Anti-Union Regulation and the Opioid Crisis

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Abstract

Existing literature provides ample evidence of the detrimental impact of de-unionization on various determinants of individual health. However, there is a lack of direct empirical research linking de-unionization to objective health outcomes. By leveraging the differential timing of Right-to-Work law implementation across four states in the United States, we quantify the role of union presence in opioid misuse. Using a synthetic control method, our analysis reveals a persistent decline in unionization rates in the affected states within 4-6 years of RTW enactment. This reduction precipitated a considerable increase in both nonfatal and fatal overdoses, as measured by opioid-related treatment admissions and overdose deaths. No similar trends are found for non-opioid drugs. Furthermore, the adverse effects of opioid misuse were concentrated among working-age males and appeared to be mediated by several workplace-specific risk factors of opioid addiction, including increased occupational hazards, reduced wages, extended working hours, and greater work-related stress.

JEL Classification: I12; I18; J51; K31

Keywords: Opioid, Drug, Fatal and Nonfatal Overdose, Right-to-Work, Union Density

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1. Introduction

The U.S. Bureau of Labor Statistics (2023) reports that union membership declined from 20.1% in 1983 to 10.1% in 2022, the lowest rate in four decades.[1] As important labor market institutions that balance the power between capital and labor, the decline of American unions is considered a threat to public health among epidemiologists and sociologists (Wright 2016; Hagedorn et al. 2016). While the precipitous decline in unionization can be traced to several larger macroeconomic shifts, such as the structural transformation from a manufacturing-centric to a predominantly service-based economy, a growing dependence of non-standard employment arrangements, and the globalization of production processes, the passage of anti-union legislation, in particular, Right-to-Work (RTW) laws has been regarded as a major catalyst for these trends (Fortin et al. 2023; Murphy 2023).

RTW laws in the U.S. emerged as a response to the harsh enforcement of union security clauses, such as compulsory union membership, that originated from the National Labor Relations Act (NLRA) of 1935. These laws permit employees covered by a union contract to opt out of paying union dues while still being eligible for union benefits. This situation leads to a widespread "free rider" problem (Ichniowski and Zax, 1991). Consequently, unions experience a decline in financial resources, which weakens their ability to negotiate favorable contract terms. As financial support decreases, the demand for union services among paying members also declines, contributing to a downward spiral in unionization rates over time.

As of the time of writing, 27 states have implemented RTW laws (Table A1). The RTW movement gained significant momentum over the last decade when even traditionally labor-friendly states such as Indiana (2012), Michigan[2] (2013), and Wisconsin (2015) adopted RTW laws in their stated efforts to attract businesses and investment. West Virginia and Kentucky followed suit in 2016 and 2017, respectively. Some local governments in non-RTW states (e.g., cities and counties in Illinois) have attempted to create "union-free" zones in a move to bypass state laws (Flavin and Shufeldt 2016). In 2019, the Supreme Court ruling in *Janus v. American*

[1] See https://www.bls.gov/opub/ted/2023/union-membership-rate-fell-by-0-2-percentage-point-to-10-1-percent-in-2022.htm, retrieved May 16, 2025.
[2] Michigan repealed RTW regulation in 2023, but it is included in our analysis, as the data used in this study spans from 1992 to 2018.

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Federation of State, County, and Municipal Employees (Janus vs AFSCME) extended RTW laws for the entire U.S. public sector, effectively laying the groundwork for anti-union lawmakers to push for a national RTW law.

But the trends are not entirely unidirectional. The repeal of RTW in Missouri in 2018 and Michigan in 2023 – two rare reversals in over 50 years – are of particular interest and may be viewed as evidence of rising concerns over the erosion of labor rights in post-COVID America. Although strong political mobilization and shifting power dynamics were at the heart of both of these reversals, declining wages and growing inequality enabled Missouri unions to successfully frame the opposition to RTW laws "not just as a "union" concern, but as a threat to wages and working conditions for all workers in the state."[3] Similarly, even though the repeal in Michigan in 2023 benefited from political shifts within the state legislature, the lack of conclusive evidence that RTW had delivered significant economic advantages in Michigan since 2012 was a significant factor in the repeal.[4] According to a 2022 Gallup study, public approval of unions increased during the pandemic and subsequently rose to a level not seen in nearly six decades (Gallup 2022).[5] Thus, our current research is especially relevant in this unique moment – on the one hand, these recent reversals have the potential to encourage further pro-labor policy initiatives; and on the other hand, they are likely to galvanize opposition from business,

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[3] 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.

[4] 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.

[5] There were massive protests when Wisconsin became the 25th state to enact RTW laws in 2015. In November, 2019, more than 15,000 teachers and supporters rallied at the Indiana statehouse, organized by the Indiana State Teachers Association and AFT Indiana, less than one week after teachers in Little Rock, Arkansas observed a one-day strike to defend their collective bargaining rights, and one month after 25,000 educators with the Chicago Teachers Union went on an 11-day strike for improved work conditions. See https://inthesetimes.com/article/indiana-teachers-strike-walkout-statehouse, retrieved May 16, 2025.

lawmakers and other stakeholders, potentially leading to future attempts to reinstate, strengthen, and expand RTW.

While existing studies have understandably prioritized the impact of labor unions on the more quantifiable labor market outcomes (e.g., employment, wages, labor and organizational productivity, etc.), the interconnected socio-economic dimensions of unions' role in worker health and well-being have received relatively less attention (see Appendix A for a literature review). Our research makes a direct contribution to this space. We estimate the effects of RTW and de-unionization on public health by examining a specific health outcome: opioid dependency, measured by the number of treatment admissions to the facilities that report to state administrative data systems and overdose mortality. To our knowledge, this is the first study analyzing the effects of de-unionization on opioid misuse. Two existing studies that have analyzed the relationship of union density with drug-related mortality both suggest that declining unionization exacerbates drug misuse (DeFina and Hanon 2019; Elsenberg-Guyot et al. 2020).[6] However, it is not immediately clear whether the decline in unionization causes increased mortality, or if the correlation is driven by unobserved differences across states.[7]

We address these concerns by leveraging one plausibly exogenous component of the variations in union density, the timing of RTW laws introduced from 2001 to 2015 across four U.S. states, Oklahoma (2001), Indiana (2012), Michigan (2013), and Wisconsin (2015), using a rich dataset we compiled from a variety of sources, including the Treatment Episode Data Set (TEDS),

[6]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. Elsenberg-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.

[7]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 U.S. manufacturing due to outsourcing or technology replacing skilled workers. These economic changes led to the rise of a postindustrial economy, causing elevated hopelessness and despair (Case and Deaton 2015). 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.

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Centers for Disease Control and Prevention (CDC) Multiple Cause of Death (MCOD), Current Population Survey (CPS), Union Membership and Coverage Database, Bureau of Labor Statistics (BLS) Survey of Occupational Injuries and Illness (SOII), and Census of Fatal Occupational Injuries (CFOI). This approach allows us to assess whether the previously observed union-mortality relationship reflects the pre-existing trends among states or whether changes in mortality occurred due to de-unionization. Through a variant of the original synthetic control method (SCM) of Abadie and Gardeazabal (2003) that accommodates multiple treated units (Cavallo et al. 2012; Galiani and Quistorff 2017), we construct a synthetic control that matches the moments of pre-RTW outcome for each switcher state and then use the collection of synthetic controls to provide a counterfactual scenario for the evolution of the switcher states in the absence of RTW laws. Even though the practice of estimating synthetic control weights separately for each treated unit relies on our ability to find good synthetic controls for every unit (Ben-Michael et al. 2022), it is one of the standard approaches in estimating the impact of a treatment adopted gradually over time (e.g., Donohue et al. 2019; Mitze et al. 2020). Importantly, unlike the difference-in-differences (DiD) technique that restricts the effects of the unobservable factors to be time-invariant, this procedure eliminates the parallel trends assumption and enables us to make inferences robust to time-varying confounders associated with both drug demand and supply (e.g., general social and economic decline, competition, and availability and cost of illicit drugs), even in situations where there are few treated clusters.

Deviating from the existing literature, we also examine RTW's relation with nonfatal overdoses that do not necessarily lead to deaths, an issue that has not been extensively explored in the field of research on opioid use disorder (OUD). Accurate data on nonfatal overdoses is challenging to obtain, given that such events may only be reported if an individual receives documented medical services (e.g., emergency room visits and hospitalization). Still, the limited information available suggests that overdose victims are at risk of not only subsequent overdose (Zibbell et al. 2019) but also other comorbidities. Besides the standard health complications such as respiratory depression and hypoxia-related brain injuries, which have been shown to contribute to a large number of subsequent complications and the onset of severe disability, nonfatal overdoses of opioids are also linked to the susceptibility of some areas of the brain to hypoxic injury, with short-term memory loss and changes in cognitive and physical functioning

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documented. In this respect, the lack of consideration for nonfatal events may have led to an underestimation of the drug-related harms induced by de-unionization in earlier studies.

Finally, we explore the plausible pathways through which anti-union regulation might be related to overdose behavior. While the reduced-form approach we employ only allows us to speculate on the underlying mechanisms of the observed RTW effects, the several RTW-induced risk factors of drug misuse identified by the current analysis may constitute new areas of future research to shed more light on the well-being consequences of de-unionization.

To foreshadow what follows, we find significant support for the general perception that the decline in union density contributes to the opioid crisis. Specifically, we observe a persistent rise in both fatal and nonfatal overdoses of opioids within 4-6 years of RTW enactment in the states that adopted RTW. The increase in opioid dependency follows a sustained decline in the unionization rates for the same states. However, the decline in unionization does not affect the misuse of non-opioids, such as sedatives, stimulants, antidepressants, and cocaine. Given that the RTW-induced dependence on opioids appear to be confined to working-age males who have a higher likelihood of engaging in the labor market, working in dangerous conditions, and experiencing life stress arising from lower job security and job control, we identify four plausible contributors to the RTW-gradient of opioid harms: increased occupational hazards, reduced wages, extended working hours, and greater work-related stress, after examining a set of alternative workplace-specific outcomes as potential channels through which de-unionization may affect drug use.

We conduct a battery of checks to confirm the validity of our results. The observed RTW effects align in both magnitude and timing with those derived from staggered DiD estimators (Callaway and Sant'Anna 2021), even after accounting for the switcher states that do not meet the data requirements for SCM and are therefore excluded from the main analysis (Section 7). Additionally, we find no evidence that the RTW effects are influenced by potential biases related to sparse synthetic control weights or overfitting (Abadie 2021; Abadie and Vives-i-Bastida 2022). Other factors such as the reformulation of OxyContin (Alpert et al. 2018), concurrent policies like the Triplicate Programs (Alpert et al. 2022), Prescription Drug Monitoring Programs (Buchmueller and Carey 2017), Medical Marijuana Laws (Powell et al. 2018), measurement errors in opioid overdose death data (Alpert et al. 2022), and the entry of illicit drugs do not

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appear to drive the results either. We investigate different donor pools, treatment groups, predictive covariates, definitions of opioid misuse, and placebo outcomes, including deaths attributable to breast cancer and influenza/pneumonia, to support these conclusions.

These results speak to a broader body of work that documents the impact of de-unionization on important predictors of personal health. A few prominent examples include decreased wages (Card et al. 2020; Fortin et al. 2023), increased income inequality (Farber et al. 2021), reduced health care access (Buchmueller et al. 2002), compromised workplace safety (Zoorob 2018), and deteriorated subjective well-being (Artz et al. 2022; Chen and Islam 2023). Outside of the literature on occupational health and safety, most existing studies that directly link de-unionization to objective health outcomes are either qualitative/theoretical (Hagedorn et al. 2016) or exclusively focus on union members (Reynolds and Brady 2012). The latter may have limited external validity given the shrinking share of union workers and a potentially different effect of unionization on nonunion outcomes. From this perspective, the current study offers the first evidence of how RTW laws affected the general public, specifically regarding a devastating substance misuse-related national health crisis that has gripped the U.S., overlapping with the time when RTW legislation experienced its greatest success.

Over the past three decades, the U.S. has confronted a growing crisis of substance addiction, most starkly illustrated by the rise in deaths from drug overdoses (SHADAC 2023). About three-fourths of these deaths involved opioids, which include natural and semi-synthetic opioids (e.g., prescription painkillers such as oxycodone and hydrocodone), synthetic opioids (e.g., fentanyl), and illicitly made heroin. Since 2011, the years the CDC declared an "epidemic" of overdoses from prescription painkillers, the opioid crisis has evolved to one increasingly driven by illicitly trafficked heroin and fentanyl, and yet opioid overdose remains a leading cause of injury-related death in the United States (CDC 2023). In this regard, the current study ascribes a significant role to anti-union regulation like RTW to the drastic rise in opioid dependence and thereby contributes to the strand of research on the economic underpinnings of the overdose epidemic. In particular, the evidence found in this study echoes the small, but growing body of literature in 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

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and Evangelist 2022).[8] These results can have immediate implications for the development of overdose prevention strategies and facilitate a deeper understanding of the public health impact of labor market institutions more broadly.

This paper proceeds as follows. Section 2 describes the data, institutional background, and estimation strategy. Sections 3 through 6 present the results from the SCM analysis, conduct robustness checks, examine underlying heterogeneity, and investigate the mechanisms involved. Section 7 replicates the main findings using a staggered DiD approach, and Section 8 offers concluding remarks.

2. Data, Method, and Sample Construction

2.1 Data

Data on drug-related admissions are collected from the 1992-2018 TEDS through the Substance Abuse and Mental Health Services Administration (SAMHSA).[9] Since 1992, the SAMHSA has mandated that all states provide treatment admission data from publicly funded facilities. This requirement extends to facilities that receive funding from federal block grants, Medicare/Medicaid, or state funds, irrespective of whether they also cater to privately insured or self-paying patients. While facilities exclusively serving privately insured or cash-only patients are not reflected in the sampling frame, examinations of national spending on substance misuse treatment show that the public sector has consistently paid over 75% of all substance abuse treatment in the U.S. since 1998 (Powell et al. 2018). Thus, in theory, the TEDS data should capture the vast majority of drug-related admissions. One technical issue with the TEDS data is that not all states submit their admission records for every year. To create a balanced panel, we

[8] Kravitz-Wirtz et al. (2020) find that Medicaid expansion was associated with reductions in opioid overdose deaths, particularly deaths involving heroin and synthetic opioids other than methadone from 2001 to 2017. Wu and Evangelist (2022) reveal that the harmful effects of job loss on opioid overdose mortality decline with increasing state unemployment insurance benefit levels.

[9] Data from 2019 and onward are excluded due to the implementation of RTW laws for all public-sector unions, both at the state and federal levels, following the U.S. Supreme Court's ruling in the case *Janus vs AFSCME*. Since the ruling was not issued until June 27, 2018, we consider 2018 a fully treated year, although we also perform a robustness check for the samples that exclude 2018 data (Section 4.1).

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exclude six states that have missing records, including one switcher state (i.e., Indiana) (Table A2). However, as shown in both the SCM and DiD analyses, omitting these states has a minimal effect on our ability to match the pre-RTW paths of actual admissions with those of the synthetically constructed units, as well as on our main findings.

We identify opioid-related cases as those in which patients list either "heroin," "non-prescription methadone," or "other opioids and synthetics" as their primary substance of use. The category "other opioids and synthetics" includes various pain relievers, such as oxycodone and fentanyl, among others. Due to the rise in the misuse of heroin during our observation period, we group it with "other opioids and synthetics" following the existing literature (Powell et al. 2018). We also examine "other opioids and synthetics" separately as a robustness check in Appendix C.4.

Our measure of overdose deaths is derived from the 1999-2018 CDC MCOD. The data provides a primary cause of death along with up to 20 contributing causes, using the International Classification of Diseases, Tenth Revision (ICD-10) codes. To identify cases of fatal drug overdose, we adopt a broad definition where the primary cause of death falls under accidental poisoning (X40-X44), intentional self-poisoning (X60-X64), assault (X85), or poisoning of undetermined intent (Y10-Y14), regardless of the intent behind the death (SAMHSA 2018). We then further define a fatal opioid overdose as any case in which an opioid is listed as a contributing cause of death. This includes various categories such as opium (T40.0), heroin (T40.1), natural and semisynthetic opioids (T40.2), methadone (T40.3), synthetic opioids (T40.4), and other or unspecified narcotics (T40.6). Given the complexities involved in categorizing drug overdose deaths — particularly those related to synthetic opioids that do not have specific ICD-10 codes — this broader definition helps reduce the risk of substance-specific classification errors (Alpert et al. 2022). Additionally, we conduct our analysis using two narrower definitions, one of which excludes deaths caused by heroin and synthetic opioids such as fentanyl (Appendix C.4). Lastly, since the CDC considers an underlying death count of fewer than 20 to be unreliable, we exclude four never-RTW states from the main analysis on overdose deaths (Table A2).

In the subsequent analysis, we show a highly consistent pattern between the results obtained for treatment admissions and overdose deaths. Since the latter is less likely to be subject to reporting biases, the close correspondence between these outcomes suggests that the potential differences

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in the scope of facilities included in TEDS — i.e., individuals unreported in TEDS due to disparities in licensure systems and health care payment structures — do not drive the observed gaps in opioid misuse.

Other state-level characteristics are compiled from four different sources. The effective dates of the RTW legislation are constructed from the National Right to Work Committee (Table A1). Population denominators for the analysis involving TEDS come from Federal Reserve Economic Data. The remaining socioeconomic characteristics of each state are obtained through the Annual Social and Economic (ASEC) component of the CPS, Integrated Public Use Microdata Series. Since income-related information in the CPS was reported for the previous year, they are lagged by one year and, when appropriate, adjusted to reflect 1999 constant dollars using the Consumer Price Index.[10]

Finally, data from two additional sources are utilized to investigate the first-stage outcomes and pathways through which RTW legislation affects opioid misuse. They include 1) the Union Membership and Coverage Database, which provides estimates of union density using the CPS and the same method that the BLS employs for corresponding estimates at the national level (Hirsch et al. 2001), and 2) the SOII and CFOI for workplace fatal and nonfatal injuries.

2.2 Method

The present study employs the SCM, originally proposed in Abadie and Gardeazabal (2003), to assess the impact of the RTW legislation on opioid misuse. SCM establishes a counterfactual trajectory of an outcome for each state that enacted RTW laws by computing a weighted average of the states that "never" adopted RTW laws during the observation period, intending to closely mirror the pre-RTW trajectory of the outcome in the adopting state. To smooth out stochastic variations in our estimates and produce results that are as generalizable as possible, we adopt a variant of the original SCM approach that aggregates multiple events into a single estimated treatment effect (Cavallo et al. 2013 and Galiani and Quistorff 2017). This is accomplished by matching each RTW-switcher-state with its synthetic counterpart, deriving respective year-by-year estimates for the post-RTW period, and then averaging the RTW effect across states.

[10] https://cps.ipums.org/cps/cpi99.shtml, retrieved July 30, 2022.

