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How do right-to-work (RTW) laws impact workplace safety?

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RTW laws allow employees covered by a union who are not union members to choose not to pay union dues. These laws weaken unions and decrease unionization. This study explores the impact of right-to-work (RTW) laws on workplace safety using a two-way fixed effects and a difference-in-differences approach, focusing on the five right-to-work laws passed by states in the 2010s.

For my two-way fixed effects analysis, I construct a panel dataset from 2007-2019 using yearly state-level data from BLS for all 50 states with available data. My outcome variables are nonfatal occupational injury and illness rates and occupational fatality rates. I include state and year fixed effects with a large set of controls. I find that RTW laws reduce unionization and harm workplace safety. Specifically, I find that RTW laws decrease union coverage and membership by about 2.1% and 2.2%, respectively. I find that RTW laws increase occupational fatality rates by about 0.22 per 100,000 employees through decreasing unionization. This number is about 5% of the mean occupational fatality rate in my sample and suggests that right-to-work laws cost hundreds of lives each year. I also find, however, that right-to-work laws decrease the rate of reported nonfatal occupational injuries and illnesses by about 6%, and I am able to replicate this finding when looking at specific industries. I find that this decrease is likely being driven by increased reporting in unionized workplaces rather than a true improvement in workplace safety.

Honors Project in Economics

Advisor: Professor Gary Krueger
Economics

I. Introduction

On April 5, 2010, there were 31 miners on the site of the Upper Big Branch coal mine in Montcoal, West Virginia, when a coal-dust explosion 1,000 feet underground killed 29 miners. The terrible accident could have been avoided. In the year before the blast, records show that federal regulators "cited the mine eight times for 'substantial' violations relating to the mine's methane control plans."[1] Investigators found that poor ventilation and a dangerous build-up of methane led to the deadly explosion, and the company that owned the mine settled with the U.S. attorney's office for $209 million in civil and criminal penalties for its role in the accident.[2] The CEO of the company was later sentenced to a year in prison on a charge of conspiracy to willfully violate mine health and safety standards as a result of the deadly event.[3]

In April of 2020, meatpacking workers in a Tyson Foods pork-processing plant in Waterloo, Iowa, continued to work amidst the deadly COVID-19 pandemic. Those employees were required to work long hours in cramped conditions without sufficient protective gear. Over 1,000 of the plant's 2,800 employees would contract the virus in the first few months of the pandemic. Lawsuits filed on behalf of deceased Waterloo employees allege that the company failed to implement appropriate safety measures, downplayed the risk of the virus, covered up the outbreak, incentivized sick workers to continue working, and otherwise disregarded workplace safety. As employees worked in these dangerous conditions, supervisors at the plant are accused of privately gambling on the number of workers who would contract the COVID-19 virus.[4]

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[1] Ian Urbina, "West Virginia Mine Blast: An Abrupt End to a Typical Shift," The New York Times, April 9, 2010, https://www.nytimes.com/2010/04/10/us/10westvirginia.html.
[2]Plumer, Brad. "W.Va. Coal Mine to Pay Historic $209M Settlement in Blast That Killed 29 Miners." Washington Post, 6 Dec. 2011., https://www.washingtonpost.com/business/economy/wva-coal-mine-to-pay-historic-209m-settlement-in-blast-that-killed-29-miners/2011/12/06/gIQAbClqZO_story.html.
[3]Berman, Mark. "Former Coal CEO Sentenced to a Year in Prison after 2010 West Virginia Coal Mine Disaster." Washington Post, 26 Oct. 2021. https://www.washingtonpost.com/news/post-nation/wp/2016/04/06/former-coal-ceo-sentenced-to-a-year-in-prison-for-2010-west-virginia-coal-mine-disaster/.
[4] See: Kauffman, Clark, et al. "Lawsuit: Tyson Managers Bet Money on How Many Workers Would Contract COVID-19." Iowa Capital Dispatch, 18 Nov. 2020, https://iowacapitaldispatch.com/2020/11/18/lawsuit-tyson-managers-bet-money-on-how-many-workers-would-contract-covid-19/ and "Fernandez v. Tysons Food Complaint," https://www.chamberlitigation.com/sites/default/files/Fernandez%20v.%20Tysons%20Food%20Complaint.pdf.

It wasn't just the Tyson Foods plant in Waterloo that saw increased concerns about workplace safety during the pandemic. All across the country, workplace safety was thrust to a top priority as workers became concerned about catching COVID-19 in their workplaces. In numerous instances, unions were able to step in to protect workers.[5] For example, the United Auto Workers "persuaded General Motors, Ford, and Fiat Chrysler to shut down operations for two weeks to slow the spread of the virus, and they negotiated with the companies to provide all workers with protective gear, including masks" (McNicholas et al., 2020).

How unions can improve workplace safety

There are a number of ways that unions can protect the health and safety of workers, including by bargaining directly with employers to secure enhanced safety measures, better protective equipment, better compensation for injured workers, and more. Before the COVID-19 pandemic, unions had been shown to improve workplace health and safety (Zoorob, 2018; Hagedorn, 2016). According to Weil (1991):

> "Labor unions devote substantial resources to monitoring and improving public health," and, "Currently [1991], eight of the top ten unions maintain separate health and safety departments, many staffed with epidemiologists, physicians, industrial hygienists, and public health specialists. Activities of these departments include sponsoring original research on health risks; creating training materials and programs concerning workplace health and safety; and keeping abreast changes in health and safety technologies and in laws" (p. 22).

Unions were also instrumental in developing and passing the Occupational Safety and Health Act of 1970 (Schurman et al.,1998, as cited in Donado, 2015), and they have helped gain recognition for occupational diseases caused by exposure to dangerous chemicals (Donado, 2015).

Another key role that unions can play in enhancing workplace safety is monitoring workplaces for compliance with safety-related agreements, reporting suspected safety violations, and protecting employees who report violations from retaliation, thereby improving enforcement of OSHA standards. The Occupational Health and Safety Administration (OSHA), founded in 1970,

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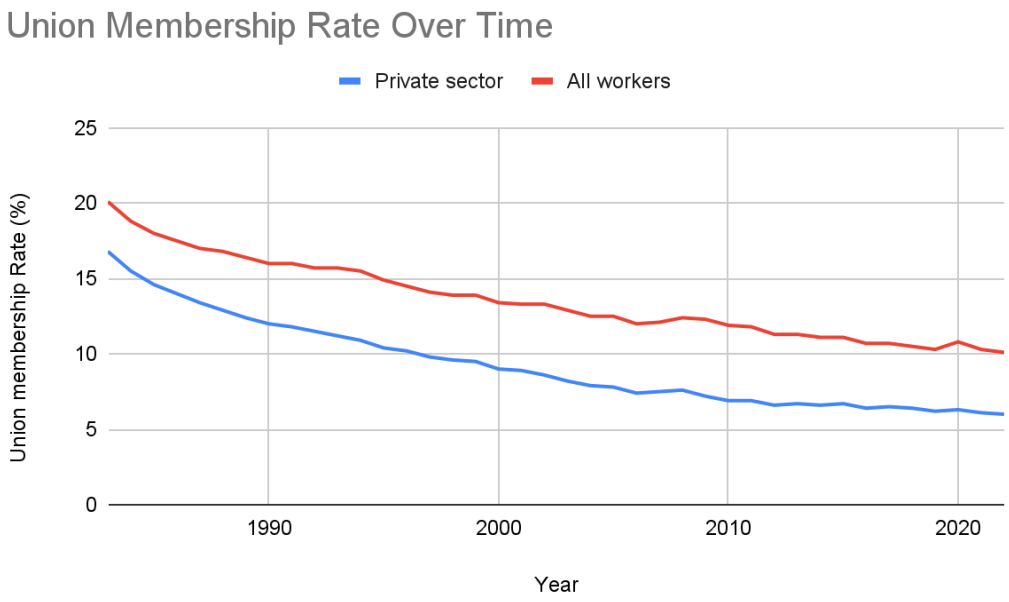
[5]See, e.g., "Why Unions Are Good for Workers—Especially in a Crisis like COVID-19: 12 Policies That Would Boost Worker Rights, Safety, and Wages." Economic Policy Institute, https://www.epi.org/publication/why-unions-are-good-for-workers-especially-in-a-crisis-like-covid-19-12-policies-that-would-boost-worker-rights-safety-and-wages/. Accessed 6 Apr. 2023. and "Grocery Store Workers Fight to Wear Masks, Gloves While on the Job." KQED, 26 Mar. 2020, https://www.kqed.org/news/11808742/grocery-store-workers-fight-to-wear-masks-gloves-while-on-the-job.

enforces the rights of workers to safe and healthy workplaces.[6] But OSHA is considered to be understaffed and underfunded, with "1,850 inspectors responsible for the health and safety of 130 million workers."[7] Many scholars have found evidence that unions increase OSHA enforcement activity (Sojourner and Yang, 2016; Weil,1991,1992).

Unionization and right-to-work laws

Union membership has declined dramatically in the U.S. over the past 40 years. In 1983, the union membership rate was 20.1%. By 2022, it fell to 10.1%, the lowest union membership rate on record. Below is a graph showing union membership rates for all workers and private sector workers from 1983 (the first year with available data) to 2022:[8]

Figure 0.1



The reasons for the steep decline in unionization over past decades are not entirely clear. According to the White House's Council of Economic Advisors, "Globalization, technological change, and employer concentration are commonly cited as key factors, eroding union power and increasing employers' bargaining position relative to workers."[9] Autor et al. (2017) and others have documented an increase in the monopsony power of employers in recent decades.

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[6] About OSHA | Occupational Safety and Health Administration. https://www.osha.gov/aboutosha. Accessed 6 Apr. 2023.
[7] Commonly Used Statistics | Occupational Safety and Health Administration. https://www.osha.gov/data/commonstats. Accessed 6 Apr. 2023.
[8] Data from BLS.
[9] "The State of Our Unions | CEA." The White House, 5 September, 2022, https://www.whitehouse.gov/cea/written-materials/2022/09/05/the-state-of-our-unions/.

Another important part of the story are the many obstacles employees face when attempting to form a union. Employers often hire union avoidance consultants and coordinate anti-union campaigns when employees try to unionize.[10] According to the Economic Policy Institute, employers are charged with violating federal law in 41.5% of all union election campaigns.[11]

Related to the decline in unionization in the U.S., there has been a relatively-recent uptick in anti-union right-to-work (RTW) laws that allow workers protected by a union to choose not to pay dues. According to the National Labor Relations Board (NLRB), in states with right-to-work laws, "it is up to each employee at a workplace to decide whether or not to join the union and pay dues, even though all workers are protected by the collective bargaining agreement negotiated by the union."[12] In all states, employees covered by a collective-bargaining agreement can choose whether to become full members of a union. If you are a member, you have the right to vote on union business. You can elect union officials, participate in ratification of collective-bargaining agreements, and vote on negotiation issues (depending on the union). If you work in a unionized shop but choose not to be a member, the union represents you but you will be unable to vote on internal union matters. In non-RTW states, employees who choose not to become members of the union are still required to pay at least the share of dues used directly for representation, such as collective bargaining and contract administration.[13] In RTW states, employees can choose not to join the union and not pay any dues, even though the union must represent them and they enjoy the benefits of the collective-bargaining agreement negotiated by the union.[14]

Currently, 26 states[15] have right-to-work (RTW) laws in place. These laws are thought to decrease unionization rates and weaken union bargaining power. The Taft-Hartley Act of 1947 allowed states to pass RTW laws, and by 1964, 19 states had adopted those laws. A few states had passed RTW laws starting as early as 1944, but the legality of these laws was murky before the 1947 Taft-Hartley Act. From 1964-2010, just three more states passed RTW legislation. But

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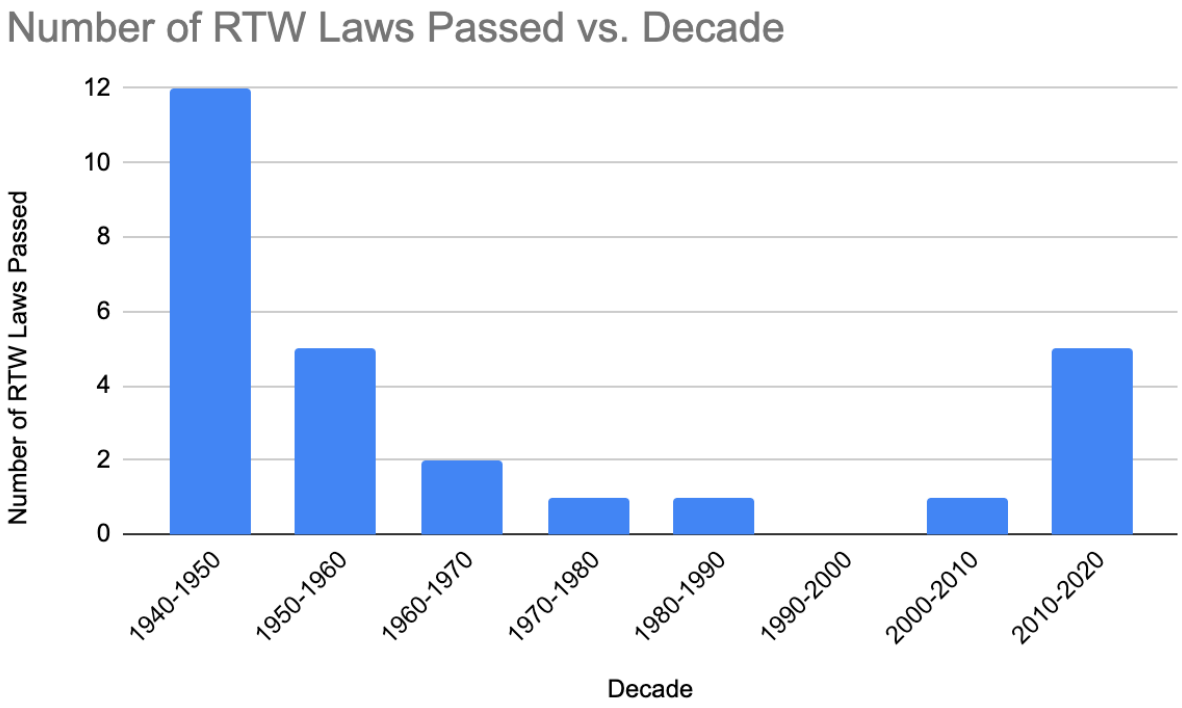
[10] "Unionization Increased by 200,000 in 2022: Tens of Millions More Wanted to Join a Union, but Couldn't." Economic Policy Institute, https://www.epi.org/publication/unionization-2022/ Accessed 6 Apr. 2023.
[11] Ibid.
[12] National Labor Relations Board, "Employer/Union Rights and Obligations," https://www.nlrb.gov/about-nlrb/rights-we-protect/your-rights/employer-union-rights-and-obligations.
[13] National Labor Relations Board, "Union Dues," accessed on April 3, 2023, https://www.nlrb.gov/about-nlrb/rights-we-protect/the-law/employees/union-dues.
[14] Ibid.
[15] Not including Michigan, which repealed its RTW law in 2023.

since 2010, five more states have passed RTW legislation: Indiana (2012), Michigan (2013)[16], Wisconsin (2015), West Virginia (2016), and Kentucky (2017). Below, Figure 0.2 shows the number of RTW laws passed by decade:

Figure 0.2



Southern states all have RTW laws in place, along with some states in the Midwest and West.[17]

In 2018, the Supreme Court ruled in *Janus v. AFSCME*[18] that the mandatory collection of fees from public-sector employees violated the First Amendment.[19] This ruling made all public sector (government) employees, in effect, covered by RTW laws.

Motivation

One of the reasons why right-to-work is worthy of study is the relatively-recent uptick in RTW laws led by a renewed push from Republican lawmakers. Most RTW states passed RTW laws

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[16] Michigan repealed its RTW law in 2023.
[17] This website has a map of states with RTW laws and shows year of enactment: https://ballotpedia.org/Right-to-work_laws
[18] *Janus v. AFSCME, Council 31*, 585 U.S. __, 138 S.Ct. 2448 (2018). https://www.supremecourt.gov/opinions/17pdf/16-1466_2b3j.pdf
[19] Oyez, "Janus v. American Federation of State, County, and Municipal Employees, Council 31," https://www.oyez.org/cases/2017/16-1466.

before 1960, but five states passed right-to-work laws in the 2010s. Because many states passed RTW laws so early (pre-1964), there was a lack of detailed economic microdata to study the impact of these laws and control for underlying state differences (Fortin et. al, 2022). Additionally, the early RTW laws were passed prior to the establishment of the OSHA in 1970. The recent RTW laws have provided an opportunity for economists to more-effectively study their impact.

In 2018, the year of the Supreme Court's ruling in *Janus* that effectively made all public employees covered by right-to-work laws, there were over 7.2 million public sector employees who belonged to unions according to BLS. Since so many employees were affected by this ruling, it is important to understand the impact of RTW laws.

Much of the empirical literature on unions and right-to-work laws focuses on how they impact wages. Workplace safety, however, is also a key issue, and not solely in the context of the COVID-19 pandemic. According to BLS, in 2019 there were 2,814,000 private industry nonfatal occupational injuries and illnesses ("NFOIAI") and 5,333 occupational fatalities in the U.S..[20] Of course, this has a tremendous impact on the individuals and families of those who suffer occupational injuries, illnesses, or fatalities. This also has substantial economic costs. According to OSHA, occupational injuries and illnesses cost American employers more than $97.4 billion a year in workers' compensation costs.[21] Other economic costs include the lost productivity from those unable to work. Studying right-to-work laws may also allow us to gauge the impact that unions have on safety more broadly.

If RTW laws decrease unionization and/or weaken union bargaining power, and unions help protect the health and safety of workers, these laws could harm workplace safety. In this paper, I examine the following question: What is the impact of right-to-work laws on workplace safety?

