Rate Panel OLS Model Result Table

	Dependent variable:			
	poverty_rate			
	HC1 robust SE	HC1 robust SE	HC1 robust SE	HC1 robust SE
	(1)	(2)	(3)	(4)
epr	-103.685***	-103.685***	-148.430***	-148.430***
	(27.636)	(27.636)	(23.850)	(23.850)
fem_pop_total	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0.00000)	(0.00000)	(0.00001)	(0.00001)
ba_or_higher	0.054***	0.054***	0.061***	0.061***
	(0.005)	(0.005)	(0.021)	(0.021)
union_members_percentage	-0.716***	-0.716***	-0.227**	-0.227**
	(0.165)	(0.165)	(0.114)	(0.114)
mer	-46.012	-46.012	4.555	4.555
	(45.774)	(45.774)	(51.046)	(51.046)
dem_share	16.157***	16.157***	16.947***	16.947***
	(3.373)	(3.373)	(1.199)	(1.199)
medicaid_expansion	3.251***	3.251***	3.784***	3.784***
	(0.586)	(0.586)	(0.528)	(0.528)
WI_YES_RTW	-3.933***			
	(0.859)			
WI_NO_RTW		3.933***		
		(0.859)		
MN_PRE_2015			-0.001	
			(0.070)	
MN_POST_2015				0.001
				(0.070)
Observations	36	36	36	36
F Statistic (df = 8; 26)	3.350***	3.350***	3.067**	3.067**

Note: *p<0.1; **p<0.05; ***p<0.01

Minnesota/Wisconsin Per capita income Panel OLS Model Result Table

	Dependent variable:			
	annual_pci			
	HC1 robust SE	HC1 robust SE	HC1 robust SE	HC1 robust SE
	(1)	(2)	(3)	(4)
epr	103,203.200***	103,203.200***	92,735.910**	92,735.910**
	(31,086.600)	(31,086.600)	(39,513.640)	(39,513.640)
fem_pop_total	0.089***	0.089***	0.088***	0.088***
	(0.001)	(0.001)	(0.0003)	(0.0003)
ba_or_higher	-9.559	-9.559	-7.098	-7.098
	(19.150)	(19.150)	(14.111)	(14.111)
union_members_percentage	-871.727***	-871.727***	-790.308***	-790.308***
	(140.116)	(140.116)	(55.854)	(55.854)
mer	-21,262.180	-21,262.180	-10,372.960	-10,372.960
	(28,020.980)	(28,020.980)	(34,553.640)	(34,553.640)
dem_share	5,527.158**	5,527.158**	6,377.442*	6,377.442*
	(2,683.193)	(2,683.193)	(3,633.815)	(3,633.815)
medicaid_expansion	-703.434***	-703.434***	-779.687***	-779.687***
	(189.705)	(189.705)	(218.247)	(218.247)
WI_YES_RTW	-801.169			
	(572.658)			
WI_NO_RTW		801.169		
		(572.658)		
MN_PRE_2015			-430.787***	
			(153.592)	
MN_POST_2015				430.787***
				(153.592)
Observations	36	36	36	36
F Statistic (df = 8; 26)	308.584***	308.584***	305.727***	305.727***

Note: *p<0.1; **p<0.05; ***p<0.01

11 Results—Difference in Differences with Multiple Treatment Periods

Average Treatment Effects Results Table for Callaway and Sant'Anna (2018)'s
Aggregated Difference-in-Differences Method

	annual_pci	poverty_rate	snap_users
Simple ATT :	316.6841	-0.4186063	-220301.4
Std.Error	(580.3686)	(0.2044784)	(32183.02)
Selective ATT :	317.1451	-0.4520158	-229742.6
Std.Error	(580.3686)	(0.2044784)	(32183.02)
Dynamic ATT :	310.1623	-0.3028931	-222413.7
Std.Error	(581.2444)	(0.2646039)	(56756.48)
Calendar ATT :	308.2873	-0.4253453	-204967.6
Std.Error	(545.5827)	(0.202978)	(48717.79)

