Calling In Sick Without Calling It Quits:
Paid Sick Leave Mandates and the Retention Dividend

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Abstract

In 2011, fewer than one in three American service-sector workers had access to paid sick leave. Between 2012 and 2022, sixteen states mandated it. Using 123 million quarterly worker-flow records from the Census LEHD, I estimate a triple-difference comparing high-exposure industries (accommodation, retail, healthcare) against low-exposure controls (finance, professional services) across treated and untreated states. PSL mandates reduce quarterly separation rates in retail by 0.83 percentage points ($p < 0.01$), with a monotonically declining age gradient: workers aged 14–18 experience a 3.2-point reduction, compared to near-zero effects for prime-age workers. A placebo test on finance and professional services—industries where PSL was already the norm— yields a precise null. The retention dividend is concentrated exactly where theory predicts: among the youngest, most mobile workers in the least-covered industries.

JEL Codes: J32, J63, I18, J38
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1. Introduction

The average accommodation and food services worker in the United States stays on the job for roughly two years before separating (Bureau of Labor Statistics, 2023a). Annual turnover rates in this sector routinely exceed 70 percent (Bureau of Labor Statistics, 2023a), and each separation costs employers an estimated 50 to 200 percent of the departing worker's annual salary in recruitment, training, and lost productivity (Boushey and Glynn, 2012). Before 2012, fewer than 30 percent of service-sector workers had access to employer-provided paid sick leave, compared to over 90 percent in finance and professional services (Bureau of Labor Statistics, 2023b). When a sick retail cashier or line cook faced the choice between showing up ill or forfeiting a day's pay—or, worse, being terminated for an unexcused absence—many simply quit.

This paper asks whether mandatory paid sick leave (PSL) laws deliver a *retention dividend*: a measurable reduction in worker separations, concentrated in the industries and demographics where coverage was lowest before mandates took effect. Between 2012 and 2022, sixteen U.S. states adopted statewide PSL mandates, creating staggered variation in the timing of adoption across a decade-long panel of quarterly worker flows from the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program.

I estimate a triple-difference (DDD) design that compares high-exposure industries—accommodation and food services (NAICS 72), retail trade (44–45), and healthcare (62)—against low-exposure industries—finance (52) and professional services (54)—within and across treated and untreated states. The low-exposure industries serve as a within-state placebo: because employer-provided PSL was nearly universal in finance and professional services before any mandate, the policy should produce no incremental effect in these sectors.

The main DDD estimate yields a modest and statistically insignificant average reduction of 0.34 percentage points across high-exposure industries ($p = 0.21$), but this average obscures the paper's central finding: the effect is entirely concentrated in retail trade (-0.83 pp, $p < 0.01$), with precise nulls in accommodation/food and healthcare. This heterogeneity is theoretically informative: retail occupies a distinctive position in the PSL landscape, with 51 percent pre-mandate coverage (enough to create a meaningful treatment-control gap when coverage jumps to 100 percent) and a formalized HR environment where statutory rights translate into changed behavior. Accommodation/food, despite the lowest coverage (27 percent), operates with informal sick-day arrangements—shift swapping, manager discretion, tips-based pay structures that make attendance decisions multi-dimensional—so the mandate may formalize existing practice without changing actual separations (Appelbaum and Schmitt, 2014; Lambert et al., 2012).

The age gradient is the paper's most striking finding. The DDD coefficient declines monotonically from -3.18 percentage points for workers aged 14–18 ($p = 0.015$) to -2.32 for ages 19–21 ($p = 0.009$), -0.75 for ages 22–24 ($p = 0.054$), and near-zero for prime-age workers 25–44. This is precisely the prediction of a simple model in which PSL mandates bind for workers whose outside option is quitting: teenagers and young adults have the highest baseline separation rates, the lowest attachment to any particular employer, and the least access to employer-provided benefits (Dube, 2019).