Paper outline and key findings

In Section II I provide background information on unions, OSHA, workers' compensation, and the nature of common occupational injuries, illnesses, and fatalities. Section III contains a review of the empirical literature relevant to my research question. In Section IV I outline my

[20]Data from BLS. https://data.bls.gov/timeseries/ISU00000000061100; https://data.bls.gov/timeseries/FWU00X00000080N00

[21] Occupational Safety and Health Administration, "All About OSHA," https://www.osha.gov/sites/default/files/publications/all_about_OSHA.pdf.

empirical strategy and conceptual model as well as provide an overview of the data. In sections V-VIII, I analyze the impact of RTW laws on unionization, occupational fatalities, non-fatal occupational injuries and illnesses (NFOIAI), non-fatal occupational injuries and illnesses requiring one or more days away from work to recover (henceforth, severe injuries), using two-way fixed effects regressions and a large panel data set from 2007-2019. Section X contains the results of my difference-in-differences analyses comparing similar states, and Section XI contains my conclusion section.

In my two-way fixed effects analyses, I find that RTW laws harm workplace safety by decreasing unionization and increasing occupational fatalities. I find that RTW laws decrease union coverage and membership by about 2.1% and 2.2%, respectively. I find that right-to-work laws increase occupational fatality rates by about 5% through decreasing union coverage rates. However, I find that RTW laws actually decrease the number of reported NFOIAI. I find that RTW laws decrease reported NFOIAI rates by about 6%. I replicate this finding of RTW laws decreasing NFOIAI rates looking specifically at the construction and manufacturing industries. However, I believe that this finding is due to increased reporting by unionized employees rather than a true improvement in workplace safety. This is because of my finding that RTW laws increase occupational fatalities. Additionally, when I observe severe injuries involving fractures, which I hypothesize are less likely to suffer from reporting bias, I do not find that RTW decreases these types of injuries. When looking at all severe injuries (not just those involving a fracture), I do not find any coefficient with statistical significance that I consider to be reliable.

In my subsequent differences-in-differences analyses comparing each recent RTW state to a similar state, I am unable to replicate these findings. However, these estimates are very sensitive, and are statistically insignificant. My yearly data is quite suboptimal for difference-in-differences. I argue that my two-way fixed effects estimates are more reliable.

II. Background Information

History of unions

Collective bargaining has long been a part of the U.S. economy; the first ever recorded instance of worker strike in America occurred in 1768.[22] Labor organizations before the late 1800s were typically local and small, and unions were not very common. The American Labor movement

[22]"Labor Movement." History.com, 31 Mar. 2020, https://www.history.com/topics/19th-century/labor.

picked up steam in the late 1800s, when the pace of industrialization in the country increased following the Civil War.[23] The first major labor organization in the U.S. was the Knights of Labor, founded in 1869. Membership in the Knights of Labor grew to 700,000 by 1886.[24] Unions in this era often advocated for an eight-hour workday, an end to child labor, as well as bargaining for improved working conditions and higher wages. However, forming a labor union in the late 1800s and early 1900s was no easy task. Employers would take extreme measures to prevent employees from forming a union and to end employee strikes. Employers, often with the help of police or militias, would use violence against workers. Unionized employees used violence as well. This resulted in a number of deadly clashes.[25]

In 1935, Congress passed the National Labor Relations Act of 1935, also known as the Wagner Act. The Wagner Act codified the rights of employees to form unions and collectively bargain, and established the National Labor Relations Board.[26] Prior to the passage of the Wagner Act, unions and collective bargaining were much more likely to be subject to unfair labor practices by employers and unfavorable court rulings.[27] In the 1950's, almost one third of workers belonged to a union.[28] But unionization has declined significantly in recent decades. In 2022, the union membership rate was just 10.1 percent. Still, unions play a key role in our economy, and there were 14.2 million American union members in 2022.[29] And even though the Wagner Act is meant to protect employees from retaliation due to unionization, employees continue to face steep barriers to forming unions.

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[23] Labor Wars in the U.S. | American Experience | PBS. https://www.pbs.org/wgbh/americanexperience/features/theminewars-labor-wars-us/. Accessed 6 Apr. 2023.
[24] "The Knights of Labor." Khan Academy, 11 Nov. 2020, https://www.khanacademy.org/humanities/us-history/the-gilded-age/gilded-age/a/the-knights-of-labor.
[25]Labor Wars in the U.S. | American Experience | PBS. https://www.pbs.org/wgbh/americanexperience/features/theminewars-labor-wars-us/. Accessed 6 Apr. 2023.
[26] "1935 Passage of the Wagner Act." National Labor Relations Board, 29 Apr. 2015, https://www.nlrb.gov/about-nlrb/who-we-are/our-history/1935-passage-of-the-wagner-act.
[27] "What Is The Wagner Act and Why Was It Important?" Zippia, 29 Mar. 2021, https://www.zippia.com/advice/wagner-act/.
[28] "Labor Unions." National Museum of American History, 18 June 2015, https://americanhistory.si.edu/american-enterprise-exhibition/consumer-era/labor-unions.
[29] Union Members- 2022. Bureau of Labor Statistics, 19 Jan. 2023, https://www.bls.gov/news.release/pdf/union2.pdf.

OSHA

OSHA[30] was established in 1970 under the Occupational Safety and Health Act of 1970 (OSH Act).[31] The act established the right of every worker to "safe and healthful working conditions." OSHA is a government agency that establishes safety and health standards for workplaces and is responsible for enforcing those standards; it inspects workplaces and responds to fatalities, catastrophes, and complaints. OSHA is also responsible for issuing citations and fines. Although occupational injuries, illnesses, and fatalities have decreased significantly since its inception, OSHA is considered to be vastly underfunded and criticized by some as ineffective.[32]

Another aspect of OSHA that is relevant is the role of state-run OSHA plans. The OSH Act encourages states to develop, operate, and enforce their own job safety and health plans. Having an OSHA-approved state plan means that workplace safety standards are enforced by the state rather than federal OSHA, subject to federal monitoring. OSHA enforcement is likely improved in states with an OSHA program administered by state agencies, as Zullo (2011) finds in his analysis. There are currently 22 state plans covering both private sector and state and local government workers.[33]

What happens when an employee is injured on the job?

When an employee is injured or becomes ill from their work, they are eligible for workers' compensation. Workers' compensation programs are run at the state level and vary by state. Nearly all businesses are required to purchase workers' compensation insurance or become self-insured. In Minnesota, when a worker is injured on the job, they are to report their injury to their employer. The employer then files a claim with its insurer, which can either accept or deny the claim. If the claim is denied, the injured worker can dispute the decision in court. If the claim is accepted, the injured worker will be entitled to medical, wage, and vocational rehabilitation benefits. Generally, if the injury leaves an employee completely unable to work for a period of time, wage benefits amount to two-thirds of an employee's average wage. However, states differ

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[30] I use OSHA to refer to the agency. When discussing the Occupational Safety and Health Act of 1970, I will write out the name or refer to it as the "OSH Act".
[31] About OSHA | Occupational Safety and Health Administration. https://www.osha.gov/aboutosha. Accessed 6 Apr. 2023.
[32] Hale, Nathan, and John Leeth. "Evaluating OSHA's Effectiveness and Suggestions for Reform." Mercatus Center- George Mason University, April 23, 2013. https://www.mercatus.org/students/research/policy-briefs/evaluating-oshas-effectiveness-and-suggestions-reform.
[33]This link includes a description of state plans and shows which states have state plans in place: https://www.osha.gov/stateplans

in their workers' compensation programs. Courts and insurance companies in different states may also have varying standards and propensities for accepting claims and assigning benefits. For example, the waiting period to receive workers' compensation benefits after an injury ranges from 3-7 days, with many states on either side of the range.[34] There is also variation in how much of an employee's average weekly wage they are entitled to if they are unable to work.

Common workplace injuries

What are typical workplace injuries? Transportation incidents are the leading cause of workplace fatalities, accounting for over 40% of all workplace fatalities from 2011-2019. 16% of workplace fatalities from 2011-2019 were due to falls, slips, or trips, and another 16% were due to violence. 14.8% were due to contact with objects and equipment. The leading causes of nonfatal injuries and illnesses (NFOIAI) involving days away from work (severe injuries) from 2011-2019 were: overexertion and bodily reaction; falls, slips, or trips; and contact with objects and equipment.[35] This data comes from BLS; compiled on the CDC website (see footnote).

Both non-fatal occupational injuries and illnesses as well as fatal occupational injuries are concentrated in certain industries. The industries with the most occupational fatalities from 2011-2019 were: construction; transportation and warehousing; agriculture, forestry, fishing and hunting; public administration; and administrative and waste services. The industries with the most nonfatal occupational injuries and illnesses from 2007-2019 were health care and social assistance; manufacturing; and retail trade.[36] I discuss this further in my conceptual model.

III. Literature Review

Economists' views of unions

When employees form a labor union, they increase their bargaining power and can improve their wages. In "The Analysis of Union Behavior," Farber (1984) describes unions' fundamental role as "organizations that seek to create or capture monopoly rents available in an industry" (p. 9). There is a consensus in the empirical literature that unions increase wages and reduce

[34] State Worker Comp Summary Fact Dashboards | NIOSH | CDC. 8 Apr. 2021, https://www.cdc.gov/niosh/topics/workercomp/cwcs/dashboard.html.
[35] All data in this section comes from: CDC - NIOSH Worker Health Charts. https://wwwn.cdc.gov/Niosh-whc/chart/BLS-II?T=ZS&V=C&D=RANGE. Accessed 6 Apr. 2023.
[36] Ibid.

inequality.[37] However, economists have varying views on labor unions. Unions are sometimes inefficient from a classical economic perspective, as they can raise wages above competitive levels. But they have some desirable impacts on society, such as protecting the wages of the most vulnerable workers (Blanchflower and Bryson, 2003).

Economists have typically focused on how unions impact wages. There is a consensus in the literature that unions increase wages, and there is a vast economic literature that attempts to estimate the union wage premium. Freeman and Meedoff (1984), Card (1996), and Blanchflower and Bryson (2003) all find a significant union wage premium that varies across people, time, and markets.

Right-to-work laws and unionization

How do right-work-laws affect unionization? As mentioned in the introduction, there are a number of ways these laws harm unionization. In RTW states, employees covered by unions have a greater incentive to not become union members, as they can avoid paying dues altogether. In theory, this should result in lower union membership, less union revenue, less effective unions, fewer unions, and less union coverage in RTW states. The empirical literature does bear this out, and there is a consensus that RTW laws decrease unionization. However, there is disagreement about the extent to which RTW laws do so, and it can be difficult to attribute changes in unionization to RTW laws themselves, as RTW laws also reflect a state's anti-union sentiment. RTW laws can also create momentum for other anti-union policies, or conversely propel unions to increase their organizing efforts.

Moore and Newman (1985) review the empirical literature on RTW laws and unionization. The authors find that most studies find a negative effect of RTW laws on unionization, but "the measured effects of RTW laws on union membership are highly sensitive to the choice of estimation techniques." (p.576). Studies that used a "single-equation OLS model with RTW variable exogenous" estimation technique generally had negative and statistically significant estimates of the RTW coefficient on union density. In studies that treated both unionism and RTW as endogenous and studies that used simultaneous-equation models, results were more mixed, though most still had a negative coefficient on RTW. Almost all studies used data at the state or metropolitan statistical level. Across all of the studies Moore and Newman examined,

[37] See Farber et al. (2018): "Unions and Inequality Over the Twentieth Century: New Evidence from Survey Data" for an analysis of unions' impact on inequality.

the average right-to-work coefficient on union membership was -2.718, and the average t-statistic was around -2.

One notable paper examined in Moore and Newman's review of the literature comes from Lumsden and Peterson (1975). Lumsden and Petersen estimate a model for the supply and demand for unionization and estimate unionization rates as a function of control variables and a RTW dummy variable for 1953 and 1968. They use BLS data at the state level. The authors also assign RTW treatment to states that would later pass RTW laws following the Taft-Hartley Act in 1947, but had not in 1939, for a 1939 regression to serve as a comparison group (the first RTW law was not until 1944, and those RTW laws faced legal challenges prior to the Taft-Hartley Act in 1947). Their coefficient on RTW for their 1953 regression is very similar to 1939, which was before RTW laws were enacted. They test the impact of RTW laws in other ways as well, and conclude that differences in unionization in RTW states "reflect tastes and preferences of the population rather than a substantive impact of the laws themselves" (Lumsden and Peterson, 1975, p.12). But panel data was not as well understood in 1975 as it is today, and Lumsden and Peterson did not employ two-way fixed effects regressions. If they had used an estimation technique like this, they may have identified substantive effects of RTW.

Fortin et al. (2022) cite two papers finding differing impacts of RTW on unionization: Ellwood and Fine (1987) and Farber et al. (2020). Ellwood and Fine (1987) examine the impact of RTW laws by looking at flows into unionism through organizing rather than looking at the stock of unionism. They use both cross-sectional and time-series methodologies with state-level data, and find that organizing decreases by nearly 50% in the first 5 years after a RTW law is passed, and membership in unions decreases by 5-10% after a RTW law is passed. Because their paper was published in 1987, it does not use evidence from the states that passed RTW laws more recently. Also, some may criticize their focus on the flows to unionization rather than the stock of unions. Farber et al. (2020) evaluate RTW laws as a possible instrumental variable for exogenous changes in union density, but they fail to find a significant impact of RTW on union density. As their analysis focused on unions and inequality in the 20th century, they do not consider the recently passed RTW laws that are the focus of my analysis. Observing states that passed RTW laws in the 40s and 50s, they find that union density in states that passed RTW laws in those decades was already lower than in states that did not, and that after the passage of RTW laws, unionization in those states followed national trends. They also construct a panel database similar to Ellwood and Fine (1987) looking at NLRB election data at the state level.

Interestingly, they are unable to replicate the findings of Ellwood and Fine (1987) of a significant impact of RTW laws on union elections.

The most recent work on the impact of RTW on unionization and wages comes from Fortin et al. (2022). The authors use Current Population Survey data from 2003-2019, with complementary data starting in 1983. First, they use an event study/difference-in-differences design to analyze the impact of RTW laws. In their difference-in-differences estimate they find that RTW laws reduce unionization by close to two percent. They also find that RTW has a larger impact on women relative to men, and has a disproportionate impact on unionization rates in construction, education, and public administration (three high-unionization industries). Their event study estimates show that the decreases in unionization as a result of RTW laws begin fairly immediately and continue throughout their sample. The authors then use a differential exposure design that exploits industry differences in unionization rates. Interestingly, when they use RTW as an instrument to measure the impact of unionization on wages, they find much larger coefficients than in corresponding OLS estimates. This suggests that the effect of RTW laws is not entirely mediated through changes in the unionization rates, and RTW may lower union threat effects and reduce union bargaining power. Their finding of union threat effects is consistent with other literature on the impact of unionization and wages (Farber et al., 2021, and Fortin, Lemieux, and Lloyd, 2021, as cited in Fortin et al., 2022). The paper provides strong evidence that RTW laws decrease unionization and wages.

Based on economic theory and a review of the literature, we expect that RTW laws decrease unionization. But it is important to remember that differences in unionization between RTW and non-RTW states are not solely attributable to RTW laws themselves, and that not all of the literature finds a large negative impact of RTW on unionization. Another key takeaway that relates to my research is the finding of a substantial union threat effect in Fortin et. al (2022), whereby unions are weakened in RTW states. This finding is consistent with other economic literature. Thus, even if unionization does not decrease in RTW states, RTW laws may still impact workplace safety if unions experience reduced bargaining power.

Unions and workplace safety

Economic theory and anecdotal evidence would suggest that unions improve workplace safety. For example, Hagedorn et al. (2016) interview union organizers and examine 16 union contracts from industries in the Pacific Northwest. They find that union contracts seek to improve key

determinants of public health, and that unions promote public health along with a culture of workplace safety.

The empirical literature on the impact of unionization on workplace safety, however, is more ambiguous. The literature suggests that unionization *decreases* fatalities but *increases* nonfatal injuries and illnesses. I first turn to a meta-analysis performed by Donado (2015) that extensively reviews the literature on unionization and workplace safety. In 10 of the 11 estimates of the impact of unionization on fatal workplace injuries that Donado considers, unions are associated with *fewer* fatal occupational injuries. However, in 27 of the 32 estimates of the impact of unionization on nonfatal workplace injuries that Donado considers, unions are associated with *more* nonfatal occupational injuries. These estimates cover a broad span of industries, geographies, years, and estimation strategies. This finding for nonfatal injuries is perplexing and contrary to anecdotal evidence and union activities (Donado, 2015). According to Li et al. (2017) and Donado (2015), the literature considers three primary explanations for unions being associated with more nonfatal injuries: reporting, whereby employers are more likely to report injuries in the presence of a union; selection, whereby more dangerous workplaces are more likely to unionize, and, finally, that unions may bid up wages in exchange for safety. Reporting is the most commonly cited explanation.

After doing a meta-analysis of the literature regarding unionization and workplace safety, Donado (2015) performs his own analysis. He uses panel data at the individual level from the National Longitudinal Survey of Youth 1979 from 1988-2000. Using both OLS and FE regressions with a wide set of control variables, the author finds that unionization substantially increases nonfatal occupational injury.

Appleton and Baker (1984), Boal (2009), and Morantz (2013) each examine the impact of unionization on workplace safety in the coal mining industry. Appleton and Baker (1984) use a cross-sectional analysis of the coal underground mining industry in 1979. The authors find that higher unionization rates are associated with higher levels of nonfatal injury even with a number of control variables. The authors propose that the injury gap results from a union mine policy that places no time limit on how long a worker must be on the job before they bid on a new job. This results in higher turnover rates at unionized mines, and, “miners working at new, unfamiliar jobs are more likely to be injured than experienced miners.” (Appleton and Baker, 1984, p. 145). Boal (2009) uses panel data at both the state and mine levels to examine the impact of

unionization on safety in the U.S. coal mining industry from 1897-1929. He finds that unionism significantly reduced fatalities. Morantz (2013) compares union mines to non-union mines from 1993 to 2010 to estimate negative binomial regression models on non-traumatic injuries, total injuries, traumatic injuries, and fatalities, with a treatment dummy for the presence of a union and a large number of covariates that are likely to affect mine safety. She finds that unionization is associated with a “sizable and robust decline” in both traumatic injuries and fatalities (Morantz, 2013, p. 109). Unionization, however, was found to predict higher total and nontraumatic injury cases. Morantz suggests as an explanation that workers protected by unions may be more likely to report their injuries.