12 Results—Indiana Synthetic Control

	Poverty Rate weight
Arkansas	0.12
Iowa	0.49
Ohio	0.39

Table 2: State Weights for poverty rate computed by Abadie et al. (2010)'s synthetic control procedure

	Log Per capita income (annual) weight
Alabama	0.001
Arkansas	0.250
Iowa	0.147
Mississippi	0.001
Ohio	0.598

Table 3: State Weights for Log annual per capita income computed by Abadie et al. (2010)'s synthetic control procedure

	Log SNAP Usage weight
Alabama	0.052
Arkansas	0.242
Iowa	0.228
Ohio	0.477

Table 4: State Weights for Log SNAP Usage computed by Abadie et al. (2010)'s synthetic control procedure

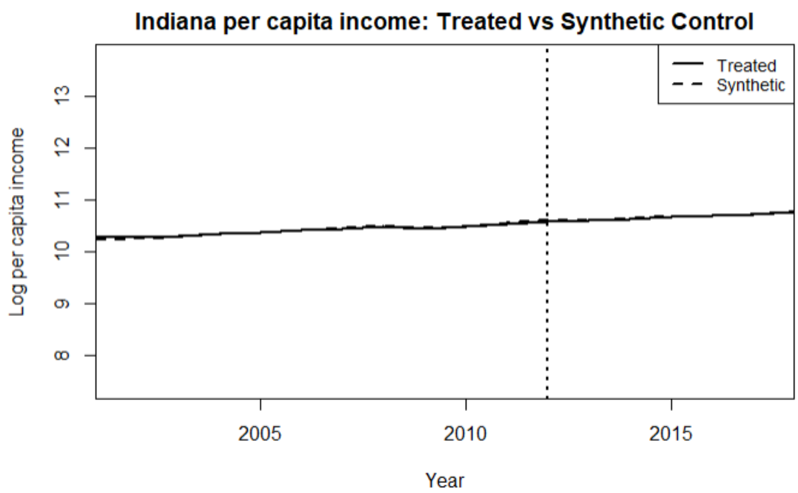


Figure 22: Effects of intervention on per capita income.

	Poverty Rate predictor means		
	Treated	Synthetic	Sample Mean
Employment-Population Ratio	0.662	0.647	0.628
Female Population Total	3215869.727	3221585.400	2985487.656
Bachelor's or Higher	12.264	12.997	14.871
Manufacturing Employment Rate	0.177	0.143	0.094
Union Members Percentage	12.045	12.042	11.120
Democratic Vote Share	0.436	0.447	0.476

Table 5: Indiana Synthetic Control Poverty Rate predictor means.

	Log annual per capita income predictor means		
	Treated	Synthetic	Sample Mean
Employment-Population Ratio	0.622	0.620	0.628
Female Population Total	3215869.727	4112484.726	2985487.656
Bachelor's or Higher	12.264	12.526	14.871
Manufacturing Employment Rate	0.177	0.144	0.094
Union Members Percentage	12.045	11.999	11.120
Democratic Vote Share	0.436	0.412	0.476

Table 6: Indiana Synthetic Control Log annual per capita income predictor means.

