

Petach, Luke

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# Labor Unions and Deaths of Despair: Evidence from Right-to-Work Laws

Luke Petach\*

March 2026

## Abstract

This paper examines the relationship between union membership and deaths of despair. Using state-level variation in the timing of the adoption of right-to-work (RTW) laws as a natural experiment, I show that right-to-work laws are associated with a decline in union membership and an increase in deaths of despair. Two-way fixed-effects (TWFE) difference-in-differences (DiD) estimates suggest that the adoption of a right-to-work law is associated with an approximately 2.6 percentage point reduction in union membership at the state-level and an increase in deaths of despair mortality between 12 and 13 additional persons per 100,000, suggesting that each percentage point decline in union membership is associated with approximately five additional deaths from suicide, drug overdose, or alcoholic liver disease per 100,000 persons. I support the TWFE results with state-level estimates from the [Callaway and Sant'Anna \(2021\)](#) estimator and the [Borusyak et al. \(2024\)](#) estimator, which are robust to concerns about treatment effect heterogeneity and variation in treatment timing. Estimates from a county-level specification using the [Callaway and Sant'Anna \(2021\)](#) estimator similarly suggest that RTW laws increase deaths of despair mortality by 6 to 11 additional persons per 100,000.

**JEL Codes:** J51, I12, I14

**Keywords:** Labor Unions, Right-to-Work Laws, Deaths of Despair, Health Economics, Labor Economics

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\*Associate Professor of Economics, Jack C. Massey College of Business. Belmont University. 1900 Belmont Boulevard, Nashville, TN, 37212. [luke.petach@belmont.edu](mailto:luke.petach@belmont.edu). GLO Fellow, Global Labor Organization.

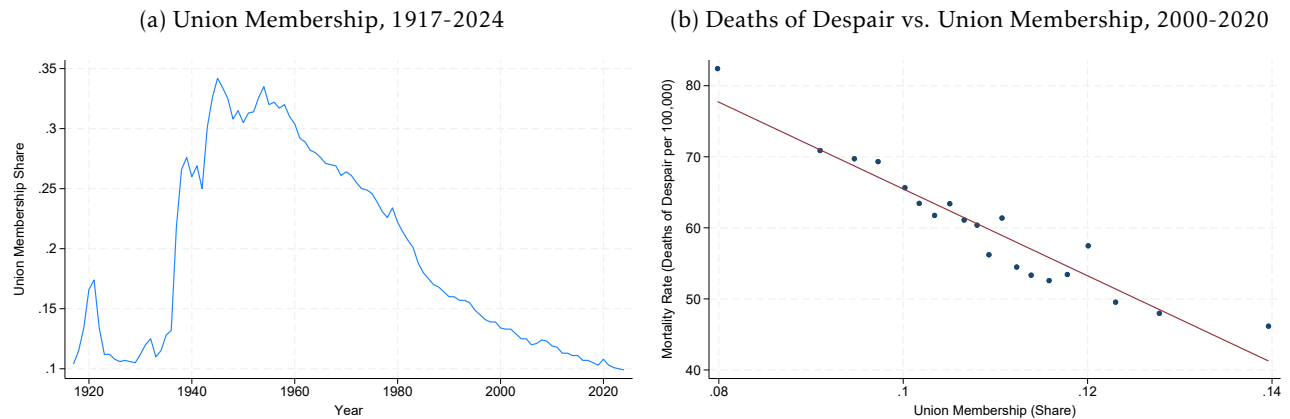
# 1 Introduction

In their groundbreaking work on “deaths of despair,” [Case and Deaton \(2015, 2020, 2022\)](#) draw attention to the dramatic increase in deaths from suicide, drug overdose, and alcoholic liver disease—particularly among middle-aged white, non-Hispanics—in the United States in the first two decades of the twenty-first century. While a large literature has examined the opioid epidemic in the United States as a significant contributing factor to the rise in midlife mortality and morbidity ([Alpert et al., 2022](#); [Langford and Feldman, 2024](#); [Arteaga and Barone, 2026](#)), relatively little work has been done on the economic and social drivers of deaths of despair. [Dow et al. \(2020\)](#) provide evidence that higher minimum wages and a more generous earned income tax credit (EITC) are associated with fewer suicides across states, [Knapp et al. \(2019\)](#) show that various measures of economic insecurity are positively correlated with deaths of despair across counties, and [Giles et al. \(2023\)](#) find that declining religiosity is associated with higher mortality rates at the state-level. In a recent paper, [Case and Deaton \(2022\)](#) suggest that the decline of unions may be a contributing factor to the rise in deaths of despair. Figure 1 provides *prima facie* evidence to this effect: although the decline of union membership in the United States pre-dates the increase in mortality from suicide, drug overdose, and alcoholic liver disease, Figure 1b indicates that union membership across states is strongly negatively correlated with deaths of despair between 2000 and 2020.

In this paper, I provide empirical evidence of the effect of union membership on deaths of despair across US states over the period from 2000 to 2020. To estimate the effect of unions on deaths of despair, I leverage the enactment of state-level “right-to-work” (RTW) laws. RTW laws allow employees covered by a collective bargaining agreement to opt-out of paying union dues—even if those employees otherwise benefit from the gains obtained via collective bargaining. The free-rider problem that results subsequently harms union finances, reduces the ability of unions to organize effectively, and ultimately weakens the power of unions in the workplace ([Freeman and Medoff, 1984](#); [Matsa, 2010](#); [Feigenbaum et al., 2018](#); [Fortin et al., 2022](#)). The loss of worker power may result in a reduction in fringe benefits, lower wages, a reduction in employment security, and a general sense of reduced agency—all of which could plausibly contribute to an increase in deaths of despair.

Using a two-way fixed-effects (TWFE) difference-in-differences (DiD) approach that exploits

Figure 1: Union Membership and Deaths of Despair



Notes: Aggregate union membership data obtained from [Freeman \(1997\)](#) and the Bureau of Labor Statistics (BLS). State-level union membership data obtained from [Hirsch et al. \(2026\)](#). State-level deaths of despair estimates obtained from the Centers for Disease Control and Prevention (CDC) Multiple Cause of Death files ([Centers for Disease Control and Prevention, 2026](#)). Figure 1b presents a binned scatterplot of the state-level deaths of despair mortality rate against state-level union membership for the period 2000-2020, where both variables have been residualized on state-fixed effects.

the differential timing of the adoption of right-to-work laws across states, I find that state-level RTW laws are associated with lower union membership and higher rates of deaths of despair. TWFE-DiD estimates suggest that the enactment of a RTW law reduces the share of employees in a union by approximately 2.6 percentage points (relative to a sample average union membership rate of approximately 11 percent) and increases the deaths of despair mortality rate between 12 and 13 persons per 100,000, suggesting that each percentage point decline in union membership is associated with approximately five additional deaths from suicide, drug overdose, or alcoholic liver disease per 100,000 persons. To address concerns about bias stemming from heterogeneous treatment effects and/or variation in treatment timing, I support the TWFE estimates with results from several alternative estimators robust to concerns about treatment effect heterogeneity. Results from the [Borusyak et al. \(2024\)](#) imputation estimator and the CSDID estimator ([Callaway and Sant'Anna, 2021](#)) support the baseline TWFE findings. Estimates from a county-level specification using the [Callaway and Sant'Anna \(2021\)](#) estimator similarly suggest that RTW laws increase deaths of despair mortality by 6 to 11 additional persons per 100,000. Finally, I provide evidence of at least one mechanism by which the reduction in union membership caused by the enactment of right-to-work laws may increase deaths of despair: changes in access to care driven by changing insurance

status. The share of the labor force covered by private health insurance falls by approximately one percentage point following the enactment of right-to-work laws. The fall in private health insurance coverage is roughly offset by an increase in Medicaid coverage. If individuals formerly covered by private health plans lose access to various health care providers—including mental health providers—this provides an additional mechanism by which declining union membership may result in increased mortality.