Li et al. (2017) and Zoorob (2018) look at the relationship between unionization and workplace safety and find positive impacts on workplace safety. Li et al. (2017) observe establishments before and after union elections in the manufacturing industry from 1999 to 2010 and find that: “The results indicate that union elections improved occupational safety. First, workplace inspections trended upwards before the election, then decreased immediately after the election, due almost entirely to employee complaints. Second, accident case rates were relatively stable before the election, then trended downwards after the election, due to accidents involving days away from work, job restrictions, and job transfers” (Li et al., 2017, p.0). Interestingly, Li et al. (2017) find that positive impacts on worker safety are present regardless of the election outcome, pointing to a union threat effect. This finding of a threat effect is consistent with the findings of Fortin et al. (2022) and others. A key advantage of the empirical strategy used by Li et. al (2017) is that by tracking establishments before and after union elections rather than comparing union and nonunion workplaces, they address the selection issue in which more dangerous firms may be more likely to unionize (Li et al., 2017). Zoorob (2018) uses a two-way fixed effects model on a panel dataset from 1992-2016 to evaluate the impact of a state’s unionization rate on workplace fatalities. He finds that a 1% increase in unionization leads to a 2.8% decrease in occupational fatalities, and his estimate is largely unchanged after adding in his control variables. Zoorob also writes about the impact of RTW on occupational fatalities, which I discuss more fully in a subsequent section where I identify possible weaknesses in his methodology and relevant criticisms of his overall findings on unionization and workplace safety. For now, suffice it to mention that his paper is published as a short report, and his control variables may not account for all of the time-varying differences between states that affect occupational fatalities.

Potential explanations for the positive relationship between unionization and nonfatal injuries

Why is unionization often associated with increased nonfatal occupational injuries and illnesses? In my view, the most compelling explanation is that unionized employees are more likely to report their injuries. Reporting is the most commonly cited explanation in the literature (Donado, 2015). The first key piece of evidence supporting the reporting hypothesis comes from the relationship between unionization and occupational fatalities. Most of the literature finds that unionization is associated with lower fatalities, and fatalities are much less likely to be misreported. If there is an underlying "workplace safety level" for establishments, which equally affects employees' susceptibility to both nonfatal injuries (the true number) and occupational fatalities, then occupational fatalities should arguably be a truer measure of this "workplace safety level" since they are less likely to be misreported.

Another key reason why the reporting explanation is compelling is that there is good reason to believe that nonfatal injuries are less likely to be reported in nonunion workplaces. Employees covered by union agreements may be more willing to report injuries to their employers since unions can protect them from backlash. Additionally, unions might better monitor the reporting of injuries by employers (Donado, 2015). Unionized employees also have greater access to paid sick days: 92% of employees covered by a union contract have access to paid sick days, compared with 77% of nonunion workers.[38] Employees are more likely to take off work (and in doing so report their injuries) if they have paid sick leave.

Recall that there is a waiting period to receive workers' compensation benefits from 3-7 days that varies across states. If an employee has an injury that takes less than the waiting period to recover, if an employee has access to paid sick leave, they may be more likely to report their injury to their employer and take a couple days off from work. Having paid sick leave may also make an employee more likely to report an injury and seek workers' compensation, as they can use their paid sick leave in the meantime. Further, unionized employees may be more likely to report their injury and seek workers' compensation because their union could potentially help hold the employer accountable if their workers' compensation claim is unfairly rejected.

[38] https://www.bls.gov/news.release/ebs2.t06.htm as cited in "Unionization Increased by 200,000 in 2022: Tens of Millions More Wanted to Join a Union, but Couldn't." Economic Policy Institute, https://www.epi.org/publication/unionization-2022/. Accessed 6 Apr. 2023.

Unions and OSHA enforcement

The seminal paper in the literature related to unionization and OSHA enforcement comes from Weil (1991). He examines OSHA enforcement activity in the manufacturing industry at the establishment level, focusing on 1985, and excluding states with a state OSHA program. Weil employs OLS regressions with OSHA activity as the dependent variable, an establishment's union status as the independent variable, and a number of control variables that impact an establishment's likelihood of being subject to OSHA activity. He finds that unionized workplaces are more likely to be inspected by OSHA and to have more intense inspections, more violations cited per assessment, harsher penalties assessed, and fewer successful employer appeals of penalties. In each regression, Weil controls for a number of confounding variables. He claims that, "the union effect cannot be explained as simply the result of union presence in more dangerous workplaces" (Weil,1991, p.30). Weil (1992) performs a nearly-identical analysis of the construction industry, similarly finding that unionized construction sites experience more frequent and intense OSHA inspections. The author also notes that, "union employers are required to correct violations of safety and health standards more quickly and bear higher overall penalty costs than their nonunion counterparts" (p.121).

Based on a review of the literature, we expect that RTW laws increase OSHA enforcement. The mechanism through which unionization improves OSHA enforcement is fairly straightforward, and is outlined in Weil (1991) and others. Unionized workers likely have improved information concerning health and safety risks in their workplaces and a better understanding of their rights under OSHA, and unions can protect employees from retaliation in response to an OSHA complaint. Thus, unionized workers are more likely to initiate complaints, and unions can help ensure inspections are thorough and punishments are sufficiently harsh.

RTW laws and workplace safety

The research most relevant to my analysis comes from Zullo (2011) and Zoorob (2018). Zullo (2011) looks at the construction industry from 2001-2009 to test the impact of unionization and RTW laws on occupational fatalities in the construction industry. He looks at fatalities as a function of a RTW, union density, a number of control variables, and time fixed effects. Zullo finds that unionization is associated with lower occupational fatalities. He does not find a statistically significant independent impact of RTW laws, but his interaction term with union density and RTW laws shows that RTW laws decrease the effect of union density on lowering occupational fatalities. However, Zullo does not use state fixed effects, as no states changed

their RTW status in the period he examines. This may mean that his RTW coefficient and interaction coefficient are the result of pre-existing differences between RTW and non-RTW states.

Zoorob (2018) uses two-way fixed effects to measure the impact of unionization on workplace safety as measured by fatalities. He then examines the impact of RTW laws on workplace safety. He uses a state level panel dataset of occupational fatalities from 1992-2016. Using a two-stage least squares approach, he finds that RTW laws reduce unionization by 2.85% and increase workplace mortality by 14.2%. Zoorob's use of a two-way fixed effects specification in his analysis seems appropriate. But given that this was published in the form of a short report, Zoorob is unable to investigate further what is contributing to the coefficients on RTW and unionization. I also believe there is bias not captured by his control variables. The coefficients on policy liberalism and the proportion of workers employed in trade, transport, and utilities both had a larger coefficient than the coefficient on unionization, and it is possible that better measures of state policy changes and more detailed industry breakdowns would allow these control variables to account for more of the variation between states in fatalities. Further, Zoorob does not control for demographic changes in states. Zoorob also fails to consider the direct impact RTW laws may have on occupational fatality rates.

This is a smaller body of literature than the literature on unionization and workplace safety more broadly. I hope to contribute to both discussions through my analysis.

IV. Research Design and Conceptual Model

Empirical Strategy

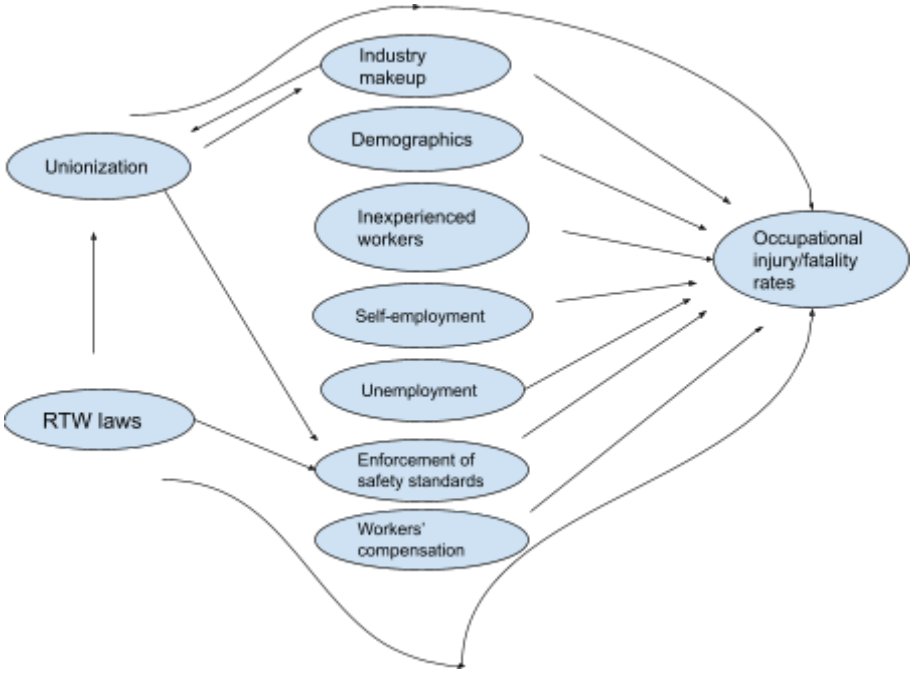
I use two-way fixed effects to examine a large panel data set of state occupational fatality rates and nonfatal occupational injury and illness (NFOIAI) rates from 2007-2019, constructed using mainly BLS data. I first perform two-way fixed effects analyses to examine the impact of right-to-work laws on unionization rates. I then turn to examine the impact of RTW on occupational fatality rates, NFOIAI rates, and NFOIAI with days away from work rates ("severe injuries"). I also examine NFOIAI in the construction industry and the manufacturing industry to mitigate potential industry bias in my larger panel data set. Additionally, I examine severe injuries leading to one or more days away from work that involve a fracture of bone or teeth. I hypothesize that these types of injuries involving fractures are less likely to be misreported by

companies or employees. In all regressions, I use two-way fixed effects regressions with an extensive set of controls. After examining my panel data set, I turn to a difference-in-differences analysis comparing Indiana to Kentucky, and then comparing the other recent RTW states to similar states.

My dataset goes from 2007-2019. The 2018 *Janus v. AFSCME* decision effectively made all public employees covered by RTW laws, so including data from 2018 and 2019 may bias downward my coefficients not focused on the private sector. However, including 2018 and 2019 allows me to get a sufficient post-RTW sample for states like West Virginia (2016 RTW law) and Kentucky (2017 RTW law). My NFOIAI data only includes the private sector, so only my analysis of occupational fatality rates and unionization rates is impacted by the *Janus* decision.

Conceptual Model and Data: Two-way fixed effects

The conceptual model for my two-way fixed effects analysis is:
State workplace safety=F(industry makeup, unionization, demographics, inexperienced workers, self-employment, unemployment, enforcement of workplace safety standards, workers' compensation, and RTW laws). Below is a DAG showing my conceptual model:



My dataset goes from 2007-2019, using annual data for all 50 states with available data. The appendix includes a full list of data sources and data issues.

State workplace safety

The dependent variable in my analysis is workplace safety. To measure workplace safety, I look at occupational fatalities, nonfatal occupational injuries and illnesses (NFOIAI), as well as NFOIAI cases involving days away from work (severe injuries). The rationale behind separately considering occupational fatalities is that NFOIAI data may be prone to reporting bias if unionized employees are more likely to report their injuries (Li et al., 2017).

The fatal occupational injury rate data comes from the BLS Census of Fatal Occupational Injuries.[39] The NFOIAI rates and severe injury rates come from the BLS annual Survey of Occupational Injuries and Illnesses (SOII), compiled by the CDC.[40] How are the estimates in the SOII calculated? BLS selects a representative sample of firms, notifies them of their requirement to maintain records of workplace injuries in the following year, and then collects and processes the data. Each year, approximately 250,000 establishments are selected to participate in the survey.[41] Recordable injuries/illnesses include fall into the following categories: death; days away from work; restricted work or transfer to another job; medical treatment beyond first aid; diagnosis of a significant injury or illness by a physician or other licensed care professional (Wiatrowski, 2014, p. 3). Fatal injury rates exclude occupational fatalities from illnesses. The non-fatal injury survey "excludes all work related fatalities as well as nonfatal work injuries and illnesses to the self-employed; to workers on farms with 10 or fewer employees; to private household workers; to volunteers; and to federal government workers."[42] The NFOIAI rates I use are just for the private sector, so they also exclude local and state government workers. The non-fatal injury and illness rates are presented as an incidence rate of non-fatal injury or illness per 100 full-time equivalent workers. The severe injury rate is presented as an incidence rate per 10,000 full-time equivalent workers. The fatal injury rate data is presented as a rate per 100,000 full-time equivalent workers. BLS uses total hours worked by employees in a state to calculate each state's number of full-time equivalent workers. Employees who are not counted

[39] Fatal injury rates by state, all ownerships, 2007-20. Bureau of Labor Statistics. https://www.bls.gov/iif/state-data/fatal-injury-rates-by-state-2007-2020.htm
[40] CDC - NIOSH Worker Health Charts. https://wwwn.cdc.gov/Niosh-whc/chart/BLS-II?T=ZS&V=C&D=RANGE. Accessed 6 Apr. 2023.
[41] William J. Wiatrowski. "The BLS Survey of Occupational Injuries and Illnesses: A Primer." Bureau of Labor Statistics. https://www.bls.gov/iif/soii_primer.pdf.
[42] "Survey of Occupational Injuries and Illnesses: Concepts." Bureau of Labor Statistics, https://www.bls.gov/opub/hom/soii/concepts.htm.

in a state's NFOIAI rate are similarly excluded in the denominator (full-time equivalent employees) for injury rates.

Non-fatal occupational injury and illness data is not available in most years in my sample for Colorado, Florida, Idaho, Mississippi, New Hampshire, North Dakota, Oklahoma, Rhode Island, and South Dakota. These nine states are excluded from my analysis of non-fatal injuries. For Ohio, my NFOIAI data starts in 2012, and for Pennsylvania, it starts in 2011. Massachusetts is missing NFOIAI data in 2009. All other states have NFOIAI rate data from 2007-2019. All data for severe injuries (NFOIAI cases involving days away from work) starts in 2011.[43] The same 9 states are excluded. For fatal injury rates, all 50 states are included and have data from 2007-2019. The data I use for NFOIAI in the construction and manufacturing industries, as well as the data I use for severe injuries involving fractures, also come from the BLS annual Survey of Occupational Injuries and Illnesses (SOII), compiled by the CDC.[44]

There are limitations to using these measures to capture a state's level of workplace safety. The NFOIAI data is prone to undercounting. Scholars have shown that many injuries that should be reported are not,[45] and the GAO points out that both employees and establishments have incentives to not report injuries/illnesses.[46] The fatal occupational injury numbers should be more reliable, though there can be issues with these as well. Many Americans die from work-related illnesses that are not included in the survey.[47] Another issue is that it can be difficult to determine whether the cause of death or injury/illness was work-related.

Industry

Industry is an important factor in workplace safety. I expect that states with more employees working in dangerous industries will have worse workplace safety. It is evident in the data that occupational fatalities and NFOIAI are concentrated in certain industries.

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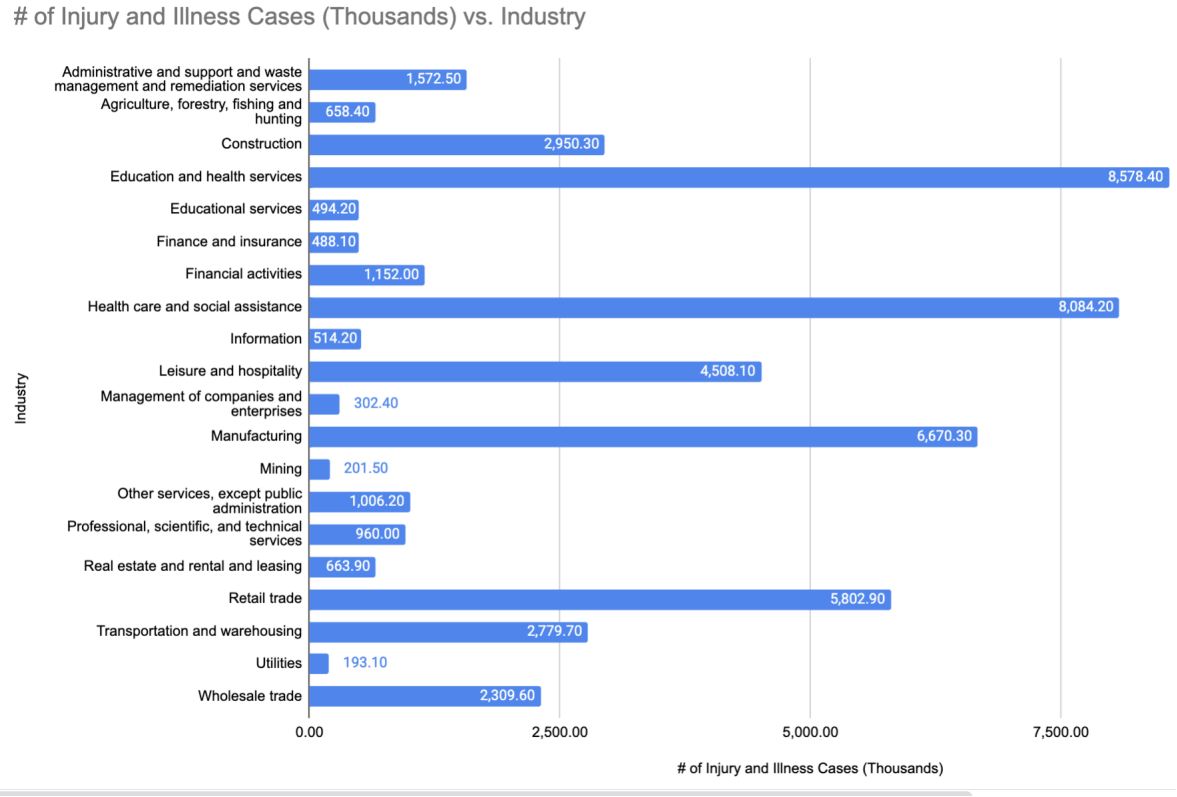
[43] With the exception of Ohio, which starts in 2012.
[44]CDC - NIOSH Worker Health Charts. https://wwwn.cdc.gov/Niosh-whc/chart/BLS-II?T=ZS&V=C&D=RANGE. Accessed 6 Apr. 2023.
[45] "Survey of Occupational Injuries and Illnesses Data Quality and Research." Bureau of Labor Statistics, accessed 3 April 2023, https://www.bls.gov/iif/data-quality-research/data-quality.htm.
[46] U.S. Government Accountability Office. "Occupational Safety and Health: Enhanced OSHA Outreach, Guidance, and Standards Have Improved Safety, but Challenges Remain." 16 Nov. 2009, https://www.gao.gov/products/gao-10-10.
[47] AFL-CIO. "Death on the Job: The Toll of Neglect - 2022." AFL-CIO, 28 Apr. 2022, https://aflcio.org/reports/death-job-toll-neglect-2022.