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For inference, we implement a modified version of the classical permutation test by examining whether or not the average RTW effect across states falls well inside the distribution of placebo averages, that is, the average effects estimated for respective never-RTW states when a fictitious RTW law is assigned at the same time as the state of interest. The procedure begins with estimating placebo effects for every potential control state in the donor pool and for each individual switcher state under study. At a given post-RTW period, it subsequently computes placebo averages for all possible combinations by picking a single placebo estimate from each switcher state and then ranks the magnitude of the post-pre root mean square prediction error (RMSPE) of the switcher states relative to those of the placebo averages.

A detailed description of the SCM procedure is provided in Appendix B. In all the SCM applications, we allow for covariates consist of both lagged values of the outcome and other economic variables that have predictive power for the outcome. These variables include share of population in blue-collar occupation and living below the federal poverty line, rates of labor market participation and employment, age, education, working hours, occupational composition based on the Standard Occupational Classification, industry composition derived from the North American Industry Classification System, and the proportions of individuals who are white and married. Additionally, we consider self-reported health status and state-level union density, alongside a set of opioid-related policies including Prescription Drug Monitoring Programs, the triplicate policy, and Medical Marijuana Laws. The specific combination of these variables varies depending on the data sample and the outcome being analyzed.

2.3 Sample Construction

In applying the multiple-treatment-unit SCM, a tradeoff arises between the event window's length and the treatment group's representativeness. Shortening the time horizon would increase the number of switcher states included in the analysis but at the expense of compromising the length of the event window. To address this dilemma, we construct two balanced panels in event time. The first panel spans four years and includes all switcher states (i.e., Oklahoma, Michigan, and Wisconsin for nonfatal overdose, and Indiana, Michigan, and Wisconsin for fatal overdose). The second panel covers six years and focuses on two early RTW adopters (i.e., Oklahoma and Michigan for nonfatal overdose and Indiana and Michigan for fatal overdose). Although compromising on external validity, the latter panel provides a longer-term estimate of the RTW

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legislation, accommodating the possibility that the treatment effect may manifest after a significant delay. Accounting for potentially delayed responses is crucial in our context, given the typical duration for collective agreements to be renegotiated and for unions to contribute to improving living conditions on a societal scale. For instance, while the NLRA does not stipulate a specific contract duration, all collective agreements have a predetermined length in practice. The average contract term is three years, although in recent years, many contracts have shifted towards longer terms of four or five years (Compa, 2014). Given that the TEDS and MCOD data began in 1992 and 1999, respectively, the event window for nonfatal overdoses comprises periods between 9 years pre-RTW and 4 and 6 years post-RTW, respectively, for the four and six-year samples and the event window for fatal overdoses comprises periods between 13 years pre-RTW and 4 and 6 years post-RTW, respectively, for the four and six-year samples.

The analysis excludes states that had implemented RTW legislation before the observation period, and states that adopted RTW during the observation period but lacked sufficient post-treatment data. A complete list of potential donors can be found in the footnote of Table A1.

2.4 Descriptive Statistics

Panels A-B of Table 1 presents summary statistics of the main outcomes. Over the periods of 1992-2018 and 1999-2018, the average number of opioid-related admissions and deaths per 100,000 people were 214 and 9.5, respectively, constituting 49% and 65% of all drug-related cases. Regardless of drug type, the variance in opioid misuse across states is significantly greater than that of non-opioid analgesics (224 vs. 107 for admissions and 7.3 vs. 2.6 for deaths). Both the switcher and never-RTW states show similar compositions; however, the switchers exhibit less prevalent misuse behavior across samples compared to the never-RTW states (e.g., 6.6 vs. 9.2 for switchers in the six-year panel and never-RTW states).

Panels C-D show the changes in opioid misuse between the first and last years of the observation period. While the switchers began with a lower incidence than never-RTW states, they experienced substantially faster growth than the latter in both fatal and nonfatal overdoses (i.e., a 476% vs. 381% increase for nonfatal overdose and a 1713% vs. 378% increase for fatal overdose in the six-year panel). Importantly, the four and six-year samples exhibit remarkably similar patterns despite the different state compositions.

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Figure 1 shows the unadjusted trends in opioid misuse separately within each switcher and never-RTW states. Prior to RTW adoption, neither outcome for any of the switcher states exhibited parallel movement with the never-RTW states, thus rendering a straightforward difference-in-differences comparison of the outcome problematic.

3. Main Results

3.1 Unionization Rates

The potential for wide-spread free-riding encouraged by RTW laws can limit unions' means of financial support and weaken their ability to organize. Hence, declining unionization rates would be the most direct and immediate societal response to RTW legislation. We thus begin our analysis by assessing the de-unionization impact of RTW for the adopters under consideration. While the CPS data before 1990 are disregarded due to errors,[11] we are able to utilize all states, including 23 never-RTW states and four switcher states in the analysis.

Figures 2a-2d suggest a decent match quality (i.e., 95-99%; Table 2) for the 11-year pre-RTW period between the switchers and their synthetic control groups across samples and outcomes. Before RTW, the trend and level of each synthetic unit closely resemble the actual trajectory of the unionization rate in switcher states. After RTW, however, a negative gap emerges and gradually widens over time. Turning to the average treatment effect (ATE) estimates, Panel A of Table 2 indicates that RTW induced an average decline of 1-2 ppt (or approximately 10-15%) in union membership and coverage in the switcher states 4-6 years after its passage. The magnitude of the decline in unionization remains consistent whether measured as the percentage of union members or as the percentage of workers covered by a union contract. A breakdown of the estimates by individual year reveals a similar time-series pattern across outcomes (Panel B of Table 2).

Considering that the CPS data extends back to 1990 and that a longer pre-intervention window may yield a more reliable synthetic control estimator (Abadie 2010), Figures 2e-2h replicate the

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

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analysis for the switcher states that have 22 years of pre-RTW data, Indiana, Michigan, and Wisconsin. With a comparable match quality (i.e., 94-99%; Table A3), the results indicate a more significant reduction in union density of 1-3 ppt, representing a decline of 7-20% 4-6 years after the adoption of RTW laws in these states.

Table A4 additionally displays the optimal weights assigned to potential donors of each switcher for a representative case in Table 2, union membership in the four-year panel. As expected, only a few donors receive positive weights, and we check the potential bias from the sparsity of the synthetic control weights in Appendix C.1.

It is noteworthy that we do not observe any delayed responses in union density. This stands in stark contrast to the patterns seen in opioid misuse, particularly in the case of mortality (Sections 3.2-3.3). This observation supports the hypothesis that RTW laws impact opioid misuse by reducing the prevalence of unions. If we consider unionization rates as a measure of union strength, these findings suggest that RTW laws have significantly weakened unions' negotiating power in switcher states prior to the documented increase in opioid misuse.

3.2 Treatment Admissions

Figure 3 illustrates the estimated RTW effects for treatment episodes involving opioids (top panel), all drugs (middle panel), and non-opioid drugs (bottom panel). The left figure within each panel aggregates the treatment effects of all available switchers, Oklahoma, Michigan, and Wisconsin, and the right panel aggregates the two early adopters, Oklahoma and Michigan.

The findings in the top panel, along with those in columns 1-2 of Table 3, reveal a perfect pre-RTW match (i.e., 100%) in actual admission and a positive gap between the actual and their respective synthetics 2-3 years after RTW implementation. According to the ATE estimates, the admission rate rose by 9 and 16 cases per 100,000 people within the four and six years of RTW enactment, respectively, representing an 11% and 16% increase from the number of cases one year before RTW passage in the switcher states. Despite some year-to-year variations in individual coefficients, estimates from both samples suggest an upward trend in the size of the admission gap. By year four, 40-41 cases (or 39-47% of the baseline admissions) would have been eliminated had the affected states not adopted RTW, and this figure remains high at 42 cases (or 41% of the baseline) by year six.

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Conditional on an equally excellent pre-RTW match quality (i.e., 99-100%), however, we find little effect of RTW on the admissions involving all drugs, including non-opioid analgesics (e.g., sedatives, stimulants, antidepressants, and cocaine). While the six-year panel shows a trend break upon RTW passage (column 4 of Table 3), the gap between actual admission and its synthetic unit dissipates after the fourth year of RTW implementation, leading to a null estimate for the entire duration of observation, even though the estimated relationship remains positive. A separate analysis of the admissions involving non-opioids yields consistent findings. Despite a positive gap between actual admissions and their synthetic counterparts, neither sample suggests a statistically significant overall estimate at the 5% level (columns 5-6 of Table 3).

3.3 Mortality

The mortality results in Figure 4 support similar conclusions. Across the samples, the number of deaths involving opioids in the switchers – Indiana, Michigan, and Wisconsin in the four-year panel and the former two in the six-year panel – and never-RTW states evolves similarly 13 years before RTW enactment, with the pre-RTW match quality reaching 100% (columns 1-2 of Table 4). After the inception of RTW, however, a visible gap gradually emerges, with an average increase of 1 to 4 deaths per 100,000. This represents an increase of 11% to 66% compared to the pre-RTW levels. To provide context for these figures, overdose deaths surged by 211% and 1695% in the switcher states within these two samples over the same period, respectively.

The timing of the RTW effects on nonfatal and fatal overdoses also suggests that nonfatal overdoses may lead to fatal ones. When there are significantly positive RTW effects, there is an additional two-year delay in mortality responses compared to treatment episodes. This delay could indicate a greater cumulative impact of RTW on opioid misuse, as individuals may progressively increase their visible dosage or engage in "polydrug" use (i.e., the simultaneous consumption of multiple drugs), thereby raising the risk of a fatal overdose.

Just like the case of treatment admissions, the estimated RTW effect for deaths involving non-opioid drugs diminishes over time. Consequently, we do not detect any significant RTW effect for drugs overall (columns 3-6 of Table 4; middle and bottom panels of Figure 4).

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3.4 Non-Linear Effects

The average effects of RTW may conceal significant disparities among the switcher states. For instance, RTW was introduced at distinct points in time, with Oklahoma residents experiencing the legislation at least 10 years earlier than those in Indiana, Michigan, and Wisconsin. While Oklahoma and Indiana have traditionally maintained low to moderate unionization rates, Michigan and Wisconsin—more recent adopters of RTW—have had a considerable number of union members and strong support for unions prior to RTW passage. Moreover, these two states have undergone a political shift from a reliable Democratic party majority to becoming solid swing states. Finally, while the SCM estimates may be robust against unobserved state-level factors, such as anti-union sentiment and political orientation, the RTW effects could vary for state residents depending on existing union density. Specifically, RTW laws might have more significant impacts in states with higher union densities, since unions in states with lower densities may lack the power to organize workers effectively and improve working conditions. Conversely, the opposite might be true if collective bargaining faces diminishing returns.

To explore these possibilities, we conduct the SCM analysis separately for states with unionization rates at or above the national average one year before RTW adoption (i.e., Michigan and Wisconsin) and for states with unionization rates below the national average (i.e., Oklahoma and Indiana). The estimates are presented in Table A5 and indicate that RTW laws have a more detrimental effect in states with higher union density than those with lower density. This finding is consistent with the results reported by Eisenberg-Guyot et al. (2020) regarding overdose and suicide mortality. Among the most comparable states that adopted RTW laws around the same time — Indiana (2012), Michigan (2013), and Wisconsin (2015) — and over an equal observation window, RTW induced an additional two deaths per 100,000 people, representing a 12-ppt increase from the baseline.

The evidence presented in Section 3 suggests that, without RTW laws, union density in the switcher states would have been 1-3 ppt (or 7-20%) higher than pre-RTW levels. Approximately 2 to 3 years after this decline, the same states saw a significant increase in both nonfatal and fatal opioid misuse. While we cannot completely rule out the possibility of cohort differences, the adverse impact of RTW laws appears to be more pronounced in more recent adopters with higher pre-existing union density than earlier adopters. Furthermore, despite the analyses' varying time

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frames, remarkably similar patterns emerge between the TEDS and the MCOD outcomes when the same switcher states are tracked over time (e.g., columns 2-3 of Table A5 for Michigan and Wisconsin). This provides indirect evidence that the increase in treatment admissions associated with RTW adoption likely represents actual increases in harm, rather than a concurrent uptick in treatment availability or reporting.

4. Robustness

We draw upon the SCM requirements outlined in Abadie (2021) and existing research on opioid misuse to inform our robustness analysis. Specifically, we evaluate the sensitivity of our results regarding the following concerns:

First, since our SCM procedure assigns positive weights to only a few donor states (Section 3.1), there is a concern that our estimates may be heavily influenced by the performance of a small number of states (Abadie, 2021). Furthermore, while a close pre-treatment fit is crucial, it does not guarantee good performance of synthetic control estimators due to the risk of overfitting, particularly if this fitting occurs within a short pre-intervention period (Abadie and Vives-i-Bastida, 2022).

Second, our baseline specifications include the states that implemented the triplicate prescription programs. Existed in three never-RTW states, California (1992-2004), Illinois (1992-2000), and New York (1992-2001), the triplicate programs are known for their long-term effects on opioid overdose deaths (Alpert et al. 2022). If the historical impacts of such policies differ significantly from states that did not implement similar measures, this omission could lead to an overestimation of the RTW effects. Similarly, our estimates may be misleading when the geographic areas disproportionately impacted by the epidemic, such as the Appalachian region (Shiels et al., 2020), are included in the donor pool (i.e., Maryland, New York, Ohio, and Pennsylvania). Likewise, neighboring states may not be suitable controls due to potential

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interstate spillovers in resident preferences, state policies, and local labor market conditions,[12] and the same applies to donor states that implemented public sector RTW laws in 2018.

Third, the reformulation of OxyContin in 2010 could accelerate the use of substitute drugs, including heroin, depending on the pre-reformulation OxyContin misuse rates (Alpert et al. 2018). Considering that two out of four switcher states had a high pre-reformulation OxyContin misuse rate during the observation period (i.e., Indiana and Wisconsin),[13] the observed RTW effects can be a natural result of the introduction of abuse-deterrent opioids.

Fourth, besides triplicate policies and abuse-deterrent drug formulations, the implementation of Prescription Drug Monitoring Programs (PDMPs) in 15 never-RTW states and two switcher states, Indiana (1998) and Wisconsin (2013), during the observation period can significantly impact opioid-related harm and usage patterns.[14] While evidence on the effectiveness of PDMPs is mixed, prior studies suggest that policies mandating prescribers to use PDMP at the point of care have the potential to reduce opioid misuse (e.g., Buchmueller and Carey 2017; Wen et al. 2019). The enactment of medical marijuana laws (MMLs), including Michigan's MML adoption in 2008, may have a similar effect.[15] Related research indicates that MMLs are associated with

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[12] For instance, suppose that increased competition from RTW states forces wages down in non-RTW states and that lowered prevailing wage drives up opioid misuse in the latter, including border states in the donor pool could bias our estimates upward.

[13] We rely on the exposure measure computed in Alpert et al. (2018) using the 2004-2008 National Survey on Drug Use and Health (NSDUH) to guide relevant analysis, given that state-level prevalence estimates for nonmedical use of OxyContin are unavailable in the public-access files of the NSDUH. The Figure A1 in Alpert et al. (2018) classifies states into four categories in terms of their pre-reformulation OxyContin misuse rate from very low (0-0.49%), low (0.50-0.64%), high (0.65-0.79%), to very high (0.8-1.15%). Judging from this information, all switcher states except for Michigan had a high rate of pre-reformulation OxyContin misuse. While the observation period of Oklahoma ended before 2010, reformulation could impact our results obtained for the remaining switchers.

[14] These states include Colorado (2007), Connecticut (2008), Delaware (2012), Maine (2004), Maryland (2010), Massachusetts (1994), Minnesota (2010), Montana (2012), New Hampshire (2014), New Jersey (2011), New Mexico (2005), Ohio (2006), Oregon (2011), Vermont (2009), and Washington (2011). Data are sourced from PDMP Training and Technical Assistance Center: https://www.pdmpassist.org/Policies/Maps/PDMPPolicies, retrieved July 9, 2024.

[15] While some never-RTW states introduced MMLs and/or legal protection of dispensaries prior to 1992, including California (1996), Maine (1999), and Oregon (1998)), 8 others adopted such policies from 1992-2018 (i.e., Connecticut (2012), Illinois (2013 and 2014), Maryland (2014), Massachusetts (2012 and 2013), New Hampshire (2013), New York (2014), Rhode Island (2006), Vermont (2004). Data are drawn from Powell et al. (2018).

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reduced opioid-related harms, with the critical aspect of MMLs facilitating a reduction in overdose deaths being a relatively liberal allowance for dispensaries (Powell et al. 2018). Given the potential overlap in the population that may benefit from medical marijuana use and the presence of unions, the presence of MML-related policies could mediate the observed results.

Finally, since our current categorization of opioid misuse combines heroin, natural opioids, synthetic opioids, and unspecified narcotics, measurement errors in opioid-related mortality (Alpert et al. 2022) and the entry of illicit drugs could skew our results.

We address these concerns in Appendix C by exploring different donor pools (C.1), treatment groups (C.2), predictive covariates (C.3), definitions of opioid misuse (C.4), and placebo analysis (C.5 and C.6).

5 Characteristics of Overdose Victims

The RTW effects documented in Section 3 can be linked to various pathways that exhibit correlations with de-unionization in the existing literature. To pinpoint some of the most probable mechanisms by which RTW operates, Appendix D profiles overdose victims by implementing the SCM separately for individuals of different age groups, genders, and educational attainment. Unlike income and labor market participation, these characteristics are generally immune to the effects of opioid misuse (with the possible exception of education), mitigating concerns regarding reverse causality.

In summary, we find that the RTW effects are primarily limited to males aged 18 to 54 years, or individuals who tend to be involved in the labor market, engage in physically demanding occupations, and place a higher significance on their work roles as a source of job satisfaction. Moreover, the results show that RTW disproportionally affects economically disadvantaged groups (i.e., individuals without a college degree). These observations align with the idea that RTW laws limit unions' ability to improve specific physical and psychosocial workplace conditions (e.g., wages, fringe benefits, and worker safety) rather than engaging in political advocacy and that the wide-ranging protections against labor market volatility that unions provide are particularly crucial for less skilled workers in the maintenance of their physical and mental health.

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6 Underlying Mechanisms

Work is central to the psychological health and well-being of individuals. A job is more than just not being unemployed – dimensions of job quality are critical considerations for workers. At the institutional level, concern for the quality of employment has led to changes introduced in the organization of work since the 1990s (e.g., OSHA), and most scholars agree that the focus should not only be on creating more jobs but also on creating better jobs (see Cascales Mira, 2021, for a literature review). Thus, guided by the findings in Section 5, we explore several work-related risk factors that may serve as potential channels to elucidate the relationship between RTW and opioid misuse. The specific mechanisms examined include occupationally induced fatalities and nonfatal injuries/illness (Section 6.1), self-rated health status (Section 6.2), income and wages (Section 6.3), as well as unemployment and long working hours (Section 6.4).

6.1 Workplace Safety

In light of the evidence that attributes union activity to improving occupational health and safety, Figure A6a-A6d and Tables A9 present the SCM results for RTW on occupationally induced fatalities and nonfatal injuries/illnesses. The data comes from the BLS SOII and CFOI. Due to the year-to-year variations in state participation in the SOII and the missing information on public sectors in a significant number of participating states before 2001, we restrict the event window to 2002-2018 to increase the size of the donor pool and pre-RTW match quality. As a result, Indiana, Michigan, and Wisconsin are included as the treated units for the four-year window. Michigan and Wisconsin are included for the six-year window analysis.

