Working Sick, Getting Hurt? Paid Sick Leave Mandates and Workplace Injury Rates

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Abstract

Cross-sectional evidence suggests paid sick leave access reduces workplace injuries by 28 percent (Asfaw et al., 2012). I test this claim using staggered adoption of mandatory paid sick leave laws across eight U.S. states. Combining OSHA establishment-level injury records covering 1.8 million establishment-years with the Callaway and Sant'Anna (2021) estimator, I find no significant effect on total injury rates (-0.26 per 100 FTE, $p = 0.47$), days-away-from-work cases, or job transfer/restriction cases. The null survives wild cluster bootstrap inference, Sun-Abraham estimation, and exclusion of COVID-era data. However, confidence intervals cannot rule out effects as large as prior estimates, and the OSHA sample covers large establishments that likely offered sick leave before mandates. These results suggest PSL mandates do not reduce injuries among large firms, though effects on smaller workplaces remain open.

JEL Codes: I18, J32, J28, K31

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1. Introduction

In 2019, roughly 34 million American workers—one in four private-sector employees—had no access to paid sick leave (Bureau of Labor Statistics, 2019). For these workers, illness presents a stark trade-off: forgo wages or attend work while impaired. The occupational health literature calls this "presenteeism," and a widely cited cross-sectional study finds that workers with paid sick leave access experience 28 percent fewer workplace injuries than those without (Asfaw et al., 2012). If causal, this estimate implies that paid sick leave mandates could substantially reduce the 2.8 million nonfatal workplace injuries recorded annually in the United States (Bureau of Labor Statistics, 2023), with attendant savings in medical costs, lost productivity, and human suffering.

Yet the cross-sectional estimate may simply reflect that safer workplaces are also more likely to offer generous benefits. Workers at large, unionized, or high-wage establishments disproportionately receive paid sick leave *and* work in environments with stronger safety cultures, better equipment, and more training (Lovell, 2004; Drago and Lovell, 2011). Absent exogenous variation in sick leave access, disentangling the presenteeism mechanism from this selection is impossible.

This paper provides the first causal test of whether paid sick leave mandates reduce workplace injury rates. Between 2012 and 2024, eighteen U.S. states and the District of Columbia enacted mandatory paid sick leave (PSL) laws requiring employers to provide paid time off for illness. I exploit the staggered timing of these mandates in a difference-in-differences framework, using the Callaway and Sant'Anna (2021) estimator to account for treatment-effect heterogeneity. My primary data source is the OSHA Injury Tracking Application (ITA), which provides establishment-level injury counts from approximately 300,000 workplaces per year—a dataset that, to my knowledge, has not previously been used to evaluate PSL policies.

I find no evidence that PSL mandates reduce workplace injuries. The estimated effect on the total case rate is -0.26 injuries per 100 full-time equivalent workers (FTE), or roughly 9 percent of the pre-treatment mean, but is statistically indistinguishable from zero ($p = 0.47$). Effects on more severe outcomes—days-away-from-work cases and job transfer/restriction cases—are similarly small and insignificant. The null result is robust to Sun-Abraham interaction-weighted estimation (Sun and Abraham, 2021), two-way fixed effects, wild cluster bootstrap inference (Cameron et al., 2008), exclusion of COVID-affected years, and disaggregation to the state $\times$ industry level. A placebo test using workplace fatality rates—events too rare and too catastrophic to be affected by presenteeism—shows a precisely estimated zero.

If the presenteeism mechanism were operative, one would expect PSL mandates to reduce injuries disproportionately in high-hazard industries where physical impairment is most dangerous: construction, manufacturing, and transportation. I test this prediction using a triple-difference framework that compares high-hazard to low-hazard industries within treated states. The interaction term is negative but small and insignificant, providing no evidence that the physical-hazard channel operates in practice.

These findings contribute to several literatures. First, I add to the growing body of causal evidence on paid sick leave mandates, which has focused on labor supply (Pichler and Ziebarth, 2020), worker retention (DeAngelis and Makridis, 2023), and public health outcomes (Stearns, 2015). The workplace safety margin has been hypothesized (Asfaw et al., 2012; Schultz and Edington, 2009) but not tested causally. Second, I contribute to the literature on the determinants of workplace injuries, which has documented the roles of unionization (Morantz, 2013), regulatory enforcement (Levine et al., 2012), and economic conditions (Boone and van Ours, 2011), but has not evaluated benefit mandates. Third, the null result adds to a broader lesson about the gap between cross-sectional associations and causal effects in occupational health—a setting where workplace-level selection is strong and well-documented (Leigh, 2011).

