Online appendixes

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

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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.[1] 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 opioid use disorder (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.[2]

[1] 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.

[2] 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.

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 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 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 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 SCM estimates from alternative specification

In this section, we report SCM results from an alternative specification that uses the full set of pre-treatment outcomes as predictors, an approach with appealing asymptotic properties under certain conditions (Ferman et al. 2020) and a harmonized implementation across outcomes.

We implement this alternative specification for our main outcomes—opioid-related admissions and mortality. Results are presented in Figures A1 and A2 and Tables A6 and A7. Overall levels and year-by-year patterns closely track those obtained from our preferred, more parsimonious specifications that rely on a smaller set of pre-treatment outcomes and predictive covariates.

Figures A1 and A2 display pre-treatment fits and post-RTW patterns that are remarkably similar to those in Figures 2 and 3 from the preferred specification. For opioid-related admissions (Table A5), the alternative SCM yields an average effect of 9.92 (vs. 9.35 for the preferred specification) at four years post-RTW and 16.60 (vs. 16.38) at six years; both are significant at the 1% level. The annual estimates again show impacts that grow over time. For mortality (Table A6), the alternative SCM produces an average effect of 0.78 (vs. 0.82) at four years and 4.39 (vs. 3.71) at six years, neither statistically significant. As before, we observe a delayed response: the yearly estimates become significant only in year 6, with an effect of 10.25 (vs. 8.34) that is significant at the 10% level.

Appendix C Additional SCM robustness checks

C.1 Reformulation of OxyContin

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),[3] the observed RTW effects can be a result of the introduction of abuse-deterrent opioids.

To investigate the potential impacts of OxyContin reformulation, we focus on Michigan as our switcher state, since Michigan had low initial exposure to OxyContin, as per Alpert et al. (2018). The results are reported in Table A10 and depicted in Figure A6. We show 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 impacts 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

[3] 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.

primarily unrelated to OxyContin reformulation. Regardless, based on the evidence for Michigan, it seems unlikely that OxyContin reformulation can explain our findings.

C.2 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).

Table A11 and Figure A7 reproduce 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 admission 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.

Even though there is 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 observe a notable increase in opioid misuse across all three cases. While the average effect of RTW on admissions related 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.

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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.

Table A2 Missing data in TEDS and MCOD

State	*TEDS (1992-2018)*	*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	

Table A3 Union membership and coverage rates (22-year pre-period) (SCM)

	Union membership		*Union coverage*	
	4-year follow-up (1)	6-year follow-up (2)	4-year follow-up (3)	6-year follow-up (4)
Panel A: Overall estimates				
Average treatment effect	-2.569***	-1.563**	-2.488***	-1.496**
	(0.000)	(0.043)	(0.000)	(0.017)
Panel B: Individual estimates				
1 year after	-2.405***	-1.578***	-2.205***	-1.457
	(0.000)	(0.000)	(0.000)	(0.110)
2 years after	-3.083***	-2.144**	-3.071***	-2.120***
	(0.000)	(0.032)	(0.000)	(0.002)
3 years after	-2.157***	-0.762	-1.994***	-0.653
	(0.000)	(0.336)	(0.000)	(0.423)
4 years after	-2.631***	-1.489*	-2.681***	-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	0.969	0.986
Pre-RTW unionization rate	13.15	13.91	13.97	14.74
# of potential donors	23	23	23	23

Notes: This table reports SCM estimates with standardized placebo-based p-values in parentheses rather than traditional standard errors to accurately reflect the permutation-based inference used in the analysis. The estimates use CPS 1990–2018, pooling Indiana, Michigan, and Wisconsin for the 4-year follow-up, and Indiana and Michigan for the 6-year follow-up. Pre-RTW match quality is measured by the proportion of placebo states with a pre-intervention RMSPE at least as large as the average RMSPE for the treated units. The pre-RTW unionization rate is the number of individuals belonging to a union or covered by a union contract per 100 workers in the switcher states, measured one year before RTW passage for each respective sample. The predictive covariates used in the models are listed in Table A5. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table A4 A sample of synthetic control weights

	Oklahoma	Indiana	Michigan	Wisconsin
Alaska	0	0	0	0
California	0	0	0	0
Colorado	0.708	0.177	0	0
Connecticut	0	0	0	0.274
Delaware	0	0	0	0.307
District of Columbia	0	0	0	0
Hawaii	0	0	0.232	0
Illinois	0	0	0	0.012
Maine	0	0.23	0	0
Maryland	0	0	0	0
Massachusetts	0	0	0	0
Minnesota	0	0	0	0
Montana	0	0	0	0
New Hampshire	0	0	0	0
New Jersey	0	0.044	0.274	0
New Mexico	0.122	0.175	0	0
New York	0	0	0.119	0
Ohio	0	0.374	0.375	0.406
Oregon	0	0	0	0
Pennsylvania	0	0	0	0
Rhode Island	0	0	0	0
Vermont	0.17	0	0	0
Washington	0	0	0	0

Notes: This table presents the weights assigned to potential donors in the SCM estimates of union membership in column 1 of Table 2.