Even though there is little difference in the total number of fatal and nonfatal incidents when pooled together (Figure A6a-A6b and columns 1-2 of Table A9), we observe a significant rise in the number of workplace fatalities when disaggregated from nonfatal events (Figures A6c-A6d and columns 3-4 of Table A9). This effect begins 2-3 years after RTW enactment and then persists over time, suggesting that workplace fatalities would have been 13-15% lower between the fourth and sixth year of RTW enactment (i.e., 0.4 and 0.6 cases per 100,000 workers by the fourth and sixth years, respectively), relative to the baseline had the states not adopted the policy. This result is consistent with prior research findings that non-union workers are less likely to report workplace injuries due to fear of retaliation from employers (Hirsch et al. 1997; Leigh and

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Chakalov 2021; Johnson et al. 2022). Since it is not otherwise rational to forego reporting workplace fatalities, and there is little reason to think that fatal injuries increased while nonfatal injuries tended in the opposite direction, we interpret the combination of psychological factors (e.g., job insecurity) and a riskier physical work environment as a plausible underlying mechanism in which RTW laws increase opioid misuse.

6.2 Personal Health

Since not all occupationally induced injuries resulting in long-term pain management would be classified as a workplace accident under OSHA standards and hence reported by the employer (e.g., wrist and shoulder pain from working prolonged hours or depression and anxiety due to work-related stress), [16] the estimated injury effect in Section 6.1 will likely underestimate the RTW impact on workers' physical and mental conditions. This section thus examines self-rated health status using data from the 1996-2018 CPS, during which period relevant questions were asked.

Figure A7 and Table A10 show the estimated RTW effects for all individuals on two different measures of subjective health, health on a five-point scale with a higher value corresponding to a worse status (columns 1-2) and the incidence of a "fair" or "poor" report (columns 3-4). Results reveal a consistent decline in perceived health across samples and outcomes despite a relatively low pre-RTW match quality for the former (i.e., 82-89%). Treating the latter as our preferred measure, which has a pre-RTW match quality greater than 92%, we observe an average decrease in the likelihood of reporting a "fair" or "poor" rating by 1 ppt within the 4-6 years of RTW enactment, representing an 8-9% increase from the baseline.

These declines are driven by working-age individuals, the demographic group for whom the results for opioid misuse are observed (Figure A6e-A6h and Table A11). With a substantially improved match quality across the board (i.e., 98-100%), results show that the likelihood of

[16] OSHA has specified which workplace incidents need to be recorded by employers. These include (1) death, (2) days away from work, (3) restricted work activity or transfer to a new or different job due to injury/illness, (4) medical treatment beyond first aid, and (5) loss of consciousness. Specific additional reporting criteria for less generalizable workplace circumstances include, for example, needlestick and sharp object injury, cases resulting in blood contamination, cases involving medical removal of specific chemical substances, cases involving occupational hearing loss, and tuberculosis exposure in the workplace.

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reporting a "fair" or "poor" rating declined by almost the same amount as for the all-individual case for the four and six-year samples (i.e., 1 ppt or 9-10% relative to the baseline). On a five-point scale, the corresponding decline averages at 0.05 points or 2% from the baseline. In relative terms, therefore, it appears that most of the decline in perceived health is driven by an increase in the incidence of a "fair" or "poor" report.

While the initiation of opioid use may stem from pre-existing physical or mental health conditions, or stress as a coping mechanism, opioid dependence can, in turn, affect perceived health either directly through the consumption of opioids or indirectly via the detrimental health and social consequences of opioid misuse. Although we do not have credible identifying variations to explicitly address the issue of reverse causality, the temporal patterns observed for self-rated health and opioid mortality suggest that in the same switcher states (i.e., Indiana, Michigan, and Wisconsin), the decline in the former occurred at least 1-2 years before the increase in the latter. Moreover, while not shown in the paper, we observe a one-year lag for the treatment admission of opioids when Michigan and/or Wisconsin are individually examined. Based on the current evidence, it appears unlikely that the association between RTW and self-rated health is solely driven by the higher incidence of opioid misuse.

6.3 Income and Wages

Unions can play a crucial role in setting wages as well as income inequality across the economy. Low incomes not only directly impact individual health by exposing people to risk factors like stress, toxins, and poor-quality housing, but they also correlate with important mediating factors related to opioid-related harm, such as access to treatment.

Figure A8a-A8d and Table A12 examine the impact of RTW on before-tax, after-transfer household income and individual wages using data from the1990-2019 CPS. With a pre-RTW match quality of 97-100%, we observe a decline in household income across event windows for states in both the four-year and six-year windows that is primarily driven by the decrease in wages. Column 4 of Table A12 estimates a $1,649 reduction in wages starting in the second year of RTW implementation, which accounts for 5-6% of the pre-RTW level. The decrease in wages steepens to $2,633 in the sixth year, or a decline of 7-8% from the pre-RTW level. The 4-year sample in column 3 shows similar results. Overall, the decrease in individual wages accounts for 55-83% of the overall decline in household income. Importantly, the wage-led decline in

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household income coincides with an increase in opioid overdose behavior. This suggests that greater exposure to material deprivation or a deteriorating financial situation associated with the passage of RTW laws may partly explain the adverse effects observed in this study.

These results bolster the existing findings on the wage depression impact of de-unionization for both union and non-union workers (Card et al. 2020; Farber et al. 2021), including the influential study by Fortin et al. (2023), who document the wage and unionization effects of recent RTW adoption in five U.S. states, where three out of the five overlap with the switcher states examined in our analysis.

6.4 Employment and Long Working Hours

Are the declines in wages observed in Section 6.3 primarily driven by changes in unemployment, working hours, or hourly wages? Knepper (2020) posits that the key target of collective bargaining is not necessarily wages but rather fringe benefits, an important determinant of job quality. Thus, while there are conflicting theories regarding the employment effects of unionization, both on the extensive (employment) and intensive (hours worked) margins (Montgomery, 1989), the importance workers place on the non-pecuniary benefits of employment is non-trivial.[17],[18] Beyond the most obvious risk of financial hardship, job loss and unemployment inflict significant, long-term psychological and social costs on individuals, particularly low-wage workers. Thus, weakening of union strength can create a dual burden on the well-being of workers. On the one hand, the risk of job loss, job displacement and unemployment can elevate health risks and lead to coping strategies such as opioid use. On the other hand, while being employed can positively influence overall health, worsening job quality,

[17] In April 2016, unions representing nearly 40,000 employees of Verizon, the largest telecommunications company in the U.S., went on strike to protest the company's decision to cut health care and retirement benefits despite reportedly offering a six percent 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.

[18] 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.

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characterized by increased workloads, reduced benefits (e.g., cuts to health insurance coverage) can harm worker well-being and contribute to opioid misuse.

Figures A8e-A8f and columns 1-2 of Table A13 present the estimated effects of RTW laws on employment rates, based on data from 1990-2019 CPS. Since workers may either remain unemployed or exit the labor force after being displaced, we consider the probability of being employed as a more comprehensive measure of economic opportunities compared to unemployment rates, although the results for unemployment are quite similar. Our findings indicate a positive impact of RTW laws on employment, with employment rates increasing by 0.5-0.7 ppt in RTW-adopting states 4-6 years after implementation. This represents about a 1% increase relative to pre-RTW levels. These results align with existing research on the aggregate labor market implications of unionization, which suggests that stronger unions tend to decrease employment and increase unemployment by a small but significant margin (Montgomery 1989; Blanchflower et al. 1991).

Meanwhile, the share of workers working long hours also increased following the passage of RTW (Figures A8g-A8h and columns 3-4 of Table A13). Examining a subset of the CPS from 2001 to 2018, during which data on usual working hours were available, shows that RTW resulted in a 6-10% rise in the share of workers logging more than 45 hours per week compared to pre-RTW levels (or 1-2 ppt). However, it should be noted that the pre-RTW match quality for working hours is relatively low (i.e., 92-97%), and the observable effects appear to taper off a few years after the implementation of RTW. Additionally, the volatility of the data series prevents us from attaining a satisfactory pre-RTW match quality for other definitions of long hours. Despite these limitations, our findings are consistent with those of Gihleb et al. (2024), who report similar trends among full-time employees working more than 45 hours per week and for other criteria related to long hours in states that enacted RTW laws between 2005 and 2019.

7. Comparing SCM with DiD Estimates

Due to the limitations of the SCM mentioned in Appendix C, we replicate the main findings using the staggered DiD estimators proposed by Callaway and Sant'Anna (2021). These estimators identify causal effect parameters even when differences in observed characteristics lead to non-parallel outcome dynamics between groups or units that are first treated at different times. They

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also enable us to estimate relevant parameters for each group based on their specific treatment periods, thereby addressing the challenges posed by heterogeneous and dynamic treatment effects.

To this end, we begin by performing a log transformation on both nonfatal and fatal overdoses to linearize their exponential growth patterns and reduce variance (Powell 2018). We utilize 6 years of pre-RTW and 6 years of post-RTW data to maintain a relatively equal length for both the pre- and post-periods. We define R_{st} as the time relative to RTW adoption and subsequently implement the following equation using three DiD methods: outcome regression, inverse probability weighting, and an improved doubly robust procedure to confirm our results.

$$Y_{st} = \alpha_s + \phi_t + \sum_{r \neq 0} \mathrm{I}[R_{st} = r]\beta_r + X'_{st}\theta + \varepsilon_{st}.$$

Since the summation runs over all possible values of R_{st} except for 0, the year before RTW adoption, β_r captures the change in outcome relative to its pre-RTW level r years after a RTW law passes.

In addition to state and year fixed effects (α_s and ϕ_t), we adjust several covariates that may influence both RTW adoption and opioid misuse. These covariates include gender, age, educational attainment, and concurrent policies including PDMP, the Triplicate program, and MML. To avoid mechanical endogeneity, we deliberately exclude factors that could be influenced by RTW adoption, such as household income, employment status, and industry composition. Standard errors are clustered at the state level. Due to the limited number of switcher states, which can vary greatly in size, we utilize wild cluster bootstrapped t-statistics to correct for potential bias. (MacKinnon and Webb 2017).

Figures 5a and 5b display the estimated dynamic effects of RTW for the same switcher states analyzed in the SCM using the outcome regression DiD estimator. There is no evidence of pre-trends, as all pre-RTW estimates are statistically insignificant. In contrast, most of the post-RTW coefficients are positive and significantly different from zero. In both cases, the magnitude of these coefficients gradually increases over time. Notably, there is a delay of three years in the effects of RTW on fatal overdoses compared to non-fatal overdoses, consistent with the findings from the SCM analysis. On average, these estimates indicate that RTW increased non-fatal misuse by 36% and fatal misuse by 22%. These magnitudes are comparable to those obtained from the SCM analysis, which found increases of 13% and 34%, respectively.

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The DiD method does not require a balanced panel for each treated unit; therefore, Figures 5c-5d and Panel B of Table 5 include switcher states that do not meet the data requirements for the SCM and are thus excluded from the main analysis (i.e., Indiana (2012) in the analysis of nonfatal overdoses and Oklahoma (2001) in the analysis of fatal overdoses). Figures 5e-5f and Panel C of Table 5 further incorporate two late adopters of RTW during the observation period: West Virginia (2016) and Kentucky (2017). Neither modification has any substantive impact on our estimates.

Panel C of Table 5 shows the estimated group average treatment effects for the sample that includes all RTW adopters from 2001 to 2018. While there is no consistent pattern observed between states with higher pre-existing union density and those with lower union density, we do find a statistically significant positive impact of RTW laws on opioid misuse in 7 out of the 8 states/groups that have at least four years of post-period data. The exceptions are West Virginia and Kentucky, where RTW adoption occurred 1-2 years before 2018.

Finally, Panel D of Table 5 presents results obtained through inverse probability weighting and improved doubly robust DiD estimators. We face a limited overlap problem due to the small number of treatment units (Callaway and Sant'Anna, 2021). As a result, we exclude the problematic covariates from the regressions whenever this issue arises, and we do not consider these cases as our preferred specifications. Nevertheless, our conclusions remain largely unchanged.

Overall, the DiD estimates support the view that the adoption of RTW laws leads to an increase in opioid misuse. These estimates are consistent with those generated by the SCM analysis, both in terms of the magnitude and timing of the observed effects of RTW. They also remain robust even with the additional inclusion of the switcher states that were omitted from the SCM analysis. While recent research has highlighted several significant limitations of the pre-testing approach as a validation tool for the conditional parallel trend assumption — including the potential for low power and pre-test bias (see Roth et al., 2023 for a literature review) — the largely consistent patterns that emerged from these two approaches lend additional credibility to our results.

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8. Conclusion

This paper explores the implementation of RTW laws as a potential explanation for the rise of opioid dependence in the U.S. We hypothesize a decline in unionization rates as the primary channel through which RTW laws impact opioid dependence, where the typical union beneficiary coincides with the population most affected by the crisis – less educated working-age males. Taking advantage of the four states that instituted RTW after the 2000s (i.e., Oklahoma, Indiana, Michigan, and Wisconsin) while also offering sufficient pre- and post-invention information, we conduct a synthetic control analysis to evaluate the causal impact of RTW on drug misuse. The synthetic control approach offers a systematic way to select the control group and purges the effects of time-varying unobservable confounders from the estimates, providing valid inferences in a scenario where the standard cluster-robust variance estimator does not apply. It also allows cross-validation of results obtained from the canonical two-way fixed effects models in earlier studies.

To allow for a potentially delayed response to RTW implementation, we analyze two samples where a different set of switcher states is matched to a different pool of donor states. This method enhances the study's external validity by considering the states with varying levels of pre-existing union density, albeit resulting in variations in the estimated RTW effects. Among the four states in our study, the fact that we find a significant impact of RTW on opioid misuse between the states that have relatively high levels of unionization (i.e., Michigan and Wisconsin) and at least one state that has had below-average union density and weak union support (i.e., Indiana) suggests that the detriment of RTW operates independently of the underlying business climate (Holmes 1998) or pre-existing anti-union sentiment among state residents. From this perspective, we believe our findings have broader implications despite the state-specific analysis.

Our results suggest that the decline in the unionization rate due to RTW laws led to a sustained increase in opioid dependence. Accounting for the RTW laws going into effect in the switcher states, there was a 13% increase in opioid-related treatment admissions and a 34% rise in opioid-related overdose deaths, on average, compared to pre-RTW levels within 4-6 years following the implementation of RTW laws. While these percentages are significant, the raw numbers reveal a far more alarming trend in comparison: during the same post-implementation period, these states experienced an average 151% increase in opioid-related nonfatal overdoses and a staggering

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953% surge in opioid-related fatal overdoses. Thus, in the context of the overall impact, the introduction of RTW laws may account for 4-8% of the observed increase in opioid addiction.

Consistent with studies examining the union density, wage, and employment consequences of RTW laws (Fortin et al. 2023; Murphy 2023; Gihleb et al. 2024), our first-stage findings indicate that RTW adoption induced an average decline of 14% in unionization rates. This decline, coupled with our reduced form estimates on opioid misuse, suggests that the deterioration of work conditions associated with a 10% decrease in union density resulted in increases of 9% in nonfatal overdoses and 24% in fatal overdoses in the affected states. While prior research has not analyzed the effects of unionization on opioid misuse, the magnitude of the latter estimate is comparable to the previously published figures on drug-related mortality. For example, one study finds that a 10% increase in union density correlated with a 27% decrease in the all-drug death rate using data from 1999-2016 MCOD (DeFina and Hannon 2019; model 2 on p. 9). Another study, which used pre-1999 mortality data, estimates that a 10% increase in the three-year moving average of union density led to a 17% decrease in deaths from drug poisoning, suicide, and alcohol (Eisenberg-Guyot et al. 2020).

In contrast to previous literature, we do not find a causal link between RTW laws and the misuse of nonopioid drugs. Opioids pose a higher risk for addiction and mortality relative to nonopioid analgesics. Studies on mortality rates, however, suggest that nonopioid overdose rates rose almost as fast as those involving opioids during the years 1999-2016, roughly the same time frame as that of the current study (Ruhm 2019). Importantly, since fatal overdoses increasingly involve the simultaneous use of nonopioids and opioid drugs (Ruhm 2019), this implies that the RTW-induced opioid harms are confined to the consumption of opioids without concurrent involvement of nonopioids. Hence, while this trend could be driven by substitution away from nonopioid drugs toward cheaper and more potent illicit opioids such as heroin and fentanyl, another plausible explanation is that most misuses of drugs are initiated in the context of pain, given the prominent role opioids play in pain management and the prevalence of musculoskeletal pain in industries most affected by the decline of unions.

Indeed, our mechanism investigation indicates that RTW legislation results in social and labor market conditions conducive to increases in chronic incidences of pain among the residents of the RTW-adopting states. While this analysis is exploratory, our findings – along with related

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studies that find a concentration of opioid-related overdoses in industries and occupations exhibiting higher rates of occupational injuries/illness and higher job insecurity, such as construction and fishing (Hawkins et al. 2019) – do provide some interesting insights. First, RTW laws increased the incidence of fatal injuries among the switcher states by 14% compared to pre-RTW levels, four to six years after the adoption of these laws. When viewing it side by side with existing findings on the occupational safety effects of RTW laws (e.g., Zoorob 2018), this evidence suggests that RTW may lead to higher rates of opioid dependence by exposing workers in physically demanding jobs to more hazardous working conditions, such as toxic chemicals or fatigue and sleep deprivation resulting from frequent overtime requirements.

Second, to the extent that traumatic injuries miss out on occupationally induced chronic conditions that would not be classified as an OSHA event, we find a significant decline in perceived health in exposed states after RTW passage. Research finds evidence of a causal relationship between work-related musculoskeletal pain (e.g., arthritis, rheumatism, chronic back or neck problems, and frequent severe headaches) treatment and opioid misuse, addiction, and overdose fatalities. For example, opioids initiated for musculoskeletal pain are strongly associated with long-term opioid use and OUD among construction workers and they are more likely to be prescribed opioids for a longer duration than other workers (Dale et al 2021). The observed decrease in self-rated health is due to an increase in the likelihood of a "fair" or "poor" rating and concentrates among the working-age population, the group that drives our overdose results. This is plausible if RTW increases the incidence of chronic diseases such as musculoskeletal pain when it decreases flexible work arrangements, such as the ability to choose shifts, take breaks, require predictable schedules and hours or work from home. The less flexible work schedule could also reduce the likelihood of workers seeking care or caring for sick family members, which can, in turn, induce stress and strain related to the job (Leigh and Chakalov 2021). Likewise, RTW may contribute to the rise of mental illness (e.g., mood or anxiety disorder and psychiatric symptoms) when individuals are less protected against the physical and psychological effects of stress as the potency of collective bargaining declines. The fact that the decline in perceived health was observed at least 1-2 years earlier than the rise in opioid misuse for the same exposed states addresses possible concerns over reverse causality and provides the alternate possibility that the deterioration in personal health might serve as, at least in part, a determinant, rather than a consequence of the more prevalent opioid misuse.