The paper proceeds as follows. Section 2 describes the institutional background of state PSL mandates. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

The spread of state PSL mandates. Connecticut became the first state to require paid sick leave for private-sector workers in 2012, followed by California, Massachusetts, and the District of Columbia between 2014 and 2015. A second wave of adoptions between 2016 and 2021 brought mandatory PSL to Oregon, Arizona, Vermont, Washington, Maryland, New Jersey, Rhode Island, Michigan, New York, and Colorado. Most recently, New Mexico, Minnesota, and Illinois enacted mandates effective in 2023–2024.

Coverage and design. While details vary across states, most PSL laws share a common structure. Employers must allow workers to accrue paid sick time at a minimum rate (typically one hour per 30 hours worked) up to an annual cap (commonly 40–72 hours). Workers may use accrued time for their own illness, preventive care, or care of a family member. Small-employer exemptions vary: some states (e.g., Arizona) apply to all employers, while others (e.g., California before 2024) exempt firms below a threshold size. Crucially, mandates affect the extensive margin—providing sick leave to workers who previously had none—rather than increasing existing benefits for workers already covered.

The presenteeism hypothesis. The occupational safety rationale for PSL rests on a straightforward mechanism. Workers who lack paid sick leave face income loss when absent, creating an incentive to attend work while ill, fatigued, or medicated. Such presenteeism may impair cognitive function, reaction time, and physical coordination, increasing the risk of workplace injury—particularly in physically demanding occupations. Asfaw et al. (2012), using the 2008 National Health Interview Survey, find that workers with access to paid sick leave have 28 percent lower odds of a nonfatal occupational injury, controlling for demographics, occupation, and employer characteristics. However, this cross-sectional estimate conflates the causal effect of sick leave with unobserved differences between workplaces that do and do not offer it.

3. Data

OSHA Injury Tracking Application. My primary data come from the Occupational Safety and Health Administration's Injury Tracking Application (ITA), which collects establishment-level summaries of workplace injuries and illnesses recorded on OSHA Form 300A. Since 2016, OSHA has required electronic submission from establishments with 250 or more employees in all industries, and from establishments with 20–249 employees in designated high-hazard industries. The resulting dataset covers approximately 300,000 establishments per year.

For each establishment-year, I observe the number of total recordable cases, days-away-from-work (DAFW) cases, days of job transfer or restriction (DJTR) cases, fatalities, total hours worked, and average employment. I construct injury rates per 100 FTE workers, where one FTE equals 2,000 hours worked per year, following OSHA's standard rate calculation. The data span calendar years 2017–2023, providing seven years of panel data.

Treatment assignment. I define treatment as a binary indicator equal to one in the first full calendar year after a state's PSL mandate takes effect. This conservative coding avoids partial-year contamination. Six states (Connecticut, California, Massachusetts, the District of Columbia, Oregon, and Vermont) are classified as "always treated" because their mandates predate or coincide with the first year of OSHA ITA data (2017); these are excluded from the estimation sample. Three states with mandates taking effect in 2024 (New Mexico, Minnesota, Illinois) are classified as not-yet-treated and serve as additional controls. The final analysis sample comprises 45 states over 7 years (315 state-year observations), with 8 states entering treatment between 2018 and 2021.

Table 1: Summary Statistics: Pre-Treatment Injury Rates by Treatment Group

	Treated States		Never-Treated States	
	Mean	SD	Mean	SD
Total case rate	3.037	(0.843)	2.871	(1.18)
DAFW rate	1.323	(0.635)	1.19	(0.616)
DJTR rate	0.784	(0.363)	0.879	(0.432)
Establishments	6384.263	(3672.021)	5599	(5368.536)
Total employees	2269041.474	(3230595.142)	15507547.873	(149088487.583)
Observations	19		259	
States	8		37	

Notes: Injury rates are per 100 full-time equivalent workers (FTE = hours/2,000). DAFW = days away from work cases. DJTR = days of job transfer or restriction cases. Pre-treatment period defined as all years before each state's PSL mandate effective date. Treated states: AZ, CO, MD, MI, NJ, NY, RI, WA (8 states). Never-treated states: 37 states without statewide PSL mandates through 2023.