Table A5 Predictors used in the primary SCM specifications

Union coverage (4-year follow-up)	Pretreatment outcomes from years -3 and -6; total number of workers from years -5 and -6; the share of workers in service occupations from years -2 and -9. The share of workers in service occupations was constructed based on the 2010 Standard Occupational Classification System codes available in the CPS.
Union coverage (6-year follow-up)	Pretreatment outcomes from years -1 and -6; total number of workers from years -1 and -7; the share of workers in service occupations from year -9; the share of workers in professional occupations from year -2. The shares of workers in service and professional occupations were constructed based on the 2010 Standard Occupational Classification System codes available in the CPS.
Union membership (4-year follow-up)	Pretreatment outcomes from years -1, -2, and -6; total number of workers from years -2 and -6; union coverage from year -2; the share of workers in production occupations from year -1; the share of workers in professional occupations from year -8. The shares of workers in production and professional occupations were constructed based on the 2010 Standard Occupational Classification System codes available in the CPS.
Union membership (6-year follow-up)	Pretreatment outcomes from years -1 and -6; total number of workers from years -1 and -7; union coverage from year -6; the share of workers in service occupations from years -4. The share of workers in service occupations was constructed based on the 2010 Standard Occupational Classification System codes available in the CPS.
Admissions: Opioids (4-year follow-up)	Pretreatment outcomes from years -1, -2, -4, -5, -6, -8, and -9
Admissions: Opioids (6-year follow-up)	Pretreatment outcomes from years -1, -2, -3, -5, -7, and -8
Admissions: Non-opioids (4-year follow-up)	Pretreatment outcomes from years -1, -3, -4, -6, and -9; the total number of opioid-related admissions from years -3, -4, -7, and -9
Admissions: Non-opioids (6-year follow-up)	Pretreatment outcomes from years -1, -3, -5, -6, -8, and -9; total number of opioid-related admissions from years -1, -2, and -3
Mortality: Opioids (4-year follow-up)	Pretreatment outcomes from years -1, -7, -8, -10, and -12
Mortality: Opioids (6-year follow-up)	Pretreatment outcomes from years -9 and -10
Mortality: Non-opioids (4-year follow-up)	Pretreatment outcomes from years -1, -2, -4, and -5
Mortality: Non-opioids (6-year follow-up)	Pretreatment outcomes from years -1 to -11

Notes: This table presents predictors used in the primary SCM specifications.

Table A6 Opioid-related admissions, alternative specification (SCM)

	4-year follow-up	6-year follow-up
Panel A: Overall estimates		
Average treatment effect	9.923***	16.602***
	(0.000)	(0.000)
Panel B: Yearly estimates		
1 year after	-17.743***	-18.745***
	(0.000)	(0.000)
2 years after	2.608***	0.719***
	(0.002)	(0.000)
3 years after	12.961***	4.943***
	(0.000)	(0.000)
4 years after	41.868***	40.605***
	(0.001)	(0.000)
5 years after		32.622***
		(0.000)
6 years after		39.468***
		(0.000)
Pre-RTW match quality	1.000	0.988
Pre-RTW admission rate	87.36	102.28
# of potential donors	18	18

Notes: This table reports SCM estimates using all pre-treatment outcomes as predictors; standardized placebo-based p-values are in parenthesis rather than traditional standard errors to accurately reflect the permutation-based inference used in the analysis. The estimates use TEDS 1992-2018, pooling Oklahoma, Michigan, and Wisconsin for the 4-year follow-up sample and the former two for the 6-year follow-up sample. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. The pre-RTW admission rate measures the average number of admissions per 100,000 people in the switcher states for each data sample one year before the passage of RTW laws. * p<0.10; ** p<0.05; *** p<0.01.

Table A7 Opioid-related mortality, alternative specification (SCM)

	4-year follow-up	6-year follow-up
Panel A: Overall estimates		
Average treatment effect	0.778	4.387
	(0.258)	(0.319)
Panel B: Yearly estimates		
1 year after	-0.197	1.082
	(0.370)	(0.357)
2 years after	0.069	1.120
	(0.786)	(0.620)
3 years after	0.760	2.664
	(0.993)	(0.402)
4 years after	2.480	4.508
	(0.568)	(0.360)
5 years after		6.705
		(0.255)
6 years after		10.248*
		(0.091)
Pre-RTW match quality	0.975	0.868
Pre-RTW admission rate	7.77	6.20
# of potential donors	19	19

Notes: This table reports SCM estimates using the full set of pre-treatment outcomes as predictors; standardized placebo-based p-values are in parenthesis rather than traditional standard errors to accurately reflect the permutation-based inference used in the analysis. The estimates use MCOD 1999-2018, pooling Indiana, Michigan, and Wisconsin for the 4-year follow-up sample and the former two for the 6-year follow-up sample. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. The pre-RTW mortality rate measures the average number of deaths per 100,000 people in the switcher states for each data sample one year before the passage of RTW laws. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table A8 Concurrent programs and other confounders

	Triplicate (1)	PDMP (2)	MML (3)	Appalachian (4)	Bordering states (5)
Panel A: Opioid-related admissions					
RTW	17.858**	-24.262	15.633	14.217***	16.283***
	(0.040)	(0.389)	(0.349)	(0.008)	(0.006)
Pre-RTW match quality	0.991	0.889	0.990	1.000	1.000
Excluded states	CA, IL, NY	None	CA and ME for OK and CO, CT, DE, HI, MD, MN, NJ, NM, NY, OH, and VT for MI	MD, NY, OH	CO and NM for OK and OH for MI
# of potential donors	15	18	7-16	15	16-17
Switcher states	OK and MI	OK	OK and MI	OK and MI	OK and MI
Panel B: Opioid-related mortality					
RTW	3.218**	4.75***	6.033**	4.753***	3.825***
	(0.027)	(0.000)	(0.019)	(0.000)	(0.000)
Pre-RTW match quality	1.000	1.000	0.989	1.000	1.000
Excluded states	CA, IL, NY	CA, DE, HI, IL, NH, NY, PA, RI	CA, ME, and OR for IN and CO, CT, DE, HI, MD, NJ, NM, NY, and OH for MI	MD, NY, OH, PA	IL and OH for IN and OH for MI
# of potential donors	16	11	10-16	15	17-18
Switcher states	IN and MI	IN	IN and MI	IN and MI	IN and MI

Notes: Column 1 excludes triplicate states from the donor pool. Column 2 limits the donor pool to never-RTW states that had the same PDMP status as the switcher states before their RTW adoption. For example, Indiana (2012) adopted PDMP in 1998 before its RTW adoption; therefore, we include the 11 never-RTW states that also implemented PDMP prior to 2012 as potential donors. Michigan, being a non-PDMP state, is excluded from this analysis because the vast majority of never-RTW states (14 states) had implemented PDMP prior to its RTW adoption. Following the same rationale, Column 3 restricts the donor pool to never-RTW states that had the same MML status as the switcher states before their RTW adoption. Columns 4-5 exclude Appalachian states, and bordering states from the donor pool, respectively. Standardized placebo-based p-values are indicated in parentheses. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table A9 Falsification tests