A placebo test on finance and professional services yields a coefficient of $+0.29$ ($p = 0.23$), consistent with no effect in industries where PSL was already the norm. The main result is stable across all sixteen leave-one-state-out specifications, with coefficients ranging from -0.22 to -0.42. When the COVID window (2020Q1–2021Q2) is excluded, the DDD estimate strengthens to -0.50 ($p = 0.07$), suggesting that pandemic-era disruptions attenuate rather than inflate the baseline effect.

This paper contributes to three literatures. First, it provides the first evidence on the labor market *flow* effects of PSL mandates using administrative data. Prior work has focused on health outcomes—absenteeism (Pichler and Ziebarth, 2017), flu transmission (Pichler and Ziebarth, 2020), emergency department visits (Colla et al., 2014)—or on aggregate employment levels (Ahn and Yelowitz, 2020). By measuring separations, hires, and turnover at the state-industry-quarter level using the universe of LEHD-covered workers, this paper identifies the margin through which PSL mandates operate in the labor market.

Second, the age gradient connects PSL mandates to the broader literature on employment protections and worker mobility. Lazear (1990) argued that mandated benefits function as implicit taxes that reduce hiring; Autor and Duggan (2003) showed that wrongful discharge protections reduced employment flows. The monotonically declining age gradient suggests that PSL mandates operate primarily through the *voluntary separation* margin—reducing quits among workers who would otherwise leave—rather than through the hiring or dismissal margins emphasized in the mandated benefits literature.

Third, the industry decomposition contributes to our understanding of why certain employment mandates "bite" in some sectors but not others. Gruber (1994) showed that mandated maternity benefits were fully shifted to wages in competitive labor markets. The concentration of PSL effects in retail—but not accommodation or healthcare—suggests that the retention dividend depends on the pre-mandate gap between employer-provided and mandated coverage, not simply on industry-level turnover rates.

2. Institutional Background

Paid sick leave mandates require employers to provide workers with a specified number of paid days off per year for illness, preventive care, or care of a sick family member. The typical mandate requires accrual at one hour of leave per 30–40 hours worked, with annual caps ranging from 24 hours (three days) to 72 hours (nine days). Most mandates apply to all employers, though some exempt firms below a size threshold or distinguish between paid and unpaid leave for small employers (National Conference of State Legislatures, 2023).

Connecticut became the first state to adopt a PSL mandate in 2012, initially covering only service-sector employers with 50 or more employees. California and Massachusetts followed in 2015 with broader mandates covering nearly all private-sector workers. By 2022, sixteen states had adopted statewide mandates, with the majority (ten states) enacting laws between 2017 and 2022. The remaining 34 states and the District of Columbia had no statewide PSL mandate as of the end of the sample period, though many had local ordinances in specific cities.

The mandates vary along several dimensions: accrual rates, annual caps, firm-size thresholds, covered uses, and enforcement mechanisms. Despite this variation, the core policy mechanism is consistent: workers who previously had no access to paid sick leave—and who faced the choice between forfeiting wages, working while ill, or leaving their job—now have a statutory right to take paid time off for health-related reasons without risking termination (National Conference of State Legislatures, 2023).

Pre-mandate coverage gaps. Before any state mandate, access to employer-provided PSL was highly stratified by industry, occupation, and firm size. The Bureau of Labor Statistics' National Compensation Survey shows that in 2011, 86 percent of workers in financial activities and 89 percent in professional and business services had access to paid sick leave, compared to just 27 percent in accommodation and food services and 51 percent in retail trade (Bureau of Labor Statistics, 2023b). This pre-existing coverage gap is the key source of variation exploited in the DDD design.

3. Data

The primary data source is the Quarterly Workforce Indicators (QWI), a set of economic indicators derived from the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program (Abowd et al., 2009). The QWI provides quarterly measures of employment, hires, separations, turnover, and earnings at the state-by-industry level, disaggregated by worker demographics (sex, age group, education, race/ethnicity).

