Revisiting the Occupational Health Impact of the Right-to-Work Laws: A Research Note

Abstract

This research note re-evaluates the occupational health impact of Right-to-Work (RTW) legislation, incorporating recent developments in causal inference techniques. It is particularly urgent to understand this question in an era marked by an uptick in the adoption of anti-union legislation and increases in workplace fatalities and injuries. Using a state-year-level dataset spanning 28 years collected from multiple data sources, we apply an innovative generalized synthetic control method to overcome several limitations of the traditional two-way fixed effects approach to examine the effect of RTW laws on occupational fatal injuries as well as various other health outcomes. Robustness checks were also conducted using a wide range of alternative methods for two-way fixed effects adjustments. Inconsistent with findings from previous studies, we find null effects on occupational fatal injuries, as well as on all other health outcomes. Our findings highlight the importance of revisiting research questions using updated methodological tools.

Key Words: Right-to-work; Occupational health; Mortality; Counterfactual methods; Generalized synthetic control

Introduction

Unionization is a hotly contested issue in the United States, sparking debate between pro-labor and pro-business factions across the nation. In recent years, union-related policies have played a growing role in states' policy agendas. Though some policies, such as the newly passed Protecting the Right to Organize (PRO) Act, have strengthened unions' ability to organize workers and collectively bargain, the more dominant trend appears to have been towards policies that weaken organized labor. Since the Taft-Hartley Act of 1947, which permitted individual states to pass Right-To-Work (RTW) laws, many states have implemented these laws. Broadly defined, such laws prohibit labor unions from requiring workers to pay union membership fees, even if the workers receive union representation in collective bargaining. While proponents of RTW laws assert that they spur economic development and industrial growth by providing a "favorable labor climate" to attract businesses (Moore 1998), opponents argue that they stifle union funding and weaken the unions' power to protect workers' rights and collectively bargain. Currently, 27 states have RTW laws. In Appendix Figure A1, we present a map displaying the RTW status of all states up to 2019. Consistent with this trend, union membership has trended downwards in recent decades, falling from 20.1% of the U.S. labor force in 1983 (17.7 million members) to 10.3% in 2019 (14.6 million members) (Bureau of Labor Statistics 2020).

Studies on the economic effects of RTW laws have produced mixed results. Several studies have found evidence of a positive economic impact (e.g., Chava et al. 2020; Holmes 1998). By contrast, other studies have reached inconclusive results regarding the effect of RTW laws on wages and employment levels (Carroll 1983; Gould and Kimball 2015; Lafer and Allegretto 2011; Moore et al. 1986; Reed 2003; Wessels 1981). A comprehensive literature review of relevant studies from 1980 to 2011 by Bruno et al. (2015) found that RTW laws had minimal to no impact on wages and employment levels, as RTW laws were estimated to reduce wages by 0 to 5% and impact employment levels by 0 to ±3%. While the economic impacts of RTW laws remain debated, a substantial literature agrees that they tend to weaken union bargaining power and reduce unionization rates (Carroll 1983; Ellwood and Fine 1987; Eren and Ozbeklik 2016; Garofalo and Malhotra 1992; Lumsden and Petersen 1975; Moore 1998; Zoorob 2018; Zullo 2011).

In addition to the considerable literature focusing on their economic implications, there has been a growing literature examining the health impacts of RTW laws. It is particularly urgent to understand this question in an era with an uptick in the adoption of anti-union legislation and increases in workplace fatalities and injuries (AFL-CIO 2022; National Safety Council 2020). There are theoretical reasons to investigate this relationship, as several studies have found that unionization promotes health (Eisenberg-Guyot et al. 2020a; Eisenberg-Guyot et al. 2020b; Reynolds and Brady 2012), mainly through three mechanisms. First, compared to non-unionized workers, unionized workers are more likely to receive employer-sponsored health insurance, pay lower cost-shares, and remain covered by their employer after retirement (Buchmueller et al. 2002; Fichtenbaum and Olson 2002; Gould and Kimball 2015; Walters and Mishel 2003). Second, unions also improve workplace safety by providing resources such as protective equipment, accident prevention programs, and training on safety practices (Hagedorn et al. 2016; Zullo 2011). Third, unions deliver interventions that directly aim to promote worker health. Throughout the U.S. labor movement, unions have supported smoking restrictions in the workplace, sponsored programs to screen and treat for hypertension, and improved environmental conditions to eliminate asthma triggers (Malinowski et al. 2015). Unions may also promote psychosocial influences on health, such as job stability, democratic participation, and a sense of belonging (Hagedorn et al. 2016). Therefore, RTW laws' negative impact on union membership may have added effects on workers' and their communities' health.

Two studies have investigated the link between RTW laws and occupational health, both of which used state-level annual data. Applying linear regression models controlling for various covariates including GDP per employee, percentage of building construction employees, percentage of heavy/civil engineering employees, and whether a state has an Occupational Safety and Health Administration (OSHA)—approved health and safety program, Zullo (2011) shows no significant associations between RTW laws and two health outcomes: fatality rates in the construction industry between 2001 and 2009, and fatalities in construction occupations between 2003 and 2009. However, this study finds that RTW laws weakened the positive effect of unionization on fatality rates in construction, suggesting that RTW laws may undermine union efforts to protect worker safety. In a more recent study, using RTW laws as an instrumental variable of unionization, Zoorob (2018) finds that for every 1% decline in unionization rates attributable to RTW laws during 1992-2016, worker fatality rates increased by 5%. By reducing unionization rates, RTW laws were estimated to have increased workplace mortality rates by 14.2% during the period of 1992-2016. The effect's magnitude largely aligns with the reduced-form estimate from a two-way fixed-effects (TWFE) model that examines the association between RTW laws and workplace fatality rates.

By using TWFE models, Zoorob (2018) represents an important effort to mitigate endogeneity in analyzing the effects of RTW laws. However, it's worth noting that the methodologies for enhancing TWFE analyses have significantly advanced in recent years since the study was published. Most importantly, TWFE models, typically controlling for location-specific (e.g., state, county) and time-specific shocks in policy evaluations, have recently been found to lead to biases in the setting of staggered policy adoptions (Baker et al. 2021; Callaway and Sant'Anna 2021; De Chaisemartin and D'Haultfoeuille 2020; Goodman-Bacon 2021), as in the case of RTW laws (see Appendix Figure A2). This is because the strict functional form assumption of TWFE models, which allows only one parameter to capture the treatment effect for all locations, implies two key substantive assumptions: “constant treatment effect” and “no carryover effect” (Imai and Kim 2019). The former means that the treatment effect is the same for each cohort receiving the treatment. The latter means that past treatments cannot affect current outcomes. Violations of these assumptions can generate biased estimates. In our case, each of these two assumptions is likely violated. First, the treatment effect may differ between states that passed RTW laws at different times. For example, Oklahoma’s sociodemographic characteristics differ substantially from those of Michigan and Indiana. As a result, Oklahoma, which passed RTW laws in 2001, may respond differently to the passage of RTW laws in comparison to Michigan and Indiana, where RTW laws were passed in 2012. Second, the adoption of RTW laws may have a lagged effect, given that there may be a time delay between their formal passage and their implementation, and a further delay until individual health responses become apparent. Therefore, it is important to estimate dynamic treatment effects instead of the Average Treatment Effect on the Treated (ATT).

In addition to the assumptions implied by their functional form, TWFE models also assume strict exogeneity. This strong assumption is often easily violated and hard to test using diagnostics (Liu et al. 2022). In our case, it is possible that there exist unobserved time-varying state characteristics that affect both the likelihood of passing RTW laws and occupational health, even if we control for a range of time-varying state characteristics. For example, if the imposition of RTW laws in a state reflected a conservative shift in public opinion which also affected occupational health, and measures of public opinion were not well captured in the data, then a TWFE model may generate biased estimates due to the violation of the strict exogeneity assumption. For similar reasons, in theory, RTW laws can impact occupational fatal injuries through various channels (e.g., wages and employment), not solely via unionization. Hence, it’s challenging to substantiate the exclusion restriction assumption (i.e., RTW laws solely influence workplace fatalities by affecting unionization rates) utilized in the instrumental variable approach as demonstrated in Zoorob (2018).

Data and Methods

Following Zoorob (2018), we construct our main dependent variable – occupational fatal injuries from the U.S. Bureau of Labor Statistics (BLS) Census of Fatal Occupational Injuries (CFOI). BLS-CFOI collects injury mortality data for each state from sources including death certificates, workers’ compensation reports, medical examiner reports, and federal and state agency administrative reports. To validate that each injury is fatal and work-related, the case must be substantiated by at least two sources. The fatal injury rates are expressed as the number of injuries per 100,000 population aged 25-64 in the private industry. Following previous studies (VanHeuvelen 2020; Zoorob 2018), we consider several time-varying state-level covariates, including the total population aged 25-64 employed in all occupations, percentage of male workers, percentage of White workers, percentage of college graduates, percentage of high school (including GED) graduates, percentage of the population living in urban areas, and an index of a state’s policy liberalism. Although policy liberalism may serve as a potential mediator between RTW laws and occupational fatal injuries, we included it in our model to facilitate a more comparable analysis with previous studies, such as Zoorob (2018), and to address its potential confounding effects. Excluding policy liberalism in Online Appendix S1 yielded results consistent with our main findings. Data sources for these covariates are documented in Online Appendix S2. We use the natural logarithm of all rates to alleviate concerns from outliers and improve the interpretability of results. Robustness checks, which further control for changes in industry compositions within each state over time, are presented in Online Appendix S3. These findings align consistently with our main results.

We construct a state-year panel dataset comprising 50 U.S. states for the years 1992-2019. 21 states had RTW laws before 1992, with most of them passing these laws before 1965. We excluded these 21 states from our analyses as they do not provide any variations to identify causal effects during our sample period. Additionally, 23 states never passed RTW laws (designated as 'non-RTW states'), while 6 states enacted RTW laws during our observed period (classified as 'RTW states'). These 6 states include Oklahoma, which enacted the law in 2001, Michigan and Indiana, which enacted the law in 2012, Wisconsin, which passed the law in 2015, West Virginia, which approved the law in 2016, and Kentucky, which passed the law in 2017.

Table 1 shows the weighted and unweighted summary statistics of all variables, separately for the 6 RTW states and 23 non-RTW states. We also conducted *t*-tests to examine whether the weighted means of the two groups of states are statistically different. Consistent with previous studies (Eren and Ozbeklik 2016), on average, non-RTW states are more populated, more liberal, and have higher percentages of male, college-educated, and urban workers compared to RTW states.

[Table 1 About Here]

To minimize the methodological concerns discussed earlier, our study revisits the topic of the effect of RTW laws on occupational fatal injuries, applying an innovative generalized synthetic control (GSC) method (Xu 2017). Intuitively, it requires estimators to treat treated observations as missing, train prediction models only with data in the untreated group (i.e., the states without RTW laws), and use the estimated models to impute 'missing' observations as counterfactuals of the treated observations. By doing so, the GSC relaxes the strict functional form assumption required by TWFE models, allowing for dynamic and heterogeneous treatment effects (Liu et al. 2022; Xu 2017). Confidence intervals are obtained following a parametric bootstrap method detailed in Liu et al. (2022) and Xu (2017). In addition, with the latent factor model framework, GSC can account for certain unobserved time-varying confounders, as long as they can be decomposed into heterogeneous impacts of a common trend (e.g., a nationwide shifting ideology towards conservatism) on different states and a series of common shocks (e.g., the Great Recession) to all states (Xu 2017). An additional advantage of the GSC approach over TWFE models is that it makes it easier to make diagnostics and visualizations for the assumptions (Gobillon and Magnac 2016; Xu 2017). Compared with the traditional synthetic control method, the GSC is more flexible to allow for more than one treatment units (Xu 2017). The GSC can be considered a specific case of the Interactive Fixed Effects Counterfactual Estimator (IFEct) when the treatment does not revert (Liu et al. 2022). However, GSC utilizes a conformal inference approach (parametric bootstrap) as implemented in the 'gsynth' package, while the inference approach for IFEct, available in the 'fect' package (whether through jackknife or nonparametric bootstrap), requires a substantially larger number of units. Given that our dataset does not feature a particularly large number of units, the GSC method is more suitable.

Though the GSC approach is not free of assumptions, its assumptions are generally considered much weaker than those of TWFE models. It assumes a correct functional form, the weaker form of strict exogeneity, certain structures on the error term, and regularity conditions (Xu 2017). The last two assumptions ensure consistency and convergence of the treatment effect estimator, as well as valid inference. We conducted our analyses using the R package 'gsynth'.

Results

The estimated dynamic treatment effects on occupational fatal injuries are shown in Figure 1. The treatment effects were statistically insignificant at the 5% level across the sample period. In Appendix Table A1, we present the ATT estimates from both the TWFE and GSC approaches, utilizing a sample from 1992-2019 and excluding the states that are always treated. Consistent with Zoorob (2018), results from the TWFE model show that RTW laws were associated with a 15% increase in occupational fatal injuries. By contrast, the GSC result shows an insignificant effect. Although the coefficients are similar, the GSC model's standard error (SE) is much larger than that of the TWFE model, primarily because the GSC accounts for unobserved unit-time-varying factors, unlike the TWFE. High correlation between RTW treatment and these factors leads to inflated SEs due to multicollinearity. Additionally, the GSC's SE is derived from a parametric bootstrap method (Xu 2017), more robust than the TWFE's classic cluster method, though this only slightly affects SE differences.