	Breast cancer	Influenzas/pneumonia
Panel A: Overall estimates		
Average treatment effect	-0.043	0.262
	(0.648)	(0.320)
Panel B: Yearly Estimates		
1 year after	-0.069	0.771
	(0.820)	(0.129)
2 years after	-0.207	0.274
	(0.159)	(0.240)
3 years after	0.016	0.499
	(0.970)	(0.389)
4 years after	0.093	-0.477
	(0.629)	(0.870)
5 years after	-0.155	0.552
	(0.384)	(0.346)
6 years after	0.067	-0.048
	(0.684)	(0.601)
Pre-RTW match quality	1.000	1.000
Pre-RTW admission rate	7.31	17.85
# of potential donors	23	23

Notes: This table presents SCM estimates with standardized placebo-based p-values in parentheses. Estimates are obtained from replicating the main SCM analysis for two outcomes less likely affected by RTW, i.e., deaths attributable to 1) breast cancer and 2) influenza/pneumonia. The pre-RTW case rate measures the incidence of each condition per 100,000 people in the switcher states one year prior to RTW passage. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. * p<0.10; ** p<0.05; *** p<0.01.

Table A10 OxyContin reformulation

	Michigan (All states) (1)	Michigan (Similar pre-reformulation misuse rates) (2)
Panel A: Opioid-related admissions (Switcher states: OK and MI)		
RTW	60.605***	54.912***
	(0.000)	(0.000)
Pre-RTW match quality	1.000	1.000
Excluded states	None	CT, DE, ME, MA, MT, NH, NM, RI, VT
# of potential donors	18	9
Panel B: Opioid-related mortality (Switcher states: IN and MI)		
RTW	7.548***	6.385***
	(0.000)	(0.000)
Pre-RTW match quality	0.895	1.000
Excluded states	None	CT, DE, ME, MA, NH, NM, OR, RI, VT, WI
# of potential donors	19	9

Notes: This table analyzes opioid-related admissions and mortality in Michigan using all never-RTW states and never-RTW states that had similar pre-reformulation misuse rates of OxyContin as potential donors, respectively. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. * p<0.10; ** p<0.05; *** p<0.01.

Table A11 Alternative measures of opioid misuse

	Admission	*Mortality*	
	Prescription opioids	Prescription opioids	All opioids (CDC)
	(1)	(2)	(3)
Panel A: Overall estimates			
Average treatment effect	16.911	4.832**	3.428**
	(0.252)	(0.040)	(0.018)
Panel B: Yearly estimates			
1 year after	-0.244	0.168	0.803
	(0.987)	(0.607)	(0.422)
2 years after	12.253**	-0.089	0.830
	(0.019)	(0.974)	(0.813)
3 years after	17.849***	0.478	2.323
	(0.006)	(0.362)	(0.253)
4 years after	23.006*	0.965	3.454
	(0.056)	(0.107)	(0.280)
5 years after	26.323*	1.111	5.426*
	(0.068)	(0.112)	(0.075)
6 years after	22.280	2.199**	7.7129***
	(0.151)	(0.010)	(0.000)
Pre-RTW match quality	1.000	1.000	1.000
Pre-RTW admission rate	42.19	3.45	5.40
# of potential donors	18	14	15

Notes: This table presents SCM estimates with standardized placebo-based p-values in parentheses rather than traditional standard errors to accurately reflect the permutation-based inference used in the analysis. Columns 1-3 reproduce the main results using two alternative measures of opioid misuse: 1) prescription opioids and 2) all opioids excluding "other/unspecified narcotics" (T40.6) from the list of contributing causes. The pre-RTW rate measures the rate of hospital admissions and mortality in the switcher states one year prior to RTW passage. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. * p<0.10; ** p<0.05; *** p<0.01.

Table A12 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: Opioid-related admissions			
Average treatment effect	-11.027	27.19***	57.546***
	(0.400)	(0.006)	(0.000)
Pre-RTW match quality	0.900	1.000	0.934
Pre-RTW admission 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	18	18	18
Panel B: Opioid-related mortality			
Average treatment effect	4.75***	1.508**	6.777*
	(0.000)	(0.050)	(0.053)
Pre-RTW match quality	1.000	1.000	0.895
Pre-RTW mortality 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: This table reports the average RTW effects by SCM for low and high union density states using data from the 1992-2018 TEDS and 1999-2018 MCOD. Standardized placebo-based p-values are reported in parenthesis. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. * p<0.10; ** p<0.05; *** p<0.01.

Table A13 RTW effects by individual characteristics

	All individuals			*Working-age individuals*		*Working-age men*	
	Average (All ages)	Working age (18 to 54)	Non-working age (18-/54+)	Men	Women	Without college degree	With college degree
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Opioid-related admissions							
Average treatment effect	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
Panel B: Opioid-related mortality							
Average treatment effect	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	--	--	--

Notes: This table presents SCM estimates by individual age, gender, and educational attainment for the same set of states used in column 2 of Tables 3 and 4. Standardized placebo-based p-values are reported in parentheses. Pre-RTW match quality is measured by the proportion of placebo states with a pre-intervention RMSPE at least as large as the average RMSPE for the treated units. The definitions of pre-RTW case rate are the same as those in Tables 3 and 4. * p<0.10; ** p<0.05; *** p<0.01.