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Third, supporting the evidence related to self-rated health, we observe a 4% decline in real wages in states that have implemented RTW laws, within 4 to 6 years of the law's implementation. Furthermore, our results indicate that "long working hours" may be another potential explanation for the observed relationship between RTW laws and opioid misuse. We find that RTW resulted in a 6-10% rise in the share of workers logging more than 45 hours per week compared to pre-RTW levels. This rise in working hours, combined with increased exposure to material deprivation, can affect job quality – a critical aspect of what constitutes a "good job." Although, there is no definitive answer from scholars on the attributes of a "good job" but according to Clark (2015, p3), "Good jobs were those that were well-paid, with perhaps some attention to the length of the work week." The associated stressors from worsening job quality, which are highly correlated with job satisfaction (Clark 2015), may lead to a higher tendency to engage in unhealthy behaviors, ultimately increasing the risk of opioid misuse, as shown by Chen and Islam (2023). Using variations in RTW implementation among contiguous counties that straddle state borders, the authors show that RTW-overdose linkage can be partially stress-driven. They note that the decline in unionization due to RTW passage in six adopter states has led to increased concerns about workplace safety, poorer employee-employer relationships, less worker autonomy, and a lower level of job and financial satisfaction for the non-college-educated workforce. Our findings are also consistent with those of Gihleb et al. (2024), who report similar trends among full-time employees working more than 45 hours per week and for other criteria related to long hours in states that enacted RTW laws between 2005 and 2019. These results raise the possibility that job-related stress, driven by pecuniary and non-pecuniary factors, may explain the more prevalent risk behaviors among the economically vulnerable.

Taken together, our study demonstrates that the RTW legislation is one possible cause of the rising opioid dependence and the geographic diversity in the fatal and nonfatal opioid overdoses in the U.S. since the 2000s. Assuming that the RTW legislation primarily affects overdose behavior by diminishing union power, these results also underscore the crucial role of labor unions in insulating the working class from the vicissitudes of market fluctuations as labor continues to struggle for a place in political decision-making. Considering that de-unionization disproportionately affects the less educated working-age males, low-status workers may be at "double jeopardy" for the adverse effects of declining unionization. The two recent RTW reversals (i.e., Missouri and Michigan) as well as the Gallup data on rising support of unions in

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the post-Great Recession and post-COVID America appear to confirm our results that the working class is reacting to the pain of anti-union legislation.

Given our finding of a null effect on the misuse of non-opioids, the role of de-unionization in drug-related mortality identified in this study is less extensive than what is reported in existing studies. Nevertheless, our analysis establishes a connection between de-unionization and the fundamental nature of production, which ultimately shapes a nation's quality of life. Our results support earlier findings that the institutional functions of labor unions extend beyond purely economic issues. Consequently, the implications of declining unionization in the U.S. could be more profound and far-reaching than has been previously recognized.

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Data availability: All data utilized in this study is publicly accessible. This includes the Treatment Episode Data Set, the Centers for Disease Control and Prevention's Multiple Cause of Death database, the Current Population Survey, the Union Membership and Coverage Database, the Bureau of Labor Statistics' Survey of Occupational Injuries and Illnesses, and the Bureau of Labor Statistics' Census of Fatal Occupational Injuries.

Declarations

Conflict of interest: The authors declare no competing interests.

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Table 1 Descriptive Statistics of Primary Outcomes

	Average	Never-RTW States	Switchers (4-Year Sample)	Switchers (6-Year Sample)
Panel A: Admission Rate, per 100,000 people (TEDS 1992-2018)				
Opioid	213.75	237.37	72.07	90.89
(SD)	(224.24)	(232.05)	(74.85)	(83.17)
Non-Opioid	225.19	235.72	162.04	206.57
(SD)	(106.90)	(107.85)	(74.91)	(44.64)
All Drugs	438.95	473.09	234.11	297.45
Panel B: Mortality Rate, per 100,000 people (MCOD 1999-2018)				
Opioid	9.49	9.18	6.90	6.61
(SD)	(7.27)	(6.76)	(5.09)	(5.46)
Non-Opioid	5.09	4.84	5.80	6.82
(SD)	(2.56)	(2.56)	(2.44)	(2.36)
All Drugs	14.58	14.02	12.71	13.43
Panel C: Changes in Opioid-Related Admissions (1992-2018)				
Admission rate in 1992	81.97	91.97	21.92	32.23
Admission rate in 2018	401.34	442.18	156.30	185.49
% Change between 1992 and 2018	389.62	380.79	613.05	475.52
States Included	20-21 States	18 States	OK, MI, and WI	OK and MI
Panel D: Changes in Opioid-Related Mortality (1999-2018)				
Mortality rate in 1999	3.51	4.13	1.2	1
Mortality rate in 2018	19.8	19.74	17.07	18.13
% Change between 1999 and 2018	465.43	377.97	1322.5	1713
States Included	21-22 States	19 States	IN, MI, and WI	IN and MI

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Table 2 Union Membership and Coverage Rates (11-Year-Pre-Period)

	Union Membership		Union Coverage	
	4-Year Window	6-Year Window	4-Year Window	6-Year Window
	(1)	(2)	(3)	(4)
Panel A: Overall Estimates				
Average Treatment Effect	-1.714***	-1.241**	-1.728**	-1.239**
	(0.000)	(0.038)	(0.012)	(0.018)
Panel B: Individual Estimates				
1 Year After	-1.666***	-0.828***	-1.679**	-1.194*
	(0.000)	(0.001)	(0.020)	(0.065)
2 Years After	-1.685***	-0.922***	-1.417*	-0.692*
	(0.000)	(0.006)	(0.072)	(0.091)
3 Years After	-1.496***	-0.836*	-1.577**	-0.733*
	(0.000)	(0.074)	(0.021)	(0.089)
4 Years After	-2.010***	-1.421***	-2.238***	-1.693***
	(0.000)	(0.002)	(0.000)	(0.005)
5 Years After		-1.586**		-1.453**
		(0.010)		(0.021)
6 Years After		-1.853***		-1.666**
		(0.005)		(0.022)
Pre-RTW Match Quality	0.986	0.986	0.949	0.946
Pre-RTW Rate	11.56	11.53	12.45	12.43
# of Potential Donors	23	23	23	23

Notes: Standardized placebo-based p-values are reported in parentheses. The pre-RTW rate indicates the number of individuals belonging to a union or covered by a union contract per 100 workers in the switcher states one year before RTW passage.

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Table 3 Treatment Admissions (SCM)

	Opioids		All Drugs		Non-Opioid Drugs	
	4-Year Window	6-Year Window	4-Year Window	6-Year Window	4-Year Window	6-Year Window
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall Estimates						
Average Treatment Effect	9.352***	16.380***	31.20	60.742*	37.702	48.608*
	(0.000)	(0.006)	(0.157)	(0.077)	(0.169)	(0.058)
Panel B: Individual Estimates						
1 Year After	-18.496***	-19.757***	2.752	19.390***	22.128***	32.236***
	(0.000)	(0.000)	(0.333)	(0.009)	(0.000)	(0.000)
2 Years After	2.093***	-0.096***	26.402	50.641***	41.012***	57.693***
	(0.002)	(0.000)	(0.168)	(0.012)	(0.002)	(0.000)
3 Years After	12.460***	4.100***	38.082	43.534**	26.317**	39.102***
	(0.000)	(0.000)	(0.397)	(0.028)	(0.031)	(0.003)
4 Years After	41.352***	39.780***	57.565	108.174***	61.351**	75.359***
	(0.001)	(0.000)	(0.141)	(0.000)	(0.014)	(0.003)
5 Years After		32.561***		51.356		50.500**
		(0.000)		(0.160)		(0.012)
6 Years After		41.501***		91.357		36.757
		(0.000)		(0.324)		(0.238)
Pre-RTW Match Quality	1.000	1.000	0.996	0.997	0.992	0.997
Pre-RTW Case Rate	87.36	102.28	219.46	269.01	132.11	166.73
# of Potential Donors	18	18	18	18	18	18

Notes: Standardized placebo-based p-values are reported in parentheses.

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Table 4 Overdose Deaths (SCM)

	Opioids		All Drugs		Non-Opioid Drugs	
	4-Year Window	6-Year Window	4-Year Window	6-Year Window	4-Year Window	6-Year Window
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall Estimates						
Average Treatment Effect	0.815***	4.074***	-1.336	1.043	1.589	2.158
	(0.000)	(0.008)	(0.936)	(0.911)	(0.113)	(0.107)
Panel B: Individual Estimates						
1 Year After	0.119	0.905	0.127	1.036	1.293***	1.985***
	(0.750)	(0.355)	(0.836)	(0.444)	(0.006)	(0.000)
2 Years After	0.052	0.867	-0.405	0.839	1.930**	3.072***
	(0.729)	(0.601)	(0.637)	(0.605)	(0.017)	(0.000)
3 Years After	0.726	2.366	0.107	1.464	1.820**	2.925***
	(0.927)	(0.258)	(0.875)	(0.609)	(0.023)	(0.003)
4 Years After	2.364	4.155	-1.165	-0.343	1.312	1.176
	(0.398)	(0.161)	(0.561)	(0.936)	(0.396)	(0.881)
5 Years After		6.266**		0.780		2.190
		(0.030)		(0.919)		(0.324)
6 Years After		9.884***		2.482		1.601
		(0.000)		(0.652)		(0.357)
Pre-RTW Match Quality	1.000	1.000	0.991	0.947	0.946	0.814
Pre-RTW Mortality Rate	7.77	6.20	14.33	14.1	6.57	7.9
# of Potential Donors	19	19	19	19	19	19

Notes: Standardized placebo-based p-values are reported in parentheses.

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Table 5 Treatment Admissions and Overdose Deaths (DiD)

	Treatment Admissions	Overdose Deaths
Panel A. Dynamic Effects (Original Sample)		
Post-RTW Average	0.357***	0.219**
	(0.102)	(0.107)
1 Year After	0.011	0.0003
	(0.098)	(0.078)
2 Years After	0.234***	0.042
	(0.065)	(0.085)
3 Years After	0.365***	0.180*
	(0.131)	(0.099)
4 Years After	0.597***	0.221
	(0.160)	(0.150)
5 Years After	0.508***	0.519***
	(0.131)	(0.142)
6 Years After	0.538***	0.569***
	(0.205)	(0.127)
Switcher States	OK, MI, WI	IN, MI, WI
Panel B. Dynamic Effects (Alternative Samples)		
Additional Early Adopters	0.337***	0.211**
	(0.093)	(0.087)
Switcher States	OK, IN, MI, WI	OK, IN, MI, WI
Additional Early + Late Adopters	0.220*	0.128
	(0.118)	(0.097)
Switcher States	OK, IN, MI, WI, WV, KY	OK, IN, MI, WI, WV, KY
Panel C. Heterogeneous Effects (Additional Early + Late Adopters)		
Oklahoma (2001)	0.478***	0.176**
	(0.119)	(0.085)
Indiana (2012)	0.284**	0.190
	(0.143)	(0.124)
Michigan (2013)	0.250**	0.397***
	(0.127)	(0.078)
Wisconsin (2015)	0.334**	-0.003
	(0.123)	(0.951)
West Virginia (2016)	-0.187	-0.269
	(0.213)	(0.193)
Kentucky (2017)	-0.454	-0.096
	(0.351)	(0.120)
Panel D. Dynamic Effects (Original Sample; Alternative Estimators)		
Inverse Probability Weighting	0.300***	0.481***
	(0.074)	(0.131)
Improved Doubly Robust	0.314***	0.343**
	(0.062)	(0.169)

Notes: Wild cluster bootstrapped standard errors (1000 replications) are reported in parentheses.

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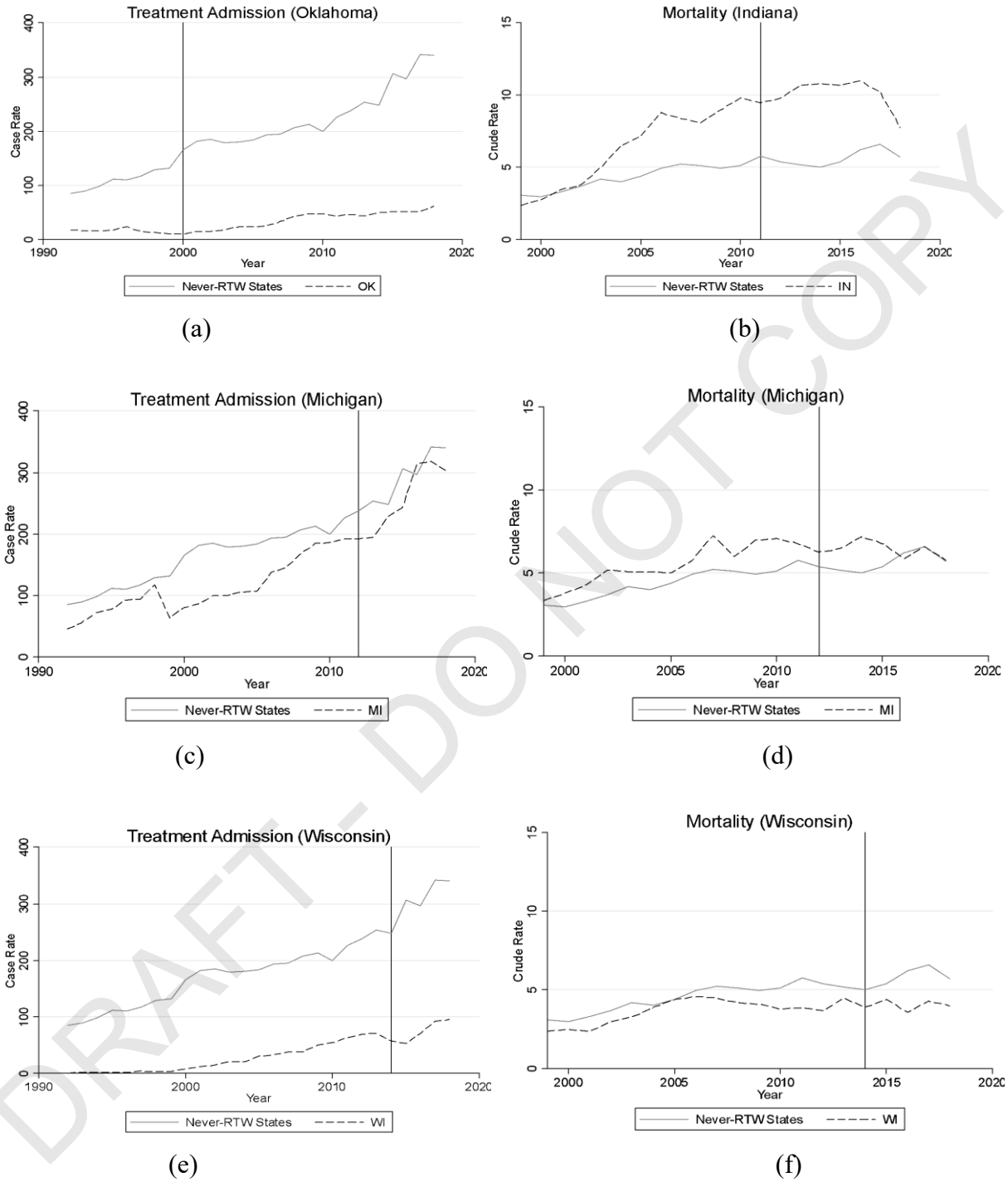
Figure 1 Unadjusted Trends in Opioid Misuse



Notes: The vertical line indicates the year before RTW adoption.

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Figure 2 Union Membership and Coverage Rates

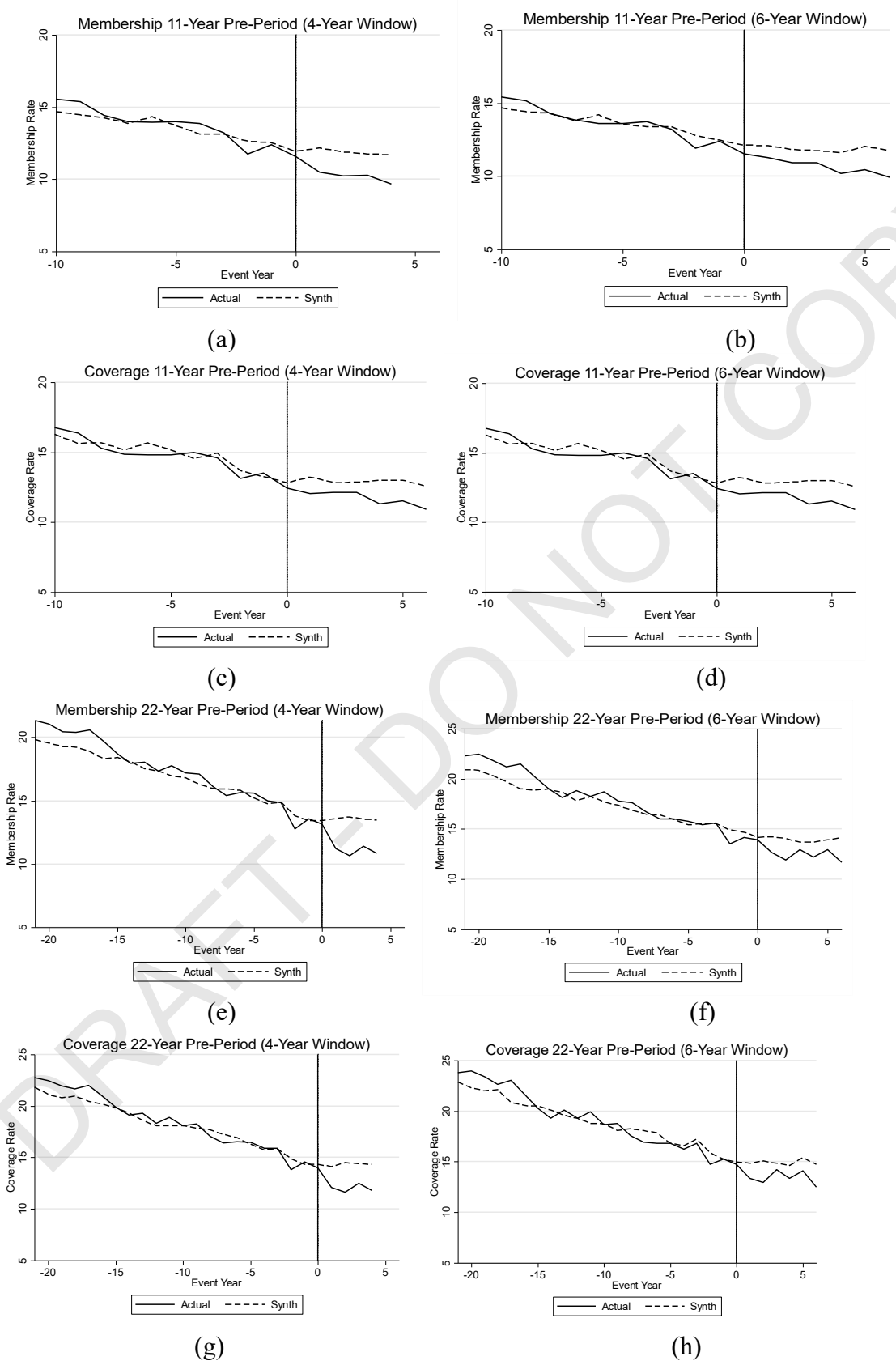

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Figure 3 Treatment Admissions (SCM)