[Figure 1 About Here]

We conducted various additional analyses in the Appendix. Results for other long-run and short-run health outcomes are shown in Online Appendix S4. Heterogeneous effects by each treated state are shown in Online Appendix S5. In Online Appendix S6, we performed robustness checks using various alternative approaches for TWFE adjustments. All these additional analyses are consistent with our main findings. In Online Appendix S7, we delve further into the potential reasons behind the differing results observed between the GSC and TWFE models. In this effort, we provide additional details on our GSC findings, including the number of factors, the trajectory of these factors, and the specifics of estimated factor loadings across states. Our analysis reveals that the primary drivers of these differential results are violations of strict exogeneity in the TWFE models and variations in uncertainty estimates (e.g., SEs are larger in GSC models if latent factors are highly correlated with the treatment). In other words, TWFE models failed to account for some time-varying unobserved confounders, and previous studies were overly optimistic about the precision of TWFE estimates.

Discussion

Applying an innovative GSC approach to overcome important limitations of TWFE models, we did not find convincing support for the previous finding that RTW laws increased occupational fatal injuries, as opposed to Zoorob (2018). This finding was further confirmed by applying a wide range of alternative methods of TWFE adjustments. We also examined a wide range of long-run and short-run population health outcomes, and the findings are consistent with the finding for occupational fatal injuries.

The observed correlation in TWFE models between the passage of RTW laws and higher occupational fatality rates within a state could be attributed to time-varying, unobserved factors that influence both the enactment of RTW laws and the increased injury rates. For instance, states adopting RTW legislation may have experienced a shift towards more conservative policies around the time these laws were enacted, especially in comparison to non-RTW states. This shift could lead to the adoption of other policies and the establishment of institutions that negatively impacted population health, and the passage of RTW laws. Consequently, the causal impact of RTW laws on population health may be minimal, given the influence of other policies and institutions.

There remain several limitations in our study. First, the current study focuses on state-level changes, and therefore we are unable to uncover heterogeneity at the individual level. Future studies with individual level data are needed to further examine the heterogeneous effects on different sociodemographic groups. In particular, more granular data may provide insight into racial and socioeconomic stratification, which has been a driving force behind the development and decline of labor rights (Dixon 2007; Farhang and Katznelson 2005). Second, although the GSC approach has much weaker assumptions compared to TWFE models, it still has assumptions. For example, our analyses assume that the model is not mis-specified in terms of functional forms (Xu 2017). Finally, RTW laws may impact population health over a longer term than the timeframe assessed in this study. RTW laws have been linked to rising economic inequality over the long term (VanHeuvelen 2023), potentially leading to lasting effects on population health. The enactment of RTW laws might also lead to a wider legislative trend towards deregulation and diminished worker protections. Such a shift could profoundly influence health determinants over the long haul, affecting environmental safeguards, healthcare access, and social welfare provisions.

It's crucial to note that while our study didn't reveal causal effects of RTW laws on certain population health outcomes, this doesn't imply that RTW laws have no impact on population health. It's plausible that RTW laws might have more notable effects on certain short-term health outcomes, like employees' mental health, which were not examined in this study. For example, unions often contribute to stronger social networks and higher levels of social capital within communities. RTW laws, by undermining unions, may weaken these social structures, potentially leading to poorer mental health outcomes.

Despite the lack of direct causal evidence connecting RTW laws to population health outcomes, it's crucial to note that population health is, on average, poorer in RTW states compared to non-RTW states, as illustrated in Table 1 and Appendix Table A2. This discrepancy suggests that policymakers in RTW states need to carefully consider the potential risks and costs these laws impose on already strained health systems and escalating health budgets. Such considerations are particularly pressing when evaluating RTW laws or other policies that undermine labor organizations. For example, RTW laws might compromise workplace safety standards due to diminished bargaining power of workers. This could lead to a rise in workplace accidents and nonfatal injuries, increasing the burden on healthcare systems.

Future research should keep focusing on how changes in labor and industrial policies ‘spill over’ to health impacts and the well-being of employees (Dixon 2015), especially using rigorous research designs. Furthermore, there is an urgent need for more comprehensive research to explore the broader impacts of RTW laws in both the short and long term, especially in an environment where anti-union legislation is increasingly prevalent.

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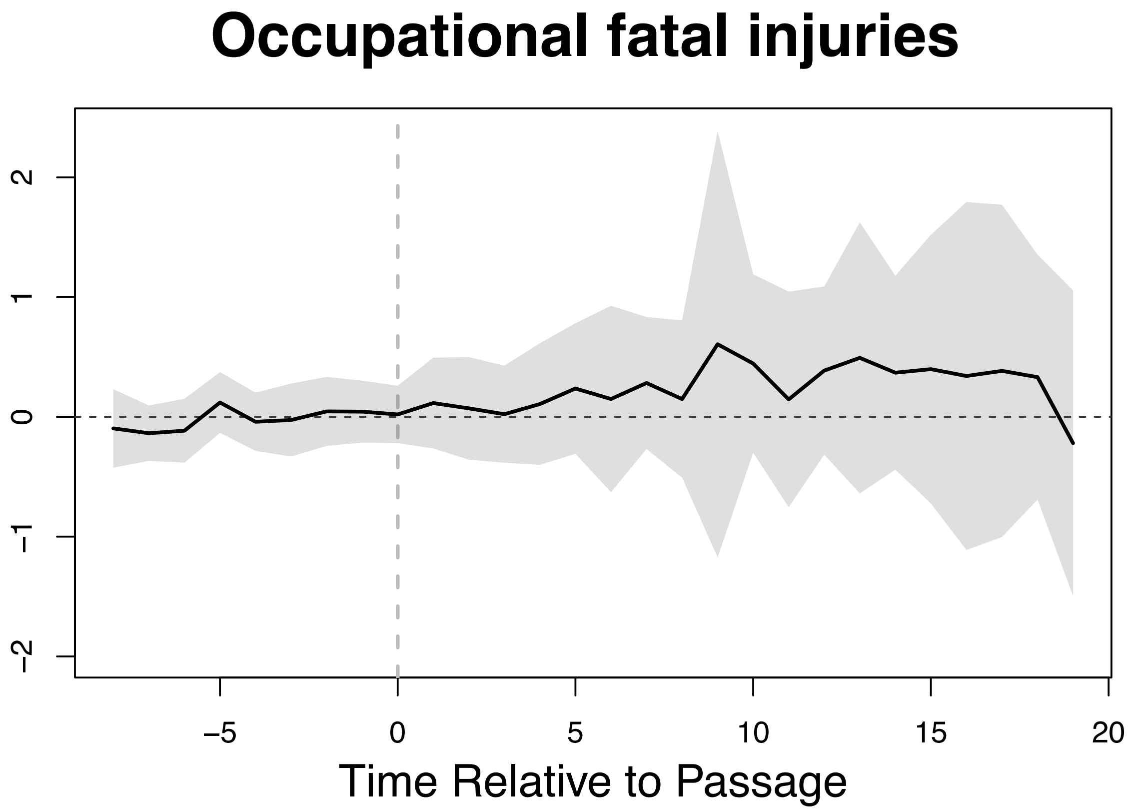
Figures

Figure 1. Dynamic Treatment Effects of Right-to-work Laws on Occupational Fatal Injuries, Generalized Synthetic Control Estimates



Note: The black solid line plots the point estimates of the dynamic treatment effects and the grey areas indicate 95% confidence intervals. Confidence intervals were estimated using the parametric bootstrap method with 1000 bootstrap runs. The results are weighted by the employed population. The 21 states that passed RTW laws before 1992 were excluded from our analysis.

Tables

Table 1. Summary Statistics

	RTW states				Non-RTW states				Difference	P-value
	Unweighted		Population-weighted		Unweighted		Population-weighted			
	Mean	SD	Mean	SD	Mean	SD	Mean	SD		
Main health outcome										
Occupational fatal injuries	1.77	0.46	1.65	0.44	1.37	0.66	1.35	0.43	0.30***	0.00
Control variables										
% male workers	0.53	0.01	0.53	0.01	0.52	0.02	0.53	0.02	-0.01***	0.00
% white workers	0.89	0.06	0.89	0.05	0.84	0.15	0.82	0.09	0.06***	0.00
% college workers	0.29	0.05	0.29	0.05	0.36	0.07	0.37	0.06	-0.07***	0.00
% high-school workers	0.93	0.02	0.93	0.02	0.93	0.03	0.92	0.03	0.02***	0.00
% urban	0.63	0.10	0.67	0.08	0.76	0.16	0.84	0.10	-0.17***	0.00
log(total population)	14.68	0.53	14.93	0.46	14.46	1.14	15.58	0.93	-0.66***	0.00
Policy liberalism index	0.32	0.63	0.20	0.63	-1.03	0.88	-1.32	1.01	1.52***	0.00

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Occupational fatal injuries are log-transformed deaths per 100,000 population. We report both unweighted and weighted statistics. The latter were weighted by state-year total population. T-tests were conducted to examine differences in weighted population means. RTW states include the 6 states that passed RTW laws during the period of 1992-2019. Non-RTW states include the 23 states that never passed RTW laws. Positive policy liberalism index values indicate conservativeness, and negative values indicate liberalism.

Appendix Tables

Appendix Table A1. Estimated Average Treatment Effects on the Treated

	(1)	(2)	(3)	(4)	(5)	(6)
	TWFE partial sample (1992-2016)	TWFE full sample (1992-2016)	GSC partial sample (1992-2016)	TWFE partial sample (1992-2019)	TWFE full sample (1992-2019)	GSC partial sample (1992-2019)
Panel A: Fatal injuries						
Occupational fatal injuries	0.162*	0.150**	0.293	0.150**	0.096	0.163
	(0.066)	(0.053)	(0.215)	(0.046)	(0.061)	(0.179)
Panel B: All-cause mortality						
All-cause mortality				-0.007	0.032	-0.026
				(0.037)	(0.034)	(0.026)
Panel C: Mortality Related to "Deaths-of-Despair"						
Drug-overdose mortality				-0.132	0.114	-0.414
				(0.144)	(0.123)	(0.290)
Alcohol-related mortality				-0.038	0.029	-0.040
				(0.039)	(0.035)	(0.056)
Suicide mortality				-0.068*	-0.022	-0.073
				(0.027)	(0.027)	(0.042)
Panel D: Short-run health outcomes						
Weighted health status 1996-2019				0.003	-0.011	-0.008
				(0.016)	(0.020)	(0.029)
Rate of quitting job or retired for health reasons				-0.001	-0.001	0.001
				(0.001)	(0.001)	(0.002)
Rate of having work disability				0.000	0.002	0.001
				(0.002)	(0.002)	(0.002)
Rate of sickness-related absence				-0.001	0.000	-0.003
				(0.002)	(0.002)	(0.005)
Observations	725	1250	725	812	1400	812
# Units	29	50	29	29	50	29

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. TWFE: Two-way fixed effects models. GSC: Generalized Synthetic Control Method. Standard errors in parentheses are clustered at the state level. The dependent variable is log-transformed. The standard errors for GSC estimates were estimated using the parametric bootstrap method with 1000 bootstrap runs. Columns (1) and (4) report the Two-way Fixed Effects (TWFE) estimates for the partial sample (with 29 states). Columns (2) and (5) report the TWFE estimates for the full sample (with 50 states). Columns (3) and (6) report the GSC estimates for the partial sample (with 29 states). The results are weighted by the employed population. The 29 units include 23 never-treated states and 6 treated states in the sample period for estimation. In columns (1)-(3), we restricted the analysis sample to the period of 1992-2016 similar as in Zoorob (2018).