Table A14 Workplace safety

	Fatal and non-fatal injuries		*Fatal injuries*	
	(1) 4-year follow-up	(2) 6-year follow-up	(3) 4-year follow-up	(4) 6-year follow-up
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: Yearly 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
# of potential donors	13	13	23	23

Notes: This table reports SCM estimates with standardized placebo-based p-values in parentheses rather than traditional standard errors to accurately reflect the permutation-based inference used in the analysis. The estimates for occupational injuries, illnesses, and fatalities are obtained using data from the 2002-2018 BLS SOII and CFOI, pooling Indiana, Michigan, and Wisconsin when applicable. Pre-RTW match quality is measured by the proportion of placebos with a preintervention RMSPE at least as large as the average of the treated units. The pre-RTW case rate measures the average number of injuries and/or illnesses per 100,000 workers in the switcher states for each data sample one year before the passage of RTW laws. * p<0.10; ** p<0.05; *** p<0.01.

Table A15 Personal health (all individuals)

	Health (1 to 5 Scale)		*Fair/Poor Health*	
	(1) 4-year follow-up	(2) 6-year follow-up	(3) 4-year follow-up	(4) 6-year follow-up
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: Yearly 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
# of potential donors	23	24	23	24

Notes: This table reports SCM estimates for all individuals, with standardized placebo-based p-values in parenthesis. The estimates for self-reported health are obtained using CPS 1996-2018, pooling Indiana, Michigan, and Wisconsin when applicable. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. The pre-RTW case rate measures the average health rating in the switcher states for each data sample one year before the passage of RTW laws. * p<0.10; ** p<0.05; *** p<0.01.

Table A16 Personal health (individuals aged 18-54)

	Self-Report Health (1 to 5 Scale)		*Fair/Poor Health*	
	(1)	(2)	(3)	(4)
	4-year follow-up	6-year follow-up	4-year follow-up	6-year follow-up
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: Yearly 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
# of potential donors	23	24	23	24

Notes: This table reports SCM estimates for working-age individuals, with standardized placebo-based p-values in parentheses rather than traditional standard errors to accurately reflect the permutation-based inference used in the analysis. The estimates for self-reported health use CPS 1996-2018, pooling Indiana, Michigan, and Wisconsin when applicable. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. The pre-RTW case rate measures the average health rating in the switcher states for each data sample one year before the passage of RTW laws. * p<0.10; ** p<0.05; *** p<0.01.

Table A17 Income and wages

	Household income		Individual wages	
	(1) 4-year follow-up	(2) 6-year follow-up	(3) 4-year follow-up	(4) 6-year follow-up
Panel A: Overall estimates				
Average treatment effect	-2143.507	-2044.033	-1778.66**	-1118.814**
	(0.194)	(0.501)	(0.022)	(0.048)
Panel B: Yearly estimates				
1 year after	-1625.735	-1159.275	-142.823	276.141
	(0.220)	(0.583)	(0.626)	(0.784)
2 years after	-3226.399**	-2677.021	-2089.876**	-1649.484*
	(0.031)	(0.174)	(0.018)	(0.094)
3 years after	-1709.347	-3040.754	-2379.15**	-1815.799*
	(0.384)	(0.199)	(0.016)	(0.082)
4 years after	-2012.545	-1189.534	-2502.8***	-1537.471
	(0.237)	(0.601)	(0.007)	(0.250)
5 years after		261.051		646.599
		(0.919)		(0.330)
6 years after		-4458.666		-2632.868**
		(0.154)		(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
# of potential donors	23	23	23	23

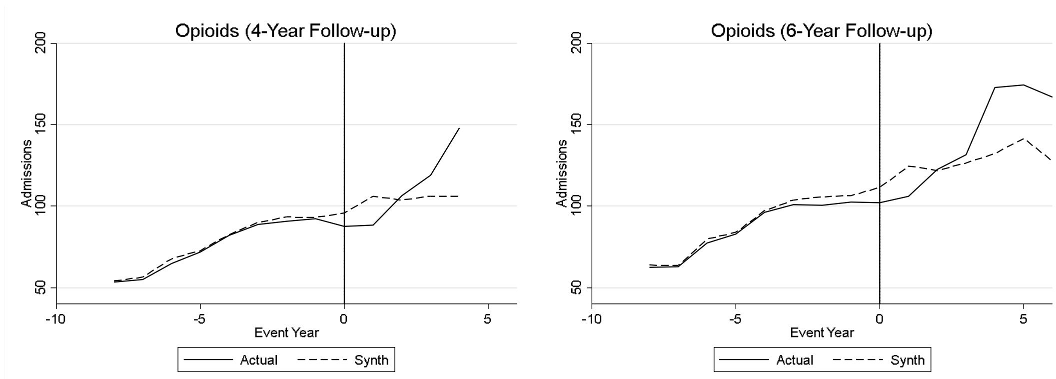
Notes: This table reports SCM estimates, with standardized placebo-based p-values in parentheses rather than traditional standard errors to accurately reflect the permutation-based inference used in the analysis. The estimates use data from the 1990-2019 CPS, pooling Oklahoma, Indiana, Michigan, and Wisconsin when applicable. All variables are lagged by one year since the CPS provides information for the previous year and are inflated/deflated to the amount they would have represented in 1999 using the CPI index provided by the CPS (https://cps.ipums.org/cps/cpi99.shtml). Wage estimates use the worker sample. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. The pre-RTW case rate measures the average household income or individual wages in the switcher states for each data sample one year before the passage of RTW laws. * p<0.10; ** p<0.05; *** p<0.01.