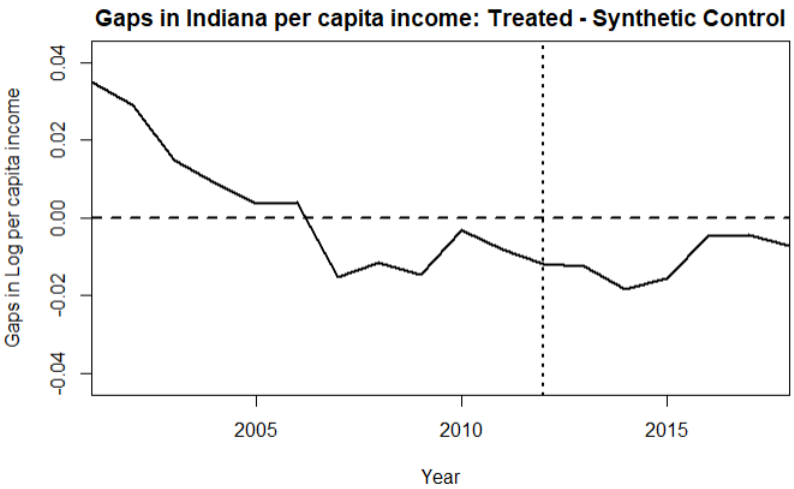
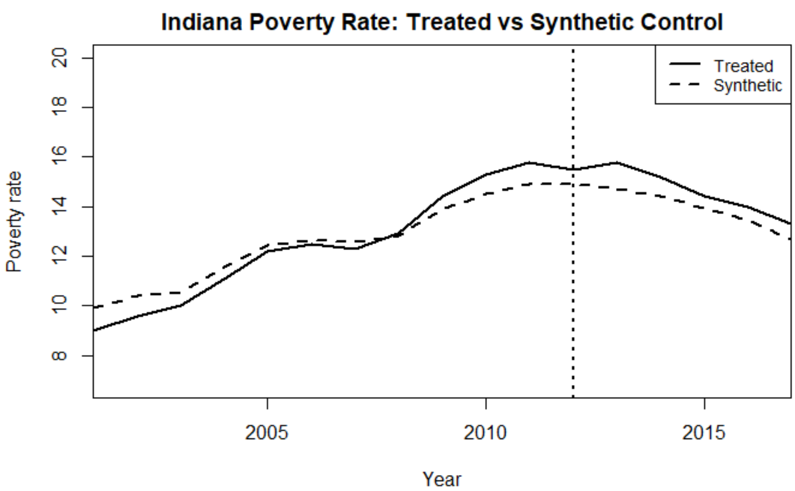


Figure 23: Gaps between Indiana and synthetic on per capita income.



	Log SNAP usage predictor means		
	Treated	Synthetic	Sample Mean
Employment-Population Ratio	0.622	0.623	0.628
Female Population Total	3215869.727	3625108.333	2985487.656
Bachelor's or Higher	12.264	12.507	14.871
Manufacturing Employment Rate	0.177	0.145	0.094
Union Members Percentage	12.045	11.508	11.120
Democratic Vote Share	0.436	0.421	0.476

Table 7: Indiana Synthetic Control Log SNAP usage predictor means.

Figure 24: Effects of intervention on poverty rate.

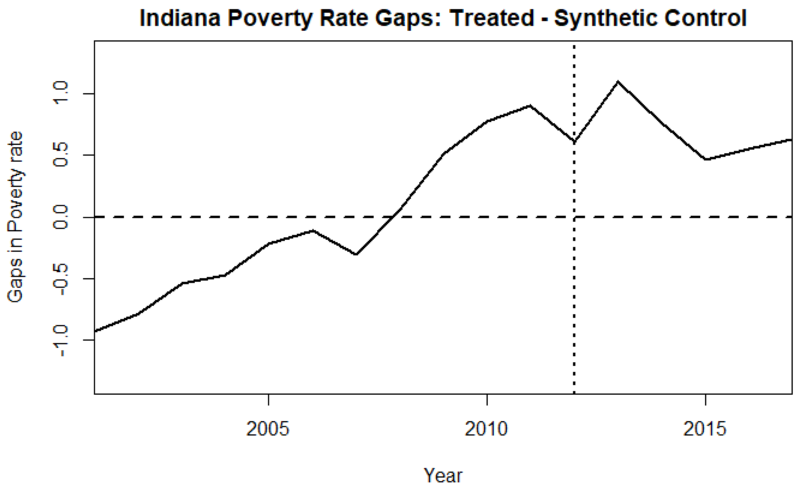


Figure 25: Gaps between Indiana and synthetic on poverty rate.

