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ORIGINAL PAPER

Right-to-work laws, union decline, and the opioid crisis

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Abstract
A large literature links de-unionization to determinants of health, but direct evidence on objective health outcomes remains scarce. Exploiting the staggered implementation of right-to-work (RTW) laws across four US states, we quantify the role of union presence in opioid misuse. We combine the synthetic control method with staggered differences-in-differences to estimate time-varying effects. RTW adoption is followed by a persistent decline in unionization within 4 to 6 years, and this decline is associated with sizable increases in both nonfatal and fatal opioid overdoses, with no comparable changes for non-opioid drugs. Effects are concentrated among working-age men and align with channels tied to workplace conditions, including higher occupational risk, lower wages, longer hours, and elevated work stress. Taken together, the evidence indicates that the decline in unionization meaningfully contributes to the opioid crisis in affected states.

Keywords Opioid · Drug · Fatal and nonfatal overdose · Right-to-work · Unionization rate

JEL Classification I12 · I18 · J51 · K31

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1 Introduction

Union membership in the United States fell from 20.1% in 1983 to 10.1% in 2022—the lowest rate in four decades (U.S. Bureau of Labor Statistics 2023). Because unions are key labor-market institutions that mediate power between capital and labor, their erosion has been framed as a public-health concern by epidemiologists and sociologists (Wright 2016; Hagedorn et al. 2016). Unions influence a wide range of working conditions, including working hours, wages, job security, and workplace safety, all of which are fundamental determinants of health. As these protections have weakened, many communities have simultaneously faced increasing economic precarity. This broader backdrop is especially consequential given the decades-long opioid crisis that has ravaged the United States.[1] The coincidence of declining labor protections and rising opioid misuse raises the question whether institutional erosion in the labor market may have exacerbated the crisis by heightening workers' exposure to hazardous conditions, economic strain, and stressors linked to substance use.

Right-to-work (RTW) statutes emerged as state-level responses to union-security provisions authorized by the National Labor Relations Act (NLRA) of 1935. They allow employees covered by a collective-bargaining agreement to decline union membership and dues while still receiving the contract's protections, creating a classic free-rider problem (Ichniowski and Zax 1991). The resulting loss of financial resources weakens unions' bargaining capacity and, over time, depresses membership, reinforcing the downward trend in unionization. While declining unionization reflects broad macroeconomic shifts—structural change from manufacturing to services, a growing reliance on nonstandard employment arrangements, and the globalization of production—the enactment of anti-union legislation, especially RTW laws, is widely viewed as a major catalyst (Fortin et al. 2023; Murphy 2023).

As of this writing, 27 states have enacted RTW laws (Table A1). The movement accelerated over the last decade as even traditionally labor-friendly states—Indiana (2012), Michigan (2013), and Wisconsin (2015)—adopted RTW in stated efforts to attract business and investment, followed by West Virginia (2016) and Kentucky (2017). Some local governments in non-RTW states have also attempted to create "union-free" zones (e.g., cities and counties in Illinois) to circumvent state law (Flavin and Shufeldt 2016). In addition, the 2019 Supreme Court's decision in *Janus v. American Federation of State, County, and Municipal Employees* extended RTW principles to the entire public sector by prohibiting mandatory agency fees.

Trends, however, are not entirely one-way. The repeals of RTW in Missouri (2018) and Michigan (2023)—two rare reversals in more than 50 years—signal heightened concern about the erosion of labor rights in post-coronavirus disease (COVID) America. In Missouri, unions successfully framed RTW not merely as

[1] Roughly three-quarters of drug-overdose deaths now involve natural, semi-synthetic, or synthetic opioids (State Health Access Data Assistance Center 2023), and since 2011—when the Centers for Disease Control and Prevention (CDC) declared an epidemic of prescription-opioid overdoses—opioid mortality has remained among the leading causes of injury-related death (CDC 2023).

a union issue but as a threat to wages and working conditions for all workers[2]; in Michigan, political shifts combined with inconclusive evidence of RTW's economic benefits since 2012 helped motivate repeal.[3] Public approval of unions also rose during the COVID pandemic to its highest level in nearly six decades (Gallup 2022). This context makes our study especially timely: recent reversals may encourage further pro-labor initiatives, even as they galvanize opposition from business and political actors, potentially spurring efforts to reinstate, strengthen, or expand RTW.

To our knowledge, this is the first study to examine the effects of de-unionization on opioid misuse. Economic precarity can shape not only drug demand but also vulnerability to fatality. Case and Deaton (2015) first proposed that a structural shift toward a postindustrial economy contributed to the rise of "deaths of despair" among the US working class, with opioids serving as a refuge from the physical and psychological trauma of economic disadvantage, isolation, and hopelessness. Since then, several studies have reinforced the view that economic shocks—especially job loss, declining labor-market opportunities, and regional economic downturns—intensify substance use and overdose mortality. Carpenter et al. (2017) find that substance use disorders are strongly countercyclical, driven in particular by prime-aged (ages 31–64) white men with low educational attainment. Hollingsworth et al. (2017) show that opioid overdoses rise with increases in unemployment, underscoring how weakened economic conditions elevate both drug misuse and overdose deaths. Heyman et al. (2019) document strong county-level associations between socioeconomic disadvantage and overdose mortality.

While prior work has emphasized unions' effects on readily quantifiable labor-market outcomes—such as employment, wages, and productivity—the broader socioeconomic dimensions of unions' role in population health and well-being have received comparatively less attention (see Appendix A for a literature review). We contribute to this gap by estimating the effects of RTW laws and de-unionization on public health, focusing on opioid dependence—measured by the number of opioid-related admissions to substance abuse treatment facilities and overdose deaths. Our findings indicate that anti-union regulations, such as RTW laws, meaningfully contribute to the rise in opioid dependence, thereby enhancing the understanding of the economic underpinnings of the overdose epidemic.

Our findings support a small but growing literature showing that policy interventions and collective organizations can serve as effective counterweights to market forces in reducing opioid-related harms (Kravitz-Wirtz et al. 2020; Wu and

[2] In Missouri, lower than national average unemployment rates and solid job growth during the recovery following the Great Recession, without RTW, further weakened the argument that RTW was necessary for economic growth. See https://extension.missouri.edu/publications/dm3000, retrieved May 16, 2025.

[3] Michigan's adoption of RTW in 2013 was viewed by a significant proportion of residents as a renunciation of the state's identity as a historical stronghold of the American labor movement, not to mention its working-class traditions. To them, the repeal was a "correction of an anomaly imposed by a specific political moment." Thousands of union supporters descended on the State Capitol to protest in 2012 when the Republican-controlled Statehouse pushed the RTW legislation through without hearings. See https://apnews.com/article/right-to-work-repeal-michigan-democrats-b4304a2780909d37e76f211c7b070a6b, retrieved May 16, 2025.

Evangelist 2022). Two studies relating union density to drug-related mortality suggest that declining unionization exacerbates drug misuse (DeFina and Hanon 2019; Eisenberg-Guyot et al. 2020).[4] However, whether this association is causal or instead reflects unobserved state-level differences remains unclear.[5]

We address this concern by exploiting a plausibly exogenous source of variation in unionization rates—the staggered enactment of RTW laws in four US states between 2001 and 2015: Oklahoma (2001), Indiana (2012), Michigan (2013), and Wisconsin (2015). To conduct our analysis, we assemble a rich dataset from the Treatment Episode Data Set (TEDS), the Centers for Disease Control and Prevention Multiple Cause of Death (MCOD), the Current Population Survey (CPS), the Union Membership and Coverage Database, the Bureau of Labor Statistics (BLS) Survey of Occupational Injuries and Illness (SOII), and the Census of Fatal Occupational Injuries (CFOI).

We implement a variant of the synthetic control method (SCM; Abadie and Gardeazabal 2003) that accommodates multiple treated units and staggered adoption (Cavallo et al. 2013; Galiani and Quistorff 2017), and we complement it with staggered differences-in-differences (DiD) estimators (Callaway and Sant'Anna 2021). A key advantage of SCM is that it does not require a parallel-trends assumption; instead, it exploits rich pre-RTW information to closely match pre-trends between each treated (switcher) state and its synthetic control. This design suits our setting: we analyze a small number of treated/switcher states—2 or 3, depending on the outcome—and a limited set of never-treated states. We also observe 9–12 years of pre-treatment outcomes and covariates, which enable us to construct credible synthetic controls and to directly assess pre-treatment fit. This ability to explicitly evaluate and demonstrate pre-treatment balance makes SCM compelling in our case. SCM is, however, relatively data-intensive and sensitive to implementation choices. By contrast, staggered DiD relies on a strong parallel-trends assumption but is otherwise straightforward and standardized. The close agreement between the two approaches strengthens the credibility of our conclusions.

Beyond mortality, we examine the relationship between RTW laws and nonfatal overdoses—an outcome largely neglected in the existing literature. Overdose survivors may face elevated risks of subsequent overdose (Zibbell et al. 2019). In addition, nonfatal opioid overdoses are associated with respiratory depression and

[4] Comparing drug-related mortality across states, which aggregates deaths classified as "unintentional," "intentional," and "underdetermined," DeFina and Hanon (2019) find that a decline in union density increases drug deaths through a standard two-way fixed-effects model. Eisenberg-Guyot et al. (2020) reach a similar conclusion for overdose and suicide deaths while finding a null effect of de-unionization on all-cause mortality.