Below is a chart showing NFOIAI by industry from 2007-2019. Data comes from the BLS and is compiled by the CDC:[48]

Figure 0.3: NFOIAI by industry 2007-2019

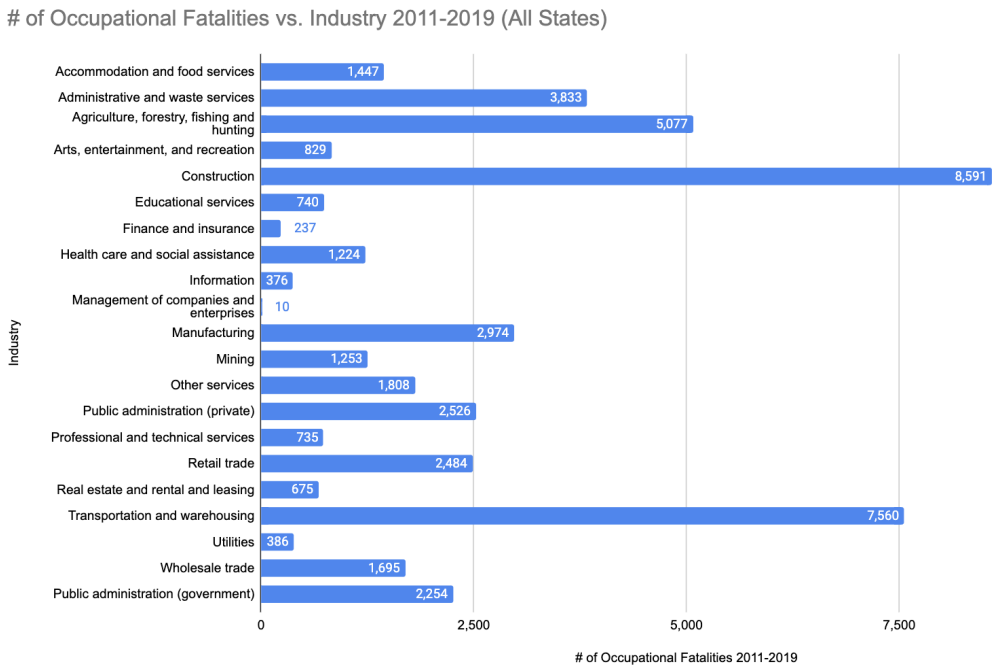


The industries with the most NFOIAI from 2007-2019 were health care and social assistance, manufacturing, retail trade, transportation and warehousing, construction, and wholesale trade.

Below is a chart showing occupational fatalities by industry from 2011-2019. Data comes from BLS and is compiled by the CDC:[49]

[48] CDC, "NIOSH Worker Health Charts" https://wwwn.cdc.gov/Niosh-whc/chart/BLS-II?T=ZS&V=C&D=RANGE
[49] CDC, "NIOSH Worker Health Charts" https://wwwn.cdc.gov/Niosh-whc/chart/BLS-II?T=ZS&V=C&D=RANGE

Figure 0.4: Occupational fatalities by industry



The industries with the most occupational fatalities from 2011-2019 were: construction; transportation and warehousing; agriculture, forestry, fishing and hunting; public administration; and administrative and waste services.

To control for industry in my two-way fixed effects analysis for NFOIAI, I control for the percent of a state's employees who work in the following industries: agriculture, forestry, fishing and hunting; health care and social assistance; leisure and hospitality; manufacturing; retail trade; and transportation and warehousing. These are the industries with an incidence rate of NFOIAI more than 10% above the rate for all industries in 2017.[50] I control for the same industries in my analysis of NFOIAI with days away from work, as the distribution of incidence rates by industry is similar.[51] In analyzing occupational fatalities, I control for the percent of employees that work in the following industries: agriculture, forestry, fishing and hunting; transportation and warehousing; construction; management, administrative, and waste services; mining and logging; and wholesale trade. These industries had over 120 fatalities in 2018 and a fatal injury rate higher than the rate for all industries.[52] I also control for a state's employment in public

[50] Ibid.
[51] Ibid.
[52] Bureau of Labor Statistics. "National Census of Fatal Occupational Injuries in 2018." 17 Dec. 2019, https://www.bls.gov/news.release/archives/cfoi_12172019.pdf.

administration for occupational fatality rates. BLS does not report incidence rates for government industries, but public administration makes up a sizable portion of occupational fatalities. The data for employment by industry comes from BLS Current Employment Statistics and the BLS Quarterly Census of Employment & Wages. Some states are missing employment data for certain years in certain industries. I make assumptions and supplement my data where necessary. I discuss data sources and issues further in the appendix.

Controlling for employment in these industries is far from a perfect measure of the impact of industry on workplace safety. Within these industries, there are certain sub-industries and types of businesses that are much more dangerous than others. States have changes in their industry makeups that impact injury and fatality rates but are not detected by my industry controls. Nor can I control for every industry. Focusing on NFOIAI in the construction and manufacturing industries allows me to mitigate the impact of industry on my analysis of NFOIAI.

Unionization

Unionization is also included in my conceptual model and two-way fixed effects analyses. I hypothesize that unionization has a positive impact on workplace safety. Unions can bargain directly with employers for safer working conditions and there is evidence that unions can improve OSHA enforcement (Sojourner and Yang, 2016; Weil,1991,1992). Still, I expect that unionization will be associated with higher NFOIAI and lower occupational fatalities. This is consistent with Donado (2015)'s review of the literature and my own. The possible association between unionization and higher NFOIAI would likely be attributable to reporting bias.

Unionization data for my regressions comes from the BLS Current Population Survey. Union coverage rather than union membership is used primarily throughout, consistent with Fortin et al. (2022) and others, though I also include union membership in my dataset. I use the percent of a state's employed population covered by a union for my regressions. To fully understand the impact of RTW laws on unionization, I also observe private-sector unionization rates. These also come via the BLS Current Population Survey but are compiled on unionstats.com.[53]
Unionization data for the construction and manufacturing industries also come via the BLS Current Population Survey but are compiled on unionstats.com.[54]

[53] Barry T. Hirsch, David A. Macpherson, and William E. Even. https://www.unionstats.com
[54] Ibid

Demographics

I also include demographic information in my conceptual model. The demographics I control for are gender, education, and age. Men are much more likely to be fatally injured on the job according to BLS data. In 2019, almost 92% of occupational fatalities were men.[55] Men also have a higher incidence rate of nonfatal injuries.[56] Education is included because more educated workers may be less likely to work dangerous jobs and may be better at avoiding occupational injury/death. BLS data shows that workers without a college degree are more prone to occupational injury.[57] Age is included in my conceptual model, as BLS data shows that older workers are much more likely to be fatally injured on the job. Thus, I hypothesize that states with more older workers will have more occupational fatalities. The fatal injury rate for workers aged 65+ (10.3 per 100,000 full-time equivalent (FTE) workers) is much higher than for all workers (3.5 per 100,000 FTE workers). The rate for employees ages 55-64 is also higher than that rate for all workers.[58] The same relationship does not exist for NFOIAI. In fact, there is some evidence that younger workers are more prone to occupational injuries and illnesses.[59] However, this is likely due to younger workers working more dangerous jobs, and after controlling for industry and other demographic information, older workers may actually be more prone to NFOIAI as they are to fatal occupational injuries. As a result, I also include age controls for my analysis of non-fatal occupational injuries and illnesses.

In my two-way fixed effects analysis, to control for demographics, I include control variables for the percent of a state's workforce that has a bachelor's degree or higher and the percent of a state's workforce that is male. For my analysis of occupational fatalities, I control for the percent of a state's workforce that is 65 or older or in the 55-64 age group. For my analyses of NFOIAI, I control for those two age groups as well as the percent of employees who are 16-19 years old and 20-24 years old. My data for age groups comes from the BLS Local Area Unemployment Statistics, and my data for percent of the workforce that is male and for the percent of the workforce with a bachelor's degree or higher comes from the BLS Geographic Profile of

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[55] Bureau of Labor Statistics. "Injuries, Illnesses, and Fatalities." https://www.bls.gov/iif/.
[56] "Bureau of Labor Statistics." U.S. Department of Labor, 4 Nov. 2020, https://www.bls.gov/news.release/archives/osh_11042020.pdf.
[57] Michael R. Pergamit and Parvati Krishnamurty, "Multiyear nonfatal work injury rates," Monthly Labor Review, U.S. Bureau of Labor Statistics, May 2006, https://www.bls.gov/opub/mlr/2006/05/art5full.pdf.
[58]Jenkins, Catherine A. "Fatal occupational injuries to older workers." Monthly Labor Review, U.S. Bureau of Labor Statistics, Dec. 2020, https://www.bls.gov/opub/mlr/2020/article/fatal-occupational-injuries-to-older-workers.htm.
[59] Guerin et al. "Nonfatal Occupational Injuries to Younger Workers — United States, 2012–2018", CDC, September 2020 https://www.cdc.gov/mmwr/volumes/69/wr/mm6935a3.htm#:~:text=177%E2%80%93254

Employment and Unemployment. The data for educational attainment only considers people aged 25+. I am able to control for these demographics fairly easily and effectively, but there are other demographic factors impacting injury/fatality rates that I am unable to control for.

Inexperienced Workers

I also include the number of inexperienced workers in my conceptual model. Workers new to a job may have less training, be less familiar with the hazards of a job, and be less experienced in their lines of work.[60] BLS data shows that inexperienced workers are more prone to occupational injury. I expect that states with more inexperienced workers will have worse workplace safety. Appleton and Baker (1984) suggest that increased job mobility in unionized coal mines led to higher rates of injury. Additionally, Chelius (1974) includes the number of new hires in his analysis of the impact of unionization on occupational injuries.

To control for the number of inexperienced workers in my analysis, I use the number of new hires a state has in a given year as a percentage of total state employment in that year. New hires data comes from BLS via the Job Openings and Labor Turnover Survey.

Self-employment

Self-employment is considered in my conceptual model only for occupational fatalities. Self-employed workers made up over 20% of occupational fatalities in 2019[61] despite comprising around 10% of the workforce.[62] This is not included in my conceptual model for NFOIAI, as self-employed workers are excluded from the nonfatal rates. This data comes from the BLS Geographic Profile of Employment and Unemployment.

Unemployment

Unemployment is also included in my conceptual model. In times of high unemployment, workers may be more willing to accept dangerous jobs. I expect that states with a higher level of unemployment will have higher occupational injury and fatality rates. Chelius (1974) and Loomis et al. (2009) each include unemployment in their analyses. Loomis et al. (2009) note that

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[60] "Managing High Injury Rates." Markel Insurance, accessed April 3, 2023, https://www.markel.com/insurance/insights-and-resources/managing-high-injury-rates.

[61] Bureau of Labor Statistics. “Table A-7. Fatal occupational injuries by selected event or exposure and age, all United States, 2019.” U.S. Department of Labor, 2020, https://www.bls.gov/iif/fatal-injuries-tables/fatal-occupational-injuries-table-a-7-2019.htm.

[62] Victoria Gregory, Elisabeth Harding, Joel Steinberg, "Self-Employment Grows during COVID-19 Pandemic," Federal Reserve Bank of St. Louis, July 05, 2022, https://www.stlouisfed.org/on-the-economy/2022/jul/self-employment-grows-during-covid-19-pandemic.

increased unemployment may reduce union bargaining power by providing employers with a larger pool of employees that they can use to break strikes. Additionally, high unemployment may force employees to work dangerous jobs, and disincentivize them from reporting injuries. To control for unemployment, I use data for annual unemployment rates from the Federal Reserve Bank of St. Louis for each state.

Enforcement of workplace safety standards

I also hypothesize that states with better government enforcement of workplace safety standards will have better workplace safety. This follows from the literature. I expect that states with more economic regulation and workplace safety enforcement will have improved workplace safety.

Zullo (2011) finds that states with a state-run OSHA have lower occupational fatality rates. Currently, there are 22 state plans that cover both private and state and local government employees, and 7 that cover only state and local government employees.[63] A state's level of OSHA enforcement, however, can vary in ways not captured by considering whether a state has an OSHA plan. Since my data for the existence of a state plan does not change over time, I exclude this from my two-way fixed effects analyses.

In some analyses, enforcement of workplace safety standards is included in conceptual and regression models as "policy liberalism." Zoorob (2018) explains that in his two-way fixed effects model, "'policy liberalism' was used to hold constant potential time-varying differences between states in their propensity for economic regulation, which could affect occupational fatalities through workplace safety regulations." (Zoorob, 2018, p.737). Zoorob uses a measure of policy liberalism pulled from the political science literature. Unfortunately this data is only available through 2014. I use the same measure and extend the 2007-2014 trend through the 2015-2019 period. This is imperfect and could be seen as a limitation of my analysis. However, I am unable to find more recent data or a similarly-effective measure of policy liberalism. This measure of policy liberalism comes from Caughey and Warsaw (2016), compiled by Michigan State University's Institute for Public Policy and Social Research.[64] The measure considers 148 state

[63] Occupational Safety and Health Administration. "State Plans." OSHA, United States Department of Labor, https://www.osha.gov/stateplans/.
[64] Caughey, Devin, and Christopher Warshaw. 2016. "The Dynamics of State Policy Liberalism, 1936–2014." American Journal of Political Science 60 (4): 899–913. http://ippsr.msu.edu/public-policy/correlates-state-policy

policies on issues including abortion, social welfare, economic regulation, and more. Right-to-work is also included, so this is a potential source of collinearity. I expect that states with more economic regulation and workplace safety enforcement will have improved workplace safety.

Workers' Compensation

I also include workers' compensation in my conceptual model. There are competing effects from a state having a more generous workers' compensation program. A more generous workers' compensation program incentivizes businesses to spend more on safety precautions and do more to protect workers. More generous benefits, however, also incentivize workers to report more injuries and reduce the incentives for workers to prioritize workplace safety. I expect that a more generous workmen's compensation program is associated with higher NFOIAI rates due to increased reporting, but lower occupational fatality rates due to improved safety. Unfortunately, state differences in workers' compensation programs are virtually impossible to capture with data. Workers' compensation programs are run entirely at the state level. These programs vary, and I am unable to find data that fully measures how generous a state's program is and how it changes over time.

I am unable to track changes over time to this, although I do have data for each states' waiting period to receive workers' compensation benefits from 2015 compiled by the CDC.[65] Since my data for this does not vary over time, it is omitted from my two-way fixed effects analyses. This could be seen as a limitation of my analysis, as changes in workers' compensation programs likely impact workplace safety.

RTW laws

Lastly, RTW is included in my conceptual model. RTW laws make it harder to unionize and harder for unions to operate, and thus are likely to decrease unionization rates and union bargaining power. Much of the literature has found that RTW laws substantially decrease union threat effects (Fortin et al., 2022). If unions do, in fact, protect workers, I expect that RTW laws will negatively impact workplace safety. This effect may be facilitated through decreasing union density, weakening union bargaining power, and decreasing union threat effects. I expect that RTW laws will increase occupational fatalities, although they may decrease the number of

[65] State Worker Comp Summary Fact Dashboards | NIOSH | CDC. 8 Apr. 2021, https://www.cdc.gov/niosh/topics/workercomp/cwcs/dashboard.html.

reported NFOIAI. I include an indicator variable for whether or not a state has a RTW law in place at some point during the year. Luckily, most states enacted their RTW laws at the beginning of a year. Indiana's RTW law went into effect in March 2012; Michigan's RTW law in March 2013; West Virginia's on July 1, 2016; and Kentucky's in January 2017. The case of Wisconsin is a bit more complicated. Wisconsin passed its private sector RTW law in March of 2015. However, its public sector was already RTW under Act 10, passed in March 2011. Act 10 placed strict restrictions on public sector unions and additionally made them right-to-work.[66] I treat Wisconsin as a RTW state as of 2015. This may bias downward my coefficients for regressions not focused on the private sector, since Wisconsin was affected by a public sector RTW law in 2011. To account for this, I sometimes exclude Wisconsin from my analyses.

Data overview: Panel dataset

Summary Statistics

The below table shows the means of key variables for states that had RTW laws throughout the period of my analysis, states that did *not* have RTW laws at any point in my sample, and states that passed RTW laws during the 2007-2017 period. These states are labeled as "Recent RTW".