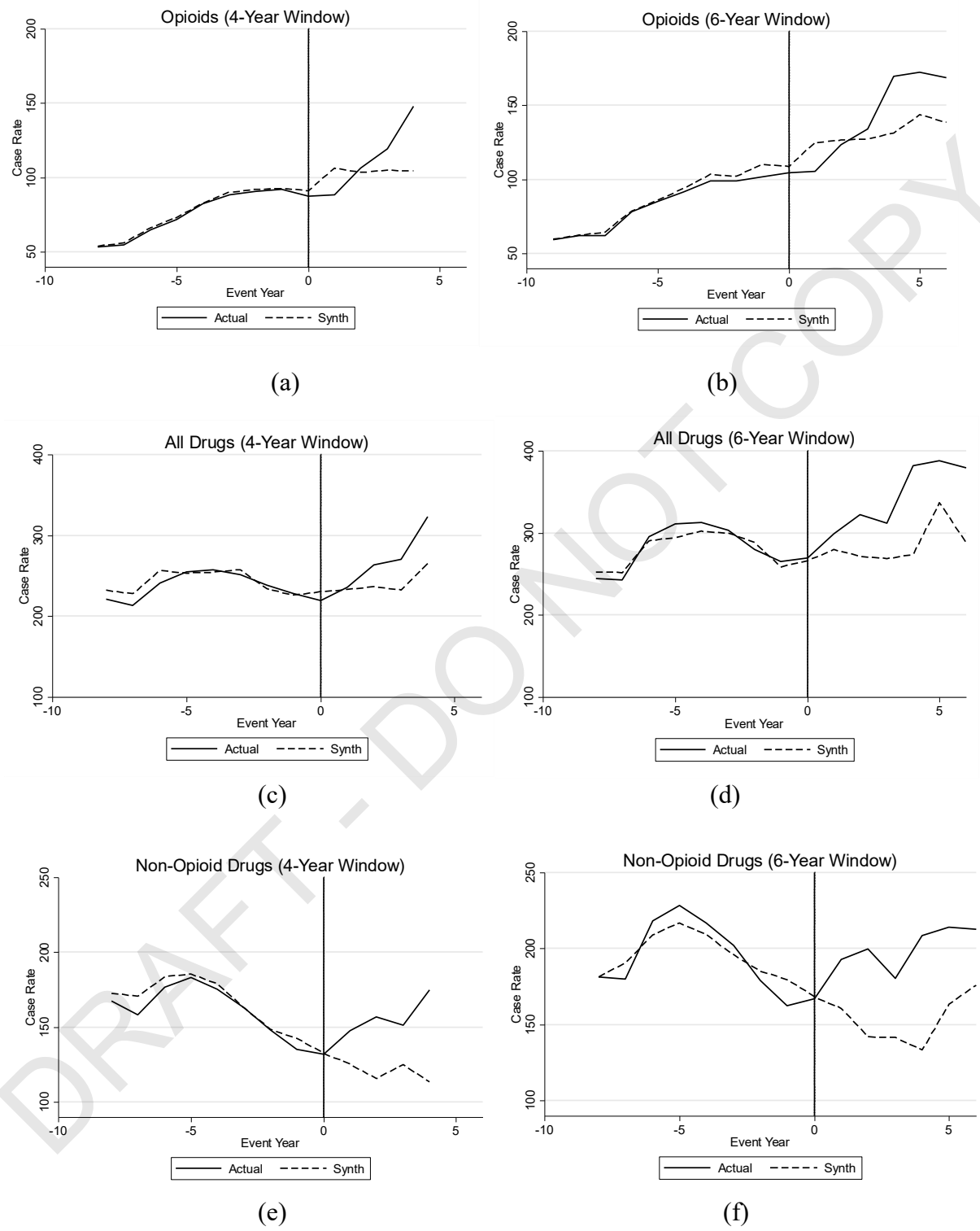

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Figure 4 Overdose Deaths (SCM)

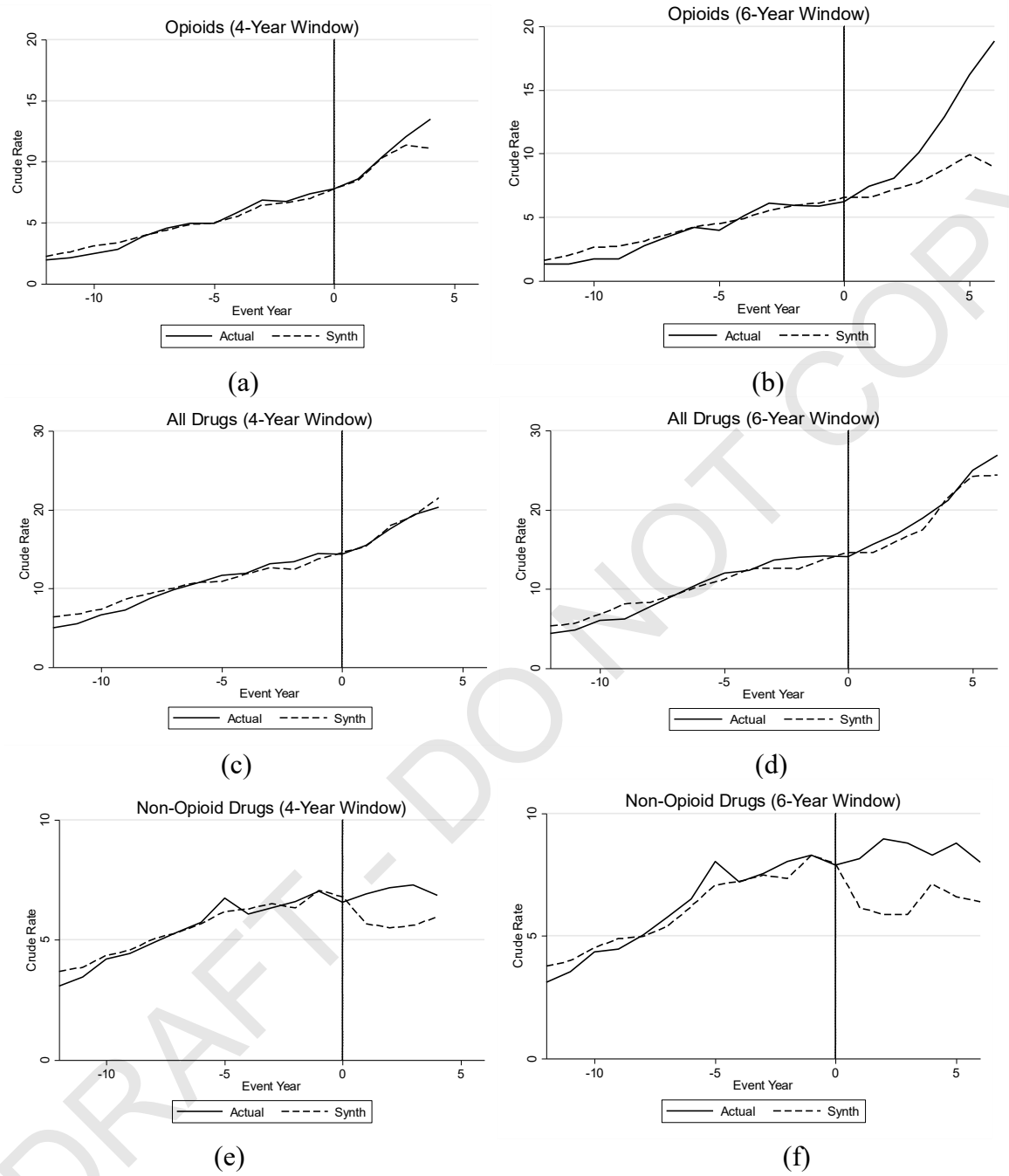

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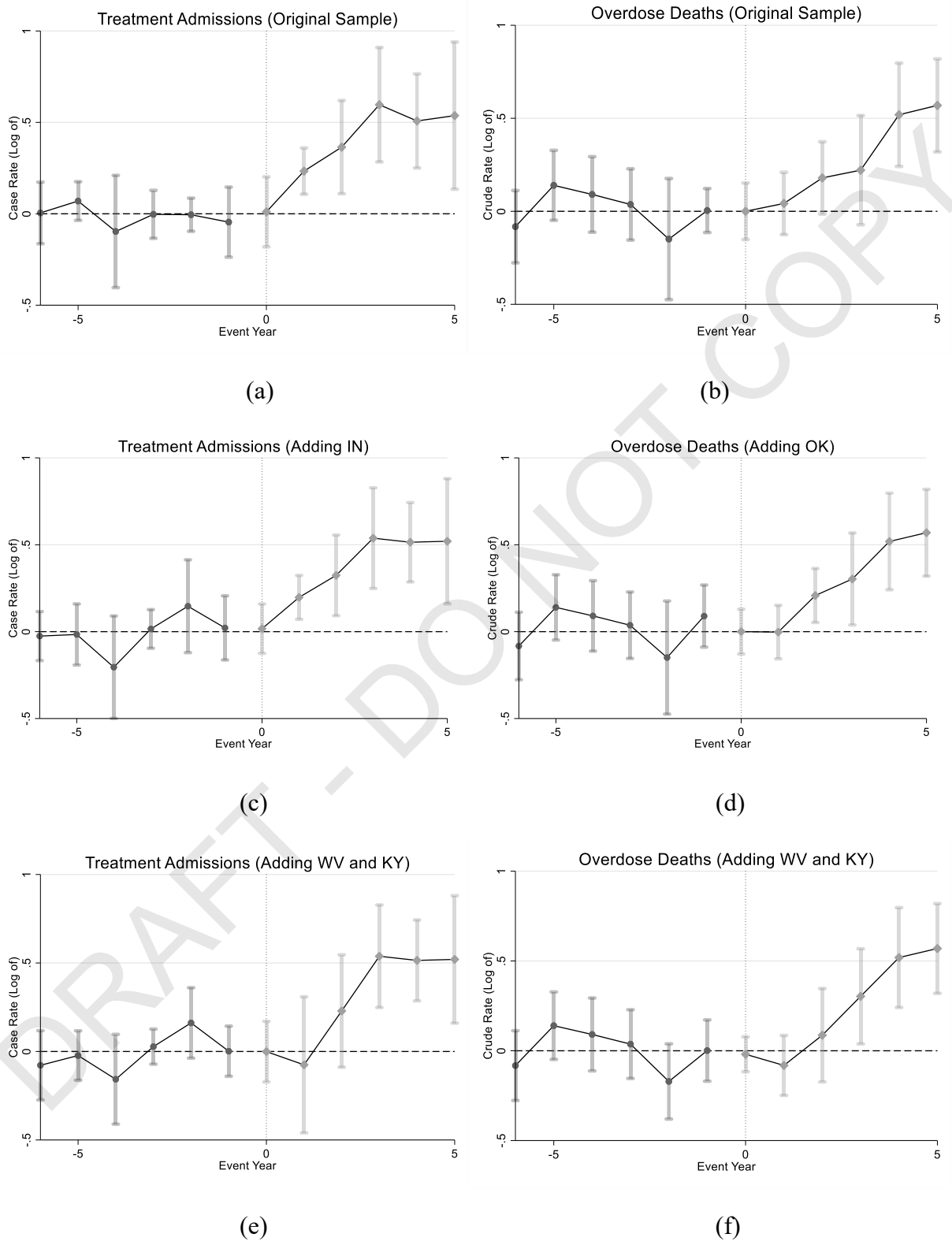
Figure 5 Treatment Admissions and Overdose Deaths (DiD)



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Online Appendixes

Appendix A Unionization and Determinants of Drug Misuse

Declining role of unions has been shown to have a negative effect on worker health (e.g., Wright 2016) and well-being (Artz et al 2022; Chen and Islam 2023). Lower unionization reduces union protections of worker rights, such as workplace safety, standard work schedules, flexibility, etc. (Gihleb et al 2024), which are particularly important for occupations involving repetitive, manual labor and/or hazardous work conditions, i.e., blue-collar jobs in manufacturing, construction, mining, etc. Blue-collar workers experience more injuries, accidents, and fatalities.[19] Research has also found evidence of a causal relationship between work-related pain treatment and opioid misuse, addiction, and overdose fatalities (Hawkins et al 2019). Opioids initiated for musculoskeletal pain were strongly associated with long-term opioid use and OUD among construction workers (Dale et al 2021). According to data from the National Institute for Occupational Safety and Health (NIOSH), 44% of workers' compensation claims in 2017 included at least one prescription for opioids. Construction workers and miners were more likely to be prescribed opioids (and for a longer duration) than other workers.[20]

It is worth noting that the overall impact of RTW laws on overdose rates is not immediately obvious, both in terms of direction and magnitude. While some studies report that states with pro-business RTW saw a much faster growth in overall employment, nominal income levels and manufacturing activity than non-RTW states (e.g., Holmes 1998), others find that the passage of

[19] Additionally, a literature review compiled by the American Psychological Association reports that blue-collar workers face frequent overtime requirements, lower managerial support, and lower control over work schedules, resulting in higher job-related stress. Overtime work, mandatory or voluntary, is also associated with higher work-related accidents in blue-collar workers due to fatigue and sleep deprivation. See *Work, Stress and Health & Socioeconomic Status*. (n.d.). https://www.apa.org. Retrieved May 19, 2025, from https://www.apa.org/pi/ses/resources/publications/work-stress-health.

[20] For a summary of the industrial-organizational psychology literature, see *Protecting Workers from Opioid Use Disorder*. (n.d.). Retrieved May 19, 2025, from https://www.apha.org/policy-and-advocacy/public-health-policy-briefs/policy-database/2021/01/13/protecting-workers-from-opioid-use-disorder.

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RTW laws is followed by a statistically significant decline in wage income of employees (e.g., Eren and Ozbeklik 2016; Fortin et al 2023). While Card et al. (2020) find that unions are crucial to raising wages for working people and reducing income inequality, Jordan et al. (2020) detect no relevant impact of RTW laws on income inequality in a study of the four states that implemented RTW laws between 1960 -2000s. To a considerable extent, the conflicting results in the strand of literature on RTW may stem from the same lack of consensus in the research on the socioeconomic impact of unions. On the one hand, unions impose certain costs on both employees and employers. The most obvious costs of unions are the mandatory dues and potential loss of income due to strikes for members (Hammer and Avgar 2007), and lower investment and productivity for firms (e.g., Holmes, 1998; Alder et al., 2023). Unionization also impedes a firm's ability to compete and survive in a globalized world, precipitating the psychosocial conditions that create the demand for opioids, for example, job loss and status declines (Doucouliagos and Laroche 2009). The rigidity of union contracts can create distress among workers (Brochu and Morin, 2012) and frustrate newer or higher-performing employees by undervaluing their education and experience. Additionally, the higher disposable income that often accompanies stronger unionization may lead to increased spending on nonessential items, such as alcohol, cigarettes, and potentially opioids (Burgard and Kalousova, 2015). If unionization enhances individuals' ability to afford opioids without sufficiently addressing the stressors that come with job insecurity, it could inadvertently heighten the risk of opioid misuse.

On the other hand, there is evidence that collectively bargained union contracts can lead to higher wages as well as provide non-monetary workplace benefits which are the determinants of job quality. These benefits may include improved work-time arrangements (e.g., working hours, scheduling, overtime pay, and sick days) as well as increased decision-making power (Hagedorn et al. 2016), enhanced job benefits like pension and health insurance (Buchmueller et al. 2002), and safer working conditions (Weil 1991). The comprehensive benefit package negotiated by unions can help alleviate the impact of psychosocial stressors (Eisenberg-Guyot et al. 2020). In addition to advocating for extra benefits, unions inform workers about existing policies and help ensure their access to those benefits, such as parental leave, special paid leave, and job-sharing options (Budd and Mumford 2004). Unions also provide a collective voice for members to express their frustrations to management (Flavin et al. 2010), offer legal protection for workers, combat discriminatory treatment (Budd and McCall 1997), and act as a buffer against job anxiety

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that may arise from organizational changes or workplace innovations (Bryson et al. 2013; Blanchflower and Bryson 2020). Western and Rosenfeld (2011) argue that unions help institutionalize norms of equity, reduce the dispersion of non-union wages in highly unionized regions and industries, and explain 20-33% of the growth in inequality, an effect comparable to the growing stratification of wages by education.

Given the evidence that unions positively impact the entire labor market, it is plausible that the union-induced higher incomes will translate into improved healthcare access at a greater scale than the actual level of union membership would imply. Olson (2019) reports that the threat of unionization as measured by private-sector union density has a direct positive impact on the probability that non-union males working full-time have health insurance through their employer and that all of the decline in employer-provided health insurance coverage for this group over the 1983 to 2007 period is explained by the decline in private-sector union density. Taken together with the findings of Gruber (2000) that health insurance is a key factor for labor market participants, via changes in working conditions and workplace safety, the impact of union busting on health outcomes becomes even more significant. According to Zoorob (2018), RTW leads to about a 14% increase in occupational mortality through decreased unionization over the period of 1992-2016. This result echoes those of Weil (1991) and Li et al. (2017), where the authors claim that union workplaces receive more health and safety inspections from the federal agency Occupational Safety and Health Administration.

In addition to addressing the physical, psychological, and social conditions of work, a strong union presence, union membership aside, may improve health and welfare by advancing economy-wide labor rights, compensation norms, social protections, and public health programs (Eisenberg-Guyot et al. 2020; Stansbury and Summers 2020). For example, Feigenbaum et al. (2018) find that by weakening a union's ability to turn out voters and contribute to candidates, RTW can significantly reduce the likelihood of working-class candidates holding elected office as well as the extent to which state legislative policy shifts to the ideological right, both on labor rights and norms like prevailing wage and minimum wage laws.

Importantly, unionization creates spillover benefits for both non-union workers and non-workers across various domains. These benefits range from widely discussed issues such as wages, income inequality, and workplace safety to more recent areas of focus like health and subjective

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well-being. Research on the effects of unions on non-union workers is not only limited, often due to methodological constraints imposed by data limitations. The current research highlights the health benefits of unionization for both union and nonunion members, suggesting that unions can play a significant role in reducing drug misuse.

Finally, while Makridis (2019) finds that RTW increases worker well-being, other studies have found that increased union density leads to greater life satisfaction (Flavin et al. 2010; Flavin and Shufeldt 2016; Artz et al 2022; Chen and Islam 2023). Across different methodologies and data coverage, this strand of literature reports a beneficial effect of unionization on aggregate life satisfaction levels when considering union members and non-members.