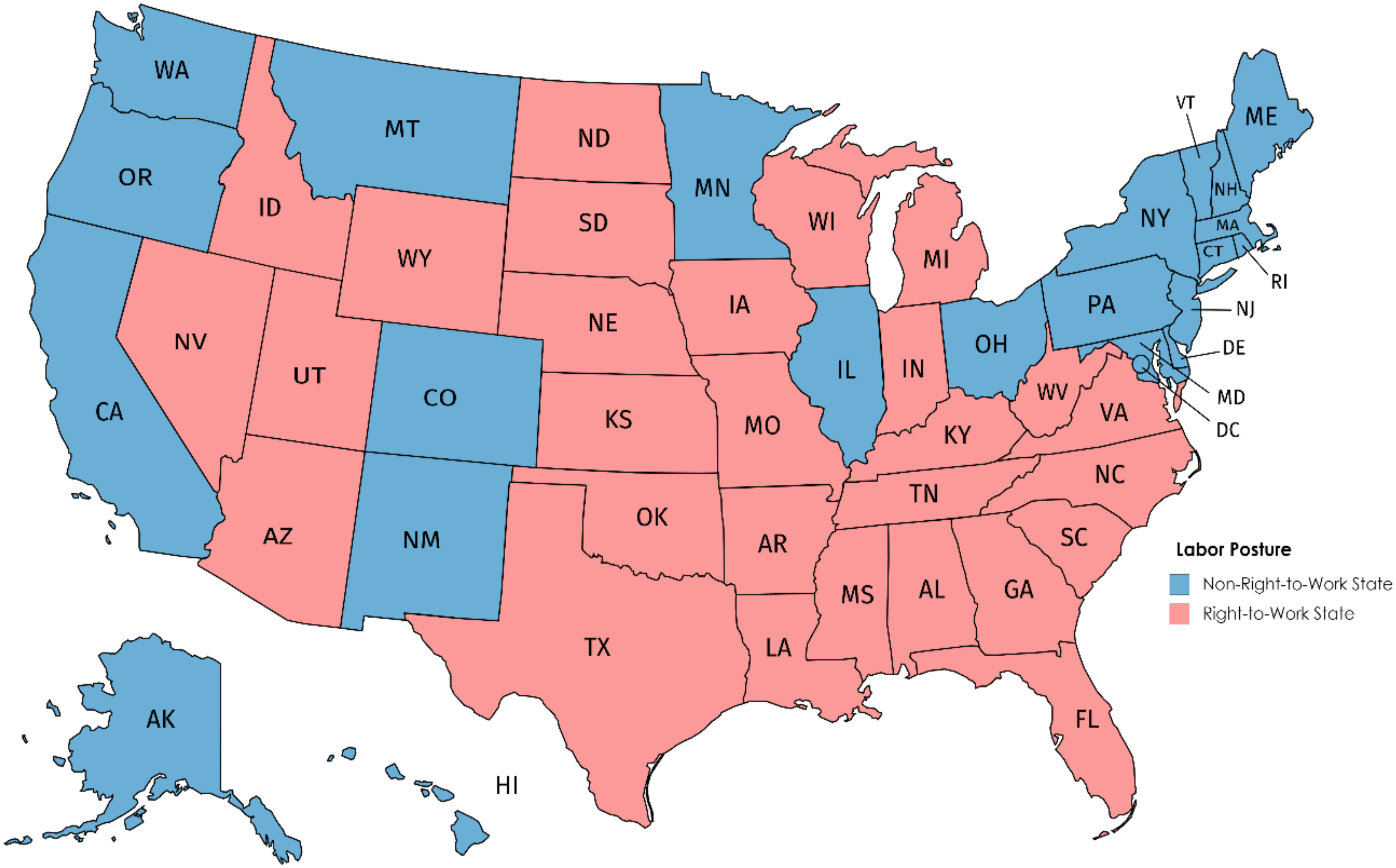
Appendix Table A2. Summary Statistics of Other Health Outcomes

	RTW states				Non-RTW states				Difference	P-value
	Unweighted		Population-weighted		Unweighted		Population-weighted			
	Mean	SD	Mean	SD	Mean	SD	Mean	SD		
Panel A: Mortality										
All-cause mortality	5.99	0.17	5.95	0.15	5.79	0.14	5.79	0.15	0.16***	0.00
Drug-overdose mortality	2.83	0.87	2.76	0.83	2.85	0.63	2.81	0.56	-0.05	0.43
Alcohol-related mortality	5.21	0.18	5.17	0.16	5.00	0.17	4.98	0.14	0.18***	0.00
Suicide	2.94	0.19	2.89	0.18	2.80	0.32	2.65	0.27	0.24***	0.00
Panel B: Short-run health outcomes										
Self-rated health 1996-2019	3.86	0.10	3.89	0.08	3.97	0.09	3.96	0.08	-0.07***	0.00
Rates of health-related job exits	0.01	0.00	0.01	0.00	0.01	0.01	0.01	0.00	0.00	0.31
Rates of work disabilities	0.03	0.01	0.03	0.01	0.03	0.01	0.02	0.01	0.00***	0.00
Rates of sickness-related absences	0.05	0.03	0.05	0.02	0.04	0.03	0.04	0.02	0.01***	0.00

Note: All the mortality variables are log-transformed deaths per 100,000 population. We report both unweighted and weighted statistics. The latter were weighted by state-year total population. T-tests were conducted to examine differences in weighted population means. RTW states include the 6 states that passed RTW laws during the period of 1992-2019. Non-RTW states include the 23 states that never passed RTW laws. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

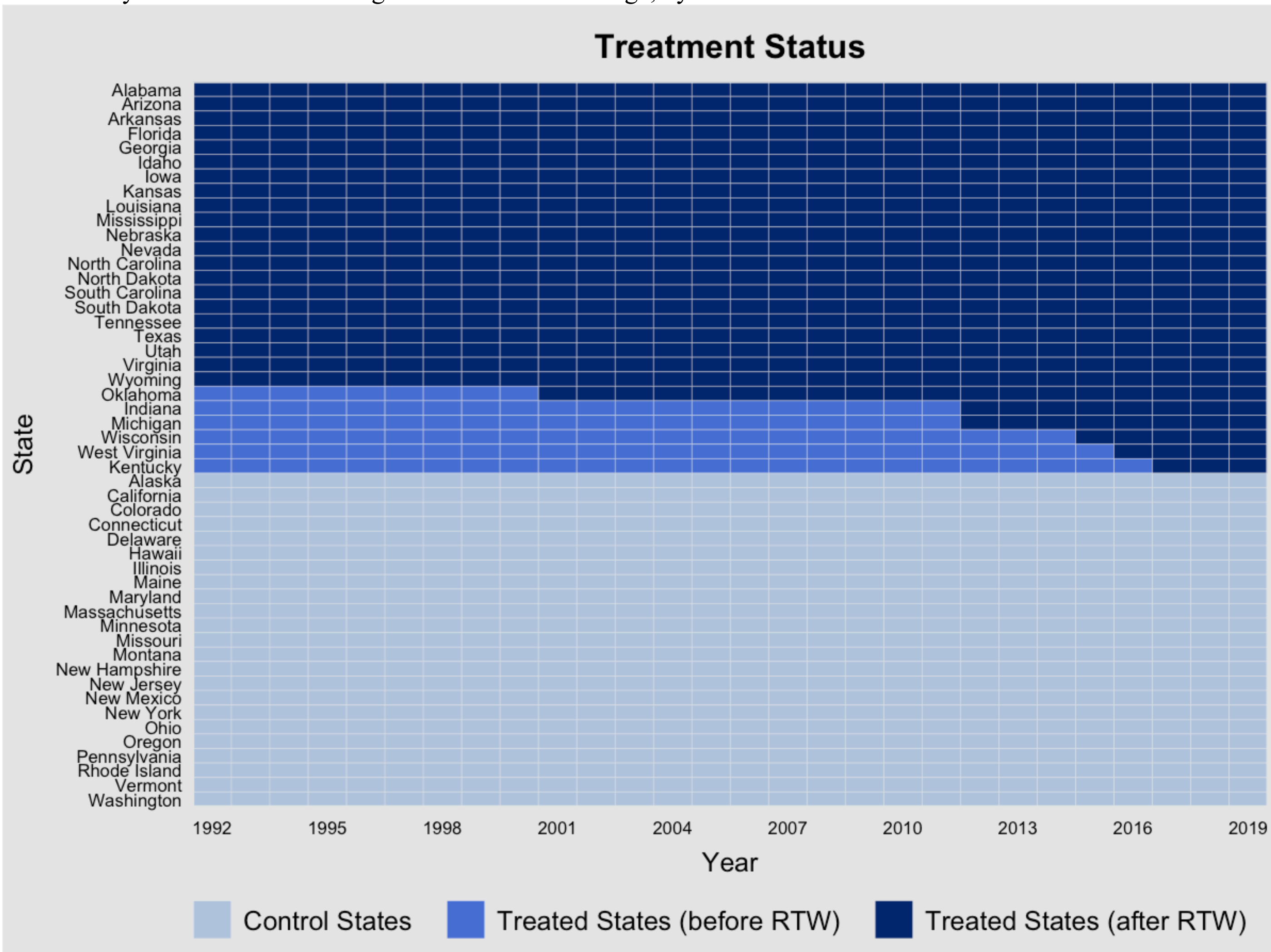
Appendix Figures

Appendix Figure A1. States with "Right to Work" Laws in 2019



Note: Data come from National Right to Work Foundation. The 23 states without Right-to-Work laws are Alaska, California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Massachusetts, Maryland, Maine, Minnesota, Missouri, Montana, New Hampshire, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, and Washington.

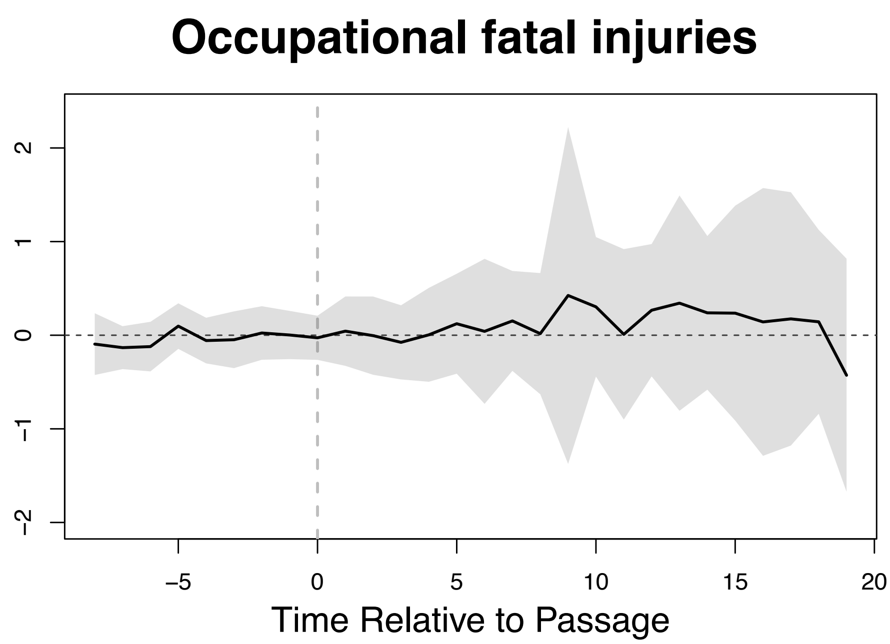
Appendix Figure A2. The Dynamic "Rollout" of Right-to-Work Law Passage, by State



Note: Data come from the National Right to Work Foundation. The sample includes 50 states. The passage of RTW legislation can be understood as an example of staggered policy adoption. In no state were RTW laws implemented and removed intermittently. We order the states based on the timing of RTW laws' passage. Those states that did not have RTW laws in place during our sample period (1992-2019) are in light blue. The state-years that have RTW laws during our sample period (1992-2019) are shown in dark blue. The years before states passed RTW laws during our sample period are in medium blue.

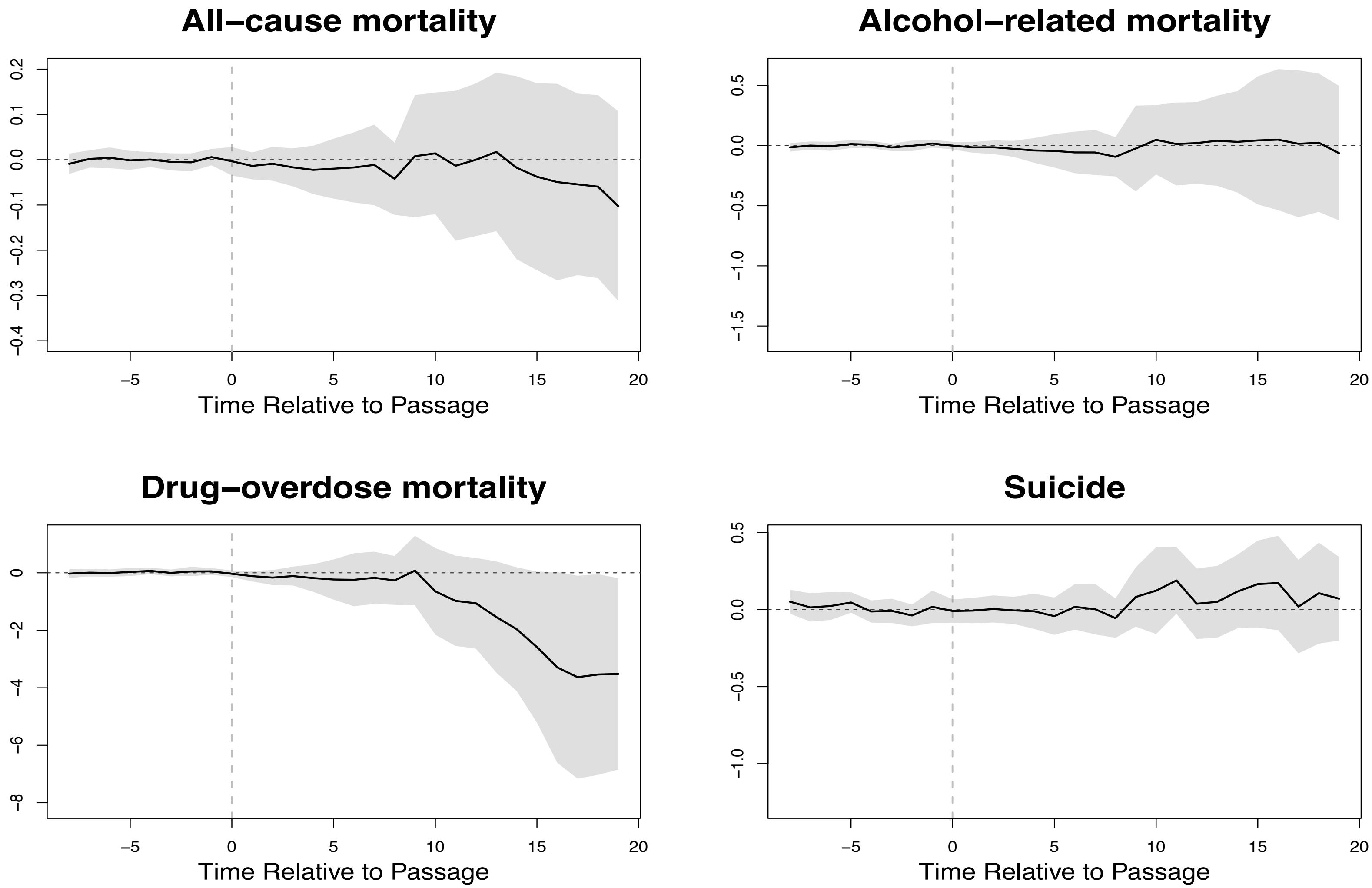
Online Appendix S1. Robustness Checks Excluding Policy Liberalism