Key variables of interest by RTW status

	Always RTW	Never RTW	Recent RTW
NFOIAI Rate	3.23	3.69	3.86
Severe Injury Rate	83.83	118.58	100.93
Occupational Fatality Rate	5.24	3.41	4.69
Rate of "Severe" Fracture	8.7	9.38	10.07
Union Coverage	7.5	15.68	13.15
Union Membership	6.16	14.36	11.97
Percent bachelors or higher	28.99	35.01	28.13

RTW states have higher occupational fatality rates, lower NFOIAI rates, lower severe NFOIAI rates, and less unionization than non-RTW states. States that passed RTW laws during my

[66] "Wisconsin Collective Bargaining." Ballotpedia, https://ballotpedia.org/Wisconsin_collective_bargaining.

sample are in the middle of the the two groups for those variables, with exception of NFOIAI rates (but not severe injury rates), where their mean is higher than both groups. The percentage of employees with a bachelor's degree or higher of education is much lower in always-RTW states, and the states that recently passed RTW laws are similarly educated compared to the always-RTW states.

Right away, we can see something interesting. RTW states have much higher rates of occupational fatalities, which would point to them being more dangerous. However, they have much lower NFOIAI and "severe injury" rates. This is without controls, but the differences between the two groups are clear. Already, this points to reporting bias for NFOIAI, since RTW states have significantly higher occupational fatality rates and significantly lower NFOIAI rates. The rate of severe injuries involving a fracture are much closer between the two groups than for all NFOIAI and for all severe injuries.

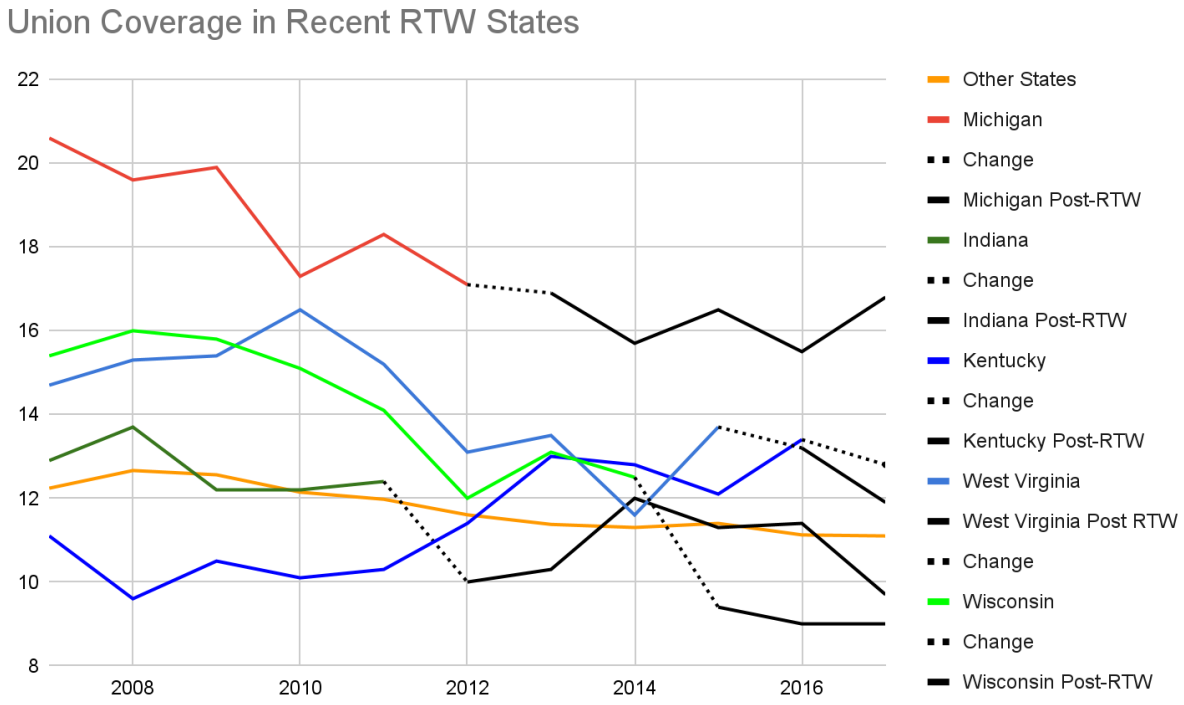
V. Panel data results: Unionization

Unionization

What is the impact of RTW laws on unionization? Union coverage and membership is much lower in RTW states than non-RTW states. Of course, much of this is likely due to underlying differences between states and not RTW laws themselves.

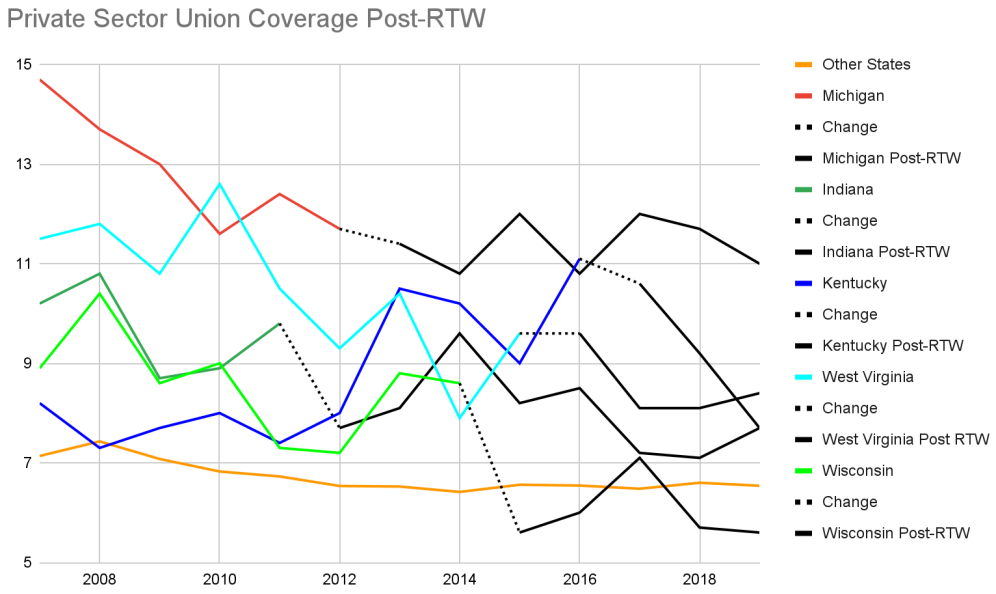
The below graph shows union coverage in states that passed RTW laws during the sample period compared to the average of the other 45 states from 2007-2017. 2018 and 2019 are excluded, as the 2018 *Janus vs. ASFC* decision made public sector employees effectively covered by RTW laws. The year of RTW enactment and onward is shown with a full black line. The change from the year before enactment to the year of RTW enactment is shown with a dotted black line. The average for the other 45 states in each year is shown in orange. Note that my y-axes are condensed to show more detail.

Figure 1.1



The below graph shows the same information but for private sector union coverage. For the private sector, I include 2018 and 2019.

Figure 1.2



Looking at the graphs, it appears as though union coverage decreases relative to the national trend in states that pass RTW laws. That being said, the decrease is not particularly stark, and it varies across states. These graphs also do not consider other factors that can impact unionization. Graphs for union membership are similar, but show a slightly larger effect.

Next, I use two-way fixed effects regressions to estimate the impact of RTW laws on unionization. I use total union coverage/membership and private union coverage/membership as the dependent variables. I control for state employment in local government, state and federal government, transportation and warehousing, construction, and private educational services. These are all high unionization industries.[67] Some other private industries have higher unionization rates but make up a much smaller percentage of total employment, and thus are not included. I include state and year fixed effects in my regressions. In my regressions focused on the private sector, I exclude the controls for government employment. My data for all four regressions runs from 2007-2019 and includes all 50 states. Initially, I had planned to exclude 2018 and 2019 for my regressions not focused solely on the private sector to account for the *Janus* decision. However, including 2018 and 2019 allows me to observe a more complete post RTW-period for states like West Virginia (2016 RTW law) and Kentucky (2017 RTW law). Additionally, including those two years actually slightly increases my coefficients on RTW, and results for those two years should be an underestimate of the true effect of RTW.

[67]Union Members-2019, Bureau of Labor Statistics, 22 Jan. 2020, https://www.bls.gov/news.release/archives/union2_01222020.pdf.

Table 1.3: Two-way fixed effects for the impact of RTW on unionization

VARIABLES	Union Cov	Union Mem	Private Cov	Private Mem
Right-to-work	-2.062***	-2.213***	-1.149***	-1.205***
	(-3.081)	(-3.416)	(-3.011)	(-4.044)
Transp. and warehousing	0.451	0.253	0.0585	-0.159
	(1.218)	(0.753)	(0.221)	(-0.683)
Construction	0.00402	0.0535	0.178	0.224*
	(0.0215)	(0.314)	(1.200)	(1.708)
Private educ. Services	-0.0495	0.0258	0.248	0.493
	(-0.0819)	(0.0491)	(0.558)	(1.328)
Local Government	-0.488*	-0.381		
	(-1.772)	(-1.442)		
Other Government	0.415	0.471*		
	(1.423)	(1.752)		
Constant	14.17***	11.71***	6.352***	5.661***
	(5.132)	(4.559)	(6.048)	(6.033)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	650	650	650	650
R-squared	0.337	0.344	0.195	0.241
Number of state1	50	50	50	50
r2_o	0.309	0.321	0.267	0.179
r2_b	0.311	0.324	0.304	0.177
r2_w	0.337	0.344	0.195	0.241
F	6.197	6.627	3.536	4.262
F_f	.	.	.	.
rho	0.950	0.957	0.930	0.940
Robust t-statistics in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

The coefficient on right-to-work is the coefficient of interest. My results suggest that the passage of a RTW law is associated with a sizable and statistically significant decrease in total union membership and coverage rates. My model suggests that a RTW law decreases a state's union coverage rate by about 2.1% and its union membership rate by about 2.2%. This is quite sizable. The union coverage coefficient is about 17% of the mean union coverage (11.83) in my sample, and the union membership coefficient is about 21% of the mean union membership (10.5). Both coefficients are statistically significant at the 5% level. For private union membership and coverage, my results suggest that the passage of a RTW law is associated with a sizable and statistically significant decrease of just over 1%. Each coefficient is more than

16% of the mean private union coverage (6.99) and private union membership (6.23) in my sample. My result is consistent when comparing states that switched RTW status to never-RTW states, always-RTW states, and other recent RTW states.

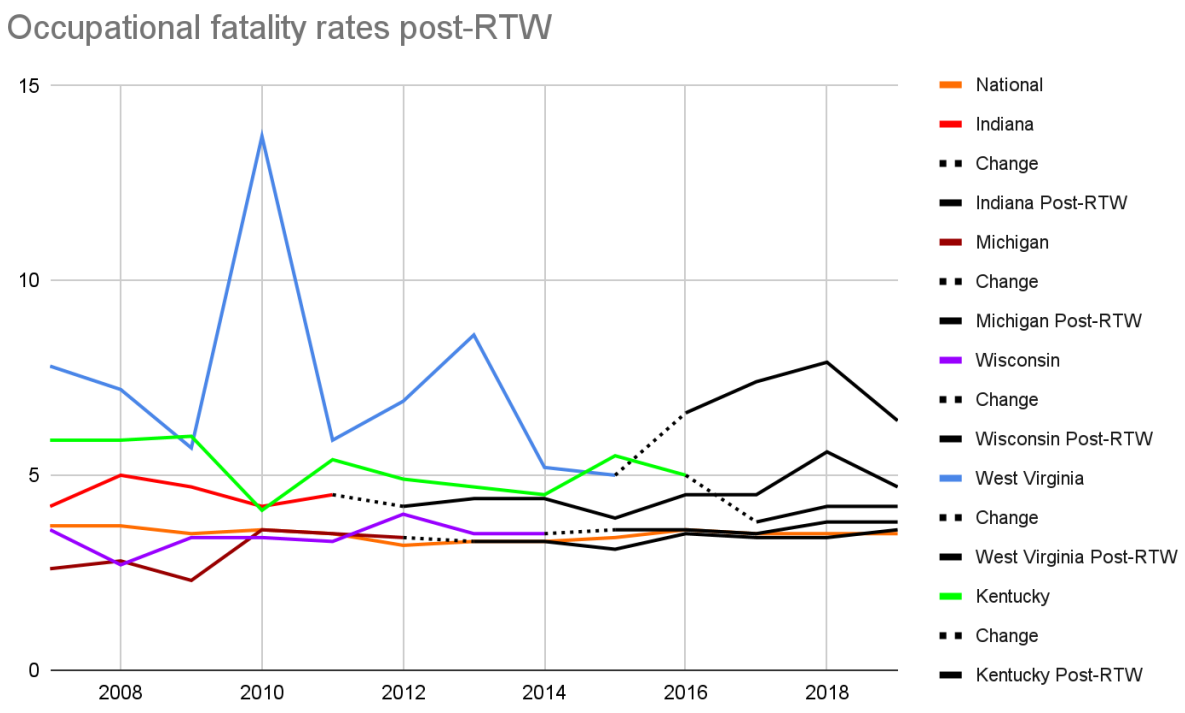
If we exclude Wisconsin in the first two regressions (since they had public sector RTW in 2011 under act 10), my RTW coefficient decreases in size. The coefficient becomes -1.4 for union coverage and -1.56 for union membership, maintaining statistical significance in both. However, my preferred specification does include Wisconsin. If anything, we should think of their coefficient as an underestimate, since they had 4 years of public sector RTW and restriction before their 2015 RTW law. I can exclude any of the 5 recent RTW states and my result does not change significantly.

One note of caution is that there are significant pre-trends for RTW states, meaning that their union coverage rates were decreasing faster than other states pre-RTW. However, looking at Figures 1.1 and 1.2, it appears that union coverage had leveled out or begun to increase in the recent RTW states prior to their RTW laws.

VI. Panel data results: Occupational fatality rates

What is the impact of RTW laws on fatal occupational injuries? The below graph shows occupational fatalities in states that passed RTW laws during the sample period and national trends during that time.

Figure 2.1



Looking at the graph, it is difficult to decipher an impact of RTW laws on occupational fatality rates in the states that recently passed RTW laws. However, once again, this is without control variables for other factors that may impact fatality rates, and is comparing these states to national trends rather than to similar states.

Two-way fixed effects for occupational fatalities

Here, I use two-way fixed effects to examine the impact of RTW laws and unionization on occupational fatality rates. I control for employment in agriculture, forestry, fishing and hunting; transportation and warehousing; construction; management, administrative, and waste services; mining and logging; wholesale trade; and public administration. I also control for the percent of a state's workforce that is male, has a bachelor's degree or higher, is self-employed, is age 65+, is age 55-64, a state's unemployment rate, policy liberalism, and new hires as a percent of total employment. I include state and year fixed effects. My dataset goes from 2007-2019 and includes all 50 states. Including 2018 and 2019 means that my results may be a slight underestimate because of the Janus decision in 2018, which made all public sector employees RTW. However, including 2018 and 2019 allows me to get a larger post-RTW period for states like West Virginia (2016 RTW law) and Kentucky (2017 RTW law). Additionally, the majority of

occupational fatalities occur in private industry. In 2019, 92% of occupational fatalities occurred in private industry.[68] Model 1 includes RTW but not union coverage, model 2 includes union coverage but not RTW, and model 3 includes both union coverage and RTW. Allowing RTW and union coverage to interact yields a tiny and insignificant coefficient, and thus I do not include it in my results table.

Table 2.2: Two-way fixed effects results for occupational fatality rates

VARIABLES	Fatality Rate	Fatality Rate	Fatality Rate
Right-to-work	0.237		0.153
	(0.884)		(0.445)
Union Coverage		-0.0477	-0.0419
		(-0.973)	(-0.711)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	650	650	650
R-squared	0.220	0.221	0.221
Number of state1	50	50	50
r2_o	0.353	0.357	0.359
r2_b	0.382	0.386	0.389
r2_w	0.220	0.221	0.221
F	24.42	25.84	24.81
F_f	.	.	.
rho	0.767	0.766	0.765
Robust t-statistics in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

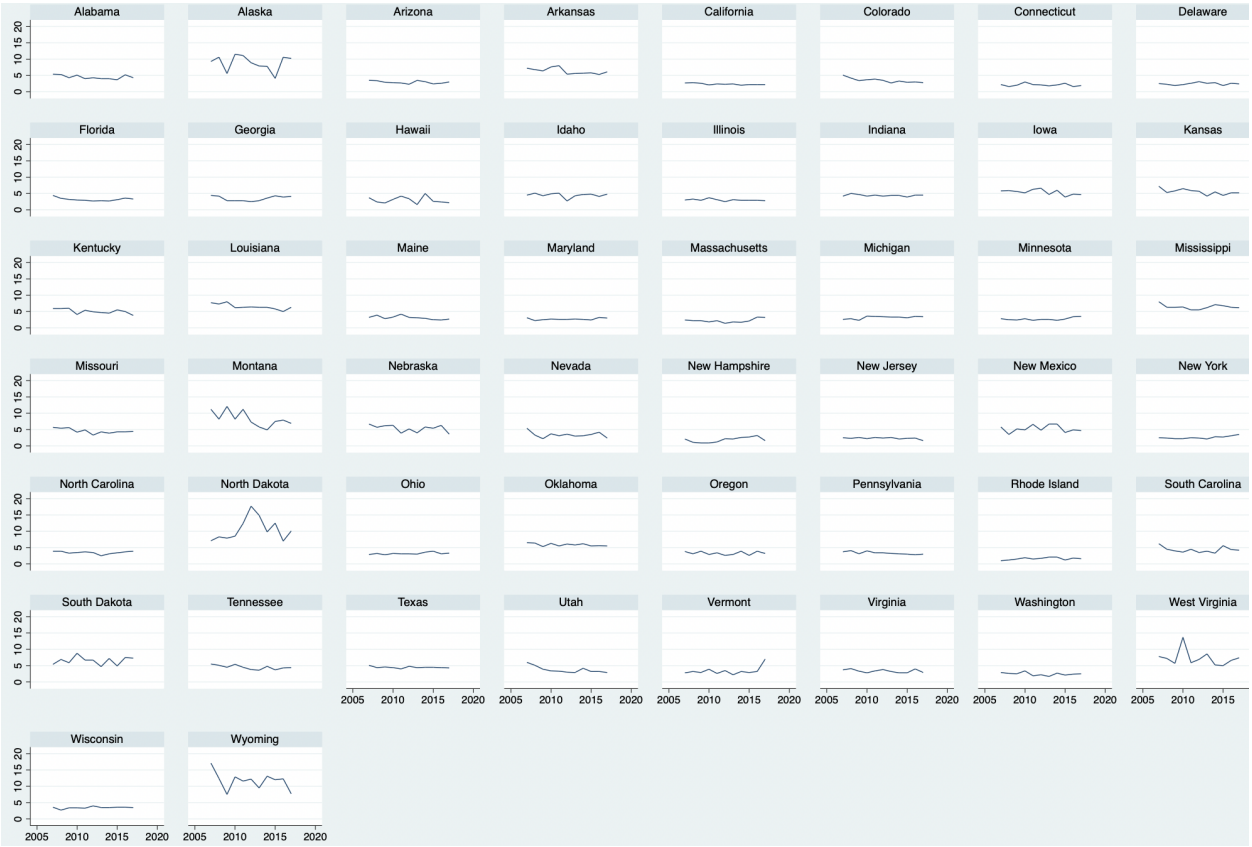
Model 1, which includes RTW but not union coverage, predicts that the passage of a RTW law leads to an increase in a state's occupational fatality rate of 0.237. This is about 5.5% of the mean occupational fatality rate (4.34). This coefficient is not statistically significant, though it does have a p-value of 0.381. In model 2, which includes union coverage but not RTW, the coefficient on union coverage is negative but statistically insignificant. This negative coefficient is consistent with RTW increasing occupational fatalities. Model 3 includes both RTW and union coverage, and in model 3 the right-to-work coefficient decreases to 0.153. These results

[68] National Census of Fatal Occupational Injuries in 2019. Bureau of Labor Statistics, 16 Dec. 2020, https://www.bls.gov/news.release/archives/cfoi_12162020.pdf.

suggest that RTW laws increase occupational fatalities, but none of my coefficients of interest are statistically significant.