The rest of the paper is organized as follows. Section 2 reviews the existing literature on right-to-work laws, the economic effects of labor unions, and the literature on the relationship between union membership and health outcomes. Section 3 introduces the data and estimation strategy. Section 4 presents the results. Section 5 offers some concluding remarks.

## 2 Literature Review

The findings in this paper contribute to the large literature on the economic effects of labor unions (Freeman and Medoff, 1984; Card, 2001; Farber et al., 2021; Naidu, 2022; Jäger et al., 2024). Freeman and Medoff (1984) famously distinguish between the “monopoly face” of labor unions—in which labor unions are an institution that acts to raise equilibrium wages above the level found in a competitive labor market—and the “collective voice/institutional response” face—in which labor unions use their “voice” (à la Hirschman, 1970) to provide workers a means of communicating with management and effecting positive change in the workplace. As Freeman and Medoff (1984) note, many aspects of the workplace—including safety conditions, lighting, heating, the speed of production, the existence of grievance procedures, policies for promotion, etc.—are effectively public goods. Above and beyond any negative effects stemming from the reduction in wage growth that is likely to accompany a decline in union membership, a decline in union membership is likely to negatively impact workers by reducing their ability to exercise their collective voice, subsequently reducing access to workplace public goods. Insofar as this loss of agency and workplace amenities negatively impacts workers’ mental and physical health, it may contribute to higher mortality and morbidity.

A number of empirical papers have highlighted the positive link between union membership and both workplace public goods and productive efficiency. Using patient discharge data from

California hospitals between 1996 and 2005, [Dube et al. \(2016\)](#) show that hospitals with a successful union election outperform those with a failed union election on 12 out of 13 nurse-sensitive patient outcomes. [Ash and Seago \(2004\)](#) show that hospitals where nurses are unionized have 5.5% lower patient heart attack mortality compared to non-union hospitals. [Han and Garcia \(2023\)](#) show that labor unions reduce self-reported stress levels among teachers. [Han and Maloney \(2022\)](#) show that middle-class school districts with collective bargaining agreements have higher average test scores than similar districts not covered by collective bargaining agreements. [Petach and Wyant \(2023\)](#) show that union workers are more likely to have health insurance coverage, more likely to have a regular health care provider, visit office-based health care providers more often, and pay a lower share of their health expenditures out of pocket. Finally, [Morantz \(2013\)](#) shows that unionization is associated with a significant reduction in traumatic injuries and fatalities in underground, bituminous coal mines. By demonstrating the relationship between union membership and deaths of despair, this paper thus provides additional evidence regarding the potential importance of the “collective voice/institutional response” face of labor unions (above and beyond whatever effect the “monopoly face” may have on wages).

The second contribution of this paper is to the literature on the economic and social causes of deaths of despair. While there is a large literature highlighting the role played by the opioid epidemic as a driver of deaths of despair ([Alpert et al., 2022](#); [Langford and Feldman, 2024](#); [Arteaga and Barone, 2026](#)), few scholars have examined the ways in which various other social, economic, and political forces may have contributed to the increase in mortality from suicide, drug overdose, and alcoholic liver disease. [Dow et al. \(2020\)](#) leverage state-level variation in minimum wages and EITC generosity to estimate the effect of labor market policies on deaths of despair. [Dow et al. \(2020\)](#) find that a 10 percent minimum wage increase reduces non-drug suicides among low-educated adults by 2.7 percent. [Giles et al. \(2023\)](#) use the repeal of “blue laws” (laws prohibiting economic activity on certain days of the week, usually Sunday) as a source of plausibly exogenous variation in religiosity across states. [Giles et al. \(2023\)](#) find that decreasing state-level religiosity is associated with higher mortality rates due to suicide, drug overdose, and alcoholic liver disease. Finally, [Knapp et al. \(2019\)](#) show that various measures of economic insecurity are positively correlated with deaths of despair across counties. This paper contributes to the growing literature on the economic and social causes of deaths of despair by highlighting the importance of labor

unions as an economic and social institution influencing mortality rates across states.

Finally, this paper contributes to the growing literature on the economic, social, and political consequences of right-to-work (RTW) laws. [Chava et al. \(2020\)](#) show that RTW laws decrease wages for unionized workers and increase profits for labor-intensive firms, leading the authors to conclude that RTW laws primarily impact workers and firms by decreasing worker bargaining power. [Fortin et al. \(2022\)](#) use state-level variation in RTW laws to estimate the effect of RTW law adoption on unionization and wage rates. [Fortin et al. \(2022\)](#) find that RTW reduce wages and union membership. Finally, [Feigenbaum et al. \(2018\)](#) use state-level variation in RTW laws to estimate the effect of unions on politics and policy between 1980 and 2016. [Feigenbaum et al. \(2018\)](#) find that RTW laws reduce county-level Democratic Presidential vote shares by approximately 3.5 percentage points in subsequent elections. This paper contributes to the literature on RTW laws by highlighting the potential public health consequences of these laws via their effect on mortality from deaths of despair.

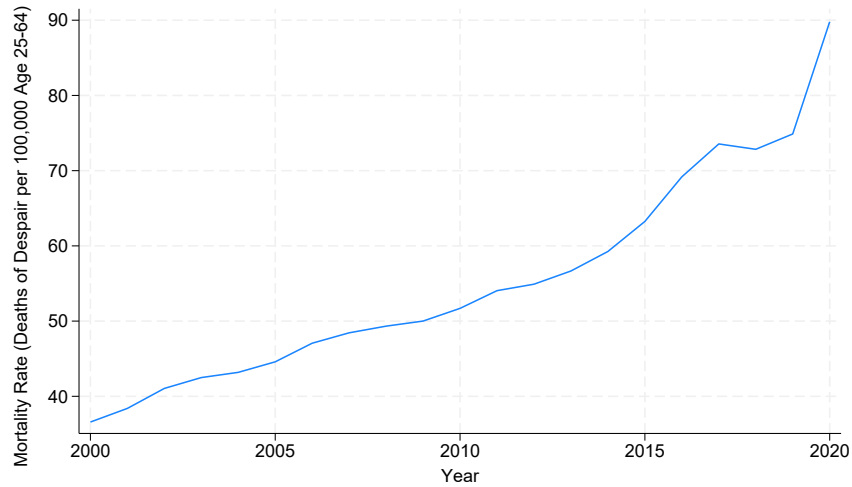
### 3 Data and Estimation Strategy

#### 3.1 Data

*Mortality Data.* Data on state-level mortality are obtained from the Centers for Disease Control and Prevention (CDC) Multiple Cause of Death files for the years 2000-2020. The Multiple Cause of Death database contains mortality and population counts for all U.S. counties. Data are based on death certificates for U.S. residents. Each death certificate contains a single underlying cause of death, up to twenty additional multiple causes, and demographic data ([Centers for Disease Control and Prevention, 2026](#)). I focus on deaths of working-aged individuals between the ages of 25 and 64. The CDC data identify cause of death using International Classification of Diseases (ICD) codes. For the period between 2000 and 2020, ICD-10 codes are used. To capture mortality due to “deaths of despair,” I focus on deaths stemming from suicides, alcoholic liver disease, and drug overdoses, following the work of [Case and Deaton \(2020, 2022\)](#). Table 10 in Appendix A lists the ICD-10 codes used to construct the state-level deaths of despair estimates. The ICD-10 codes used here produce a definition of deaths of despair similar to that used elsewhere in the literature (e.g., see [Social Capital Project, 2019](#); [Ruhm, 2022](#); [Giles et al., 2023](#)). Figure 2 plots the average

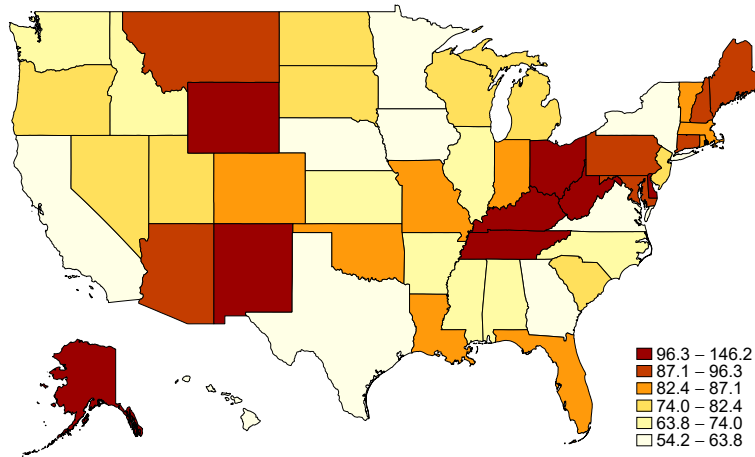
state-level crude mortality rate from deaths of despair between 2000 and 2020. Figure 3 maps the mortality rate across states for the year 2019. Mortality is highest in Appalachia (Tennessee, Kentucky, Ohio, West Virginia), the Mountain West (New Mexico, Wyoming, and Montana) and New England (New Hampshire, Maine).