Table A18 Employment and long working hours

	Employment rate		Long working hours (45+)	
	(1) 4-year follow-up	(2) 6-year follow-up	(3) 4-year follow-up	(4) 6-year follow-up
Panel A: Overall estimates				
Average treatment effect	0.007***	0.005***	0.017**	0.010***
	(0.002)	(0.000)	(0.010)	(0.002)
Panel B: Yearly estimates				
1 year after	-0.004	-0.008	0.016***	0.013***
	(0.437)	(0.212)	(0.004)	(0.000)
2 years after	0.002	0.005	0.023***	0.023***
	(0.869)	(0.600)	(0.000)	(0.009)
3 years after	0.011**	0.010*	0.021**	0.005
	(0.044)	(0.083)	(0.034)	(0.488)
4 years after	0.017***	0.009*	0.007	0.007
	(0.008)	(0.095)	(0.329)	(0.314)
5 years after		0.004		0.003
		(0.374)		(0.836)
6 years after		0.008		0.009
		(0.216)		(0.560)
Pre-RTW match quality	0.997	0.996	0.916	0.970
Pre-RTW level	0.568	0.552	0.162	0.159
# of potential donors	23	23	23	23

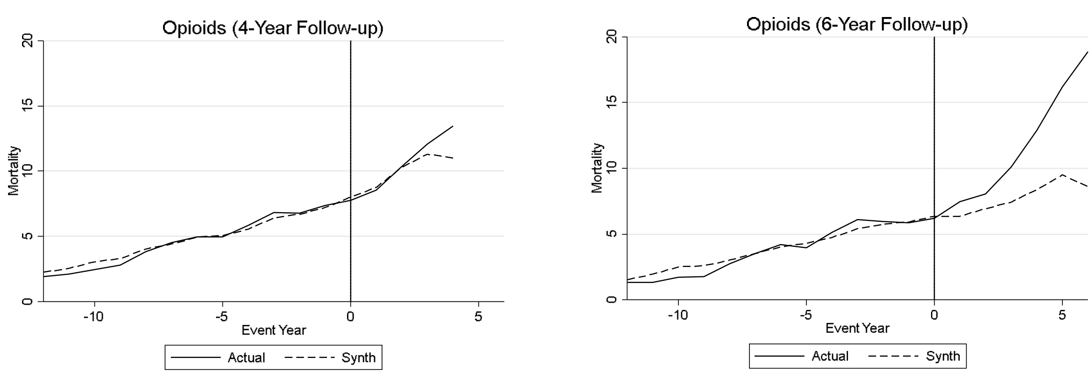
Notes: This table reports SCM estimates, with standardized placebo-based p-values in parentheses rather than traditional standard errors to accurately reflect the permutation-based inference used in the analysis. The estimates use data from the 1990-2019 CPS, pooling Oklahoma, Indiana, Michigan, and Wisconsin when applicable. Working hour estimates are obtained for the worker sample. Pre-RTW match quality is measured by the proportion of placebos with a pre-intervention RMSPE at least as large as the average of the treated units. The pre-RTW case rate measures the average employment rate or share of workers working more than 45 hours per week in the switcher states for each data sample one year before the passage of RTW laws. * p<0.10; ** p<0.05; *** p<0.01.

Figure A1 Opioid-related admissions, alternative specification (SCM)



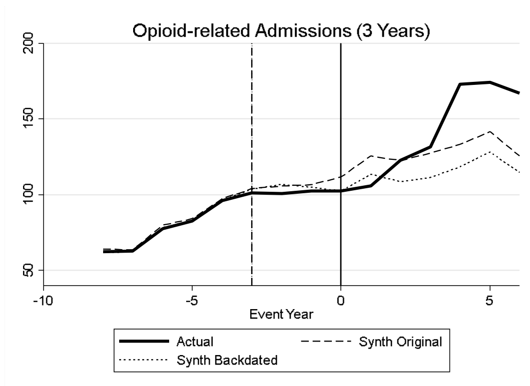
Notes: The left and right panels present the estimated RTW effects on admissions for the switcher states in our 4-year sample (Oklahoma, Michigan, and Wisconsin) and 6-year follow-up sample (Oklahoma and Michigan), respectively, using 1992–2018 TEDS data. The analysis uses the full set of pre-treatment outcomes as predictors. The vertical line marks the year prior to RTW adoption.

Figure A2 Opioid-related mortality, alternative specification (SCM)

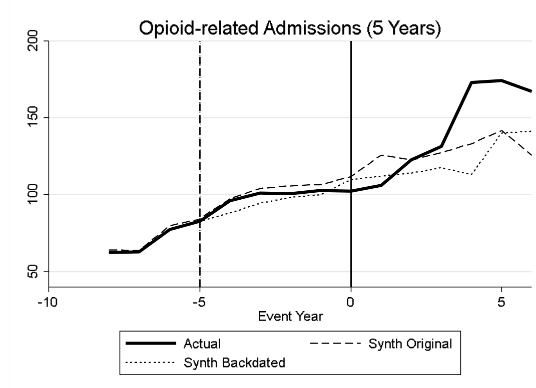


Notes: The left and right panels present the estimated RTW effects on mortality for the switcher states in our 4-year follow-up sample (Indiana, Michigan, and Wisconsin) and 6-year follow-up sample (Indiana and Michigan), respectively, using 1999–2018 MCOD data. The analysis uses the full set of pre-treatment outcomes as predictors. The vertical line marks the year prior to RTW adoption.

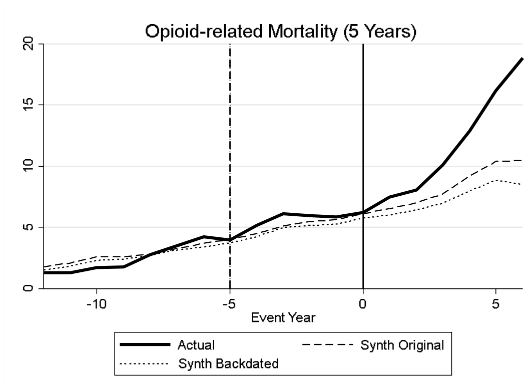
Figure A3 Overfitting and generalization



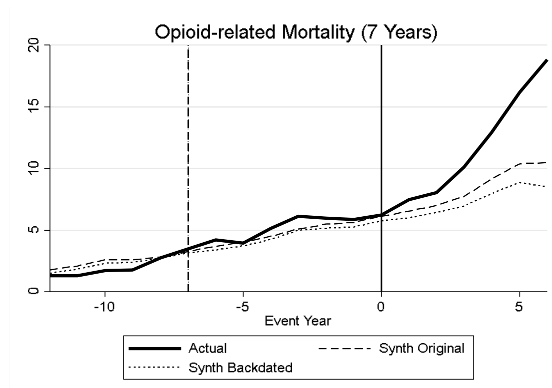
(a)