Appendix B Synthetic Control Method

Following the notation in Cavallo et al. (2013) and letting $(\alpha_{T_0+1}, \dots, \alpha_T)$ denote the set of post-RTW year-by-year synthetic control estimates for switcher-state g,[21] this amounts to calculating:

$$\bar{\alpha} = (\overline{\alpha_{T_0+1}}, \dots, \overline{\alpha_T}) = \frac{1}{G}\sum_{g=1}^{G}(\widehat{\alpha_{g,T_0+1}}, \dots \widehat{\alpha_{g,T}})$$

where T_0 is the number of pre-RTW periods out of the T total periods, and G is the total number of switcher states. If we index units $\{1, \dots, J+1\}$ such that the first unit is a switcher state and the others are "donors," the $\hat{\alpha}'s$ are computed by subtracting a linear combination of never-RTW outcome (Y_{jt}) from RTW outcome (Y_{1t}):

$$\widehat{\alpha_{1t}} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* \cdot Y_{jt} \text{ for } t \in T_0 + 1, \dots, T$$

where w_j^* -- which must sum to one and fall in the [0,1] interval – is the optimal weight for never-RTW state j chosen to minimize the pre-intervention RMSPE through a regression-based method.

For statistical inference, we first compute the treatment-specific placebo effect $\hat{\alpha}_g^{PL}$ using the available controls $\{J_g = 2, \dots, J_g + 1\}$ corresponding to treatment g for a given post-RTW period.

[21] A more complete description of this approach can be found in Cavallo et al. (2013) and Galiani and Quistorff (2017).

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We then average $\hat{\alpha}_g^{PL}$ over the treatments to construct $\bar{\alpha}^{PL}$, the average placebo effect with which the average treatment estimate $\bar{\alpha}$ is compared for inference. Since $\bar{\alpha}^{PL}$ is constructed from all possible averages where a single placebo is taken from each $\hat{\alpha}_g^{PL}$, there are $N_{\overline{PL}} = \prod_{g=1}^{G} J_g$ such possible averages. Correspondingly, the two-sided p-values (for positive effects) can be calculated as

$$p\text{-}value = \Pr(|\,\bar{\alpha}^{PL}| \geq |\bar{\alpha}|) = \frac{\sum_{i=1}^{N_{\overline{PL}}} I(|\bar{\alpha}^{PL(i)}| \geq |\bar{\alpha}|)}{N_{\overline{PL}}}$$

which indicates the fraction of all possible placebo averages with a magnitude greater than or equal to that of the average effect estimated for the switcher states. Here i represent a selection where a single placebo effect is chosen from each treatment placebo set and $\bar{\alpha}^{PL(i)}$ is the average of the placebo selection.

Appendix C Robustness

We assess how sensitive our baseline estimates are to different implementations of the SCM models. This includes exploring various donor pools (Section C.1), treatment groups (Section C.2), predictive covariates (Section C.3), definitions of opioid misuse (Section C.4), and placebo outcomes (Section C.5). For the sake of brevity, we present only the findings from the six-year panels; however, the results from the four-year panels are qualitatively similar and are available upon request.

C.1 Alternative Donor Pools

Since our SCM procedure delivers positive weights for just a few potential donors (Section 3.1), one concern is that the estimates are driven by the performance of a small number of states. Figures A1a-A1b present the findings from a leave-one-out test where, following the recommendation in Abadie (2021), we iteratively apply the original SCM models while omitting one of the potential donors in each iteration.[22] If the exclusion of any state from the donor pool

[22] Since synthetic control weights are estimated separately to minimize the pre-treatment imbalances for each switcher state, the weight assigned to a never-RTW state may vary across switcher states. For simplicity, we treat all never-RTW states as donors in this exercise.

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has a large effect on results without a discernible change in pre-RTW fit, then it might be indicative of confounding effects of other interventions or large idiosyncratic shocks on the outcomes of the excluded donor. The results of this exercise, however, do not provide any indication that this is the case. While the leave-one-out synthetics largely match the path of the original synthetic unit, all of them identify a rise in opioid misuse within the 6 years of RTW enactment.

In addition, our baseline specifications include the states that implemented the triplicate prescription programs. Existed in three never-RTW states, California (1992-2004), Illinois (1992-2000), and New York (1992-2001), the triplicate programs are known for their long-term effects on opioid overdose deaths (Alpert et al. 2022). If the historical impacts of such policies differ significantly from states that did not implement similar measures, this omission could lead to an overestimation of the RTW effects. Similarly, our estimates may be misleading when the geographic areas disproportionately impacted by the epidemic, such as the Appalachian region (Shiels et al., 2020), are included in the donor pool (i.e., Maryland, New York, Ohio, and Pennsylvania). Likewise, neighboring states may not be suitable controls due to potential interstate spillovers in resident preferences, state policies, and local labor market conditions,[23] and the same applies to donor states that implemented public sector RTW laws in 2018.

Considering the aforementioned factors, Figures A1c-A1d present results for donor pools that exclude: 1) states with triplicate policies, 2) states in the Appalachian region, and 3) neighboring states of RTW adopters, and 4) observations in 2018. Contingent upon excellent pre-RTW match quality (i.e., 99.1-100%; see columns 1-5 of Table A6), our analysis yields estimated RTW effects that closely align with those outlined in Tables 4-5. More pronounced RTW effect sizes are observed when comparing switchers to non-triplicate and non-bordering donors, while the pattern for non-Appalachian states is mixed. Furthermore, excluding observations from 2018 leads to a decrease in the estimated magnitude. However, no significant impact on our results is identified as a consequence of such alterations.

[23] For instance, suppose that increased competition from RTW states forces wages down in non-RTW states and that lowered prevailing wage drives up opioid misuse in the latter, including border states in the donor pool could bias our estimates upward.

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C.2 Alternative Treated Group

The reformulation of OxyContin in 2010 could accelerate the use of substitute drugs, including heroin, depending on the pre-reformulation OxyContin misuse rates (Alpert et al. 2018). Considering that two out of four switcher states had a high pre-reformulation OxyContin misuse rate during the observation period (i.e., Indiana and Wisconsin),[24] the observed RTW effects can be a natural result of the introduction of abuse-deterrent opioids.

To investigate the potential impact of OxyContin reformulation, Figures A1e-A1f analyze outcomes in Michigan, the only switcher state that yielded a significant RTW effect and had low initial exposure to OxyContin, as per Alpert et al. (2018). In line with the findings in columns 6-7 of Table A6, our analysis indicates that RTW results in an increase in opioid misuse when employing the baseline specifications and including all available control states as potential donors. This result holds even after excluding the 9 never-RTW states that had a high or very high pre-reformulation OxyContin misuse rate (i.e., below 0.64%) from the pool. If anything, the estimated RTW effect becomes even more pronounced in the latter scenario. The apparent limited impact of OxyContin reformulation on our findings is plausible under two circumstances: 1) the misuse associated with substitute drugs and prescription pain relievers largely counterbalance each other, leading to a null effect of reformulation on overall opioid-related harms (Alpert et al. 2018), and/or 2) the observed RTW effects are influenced by factors primarily unrelated to OxyContin reformulation. Regardless, based on the evidence for Michigan, it seems unlikely that OxyContin reformulation can explain our findings. Section 4.4 revisits this issue by analyzing the misuse patterns of prescription opioids.

[24] We rely on the exposure measure computed in Alpert et al. (2018) using the 2004-2008 National Survey on Drug Use and Health (NSDUH) to guide relevant analysis, given that state-level prevalence estimates for nonmedical use of OxyContin are unavailable in the public-access files of the NSDUH. The Figure A1 in Alpert et al. (2018) classifies states into four categories in terms of their pre-reformulation OxyContin misuse rate from very low (0-0.49%), low (0.50-0.64%), high (0.65-0.79%), to very high (0.8-1.15%). Judging from this information, all switcher states except for Michigan had a high rate of pre-reformulation OxyContin misuse. While the observation period of Oklahoma ended before 2010, reformulation could impact our results obtained for the remaining switchers.

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C.3 Changing Predictive Covariates

Besides triplicate policies and abuse-deterrent drug formulations, the implementation of Prescription Drug Monitoring Programs (PDMPs) in 15 never-RTW states and two switcher states, Indiana (1998) and Wisconsin (2013), during the observation period can significantly impact opioid-related harm and usage patterns.[25] While evidence on the effectiveness of PDMPs is mixed, prior studies suggest that policies mandating prescribers to use PDMP at the point of care have the potential to reduce opioid misuse (e.g., Buchmueller and Carey 2017; Wen et al. 2019). The enactment of medical marijuana laws (MMLs), including Michigan's MML adoption in 2008, may have a similar effect.[26] Related research indicates that MMLs are associated with reduced opioid-related harms, with the critical aspect of MMLs facilitating a reduction in overdose deaths being a relatively liberal allowance for dispensaries (Powell et al. 2018). Given the potential overlap in the population that may benefit from medical marijuana use and the presence of unions, the presence of MML-related policies could mediate the observed results.

In light of these findings, Figure A2 adds the following policy variables to the set of synthetic control predictors: 1) triplicate policy, 2) MML, 3) legal protection of dispensaries, 4) PDMP, and 5) must-access provision of a PDMP. Including these variables in the synthetic control creation process allows us to construct a synthetic group that did not adopt RTW but has similar pre-treatment policies, thereby minimizing bias. As shown, the estimated trajectories for the synthetic control groups follow a similar trend as the actual data. Both outcomes reveal a significant RTW effect that widens over time.

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[25] These states include Colorado (2007), Connecticut (2008), Delaware (2012), Maine (2004), Maryland (2010), Massachusetts (1994), Minnesota (2010), Montana (2012), New Hampshire (2014), New Jersey (2011), New Mexico (2005), Ohio (2006), Oregon (2011), Vermont (2009), and Washington (2011). Data are sourced from PDMP Training and Technical Assistance Center: https://www.pdmpassist.org/Policies/Maps/PDMPPolicies, assessed on July 9, 2024.

[26] While some never-RTW states introduced MMLs and/or legal protection of dispensaries prior to 1992, including California (1996), Maine (1999), and Oregon (1998)), 8 others adopted such policies from 1992-2018 (i.e., Connecticut (2012), Illinois (2013 and 2014), Maryland (2014), Massachusetts (2012 and 2013), New Hampshire (2013), New York (2014), Rhode Island (2006), Vermont (2004). Data are drawn from Powell et al. (2018).

C.4 Alternative Definitions of Opioid Misuse

The current categorization of opioid misuse combines heroin, natural opioids, synthetic opioids, and unspecified narcotics. The latter category includes drug overdose deaths where opioid is reported without more specific information to assign a more specific ICD–10 code (CDC 2023). Figure A3 reproduces our results using two alternative measures of opioid misuse, following existing literature (Alpert et al. 2022): 1) prescription opioids and 2) all opioids excluding "other/unspecified narcotics" (T40.6) from the list of contributing causes, to address potential measurement errors. For prescription opioids, we examine treatment episodes related to "other opioids and synthetics" for nonfatal overdoses and consider deaths from "natural and semi-synthetic opioids" (T40.2) and "methadone" (T40.3) for fatal overdoses. Since 2) is a concern specific to CDC MCOD data, we perform the exercise only for overdose mortality. Despite a significant shrinkage in the size of the donor pool for overdose deaths (from 19 to 14-15, depending on the outcome) due to missing data, we achieve a 100% pre-RTW match quality across outcomes (columns 1-3 of Table A7). During the post-RTW period, we observe a notable increase in opioid misuse across all three cases. While the average effect of RTW on treatment admissions due to prescription opioids lacks precision, the time-series patterns of individual estimates remain consistent.

It is noteworthy that the prescription opioid definition does not include deaths attributable to heroin and synthetic opioids like fentanyl. Therefore, observing a similar treatment effect using this alternative measure indirectly suggests that the reformulation of OxyContin or the emergence of illicit drugs is not a primary driver of our results.

C.5 Placebo Analyses

Next, we test whether a significant RTW effect emerges for outcomes less likely affected by RTW by focusing on deaths attributable to breast cancer and influenza/pneumonia.[27] Given the interconnected nature of the underlying causes of various conditions and the potential for opioid

[27] Breast cancer deaths are calculated by the CDC: https://wonder.cdc.gov/cancer.html, retrieved July 15 2024. Influenza and pneumonia deaths are identified from the ICD–10 codes J09–J18, following the official guidelines for coding and reporting using the ICD-10 (CDC 2002).

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misuse to contribute to a wide range of diseases that can also lead to fatal outcomes, we consider these two outcomes as less susceptible to changes in RTW legislation, as they disproportionately affect old adults (CDC 2008 and 2024), whose misuse behavior is less influenced by de-unionization (Section 5). As such, a diminished or non-existent effect observed for these outcomes would indicate that omitted factors, such as differences in healthcare infrastructure, did not drive our results. As illustrated in Figure A4, RTW switchers and their corresponding synthetic control groups follow comparable trajectories before the enactment of RTW laws, exhibiting a 100% pre-RTW match quality (columns 4-5 of Table A7). During the post-intervention period, however, no significant RTW effect is apparent.

C.6 Overfitting

While a close pre-treatment fit is crucial, it does not guarantee good performance of synthetic control estimators due to the risk of overfitting, particularly if this fitting occurs within a short pre-intervention period. To assess the magnitude of potential biases, we conduct a backdating exercise, as suggested by Abadie and Vives-i-Bastida (2022). In this exercise, we reassign the RTW implementation to roughly the middle of the pre-treatment period – 3-5 and 5-7 years earlier than the RTW law was actually passed for treatment admission and overdose death, respectively – and lag our synthetic control predictors accordingly. Our analysis reveals no evidence of diverging trends between the actual outcomes and their synthetic estimates (see Figure A5). In other words, we do not observe a treatment effect until the actual implementation of the law, even when the treatment is artificially backdated in the data. This finding suggests that the synthetic control estimators exhibit strong out-of-sample predictive power and quality.

Appendix D Characteristics of Overdose Victims

Columns 2-3 of Panel A in Table A8 stratifies the population based on age and indicates that the previously observed effect of RTW effects on opioid-related treatment admissions, as replicated in column 1 of Panel A, is predominantly concentrated among individuals in the working age bracket (i.e., 18-54 years). While missing out on older people of 55-64 years due to data

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limitations,[28] this finding suggests that aspects of work life quality, such as wages, benefits, occupational hazards, flexible scheduling, job security, work-life balance, and employer-employee relations, may mediate the impact of RTW laws on overdose behavior. This hypothesis is further corroborated by the disproportionately large RTW impact estimated for working-age males relative to their female counterparts (columns 4-5 of Panel A), which is plausible if working-age males are more likely to participate in physically demanding jobs, work in hazardous environments, or experience stress related to low job security and control (Arnold and Bongiovi 2013). Overall, the analysis reveals that working-age men account for nearly two-thirds of the RTW-induced treatment admissions involving opioids observed previously. Similarly, when examining fatal opioid overdoses, the same pattern emerges (Panel B), though we are unable to replicate the analyses for non-working-age individuals and working-age women, as only a few never-RTW states had consistently high death counts (i.e., greater than 20 per 100,000 people) in these categories throughout the observation period. However, by gauging the magnitude of the estimated ATEs for working-age individuals relative to that of the full sample, the former is clearly the driver of the observed results.

Finally, a separate investigation into working-age males of different educational attainment (columns 6-7 of Panel A) finds a notable increase in treatment episodes only among individuals without a college degree. This increase amounts to 9 cases per 100,000 (or 19% relative to the baseline). While the unavailability of comparable measures in the public-access CDC files hinders an examination of mortality outcomes, these findings align with previous studies that suggest unionization disproportionally benefits economically disadvantaged groups (Flavin et al. 2010; Chen and Islam 2023). This phenomenon is plausible if 1) the marginal utility of time and money are greater for those who face resource constraints and/or are located toward the lower end of the income distribution; 2) the less educated are more likely to hold more physically demanding jobs and face greater exposure to hazardous working conditions; 3) "exit" from a job is more difficult for this segment of the population due to fewer outside options (Korpi and

[28] To reduce disclosure risk, age at admission is classified into 11-12 categories in the TEDS data over our observation period. The lowest category combined the ages of 12-14. The highest category top-coded all ages of 55 and older prior to 2015 and then 65 and older for remaining years. Thus, the age range of 18 through 54 is the closest criterion through which we can identify working-age individuals.

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Shalev 1979), which enhances the role of collective voice (Freeman and Medoff 1984); or 4) job security is a more important decision factor for unskilled labor, facing a more elastic demand as globalization and automation become more prevalent.

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Table A1 Right-to-Work Enactment and Potential Donors

Never RTW States	Switcher States	
	Prior 1992/1999	Post 1992/1999
Alaska	Arkansas (11/7/1944)	Oklahoma (9/28/2001)
California	Florida (11/71944)	Indiana (2/1/2012)
Colorado	Arizona (11/5/1946)	Michigan (3/8/2013)
Connecticut	Nebraska (12/11/1946)	Wisconsin (3/9/2015)
Delaware	Virginia (1/12/1947)	West Virginia (2/12/2016)
District of Columbia	Tennessee (2/21/1947)	Kentucky (1/7/2017)
Hawaii	North Carolina (3/18/1947)	
Illinois	Georgia (3/27/1947)	
Maine	Iowa (4/28/1947)	
Maryland	South Dakota (7/1/1947)	
Massachusetts	Texas (9/5/1947)	
Minnesota	North Dakota (6/28/1948)	
Missouri	Nevada (12/4/1952)	
Montana	Alabama (8/28/1953)	
New Hampshire	Mississippi (2/24/1954)	
New Jersey	South Carolina (3/19/1954)	
New Mexico	Utah (5/10/1955)	
New York	Kansas (11/4/1958)	
Ohio	Wyoming (2/8/1963)	
Oregon	Louisiana (10/6/1976)	
Pennsylvania	Idaho (1/31/1985)	
Rhode Island		
Vermont		
Washington		

Notes: 1) This table shows the enactment date of RTW laws in each state. Data are collected from the National Right to Work Committee website: https://nrtwc.org/facts/state-right-to-work-timeline-2016/, retrieved March 20, 2019. 2) Due to data limitations (see Table A2), 18 states are identified as potential donors for TEDS outcomes, including California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Maine, Maryland, Massachusetts, Minnesota, New Hampshire, New Jersey, New Mexico, New York, Ohio, Rhode Island, and Vermont. In addition, 19 potential donors are used for the MCOD outcomes, with the inclusion of Oregon, Pennsylvania, and Washington, while excluding Montana and Vermont.

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Table A2 Missing Data in TEDS and CDC MCOD

State	TEDS (1992-2018)	CDC MCOD (1999-2018)
Alaska	2004-2006	2001- 2005, 2007
District of Columbia	2004-2006, 2008-2009, 2010	2009
Indiana	1997	
Montana		1999-2000
Oregon	2015-2018	
Pennsylvania	2012-2013	
Vermont		1999
Washington	2017	

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Table A3 Union Membership and Coverage Rates (22-Year Pre-Period)

	Union Membership		Union Coverage	
	4-Year Window (1)	6-Year Window (2)	4-Year Window (3)	6-Year Window (4)
Panel A: Overall Estimates				
Average Treatment Effect	-2.569***	-1.563**	-2.335***	-1.496**
	(0.000)	(0.043)	(0.000)	(0.017)
Panel B: Individual Estimates				
1 Year After	-2.405***	-1.578***	-2.057***	-1.457
	(0.000)	(0.000)	(0.000)	(0.110)
2 Years After	-3.083***	-2.144**	-2.849***	-2.120***
	(0.000)	(0.032)	(0.000)	(0.002)
3 Years After	-2.157***	-0.762	-1.914***	-0.653
	(0.000)	(0.336)	(0.000)	(0.423)
4 Years After	-2.631***	-1.489*	-2.520***	-1.263*
	(0.000)	(0.081)	(0.000)	(0.064)
5 Years After		-0.943		-1.269
		(0.227)		(0.153)
6 Years After		-2.463**		-2.215***
		(0.039)		(0.001)
Pre-RTW Match Quality	0.957	0.943	0.985	0.986
Pre-RTW Rate	13.15	13.91	13.97	14.74
# of Potential Donors	23	23	23	23

Notes: Standardized placebo-based p-values are reported in parentheses.