Figure S1-1. Dynamic Treatment Effects of Right-to-work Laws on Occupational Fatal Injuries, Generalized Synthetic Control Estimates



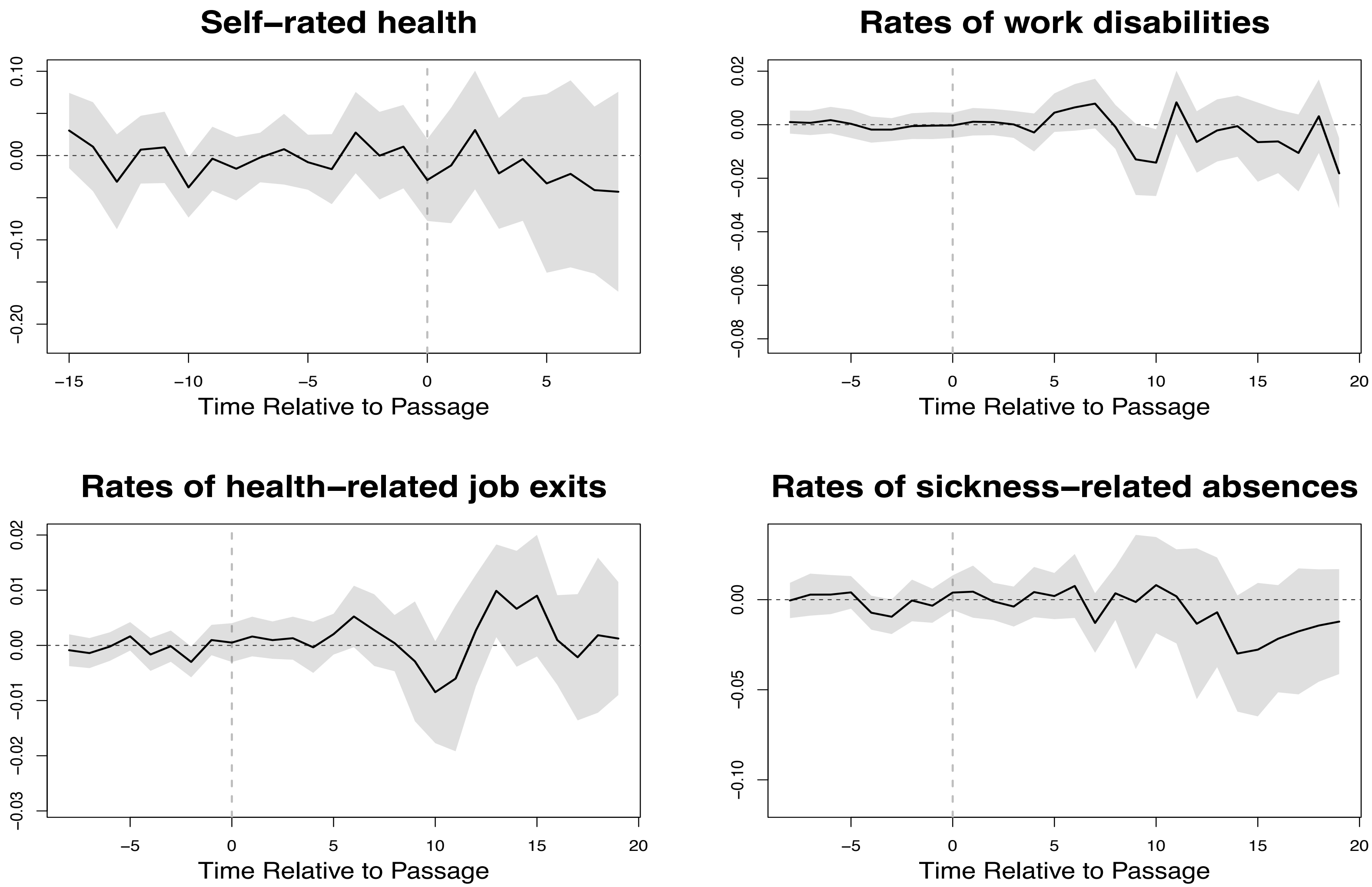
Note: The black solid line represents the point estimates of the dynamic treatment effects, while the grey areas denote the 95% confidence intervals. These intervals were estimated using the parametric bootstrap method, involving 1,000 bootstrap runs. The results have been weighted according to the employed population in each state. States that passed RTW laws before 1992, totaling 21, were excluded from our analysis. Additionally, the regression does not include the policy liberalism index.

Figure S1-2. Dynamic Treatment Effects of Right-to-work Laws on Mortality Outcomes, Generalized Synthetic Control Estimates



Note: The black solid line represents the point estimates of the dynamic treatment effects, while the grey areas denote the 95% confidence intervals. These intervals were estimated using the parametric bootstrap method, involving 1,000 bootstrap runs. The results have been weighted according to the employed population in each state. States that passed RTW laws before 1992, totaling 21, were excluded from our analysis. Additionally, the regression does not include the policy liberalism index.

Figure S1-3. Dynamic Treatment Effects of Right-to-work Laws on Other Health Outcomes, Generalized Synthetic Control Estimates



Note: The black solid line represents the point estimates of the dynamic treatment effects, while the grey areas denote the 95% confidence intervals. These intervals were estimated using the parametric bootstrap method, involving 1,000 bootstrap runs. The results have been weighted according to the employed population in each state. States that passed RTW laws before 1992, totaling 21, were excluded from our analysis. Additionally, the regression does not include the policy liberalism index.

Online Appendix S2. Data Sources

S2.1 Covariates

In this section, we detail the data sources for variable constructions. Using data of respondents who were employed full-time from the Current Population Survey (CPS) Annual Social and Economic Supplements (ASEC), we calculated the weighted estimates of the number of workers aged 25-64, and the percentages of males, Whites, college graduates, and high school graduates. The percentage of the population living in urban metropolitan areas was calculated from the Census, with inter-decennial values forward filled. The index of states' policy liberalism was obtained from Caughey and Warshaw (2016) for the years 1992-2002 (available at https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/ZXZMJB), and from the State Policy Database (https://www.statepolicyindex.com/data/) for the years 2003-2019. The index of states' policy liberalism is a dynamic measure constructed using data on more than 100 policies collected over eight decades. Details of the construction can be found in Sorens, Muedini and Ruger (2008) and Caughey and Warshaw (2016).

S2.2 Other health outcomes

As auxiliary analyses, we also examined the impact of RTW laws on a wide range of other health outcomes, including relatively long-run outcomes such as all-cause mortality, mortality related to "deaths of despair" caused by suicide, alcohol, or drug overdose (Case and Deaton 2015), and short-run outcomes such as self-rated health, rates of health-related job exits (i.e., quitting jobs or retiring for health reasons), rates of work disabilities, and rates of sickness-related absences. In Appendix Table A2, we show the summary statistics of all these outcomes.

For long-run health outcomes, we construct workers' age-adjusted mortality, for full-time employed workers between the ages of 25 and 64, in 1992-2019. Death counts in each year and state were obtained from the Centers for Disease Control and Prevention (CDC) multiple cause mortality database. To calculate annual mortality rates (deaths per 100,000 population), we obtained state population counts from the National Cancer Institute Surveillance, Epidemiology and End Results Registry (SEER). We then constructed age-adjusted mortality rates using the standard US population weights from the 2000 Decennial Census. Mortality related to "deaths of despair" is defined as relating to drug overdose, suicide, and alcohol-related diseases. Same as occupational fatal injuries, for all the mortality outcomes, we used the natural logarithm of all rates for main analysis.

We use the standard codes to specify causes of death based on the categorization in the NCHS reports. For the years 1990-1998, we identified suicide related death using the International Classification of Diseases, Ninth Revision (ICD-9) underlying cause-of-death codes E950–E959 (Murphy 2000). For years 1999-2016, we identified deaths caused by suicide using International Classification of Diseases, Tenth Revision (ICD-10) underlying cause-of-death codes U03, X60–X84, Y87.0 (Murphy et al. 2017).

For years 1990-1998, drug-poisoning related deaths are identified using the International Classification of Diseases, Ninth Revision (ICD-9) underlying cause-of-death codes E850–E858, E950.0–E950.5, E962.0, or E980.0–E980.5 (Warner et al. 2011). For years 1999-2016, drug poisoning related deaths are identified using International Classification of Diseases, Tenth Revision (ICD-10) underlying cause-of-death codes X40–X44, X60–X64, X85, and Y10–Y14 (Warner et al. 2011).

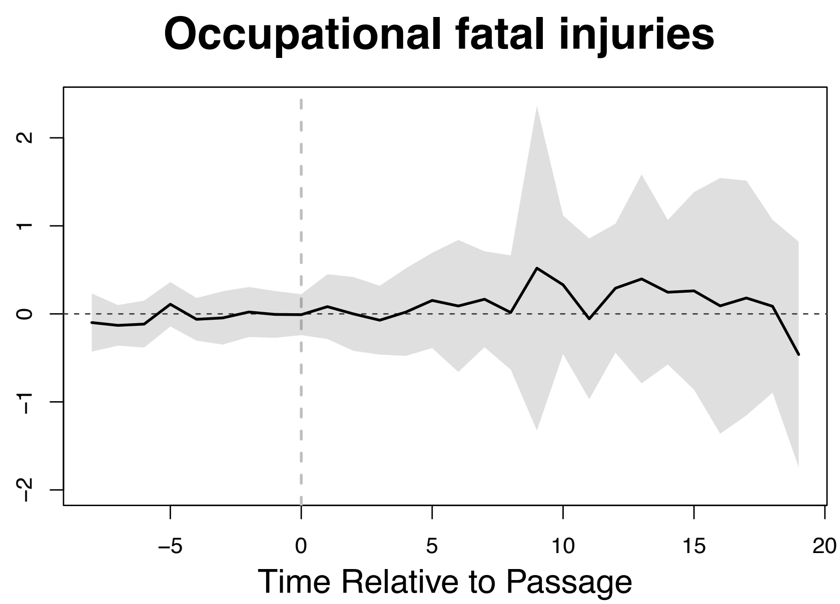
The alcohol-related death counts include deaths related to both chronic causes and acute causes. According to the Alcohol-Related Disease Impact (ARDI) criteria developed by the U.S. Centers for Disease Control and Prevention, for years 1990-1998, alcohol-related deaths are identified using the International Classification of Diseases, Ninth Revision (ICD-9) underlying cause-of-death codes 291, 305.0, 303.0, 303.9, 357.5, 425.5, 535.3, 571.0–571.3, 655.4, 760.71, 577.0, 577.1, 345, 456.0–456.2, 530.7, 571.5–571.9, 572.3, 634, 174, 574, 571.4, 150, 401-405, 410-414, 161, 155, 656.5, 764, 765, 141, 143–146, 148, 149, 696.1, 427.0, 427.2, 427.3, 433-435, 437, 362.34, 430-432, 185, 980.0, 980.1, E860.0-E860.2, E860.9, 790.3, E840-E845, E911, E960-E968, E910, E880-E888, E848, E890-E899, E922, E960-E969, E901, E820-E825, E810-E819, E917-E920, E800-E807, E826-E829, E850-E869, E924.1, E950-E959, E830-E838 (US Centers for Disease Control and Prevention). For years 1999-2014, alcohol-related deaths are identified using International Classification of Diseases, Tenth Revision (ICD-10) underlying cause-of-death codes F10.0-F10.9, G62.1, G31.2, G72.1, I42.6, K29.2, K70-K70.4, K70.9, Q86.0, P04.3, O35.4, K86.0, K85, K86.1, G40, G41, I85, I98.2, K22.6, K74.3-K74.6, K76.0, K76.9, K76.6, O03, C50, K80, K73, C15, I10-I15, I20-I25, C32, C22, O36.5, O36.4, P05, P07, C01-C06, C09-C10, C12-C14, L40.0-L40.4, L40.8, L40.9, I47.1, I47.9, I48, G45, I65-I67, I69.3, I60-I63, I69.0-I69.2, C61, X45, Y15, T51.0, T51.1, T51.9, X65, R78.0, V95-V97, W78-W79, X85-Y09, Y87.1, W65-W74, W00-W19, X00-X09, W32-W34, X85-Y09, Y87.1, X31, V02.0, V03.0, V04.0, V09.0, V12-V14(.0-.2), V19.0-V19.3, V20-V28(.0-.2), V29.0-V29.3, V30-V39(.0-.3),V40-V49(.0-.3), V50-V59(.0-.3), V60-V69(.0-.3), V70-V79(.0-.3), V81.0, V82.0, V83-V86(.4-.9), V88.0-V88.8, V89.0, V02(.1, .9), V03(.1, .9), V04(.1, .9), V09.2, V12-V14(.3-.9), V19.4-V19.6, V20-V28(.3-.9), V29.4-V29.9, V30-V39(.4-.9), V40-V49(.4-.9), V50-V59(.4-.9), V60-V69(.4-.9), V70-V79(.4-.9), V80.3-V80.5, V81.1, V82.1, V83-V86(.0-.3), V87.0-V87.8, V89.2, W24-W31, W45, V01, V05-V06, V09.1, V09.3, V09.9, V10-V11, V15-V18, V19.3, V19.8-V19.9, V80.0-V80.2, V80.6-V80.9, V81.2-V81.9, V82.2-V82.9, V87.9, V88.9, V89.1, V89.3, V89.9, X40-X49 (except X45), X60-X84, (except X65), Y87.0, V90-V94 (US Centers for Disease Control and Prevention).