An area of concern for my data is that much of the variation in occupational fatality rates is concentrated in a small number of states. Below is a graph that shows occupational fatality rates over time for all 50 states:

Figure 2.3: Occupational fatality rates over time



Most states have occupational fatality rates that are stable over time. There are a few states, however, with highly-volatile occupational fatality rates: Alaska, Montana, North Dakota, West Virginia, and Wyoming. These states generally have high occupational fatality rates. Occupational fatality rates are more volatile than non-fatal injury rates, because a single deadly accident or a deadly accident avoided can make a big impact on occupational fatality rates. For example, the huge spike in occupational fatality rates in West Virginia for 2010 was the result of

a single deadly mining accident discussed earlier.[69] Because all of the variation is concentrated in these five states, they are having an undue impact on my results and biasing my coefficients.

Two-way fixed effects excluding volatile states

If I exclude these 5 volatile states from my analysis (also excluding West Virginia, one of the states that passed a RTW law during my analysis), my results change significantly. I use the same controls as before and include state and year fixed effects. Once again, my regression runs from 2007-2019. Shown below:

Table 2.4: Two-way fixed effects for occupational fatality rates, excluding volatile states

VARIABLES	Fatalities	Fatalities	Fatalities
Right-to-work	0.233		0.0865
	(1.281)		(0.468)
Union Coverage		-0.0704***	-0.0669**
		(-2.882)	(-2.655)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	585	585	585
R-squared	0.213	0.222	0.222
Number of state1	45	45	45
r2_o	0.0505	0.0846	0.0859
r2_b	0.0408	0.0781	0.0798
r2_w	0.213	0.222	0.222
F	9.769	10.59	11.83
F_f	.	.	.
rho	0.872	0.872	0.873
Robust t-statistics in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

For model 1, which includes RTW but not union coverage, the coefficient on RTW is 0.233, with a p-value of 0.207. Interestingly, for model 2, my coefficient on union coverage is now negative and statistically significant! It is also sizable. The mean occupational fatality rate in my sample is 4.33. The coefficient on union coverage in model 2 implies that a 1 unit increase in union coverage reduces occupational fatality rates by 1.62%. For model 3, we can think of the effect of

[69] "Upper Big Branch Mine Disaster." Wikipedia, 8 Feb. 2023. Wikipedia, https://en.wikipedia.org/w/index.php?title=Upper_Big_Branch_Mine_disaster&oldid=1138108838.

RTW on occupational fatality rates as the coefficient on RTW itself in model 3 plus (the decrease in union coverage associated with RTW* the coefficient on union coverage in model 3). That yields an average treatment effect of +0.224 deaths per 100,000 employees. This is my preferred specification, since both RTW and union coverage should impact fatality rates. That is about 5% of the mean occupational fatality rate in our sample (4.33). And, our finding for union coverage is statistically significant.

In a comparison across never RTW, always RTW, and amongst the 4 states that switched RTW (excluding West Virginia due to volatility), my union coverage coefficient is consistently negative. My results are not sensitive to the exclusion of Wisconsin.

If I include West Virginia, but continue excluding the other 4 volatile states, my results differ from Table 4.4 in that the RTW coefficient increases in size in models 1 and 3, and the union coverage coefficient decreases in size and loses statistical significance in models 2 and 3 (although it remains negative).

For a placebo test of my result on RTW in model 1, I assign RTW treatment to a similar state to the recent RTW states that never had a RTW law during my sample. I assign RTW treatment to Minnesota (similar to Wisconsin), Illinois (similar to Indiana), Ohio (similar to Michigan), Missouri (similar to Kentucky), and Pennsylvania (similar to West Virginia). For each of these states, I treat them as if they received RTW treatment at the same time as the similar state. I exclude the recent-RTW states entirely, and unsurprisingly my placebo RTW coefficient is essentially 0.

Midwest States

I can also restrict my sample to just the midwest in order to focus on similar states. This sample includes three states that passed RTW laws during my sample period (Wisconsin, Indiana, and Michigan.) I include the same set of controls. I exclude North Dakota due to volatility, but my results are largely unchanged if I include the state. A two-way fixed effect regression that includes RTW but not union coverage yields a very large and statistically significant coefficient of 0.827. One that includes just union coverage yields a union coverage coefficient of -0.114, statistically significant and the 5% level. So, my estimates of the impact of RTW actually increase.

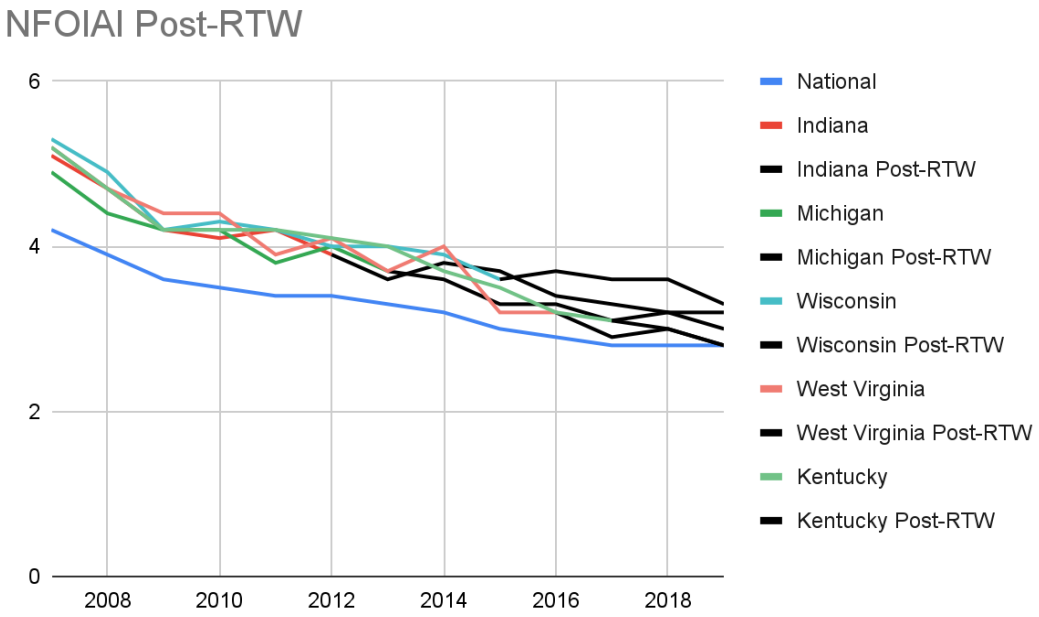
Conclusions for occupational fatality rates

Based on my two-way fixed effects results, I have sufficient evidence to conclude that RTW laws increase occupational fatalities. I believe it is valid to exclude the volatile states, since they are biasing my results. Under that specification, I find that union coverage decreases occupational fatalities, and we know that RTW decreases union coverage. This is also consistent with my positive coefficient on RTW. My preferred estimate for the average treatment effect of RTW laws on fatality rates, using both union coverage and RTW, is 0.22, which is about 5% of the mean occupational fatality rate in my sample.

VIII. Panel data results: Nonfatal occupational injuries and illnesses

What is the impact of RTW laws on nonfatal occupational injuries and illnesses? Non-fatal occupational injuries and illnesses rates are on average slightly lower in RTW states. The mean NFOIAI rate per 100 employees was 3.23 in always-RTW states, 3.69 in never-RTW states, and 3.86 in states that switched RTW status during my sample. The below graph shows NFOIAI rates in states that passed RTW laws during the sample period and national trends during that time:

Figure 3.1



Based on the graph, it does not appear that nonfatal injuries and illnesses decreased or increased significantly relative to the national trend. However, this is without controls, and this is comparing these states to the national trend rather than to similar states.

Regression results: Non-fatal occupational injuries and illnesses

My regression results suggest that RTW laws decrease NFOIAI. Results are shown below. My control variables are the percent employed in: agriculture, forestry, fishing and hunting; health care and social assistance, leisure and hospitality, manufacturing, retail trade, and transportation and warehousing; percent of employees who are male; percent of employees with a bachelor's degree or higher of educational attainment; new hires as a percent of total employment; policy liberalism; unemployment rate; and percent of the workforce aged 65+, 55-64, 16-19, and 20-24. I also include state and year fixed effects. My analysis goes from 2007-2019. These rates are for private industry, so the *Janus* decision in 2018 does not bias my results. The 9 states in which I am missing data for most years are excluded.[70] Results are shown below. Model 1 includes RTW but not union coverage, model 2 includes union coverage but not RTW, and model 3 includes both union coverage and RTW.

[70] These 9 states are: Colorado, Florida, Idaho, Mississippi, New Hampshire, North Dakota, Oklahoma, Rhode Island, and South Dakota. For Ohio, my NFOIAI data starts in 2012, and for Pennsylvania, it starts in 2011. Massachusetts is missing NFOIAI data in 2009.

Table 3.2: Two-way fixed effects for the impact of RTW and unionization on NFOIAI

	(1)	(2)	(3)
VARIABLES	NFOIAI	NFOIAI	NFOIAI
Right-to-work	-0.222**		-0.239**
	(-2.342)		(-2.563)
Union Coverage		-0.000365	-0.00999
		(-0.0375)	(-0.920)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	523	523	523
R-squared	0.881	0.877	0.882
Number of state1	41	41	41
r2_o	0.162	0.115	0.148
r2_b	0.000138	0.00561	0.00389
r2_w	0.881	0.877	0.882
F	148.5	154.1	152.9
F_f	.	.	.
rho	0.952	0.956	0.953
Robust t-statistics in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

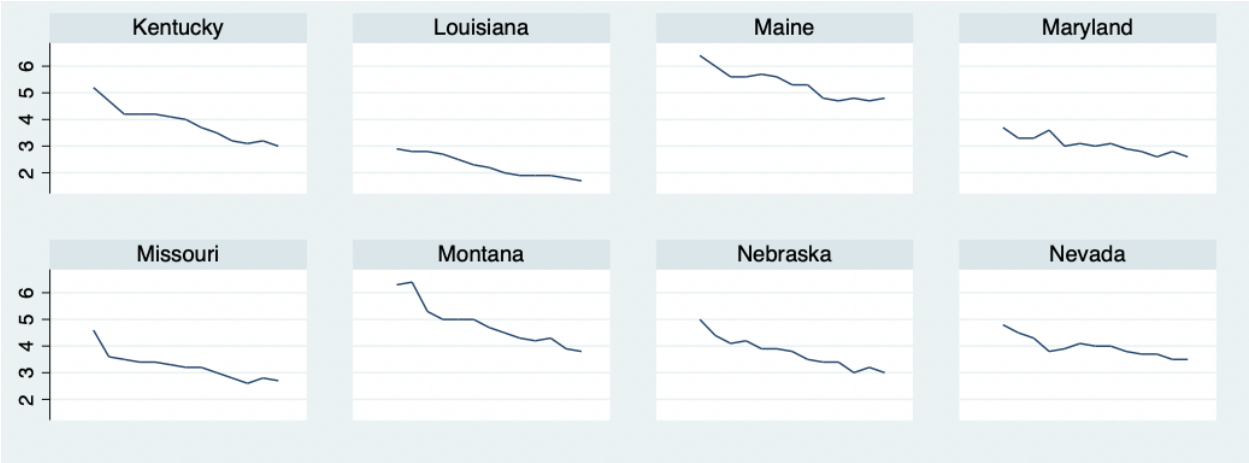
Model 1, which includes RTW but not union coverage, predicts that the passage of a RTW law leads to a decrease in a state's NFOIAI rate of -0.22. This is about 6% of the mean NFOIAI rate in my sample of 3.53. This value is statistically significant at the 5% level. In model 2, which includes union coverage but not RTW, the coefficient on union coverage is tiny and insignificant. Model 3 includes both, and in model 3 the right-to-work coefficient increases in size to -0.239. Allowing RTW and union coverage to interact yields a tiny and insignificant coefficient. My results suggest that RTW decreases non-fatal injuries. My RTW coefficient is consistently large and negative across comparisons to always-treated and never-treated groups, although it is much smaller (-0.045) for a comparison amongst only the 5 states that changed RTW status. I can remove any of the 5 states that switched RTW status during my sample, and my results are

largely unchanged. Model 1 is my preferred specification since my estimate for the impact of union coverage is so small and imprecise.

Data issues for two-way fixed effects

Below is a graph showing NFOIAI by state. I only include a random few states, since including all states makes the issue difficult to see:

Figure 3.3: NFOIAI by state



The issue is, states that start with a high level of NFOIAI have steeper downward trends and vice versa. It is essentially as if states have a lower bound for NFOIAI rates that they converge on over time. In 2007, the mean NFOIAI rate was 4.56, with a standard deviation of 0.9 and a max of 6.4. In 2017, the mean NFOIAI rate was 3.05, with a standard deviation of 0.664 and a max of 4.8. The FE model predicts that NFOIAI in each state will decrease by a set amount in each year based on the overall trend. To the model, states with a more slight downward trend will have higher NFOIAI rates relative to the model's prediction after controlling for the year fixed effects. The problem is, states with slighter trends have much lower NFOIAI rates to begin with. Although the trends are informative, this may be a bit misleading. I use two strategies to account for this. First, I exclude the states that started with a 2007 NFOIAI rate more than one standard deviation above or below the 2007 mean NFOIAI rate (the states that switched RTW status during my sample are in the middle group). My RTW coefficient remains negative and statistically significant, and it actually increases in size. I also restrict my sample to the 2011-2017 period, since the differing trends issue is most pronounced in the earlier years. My RTW coefficient remains negative and statistically significant. My negative and statistically

significant RTW coefficient also persists if I drop different control variables, drop the time trends, or use union membership rather than union coverage.

Placebo test

For my first placebo test of my results, I assign RTW treatment to a similar state to the recent RTW states that never had a RTW law during my sample. I assign RTW treatment to Minnesota (similar to Wisconsin), Illinois (similar to Indiana), Pennsylvania (similar to Michigan), Missouri (similar to Kentucky), and Ohio (similar to West Virginia). I use Ohio for West Virginia rather than Michigan because Ohio's data starts in 2012, and Michigan's RTW law was enacted in 2013. For each of these states, I treat them as if they received RTW treatment at the same time as the similar state. I exclude the recent-RTW states entirely, and unsurprisingly my placebo RTW coefficient is essentially 0.

I am still skeptical of my result for RTW, as I believe it may reflect differing trends for states that start with a high level of NFOIAI. After all, the states that switched RTW status are all significantly above the mean NFOIAI rate in 2007. The mean NFOIAI rate in 2007 for my total sample is 4.56, while the mean for the five recent-RTW states is 5.14. The lowest amongst those is Michigan at 4.9. I assign a placebo RTW treatment to states with a NFOIAI rate in 2007 within 0.2 of a recent RTW state in 2007, for each recent RTW state. I treat Nebraska as Michigan, Utah as Indiana, Kansas as Kentucky, Oregon as West Virginia, and Iowa as Wisconsin. Surprisingly, I get a coefficient on my placebo (including the placebo RTW treatment but not unionization) of -0.8 with a p-value of 0.337. This is a bit concerning, and suggests that my result may be driven in small part by my recent-RTW states starting with a high level of NFOIAI. Yet when I compare only the recent-RTW states to the placebo states, using the real RTW treatment, I still have a negative RTW coefficient of around -0.1, almost statistically significant at the 10% level. The same is not true if I compare the recent-RTW states to the placebo states using the fake RTW treatment, treating the real RTW states as if they never received RTW treatment. That coefficient is actually positive. If I add in some more states with NFOIAI rates in 2007 similarly high to my recent RTW states in 2007 (those states are: Connecticut, Nevada, Alaska, and Vermont), my result moves closer to my original result. In that regression for those 14 states (placebo states, recent RTW states, and the 4 states just discussed) including the real RTW treatment, controls, and state and year fixed effects, but without union coverage, my coefficient on RTW is -0.138, with a p-value of 0.106. If we add in

union coverage, my coefficient increases in size to -0.135. The coefficient on union coverage is still very small and statistically insignificant.