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Table A4 A Sample of Synthetic Control Weights

	Oklahoma	Indian	Michigan	Wisconsin
Alaska	0	0	0	0
California	0.001	0.01	0	0.039
Colorado	0.57	0.24	0	0
Connecticut	0	0	0	0
Delaware	0	0	0.035	0
District of Columbia	0	0.232	0.244	0.304
Hawaii	0	0.026	0.132	0.086
Illinois	0	0.159	0	0.077
Maine	0	0	0	0
Maryland	0	0	0	0.356
Massachusetts	0	0	0	0
Minnesota	0	0	0	0
Montana	0	0	0	0
New Hampshire	0	0	0	0
New Jersey	0	0	0.241	0.139
New Mexico	0.429	0.166	0	0
New York	0	0.047	0.349	0
Ohio	0	0.12	0	0
Oregon	0	0	0	0
Pennsylvania	0	0	0	0
Rhode Island	0	0	0	0
Vermont	0	0	0	0
Washington	0	0	0	0

Notes: This table reports the weights assigned to potential donors in the synthetic control analysis of union membership in column 1 of Table 2.

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Table A5 RTW Effects by Pre-Existing Unionization Rate

	Low Union Density	High Union Density	
	6-Year Window	4-Year Window	6-Year Window
	(1)	(2)	(3)
Panel A: Treatment Admissions (TEDS)			
RTW	-11.027	27.19***	57.546***
	(0.400)	(0.006)	(0.000)
Pre-RTW Match Quality	0.900	1.000	0.934
Pre-RTW Rate	10.74	132.78	193.82
Switcher States	OK	MI and WI	MI
Time Frame	1992-2006	2004-2018	2004-2018
# of Potential Donors	21	18	18
Panel B: Overdose Deaths (CDC MCOD)			
RTW	4.75***	1.508**	6.777*
	(0.000)	(0.050)	(0.053)
Pre-RTW Match Quality	1.000	1.000	0.895
Pre-RTW Rate	5.50	8.80	6.90
Switcher States	IN	MI and WI	MI
Time Frame	1999-2017	2004-2018	2004-2018
# of Potential Donors	19	19	19

Notes: Standardized placebo-based p-values are reported in parentheses.

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Table A6 Alternative Donor Pools

	Original Model	Triplicate States	Appalachian States	Bordering States	2018 Data	Michigan (All States)	Michigan (Similar Pre-Reformulation Misuse Rates)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Treatment Admissions (Switcher States: OK and MI)							
RTW	16.380***	17.858**	14.217***	16.283***	11.123***	60.605***	54.912***
	(0.006)	(0.040)	(0.008)	(0.006)	(0.006)	(0.000)	(0.000)
Pre-RTW Match Quality	1.000	0.991	1.000	1.000	1.000	1.000	1.000
Excluded States	None	CA, IL, NY	MD, NY, OH	CO and NM for OK and OH for MI	None	None	CT, DE, ME, MA, MT, NH, NM, RI, VT
# of Potential Donors	18	18	15	16-17	18	18	9
Panel B: Mortality (Switcher States: IN and MI)							
RTW	4.074***	5.697***	5.638***	5.639***	4.429***	5.483***	6.385***
	(0.008)	(0.000)	(0.009)	(0.006)	(0.000)	(0.000)	(0.000)
Pre-RTW Match Quality	1.000	1.000	1.000	1.000	1.000	0.895	1.000
Excluded States	None	CA, IL, NY	MD, NY, OH, PA	IL and OH for IN and OH for MI	None	None	CT, DE, ME, MA, NH, NM, OR, RI, VT, WI
# of Potential Donors	19	19	16	17-18	19	19	10

Notes: Standardized placebo-based p-values are reported in parentheses.

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Table A7 Alternative Measures of Opioid Misuse and Placebo Outcomes

	Treatment Admission Prescription Opioids	Mortality Prescription Opioids	All Opioids (CDC)	Breast Cancer	Influenzas/Pneumonia
	(1)	(2)	(3)	(4)	(5)
Panel A: Overall Estimates					
Average Treatment Effect	16.911	4.832**	3.428**	-0.043	0.262
	(0.252)	(0.040)	(0.018)	(0.648)	(0.320)
Panel B: Individual Estimates					
1 Year After	-0.244	0.168	0.803	-0.069	0.771
	(0.987)	(0.607)	(0.422)	(0.820)	(0.129)
2 Years After	12.253**	-0.089	0.830	-0.207	0.274
	(0.019)	(0.974)	(0.813)	(0.159)	(0.240)
3 Years After	17.849***	0.478	2.323	0.016	0.499
	(0.006)	(0.362)	(0.253)	(0.970)	(0.389)
4 Years After	23.006*	0.965	3.454	0.093	-0.477
	(0.056)	(0.107)	(0.280)	(0.629)	(0.870)
5 Years After	26.323*	1.111	5.426*	-0.155	0.552
	(0.068)	(0.112)	(0.075)	(0.384)	(0.346)
6 Years After	22.280	2.199**	7.7129***	0.067	-0.048
	(0.151)	(0.010)	(0.000)	(0.684)	(0.601)
Pre-RTW Match Quality	1.000	1.000	1.000	1.000	1.000
Pre-RTW Rate	42.19	3.45	5.40	7.31	17.85
# of Potential Donors States	18	14	15	23	23

Notes: Standardized placebo-based p-values are reported in parentheses. The pre-RTW rate measures the incidence of each condition per 100,000 people in the switcher states one year prior to RTW passage.

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Table A8 RTW Effects by Individual Characteristics

	All Individuals			Working-Age Individuals		Working-Age Men	
	Average (All Ages)	Working Age (18 to 54)	Non-Working Age (Under 18/Over 54)	Men	Women	Without College Degree	With College Degree
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Treatment Admissions							
ATE	16.380***	15.067***	1.859	10.120***	5.698	8.958**	0.532
	(0.006)	(0.006)	(0.791)	(0.006)	(0.239)	(0.018)	(0.414)
Pre-RTW Match Quality	1.000	1.000	0.944	0.994	0.978	1.000	0.988
Pre-RTW Case Rate	102.28	93.16	9.34	49.55	44.23	48.29	1.253
# of Potential Donors	18	18	18	18	18	18	18
States							
Panel B: Overdose Deaths							
ATE	4.074***	6.770**	--	4.039	--	--	--
	(0.008)	(0.035)	--	(0.676)	--	--	--
Pre-RTW Match Quality	1.000	0.997	--	0.978	--	--	--
Pre-RTW Case Rate	6.20	10.65	--	13.95	--	--	--
# of Potential Donors	19	19	--	18	--	--	--
States							

Notes: Standardized placebo-based p-values are reported in parentheses.

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Table A9 Workplace Safety

	Fatal and Non-Fatal Injuries		Fatal Injuries	
	(1) 4-Year Window	(2) 6-Year Window	(3) 4-Year Window	(4) 6-Year Window
Panel A: Overall Estimates				
Average Treatment Effect	0.194	0.264	0.415*	0.577***
	(0.848)	(0.775)	(0.098)	(0.004)
Panel B: Individual Estimates				
1 Year After	0.473	0.181	0.341	0.224
	(0.172)	(0.615)	(0.154)	(0.234)
2 Years After	-0.038	0.389	0.143	0.337*
	(0.947)	(0.331)	(0.471)	(0.057)
3 Years After	-0.012	0.138	0.663**	0.839***
	(0.986)	(0.686)	(0.038)	(0.006)
4 Years After	0.352	0.784	0.513**	0.509*
	(0.309)	(0.130)	(0.028)	(0.079)
5 Years After		-0.017		0.834**
		(0.982)		(0.038)
6 Years After		0.111		0.719*
		(0.858)		(0.060)
Pre-RTW Match Quality	1.000	0.970	0.938	0.960
Pre-RTW Level	7.339	7.467	3.112	3.801
Number of Potential Donors States	13	13	23	23

Notes: Standardized placebo-based p-values are reported in parentheses.

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Table A10 Personal Health (All Individuals)

	Health (1 to 5 Scale)		Fair/Poor Health	
	(1) 4-Year Window	(2) 6-Year Window	(3) 4-Year Window	(4) 6-Year Window
Panel A: Overall Estimates				
Average Treatment Effect	0.049*	0.048	0.010***	0.009**
	(0.096)	(0.244)	(0.008)	(0.016)
Panel B: Individual Estimates				
1 Year After	0.049*	0.050	0.004	0.008*
	(0.054)	(0.148)	(0.222)	(0.079)
2 Years After	0.078***	0.062*	0.014***	0.015***
	(0.003)	(0.076)	(0.005)	(0.000)
3 Years After	0.046*	0.077**	0.014***	0.015***
	(0.083)	(0.038)	(0.003)	(0.000)
4 Years After	0.023	0.038	0.007**	0.009**
	(0.381)	(0.241)	(0.032)	(0.033)
5 Years After		0.037		-0.0007
		(0.342)		(0.771)
6 Years After		0.025		0.006
		(0.404)		(0.184)
Pre-RTW Match Quality	0.893	0.823	0.948	0.917
Pre-RTW Level	2.213	2.232	0.114	0.119
Number of Potential Donors States	23	24	23	24

Notes: Standardized placebo-based p-values are reported in parentheses

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Table A11 Personal Health (Individuals Aged 18-54)

	Self-Report Health (1 to 5 Scale)		Fair/Poor Health	
	(1) 4-Year Window	(2) 6-Year Window	(3) 4-Year Window	(4) 6-Year Window
Panel A: Overall Estimates				
Average Treatment Effect	0.047**	0.052**	0.008**	0.009***
	(0.013)	(0.047)	(0.022)	(0.009)
Panel B: Individual Estimates				
1 Year After	0.055**	0.075***	0.010**	0.016***
	(0.037)	(0.000)	(0.028)	(0.003)
2 Years After	0.105***	0.090***	0.015***	0.016**
	(0.000)	(0.009)	(0.006)	(0.012)
3 Years After	0.012	0.048	0.008*	0.007
	(0.749)	(0.236)	(0.094)	(0.170)
4 Years After	0.017	0.006	-0.001	-0.002
	(0.527)	(0.852)	(0.664)	(0.677)
5 Years After		0.027		0.003
		(0.415)		(0.642)
6 Years After		0.068**		0.012***
		(0.040)		(0.002)
Pre-RTW Match Quality	0.981	0.983	0.993	0.960
Pre-RTW Level	2.166	2.192	0.093	0.099
Number of Potential Donors States	23	24	23	24

Notes: Standardized placebo-based p-values are reported in parentheses

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Table A12 Income and Wages

	Household Income		Individual Wages	
	(1) 4-Year Window	(2) 6-Year Window	(3) 4-Year Window	(4) 6-Year Window
Panel A: Overall Estimates				
Average Treatment Effect	-2143.507 (0.194)	-2044.033 (0.501)	-1778.66** (0.022)	-1118.814** (0.048)
Panel B: Individual Estimates				
1 Year After	-1625.735 (0.220)	-1159.275 (0.583)	-142.823 (0.626)	276.141 (0.784)
2 Years After	-3226.399** (0.031)	-2677.021 (0.174)	-2089.876** (0.018)	-1649.484* (0.094)
3 Years After	-1709.347 (0.384)	-3040.754 (0.199)	-2379.15** (0.016)	-1815.799* (0.082)
4 Years After	-2012.545 (0.237)	-1189.534 (0.601)	-2502.8*** (0.007)	-1537.471 (0.250)
5 Years After		261.051 (0.919)		646.599 (0.330)
6 Years After		-4458.666 (0.154)		-2632.868** (0.025)
Pre-RTW Match Quality	0.997	0.967	0.994	0.967
Pre-RTW Level	64950.2	61470.45	34921.93	33502.85
Number of Potential Donors States	23	23	23	23

Notes: Standardized placebo-based p-values are reported in parentheses. Wage estimates are obtained for the worker sample.

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Table A13 Employment and Long Working Hours

	Employment Rate		Long Working Hours (45+)	
	(1) 4-Year Window	(2) 6-Year Window	(3) 4-Year Window	(4) 6-Year Window
Panel A: Overall Estimates				
Average Treatment Effect	0.007***	0.005***	0.017**	0.009**
	(0.002)	(0.000)	(0.010)	(0.015)
Panel B: Individual Estimates				
1 Year After	-0.004	-0.008	0.016***	0.012***
	(0.437)	(0.212)	(0.004)	(0.002)
2 Years After	0.002	0.005	0.023***	0.023***
	(0.869)	(0.600)	(0.000)	(0.000)
3 Years After	0.011**	0.010*	0.021**	0.004
	(0.044)	(0.083)	(0.034)	(0.584)
4 Years After	0.017***	0.009*	0.007	0.006
	(0.008)	(0.095)	(0.329)	(0.359)
5 Years After		0.004		0.002
		(0.374)		(0.737)
6 Years After		0.008		0.008
		(0.216)		(0.299)
Pre-RTW Match Quality	0.997	0.996	0.916	0.971
Pre-RTW Level	0.568	0.552	0.162	0.159
Number of Potential Donors States	23	23	23	23

Notes: Standardized placebo-based p-values are reported in parentheses. Working hour estimates are obtained for the worker sample.

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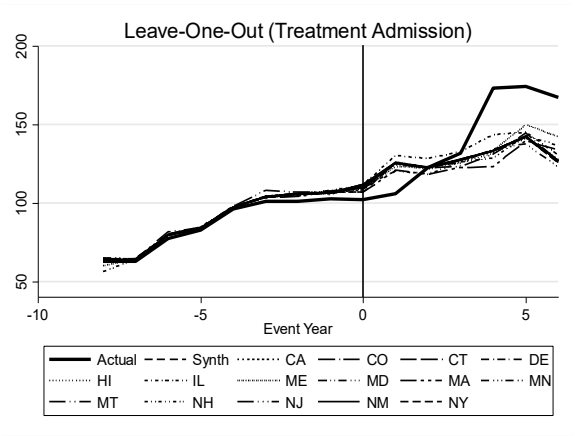
Figure A1 Alternative Donor Pools



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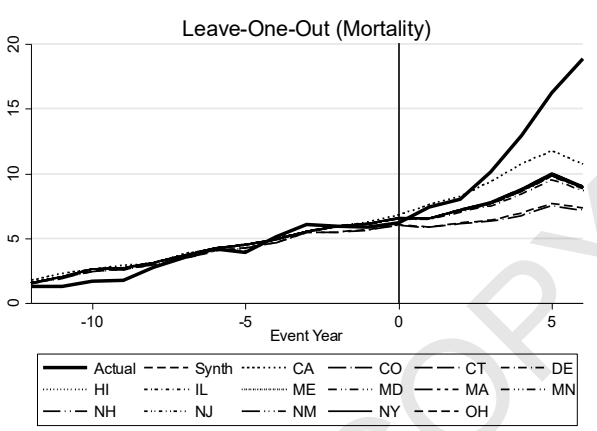

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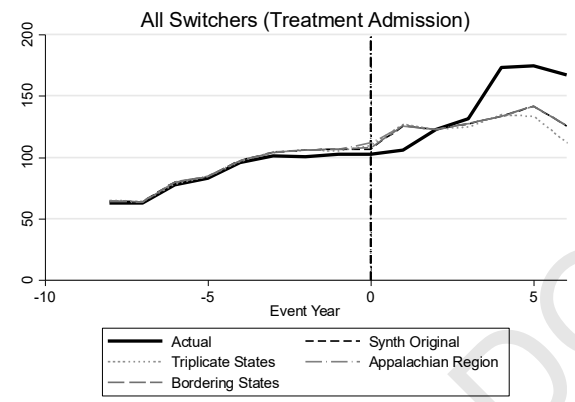

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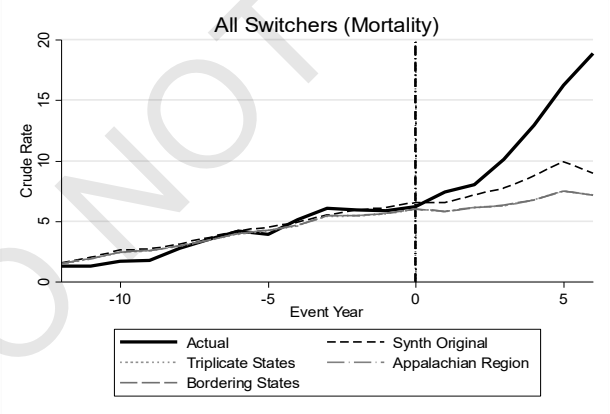

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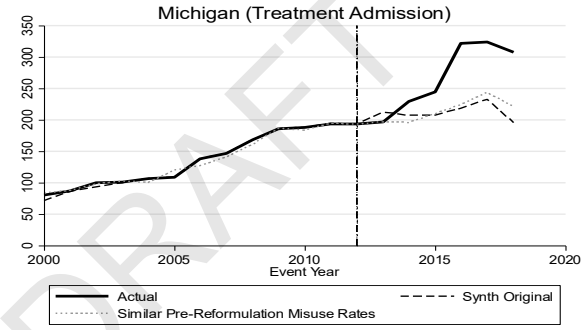

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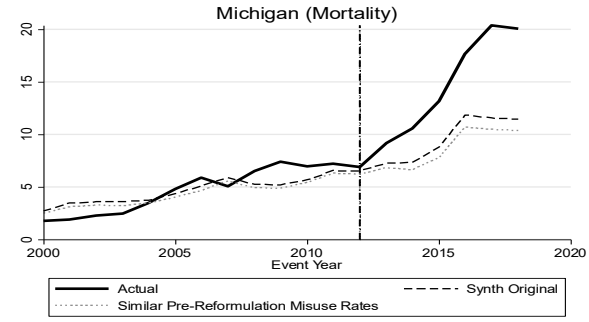

(f)

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Figure A2 Alternative Covariates and Donor Pools