To complement with long-run mortality outcomes, we use data from the Current Population Survey (CPS) Annual Social and Economic Supplements (ASEC) to construct the following short-run health outcomes: self-rated health status,[1] rate of quitting job or retired for health reasons, and rate of having work disability. All CPS measures are population weighted. We also constructed state rate of worker sickness related absence of prior week per 100,000 population using CPS monthly data.

[1] It is an ordinal measure ranging from 1 "poor" to 5 "excellent." The pre-1996 data on self-rated health is not available in the CPS.

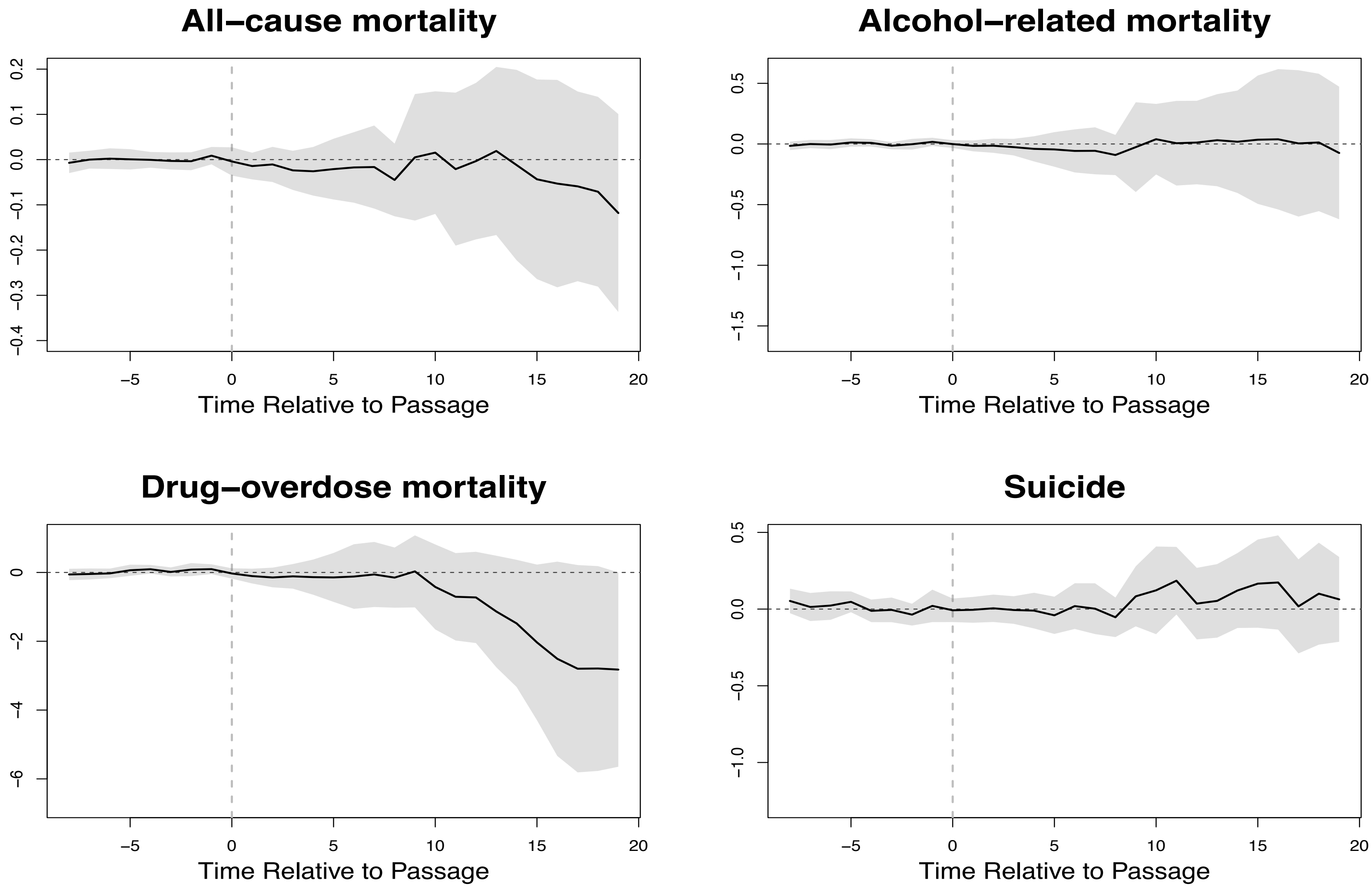
Online Appendix S3. Robustness Checks Controlling for Changes in Industry Compositions

Figure S3-1. Dynamic Treatment Effects of Right-to-work Laws on Occupational Fatal Injuries, Generalized Synthetic Control Estimates



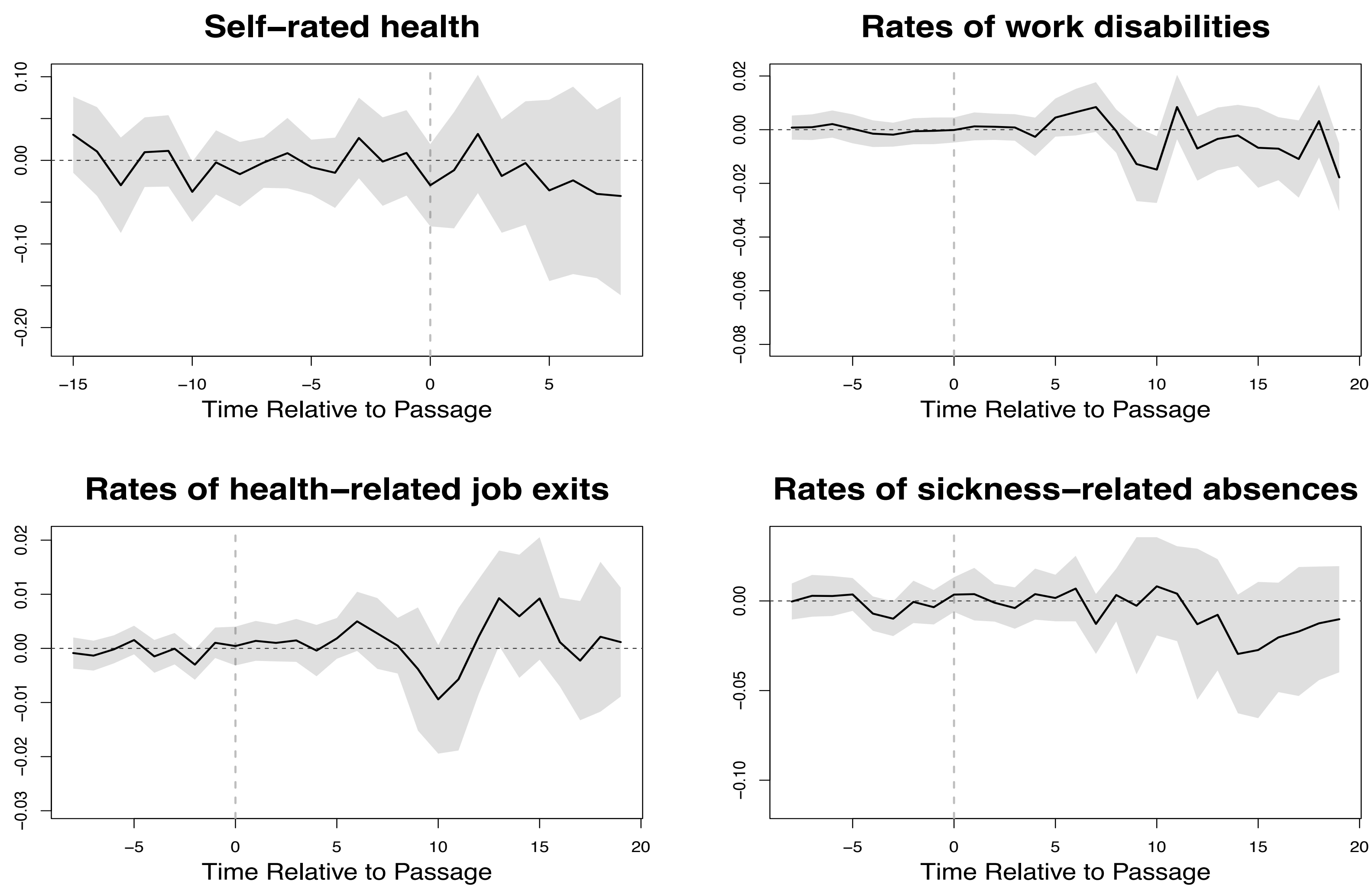
Note: The black solid line represents the point estimates of the dynamic treatment effects, while the grey areas denote the 95% confidence intervals. These intervals were estimated using the parametric bootstrap method, involving 1,000 bootstrap runs. The results have been weighted according to the employed population in each state. States that passed RTW laws before 1992, totaling 21, were excluded from our analysis. We adjusted for state-level, time-varying variables capturing shifts in industry compositions, following Zoorob (2018): 1) trade, transport, utilities percentage, 2) mining, logging, construction percentage, and 3) manufacturing percentage.

Figure S3-2. Dynamic Treatment Effects of Right-to-work Laws on Mortality Outcomes, Generalized Synthetic Control Estimates



Note: The black solid line represents the point estimates of the dynamic treatment effects, while the grey areas denote the 95% confidence intervals. These intervals were estimated using the parametric bootstrap method, involving 1,000 bootstrap runs. The results have been weighted according to the employed population in each state. States that passed RTW laws before 1992, totaling 21, were excluded from our analysis. We adjusted for state-level, time-varying variables capturing shifts in industry compositions, following Zoorob (2018): 1) trade, transport, utilities percentage, 2) mining, logging, construction percentage, and 3) manufacturing percentage.

Figure S3-3. Dynamic Treatment Effects of Right-to-work Laws on Other Health Outcomes, Generalized Synthetic Control Estimates



Note: The black solid line represents the point estimates of the dynamic treatment effects, while the grey areas denote the 95% confidence intervals. These intervals were estimated using the parametric bootstrap method, involving 1,000 bootstrap runs. The results have been weighted according to the employed population in each state. States that passed RTW laws before 1992, totaling 21, were excluded from our analysis. We adjusted for state-level, time-varying variables capturing shifts in industry compositions, following Zoorob (2018): 1) trade, transport, utilities percentage, 2) mining, logging, construction percentage, and 3) manufacturing percentage.