So, although my second placebo test was concerning on its face, I do not believe that my negative coefficient on RTW is driven by the recent-RTW states starting with a relatively-high level of NFOIAI. I find that my original negative coefficient is legitimate.

Given my results for RTW, it is puzzling that my coefficient on union coverage is essentially 0 in my regressions. The impact of right-to-work laws is mediated through unions, so if unions truly have no impact on NFOIAI rates, then there is no reason why RTW laws should impact NFOIAI rates. To accept my finding on the impact of RTW laws on NFOIAI rates, we must ignore the null finding on the impact of unions on injury rates. Because RTW acutely affects unions, it is plausible that RTW illuminates an effect of unions on injury rates that our model cannot observe due to noise in the data. If we use RTW as a proxy, our results suggest that unions are associated with increased reporting of non-fatal occupational injuries and illnesses. This is consistent with the literature.

My results suggest that RTW laws decrease NFOIAI rates by about 6%, although my null coefficient on union coverage is contradictory. I accept my NFOIAI coefficient as legitimate given that it is persistent across so many specifications. However, I believe that this is driven by increased reporting in unionized workplaces, as in my analysis of occupational fatalities, I found that unions protect workers and decrease fatalities.

Specific Industries: Construction and manufacturing

Is this negative coefficient due to state differences in industry makeup? To test this, I focus my analysis on non-fatal occupational injuries and illness (NFOIAI) rates in the construction and manufacturing industries, respectively.

Construction and manufacturing unionization

First, we need to understand the impact of RTW on unionization in these two sectors. My dataset runs from 2007-2019. Below are the results of two-way fixed effects regression for union coverage and membership in construction and manufacturing:

Table 3.4: RTW and Unionization in the manufacturing and construction industries

VARIABLES	Construction Union Coverage	Construction Union Membership	Manufacturing Union Coverage	Manufacturing Union Membership
Right-to-work	-1.945**	-2.507***	-2.740***	-2.917***
	(-2.343)	(-2.864)	(-2.921)	(-3.608)
Constant	15.38***	15.14***	12.56***	11.69***
	(28.32)	(29.30)	(22.29)	(23.06)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	650	650	650	650
R-squared	0.058	0.067	0.125	0.140
Number of state1	50	50	50	50
r2_o	0.239	0.264	0.106	0.127
r2_b	0.355	0.348	0.101	0.124
r2_w	0.0580	0.0671	0.125	0.140
F	3.581	3.292	4.481	5.357
F_f	.	.	.	.
rho	0.894	0.895	0.763	0.778
Robust t-statistics in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

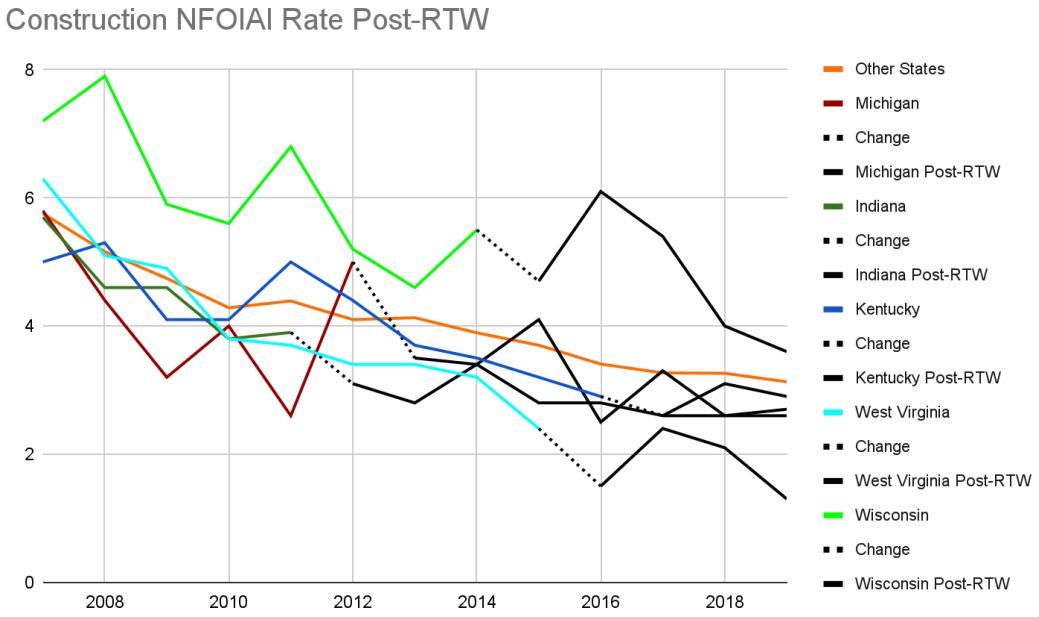
My results suggest that RTW laws decrease construction union coverage rates by about 2. That is around 13% of the mean construction union coverage rate in my sample (14.48). I find that RTW laws reduce construction union membership by about 2.5, which is around 18% of the mean construction union membership rate in my sample (13.78). I find that the passage of a RTW law reduces manufacturing union coverage by about 2.7, which is around 27% of the mean manufacturing union coverage rate in my sample (10.2). Finally, I find that the passage of a RTW law reduces manufacturing union membership by around 2.9, around 31% of the mean manufacturing union membership rate in my sample (9.29). My results are consistent for comparisons to always-RTW, never-RTW states, and comparisons within the 5 recent-RTW states. For construction union coverage, the only one of the 5 states with a positive (although small and statistically insignificant) coefficient is Michigan. For manufacturing union coverage,

the only state with a positive coefficient is Kentucky. I do not need to exclude Wisconsin since this analysis focuses on the private sector. However, doing so does not affect my results.

Construction NFOIAI

How do RTW laws affect NFOIAI rates in construction? Below is a graph showing nonfatal occupational injury and illness rates in the construction industry for recent-RTW states relative to the national trend from 2007-2019.

Figure 3.5: Construction NFOIAI



It appears as though construction injury rates decreased relative to the national trend after a state passed a RTW law, although there is significant volatility in these injury rates.

Two-way fixed effects results: Construction NFOIAI

I run two-way fixed effects regressions to determine the impact of RTW laws on construction NFOIAI rates. I include the same set of controls as before for all NFOIAI, except I exclude the industry controls, as they are irrelevant for this analysis. The same nine states[71] are excluded from my analysis as for all NFOIAI due to missing data, except I am now missing data for Ohio

[71] These 9 states are: Colorado, Florida, Idaho, Mississippi, New Hampshire, North Dakota, Oklahoma, Rhode Island, and South Dakota.

in all years and for New Mexico.[72] Model 1 includes RTW but not unionization. Model 2 includes union coverage but not RTW. Model 3 includes both. My dataset runs from 2007-2019.

Table 3.6: Two-way fixed effects results for construction NFOIAI rates

	(1)	(2)	(3)
VARIABLES	Construction NFOIAI Rate	Construction NFOIAI Rate	Construction NFOIAI Rate
Right-to-work	-0.362**		-0.326**
	(-2.331)		(-2.117)
Con. union cov		0.0273**	0.0262**
		(2.454)	(2.330)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	500	500	500
R-squared	0.671	0.675	0.677
Number of state1	39	39	39
r2_o	0.347	0.319	0.341
r2_b	0.136	0.101	0.135
r2_w	0.671	0.675	0.677
F	51.98	41.32	48.54
F_f	.	.	.
rho	0.781	0.794	0.788
Robust t-statistics in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

The mean construction nonfatal occupational injury and illness rate in my sample is around 4.07. In model 1, my RTW coefficient is negative, large, and statistically significant (about 9% of the mean construction NFOIAI rate). In model 2, which includes union coverage but not RTW, my union coverage coefficient is positive, statistically significant, and no longer essentially 0 in terms of its size. A one and a half unit increase in construction union coverage would increase the construction NFOIAI rate by 1%of the mean according to my model. Model 3 includes both RTW and union coverage, and the coefficients on the two do not change much. The positive

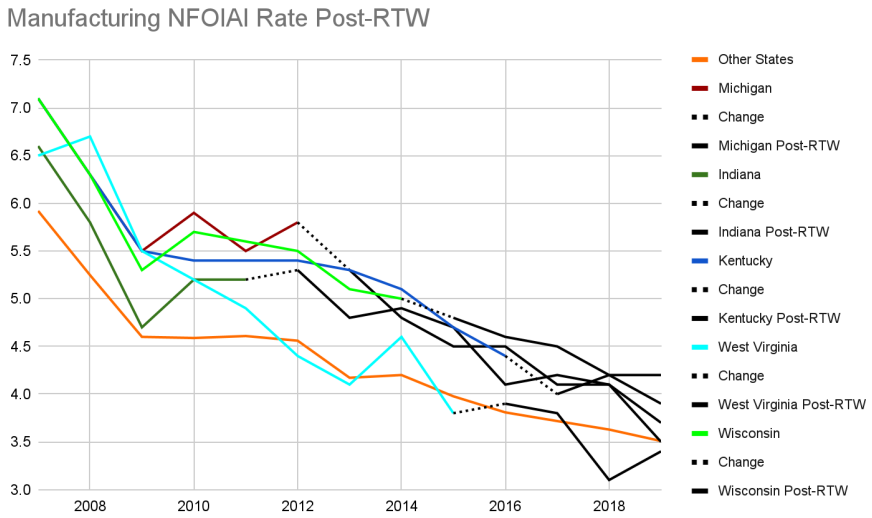
[72] I am also missing data for Arkansas in 2014. My data for Pennsylvania starts in 2011 as it does in my analysis of all industries.

coefficient on union coverage is consistent with RTW decreasing NFOIAI rates (or at least reported NFOIAI). We can use the impact of RTW on construction union coverage to generate an average treatment effect from model 3. I found earlier that RTW laws decrease construction union coverage by 1.95. Multiplying that by the coefficient on union coverage from model 3, and adding the RTW coefficient from model 3, yields an average treatment effect of -0.37709. This is almost 10% of the mean construction NFOIAI rate in my sample. My result of a negative and large impact of RTW on NFOIAI rates is consistent across comparisons with always- and never-RTW states, as well as within the 5 states that switched RTW status, although the coefficient when examining just those 5 states does not reach statistical significance. I find a negative impact of RTW when looking at each of the 5 recent RTW states individually. My construction NFOIAI data does not suffer from the issue of differing trends for states that start with high levels of NFOIAI in the same way my total NFOIAI data does. If I exclude states with high or low construction NFOIAI in 2007 (one standard deviation above or below the mean in 2007), my RTW coefficient remains large and negative, actually increasing in size.

Manufacturing NFOIAI

How do RTW laws affect NFOIAI rates in manufacturing? Below is a graph showing nonfatal occupational injury rates in the construction industry for recent RTW states relative to other states from 2007-2019:

Figure 3.7: Manufacturing NFOIAI 2007-2019



It is hard to observe the relationship here due to the steep drop in manufacturing injury rates from 2007-2009 as well as the consistent downward trend. Still, it does appear as though manufacturing injury rates decrease relative to the trend in other states.

Two-way fixed effects results: Manufacturing NFOIAI

I use two-way fixed effects regressions to determine the impact of RTW laws on manufacturing NFOIAI rates. I include the same set of controls as before for all NFOIAI, except I exclude the industry controls, as they are irrelevant for this analysis. The same 9 states are excluded as for my NFOIAI data for all industries due to a lack of data, except I am also missing data for Ohio in all years.[73] Model 1 includes RTW but not unionization. Model 2 includes union coverage but not RTW. Model 3 includes both. I do not include an interaction term here because the estimate is so far from statistical significance. My analysis runs from 2007-2019.

Table 3.8: Two-way fixed effects results for manufacturing NFOIAI rates

	(1)	(2)	(3)
VARIABLES	Manufacturing Injury Rate	Manufacturing Injury Rate	Manufacturing Injury Rate
Right-to-work	-0.303**		-0.302**
	(-2.211)		(-2.089)
Man. union coverage		0.00401	0.000540
		(0.398)	(0.0523)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	511	511	511
R-squared	0.774	0.770	0.774
Number of state1	40	40	40
r2_o	0.227	0.204	0.228
r2_b	0.0161	0.00193	0.0166
r2_w	0.774	0.770	0.774
F	39.79	33.26	38.92
F_f	.	.	.
rho	0.902	0.904	0.902
Robust t-statistics in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

[73] I am also missing Pennsylvania in 2011 as well as New Mexico and Utah in 2014.

For context, the mean manufacturing NFOIAI rate in my sample was around 4.43. My results suggest that the passage of a RTW law is associated with a large and statistically-significant decrease in injuries and illnesses of -0.303. As in my analysis of NFOIAI in all industries, my union coverage coefficient is essentially 0. In a comparison to never-RTW states my coefficient is -0.38, in a comparison to always-RTW states my coefficient is -0.17, and in a regression with only the 5 recent-RTW states the coefficient decreases significantly in size to -0.05. The graph shows a huge drop in manufacturing NFOIAI rates from 2007-2009, possibly due to the great recession. If I restrict my analysis to 2009 onward, my RTW coefficient decreases slightly in size to -0.256 and loses a bit of precision (p-value of 0.083).

Results for specific industries

These results indicate that my negative coefficient on RTW and NFOIAI for all industries is not being driven by changes in the industry makeup of states, since when we look at individual industries, the negative coefficient remains. This, combined with the positive impact of RTW on occupational fatalities, leads me to believe that the negative coefficient on RTW for NFOIAI is likely being driven by reporting rather than selection.

IX. Severe Injury Rates

When I refer to “severe injuries", I am referring to NFOIAI cases requiring one or more days away from work. I had hoped that these injuries would be less likely to suffer from reporting bias and would thus be a truer measure of workplace safety. My logic was that injuries that require days away from work are likely to be more serious injuries, and they are thus more likely to be reported to employers, and by employers to BLS. However, after looking at the data, that does not seem to be the case. One of the reasons why I theorized unionized employees may be more likely to report injuries is that they have greater access to paid sick leave. 92% of employees covered by a union contract have access to paid sick days, compared with 77% of nonunion workers.[74] If an employee has paid sick leave, they are more likely to take days away from work. So, subject to the same injury, since unionized employees have greater access to paid sick leave, they are more likely to take days away from work. Thus, when analyzing this subset of NFOIAI in the context of RTW/unionization, severe injury results may reflect differences in paid

[74] https://www.bls.gov/news.release/ebs2.t06.htm as cited in “Unionization Increased by 200,000 in 2022: Tens of Millions More Wanted to Join a Union, but Couldn’t.” Economic Policy Institute, https://www.epi.org/publication/unionization-2022/. Accessed 6 Apr. 2023.

sick leave rather than serious injuries unlikely to be subject to reporting bias. Indeed, looking at the summary statistics shown previously:

Table 4.1

	Always RTW	Never RTW	Recent RTW
Occupational Fatality Rate	5.28	3.4	4.67
NFOIAI Rate	3.33	3.79	4
Severe Injury Rate	86.1	120.94	104.59

Always RTW states, which are less unionized, have lower rates of total NFOIAI relative to Never RTW states. Interestingly, Always RTW states have even *lower* severe injuries relative to Never RTW states than for all NFOIAI. Based on the information in the table, I can calculate that a nonfatal occupational injury or illness was 23.4% more likely to require one or more days away from work in a never RTW state. It is unlikely that this disparity reflects more serious injuries occurring in Never RTW states; their occupational fatality rates are much lower than always RTW states. Thus, examining severe injuries does not seem to eliminate reporting bias. Still, I perform two-way fixed effects regressions for thoroughness.

Table 4.2: Two-way fixed effects results for severe injury rates

Here, I use two-way fixed effects to examine the impact of RTW laws and unionization on severe injury rates. My dataset goes from 2011-2019 due to a lack of data pre-2011. This dataset is focused on the private sector. I use the same set of controls as I do for NFOIAI, including year and state fixed effects. Model 1 includes RTW but not union coverage, model 2 includes union coverage but not RTW, and model 3 includes both union coverage and RTW. Results shown below:

Table 4.3: Two-way fixed effects regressions for severe injury rates

	(1)	(2)	(3)
VARIABLES	Severe Injuries	Severe Injuries	Severe Injuries
Right-to-work	-7.097		-6.368
	(-1.420)		(-1.190)
Union coverage		0.715	0.431
		(1.662)	(0.837)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	368	368	368
R-squared	0.604	0.598	0.605
Number of state1	41	41	41
r2_o	0.444	0.505	0.497
r2_b	0.424	0.496	0.484
r2_w	0.604	0.598	0.605
F	13.09	15.14	13.46
F_f	.	.	.
rho	0.908	0.900	0.899
Robust t-statistics in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

The severe injury rates are presented as a rate per 10,000 full-time equivalent employees (the standard NFOIAI data is presented as a rate per 100 employees). The mean severe injury rate for my sample is 102.8. Model 1, which includes RTW but not union coverage, predicts that the passage of a RTW law leads to a decrease in a state's severe injury rate of -7.097. In model 2, which includes union coverage but not RTW, the coefficient on union coverage is positive. This is consistent with RTW decreasing reported severe injuries. In model 3, the RTW coefficient decreases in size to -6.368 and the union coverage coefficient decreases to 0.431. However, none of these coefficients reach statistical significance.

My results at first glance appear to be consistent with RTW laws decreasing severe injury rates, but I am hesitant to accept my result. First, there is a lack of statistical significance for all of my coefficients. More importantly, our negative RTW coefficient is very sensitive. The negative result is being driven almost entirely by West Virginia. If I exclude West Virginia from our analysis, our negative coefficient on RTW decreases significantly in size to -1.4. And there is reason to believe our West Virginia result is spurious, as West Virginia's severe injury rate was decreasing significantly faster than other states even before the state passed a RTW law.