[5] The fact that union density is influenced by local economic conditions and political factors, which, in turn, impact community health outcomes may confound this relationship. For example, Pierce and Schott (2020) find that mortality from suicide and accidental poisoning rises with import competition as jobs disappear from US manufacturing due to outsourcing or technology replacing skilled workers. Charles et al. (2019) document the detrimental effect of a shrinking manufacturing sector on opioid-related mortality. If import competition or manufacturing employment correlates with union density, then failing to account for these factors in the analyses may lead to a biased estimate of the effects of de-unionization on overdose deaths.

hypoxic brain injury, which can precipitate additional complications, severe disability, short-term memory loss, and changes in cognitive and physical functioning. Excluding nonfatal events therefore likely understates the drug-related harms attributable to de-unionization.

Finally, we explore plausible pathways that could link anti-union regulation (RTW laws) to overdose behavior. While our reduced-form design does not identify mechanisms, the patterns we document under RTW suggest several potential risk channels; these results can guide future research on the well-being consequences of de-unionization.

Previewing our results, we present strong evidence that declining unionization has contributed to the opioid crisis. In states that enacted RTW laws, fatal and nonfatal opioid overdoses rise persistently within 4 to 6 years after enactment, following sustained declines in unionization. By contrast, we detect no effect on the misuse of non-opioids (e.g., sedatives, stimulants, antidepressants, and cocaine). The RTW-induced increase in opioid dependence appears concentrated among working-age males, who are more likely to participate in the labor market, work in hazardous conditions, and experience stress arising from lower job security and job control. Drawing on analyses of workplace-specific outcomes, we identify four plausible channels for the RTW gradient in opioid harms: increased occupational hazards, reduced wages, longer working hours, and greater work-related stress.

We verify our results through extensive robustness checks. The timing and magnitude of the RTW effects closely match those obtained from staggered DiD estimators.[6] We also find no evidence that the results are driven by sparse synthetic-control weights or overfitting (Abadie 2021; Abadie and Vives-i-Bastida 2022). Nor do they appear to be explained by the reformulation of OxyContin (Alpert et al. 2018), concurrent policies such as triplicate programs (Alpert et al. 2022), prescription drug monitoring programs (Buchmueller and Carey 2018), medical marijuana laws (Powell et al. 2018), measurement error in overdose mortality data (Alpert et al. 2022), or the entry of illicit drugs. We further probe alternative predictor sets, definitions of opioid misuse, and placebo outcomes (e.g., deaths from breast cancer and influenza/pneumonia) to support these conclusions.

These findings connect to a broader literature documenting how de-unionization affects key determinants of health: depressing wages (Card et al. 2020; Fortin et al. 2023), increasing income inequality (Farber et al. 2021), reducing access to health care (Buchmueller et al. 2002), compromising workplace safety (Zoorob 2018), and lowering subjective well-being (Artz et al. 2022; Chen and Islam 2023). Outside occupational health and safety, most studies directly linking de-unionization to objective health outcomes are qualitative or theoretical (Hagedorn et al. 2016) or focus exclusively on union members (Reynolds and Brady 2012), limiting external validity given the shrinking union share and potentially different effects on non-union workers. From this perspective, our study provides the first evidence on how

[6] This result holds even when we incorporate switcher states that do not meet the data requirements for SCM, and are, therefore, excluded from the main analysis (Sect. 6.1).

RTW laws affected the general public during a devastating, substance-misuse-driven health crisis that coincided with the period of greatest RTW expansion.

This paper proceeds as follows. Section 2.1 describes the data, institutional background, and estimation strategy. Sections 2.3 and 3.2 present the results from the SCM and conduct robustness checks. Section 4.3 replicates the main findings using a staggered DiD approach. Sections 4.4 and 6.1 explore the underlying heterogeneity and mechanisms. Section 7.4 concludes.

2 Data, method, and sample construction

2.1 Data

Data on admissions to substance use treatment facilities come from the 1992–2018 TEDS maintained by the Substance Abuse and Mental Health Services Administration (SAMHSA). Since 1992, SAMHSA has required states to report admissions from publicly funded facilities. This mandate covers facilities supported by federal block grants, Medicare/Medicaid, or state funds, regardless of whether they also serve privately insured or self-pay patients. Facilities serving only privately insured or cash-pay patients are not in the sampling frame. Nonetheless, national spending accounts indicate that the public sector has financed more than 75% of US substance use treatment since 1998 (Powell et al. 2018), so TEDS should capture the vast majority of admissions. A technical limitation is incomplete annual reporting by some states. To construct a balanced panel, we exclude six states with missing years, including one switcher (Indiana) as listed in Table A2. As shown in our SCM and staggered DiD analyses, excluding these states has little effect on pre-RTW fit or on the main results.

We define opioid-related admissions as cases in which the primary substance is heroin, non-prescription methadone, or "other opioids and synthetics." The latter category includes commonly misused analgesics such as oxycodone and fentanyl. Following prior work (Powell et al. 2018), we group heroin with other opioids and synthetics for the main analysis; we also examine the "other opioids and synthetics" category separately as a robustness check in Appendix C.

Fatal overdose data come from the CDC MCOD files for 1999–2018. MCOD records the single underlying cause of death and can list up to 20 additional contributing causes using the International Classification of Diseases, Tenth Revision (ICD-10) codes. We identify drug overdose deaths using a broad definition in which the underlying cause falls under accidental poisoning (X40–X44), intentional self-poisoning (X60–X64), assault (X85), or poisoning of undetermined intent (Y10–Y14), irrespective of intent. We then classify an opioid-involved overdose as any case with an opioid listed among the contributing causes: opium (T40.0), heroin (T40.1), natural and semisynthetic opioids (T40.2), methadone (T40.3), synthetic opioids (T40.4), or other/unspecified narcotics (T40.6). Given challenges in substance-specific coding—especially for synthetics—this broad definition reduces classification error (Alpert et al. 2022). We also report results using two narrower definitions, including one that excludes heroin and synthetic opioids such as fentanyl

(Appendix C). Because the CDC advises caution when underlying death counts are below 20, we exclude four never-RTW states from the main mortality analysis (Table A2).

Across outcomes, we find consistent patterns between admissions and deaths. Because mortality is less susceptible to reporting and coverage differences, the close correspondence suggests that any omissions in TEDS due to licensing or payment-system variation are unlikely to drive the observed opioid-misuse gaps.

Additional state-level characteristics are compiled from several sources. Effective dates of RTW legislation are drawn from the National Right to Work Committee (Table A1). Population denominators for TEDS-based rates are from Federal Reserve Economic Data. Socioeconomic covariates come from the CPS Annual Social and Economic Supplement via Integrated Public Use Microdata Series (IPUMS; Flood et al. 2025); because CPS income refers to the prior calendar year, we lag income variables by 1 year and, where relevant, deflate to 1999 dollars using the Consumer Price Index (CPI).[7]

Finally, we use two additional data sources to study first-stage outcomes and mechanisms through which RTW may affect opioid misuse. The Union Membership and Coverage Database provide state-year union density measures based on the CPS using the same methodology as BLS national estimates (Hirsch et al. 2001). Workplace injury and fatality measures are taken from the BLS Survey of Occupational Injuries and Illnesses (SOII) and the CFOI.

2.2 Method

We estimate the effect of RTW laws on opioid misuse using SCM. For each switcher state, SCM constructs a counterfactual outcome path as a weighted average of "never-RTW" donor states chosen to replicate the treated state's pre-RTW trajectory. To reduce idiosyncratic noise and summarize effects across units, we follow the multiple-treated-unit implementations in Cavallo et al. (2013) and Galiani and Quistorff (2017): for each switcher, we match to its synthetic counterpart, compute year-by-year post-RTW treatment effects, and average these unit-level effects to obtain an average treatment effect at each event-time year.

As a principled baseline, we consider specifications that use the full set of pre-treatment outcomes as predictors. Under standard interactive-fixed-effects assumptions, using many lagged outcomes can recover the relevant factors, and as the length of the pre-period grows, SCM based on an expanding set of pre-treatment outcomes converges to the same limit as alternatives that include additional covariates (see Ferman et al. (2020) for related asymptotic discussions). This baseline also harmonizes implementation across outcomes and avoids discretionary covariate selection.

Our preferred approach, however, is parsimonious: we use a small set of pre-treatment outcomes together with any predictive covariates, with predictor importance chosen via the standard nested optimization over predictor weights, V, and donor

7 https://cps.ipums.org/cps/cpi99.shtml, retrieved July 30, 2022.

weights, W (Abadie 2021; Abadie and Vives-i-Bastida 2022). Concretely, for each outcome, we begin with a compact set of pre-treatment outcomes and include strong covariates, i.e., only those that lower pre-treatment mean-squared prediction error (pre-MSPE). With a moderate pre-period, including every yearly lag is often unnecessary and can make the donor-weight solution numerically unstable (many nearly equivalent convex combinations fit the pre-path) while leaving pre-fit essentially unchanged. Including strong covariates also improves finite-sample performance: when omitted, their explanatory power is absorbed by unobserved factor loadings, which can increase bias and degrade finite-sample properties (Abadie and Vives-i-Bastida 2022).

This procedure minimizes in-sample MSPE, which may risk overfitting. To probe this risk, we perform a validation exercise that splits the pre-period into a training window and a holdout window (Sect. 3.3). We estimate synthetic controls using the training period and assess out-of-sample fit by predicting the switcher states' counterfactual outcomes in the holdout period. The validation results support the choice of our preferred predictor sets.