(b)



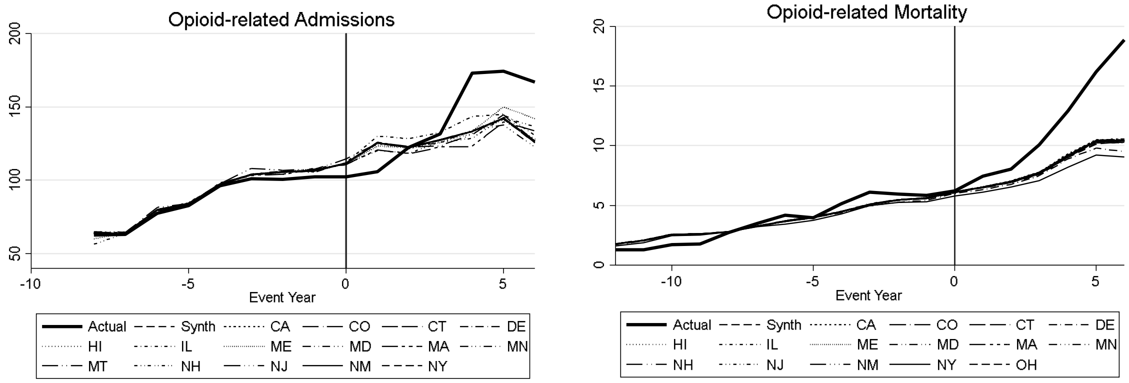
(c)



(d)

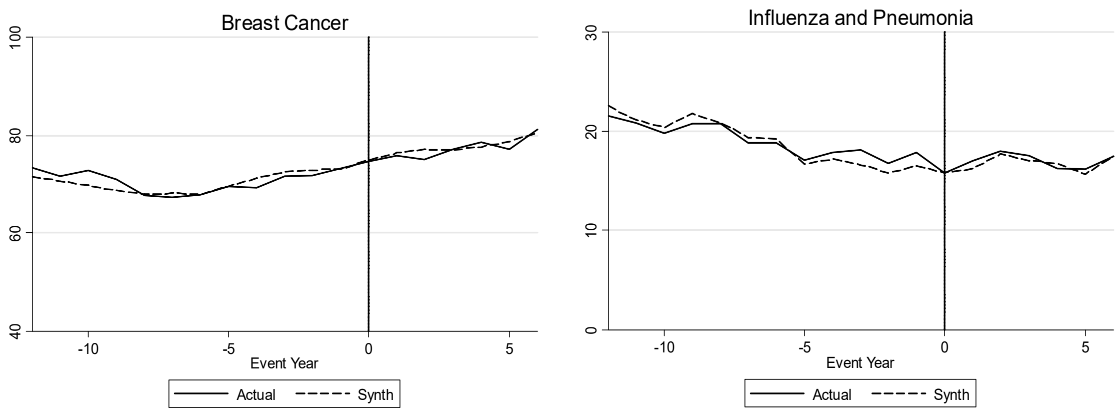
Notes: This figure shows the estimated RTW effects from a backdating exercise, where we reassign the RTW implementation to roughly the middle of the pre-treatment period, 3-5 and 5-7 years earlier than the RTW law was actually passed for opioid-related admissions and mortality, respectively, and lag our synthetic control predictors accordingly. The solid line marks the year before RTW adoption.

Figure A4 Donor-pool sensitivity



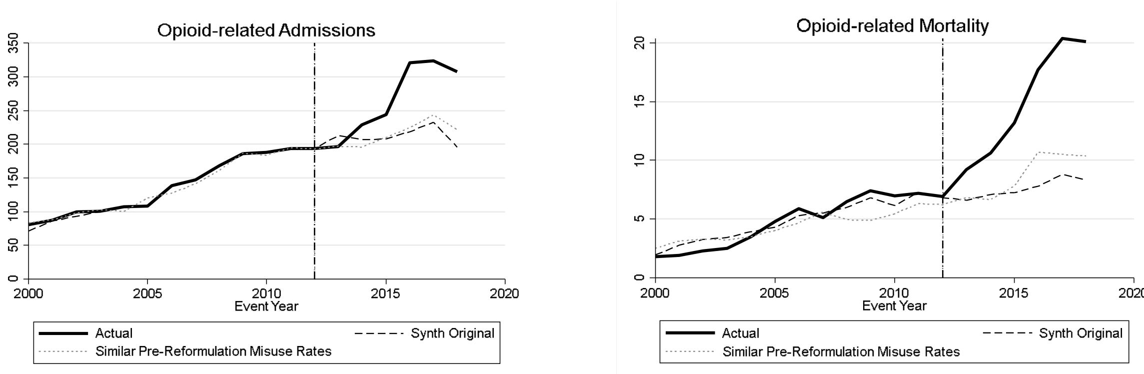
Notes: These figures present the estimated RTW effects on opioid-related admission and mortality from a leave-one-out test, where we iteratively apply the original SCM models while omitting one potential donor in each iteration.

Figure A5 Falsification tests



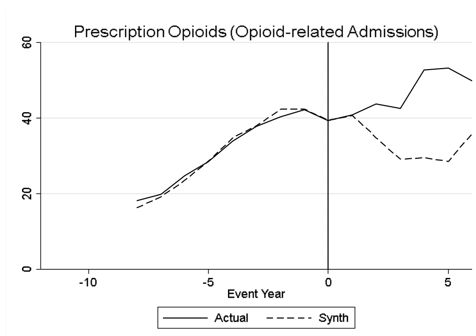
Notes: These figures show the estimated RTW effects on morality due to breast cancer and influenza/pneumonia using the same samples in column 2 of Table 4. The vertical line marks the year before RTW adoption.