Figure 2: Deaths of Despair, Average State-level Mortality Rate (per 100,000), 2000-2020



Notes: Figure 2 plots the population-weighted average state-level mortality rate (per 100,000 persons aged 25-64) for the years 2000 to 2020. Deaths of despair estimates constructed using the ICD-10 codes in Table 10.

Figure 3: Deaths of Despair, State-level Mortality Rate (per 100,000), 2019



Notes: Figure maps the crude mortality rate from deaths of despair (deaths per 100,000) across states for the year 2019.

*Right-to-Work Laws.* I obtain data on the timing of RTW law adoption from Fortin et al. (2022). As of 2020, 27 states had adopted RTW laws. There are two distinct waves of RTW law adoption in

the US. The first wave occurred in the late 1940's, spurred by passage of the Taft-Hartley Act in 1947, which explicitly granted states the right to enact RTW laws. Between 1944 and 1948, twelve (mostly Southern) states adopted RTW laws. Six additional states followed in the 1950's and 1960's. Between 1963 and 2001 only three additional states (Louisiana—in 1976, Idaho—in 1985, and Oklahoma—in 2001) adopted RTW laws. The second distinct wave of RTW law adoption occurs beginning in 2012. Between 2012 and 2017 five additional states adopted RTW laws (Kentucky, West Virginia, Indiana, Michigan, and Wisconsin). Both Missouri and New Hampshire voted on—but ultimately did not enact—RTW laws during this same time period. Table 1 lists all states that have adopted RTW laws as of 2020 along with the corresponding date of adoption. In this paper, I identify the impact of RTW laws on union membership and deaths of despair using the timing of RTW law adoption in the post-2000 wave (inclusive of Oklahoma). The estimation strategy is discussed further in Section 3.2. The identifying assumption is that—in the absence of a newly adopted RTW law—both union membership and mortality would have evolved similarly in switchers (those states with newly adopted RTW laws) and stayers (those states whose RTW law status did not change during the sample period).

*Additional State-level Data.* I obtain additional data on state-level economic and social characteristics from the Integrated Public-Use Microdata Series (IPUMS) for the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS) (Flood et al., 2025). Additional variables include the state unemployment rate, the share of the population with at least four years of college, the proportion of the state that is white, non-Hispanic, average household income, the share of the population that is 65 or older, and the share of the labor force employed in manufacturing. To address concerns that supply-side factors related to the opioid epidemic may be the main driver of deaths of despair, I collect data on the opioid prescribing rate (measured as annual kilograms sold of oxycodone and hydrocodone per 100,000 people) from the State Health Access Data Assistance Center (SHADAC), which constructs state-level estimates from the U.S. Drug Enforcement Agency's Automated Reports and Consolidated Ordering System (ARCOS) Retail Drug Summary Reports. Finally, I obtain state-level data on union membership and union coverage from Hirsch et al. (2026), who construct their estimates from monthly household CPS data using BLS methods. Table 2 presents sample summary statistics for all key variables.

Table 1: Right-to-Work Laws by State

State	Adoption
Arkansas	1944
Florida	1944
Nebraska	1946
New Mexico	1946
Iowa	1947
South Dakota	1947
Texas	1947
Tennessee	1947
North Carolina	1947
Virginia	1947
Georgia	1947
North Dakota	1948
Nevada	1952
Alabama	1953
South Carolina	1954
Utah	1955
Kansas	1958
Mississippi	1960
Wyoming	1963
Louisiana	1976
Idaho	1985
Oklahoma	2001
Indiana	2012
Michigan	2013
Wisconsin	2015
West Virginia	2016
Kentucky	2017

*Notes:* Table 1 presents all states with RTW laws and the corresponding date of adoption. Data on the timing of RTW law adoption obtained from [Fortin et al. \(2022\)](#).

Table 2: State Summary Statistics, 2000-2020

	Mean	Std. Dev.	Source
Deaths of Despair Mortality Rate (per 100,000)	55.40951	18.85353	CDC Multiple Cause of Death Files
Union Membership	.1179959	.0610661	<a href="#">Hirsch et al. (2026)</a>
Union Coverage	.1305132	.0611802	<a href="#">Hirsch et al. (2026)</a>
Right-to-Work Law Indicator	.4269494	.4948659	<a href="#">Fortin et al. (2022)</a>
Opioid Prescription Rate (kg per 100,000)	24.49241	11.62657	SHADAC
Average Household Income (Constant 1999 \$)	62778.19	8954.31	IPUMS CPS
Average Wage Income	32747.56	3899.461	IPUMS CPS
Private Insurance Share	.6715176	.0648601	IPUMS CPS
Medicaid Share	.1511299	.0459724	IPUMS CPS
Unemployment Rate	.0621058	.0215857	IPUMS CPS
College Share	.2117651	.0457381	IPUMS CPS
White, non-Hispanic Share	.6432036	.1527843	IPUMS CPS
65+ Share	.1341115	.0244343	IPUMS CPS
Manufacturing Share	.1135116	.040145	IPUMS CPS
N	1,071		

Notes: Table 2 presents summary statistics for all key variables. Observations weighted by state population.

### 3.2 Estimation Strategy

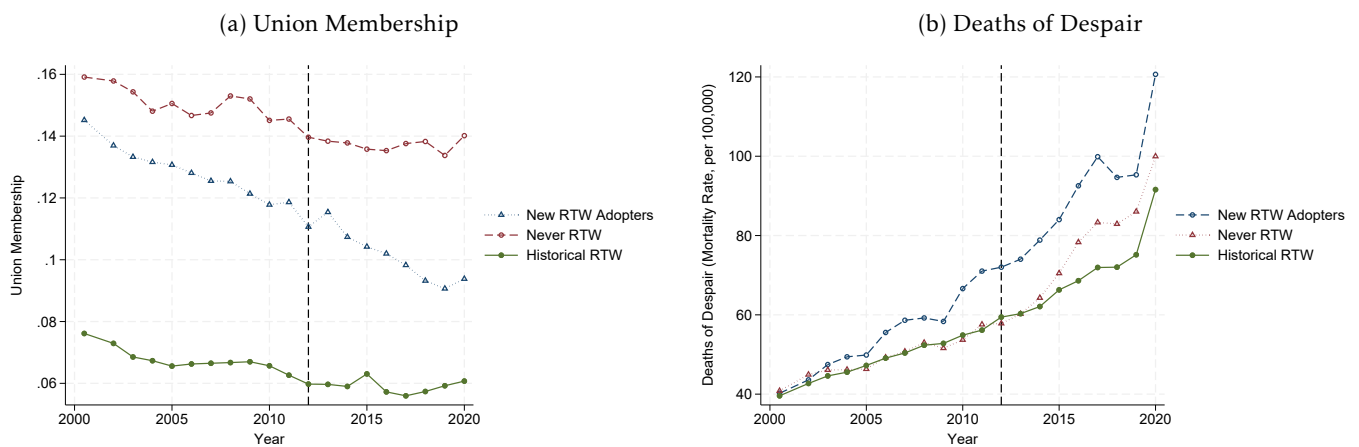
I exploit the differential timing of RTW law adoption across US states as a natural experiment to study the effect of union membership on deaths of despair. Specifically, to estimate the effect of RTW laws on union membership and deaths of despair, I implement the following TWFE DiD specification:

$$Y_{it} = \beta_0 + \beta_1 RTW_{it} + \mathbf{X}_{it}^T \alpha + \delta_t + \gamma_i + \epsilon_{it} \quad (1)$$