2.3 Sample construction

In applying a multiple-treatment-unit SCM, we face a trade-off between the length of the event window and the representativeness of the treated sample. Shorter windows allow inclusion of more switcher states but at the cost of truncated post-treatment dynamics. To balance these considerations, we construct two balanced event-time panels. The first panel follows treated states for 4 years after RTW and includes all switchers with sufficient data—Oklahoma, Michigan, and Wisconsin for nonfatal overdoses, captured by admissions to substance abuse treatment facilities and Indiana, Michigan, and Wisconsin for fatal overdoses, captured by overdose deaths. The second panel follows treated states for 6 years after RTW and focuses on early adopters with longer post-treatment windows: Oklahoma and Michigan for nonfatal overdoses, and Indiana and Michigan for fatal overdoses. Although the 6-year panel covers fewer switchers, it provides a longer horizon to detect delayed effects, which are plausible in this setting given the time required to renegotiate collective bargaining agreements and for changes in job quality to diffuse. While the NLRA does not set contract length, agreements are time-limited in practice; the average term is roughly 3 years, with many recent contracts extending to 4 years or 5 years (Compa 2014).

Data availability also constrains the event windows: TEDS begins in 1992 and MCOD in 1999. For nonfatal overdoses, the event windows extend 9 years before RTW and 4 to 6 years after RTW. In contrast, for fatal overdoses, the event windows span 13 years before RTW and then 4 to 6 years after RTW. The shorter pre-period for nonfatal overdoses compared to fatal overdoses is due to missing values in relevant datasets (Table A2). We exclude states that enacted RTW before our observation period and states that adopted RTW during the period but lack sufficient post-treatment data. The complete set of potential donor states is provided in the footnote to Table A1.

2.4 Descriptive statistics

Panels A and B of Table 1 report summary statistics for our main outcomes. Over 1992–2018 (TEDS) and 1999–2018 (MCOD), mean opioid-related admissions and overdose deaths are 214 and 9.5 per 100,000, representing 49% and 65% of all drug-related admissions and deaths, respectively. Cross-state dispersion is markedly larger for opioids than for non-opioid analgesics—224 vs. 107 for admissions and 7.3 vs. 2.6 for deaths. Switcher and never-RTW states have broadly similar outcome compositions, but switchers display lower baseline levels of misuse (e.g., 6.6 vs. 9.2 deaths per 100,000 in the 6-year follow-up sample).

Panels C and D summarize changes between the first and last years of observation. Although switchers start from lower incidence than never-RTW states, they experience much faster growth in both nonfatal and fatal overdoses— + 476% vs. + 381% (nonfatal) and + 1713% vs. + 378% (fatal) in the 6-year panel. Notably,

Table 1 Descriptive statistics of primary outcomes

	Never-RTW states	Switchers (4-year follow-up)	Switchers (6-year follow-up)
Panel A: Admission rate, per 100,000 people (TEDS 1992–2018)			
Opioid	237.37	72.07	90.89
(SD)	(232.05)	(74.85)	(83.17)
Non-opioid	235.72	162.04	206.57
(SD)	(107.85)	(74.91)	(44.64)
All drugs	473.09	234.11	297.45
Panel B: Mortality rate, per 100,000 people (MCOD 1999–2018)			
Opioid	9.18	6.90	6.61
(SD)	(6.76)	(5.09)	(5.46)
Non-opioid	4.84	5.80	6.82
(SD)	(2.56)	(2.44)	(2.36)
All drugs	14.02	12.71	13.43
Panel C: Changes in opioid-related admissions (1992–2018)			
Admission rate in 1992	91.97	21.92	32.23
Admission rate in 2018	442.18	156.30	185.49
% Change between 1992 and 2018	380.79	613.05	475.52
States included	18 States	OK, MI, and WI	OK and MI
Panel D: Changes in opioid-related mortality (1999–2018)			
Mortality rate in 1999	4.13	1.2	1
Mortality rate in 2018	19.74	17.07	18.13
% Change between 1999 and 2018	377.97	1322.5	1713
States included	19 States	IN, MI, and WI	IN and MI

The means and standard deviations (SDs) of the main outcomes analyzed in the paper are presented. Potential donors (never-RTW states) for each outcome are listed in Table A1

OK Oklahoma, *MI* Michigan, *WI* Wisconsin, *IN* Indiana the 4- and 6-year follow-up samples exhibit very similar qualitative patterns despite differences in state composition.

Figure 1 plots unadjusted trends separately for each switcher and the never-RTW group. Prior to RTW adoption, neither outcome in switcher states moves in parallel with the never-RTW group, rendering a canonical differences-in-differences comparison inappropriate and motivating our use of synthetic control.

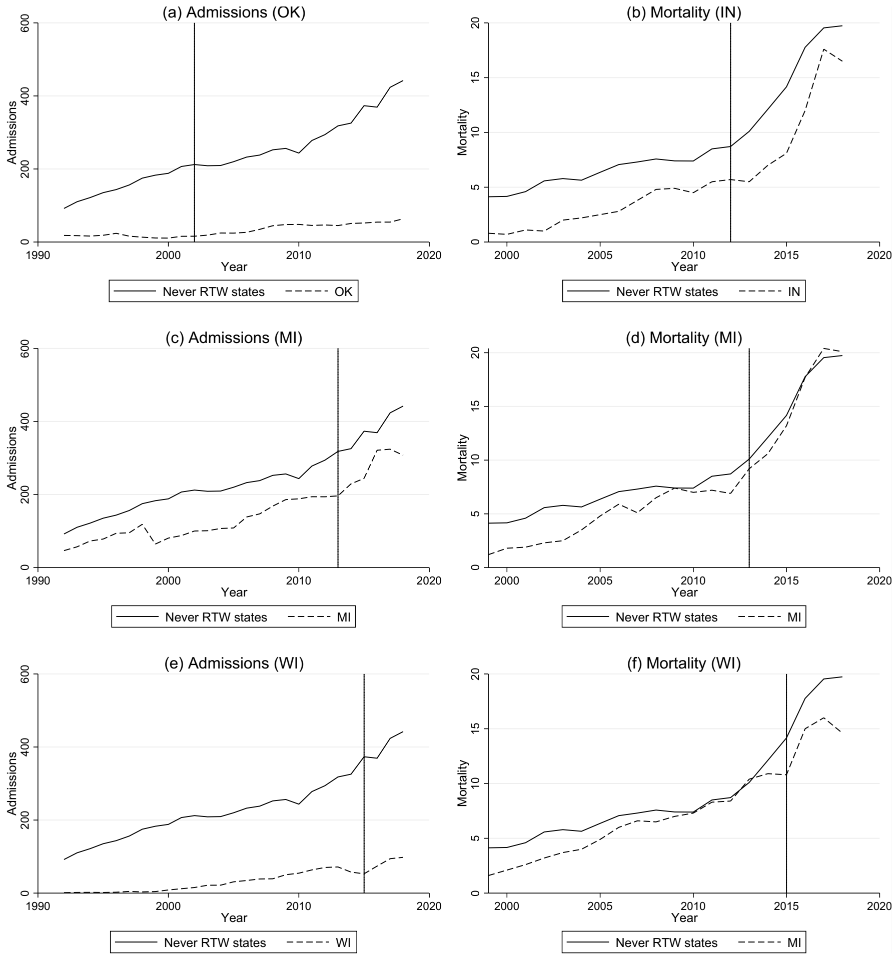

Fig. 1 Unadjusted trends in opioid misuse. This figure plots opioid-related admissions (**a**, **c**, **e**) and mortality (**b**, **d**, **f**) for the switcher states using TEDS (1992–2018) and MCOD (1999–2018), respectively. The vertical line marks the year prior to RTW adoption

3 Main SCM results

We report results from our preferred SCM specification, which uses a parsimonious set of pre-treatment outcomes and adds covariates only when doing so lowers pre-MSPE under the nested SCM optimization. Parallel results using all pre-treatment outcomes are quantitatively similar. They are provided in Appendix B for brevity.

3.1 Unionization rates

The free-rider incentives created by RTW laws reduce unions' financial resources and organizing capacity. A direct and immediate response to RTW should therefore be declining unionization. We begin by estimating the de-unionization effects for the adopters in our sample using 1990–2018 CPS. Although CPS data prior to 1990 are excluded due to known issues, the analysis uses all available states thereafter, including 23 never-RTW states and the four switcher states.[8]

Figure 2a–d shows good pre-period fit over the 11 years preceding RTW: each synthetic control closely tracks the level and trend of the treated state's unionization rate. Following RTW enactment, a negative gap opens and widens over time. Panel A of Table 2 reports average treatment effect estimates indicating an average decline of 1–2 percentage points (ppt), or 10–17% of baseline unionization rates, 4 to 6 years after adoption. The magnitudes are similar whether unionization is measured by membership or by contract coverage. Panel B of Table 2 shows that the year-by-year estimates follow a comparable dynamic pattern across both outcomes.

Because a longer pre-intervention window can improve the reliability of synthetic control estimates, Fig. 2e–h replicates the analysis for the states with 22 years of pre-RTW data—Indiana, Michigan, and Wisconsin. In these cases, the estimated reductions in union density are larger, on the order of 1–3 ppt, or 10–20%, by years 4 to 6 after adoption.

Table A4 reports the optimal donor weights for a representative specification (union membership, 4-year follow-up sample). As expected, sparsity obtains and only a small number of donors receive positive weight. Sensitivity to the donor pool is assessed in Sect. 4.