References

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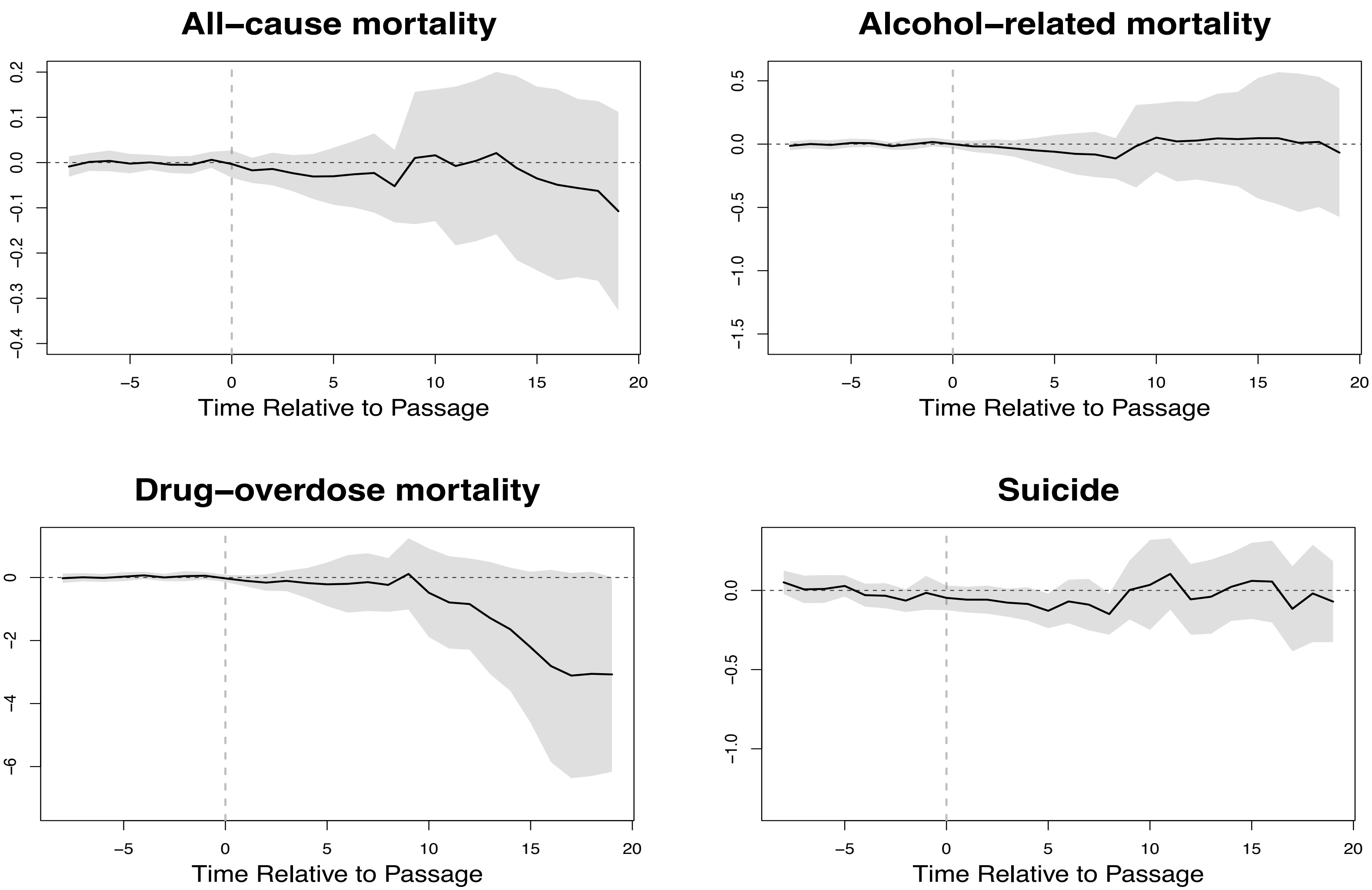
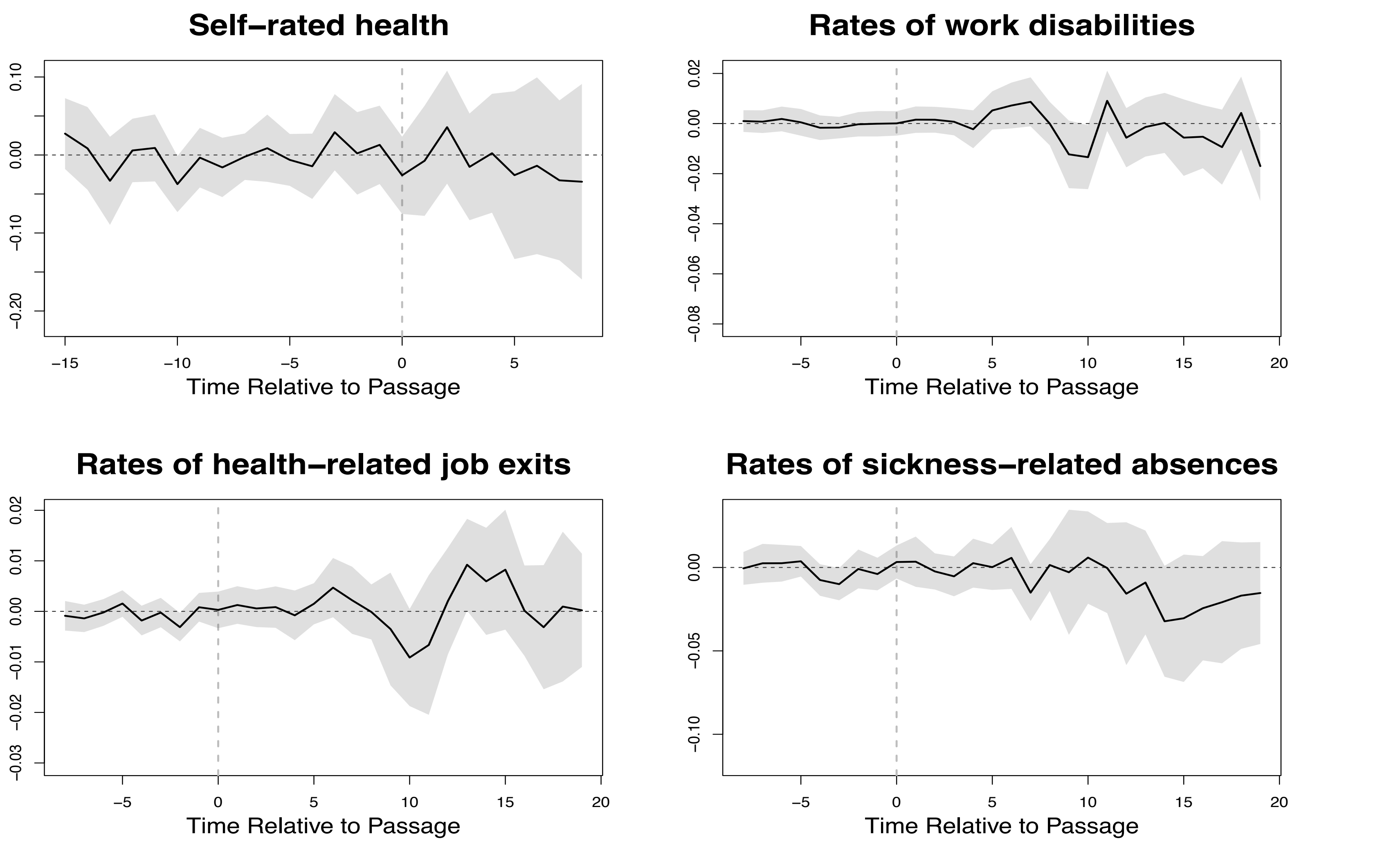
Online Appendix S4. Long-run and Short-run Health Outcomes

Figure S4-1. Dynamic Treatment Effects on Mortality Outcomes from Generalized Synthetic Control Estimates



Note: The black solid line represents the point estimates of the dynamic treatment effects, while the grey areas denote the 95% confidence intervals. These intervals were estimated using the parametric bootstrap method, involving 1,000 bootstrap runs. The results have been weighted according to the employed population in each state. States that passed RTW laws before 1992, totaling 21, were excluded from our analysis.

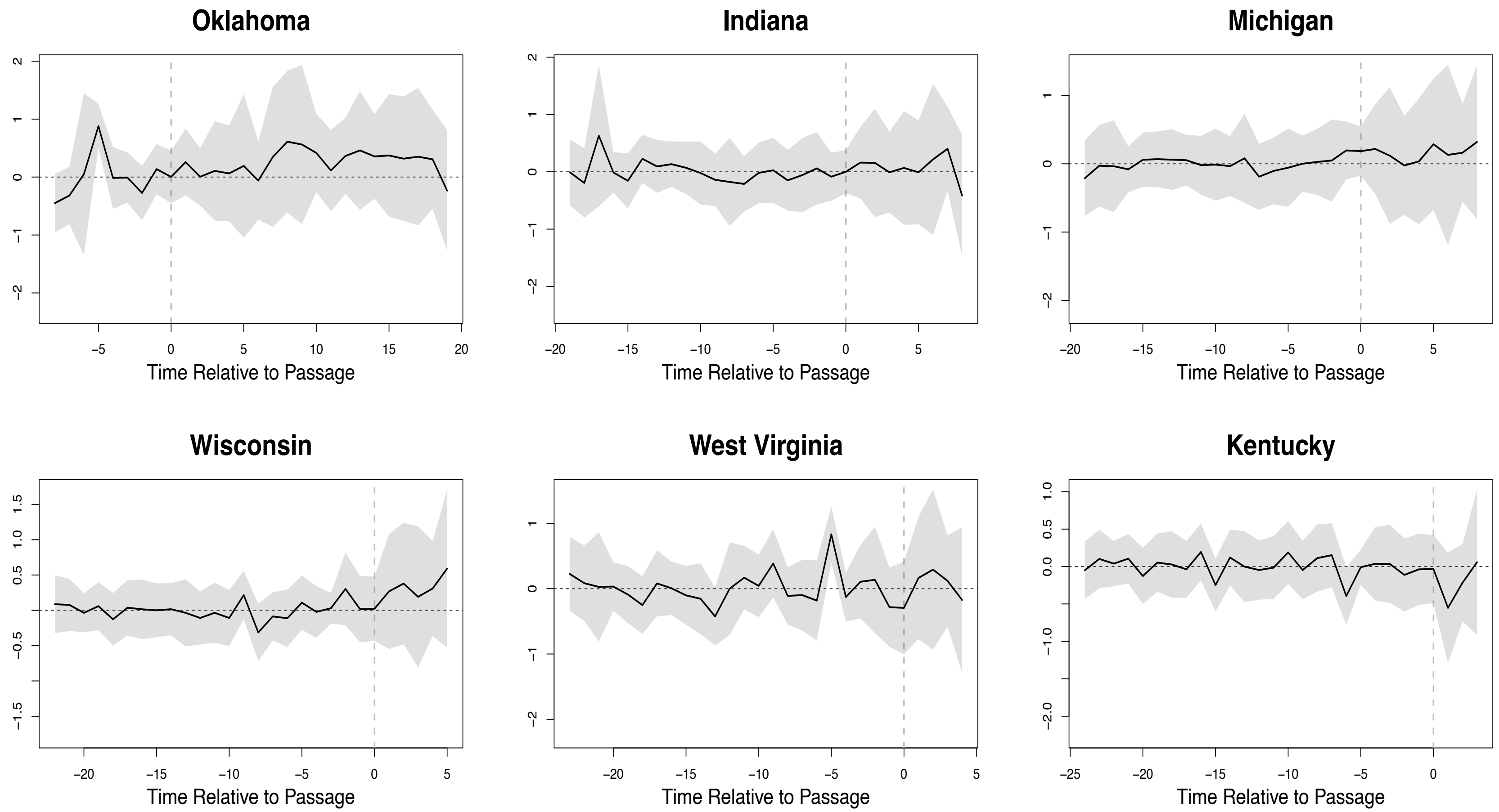
Figure S4-2. Dynamic Treatment Effects on Other Short-run Health Outcomes from Generalized Synthetic Control Estimates



Note: The black solid line represents the point estimates of the dynamic treatment effects, while the grey areas denote the 95% confidence intervals. These intervals were estimated using the parametric bootstrap method, involving 1,000 bootstrap runs. The results have been weighted according to the employed population in each state. States that passed RTW laws before 1992, totaling 21, were excluded from our analysis.

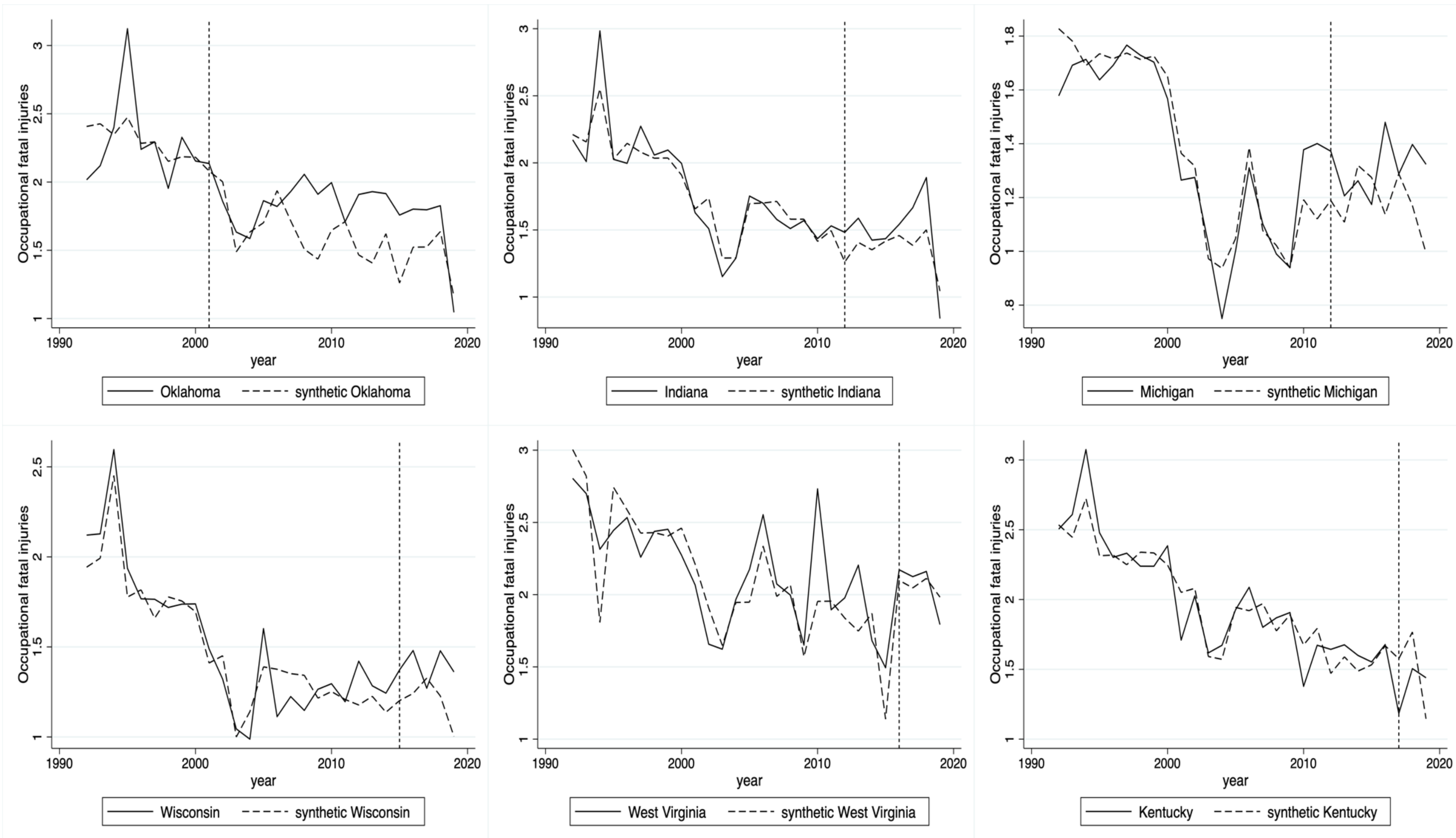
Online Appendix S5. Heterogeneous Effects by Treated State

Figure S5-1. Dynamic Treatment Effects on Occupational Fatal Injuries from Generalized Synthetic Control Estimates, by Treated State



Note: The black solid line represents the point estimates of the dynamic treatment effects, while the grey areas denote the 95% confidence intervals. These intervals were estimated using the parametric bootstrap method, involving 1,000 bootstrap runs. The results have been weighted according to the employed population in each state. States that passed RTW laws before 1992, totaling 21, were excluded from our analysis.