Where  $Y_{it}$  is the outcome variable of interest in state  $i$  at time  $t$ —e.g., union membership, union coverage, or mortality— $RTW_{it}$  is a dummy variable indicating whether a state has enacted a right-to-work law as of time  $t$ ,  $\mathbf{X}_{it}$  is a vector of state-level characteristics,  $\delta_t$  is a year-fixed effect,  $\gamma_i$  is a state-fixed effect, and  $\epsilon_{it}$  is an idiosyncratic error term. The inclusion of state-fixed effects captures any unobserved time-invariant state-specific characteristics potentially influencing the relationship between RTW laws, union membership, and mortality (such as historical political conditions or time-invariant policy differences across states) while time-fixed effects will capture any time-common shocks impacting all states (such as the Great Recession). To the extent that the timing of the adoption of RTW laws across states can be considered plausibly exogenous, the regression coefficient  $\beta_1$  gives an estimate of the causal effect of right-to-work laws. The identifying assumption of the estimator is that—conditional on controls and fixed-effects—both

union membership and mortality would have evolved similarly in states that adopted RTW laws and states that did not adopt RTW laws in the absence of the adoption of those laws. In the baseline specification, I include states that have “always” had RTW laws (for the duration of the sample). Thus, the estimator requires that—although the *level* of union membership and/or deaths of despair might be different in states with newly adopted RTW laws and states that have historical RTW laws—in the absence of such adoption the *trend* in union membership and mortality would have evolved similarly across states. This amounts to assuming that the effect of RTW laws on union membership and mortality in states that adopted RTW laws in the distant past is already fully reflected in the current levels of membership and mortality (such that there are no current trend differences due to historical adoption). Figure 4 provides *prima facie* evidence to this effect. Although historical adopters of RTW laws have much lower *levels* of union membership, the trend in union membership appears similar for “never” adopters and “always” adopters. In contrast, there appears to be a shift in the trend for “switchers” beginning in 2012 (the start of the most recent wave of RTW law adoption). Figure 4b presents similar evidence for trends in the deaths of despair mortality rate. In Section 4.5, I supplement the baseline findings with a specification that excludes the “always” adopter states, and in Section 4.6 I present results from a county-level specification that excludes counties in always adopter states: I find little difference in the estimated effect between these specifications and the baseline TWFE DiD result.

Figure 4: Union Membership and Deaths of Despair by RTW Law Adoption Status, 2000-2020



Notes: Figure 4 plots union membership and deaths of despair mortality over time according to RTW law adoption status. Dashed vertical line placed at 2012, the start of the most recent RTW adoption wave.

Finally, recent work in the DiD literature suggests that TWFE DiD estimates may be biased in settings with multiple treatment periods, variation in treatment timing, or heterogeneous treatment effects (Roth et al., 2023). Intuitively, the aggregate DiD coefficient is a weighted average of comparisons between groups of different treatment status. With differential treatment timing, this average may include comparisons of newly treated units with units treated in the recent past (so-called “forbidden” comparisons). With heterogeneous treatment effects, “forbidden” comparisons can have deleterious consequences, including the problem of “negative weights,” which can produce an estimate of the treatment effect of the opposite sign of the true effect (Roth et al., 2023). Thus, I supplement the TWFE DiD estimates obtained from Equation 1 by applying several estimators robust to concerns about heterogeneous treatment effects and variation in treatment timing (Borusyak et al., 2024; Callaway and Sant’Anna, 2021). The results from these estimators are presented in Section 4.4; the heterogeneous treatment effect results support the findings of the baseline TWFE DiD estimates.

## 4 Results

### 4.1 Baseline Results

Table 3: Estimation Results: TWFE-DiD

	(1)	(2)	(3)	(4)
	Union Membership	Union Coverage	Mortality Rate	Ln(Mortality)
A: TWFE-DiD (No Controls)				
$RTW_{it}$	-0.0259*** (0.00702)	-0.0236*** (0.00724)	13.11* (7.257)	0.108** (0.0480)
$N$	1,071	1,071	1,071	1,071
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
B: TWFE-DiD (w/ Controls)				
$RTW_{it}$	-0.0261*** (0.00689)	-0.0232*** (0.00707)	12.64* (6.504)	0.110** (0.0493)
$N$	1,071	1,071	1,071	1,071
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Notes: Standard errors in parenthesis, clustered at the state-level.  $p < 0.10$  \*  $p < 0.05$  \*\*  $p < 0.01$  \*\*\*. Control variables include the (log-) opioid prescribing rate (annual kilograms of Oxycodone and Hydrocodone sold per 100,000 persons), the unemployment rate, the share of the population with at least four years of college, (log-) average household income, the share of the state population that is sixty-five or older, and the share of the labor force employed in manufacturing.

Table 3 presents estimation results from the baseline TWFE-DiD specification. Panel A excludes

control variables, Panel B includes a full suite of time-varying state-level controls. The dependent variable in Column (1) is the union membership share, the dependent variable in Column (2) is the union coverage share, the dependent variable in Column (3) is the deaths of despair mortality rate, and the dependent variable in Column (4) is the natural log of the deaths of despair mortality rate. Standard errors are clustered at the state-level to address the possibility of serial correlation over time within states, the presence of which would bias the standard errors downward in the absence of clustering.

The results presented in Table 3 indicate that state-level adoption of right-to-work laws between 2000 and 2020 is associated with a statistically and economically significant reduction in both union membership and union coverage, and a statistically and economically significant increase in deaths of despair. Column (1) suggests that adoption of a right-to-work law reduces union membership by approximately 2.6 percentage points (a 22% reduction relative to the sample mean). Column (3) indicates that RTW laws result in between 12.6 and 13.1 additional deaths per 100,000 persons from accidental poisonings, suicide, and alcoholic liver disease. Column (4) suggests that adoption of RTW laws increase in the state deaths of despair mortality rate by approximately 11%. Taking the estimates in Column (1) and Column (3) together, the results suggest that each additional percentage point decline in union membership is associated with approximately 5 additional deaths per 100,000 from deaths of despair. Between 2000 and 2020, the total union membership rate in the United States declined by roughly three percentage points, suggesting that declining union membership is responsible for an additional 15 deaths of despair per 100,000 persons annually, roughly 28% of the increase in the deaths of despair mortality rate over the sample period.

## 4.2 Outcome Placebo Estimates

An important question about the results in Table 3 concerns whether the estimates capture the unique effect of RTW laws on deaths of despair, or whether states which adopted RTW laws were simply experiencing a secular increase in all types of mortality. As [Case and Deaton \(2020\)](#) point out, part of the decline in life expectancy experienced in the United States over the sample period is driven by a decline in the rate of reduction in heart disease mortality. However, given that heart disease may take years (or decades) to progress, there should be little short-run effect of

Table 4: Estimation Results: Alternative Cause of Death Placebo

	(1)	(2)	(3)	(4)
	Heart Disease Mortality	Ln(Heart Disease Mortality)	Cancer Mortality	Ln(Cancer Mortality)
RTW <sub>it</sub>	-3.200 (5.113)	-0.0142 (0.0210)	1.240 (2.127)	0.00413 (0.0113)
N	1,071	1,071	1,071	1,071
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Notes: Standard errors in parenthesis, clustered at the state-level.  $p < 0.10$  \*  $p < 0.05$  \*\*  $p < 0.01$  \*\*\*. Control variables include the (log-) opioid prescribing rate (annual kilograms of Oxycodone and Hydrocodone sold per 100,000 persons), the unemployment rate, the share of the population with at least four years of college, (log-) average household income, the share of the state population that is sixty-five or older, and the share of the labor force employed in manufacturing.