Finally, we find no evidence of delayed responses in unionization following RTW. This contrasts with the timing observed for opioid outcomes, particularly mortality (Sects.3 and 3.1). Interpreting unionization as a proxy for union strength, these results indicate that RTW substantially weakened bargaining power in the switcher states before the subsequent increase in opioid misuse.

8 See https://cps.ipums.org/cps-action/variables/UNION#comparability_section, retrieved January 31, 2024.

Fig. 2 Union membership (**a**, **b**, **e**, **f**) and coverage rates (**c**, **d**, **g**, **h**; SCM). Panels **a**–**d** and **e**–**h** report estimated RTW effects on unionization rates using 11 and 22 years of CPS pre-treatment data (1990–2018), respectively. In the 11-year analysis, the switcher states are Oklahoma, Indiana, Michigan, and Wisconsin for the 4-year follow-up sample, and Oklahoma, Indiana, and Michigan for the 6-year follow-up sample. In the 22-year analysis, Indiana, Michigan, and Wisconsin enter the 4-year sample, and Indiana and Michigan enter the 6-year sample. The vertical line marks the year prior to RTW adoption

3.2 Admissions

Figure 3 reports estimated RTW effects on admissions involving opioids (top) and non-opioids (bottom). Within each panel, the left plot shows event-time average treatment effects for all available switchers (Oklahoma, Michigan, Wisconsin), and the right plot shows effects for the two early adopters (Oklahoma, Michigan). Corresponding estimates appear in Table 3. While we find a negative effect in the year following RTW adoption in treated states, when considering the 4-year and 6-year windows, opioid-related admissions increase by 9 and 16 cases per 100,000 within 4 and 6 years of RTW enactment, respectively—11% and 16% relative to the level 1 year prior to adoption in the treated states.

Despite year-to-year variability, both samples show a clear upward trajectory in the treated synthetic gap. By year 4, 40–41 cases per 100,000 (39–47% of baseline admissions) would have been avoided absent RTW; by year 6, the corresponding effect is 42 cases (41% of baseline).

In contrast, we find little evidence that RTW affects admissions involving non-opioid drugs (e.g., sedatives, stimulants, antidepressants, and cocaine). Although the treated synthetic gap is positive in some years, the overall effects are not statistically significant at the 5% level in either sample (Table 3, columns 3 and 4).

3.3 Mortality

Figure 4 presents mortality results that mirror the admission findings. In both samples, opioid-related deaths in the switcher states—Indiana, Michigan, and Wisconsin in the 4-year follow-up sample, and Indiana and Michigan in the 6-year follow-up sample—track their synthetic controls closely over the 13-year pre-RTW window. Pre-RTW match quality, measured by the proportion of placebos with a pre-intervention root-mean-squared prediction error (RMSPE) at least as large as the average of the treated units, is effectively perfect (Table 4, columns 1 and 2). Following RTW adoption, a visible treated synthetic gap opens and widens, with average increases of 1–4 deaths per 100,000, corresponding to 10–60% relative to pre-RTW levels. For context, over the same period, the switcher states experienced sharp secular increases in opioid mortality—211% in the 4-year sample and 1,695% in the 6-year sample—so the RTW effects represent a nontrivial share of large underlying trends.

The timing is also informative. Where RTW effects are positive and significant, mortality responses lag admission effects by about 2 years. This pattern is consistent with a progression from nonfatal to fatal overdose risk, potentially reflecting

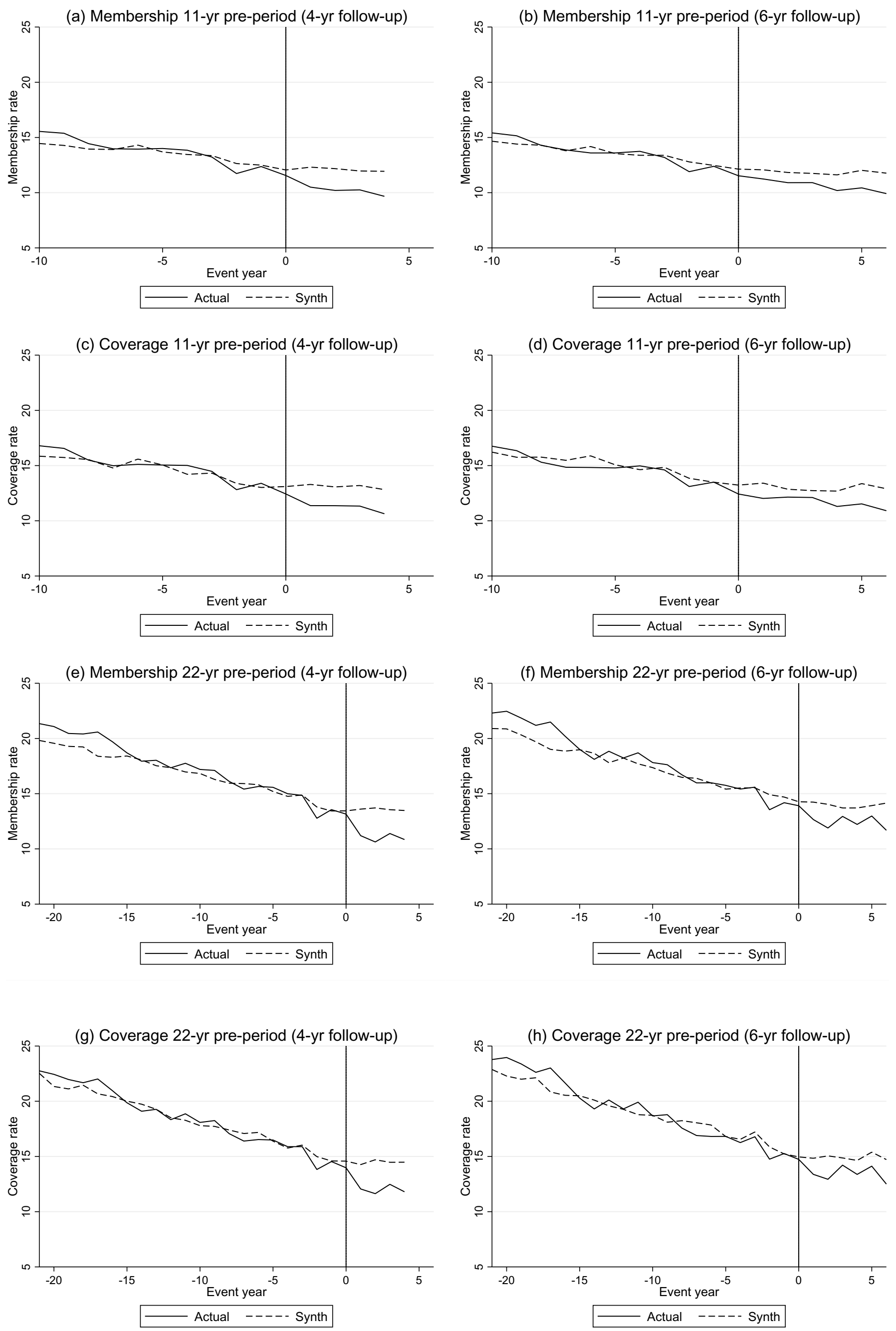
(a) Membership 11-yr pre-period (4-yr follow-up)
(b) Membership 11-yr pre-period (6-yr follow-up)
(c) Coverage 11-yr pre-period (4-yr follow-up)
(d) Coverage 11-yr pre-period (6-yr follow-up)
(e) Membership 22-yr pre-period (4-yr follow-up)
(f) Membership 22-yr pre-period (6-yr follow-up)
(g) Coverage 22-yr pre-period (4-yr follow-up)
(h) Coverage 22-yr pre-period (6-yr follow-up)
Membership rate
Coverage rate
Event year
Actual
Synth

Table 2 Union membership and coverage rates (11-year pre-period) (SCM)

	Union membership		Union coverage	
	4-year follow-up (1)	6-year follow-up (2)	4-year follow-up (3)	6-year follow-up (4)
Panel A: overall estimates				
Average treatment effect	– 1.939*** (0.000)	– 1.241** (0.038)	– 1.917*** (0.001)	– 1.322* (0.057)
Panel B: yearly estimates				
1 year after	– 1.809*** (0.000)	– 0.828*** (0.001)	– 1.919*** (0.001)	– 1.384** (0.021)
2 years after	– 1.972*** (0.000)	– 0.922*** (0.006)	– 1.700*** (0.006)	– 0.712 (0.164)
3 years after	– 1.711*** (0.001)	– 0.836* (0.074)	– 1.861*** (0.004)	– 0.621 (0.190)
4 years after	– 2.264*** (0.000)	– 1.421*** (0.002)	– 2.186*** (0.000)	– 1.390** (0.012)
5 years after		– 1.586** (0.010)		– 1.836** (0.010)
6 years after		– 1.853*** (0.005)		– 1.990*** (0.008)
Pre-RTW match quality	0.961	0.986	0.966	0.946
Pre-RTW unionization rate	11.56	11.53	12.45	12.43
# of potential donors	23	23	23	23

SCM estimates with standardized placebo-based *p* values in parentheses are reported. The estimates use CPS 1990–2018, pooling Oklahoma, Indiana, Michigan, and Wisconsin for the 4-year follow-up sample, and Oklahoma, Indiana, and Michigan for the 6-year follow-up sample. Pre-RTW match quality is measured by the proportion of placebo states with a pre-intervention RMSPE at least as large as the average RMSPE for the treated units. The pre-RTW unionization rate is the number of individuals belonging to a union or covered by a union contract per 100 workers in the switcher states, measured 1 year before RTW passage for each respective sample. The predictors used in each model are listed in Table A5

*$p<0.10$; **$p<0.05$; ***$p<0.01$

cumulative exposure, dose escalation, or polydrug use that raises the probability of a fatal event.