Figure S5-2. Dynamic Treatment Effects on Occupational Fatal Injuries from the Synthetic Control Method, by Treated State



Note: The black solid line represents the dynamic pattern of occupational fatal injuries for the observed state, while the black dashed line depicts the dynamic pattern for the synthetic state, as obtained through the original synthetic control method (Abadie et al. 2010, 2015). The results have been weighted according to the employed population in each state. States that passed RTW laws before 1992, totaling 21, were excluded from our analysis.

References

Abadie, Alberto, Alexis Diamond and Jens Hainmueller. 2010. "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program." *Journal of the American Statistical Association* 105(490):493-505. doi: 10.1198/jasa.2009.ap08746.

Abadie, Alberto, Alexis Diamond and Jens Hainmueller. 2015. "Comparative Politics and the Synthetic Control Method." *American Journal of Political Science* 59(2):495-510. doi: https://doi.org/10.1111/ajps.12116.

Online Appendix S6. Comparison of Results from Alternative Methods

In this appendix, we compare the results of Right-to-Work (RTW) laws and occupational fatal injuries using a comprehensive range of alternative methods for Two-Way Fixed Effects (TWFE) adjustments.

First, we examine the Bayesian causal panel analysis method developed by Pang et al. (2022). Compared to the synthetic control method or the latent factor models including the GSC approach, this method can estimate the uncertainty of the coefficients more accurately and correct biases caused by the potential correlation between treatment timing and time-varying latent factors (Pang et al. 2022). However, it is more computationally intense. Dynamic results using the R package "bpCausal" (Pang et al. 2022) are shown in Online Appendix Figure S6-1. Unlike the TWFE approach, which indicates positive effects, the Bayesian causal panel analysis approach yields null findings.

Second, we examine a wide range of recent advancements in DID estimators for staggered timing, as summarized by Roth et al. (2023), which introduce relaxations to certain aspects of the traditional DID framework. Specifically, our analysis concentrates on the methods developed by Borusyak et al. (2021), Callaway and Sant'Anna (2021), De Chaisemartin and D'Haultfoeuille (2020), Sun and Abraham (2021), Gardner (2022), and Cengiz et al. (2019). Detailed descriptions and comparisons of these methods are available in Roth et al. (2023). In general, these methods can help address the issues with negative weights in the setting of staggered policy adoptions when applying TWFE (Baker et al. 2021; Callaway and Sant'Anna 2021; De Chaisemartin and D'Haultfoeuille 2020; Goodman-Bacon 2021), with each method necessitating distinct identifying assumptions. For all these methods, including time-varying covariates requires us to make relatively strong assumptions (Liu et al. 2022). In addition, for the approaches introduced by Borusyak et al. (2021) and De Chaisemartin and D'Haultfoeuille (2020), coefficient estimation is confined to the last five years before the treatment to prevent an excessive inflation of the standard errors (Borusyak et al. 2021, p. 24). For consistency across methods, we only keep the last five years before the treatment, and we do not include time-varying covariates. As pointed out by Liu et al. (2022), GSC offers more flexibility in including time-varying covariates, eliminates the need to discard data, and can account for decomposable time-varying confounders compared to these methods. Results on the average treatment effects on the treated are shown in Online Appendix Table S6-1, and dynamic results are shown in Figure S6-2. With the exception of Cengiz et al. (2019), which yields results similar to those from the TWFE models, all other alternative methods consistently produce null results.

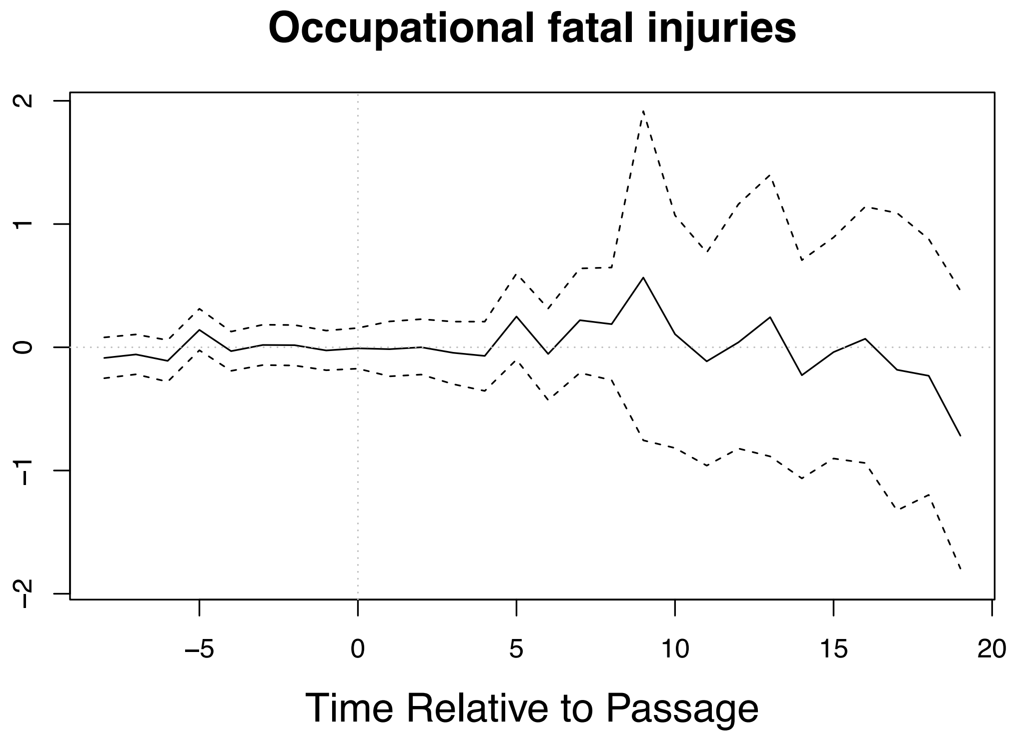
Finally, we applied the original synthetic control method developed in Abadie et al. (2010) and Abadie et al. (2015) for each treated state in Online Appendix S5. All findings consistently show no significant effects.

Table S6-1. Comparison with Alternative Methods, Effects of Right-to-Work Laws on Occupational Fatal Injuries

	Point Estimate of ATT	Standard Error	Lower Bound 90% Confidence Interval	Upper Bound 90% Confidence Interval
Borusyak et al. (2021)	0.070	0.100	-0.095	0.235
Callaway and Sant'Anna (2021)	0.006	0.051	-0.078	0.090
De Chaisemartin and D'Haultfeuille (2020)	0.017	0.064	-0.088	0.122
Sun and Abraham (2021)	0.101	0.106	-0.073	0.275
Gardner (2021)	0.096	0.105	-0.077	0.269
Cengiz et al. (2019)	0.143	0.062	0.041	0.245
GSC (Xu, 2017)	0.132	0.149	-0.113	0.377
TWFE OLS	0.150	0.083	0.013	0.287

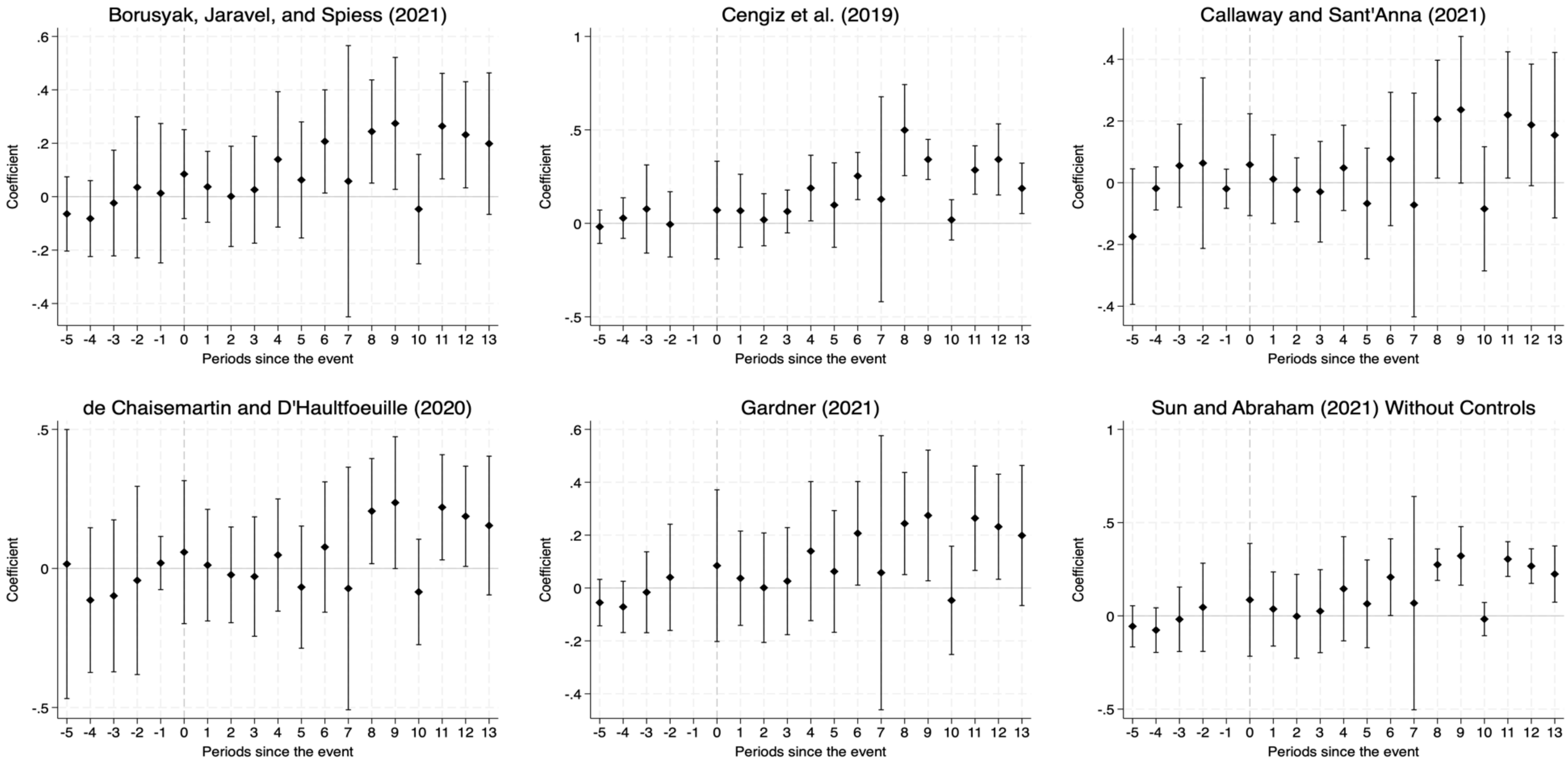
Note: ATT: Average Treatment Effects on the Treated. Standard errors are clustered at the state level. The results are weighted by the employed population. The 21 states that passed Right-to-Work laws before 1992 were excluded from our analysis. We only keep the last five years before the treatment, and we do not include time-varying covariates.

Figure S6-1. Dynamic Treatment Effects on Occupational Fatal Injuries Using Bayesian Causal Panel Analysis



Note: The black solid line plots the point estimates of the dynamic treatment effects and the dash line indicates the 95% confidence intervals. These estimates are obtained from the Bayesian Causal Panel Analysis method. The 21 states that passed Right-to-Work laws before 1992 were excluded from our analysis.

Figure S6-2. Effects of Right-to-Work Law Passage on Occupational Fatal Injuries, Using Alternative Methods, Without Controls



Note: The bars represent 95% confidence intervals. Standard errors are clustered at the state level. The results are weighted by the employed population. The 21 states that passed Right-to-Work laws before 1992 were excluded from our analysis. We only keep the last five years before the treatment, and we do not include time-varying covariates.

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Online Appendix S7. Reasons Behind the Differing Results of TWFE and GSC Models

In this Appendix, we delve deeper into the reasons for the differing results between the TWFE and GSC models. Specifically, our goal is to ascertain whether the discrepancies stem from (a) weighting issues; (b) violations of strict exogeneity; or (c) variations in uncertainty estimates. According to a recent review by Chiu et al. (2023), (b) and (c) are identified as more significant concerns than (a) in empirical research.