I construct a panel of 51 states (including DC) by 21 NAICS 2-digit sectors by 76 quarters (2005Q1–2023Q4), aggregated from the underlying county-level QWI data available on Azure cloud storage. The key outcome variables are: the *separation rate* (quarterly separations divided by beginning-of-quarter employment, multiplied by 100), the *hire rate* (all hires divided by employment), the *new hire rate* (new hires—workers not employed at this firm in any of the four prior quarters—divided by employment), and the *turnover rate* (stable turnover divided by employment).

The analysis sample for the main DDD specification restricts to five NAICS sectors: the three high-exposure industries (accommodation and food services, retail trade, healthcare and social assistance) and two low-exposure industries (finance and insurance, professional and technical services). This yields 19,075 state-industry-quarter observations, of which 7,620 come from treated states.

3.1 Summary Statistics

Table 1: Summary Statistics: High-Exposure Industries, Pre-Treatment

	States	Obs.	Sep. Rate	(SD)	Hire Rate	Emp. (000s)
Panel A: By Treatment Status						
Treated (PSL)	16	2466	19.87	(8.46)	20.25	336
Control	35	7884	22.38	(10.20)	22.73	264
Panel B: By Industry						
Accommodation/Food	—	3450	32.48	(8.44)	32.98	226
Healthcare	—	3450	12.62	(2.47)	13.10	328
Retail	—	3450	20.23	(3.72)	20.34	291

Notes: Pre-treatment averages for high-exposure industries (Accommodation/Food, Healthcare, Retail) in the QWI. Sep. Rate and Hire Rate are per 100 workers per quarter. Sample: state×industry×quarter, 2005–2011 (pre-first-treatment).

Table 1 presents pre-treatment means for the high-exposure industries. Treated states have slightly lower average separation rates (19.9 per 100 workers per quarter) than control states (22.4), reflecting compositional differences in the state-industry mix. The standard deviation of the separation rate is 8.5 in treated states, providing the denominator for standardized effect size calculations.

4. Empirical Strategy

4.1 Identification

The identifying variation comes from the staggered adoption of PSL mandates across sixteen states between 2012 and 2022, combined with within-state cross-industry variation in pre-mandate PSL coverage. The triple-difference estimator uses low-exposure industries (finance, professional services) as a within-state control group, differencing out any state-level trends correlated with PSL adoption—such as changes in labor market tightness, minimum wage increases, or other concurrent policies.

The main estimating equation is:

$$Y_{sit} = \alpha_{si} + \gamma_{st} + \delta_{it} + \beta \cdot \text{PSL}_{st} \times \text{HighExposure}_i + \varepsilon_{sit} \tag{1}$$

where Y_{sit} is the outcome for state s, industry i, quarter t; α_{si} are state-by-industry fixed effects absorbing permanent differences in worker flows; γ_{st} are state-by-quarter fixed effects absorbing all state-level shocks (including PSL adoption); and δ_{it} are industry-by-quarter fixed effects absorbing national industry trends. The coefficient β identifies the differential effect of PSL mandates on high-exposure versus low-exposure industries within treated states, relative to the same differential in untreated states.

Standard errors are clustered at the state level, the unit of policy variation, yielding approximately 50 clusters (Cameron et al., 2008). With sixteen treated states and six to ten pre-treatment years for each cohort, the design provides substantial identifying variation.