By contrast, estimated RTW effects on non-opioid drug mortality attenuate over time. The time-averaged effects are not statistically significant at conventional levels in either sample (Table 4, columns 3 and 4; bottom panels of Fig. 4).

4 SCM robustness

We structure our SCM robustness analysis around the requirements in Abadie (2021) and the opioid-misuse literature. We assess (i) potential overfitting and out-of-sample generalization, (ii) sensitivity to alternative donor pools, and (iii) trimming rules

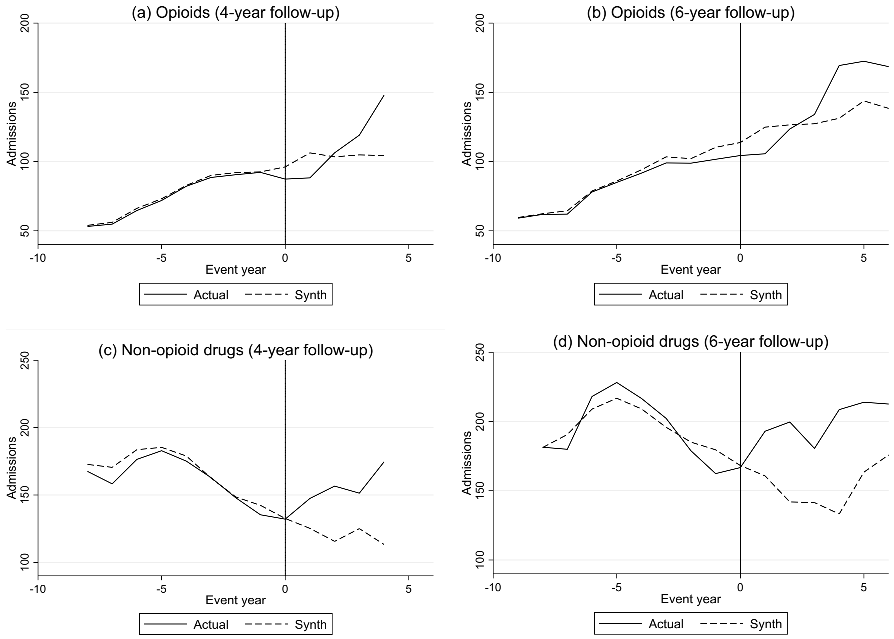

Fig. 3 Admissions (SCM). The left and right panels present the estimated RTW effects on opioid-related (**a**, **b**) and non-opioid-related (**c**, **d**) admissions for the switcher states in our 4-year follow-up sample (Oklahoma, Michigan, and Wisconsin) and 6-year follow-up sample (Oklahoma and Michigan), respectively, using 1992–2018 TEDS data. The vertical line marks the year prior to RTW adoption

that exclude donors with concurrent programs or other confounding factors. We also conduct falsification tests using mortality types unlikely to be affected by RTW. Two additional checks—examining the potential confounding role of the OxyContin reformulation and assessing measurement error using alternative definitions of opioid misuse—are reported in Appendix C. For brevity, we present results from the 6-year follow-up sample here.[9]

4.1 Overfitting and generalization

We select predictors and construct synthetic-control weights to deliver a close pre-fit between each switcher state and its synthetic counterpart. While close pre-fit is essential, it does not by itself ensure out-of-sample accuracy, especially when the pre-intervention window is modest (Abadie and Vives-i-Bastida 2022). To assess potential overfitting and the generalization of our SCM, we implement a placebo-in-time ("backdating") exercise following Abadie and Vives-i-Bastida (2022). Specifically, we reassign RTW implementation to approximately the midpoint of the

9 Results from a 4-year follow-up sample are qualitatively similar and available upon request.

Table 3 Admissions (SCM)

	Opioids		Non-opioid drugs	
	4-year follow-up (1)	6-year follow-up (2)	4-year follow-up (3)	6-year follow-up (4)
Panel A: overall estimates				
Average treatment effect	9.352*** (0.000)	16.380*** (0.006)	37.702 (0.169)	48.608* (0.058)
Panel B: yearly estimates				
1 year after	− 18.496*** (0.000)	− 19.757*** (0.000)	22.128*** (0.000)	32.236*** (0.000)
2 years after	2.093*** (0.002)	− 0.096*** (0.000)	41.012*** (0.002)	57.693*** (0.000)
3 years after	12.460*** (0.000)	4.100*** (0.000)	26.317** (0.031)	39.102*** (0.003)
4 years after	41.352*** (0.001)	39.780*** (0.000)	61.351** (0.014)	75.359*** (0.003)
5 years after		32.561*** (0.000)		50.500** (0.012)
6 years after		41.501*** (0.000)		36.757 (0.238)
Pre-RTW match quality	1.000	1.000	0.992	0.997
Pre-RTW unionization rate	87.36	102.28	132.11	166.73
# of potential donors	18	18	18	18

SCM estimates with standardized placebo-based *p* values in parentheses are reported. The estimates use TEDS 1992–2018, pooling Oklahoma, Michigan, and Wisconsin for the 4-year follow-up sample, and Oklahoma and Michigan for the 6-year follow-up sample. Pre-RTW match quality is measured by the proportion of placebo states with a pre-intervention RMSPE at least as large as the average RMSPE for the treated units. The pre-RTW admission rate is the average number of admissions per 100,000 people in the switcher states, measured 1 year before RTW passage for each sample. The predictors used in each model are listed in Table A5

*$p<0.10$; **$p<0.05$; ***$p<0.01$

pre-period—3–5 years earlier for opioid-related admissions and 5–7 years earlier for overdose deaths—and shift the predictor set accordingly.

Figure A3 shows no evidence of spurious treatment effects when the intervention is artificially backdated: synthetic and actual outcomes track closely until the true enactment date, after which the estimated effects emerge. This pattern indicates that our SCM fit is not driven by overfitting and supports the out-of-sample predictive validity of our analysis.

4.2 Donor-pool sensitivity

A defining feature of SCM is sparsity—only a few donors receive positive weights (Abadie 2021), as confirmed in Sect. 2.3.1. This naturally raises the concern that results might be driven by a small subset of donors. To assess sensitivity, we conduct a leave-one-out exercise following Abadie (2021): we re-estimate the original SCM for each treated state while iteratively omitting one donor at a time. If excluding a

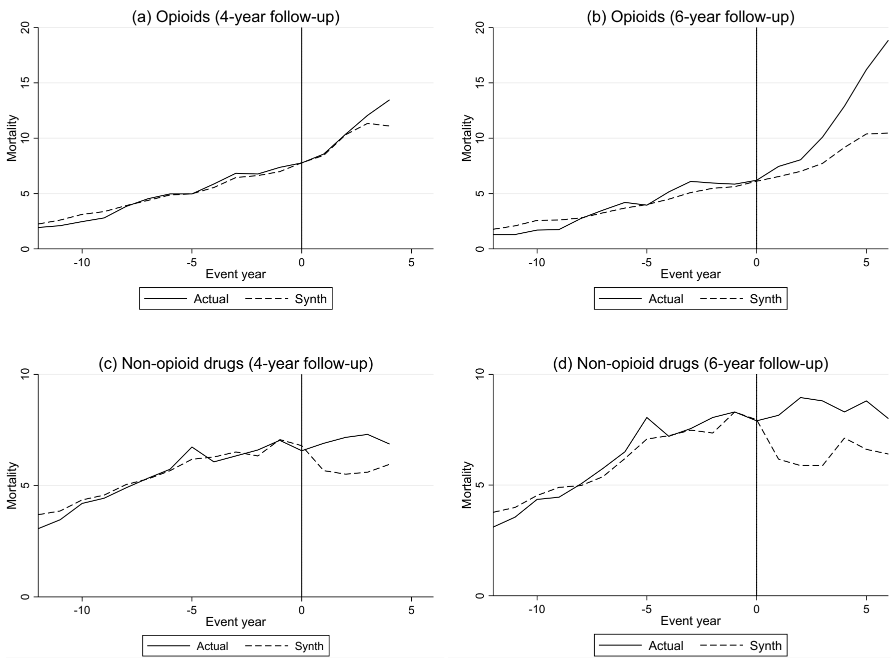

Fig. 4 Mortality (SCM). The left and right panels present the estimated RTW effects on opioid-related (a, b) and non-opioid-related (c, d) mortality for the switcher states in our 4-year follow-up sample (Indiana, Michigan, and Wisconsin) and 6-year follow-up sample (Indiana and Michigan), respectively, using 1999–2018 MCOD data. The vertical line marks the year prior to RTW adoption

donor materially changes the estimated effects without a corresponding deterioration in pre-fit (e.g., pre-MSPE), this may indicate confounding from other interventions or large idiosyncratic shocks in the excluded donor.

Figure A4 reports the results. The leave-one-out synthetic paths closely track the baseline synthetic unit, and all variants continue to show a pronounced rise in opioid misuse within 6 years of RTW enactment. In short, our findings are not driven by any single donor state.