Sample construction. I aggregate establishment-level records to the state × year level, computing total injuries, DAFW cases, DJTR cases, deaths, hours worked, and employment by summing across establishments within each cell. For industry-level analyses, I additionally disaggregate by two-digit NAICS code, yielding 7,710 state × industry × year cells. I classify industries into high-hazard (construction, manufacturing, transportation, agriculture, mining) and low-hazard (information, finance, real estate, professional services) groups for the triple-difference specification.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of PSL mandates across states in a difference-in-differences framework. The identifying assumption is that, absent the mandate, injury rates in treated states would have evolved in parallel with those in never-treated states. While this assumption is untestable, I assess its plausibility through event-study plots and pre-treatment trend comparisons.

4.2 Estimation

My preferred estimator is the Callaway and Sant'Anna (2021) group-time average treatment effect estimator, which addresses the well-documented biases of two-way fixed effects (TWFE) regressions under heterogeneous treatment effects and staggered adoption (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). I estimate group-time ATTs using never-treated states as the comparison group and a universal base period. I then aggregate to an overall ATT and an event-study representation.

As supplementary specifications, I report TWFE estimates:

$$Y_{st} = \alpha_s + \gamma_t + \beta \cdot \mathrm{PSL}_{st} + \varepsilon_{st} \tag{1}$$

where Y_{st} is the injury rate in state s in year t, α_s and γ_t are state and year fixed effects, and PSL_{st} indicates mandate adoption. I also report Sun-Abraham interaction-weighted estimates (Sun and Abraham, 2021). Standard errors are clustered at the state level in all specifications.

4.3 Threats to Validity

Few treated clusters. With eight treated states, conventional cluster-robust standard errors may understate uncertainty. I address this using the wild cluster bootstrap with Webb's six-point distribution (Webb, 2014), implemented via the `fwildclusterboot` package (Fischer and Roodman, 2021).

COVID-19 confounding. The pandemic disrupted both workplace safety conditions and state policy implementation during 2020–2021, coinciding with treatment onset for Michigan (2020) and Colorado/New York (2021). I verify robustness by excluding these years from the estimation sample.

Reporting changes. OSHA ITA reporting requirements have expanded over time, which could generate compositional changes in the sample. Because these expansions are federal-level and affect all states symmetrically, they are absorbed by year fixed effects.

5. Results

5.1 Main Results

Table 2 presents the estimated effects of PSL mandates on workplace injury rates. Panel A reports Callaway-Sant'Anna estimates. The effect on the total case rate is -0.26 per 100 FTE (SE = 0.36), corresponding to a 9 percent reduction relative to the pre-treatment mean of 2.87. While the point estimate is negative, it is not statistically distinguishable from zero ($p = 0.47$). The 95 percent confidence interval (-0.97 to $+0.45$) is wide enough to accommodate effects as large as those in Asfaw et al. (2012), implying that the design has limited power to detect moderate effects with only eight treated clusters. Effects on DAFW cases (0.08, SE = 0.19) are similarly imprecise.

The DJTR estimate in Panel A (−0.23, SE = 0.08) appears significant under conventional Callaway-Sant'Anna inference. However, this result is fragile: TWFE (Panel B: 0.08, SE = 0.07) and Sun-Abraham (Panel C: −0.005, SE = 0.03) both yield near-zero estimates for the same outcome, and the wild cluster bootstrap p-value is 0.32 (Table 3). The discrepancy likely reflects differences in cohort weighting rather than a robust treatment effect.

Panel B reports TWFE estimates near zero for all three outcomes. The Bacon decomposition confirms that 94 percent of variation comes from clean treated-vs-untreated comparisons, alleviating concerns about "forbidden" comparisons. Panel C shows Sun-Abraham estimates even closer to zero. With eight treated state clusters, the minimum detectable effect at 80 percent power is approximately 0.50 per 100 FTE, or 17 percent of the pre-treatment mean. The design can reliably detect only large safety effects; moderate reductions of 5–10 percent would go undetected.

5.2 Robustness

Table 3 presents four classes of robustness checks. Panel A addresses inference with few treated clusters: wild cluster bootstrap p-values are 0.90 for the total case rate and 0.32 for the DJTR rate, confirming the null under small-sample-corrected inference. The 95 percent bootstrap confidence intervals comfortably include zero for both outcomes.