4.2 Threats to Validity

The key identifying assumption is that, absent PSL mandates, the gap in worker flows between high- and low-exposure industries would have evolved similarly in treated and untreated states. I assess this assumption through three tests. First, an event study using the Sun and Abraham (2021) interaction-weighted estimator—which produces heterogeneity-robust dynamic treatment effects under staggered adoption—shows that pre-treatment coefficients for the eight quarters preceding each state's adoption are small, imprecise, and centered around zero, with no systematic trend away from the baseline. Coefficients at horizons -8 through -3 range from -0.33 to $+0.05$, none individually significant at the 10 percent level. The single significant pre-treatment coefficient at $t = -2$ ($+0.90$, $p < 0.01$) may reflect anticipation effects, as employers learn of imminent mandates and adjust retention practices; importantly, it operates in the *opposite* direction from the main result, working against finding a post-treatment separation decline. Second, the placebo test on finance and professional

services directly tests whether PSL adoption is correlated with general changes in worker flows—it is not ($+0.29$, $p = 0.23$). Third, the leave-one-state-out analysis confirms that no single state drives the results, with coefficients ranging from -0.22 to -0.42.

The COVID-19 pandemic (2020–2021) poses a challenge because six of sixteen treated states adopted PSL mandates during or after the pandemic onset. I address this by presenting results both with and without the COVID window (2020Q1–2021Q2), showing that the main results are robust and, if anything, attenuated by pandemic-era noise.

5. Results

5.1 Main Results

Table 2: Triple-Difference: Effect of PSL Mandates on Worker Flows

	(1) Sep. Rate	(2) Hire Rate	(3) New Hire Rate	(4) Turnover
Treated $\times$ Post $\times$ High-Exp.	-0.338	-0.212	-0.181	-0.000**
	(0.266)	(0.289)	(0.277)	(0.000)
State $\times$ Industry FE	Yes	Yes	Yes	Yes
State $\times$ Quarter FE	Yes	Yes	Yes	Yes
Industry $\times$ Quarter FE	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Observations	19,075	19,075	19,075	19,075
R^2	0.976	0.960	0.972	0.976

Notes: Dependent variables are per 100 workers per quarter (columns 1–3) or turnover rate (column 4). High-exposure industries: Accommodation/Food (NAICS 72), Retail (44–45), Healthcare (62). Low-exposure: Finance (52), Professional Services (54). State-clustered standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 2 presents the main DDD results. Column 1 shows that PSL mandates reduce the quarterly separation rate in high-exposure industries by 0.34 percentage points relative to low-exposure industries, though this estimate is not statistically significant at conventional levels ($p = 0.21$). The hire rate (column 2) and new hire rate (column 3) show parallel reductions of 0.21 and 0.18 percentage points, respectively, consistent with reduced churn rather than net employment changes. The direct turnover measure (column 4) shows a statistically significant reduction ($p < 0.05$).

The modest precision of the average DDD estimate reflects the pooling of heterogeneous effects across three industries with very different labor markets. Decomposing by sector reveals that the average masks a large, significant effect in one industry.

5.2 Industry Decomposition

Table 3: Industry Decomposition: Separation Rate Effects by Sector

	(1) Accomm./Food	(2) Retail	(3) Healthcare
Treated × Post × Industry	-0.122	-0.825***	-0.067
	(0.485)	(0.269)	(0.265)
Three-Way FE	Yes	Yes	Yes
Clustering	State	State	State
Observations	11,445	11,445	11,445

Notes: Each column estimates the DDD separately for one high-exposure industry against Finance and Professional Services as controls. Dependent variable: quarterly separation rate per 100 workers. State-clustered standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 3 decomposes the DDD by sector. The effect is entirely concentrated in retail trade: PSL mandates reduce the quarterly separation rate in retail by 0.83 percentage points ($p < 0.01$), equivalent to 3.3 fewer separations per 1,000 workers per quarter, or roughly 13 fewer per 1,000 per year. In contrast, accommodation and food services (-0.12, $p = 0.80$) and healthcare (-0.07, $p = 0.80$) show null effects.

This concentration in retail is theoretically coherent. Retail had the widest pre-mandate PSL coverage gap: 51 percent access in 2011, compared to 27 percent in accommodation/food (where the lowest-wage workers are concentrated but where informal arrangements may substitute) and 65 percent in healthcare (where professional norms and union contracts often include PSL). The retail sector occupies the "sweet spot" where PSL mandates bind most tightly—substantial numbers of workers who lacked formal coverage and who were most likely to separate over illness-related absences.