4.3 Concurrent programs and other confounders

Several policies in place during our observation window could confound the estimated RTW effects. First, three never-RTW states—California (1992–2004), Illinois (1992–2000), and New York (1992–2001)—operated triplicate prescription programs, which have documented long-run effects on opioid overdose mortality (Alpert et al. 2022). Second, Prescription Drug Monitoring Programs (PDMP) were implemented in 15 never-RTW states and in two switcher states—Indiana (1998) and Wisconsin (2013)—and may materially affect opioid use and related harms. Although evidence on PDMPs is mixed, point-of-care use mandates can reduce misuse (e.g., Buchmueller and Carey 2018). Third, medical marijuana

Table 4 Mortality (SCM)

	Opioids		Non-opioid drugs	
	4-year follow-up (1)	6-year follow-up (2)	4-year follow-up (3)	6-year follow-up (4)
Panel A: overall estimates				
Average treatment effect	0.815*** (0.000)	3.711*** (0.000)	1.589 (0.113)	2.158 (0.107)
Panel B: yearly estimates				
1 year after	0.119 (0.750)	0.914 (0.393)	1.293*** (0.006)	1.985*** (0.000)
2 years after	0.052 (0.729)	1.047 (0.607)	1.930** (0.017)	3.072*** (0.000)
3 years after	0.726 (0.927)	2.374 (0.188)	1.820** (0.023)	2.925*** (0.003)
4 years after	2.364 (0.398)	3.731 (0.172)	1.312 (0.396)	1.176 (0.881)
5 years after		5.814** (0.042)		2.190 (0.324)
6 years after		8.387*** (0.000)		1.601 (0.357)
Pre-RTW match quality	1.000	1.000	0.946	0.814
Pre-RTW unionization rate	7.77	6.20	6.57	7.9
# of potential donors	19	19	19	19

SCM estimates with standardized placebo-based *p* values in parentheses are reported. The estimates use MCOD 1999–2018, pooling Indiana, Michigan, and Wisconsin for the 4-year follow-up sample, and Indiana and Michigan for the 6-year follow-up sample. Pre-RTW match quality is measured by the proportion of placebo states with a pre-intervention RMSPE at least as large as the average RMSPE for the treated units. The pre-RTW mortality rate is the average number of deaths per 100,000 people in the switcher states, measured 1 year before RTW passage for each sample. The predictors used in each model are listed in Table A5

** $p < 0.05$; ***$p < 0.01$

laws (MMLs)—including Michigan's 2008 adoption—may influence opioid outcomes; related work links MMLs to reductions in opioid-related harms, particularly where dispensaries are permitted (Powell et al. 2018). Given plausible overlap between populations affected by these policies and our treatment context, such programs could mediate or confound observed effects.

To assess potential bias from these concurrent programs, we trim the SCM donor pool so that, for each switcher state, donors match the presence of triplicate programs, PDMPs, and MMLs. Results (Table A8) closely align with our main estimates in Tables 3 and 4; effect sizes are somewhat larger when triplicate-program states are excluded from the donor pool.

Finally, our estimates could be distorted if donors include states from the Appalachian region—disproportionately affected by the opioid crisis (e.g., Maryland, New York, Ohio, Pennsylvania)—or states neighboring a treated state, given potential interstate spillovers in preferences, policies, and local labor markets. We therefore re-estimate SCM excluding donors in the Appalachian region and, separately, excluding donors that border a treated state. The results remain largely robust to these exclusions.

4.4 Falsification tests

To further assess the validity of our SCM procedure and findings, we examine outcomes unlikely to be affected by RTW. Specifically, we consider mortality from breast cancer and from influenza/pneumonia. These causes of death primarily affect older adults (CDC 2008, 2024), whose misuse behavior is less likely to be influenced by de-unionization (Sect. 4.3). Accordingly, a null or negative RTW effect on these outcomes would suggest that our main results are not driven by omitted factors such as differences in healthcare infrastructure.

Figure A5 and Table A9 show that RTW switchers and their synthetic controls follow closely aligned trajectories prior to RTW enactment, and no significant RTW effects emerge in the post-period. These placebo-in-outcomes results bolster the credibility of our main estimates.

Two additional robustness checks appear in Appendix C. First, we assess the role of the OxyContin reformulation by focusing on Michigan, a switcher state with comparatively low initial OxyContin exposure. Second, we address potential measurement errors in the mortality data by considering alternative definitions of opioid misuse. In both cases, our results remain robust.

5 Comparing SCM with staggered DiD

As a complement to the SCM analysis, we now turn to staggered DiD, which provides a useful robustness check and allows us to compare results across two widely used approaches. Staggered DiD (see, e.g., Callaway and Sant'Anna (2021)) leverages variation in RTW timing across states to estimate its effects under staggered adoption—that is, different states may adopt RTW at different times, and once adopted, the policy remains in place for the rest of the observation window. This matches the structure of our data. The Callaway and Sant'Anna estimators proceed by estimating cohort-specific DiD effects using never-treated and/or not-yet-treated states as controls and then aggregating across cohorts. This procedure avoids the main drawback of the standard two-way fixed-effects (TWFE) estimator, which can inappropriately use already treated states as controls. The key identifying assumption is parallel trends, though only a conditional version may be required—i.e., parallel trends after controlling for covariates.

We re-analyze our main outcomes using two staggered DiD estimators: (1) a baseline estimator that includes covariates as controls in the cohort-specific DiD regressions, and (2) a doubly robust (DR) estimator that remains consistent if either the outcome regression model or the inverse-propensity weighting model (used for the weights) is correctly specified.

We log-transform both nonfatal and fatal overdose rates to linearize exponential growth and stabilize variance (Powell 2018). We use 6 years of pre-RTW data and 6 years of post-RTW data to keep the pre- and post-periods comparable in length. The staggered DiD covariates include gender, age, and educational attainment; we exclude household income, employment status, and industry composition because they may be affected by RTW adoption. Each RTW state is paired with never-treated controls. Standard errors are clustered at the state level. Given the small number of switcher states and their heterogeneous sizes, we employ wild cluster-bootstrap *t*-statistics to mitigate small-sample bias.

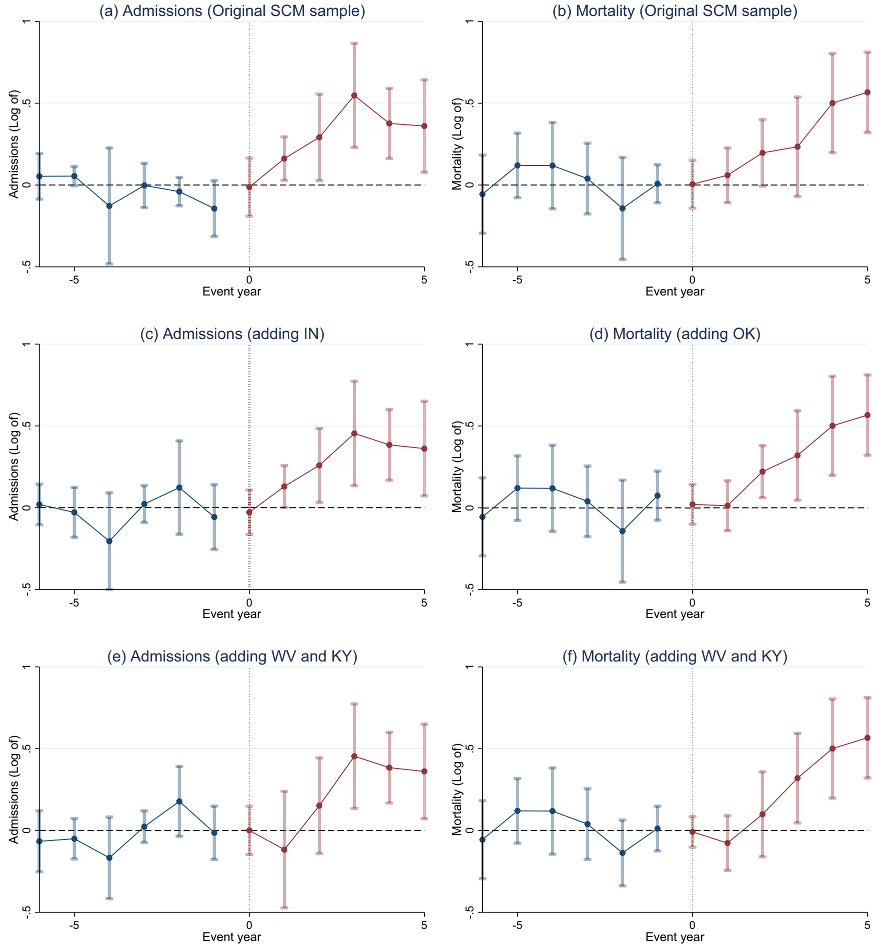

Fig. 5 Opioid-related admissions (**a**, **c**, **e**) and mortality (**b**, **d**, **f**; staggered DiD). Panels **a** and **b** plot staggered DiD estimates with 95% confidence sets constructed using wild cluster-bootstrap standard errors n1,000 replications) for the same set of switcher states used in the SCM analysis. Panel **a** uses TEDS 1992–2018, pooling Oklahoma and Michigan. Panel**b** uses MCOD 1999–2018, pooling Indiana and Michigan. The regressions control for gender, age, and educational attainment. Panels **c** and **d** incorporate Indiana in the admissions analysis and Oklahoma in the mortality analysis. Panels **e** and **f** additionally include two late RTW adopters, West Virginia and Kentucky. The vertical line marks the year in which RTW was adopted

Figure 5a and b plots regression-based staggered DiD estimates for the same set of switcher states used in the SCM analysis. Pre-trend tests show no evidence of differential trends prior to RTW; all pre-RTW coefficients are statistically indistinguishable from zero. Post-RTW, most coefficients are positive and significant, with magnitudes that grow over time. The mortality response lags the admissions response—by roughly 2 years—consistent with the SCM results.