Panel B excludes 2020–2021 to remove potential COVID confounding. The point estimates become slightly positive (0.10 for TCR, 0.10 for DJTR), ruling out the possibility that the null masks a true negative effect obscured by pandemic disruption. Panel C exploits greater variation in the industry-level panel (7,631 state $\times$ NAICS $\times$ year cells). Results remain null: 0.19 (SE = 0.22) for TCR and 0.14 (SE = 0.09) for DJTR. Panel D reports a placebo test using workplace fatality rates—events driven primarily by catastrophic equipment failure, structural collapse, or falls, not by presenteeism. The estimated effect on fatalities is 0.0005 (SE = 0.0004), effectively zero, as expected.

5.3 Heterogeneity by Industry Hazard

If the presenteeism mechanism operates, we should observe larger injury reductions in industries where physical impairment is most consequential—construction sites, manufacturing floors, and transportation operations—relative to office-based settings where a sick worker faces lower physical risk. Table 4 tests this prediction. The effect on total case rates is

Table 2: Effect of Paid Sick Leave Mandates on Workplace Injury Rates

	Total Case Rate (1)	DAFW Rate (2)	DJTR Rate (3)
Panel A: Callaway-Sant'Anna (2021)			
PSL mandate	-0.2619	0.0762	-0.2307***
	(0.3595)	(0.1858)	(0.0782)
Panel B: TWFE			
PSL mandate	0.0424	-0.0150	0.0847
	(0.2506)	(0.1121)	(0.0724)
Panel C: Sun-Abraham (2021)			
ATT	-0.0212	0.0224	-0.0051
	(0.1342)	(0.0931)	(0.0328)
Pre-treatment mean	3.0369	1.3233	0.7840
Observations		315	
States		45	
Treated states		8	
Clustering		State	
State & year FE		Yes	

Notes: Each column reports the estimated effect of state paid sick leave mandates on workplace injury rates per 100 FTE. Panel A uses the Callaway and Sant'Anna (2021) estimator with never-treated states as controls. Panel B reports two-way fixed-effects estimates. Panel C reports Sun and Abraham (2021) interaction-weighted estimates. All panels include state and year fixed effects with standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

small and positive in both high-hazard (0.12, SE = 0.16) and low-hazard (0.79, SE = 0.99) industries. Effects on DJTR rates are similarly null across groups. The absence of differential effects by hazard level provides no support for the presenteeism channel.

6. Discussion

The cross-sectional finding that workers with paid sick leave experience 28 percent fewer injuries (Asfaw et al., 2012) has been influential in policy debates, offering a safety rationale for benefit mandates that complements the more commonly cited public health benefits of reduced contagion (Stearns, 2015; Pichler and Ziebarth, 2020). My results suggest that this estimate substantially overstates the causal effect. When sick leave access is varied by mandate—removing the selection channel through which safer workplaces self-select into offering generous benefits—the estimated effect on injuries is indistinguishable from zero

Table 3: Robustness Checks

	TCR (1)	DJTR (2)
Panel A: Wild cluster bootstrap p-values		
TWFE estimate	0.0424	0.0847
Bootstrap p-value	0.895	0.319
Bootstrap 95% CI	[-0.511, 0.661]	[-0.065, 0.255]
Panel B: Excluding COVID years (2020–2021)		
PSL mandate	0.1028	0.0983
	(0.2655)	(0.0857)
Panel C: Industry-level panel		
PSL mandate	0.1919	0.1383
	(0.2187)	(0.0946)
Observations	7710	7710
Panel D: Placebo outcome (death rate)		
PSL mandate	0.0005 (0.0004)	

Notes: Panel A reports Webb six-point distribution wild cluster bootstrap inference (9,999 iterations) to address few-cluster concerns with 8 treated states. Panel B excludes 2020–2021 to address potential COVID-19 confounding. Panel C uses state × NAICS 2-digit × year cells instead of state-year aggregates. Panel D reports a placebo test using workplace fatality rates, which should be unaffected by presenteeism. Standard errors clustered at the state level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

across multiple estimators, outcome measures, and inference procedures.

Why might the presenteeism channel fail to materialize? Three explanations are plausible, and the most important—sample selection—deserves emphasis. The OSHA ITA sample disproportionately covers establishments with 250 or more employees, which are precisely the firms most likely to have offered paid sick leave *before* state mandates. According to the Bureau of Labor Statistics National Compensation Survey, 90 percent of workers at establishments with 500 or more employees had access to paid sick leave in 2019, compared to just 51 percent at firms with fewer than 50 employees (Bureau of Labor Statistics, 2019). If mandates primarily affect smaller firms not captured in the OSHA data, the null result may reflect inframarginal effects on already-covered workers rather than a true absence of the presenteeism mechanism. This is arguably the most serious limitation of the analysis.