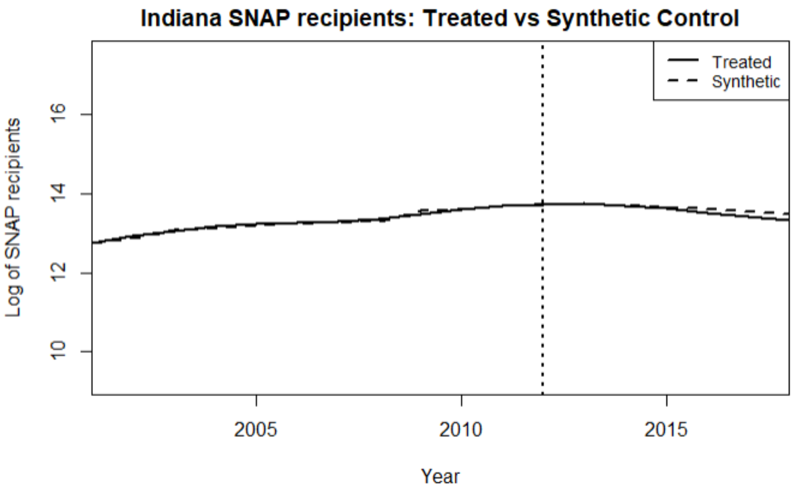


Figure 26: Effects of intervention on SNAP program usage.

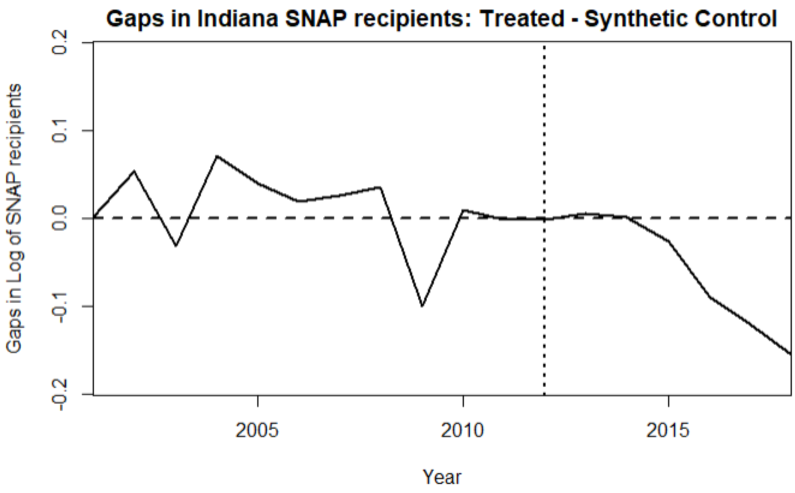


Figure 27: Gaps between Indiana and synthetic on SNAP program usage.

13 Results—Generalized Synthetic Control

	Average Treatment Effects		
	Poverty Rate	Log per capita income	Log SNAP Usage
Average Treatment Effect	−0.8273	0.015	−0.06919
Standard Error	0.8749	0.023	0.1565
Confidence Interval–lower	−2.092	-0.04133	−0.233
Confidence Interval–upper	1.241	0.04753	0.4113
p-value	0.5	0.66	0.73

Table 8: Average Treatment Effects of passing a Right-to-work law as computed by Xu (2017) Generalized Synthetic Control Method.

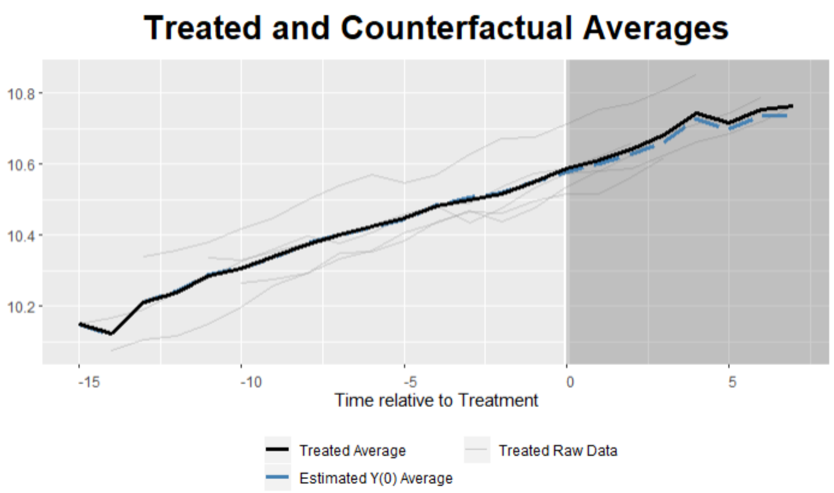


Figure 28: Graphical results for log per capita income of the Generalized Synthetic Control Method.