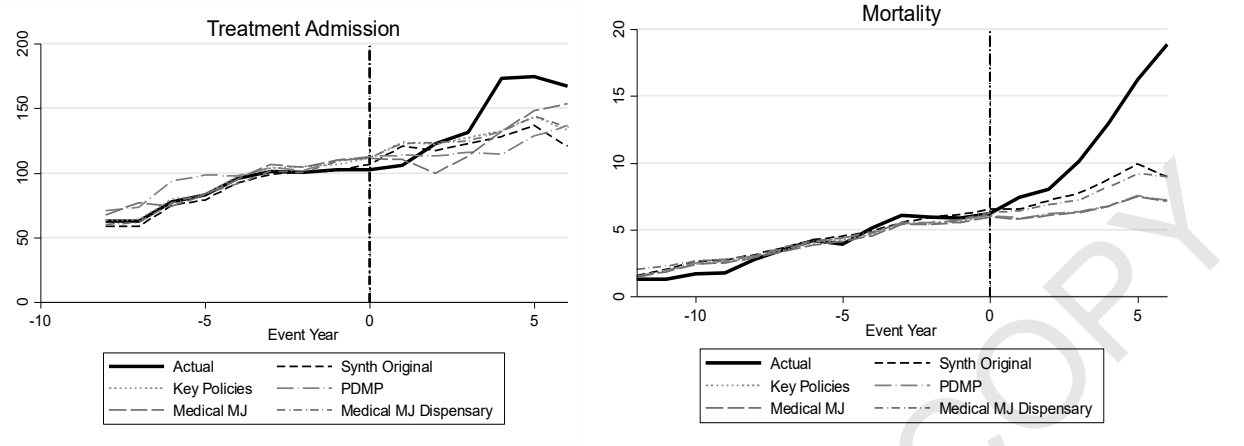
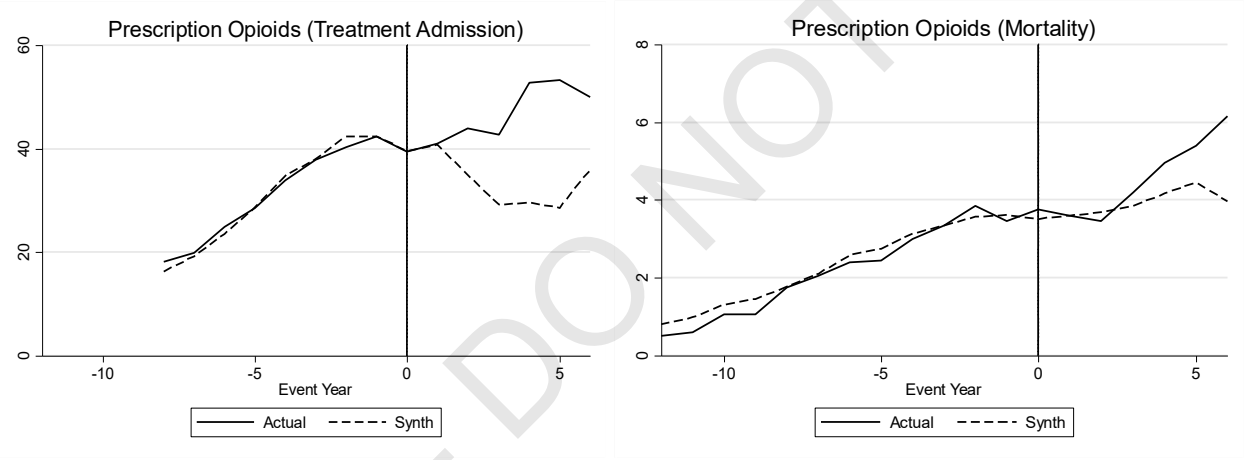
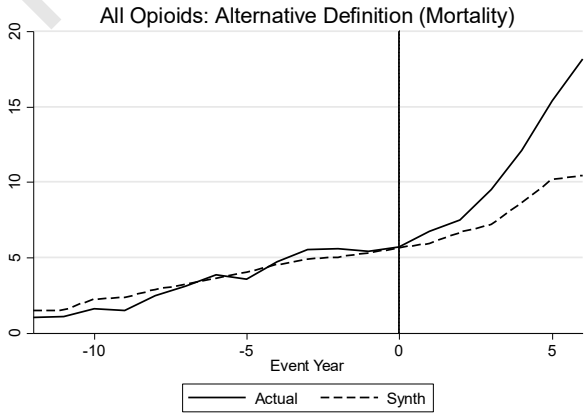


Figure A3 Alternative Measures of Opioid Misuse



(a) (b)



(c)

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Figure A4 Placebo Mortality

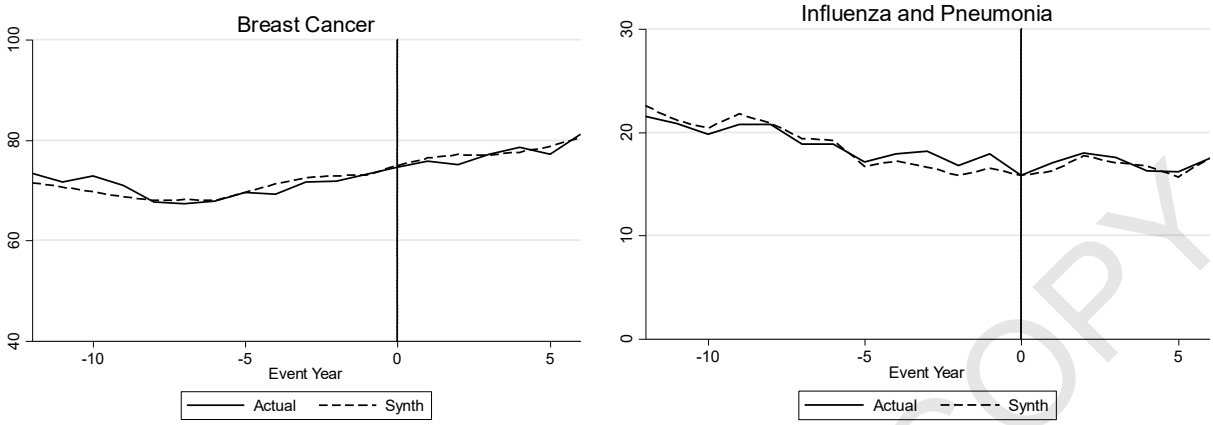

Figure A5 Out-of-Sample Validation

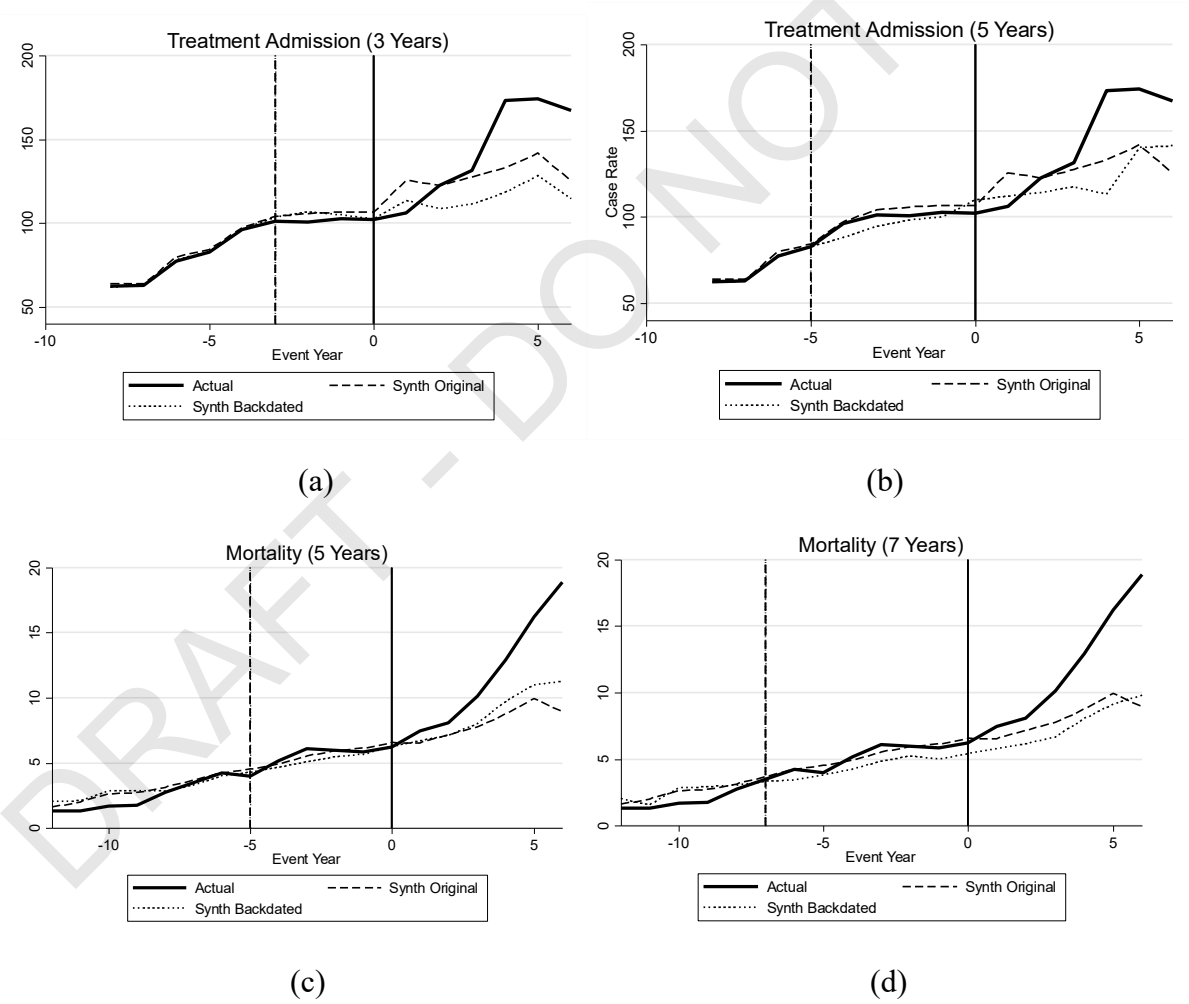

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Figure A6 Workplace Safety and Self-Rated Health (Working-Age Population)

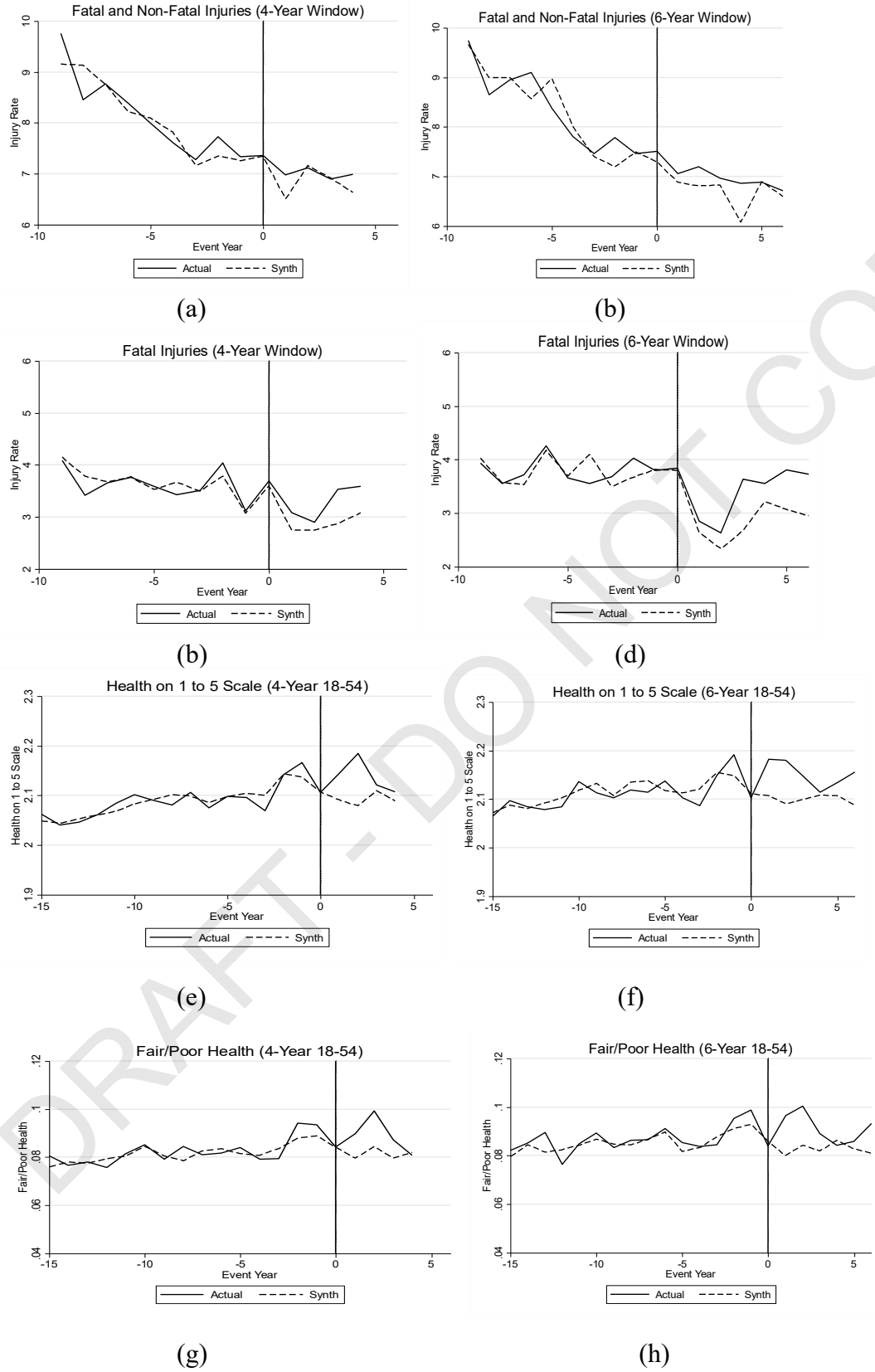

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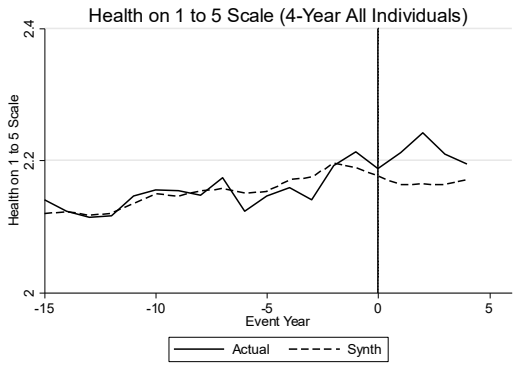
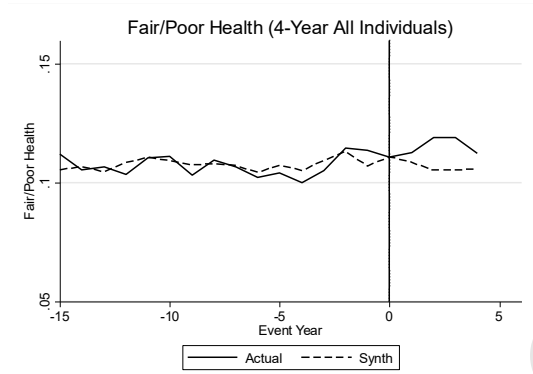
Figure A7 Self-Rated Health (All Individuals)

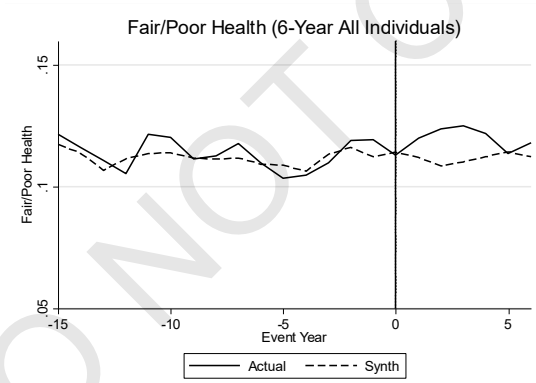


(a)

(b)



(c)



(d)

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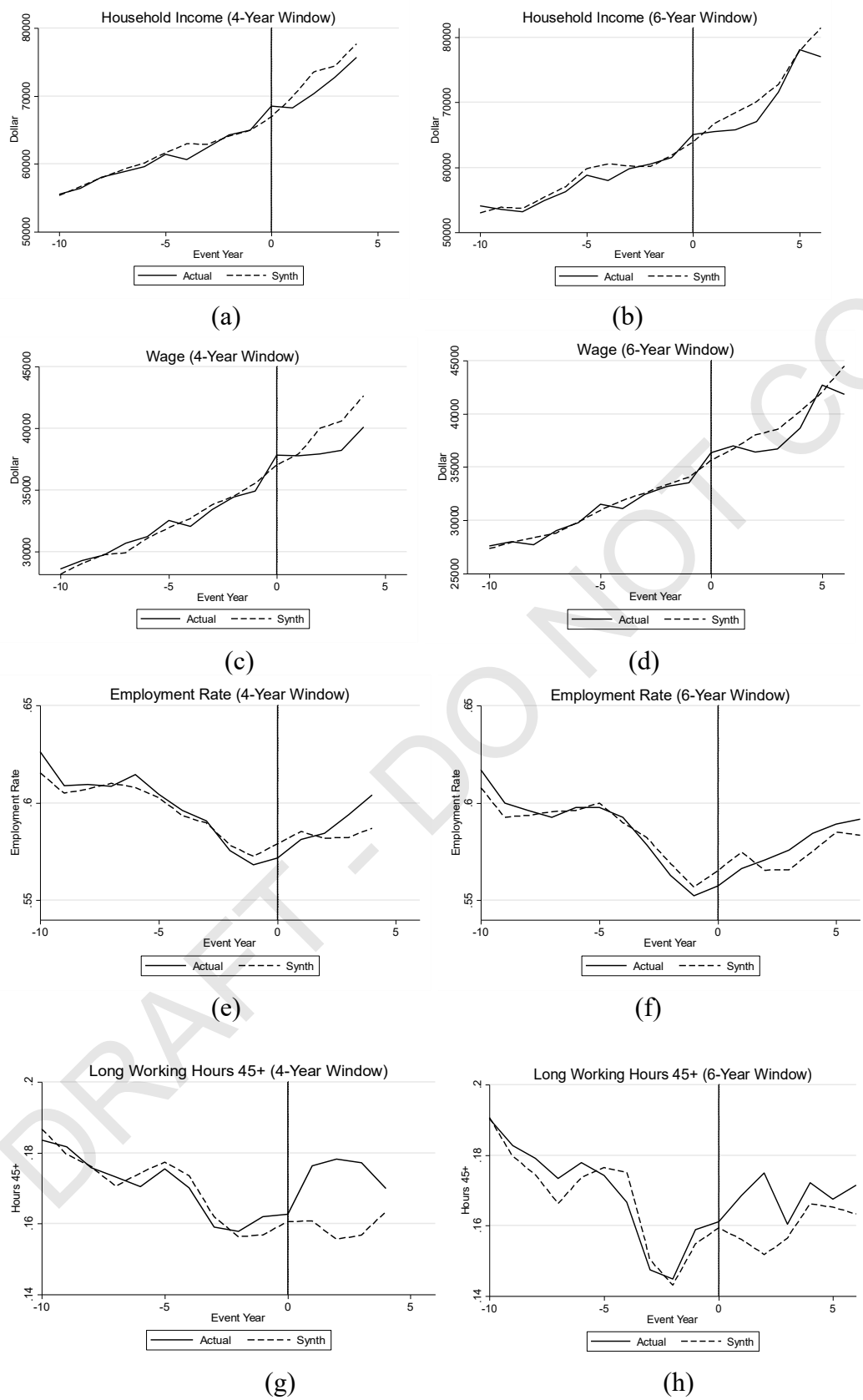
Figure A8 Income, Wages, Unemployment, and Long Working Hours



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