RTW laws on heart disease—even if RTW laws and the associated reduction in union membership are responsible for near-term increases in deaths of despair. Thus, Table 4 presents the results of two placebo tests: an estimate of the effect of RTW laws on heart disease mortality<sup>1</sup>, and an estimate of the effect of RTW laws on cancer mortality<sup>2</sup> (similar to heart disease mortality, there should be little short-run effect of RTW laws on cancer). In both cases, the estimated effect should be statistically insignificantly different from zero. The results presented in Table 4 support the baseline findings. In particular, in each case the estimated coefficients are economically small in magnitude and statistically insignificantly different from zero.

### 4.3 Temporal Placebo Estimates

Table 5: Temporal Placebo Estimates

	(1)	(2)	(3)	(4)
	Union Membership	Union Coverage	Mortality Rate	Ln(Mortality Rate)
RTW Placebo <sub>it</sub>	-0.00210 (0.00367)	-0.00111 (0.00429)	1.487 (1.062)	0.0188 (0.0288)
N	500	500	500	500
Controls	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y

Notes: Standard errors in parenthesis, clustered at the state-level.  $p < 0.10$  \*  $p < 0.05$  \*\*  $p < 0.01$  \*\*\*. Control variables include the (log-) opioid prescribing rate (annual kilograms of Oxycodone and Hydrocodone sold per 100,000 persons), the unemployment rate, the share of the population with at least four years of college, (log-) average household income, the share of the state population that is sixty-five or older, and the share of the labor force employed in manufacturing.

<sup>1</sup>ICD-10 Codes: I05-I09 (Chronic rheumatic heart diseases); I10-I15 (Hypertensive diseases); I20-I25 (Ischaemic heart diseases); I26-I28 (Pulmonary heart disease and diseases of pulmonary circulation); I30-I51 (Other forms of heart disease).

<sup>2</sup>ICD-10 Codes: C00-C97 (Malignant neoplasms).

Table 5 presents results from a temporal placebo test, assigning alternative placebo treatment dates to each treated state. In particular, for each RTW adopter in the sample, I assign an alternative treatment date 11 years prior to the actual treatment date, and then restrict the sample to the period prior to the treatment date of the first treated state (Indiana is treated in 2012; in this case, I restrict the sample to the years 2009 and earlier). Oklahoma—which adopts a RTW law in 2001—is excluded from the analysis due to insufficient pre-treatment period data. The results in Table 5 support the main findings. In each case the estimated placebo effect is both statistically insignificant and near-zero in magnitude, suggesting that the baseline result is likely not due to systematic pre-treatment differences between states (which would otherwise result in a statistically significant temporal placebo effect).

#### 4.4 Heterogeneous Treatment Effect Estimates

Table 6: Estimation Results - Heterogeneous Treatment Effects

	(1)	(2)	(3)	(4)
	Union Membership	Union Coverage	Mortality Rate	Ln(Mortality)
A: <a href="#">Borusyak et al. (2024)</a> Estimator (No Controls)				
RTW <sub>it</sub> (ATT)	-0.0127*** (0.00138)	-0.00917*** (0.00143)	10.64*** (1.218)	0.0904*** (0.0178)
N	1,071	1,071	1,071	1,071
B: <a href="#">Borusyak et al. (2024)</a> Estimator (With Controls)				
RTW <sub>it</sub> (ATT)	-0.0134*** (0.00178)	-0.00988*** (0.00192)	11.11*** (1.401)	0.0899*** (0.0215)
N	1,071	1,071	1,071	1,071
C: <a href="#">Callaway and Sant'Anna (2021)</a> Estimator (No Controls)				
RTW <sub>it</sub> (Group Avg. ATT)	-0.0158*** (0.000937)	-0.0138*** (0.00101)	6.494*** (0.921)	0.0358*** (0.0121)
N	1,071	1,071	1,071	1,071
D: <a href="#">Callaway and Sant'Anna (2021)</a> Event Study (No Controls)				
Pre-Treatment Avg.	-0.0008 (0.0014)	-.0005 (0.002)	1.96 (1.33)	0.0197 (0.014)
Post-Treatment Avg.	-0.013** (0.006)	-.0106* (0.006)	6.50*** (2.30)	0.056*** (0.0198)
N	1,071	1,071	1,071	1,071

Notes: Standard errors in parenthesis, clustered at the state-level.  $p < 0.10$  \*  $p < 0.05$  \*\*  $p < 0.01$  \*\*\*. Control variables include the (log-) opioid prescribing rate (annual kilograms of Oxycodone and Hydrocodone sold per 100,000 persons), the unemployment rate, the share of the population with at least four years of college, (log-) average household income, the share of the state population that is sixty-five or older, and the share of the labor force employed in manufacturing. [Callaway and Sant'Anna \(2021\)](#) event study estimates present the pre- and post-treatment average effect corresponding to the event study plots presented in Figure 5.

Table 6 presents results from the [Borusyak et al. \(2024\)](#) estimator and the [Callaway and Sant'Anna \(2021\)](#) estimator. The [Borusyak et al. \(2024\)](#) estimator is an imputation estimator. Specifically, [Borusyak et al. \(2024\)](#) illustrate that one can obtain the treatment effect for each unit

as the difference between the variable of interest and an imputed “never-treated” estimate of the potential outcome (obtained as the predicted value from a regression of observations in non-treated time periods on fixed-effects and control variables). To allow a comparison with the baseline estimates presented in Table 3, I report the simple average treatment effect on the treated (ATT) across all observations. I report results both without controls (Panel A) and with linear adjustment for covariates (Panel B).

The Callaway and Sant’Anna (2021) estimator separately estimates each  $2 \times 2$  group-time ATT for each treated cohort,  $g$ , in each time period,  $t$  ( $ATT(g, t)$ )<sup>3</sup>, using either never-treated or not-yet treated units as controls. Callaway and Sant’Anna (2021) show that the individual  $ATT(g, t)$  estimates can be aggregated in various ways, including by group and by event-time. To facilitate comparison with the baseline TWFE DiD estimates in Table 3, I report the group-average ATT, which is constructed as a weighted average of the effects for each group  $g$  to participate in the treatment, using never-treated units as controls. The weighted average of the group effects produces an aggregate ATT that is analogous to the ATT obtained in the canonical DiD setup with two periods and two groups. This estimate is reported in Panel C of Table 6. In Figure 5, I present event-study plots for the effect of RTW laws. In Panel D of Table 6, I report the average pre- and post-treatment effects corresponding to the event studies presented in Figure 5. Finally, both sets of estimates obtained from the Callaway and Sant’Anna (2021) estimator exclude control variables. Inclusion of controls with the Callaway and Sant’Anna (2021) estimator allows the implementation of various “doubly robust” estimation procedures where control variables are first used to predict treatment status<sup>4</sup> and then the predicted treatment status from the treatment regression are used to re-weight observations in the outcome regression. When the number of treated units in each treatment cohort is small (as is the case here), inclusion of controls risks violating the overlap assumption, as the covariates may perfectly predict treatment status when the “effective” sample size is small (Callaway and Sant’Anna, 2021).<sup>5</sup>