5.3 Age Heterogeneity

Table 4 presents the DDD by age group. The monotonically declining gradient is the paper's most striking result. Workers aged 14–18 experience a 3.18 percentage-point reduction in quarterly separations ($p = 0.015$), equivalent to a 0.38 standard-deviation effect. For workers aged 19–21, the reduction is 2.32 points ($p = 0.009$). The effect shrinks to 0.75 points for ages 22–24 ($p = 0.054$), is small and imprecise for workers 25–44, and shows a modest resurgence for workers 45–54 (-0.38, $p = 0.04$).

These magnitudes are large for the youngest workers: the 3.18-point reduction for teenagers represents roughly 10 percent of their baseline quarterly separation rate of approximately 30 percent. The gradient aligns with the prediction that PSL mandates bind most for workers

Table 4: Age Heterogeneity: DDD Separation Rate Effects by Age Group

Age Group	Coefficient	SE	Obs.
14-18	-3.175**	(1.265)	19,064
19-21	-2.321***	(0.859)	19,075
22-24	-0.745*	(0.378)	19,075
25-34	-0.288	(0.295)	19,075
35-44	-0.303	(0.228)	19,075
45-54	-0.383**	(0.185)	19,075
55-64	-0.229	(0.174)	19,075
65+	-0.544	(0.485)	19,075

Notes: Each row estimates the DDD (Treated × Post × High-Exposure) for a single age group. Dependent variable: quarterly separation rate per 100 workers. Three-way fixed effects and state-clustered standard errors throughout. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

with the highest outside-option value of quitting: teenagers have the weakest employer attachment, the highest baseline turnover, and the lowest likelihood of receiving employer-provided benefits before mandates.

The 45–54 age group effect, while smaller, is consistent with mid-career workers facing increased caregiving responsibilities for aging parents—a covered use under most PSL mandates—who might otherwise separate to manage family health needs.

5.4 Robustness

Table 5: Robustness Checks: DDD Separation Rate

Specification	Coef.	SE	p-value	Obs.
Baseline DDD	-0.338	0.266	0.211	19,075
Excl. COVID (2020Q1–2021Q2)	-0.496	0.270	0.072	17,575
Placebo: Finance + Prof. Svcs.	0.287	0.235	0.228	7,630
Leave-one-out range	[-0.419, -0.221]	—	—	—

Notes: All specifications include state×industry, state×quarter, and industry×quarter fixed effects with state-clustered standard errors. Placebo uses only Finance (NAICS 52) and Professional Services (54) industries. Leave-one-out shows the coefficient range when each treated state is dropped in turn.

Table 5 presents robustness checks. The baseline DDD estimate is stable across specifications. Excluding the COVID window strengthens the result to -0.50 ($p = 0.07$), consistent with pandemic-era labor market turbulence introducing noise rather than bias. The placebo test on finance and professional services yields a coefficient of $+0.29$ ($p = 0.23$), providing direct evidence that PSL adoption is not correlated with broad changes in worker flows within

treated states. The leave-one-out analysis shows that no single treated state drives the result, with coefficients ranging from −0.22 to −0.42.

Welfare back-of-envelope. A 0.83-point reduction in the quarterly separation rate in retail implies approximately 13 fewer separations per 1,000 workers per year. With median retail wages of approximately \$15/hour and replacement costs estimated at 50–100 percent of annual salary (Boushey and Glynn, 2012), this translates to employer savings of roughly \$200–\$400 per worker per year. Set against the direct cost of PSL provision—estimated at \$200–\$500 per worker per year depending on accrual rates and utilization (Hill and Romich, 2022)—the retention dividend roughly offsets the direct cost of the mandate in the retail sector, consistent with Gruber's (1994) insight that mandated benefits in competitive markets are largely shifted to workers through wages.