Figure A6 OxyContin reformulation

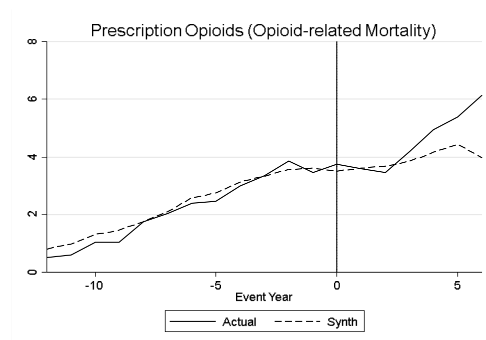


Notes: These figures show the results for Michigan, using all available states as well as states with a similar pre-reformulation misuse rate of OxyContin as potential donors. The vertical line marks the year before RTW adoption.

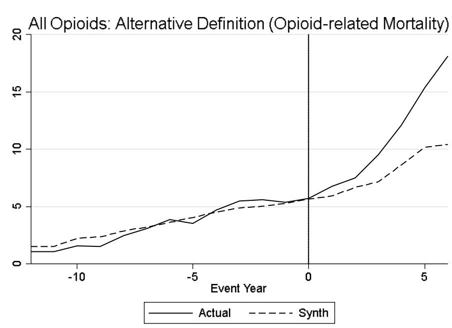
Figure A7 Alternative measures of opioid misuse



(a)



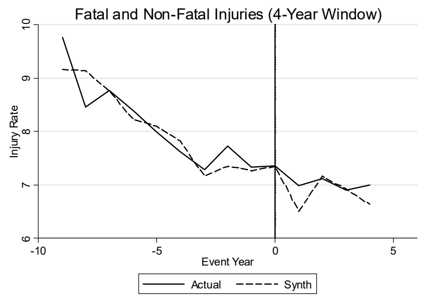
(b)



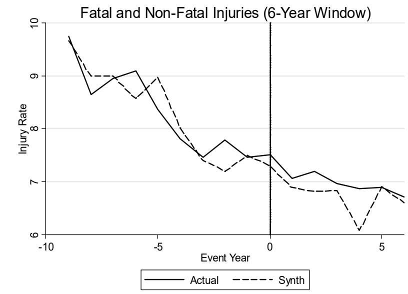
(c)

Notes: These figures show the estimated RTW effects on two alternative measures of opioid misuse: 1) prescription opioids and 2) all opioids excluding "other/unspecified narcotics" from the list of contributing causes using the same samples in column 2 of Tables 3 and 4. The vertical line marks the year before RTW adoption.

Figure A8 Workplace safety and self-rated health (working-age population)

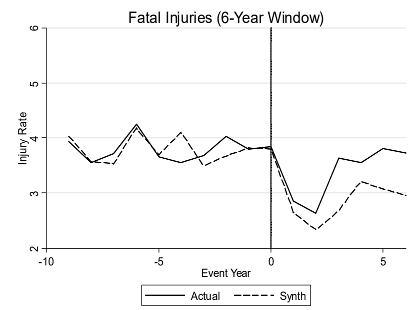


(a)

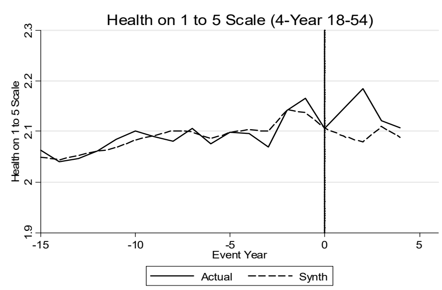


(b)

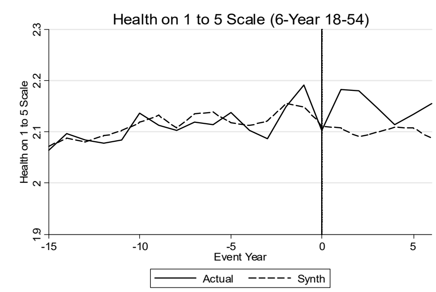
(c)



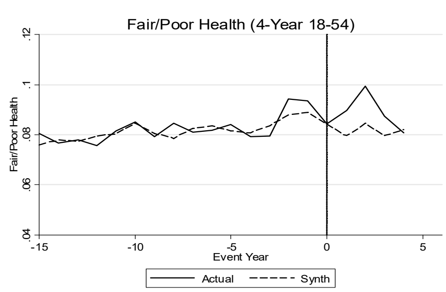
(d)



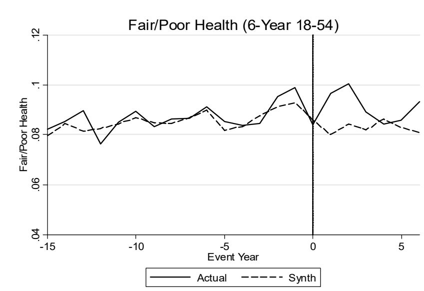
(e)



(f)



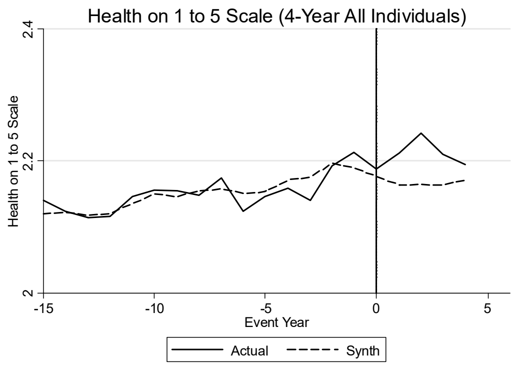
(g)



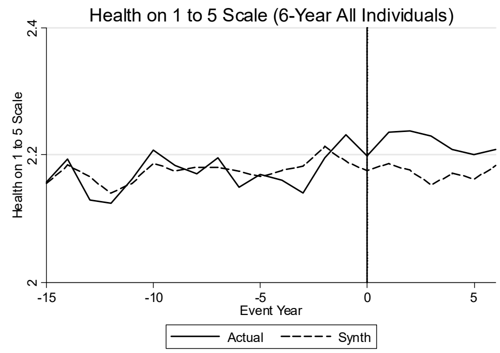
(h)

Notes: Figures A8a-A8d show the estimated RTW effect on total workplace injuries and fatal workplace injuries using data from 2002-2018 BLS SOII and CFOI, pooling Indiana, Michigan, and Wisconsin when applicable. Figures A8e-A8h show the estimated RTW effect on self-reported health for individuals aged between 18 and 54 using CPS 1996-2018, for the same group of states. The vertical line marks the year before RTW adoption.