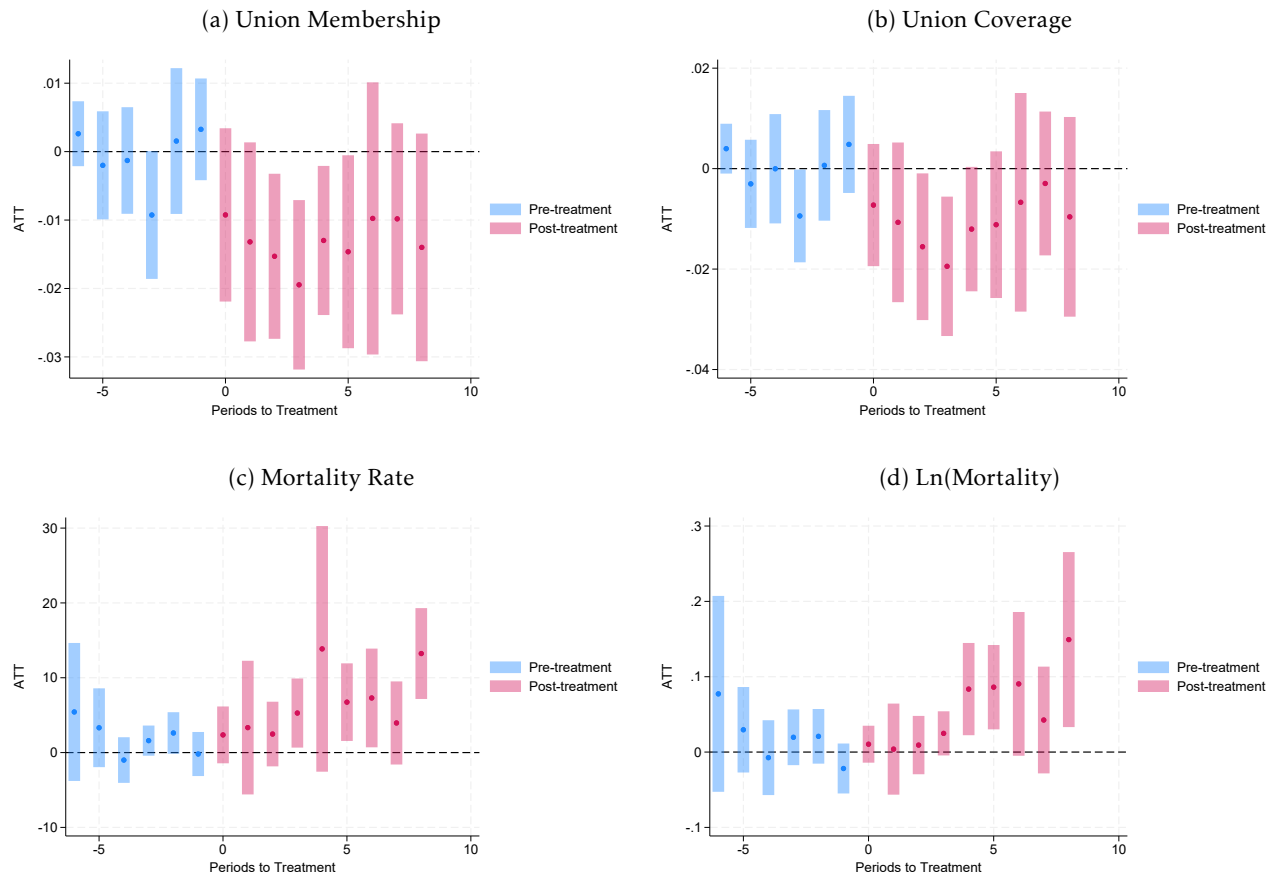
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<sup>3</sup>E.g.,  $ATT(2016, 2017)$  would be the average treatment effect on the treated in 2017 for units initially treated in 2016. One important distinction between the Callaway and Sant’Anna (2021) estimator and the Borusyak et al. (2024) estimator is that the Callaway and Sant’Anna (2021) estimator uses only the last period prior to treatment to estimate the average treatment effect, whereas the Borusyak et al. (2024) estimator uses the average across all periods prior to the treatment date.

<sup>4</sup>Specifically, the CSDID estimator uses only time-invariant controls. In the presence of time-varying covariates, one must either specify a specific pre-treatment value or use the entire pre-treatment average. The `csdid` Stata command defaults to using the pre-treatment average when time-varying controls are included.

<sup>5</sup>E.g., in the current context, because one state is treated in 2012 (Indiana) the effective sample size for

Figure 5: Callaway and Sant'Anna (2021) Event Study Plots



Notes: Figure 5 presents event study plots of the effect of right-to-work laws on union membership and deaths of despair. Estimates obtained using the Callaway and Sant'Anna (2021) estimator. The pre- and post-treatment average effects corresponding to Figure 5 are presented in Panel D of Table 6.

The results presented in Table 6 and Figure 5 support the baseline TWFE-DiD findings. Specifically, Table 6 suggests that adoption of RTW laws reduce union membership by approximately 1.3 percentage points (an 11% reduction relative to the sample mean). The estimated union effect is smaller than the one found in Table 3, but it nonetheless remains both negative and economically and statistically significant. The estimated effect of RTW laws on mortality is also slightly reduced from Table 3, with coefficients for the crude mortality rate ranging from 6.5 to 11.1 (compared to approximately 13 in Table 3), and coefficients for the log mortality rate ranging from 0.04 to 0.09 (compared to 0.11). Nonetheless, the results continue to suggest that RTW laws have a positive effect on mortality rates. Finally, the ability of the Callaway and Sant’Anna (2021) estimator to be aggregated by event-time allows an assessment of the parallel trends assumption by testing for the presence of pre-trends. The results presented in Figure 5 and Table 6 suggest there are no significant pre-trends for any of the dependent variables of interest. In each case, the average pre-treatment effect is statistically insignificant, and for each variable Figure 5 suggests that nearly every individual pre-treatment effect is insignificant (with no evidence of clear trends in any variable).

#### 4.5 Comparison with Never Adopters

Table 7: Estimation Results: Comparison with Never Adopters, BJS Estimator

	(1)	(2)	(3)	(4)
	Union Membership	Union Coverage	Mortality Rate	Ln(Mortality Rate)
<b>Borusyak et al. (2024) Estimator (No Controls)</b>				
A: $RTW_{it}$ (ATT)	-0.0108*** (0.00204)	-0.00766*** (0.00214)	7.716*** (1.837)	0.0567** (0.0263)
<i>N</i>	630	630	630	630
<b>Borusyak et al. (2024) Estimator (With Controls)</b>				
B: $RTW_{it}$ (ATT)	-0.00760** (0.00299)	-0.00513* (0.00299)	7.232*** (2.666)	0.0560 (0.0364)
<i>N</i>	630	630	630	630

Notes: Standard errors in parenthesis, clustered at the state-level.  $p < 0.10$  \*  $p < 0.05$  \*\*  $p < 0.01$  \*\*\*. Control variables include the (log-) opioid prescribing rate (annual kilograms of Oxycodone and Hydrocodone sold per 100,000 persons), the unemployment rate, the share of the population with at least four years of college, (log-) average household income, the share of the state population that is sixty-five or older, and the share of the labor force employed in manufacturing.

Table 7 presents estimation results from a specification excluding states with historical RTW laws, such that the specification amounts to comparing new RTW law adopters (states which

$ATT(2012, 2013)$  would be equal to 1.

adopted RTW laws after 2000) with states that have never implemented a RTW law. I present results using the [Borusyak et al. \(2024\)](#) estimator, both with- and without controls. In each case, the results in Table 7 are consistent with prior estimates. RTW laws have a negative effect on union membership and increase deaths of despair. The results remain statistically significant in every case, with the exception of Column 4 of Panel B, which is marginally insignificant ( $p = 0.12$ ), likely due to the reduction in sample size. In Section 4.6, I present additional results from specifications excluding historically treated states using an extended county-level sample: in each case the results are consistent with those found here (suggesting that excluding states with historical RTW laws does not invalidate the main finding).