In Figure S7-1, we compared three approaches: 1) TWFE dynamic specification, 2) a DID approach -- e.g., by setting the number of factors to 0 in GSC, and 3) GSC with cross-validated factors. After comparing the results from the three approaches, we found that the DID and GSC approaches consistently yield null findings, whereas the TWFE approach indicates positive effects. It is noteworthy that the estimates from the three approaches tend to be highly correlated and share the same direction. Despite some notable differences in individual coefficients (e.g., at t = 10), the TWFE approach does not seem to systematically under- or over-estimate the effect. Hence, we lack substantial evidence to suggest that issues with weighting significantly contributed to the discrepancies in results. From this figure alone, it's difficult to determine definitively whether violations of strict exogeneity are responsible for the observed differences in results, given that the discrepancies between the DID and GSC models are relatively minor. Although the directions of the point estimates across the three approaches are often in agreement, the TWFE approach demonstrates the smallest estimated uncertainty. These findings suggest that variations in uncertainty estimates play an important role in the discrepancies observed between the GSC and TWFE approaches.

To further explore the role of violations of strict exogeneity, we explored more details of the GSC results, including the number of estimated latent factors (see Table S7-1), the trajectory of factors (Figure S7-2), and details of estimated factor loadings across states (Figure S7-3). As shown in Table S7-1, for our primary outcome, occupational fatal injuries, the number of factors is 1. This suggests that a violation of the strict exogeneity assumption may have contributed to the differing results between the GSC and TWFE methods, in addition to previous studies' overconfidence in their precision. Table S7-1 also presents the number of factors for other health outcomes, revealing considerable variation across these outcomes. For instance, the analysis identified three latent factors for drug-overdose mortality, suggesting the presence of potential unobserved confounders. In contrast, the estimate for suicide indicated zero latent factors, implying minimal influence from unobserved confounders.

Figure S7-2 shows the trajectory of the estimated factor over time for the analysis of occupational fatal injuries. There is considerable variation in the trajectory of the estimated factor over time (SD=0.203, Max=0.425, Min=-0.253). Figure S7-3 also illustrates significant variation in the estimated factor loadings across states (SD=0.925, Max=1.455, Min=-2.722). These observations underscore the importance of including the factor term, demonstrating that GSC estimates offer advantages in accounting for unobserved confounders compared to the TWFE method.

We have additionally conducted TWFE using the full sample, including states that enacted RTW laws before 1992. The results have been included in Appendix Table A1. Utilizing the full sample resulted in a decreased coefficient magnitude and an increased standard error compared to the partial sample results. Consequently, no significant findings were observed at the 10% significance level with the full sample. Therefore, excluding states that are always under treatment does lead to different point estimates; however, the substantive result (i.e., a null result) remains the same as that of the GSC. We acknowledge that our results, obtained using the full sample and TWFE, differ from those reported in Zoorob (2018). Our analysis reveals that this discrepancy stems from variations in the length of the post-treatment period. Zoorob (2018) analyzed data from 1992-2016, while we extended our investigation to include data up to 2019. Thus, in Appendix Table A1, we replicated our analyses using data from 1992-2016, and the results from GSC still indicate insignificant findings.

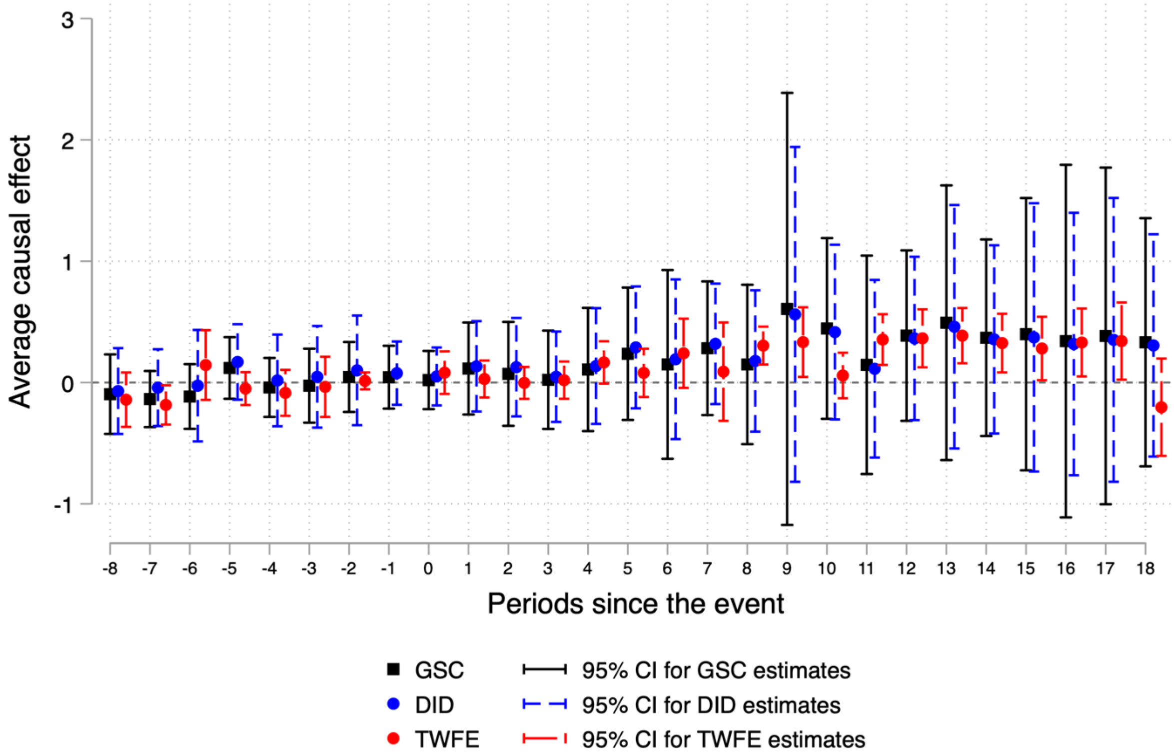
Overall, our findings indicate that inference might not be the only influencing factor, and a violation of strict exogeneity could also play a role in the observed differences between the TWFE and GSC methods.

Table S7-1. Number of Estimated Latent Factors from GSC for Each Outcome

	Number of estimated factors
Panel A: Fatal injuries	
Occupational fatal injuries	1
Panel B: Mortality outcomes	
All-cause mortality	1
Drug-overdose mortality	3
Alcohol-related mortality	2
Suicide	0
Panel C: Short-run health outcomes	
Self-rated health 1996-2019	0
Rates of health-related job exits	2
Rates of work disabilities	0
Rates of sickness-related absences	1

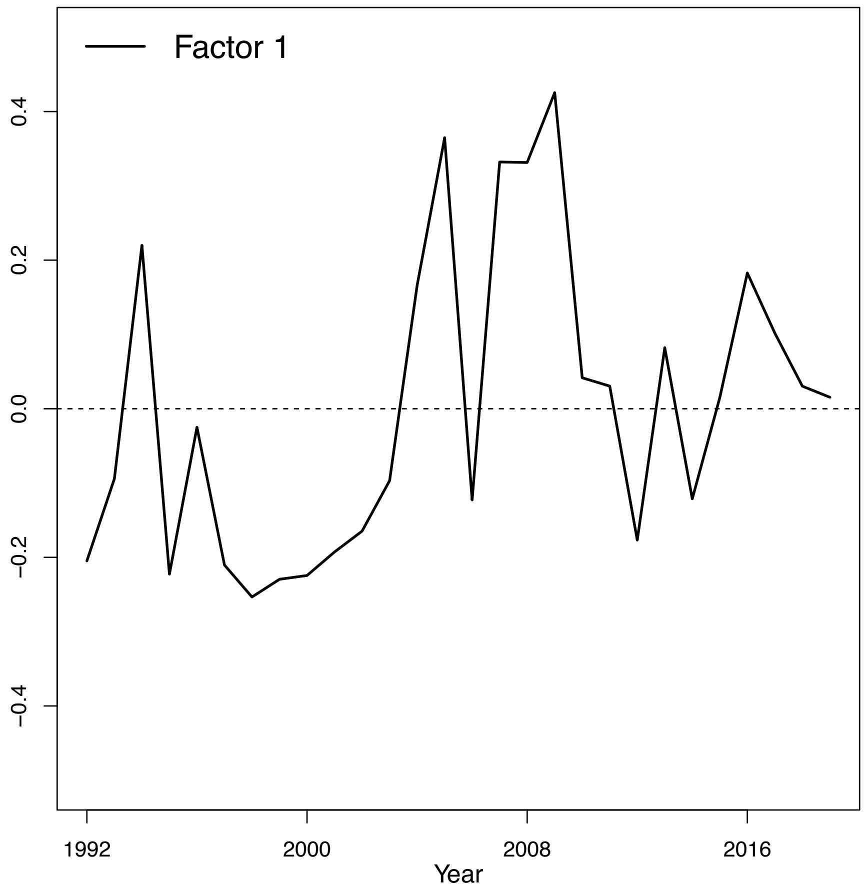
Note: This table reports the number of estimated factors using the GSC approach for each health outcome.

Figure S7-1. Comparisons between the GSC, DID, and TWFE Approaches



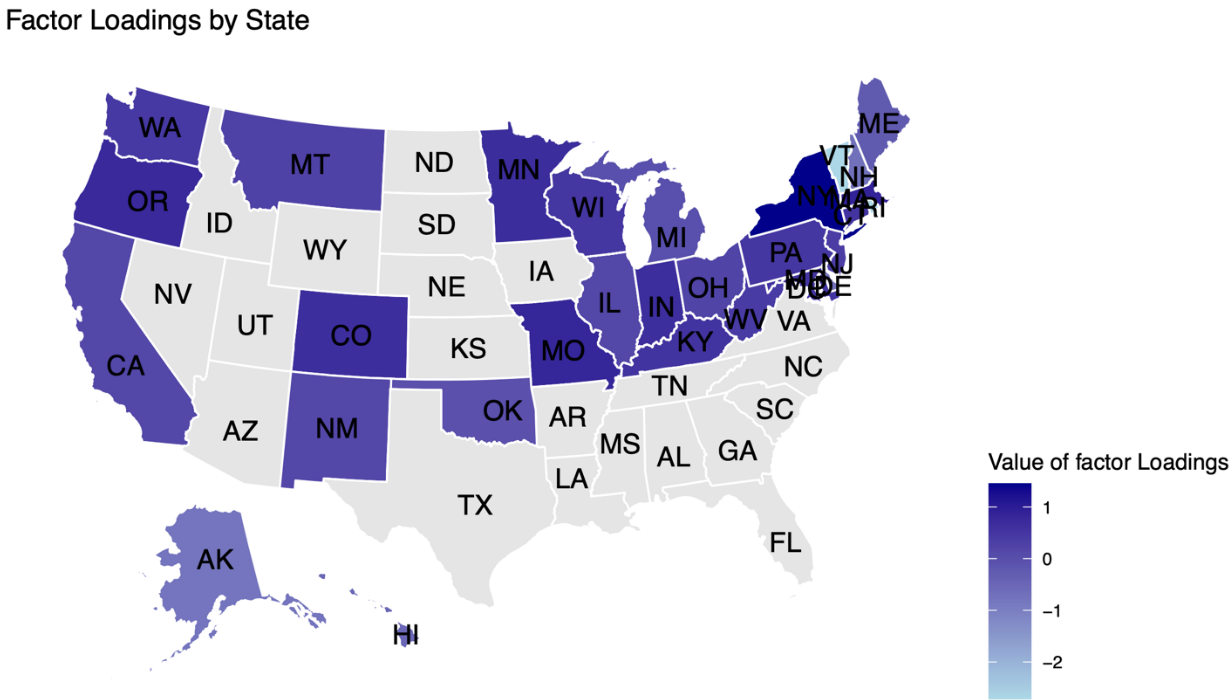
Note: The black solid line represents the point estimates of the dynamic treatment effects using the Generalized Synthetic Control Method (GSC) with cross-validated factors. The blue dashed line illustrates the point estimates from the Difference-in-Differences (DID) approach, while the red long-dashed line depicts the estimates from a Two-Way Fixed Effects (TWFE) dynamic specification. These results are weighted according to the employed population. We excluded the 21 states that enacted Right-to-Work laws before 1992 from our analysis.

Figure S7-2. The Effect of Right-to-Work Laws on Occupational Fatal Injuries: Trajectory of the Latent Factor



Note: The figure illustrates the trajectory of the estimated latent factor over various years, derived from analyzing the impact of Right-to-Work laws on occupational fatal injuries using the GSC approach.

Figure S7-3. The Effect of Right-to-Work Laws on Occupational Fatal Injuries: Factor Loadings Across States



Note: The figure displays the heterogeneity in factor loadings across different states, including the 6 states that passed RTW laws during the period of 1992-2019 and the 23 states that never passed RTW laws, derived from the analysis of Right-to-Work laws on occupational fatal injuries using the GSC approach. Those states marked gray are the 21 "always-treated" states that passed RTW laws before 1992, which are excluded from the analysis.

References

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Zoorob, Michael. 2018. "Does 'Right to Work' Imperil the Right to Health? The Effect of Labour Unions on Workplace Fatalities." *Occupational and Environmental Medicine* 75(10):736-38.