Because staggered DiD does not require a balanced panel for each treated state, Fig. 5c and d incorporates switchers excluded from the SCM sample due to data limitations (Indiana for nonfatal overdoses; Oklahoma for fatal overdoses). Figure 5e and f further adds two late adopters within our observation window, West Virginia (2016) and Kentucky (2017). Neither expansion substantively alters the results.

Table 5 showcases the strongest results from our analysis. Panel A of Table 5 reports average RTW effects from the two estimators described earlier—the regression-based and doubly robust specifications—using the original SCM sample of RTW adoptions between 2001 and 2018. Panels B and C report estimates from adding switchers omitted from the SCM analysis.

The staggered DiD estimates suggest increases of 29–42% in nonfatal overdoses and 26–35% in fatal overdoses. These figures are comparable to the SCM estimates, which indicate changes of 11–16% and 10–60% for nonfatal and fatal overdoses, respectively.

Overall, the staggered DiD results align with the SCM findings in both timing and magnitude, indicating that RTW adoption is associated with increases in opioid misuse. The conclusions remain robust when we include switchers excluded from the SCM sample and when we extend coverage to late adopters. The largely

Table 5 Opioid-related admissions and mortality (staggered DiD)

	Opioid-related admissions	Mortality
Panel A: original SCM sample		
(i) Regression	0.288*** (0.088)	0.261*** (0.098)
(ii) Doubly robust	0.415*** (0.068)	0.346*** (0.073)
Switcher states	OK, MI, WI	IN, MI, WI
Panel B: adding early adopters		
	0.260*** (0.087)	0.274*** (0.082)
Switcher states	OK, IN, MI, WI	OK, IN, MI, WI
Panel C: adding early + late adopters		
	0.206** (0.085)	0.234*** (0.081)
Switcher states	OK, IN, MI, WI, WV, KY	OK, IN, MI, WI, WV, KY

Staggered DiD estimates are reported. Panel A reports (i) regression estimates that directly add covariates as regression controls in the cohort-specific DiD estimation, and (ii) doubly robust estimates that are consistent if either the outcome regression or the inverse-propensity-score weights are correctly specified. Panels B and C report the linear regression estimates. Wild cluster-bootstrap standard errors (1,000 replications) are in parentheses

WV West Virginia, *KY* Kentucky

**$p<0.05$; ***$p<0.01$

consistent patterns from these two complementary approaches lend additional credibility to our main findings.

6 Heterogeneity

6.1 Pre-existing unionization rate

Average RTW effects may mask important heterogeneity by baseline unionization. RTW was introduced at different times—Oklahoma well before Indiana, Michigan, and Wisconsin—and these states began from markedly different union densities. Oklahoma and Indiana historically had low-to-moderate unionization, whereas Michigan and Wisconsin—later adopters—had substantially higher pre-RTW union membership and stronger pro-union sentiment. Given these contrasts, RTW impacts may be larger where unions initially had more capacity to organize and shape workplace conditions (though diminishing returns to collective bargaining could, in principle, imply the opposite).

We therefore estimate SCM effects separately for states with unionization at or above the national average 1 year prior to adoption (Michigan, Wisconsin) and below the average (Oklahoma, Indiana). Results (Table A12) indicate more adverse RTW effects in higher-union states, consistent with Eisenberg-Guyot et al. (2020) for overdose and suicide mortality. Among the most comparable adopters—Indiana (2012), Michigan (2013), and Wisconsin (2015)—over a common window, RTW is associated with roughly two additional overdose deaths per 100,000, about a 12% increase relative to baseline.

The adverse effects also appear more pronounced for the recent, higher-union adopters than for earlier adopters. While cohort differences cannot be fully ruled out, we observe strikingly similar patterns across TEDS and MCOD when tracking the same switcher states (e.g., Table A12, columns 2 and 3 for Michigan and Wisconsin). This congruence suggests that the rise in opioid-related admissions reflects genuine increases rather than contemporaneous changes in reporting or facility availability.

6.2 Individual characteristics

To probe pathways correlated with de-unionization, we implement SCM separately by age, gender, and education—traits that are largely pre-determined with respect to opioid misuse (education being a possible exception), thereby limiting reverse-causality concerns.

Table A13 shows that RTW effects are concentrated among men ages 18–54—a group more likely to be active in the labor market, employed in physically demanding jobs, and to place high salience on work roles. Effects are also disproportionately larger for individuals without a college degree. These patterns are consistent with the interpretation that RTW primarily impairs unions' capacity to improve workplace conditions (wages, benefits, safety) rather than operating mainly through political advocacy, and that the broad protections unions provide against labor-market volatility are especially consequential for less-skilled workers' physical and mental health.

7 Underlying mechanisms

Work is central to psychological health and overall well-being. Employment is not merely the absence of unemployment; dimensions of job quality are critical for workers. At the institutional level, concern for employment quality has prompted reforms in the organization of work and the enforcement of safety standards (e.g., Occupational Safety and Health Administration, OSHA), and most scholars argue that policy should prioritize creating better jobs as well as more jobs (see Cascales Mira (2021) for a review). Guided by the findings in Sect. 6, we examine work-related risk factors that may channel the relationship between RTW and opioid misuse. Specifically, we study work-related fatalities and nonfatal injuries/illnesses (Sect. 6.2), self-rated health (Sect. 7), income and wages (Sect. 7.1), and employment and long working hours (Sect. 7.2).

7.1 Workplace safety

In light of evidence linking union activity to improvements in occupational health and safety, Figure A8a–d and Table A14 report SCM estimates of RTW effects on work-related fatalities and nonfatal injuries/illnesses. The data come from the BLS SOII and CFOI. Because state participation in SOII varies by year and public-sector coverage is missing for many states prior to 2001, we restrict the event window to 2002–2018 to enlarge the donor pool and improve pre-fit. Under this window, Indiana, Michigan, and Wisconsin enter as treated units in the 4-year analysis; Michigan and Wisconsin enter in the 6-year analysis.

When fatal and nonfatal incidents are pooled, differences between treated states and their synthetic controls are small (Figure A8a–b and Table A14, columns 1 and 2). Disaggregating reveals a significant rise in workplace fatalities (Figure A8c–d and Table A14, columns 3 and 4) beginning 2 to 3 years after RTW enactment and persisting through years 4 to 6. The estimates imply that, absent RTW, fatality rates would have been 13–15% lower in years 4 to 6 (0.4–0.6 cases per 100,000 workers) relative to baseline. This pattern accords with prior evidence that non-union workers are less likely to report nonfatal injuries due to fear of retaliation (Hirsch et al. 2001; Leigh and Chakalov 2021). Because fatalities are unlikely to be under-reported and there is no reason to expect fatal and nonfatal injuries to trend in opposite directions, the combination of heightened job insecurity and more hazardous working conditions provides a plausible channel through which RTW could increase opioid misuse.

7.2 Personal health

Given that not all work-related conditions that lead to long-term pain management may not get classified as OSHA-recordable accidents—and therefore are not employer-reported (e.g., wrist or shoulder injuries from prolonged hours; depression or anxiety due to work stress)—the injury estimates in Sect. 6.2 likely understate RTW's impact on workers' physical and mental health. To capture broader health effects, we examine self-rated health using CPS data from 1996–2018, the period during which relevant questions are available.

Figure A9 and Table A15 report RTW effects for two subjective-health measures: a 5-point health scale (higher values indicate worse health; columns 1 and 2) and an indicator for reporting "fair" or "poor" health (columns 3 and 4). Despite only moderate pre-fit for the 5-point scale (82–89%), both measures point to a deterioration in perceived health. Using the "fair/poor" indicator as our preferred outcome (pre-fit > 92%), we estimate an average increase of 1 ppt in the probability of reporting fair or poor health within 4–6 years of RTW enactment—an 8–9 ppt rise relative to baseline.

These effects are concentrated among working-age adults—the same demographic driving the overdose results (Figure A8e–h and Table A16). With substantially improved pre-fit (98–100%), the fair/poor probability rises by 1 ppt (9–10% of baseline) in both the 4- and 6-year samples. On the 5-point scale, the average change is 0.05 points (2% of baseline), indicating that the deterioration in perceived health is driven primarily by an increased incidence of fair/poor reports.

While opioid initiation may arise from pre-existing health conditions or stress as a coping response, opioid dependence can itself worsen perceived health through both pharmacologic and social channels. Although our design cannot fully rule out reverse causality, the timing patterns are informative: in the same switcher states (Indiana, Michigan, Wisconsin), declines in self-rated health precede increases in opioid mortality by at least 1–2 years. Moreover, additional supplemental analyses show that opioid-related admissions exhibit roughly a 1-year lag when Michigan and/or Wisconsin is examined individually. Taken together, the evidence makes it unlikely that the association between RTW and poorer self-rated health is driven solely by higher opioid misuse.

7.3 Income and wages

Unions play a central role in wage setting and in shaping the distribution of income. Low incomes not only directly harm health—via stress, environmental exposure, and poor housing—but also correlate with mediators of opioid-related harm, including access to treatment. Using CPS data from 1990–2019, Figure A10a–d and Table A17 report synthetic control (SC) estimates of RTW effects on before-tax, after-transfer household income and on individual wages. With strong pre-fit (97–100%), we find consistent declines in household income across both the 4- and 6-year follow-up samples, driven primarily by falling wages. In the 6-year follow-up sample (Table A17, column 4), wages decline by $1,649 beginning in year 2 (5–6% of the pre-RTW level) and by $2,633 by year 6 (7–8%). The 4-year follow-up sample (column 3) yields similar magnitudes. Across specifications, wage reductions account for 55–83% of the overall decline in household income. Notably, this wage-led income decline coincides with rising opioid overdoses, suggesting that greater material deprivation and deteriorating financial conditions following RTW adoption may help explain the adverse outcomes we document.