## 4.6 County-level Results

Table 8: County-level Results - [Callaway and Sant'Anna \(2021\)](#) Estimator

	(1) Mortality Rate	(2) Ln(Mortality)
A: Full Sample, No Opioid Control		
RTW <sub>it</sub> (Group Avg. ATT)	9.635*** (2.096)	0.128*** (0.0274)
N	29,216	29,216
Controls	Y	Y
B: Excl. Historical RTW, No Opioid Control		
RTW <sub>it</sub> (Group Avg. ATT)	10.61*** (3.390)	0.156*** (0.0491)
N	16,632	16,632
Controls	Y	Y
C: Excl. Historical RTW, Excl. Oklahoma, No Opioid Control		
RTW <sub>it</sub> (Group Avg. ATT)	9.766** (3.797)	0.142*** (0.0441)
N	15,840	15,840
Controls	Y	Y
D: Excl. Historical RTW, Excl. Oklahoma, With Opioid Control		
RTW <sub>it</sub> (Group Avg. ATT)	5.966* (3.333)	0.111*** (0.0343)
N	15,840	15,840
Controls	Y	Y

Notes: Standard errors in parenthesis, clustered at the state-level.  $p < 0.10$  \*  $p < 0.05$  \*\*  $p < 0.01$  \*\*\*. Baseline controls include (log-) labor force, the unemployment rate, (log-) population, (log-) per-capita personal income, and the Republican presidential election share for the 2000-2012 elections. Panel D includes the (log-) opioid prescribing rate as an additional control. Panel A presents estimates for the full sample. Panel B drops all states with historical RTW laws, thus comparing only new adopters with never adopters. Panel C is the same as Panel B but excludes Oklahoma. Panel D is the same as Panel C but includes the opioid control variable.

Table 8 presents results from estimating the effect of RTW laws on deaths of despair at the county-level. Measures of union strength (either membership or coverage) are not widely available

at the county-level, so the county-level specifications are restricted to the use of mortality variables as the dependent variable. As in the state-level analysis, I use the CDC Multiple Cause of Death files to construct county-level mortality rate estimates ([Centers for Disease Control and Prevention, 2026](#)). Because many individual county-year mortality estimates are either missing or suppressed due to privacy concerns, I restrict the sample to those counties that have at least ten years of observable mortality data between 2000 and 2020. For missing observations within these counties, I use linear interpolation to estimate the mortality rate.

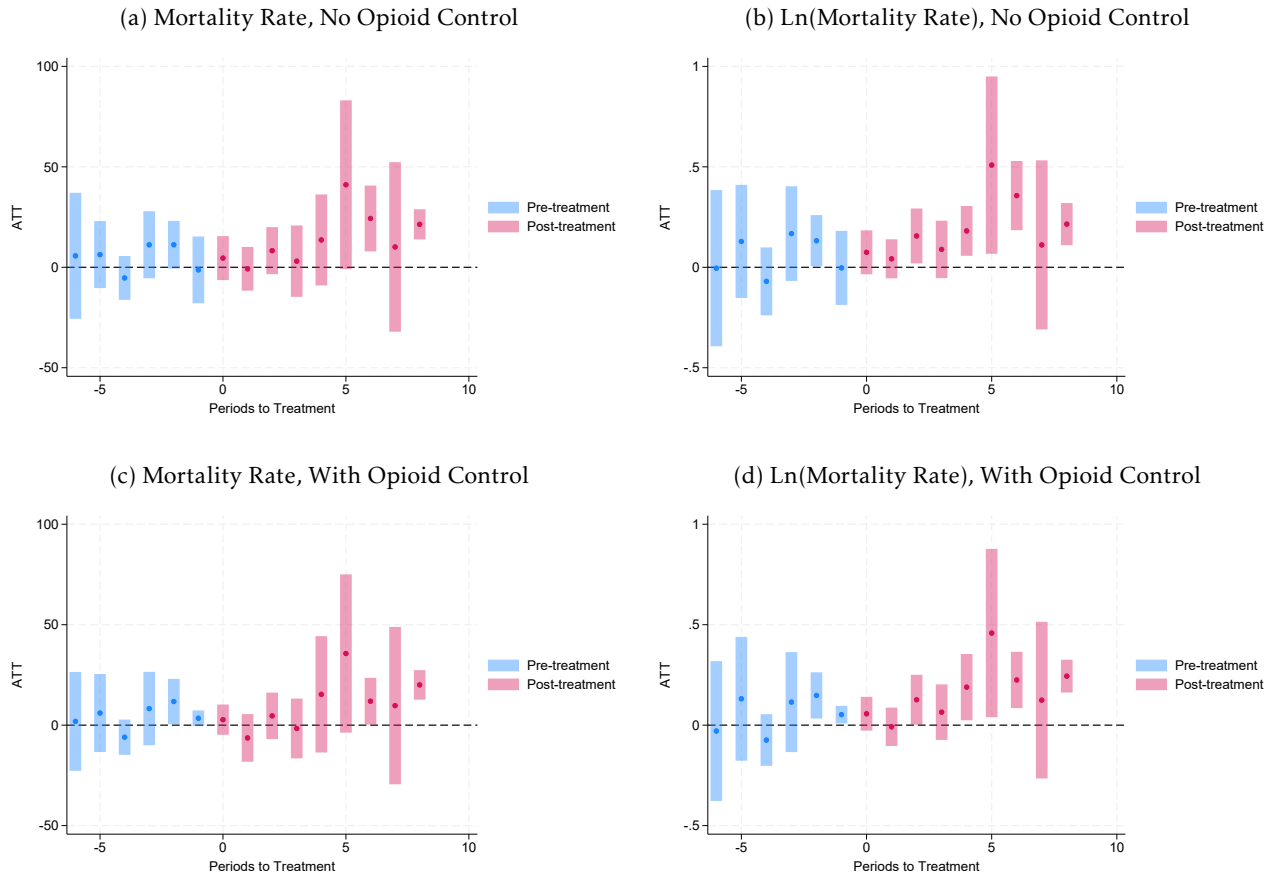
Importantly, by increasing the effective sample size for each treated cohort-year cell, the use of county-level data allows the inclusion of control variables when applying doubly-robust versions of the [Callaway and Sant’Anna \(2021\)](#) estimator. In this case, I apply the doubly-robust DiD estimator based on inverse probability weighting as in [Sant’Anna and Zhao \(2020\)](#). For control variables, I obtain data on the county-level labor force and unemployment rate from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS), data on county-level population and per-capita personal income from the Bureau of Economic Analysis (BEA) Regional Economic Accounts, data on county-level Republican presidential vote shares in the 2000, 2004, 2008, and 2012 elections from [Amlani and Algara \(2021\)](#), and data on the county-level opioid prescription rate from the CDC ([Centers for Disease Control and Prevention, 2022](#)). The final county-level sample consists of 1,328 total counties, covering the 1999 to 2020 period.

The county-level results in [Table 8](#) lend support to the state-level analysis. In every single specification, the effect of RTW laws on mortality is positive and statistically significant. In particular, [Column \(1\)](#) suggests that RTW laws increase deaths of despair mortality by 6 to 11 additional persons per 100,000. Similarly, [Column \(2\)](#) suggests that for a county located in a given state, state adoption of a RTW law increases the county-level deaths of despair mortality between 11.1% and 15.6%. Further, the county-level estimates obtained from the [Callaway and Sant’Anna \(2021\)](#) estimator exceed the state-level estimates obtained in [Table 6](#), suggesting that the ability to predict treatment status using control variables in the treatment model of the doubly-robust specification may address various important sources of heterogeneity not otherwise picked-up in the state-level specifications excluding control variables.

Finally, [Figure 6](#) presents event-study plots for the regression specifications corresponding to [Panel C](#) and [Panel D](#) of [Table 8](#). The results in [Figure 6](#) support the estimates presented in [Table](#)

8. Figure 6 suggests there are no significant pre-trends in the county-level estimates. Further, in each case, the estimated pre-treatment average effect is small in magnitude and statistically insignificant.