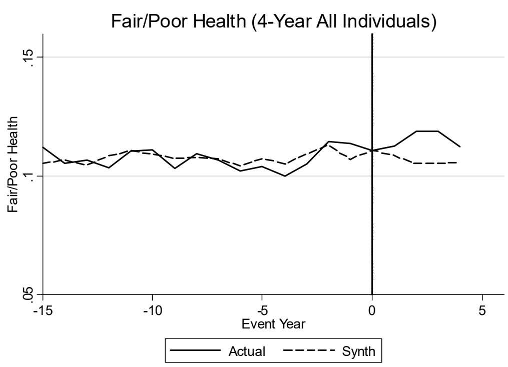
Figure A9 Self-rated health (all individuals)



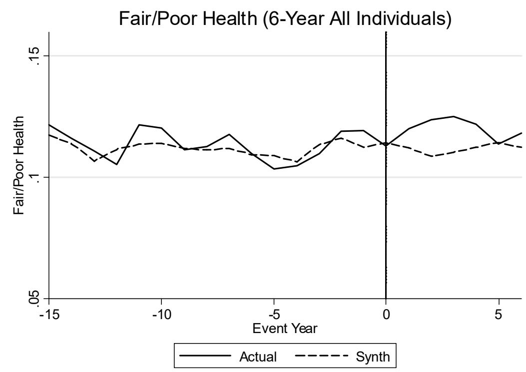
(a)



(b)



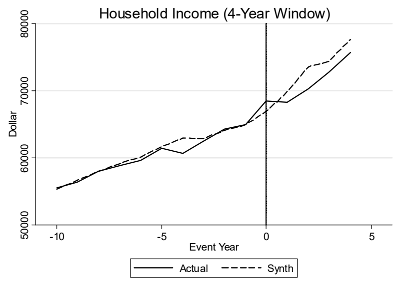
(c)



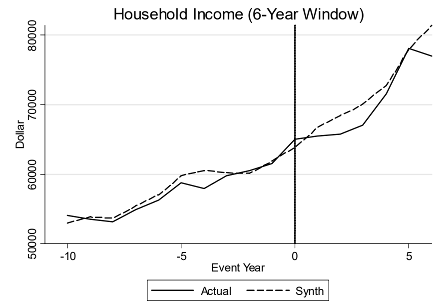
(d)

Notes: This figure shows the estimated RTW effect on self-reported health for all individuals using CPS 1996-2018, pooling Indiana, Michigan, and Wisconsin when applicable. The vertical line marks the year before RTW adoption.

Figure A10 Income, wages, unemployment, and long working hours

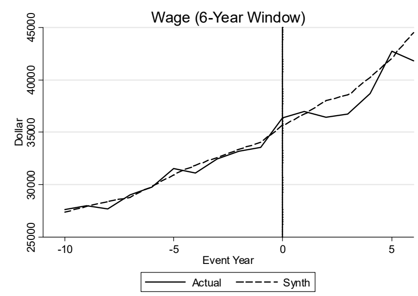


(a)

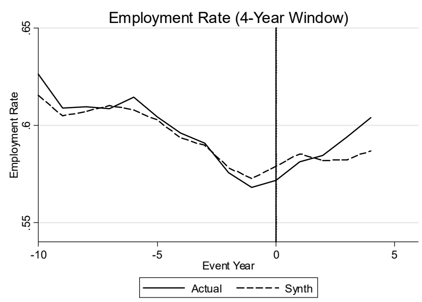


(b)

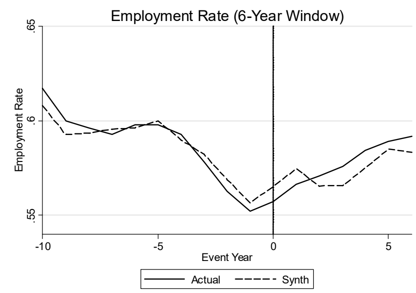
(c)



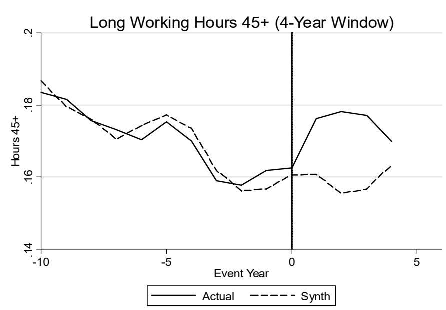
(d)



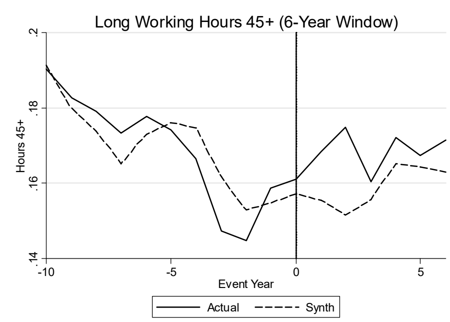
(e)



(f)



(g)



(h)

Notes: Figures A10a-A10d show the estimated RTW effects on inflation-adjusted before-tax after-transfer household income and personal wages using 1990-2019 CPS, pooling Oklahoma, Indiana, Michigan, and Wisconsin when applicable. Figures A10e-A10h show the estimated RTW effects on the employment rate and the share of workers working for more than 45 hours per week for the same group of states. The vertical line marks the year before RTW adoption.