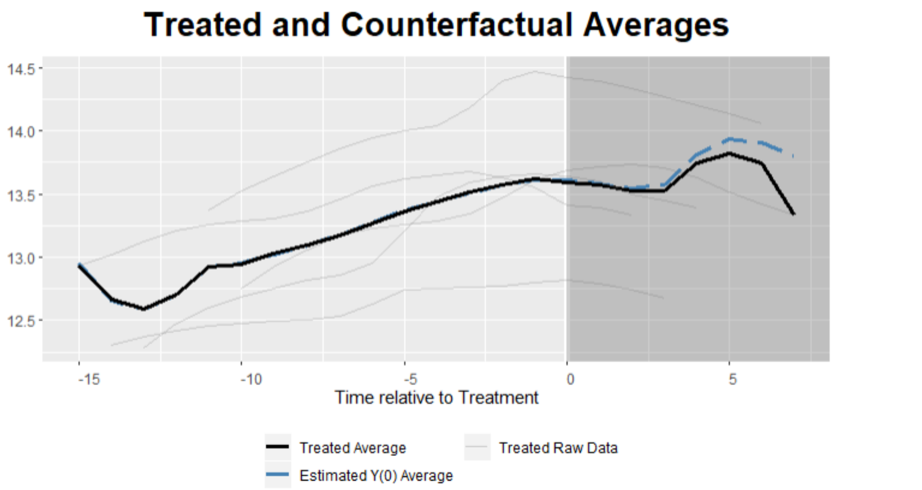


Figure 29: Graphical results for log SNAP usage of the Generalized Synthetic Control Method.

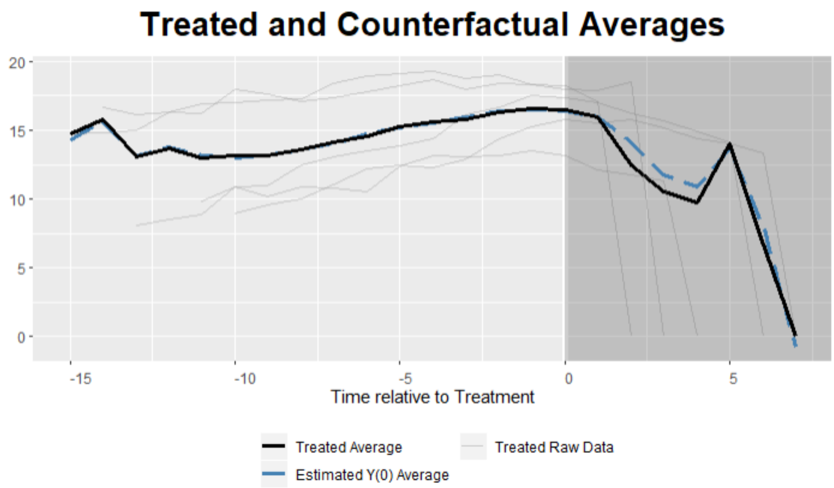


Figure 30: Graphical results for poverty rate of the Generalized Synthetic Control Method.

14 Conclusion

In conclusion, we find that there are no significant aggregate effects of the implementation of the Right-to-Work policy in Indiana, Michigan, and Wisconsin. However, we find that there are significant effects on the state level. In Michigan, SNAP program usage declined, annual per capita income declined, and poverty rate declined. In Wisconsin, SNAP program usage increased, annual per capita income increased, and poverty rate increased. Finally in Indiana, while difference-in-differences estimation is rejected, the synthetic control estimates are valid and show that there is no effect on poverty rates, a small reduction in SNAP program usage, and a small increase in annual per capita income.

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