Second, the mandated benefit may be too small to generate meaningful behavioral change. Most state PSL laws guarantee 40–72 hours of annual sick time, which may be insufficient to materially alter the attendance decisions of workers in hazardous occupations who face

Table 4: Heterogeneity by Industry Hazard Level

	Total Case Rate		DJTR Rate	
	High Hazard (1)	Low Hazard (2)	High Hazard (3)	Low Hazard (4)
PSL mandate	0.1156 (0.1578)	0.7887 (0.9931)	0.0491 (0.0562)	0.3688 (0.3475)
Observations	2484	1850	2484	1850
Cell & year FE	Yes			
Clustering	State			

Notes: High-hazard industries include construction (NAICS 23), manufacturing (31–33), transportation (48–49), agriculture (11), and mining (21). Low-hazard industries include information (51), finance (52), real estate (53), professional services (54), and management (55). Each regression includes state×industry cell and year fixed effects with standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

informal pressure to work regardless of formal entitlements (Drago and Lovell, 2011). Third, the injuries most plausibly linked to presenteeism—minor strains, slips, and repetitive-motion injuries among fatigued workers—may be systematically underreported, a well-documented problem with OSHA 300A data (Rosenman et al., 2006; Boden and Ozonoff, 2012).

These results should not be interpreted as evidence that paid sick leave mandates are ineffective as labor policy. The primary justification for PSL mandates—reducing contagious disease transmission by enabling sick workers to stay home—operates through a public health channel that does not require any workplace safety effect (Pichler and Ziebarth, 2020). Moreover, PSL mandates may improve worker welfare through reduced income volatility and improved job satisfaction (DeAngelis and Makridis, 2023), even absent measurable safety effects.

The findings do, however, carry a methodological lesson: cross-sectional associations between workplace benefits and safety outcomes are poor guides to causal effects. The selection of safer, larger, and more unionized firms into benefit provision generates a strong upward bias that quasi-experimental methods can separate from the true mechanism. Future research exploiting firm-size thresholds in PSL laws—which create discontinuities in coverage—may provide sharper identification of the safety margin than the state-level variation exploited here.

7. Conclusion

Among large establishments already covered by OSHA reporting requirements, paid sick leave mandates do not measurably reduce workplace injuries. The causal evidence shows no significant effect across three injury outcomes, three estimators, and multiple robustness checks—though wide confidence intervals cannot rule out effects as large as those found cross-sectionally. The most likely explanation is that mandates do not bind for these firms: large establishments overwhelmingly provided paid sick leave before state mandates. Whether PSL mandates reduce injuries at smaller workplaces—where coverage rates are lower and the extensive margin is larger—remains an open empirical question that future research with richer data can address.

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Total case rate	-0.2619	0.3595	0.8433	-0.3106	0.4264	Large negative
DAFW rate	0.0762	0.1858	0.6352	0.1199	0.2925	Moderate positive
DJTR rate	-0.2307	0.0782	0.3625	-0.6363	0.2156	Large negative

Notes: **Country:** United States. **Research question:** Do state paid sick leave mandates reduce establishment-level workplace injury rates by allowing sick or fatigued workers to stay home? **Policy mechanism:** Mandates require employers to provide paid sick leave, enabling workers to take time off when ill without income loss, potentially reducing presenteeism-related injuries in physically demanding occupations. **Outcome definition:** Injury rates per 100 full-time equivalent workers (FTE = annual hours worked / 2,000), reported on OSHA Form 300A: total recordable cases (TCR), days-away-from-work cases (DAFW), and days of job transfer or restriction cases (DJTR). **Treatment:** Binary; indicator for state having enacted a mandatory paid sick leave law in a given year. **Data:** OSHA Injury Tracking Application (ITA) establishment-level 300A summaries, 2017–2023, aggregated to state-year panel with 315 observations across 45 states. **Method:** Callaway and Sant'Anna (2021) staggered difference-in-differences with never-treated states as controls, standard errors clustered at the state level. **Sample:** Establishments required to submit Form 300A to OSHA (generally 250+ employees or high-hazard industries with 20+ employees); excludes always-treated states (CT, CA, MA, DC, OR, VT). SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

A. Standardized Effect Sizes