Figure 6: County-level Event-Study Estimates, Callaway and Sant’Anna (2021) Estimator, Excluding Historical RTW States and Oklahoma



Notes: Figure 6 presents event-study plots for the county-level sample, using the Callaway and Sant’Anna (2021) estimator. Event-study plots are estimated using sample comparing new adopters with never adopters, excluding the state of Oklahoma.

## 4.7 Mechanisms

Table 9 presents results from simple TWFE DiD regressions investigating possible mechanisms linking right-to-work laws, declining union membership, and deaths of despair at the state-level. Column (1) presents the effect of RTW laws on private insurance coverage, Column (2) presents the effect on Medicaid coverage, and Column (3) presents the effect on (log-) average wages. The

Table 9: Estimation Results - State-Level TWFE DiD Estimates, Mechanisms

	(1)	(2)	(3)
	Private Insurance Share	Medicaid Share	Ln(Wage)
A: No Controls			
RTW <sub>it</sub>	-0.0117** (0.00456)	0.0170** (0.00833)	-0.0356** (0.0144)
N	1,071	1,071	1,071
State FE	Y	Y	Y
Year FE	Y	Y	Y
B: With Controls			
RTW <sub>it</sub>	-0.00944** (0.00379)	0.0196** (0.00815)	-0.0340** (0.0158)
N	1,071	1,071	1,071
State FE	Y	Y	Y
Year FE	Y	Y	Y

Notes: Standard errors in parenthesis, clustered at the state-level.  $p < 0.10$  \*  $p < 0.05$  \*\*  $p < 0.01$  \*\*\*. Control variables include the (log-) opioid prescribing rate (annual kilograms of Oxycodone and Hydrocodone sold per 100,000 persons), the unemployment rate, the share of the population with at least four years of college, (log-) average household income, the share of the state population that is sixty-five or older, and the share of the labor force employed in manufacturing.

results in Table 9 suggest that one possible mechanism by which right-to-work laws increase deaths of despair is via the loss of private insurance. Column (1) suggests that the share of the population in a state covered by private insurance declines by approximately one percentage point following the adoption of a RTW law. The decline in private insurance is offset by a roughly equal increase in the share of the population covered by Medicaid. Thus, in addition to shifting the costs of social provisioning to the public sector (by moving workers off employer-sponsored health insurance plans and onto Medicaid), if the loss of private insurance results in a loss of access to various health care providers (even temporarily), it may result in increases in the mental distress and chronic pain which contribute to deaths of despair. This effect is consistent with the findings of [Petach and Wyant \(2023\)](#), who find that union workers are more likely to have a primary care provider than non-union workers and also visit health care providers more frequently than non-union workers. Finally, the results in Column (3) are consistent with the results of previous studies—such as [Chava et al. \(2020\)](#) and [Fortin et al. \(2022\)](#)—which find that RTW laws reduce wages. If wages fall on average following the implementation of RTW laws, the resulting lower incomes and worse average job prospects are likely to contribute to increased distress, which may contribute to increases in the drug usage, alcohol abuse, and depression which ultimately lead to deaths of despair.

## 5 Conclusion

This paper suggests that right-to-work laws and declining union membership are significant contributors to the recent rise in so-called “deaths of despair.” Using variation in the state-level adoption of right-to-work laws as a natural experiment, two-way fixed-effects difference-in-differences estimates suggest that right-to-work laws reduce state-level union membership by approximately 2.6 percentage points and increase state-level deaths of despair mortality by an additional 12 to 13 persons per 100,000. Taken together, these results suggest that each additional percentage point decline in union membership in a state results in approximately five additional deaths of despair per 100,000 persons. Between 2000 and 2020, the total union membership rate in the United States declined by roughly three percentage points, suggesting that declining union membership is responsible for an additional 15 deaths of despair per 100,000 persons annually, roughly 28% of the increase in the deaths of despair mortality rate over the sample period.

The TWFE DiD results are supported by state-level specifications using the [Borusyak et al. \(2024\)](#) estimator and the [Callaway and Sant’Anna \(2021\)](#) estimator, which are robust to concerns about treatment effect heterogeneity and variation in treatment timing. Additionally, county-level results using the [Callaway and Sant’Anna \(2021\)](#) estimator similarly suggest that the adoption of RTW laws increase deaths of despair by between 6 and 11 additional persons per 100,000. Finally, I provide evidence suggesting that the loss of private health insurance may be one mechanism by which right-to-work laws increase mortality from deaths of despair.

That right-to-work laws and declining union membership are associated with rising deaths of despair can be understood by observing that—although labor is a *variable* cost from the perspective of an individual firm—labor is an overhead cost from the perspective of society as a whole ([Prasch, 2005](#)). Firms and firm owners may perceive the gains in bargaining power and short-term profits facilitated via the decline of the “collective voice/institutional response” face of unions as individually beneficial. However, *someone* must pay the cost of workers’ subsistence. In the case of health insurance, the “cost” is not eliminated when employers cease to offer employer-sponsored health insurance plans, it is merely shifted from the firm to either (A) the public sector, via Medicaid or other cost-increases related to providing care for the uninsured, or (B) the worker(s) themselves, via increased out-of-pocket costs or deteriorating health. As [Prasch \(2005\)](#) points out, “[s]hould the

social overhead costs necessary for a ‘decent’ standard of living not be met from any source, society would still have to pay through the degeneration of its labor force and citizenry” (p.443). To the extent that lower wages and the loss of workplace public goods caused by right-to-work laws and declining union membership represent a failure to meet the social overhead costs necessary for a decent standard of living, rising deaths of despair are the additional costs that must now be paid by society as a whole.

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## A ICD-10 Codes

Table 10: Deaths of Despair — ICD-10 Codes

ICD-10 Code(s)	Description
F10	Mental and behavioural disorders due to use of alcohol.
F11	Mental and behavioural disorders due to use of opioids.
F12	Mental and behavioural disorders due to use of cannabinoids.
F13	Mental and behavioural disorders due to use of sedatives or hypnotics.
F14	Mental and behavioural disorders due to use of cocaine.
F15	Mental and behavioural disorders due to use of other stimulants, including caffeine.
F16	Mental and behavioural disorders due to use of hallucinogens.
K70	Alcoholic liver disease.
K74	Fibrosis and cirrhosis of liver.
X40	Accidental poisoning by and exposure to nonopioid analgesics, antipyretics and antirheumatics.
X41	Accidental poisoning by and exposure to antiepileptic, sedative-hypnotic, antiparkinsonism and psychotropic drugs, not elsewhere classified.
X42	Accidental poisoning by and exposure to narcotics and psychodysleptics [hallucinogens], not elsewhere classified.
X43	Accidental poisoning by and exposure to other drugs acting on the autonomic nervous system.
X44	Accidental poisoning by and exposure to other and unspecified drugs, medicaments and biological substances.
X45	Accidental poisoning by and exposure to alcohol.
X60-X84	Intentional self-harm.
Y10	Poisoning by and exposure to nonopioid analgesics, antipyretics and antirheumatics, undetermined intent.
Y11	Poisoning by and exposure to antiepileptic, sedative-hypnotic, antiparkinsonism and psychotropic drugs, not elsewhere classified, undetermined intent.
Y12	Poisoning by and exposure to narcotics and psychodysleptics [hallucinogens], not elsewhere classified, undetermined intent.
Y13	Poisoning by and exposure to other drugs acting on the autonomic nervous system, undetermined intent.
Y14	Poisoning by and exposure to other and unspecified drugs, medicaments and biological substances, undetermined intent.
Y15	Poisoning by and exposure to alcohol, undetermined intent.

Notes: Table 10 lists all ICD-10 codes used to construct state-level estimates of mortality due to “deaths of despair.” Mortality data are obtained from the CDC Multiple Cause of Death files ([Centers for Disease Control and Prevention, 2026](#)).