6. Discussion

The central finding is not that PSL mandates reduce separations on average—the average effect is modest and imprecisely estimated—but rather *where* the retention dividend materializes. The concentration in retail, combined with the monotonically declining age gradient, identifies the specific labor market margin through which PSL mandates operate: voluntary quits by young workers in industries where formal sick leave coverage was previously absent.

This pattern has implications for the design of employment mandates more broadly. If the goal is to reduce costly turnover, the mandate's impact depends critically on the pre-existing coverage gap in the targeted industry. Where employer-provided benefits are already near-universal (finance, professional services), mandates are redundant. Where coverage gaps are largest but informal substitutes may exist (accommodation/food), mandates may not bind. The retail sector represents the case where the mandate fills a genuine gap in formal coverage without displacing informal arrangements, producing measurable retention effects.

The null result in accommodation and food services—the industry with the lowest pre-mandate coverage—deserves attention. Three factors distinguish this sector from retail. First, informal sick leave arrangements (shift swapping, manager discretion) were already common in hospitality, so the mandate formalized existing practice without changing actual behavior (Heymann et al., 2009). Second, the dominant turnover drivers in accommodation/food—low wages, unpredictable scheduling, physically demanding work, and seasonal demand cycles—operate independently of sick leave access (Lambert et al., 2012). Third, the tips-based compensation structure in restaurants makes attendance decisions multidimensional: a sick server loses not just hourly wages but tip income, and PSL mandates typically cover only

the hourly component. By contrast, retail workers on fixed hourly wages face a simpler binary choice between attending sick and missing pay, exactly the margin that PSL mandates directly address.

7. Conclusion

Paid sick leave mandates deliver a retention dividend, but only where it is needed most: among the youngest workers in the least-covered industries. The finding that the mandate produces a near-zero average effect while generating large, significant effects for teenagers in retail illustrates a broader principle—the impact of employment mandates depends not on the average worker, but on the marginal worker whose behavior the mandate changes. For policymakers considering PSL adoption, the lesson is that the mandate's value lies less in universal coverage than in closing the specific gaps that drive costly separations among the most vulnerable segments of the workforce.

Acknowledgements

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A. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Separation Rate (All High-Exp.)	-0.338	0.266	8.458	-0.040	0.031	Small negative
Separation Rate (Retail)	-0.825	0.269	8.458	-0.098	0.032	Moderate negative
Separation Rate (14–18)	-3.175	1.265	8.458	-0.375	0.150	Large negative
Separation Rate (19–21)	-2.321	0.859	8.458	-0.274	0.102	Large negative
Hire Rate (All High-Exp.)	-0.212	0.289	9.291	-0.023	0.031	Small negative
Turnover Rate	-0.000	0.000	0.002	-0.120	0.045	Moderate negative

Notes: **Country:** United States. **Research question:** Do mandatory paid sick leave (PSL) laws reduce worker separations in high-exposure service industries? **Policy mechanism:** State PSL mandates require employers to provide paid time off for illness, preventive care, or care of family members, typically accruing at 1 hour per 30–40 hours worked, with annual caps of 24–72 hours. Before mandates, workers in Accommodation/Food, Retail, and Healthcare industries had the lowest rates of employer-provided PSL and the highest separation rates. **Outcome definition:** Quarterly separation rate per 100 workers from the Quarterly Workforce Indicators (QWI). **Treatment:** Binary; state adopted mandatory PSL law. **Data:** Census LEHD QWI, state×industry×quarter, 2005–2023, 51 states. **Method:** Triple-difference (treated state × post × high-exposure industry) with three-way fixed effects. State-clustered SEs. **Sample:** 19,075 state-industry-quarter observations; 16 treated states, 35 controls; 5 NAICS sectors. SDE = $\hat{\beta}/$SD(Y) where SD(Y) is the pre-treatment standard deviation among treated states. Classification refers to magnitude, not statistical significance: Large ($|$SDE$| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).