These findings reinforce prior evidence on wage depression from de-unionization for both union and non-union workers (Card et al. 2020; Farber et al. 2021) and align with Fortin et al. (2023), who document substantial wage and unionization effects of recent RTW adoptions in five states—three of which overlap with our switcher states.

7.4 Employment and long working hours

Are the wage declines documented in Sect. 7.1 primarily driven by unemployment, hours, or hourly pay? Knepper (2020) argues that collective bargaining often targets fringe benefits as much as wages, highlighting job quality as a central outcome. Although theories differ on how unionization affects employment at the extensive (employment) and intensive (hours) margins (Montgomery 1989), workers place substantial value on non-pecuniary aspects of jobs.[10], [11] Beyond financial hardship, job loss and unemployment impose lasting psychological and social costs, especially for low-wage workers. Weakened union strength can therefore create a dual burden: heightened risks of displacement and unemployment that may encourage coping behaviors such as opioid use, and, even among the employed, deteriorating job quality—higher workloads and reduced benefits (e.g., cuts to health insurance)—that can undermine well-being and contribute to misuse.

Using 1990–2018 CPS, Figures A10e–f and columns 1 and 2 of Table A18 report estimated RTW effects on employment. Because displaced workers may exit the labor force as well as become unemployed, we focus on the probability of being employed as a more comprehensive measure of economic opportunity; results for unemployment are similar. Employment rises by 0.5–0.7 ppt in RTW states 4 to 6 years after adoption—a 1% increase relative to pre-RTW levels—consistent with evidence that stronger unions modestly reduce employment and raise unemployment (Montgomery 1989; Blanchflower et al. 1991).

Turning to hours, Figures A10g–h and columns 3 and 4 of Table A18 show that the share working long hours increases following RTW. In CPS subsamples with

[10] In April 2016, unions representing nearly 40,000 employees of Verizon, the largest telecommunications company in the US, went on strike to protest the company's decision to cut healthcare and retirement benefits despite reportedly offering a 6% wage increase. See https://www.npr.org/sections/thetwo-way/2016/04/13/474052786/tens-of-thousands-of-verizon-workers-go-on-strike, retrieved May 16, 2025.

[11] In October and November of 2019, thousands of teachers and supporters, organized by the local AFT chapters, rallied in Chicago, Arkansas, and Indiana to demand improved work conditions—smaller class sizes and more support—rather than raises. See https://inthesetimes.com/article/indiana-teachers-strike-walkout-statehouse, retrieved May 16, 2025.

usual hours available (2001–2018), RTW is associated with a 6–10% rise (1–2 ppt) in the share working more than 45 h per week. Pre-RTW fit for the hours outcome is somewhat weaker (92–97%), the effects appear to taper a few years after adoption, and volatility in the series prevents satisfactory fit for alternative long-hours definitions. Even so, the pattern aligns with Gihleb et al. (2024), who document similar increases among full-time workers exceeding 45 h and under related thresholds in states adopting RTW between 2005 and 2019.

7.5 Mechanism discussion

Taken together, the evidence from Sects.6.2–7.2 points to a coherent mechanism linking RTW to elevated opioid misuse through deterioration in job quality and worker well-being. Three patterns are central.

First, workplace safety worsens. We estimate a 14–15% increase in occupational fatalities 2 to 3 years after RTW adoption (Sect. 6.2), with persistence through years 4 to 6. Because fatalities are unlikely to be under-reported (unlike nonfatal injuries), this pattern is consistent with increased exposure to hazardous conditions (e.g., toxic agents, fatigue from overtime) and aligns with prior work on under-reporting of non-fatal injuries among non-union workers (Hirsch et al. 2001; Leigh and Chakalov 2021) and with occupational safety effects of RTW (e.g., Zoorob 2018). Heightened physical risk plausibly raises medical exposure to opioids and the propensity to use them for pain management.

Second, perceived health declines precede overdose harms. Self-rated health worsens among working-age adults (Sect. 7), with increased fair/poor reports emerging 1 to 2 years before the rise in opioid mortality in the same switcher states. This temporal ordering mitigates reverse-causality concerns and is consistent with RTW increasing chronic, work-related conditions (e.g., musculoskeletal disorders) that commonly initiate opioid therapy. Related evidence links treatment of such conditions to long-term opioid use and opioid use disorder (OUD) (e.g., Dale et al. 2021). Reduced schedule flexibility and greater job strain—features plausibly affected by weaker bargaining power—can further impede timely care and elevate stress (Leigh and Chakalov 2021).

Third, material conditions deteriorate and hours lengthen. Real wages fall by 4–8% within 4 to 6 years of RTW (Sect. 7.1), accounting for most of the decline in household income, and the share working > 45 h/week rises by 6–10% (Sect. 7.2). These changes are consistent with broader evidence on wage depression from de-unionization (Card et al. 2020; Farber et al. 2021; Fortin et al. 2023) and with findings that long hours increase after RTW (Gihleb et al. 2024). The combination of lower pay and longer hours is a classic deterioration in job quality (Clark 2015) and plausibly raises stress-related demand for opioids (see Chen and Islam (2023) for stress-mediated pathways).

Overall, the triangulation across (i) fatal injury risk, (ii) earlier declines in self-rated health, and (iii) wage compression with longer hours supports a pathway in which RTW reduces worker bargaining power, worsens job quality, and—via increased pain, stress, and medical exposure—elevates opioid misuse. We view this mechanism evidence as suggestive rather than definitive: measurement constraints (e.g., SOII coverage, volatility in hours series) and potential residual confounding remain. Even so, the consistent timing, direction, and cross-outcome corroboration strengthen the interpretation that labor-market channels are an important contributor to the observed RTW–opioid link.

8 Conclusion

This paper examines RTW laws as a contributor to the rise in opioid dependence in the United States, focusing on union decline as a central channel. We study four post-2000 RTW adopters, Oklahoma, Indiana, Michigan, and Wisconsin, and leverage long pre- and post-intervention windows. Using SCM as our primary research design and corroborating with staggered DiD, we estimate the causal effects of RTW on opioid misuse.

Because the four RTW states differ in pre-existing unionization rates, our estimates speak to how impacts are robust to initial conditions that may reflect business climate (Holmes 1998) or underlying anti-union sentiment. The data provide 4 to 6 years of follow-up after adoption, allowing us to capture time-varying and potentially delayed responses—particularly relevant for mortality outcomes.

We find that RTW adoption induces an average 14% decline in unionization, consistent with recent evidence (Fortin et al. 2023; Murphy 2023; Gihleb et al. 2024). In the 4 to 6 years following adoption, opioid-related admissions rise by about 13% and opioid overdose deaths by about 35%. While these effects are meaningful, the broader secular changes over the same period are far larger: on average, opioid-related admissions increase by roughly 151% and fatal overdoses by roughly 953% in the affected states. Taken together, our estimates imply that RTW may account for approximately 4–8% of the total observed increase in opioid misuse.

Linking magnitudes across outcomes, a 10% decline in unionization is associated with roughly a 9% increase in opioid-related admissions and a 24% increase in fatal overdoses in RTW states. Although prior work has not directly estimated the impact of unionization on opioid misuse, these effect sizes are broadly consistent with studies connecting union density to drug-related mortality (e.g., DeFina and Hannon 2019; Eisenberg-Guyot et al. 2020).

In contrast, we find no statistically significant effects of RTW on non-opioid drug misuse. This pattern aligns with mechanisms specific to pain and opioids' central role in pain management. Consistent with that interpretation, our mechanism evidence points to RTW-induced changes in workplace conditions—greater occupational hazards, lower wages, longer hours, and heightened stress—that plausibly increase chronic pain risk and opioid exposure among working-age men.

Overall, the results indicate that RTW legislation is one plausible driver of rising opioid dependence and of geographic heterogeneity in opioid harms since the 2000s. If the primary pathway is diminished union power, the findings underscore the protective role unions can play in buffering working-class populations against adverse labor-market shocks. Because de-unionization disproportionately affects less-educated, working-age men, these groups may face "double jeopardy" from both deteriorating job quality and—consistent with the findings of Powell (2025)—heightened susceptibility to opioid harms. The recent policy reversals in Missouri and Michigan—and rising public support for unions in the post-recession and post-COVID era—are consistent with the broader interpretation that workers respond to the costs of anti-union policy.

Our results have immediate implications for overdose-prevention strategies and for understanding the public-health consequences of labor-market institutions more broadly. More generally, our evidence supports the view that unions' institutional functions extend beyond wages and employment to public health. The implications of declining unionization in the United States may therefore be more extensive than previously recognized.

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Data availability All data utilized in this study are publicly accessible. This includes the Treatment Episode Data Set (SAMSHA), the Multiple Cause of Death database (CDC), the Current Population Survey (US Census), the Union Membership and Coverage Database (compiled by unionstats.com from the CPS), the Survey of Occupational Injuries and Illnesses (BLS), and the Census of Fatal Occupational Injuries (BLS).

Declarations

Conflict of interest The authors declare no competing interests.

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