Because my result is being driven by West Virginia, a state with a significantly a starker decrease than other states even before its RTW law, I consider this to be a null finding. I do not have an estimate for the impact of RTW on total severe injuries which I am confident in. Regardless, I do not believe this subset of injuries represent a subset of NFOIAI less prone to reporting bias.

Severe injuries involving fracture

Here, I observe severe injuries involving a fracture. These are injuries requiring one or more days away from work to recover involving a fracture of bones or teeth. I hypothesize that these injuries are less likely to suffer from reporting bias. After all, it is often quite difficult to work through a fracture. And, it is less likely that an employer would not be aware of the fracture and fail to record the injury. However, there is bias introduced by virtue of these being in the severe injury subset of NFOIAI. Subject to the same fracture injury, since unionized employees have greater access to paid sick leave, they are more likely to take days away from work and be included in these rates. Unfortunately, I do not have data for fractures not in the severe injury subset. Comparing always RTW states to never RTW states, without controls, it does appear that reporting bias may be lower for severe injuries involving fracture. Total NFOIAI rates are about 12% lower in always RTW states compared to never RTW states; severe injury involving fracture rates are about 7% lower. Still, the fact that these injury rates are lower in the group of states with higher occupational fatality rates suggests that there still may be reporting bias.
My dataset goes from 2011-2019. I use the same set of controls as for NFOIAI, with new industry controls. To control for industry in this analysis, I control for the percent employed in: agriculture, forestry, fishing and hunting, construction, manufacturing, and transportation and warehousing. Each of these industries had an incidence rate of fractures requiring days away from work above the national rate in 2019 and were responsible for at least 5% of all these injuries, or had an incidence rate more than double the national rate.[75]

Results are shown below. Model 1 includes RTW but not union coverage, model 2 includes union coverage but not right-to-work, and model 3 includes both.

[75] CDC - NIOSH Worker Health Charts. https://wwwn.cdc.gov/Niosh-whc/chart/BLS-II?T=ZS&V=C&D=RANGE. Accessed 6 Apr. 2023.

Table 4.4: Two-way fixed effects regressions for severe injury rates involving a fracture

VARIABLES	Fracture	Fracture	Fracture
Right-to-work	0.460		0.744
	(0.707)		(1.108)
Union Coverage		0.140	0.172
		(1.174)	(1.386)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	368	368	368
R-squared	0.120	0.124	0.127
Number of state1	41	41	41
r2_o	0.0348	0.0339	0.0309
r2_b	0.0440	0.0444	0.0403
r2_w	0.120	0.124	0.127
F	4.477	3.653	3.905
F_f	.	.	.
rho	0.849	0.879	0.892
Robust t-statistics in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

The mean fracture rate in my sample was 9.29 (per 10,000) employees. My results suggest that right-to-work laws lead to a large increase in the rate of fractures involving days away from work of 0.46. However, this value is not statistically significant. The positive coefficient on union coverage in models 2 and 3 is contradictory to my finding for RTW, although the coefficient on union coverage is not statistically significant in either regression. For model 3, if we use our estimated effect of RTW on union coverage (-2.062), we can calculate an average treatment effect of RTW on fracture rates of 0.389, similar to model 1.

However, none of my coefficients are statistically significant. And my coefficient on union coverage is contradictory to my coefficient on RTW. So, I fail to find a coefficient I am confident in. Additionally, these rates are quite jumpy, similarly to occupational fatalities. But unlike occupational fatalities, these jumps are not concentrated in a few states which I can easily exclude.

The fact that I do not find a negative relationship between RTW and fracture rates may provide evidence of the reporting explanation for the puzzling positive coefficient on RTW and NFOIAI, if we accept that fractures are less likely to be misreported in the data. However, I still believe there is reporting bias in these rates, and I do not find a coefficient estimate I am confident in.

X. Difference-in-differences results

For my difference-in-differences analysis, I compare Indiana to Kentucky (in the years before Kentucky's 2017 RTW law). I then turn to compare the other recent RTW states to similar states. However, my data is quite suboptimal for differences-in-differences. I only have access to yearly data, and the data is quite jumpy. Even if two states have parallel trends in the pre-period, it is hard to say that the other similar state represents a true counterfactual for what would have happened in the absence of treatment. Additionally, it is difficult to attribute any differences between states in the post-RTW period to anything other than random variation. Indeed, I find no coefficient estimates with statistical significance when looking at workplace safety outcomes.

Indiana and Kentucky share a border and had a similar level of unionization in 2011, the year before Indiana's March 2012 enactment of a RTW law.[76] Another advantage of this comparison is that Kentucky later passed a RTW law in 2017. This suggests that Kentucky is similar to Indiana in terms of its political sentiment towards unions. Both states had the same waiting period for employees to receive workman's compensation benefits, and both states have a state OSHA plan covering all employees. I ignore severe nonfatal occupational injuries and illnesses in this analysis as my data only goes back to 2011, and thus I am unable to test for parallel trends. For conciseness, I include my results in a table below. To determine if parallel trends exist, I looked at the graph, as regressions do not have enough statistical power to identify differing trends with just 4 years of pre-treatment data for each state. If I report a coefficient, the states appeared to have parallel trends, or the lack of parallel trends does not significantly bias upwards my coefficient (the non-parallel trends are going the opposite direction of the coefficient). One star represents statistical significance at the 10% level, 2 stars the 5% level, and 3 stars the 1% level.

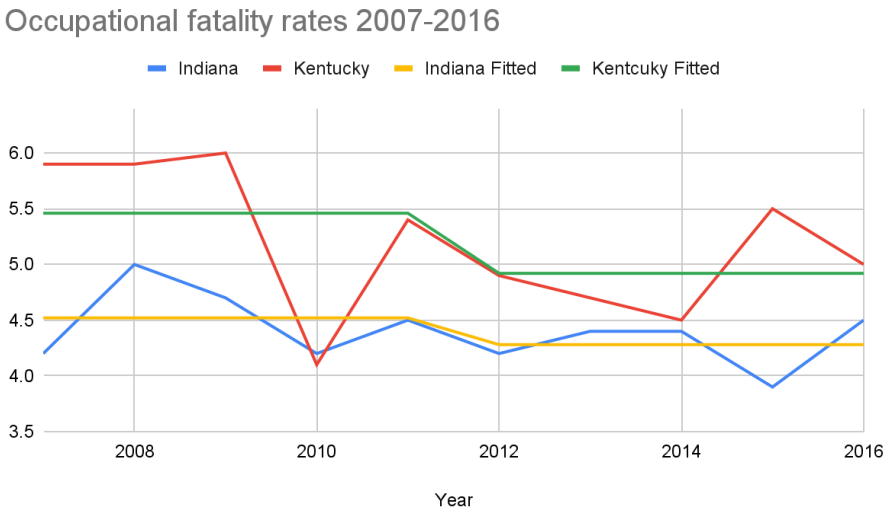
Table 5.1: Difference-in-differences results for Indiana and Kentucky

[76] In Indiana, 12.4% of employees were covered by a union in 2011. In Kentucky, that number was 10.3%. Data from BLS.

Variable	Indiana vs. Kentucky 2007-2016. Coefficient (average treatment effect)
Union coverage	-3.900***
NFOIAI	0.02
Occupational fatality rate	0.3
Construction union coverage	-6.68*
Construction injury rate	No parallel trends
Manufacturing union coverage	-7.26***
Manufacturing injury rate	0.222

These results suggest that Indiana's RTW law significantly decreased unionization in the state relative to Kentucky. However, the only two workplace safety coefficients that suggest a relationship may exist are the 0.3 on the occupational fatality rate and the 0.222 on the manufacturing injury rate. I examine the graphs of each below.

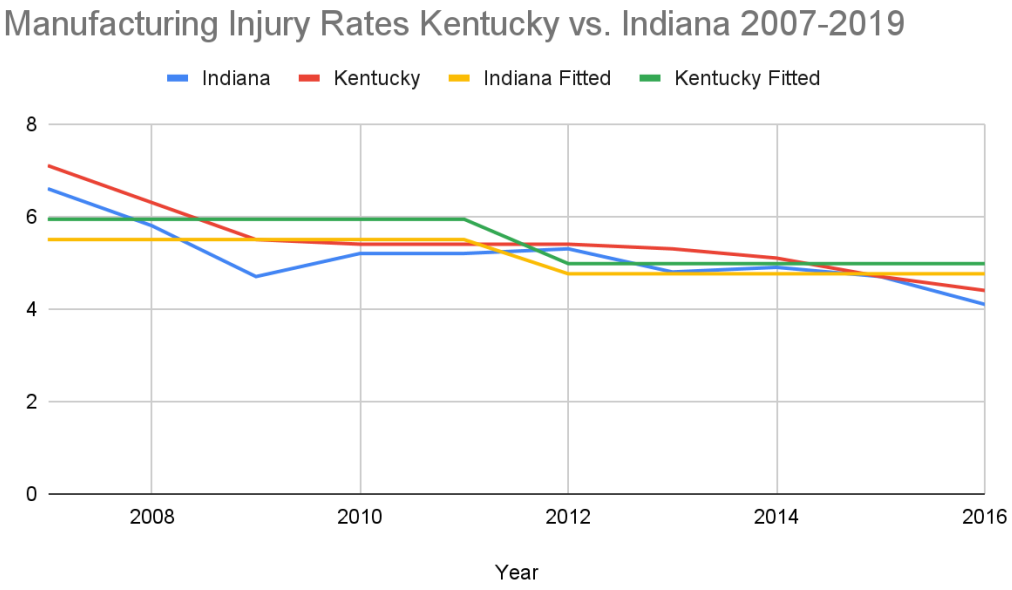
Figure 5.2: Occupational fatality rates after RTW: Kentucky and Indiana



It appears as though Indiana's occupational fatality rate is on a consistent downward trend, whereas Kentucky's occupational fatality rate is more volatile. The volatility in fatality rates for Kentucky is likely the reason for the lack of significance on my coefficient of interest. My results are consistent with RTW laws increasing occupational fatality rates. However, due to the

volatility in Kentucky's occupational fatality rate, my results do not provide strong evidence either way.

Figure 5.3: Manufacturing injury rates after RTW: Kentucky and Indiana



Looking at the graph, it appears that the two states' injury rates track each other pretty closely. Once again, it is hard to draw a definitive conclusion.

Other states

I also run difference-in-differences regressions for other states. I compare Michigan to Ohio from 2007-2017, but only for unionization and occupational fatality rates. For the other variables, I do not have data predating 2011 for Ohio. Since I can't observe pre-trends, I do not perform my analysis for other variables. I compare Wisconsin to Minnesota; Kentucky to Missouri; and West Virginia to Pennsylvania from 2011-2019. Since the Janus decision in 2018 effectively made all public sector employees RTW, including 2018 and 2019 may bias my coefficients towards 0. Nevertheless, I need to include 2018 and 2019 to get sufficient data after these states' RTW laws went into effect. As a reminder, Michigan passed a RTW law in 2013, Wisconsin in 2015, West Virginia in 2016, and Kentucky in 2017. Results are included in a table below. Once again, I look at the graphs to test for parallel trends.

Table 5.4: Differences-in-differences results

Variable	Indiana vs. Kentucky (2007-2016)	Michigan vs. Ohio (2007-2017)	Wisconsin vs. Minnesota (2011-2019)	West Virginia vs. Pennsylvani (2011-2019)	Kentucky vs. Missouri (2011-2019
Union coverage	-3.9	-1.45	-4.01	-0.32	No parallel trends
Occupational fatality rate	0.3	No parallel trends	-0.445	1.125	-1.167
NFOIAI	0.02	N/A	No parallel trends	-0.185	No parallel trends
Con. union coverage	-6.68	N/A	-3.35	No parallel trends	No parallel trends
Con. inj rate	No parallel trends	N/A	-0.18	No parallel trends	-.35
Man. union coverage	-7.26	N/A	-7.52	No parallel trends	No parallel trends
Man inj. rate	0.222	N/A	No parallel trends	0.05	-0.383

Results: Difference-in-differences

In my difference-in-differences analyses, I find very clearly that RTW laws decrease union coverage. However, I am unable to replicate my findings from my panel data set that RTW increases occupational fatality rates and decreases NFOIAI rates. This is surprising to me, but I do not put much stock into it. None of the coefficients for injury or fatality rates were anywhere near statistical significance. These results are also incredibly sensitive. If I move the pre-period forward or back by even just one year, the coefficients change drastically. I am also unable to control for differences between states in my difference-in-differences analysis. And, there is a lack of parallel trends in many of my comparisons. This is telling, as it shows that even in the absence of treatment, variables do not track closely for groups of similar states. In the

comparisons that do have parallel trends, this may be coincidental, since other groups of similar states do not have parallel trends. Thus, it is difficult to say that my comparison states represent a true counterfactual. It is also difficult to say that any differences between states in the post-RTW period is due to anything other than noise in the data, as these analyses are without controls.

We can see from this analysis that there are many other factors other than RTW that affect workplace safety. And just because a state passes a RTW law does not mean it will have a drastic increase or decrease in its fatality/injury rates compared to a similar state that does not pass a RTW law.

XI. Conclusion

Right-to-work laws harm workplace safety. I found in my two-way fixed effects analysis that RTW laws decrease union coverage by 2.062 and decrease union membership by 2.213. In my two-way fixed effects analysis for occupational fatality rates, I find that unionization is associated with a statistically significant decrease in occupational fatality rates. I also get a positive coefficient on RTW itself in regressions on occupational fatality rates when I exclude unionization. A model that includes both unionization and RTW generates an average treatment effect of 0.22 deaths per 100,000 full-time equivalent workers. This is about 5% of the mean occupational fatality rate in my sample. If we use the total employment in each of the 5 recent-RTW states, we find that the recent RTW laws cost 100s of lives every year.

I also find, however, that RTW laws decrease non-fatal occupational injury and illness rates. I am able to look specifically at the construction and manufacturing industries and replicate this finding. Although this seemingly contradicts my overall finding that RTW laws harm workplace safety, it is consistent with the empirical literature, which finds that unionization is associated with higher rates of non-fatal injury and lower occupational fatalities. In any event, I believe my negative coefficient on RTW is being driven by increased reporting in unionized workplaces. Reporting is also the most commonly cited explanation for unionization being associated with more non-fatal injuries in the literature. Indeed, I find that RTW laws increase occupational fatalities, which are unlikely to suffer from reporting bias.

In my difference-in-differences analyses comparing recent RTW states to similar states, I am unable to replicate my finding. But these estimates have no statistical significance and are highly sensitive. Thus, I believe my two-way fixed effects estimates have more validity.

Future research should focus on the source of the potential reporting bias in NFOIAI. Are the increased reports of non-fatal injuries in unionized/non-RTW workplaces the result of employees' increased willingness to take advantage of workplace compensation policies without fear of reprisals in unionized workplaces? Is the relationship due to an increased likelihood of unionized employees having access to paid sick leave? Or the result of unions holding employers accountable and ensuring accurate and fair reporting to BLS?

Another potential area for future research could be the effect of unions on workplace safety during the pandemic. Were unions able to protect workers from COVID-19 in a meaningful way?

It would also be interesting to see what happens in Michigan after their recent repeal of their RTW law. Will they see an increase in unionization or an improvement in workplace safety?

Lastly, I am very curious about the source of the decline in total NFOIAI rates. What is the cause of this?

Workplace safety is an important issue that affects millions of workers each year. Policymakers looking to improve workplace safety and/or considering RTW legislation should understand the important role that unions play in protecting workers as well as the impact of RTW laws on workplace safety. They should also strive to ensure every worker is protected from workplace injury, illness, and death, regardless of whether they are represented by a union.

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Appendix A: Data sources and issues

The data for NFOIAI and severe injuries comes from the BLS SOII, compiled by CDC worker health charts.

The data for occupational injuries comes from the Census of Fatal Occupational Injuries.

The data for employment by industry comes from BLS Current Employment Statistics and the BLS Quarterly Census of Employment & Wages.

Issues: For Delaware Agriculture, Forestry, Fishing and Hunting employment, data is only reported in 2007. I assume the percent of employment in this industry follows the trend for the other states.

For Maine Agriculture, Forestry, Fishing and Hunting, data is not reported in 2008, 2009, and 2012. I assume Maine experiences a linear change in the percent of total employment in this industry between reported years.

For Rhode Island Agriculture, Forestry, Fishing and Hunting employment, data is not reported in 2010-2011 and 2013-2019. I assume the state experiences a linear change in the percent of total employment in this industry between 2009 and 2012. After 2012 I assume the state follows national trends.

My data for agriculture, forestry, fishing, and hunting comes from the Quarterly Census of Employment and Wages via BLS.

Data for transportation and warehousing comes from the Current Employment Statistics. Some states (WV, SD, NM, DE, AR) only reported transportation, warehousing, and utilities in tandem. For these states, I subtract employment in utilities. My data on employment in utilities comes from the Quarterly Census of Employment and Wages.

My data for unionization rates comes from the BLS Current Population Survey. Rates for private industry and specific industries, the data also comes from the Current Population Survey but is compiled on unionstats.com. Citation: Barry T. Hirsch, David A. Macpherson, and William E. Even. https://www.unionstats.com

My data for employment in age groups comes from the BLS Local Area Unemployment Statistics: For South Dakota, in 2019, I only have access to data for total employees aged 16-24. I assume that the proportion that is 16-19 and 20-24 is the same as 2018. For Alaska, employees aged 65+ are not reported for 2008. I assume a linear increase from 2007-2009.

Data for the percent of workforce that is male and has a bachelor's degree or higher of education comes from the BLS Geographic Profile of Employment and Unemployment.

Self-employment data also comes from the BLS Geographic Profile of Employment and Unemployment.

New Hires data comes from the BLS Job Openings and Labor Turnover Survey.

Unemployment for each state comes from the Federal Reserve Bank of St. Louis.

Policy liberalism comes from Caughey and Warsaw (2016), compiled by Michigan State University's Institute for Public Policy and Social Research. I extend the 2007-2014 trend through the remaining years due to a lack of data.