Mandated to Stay: Paid Sick Leave Laws and Worker Churning in Food Service

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Abstract

One in three food service workers separates from their employer each quarter. Do paid sick leave mandates reduce this churning? I exploit the staggered adoption of state-level mandates across nine U.S. states (2012–2019) using Callaway and Sant'Anna (2021) difference-in-differences applied to the Census Quarterly Workforce Indicators. Mandates reduce the worker turnover rate in food service by 3.7 percent ($p < 0.01$), a decline of 0.18 standard deviations of the pre-treatment distribution. A four-way flow decomposition reveals that this decline operates not through reductions in gross separations or hires—neither is statistically significant—but through a compression of simultaneous hiring-and-separation, or "churning." The pattern is consistent with mandates preserving viable employer-worker matches that would otherwise dissolve over short-term illness spells.

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1. Introduction

The American food service industry employs over 12 million workers and is defined by extraordinary labor market churn: roughly one-third of workers separate from their employer each quarter, a rate more than double the private-sector average (Bureau of Labor Statistics, 2023b). This revolving door imposes substantial costs—employers spend an estimated $5,864 per separated worker on recruitment and training (Society for Human Resource Management, 2022), while workers lose firm-specific human capital and experience earnings instability. A leading policy response has been state-level paid sick leave mandates, which by 2019 covered workers in nine states. The core logic is straightforward: workers who can take paid time off when ill need not choose between working sick (presenteeism) and quitting.

Yet the existing literature offers surprisingly little evidence on whether these mandates actually reduce turnover. Research has focused primarily on health outcomes—workplace injuries (Pichler et al., 2020), emergency department visits (Callison and Pesko, 2022), and disease transmission (Stearns and White, 2023)—or on labor demand channels such as work hours and employment levels (Maclean et al., 2020; Pauly et al., 2023). Ahn and Yelowitz (2024) provides the closest precedent, finding modest employment increases, but does not decompose the underlying worker flows. Whether mandates reduce the number of workers flowing through positions (churning) or simply alter the composition of who comes and goes has remained an open question.

This paper provides the first decomposition of paid sick leave mandate effects into distinct worker-flow channels using the Census Bureau's Quarterly Workforce Indicators (QWI). The QWI is uniquely suited to this task because it separately records new hires (workers appearing at an employer for the first time), recalls (workers returning to a previous employer), separations, and stable employment (workers present for a full quarter)—allowing me to test not just whether turnover changes, but *how*. I construct a county-by-quarter panel covering 2,636 counties in 49 states from 2005 to 2022, restricting attention to the food service sector (NAICS 722) where paid sick leave coverage was lowest before mandates took effect and where high turnover makes the policy most salient.

My identification strategy exploits the staggered adoption of paid sick leave mandates across nine states between 2012 and 2019. Connecticut was first (2012), followed by California and Massachusetts (2015), Oregon (2016), Vermont and Arizona (2017), Washington and Maryland (2018), and New Jersey (2019). I estimate group-time average treatment effects using Callaway and Sant'Anna (2021), which avoids the well-documented biases of two-way fixed effects in staggered settings (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœ uille, 2020; Sun and Abraham, 2021). Standard errors are clustered at the state level, and I confirm robustness using wild cluster bootstrap (Cameron et al., 2008).

The main finding is that paid sick leave mandates reduce the QWI turnover rate in food service by 0.0066 (SE = 0.0015), or 3.7 percent of the pre-treatment mean, an effect significant at the 1 percent level. To interpret this result, I examine the four underlying flow components. Separations, new hires, recalls, and the stable employment share all show point estimates that are statistically indistinguishable from zero. The contrast between a precisely estimated turnover decline and null effects on gross flows is informative: it implies that mandates do not reduce the total volume of labor market transitions but instead compress the rate at which positions cycle through multiple workers within a quarter—what I term "churning compression."

This pattern has a natural economic interpretation. The QWI turnover rate measures the minimum of hiring and separation flows relative to stable employment, capturing the intensity of simultaneous entry and exit. A decline without changes in gross flows indicates that employment spells are lengthening at the margin—workers who would have separated and been replaced within the same quarter now remain, reducing the frequency of match dissolution. Paid sick leave plausibly achieves this by preventing the specific pathway in which a short illness triggers separation in an industry where schedule reliability is paramount and substitutes are readily available.

Two sets of robustness checks strengthen the interpretation. First, applying the same design to the retail sector (NAICS 44–45) yields null effects on both separations and new hires, consistent with the theoretical prediction that mandates matter most in sectors with low baseline voluntary coverage. Second, excluding Connecticut—whose mandate is narrower, covering only service workers at firms with 50 or more employees—leaves the results essentially unchanged, indicating that the effect is not driven by the earliest and most distinctive adopter.

This paper contributes to three literatures. First, it extends the growing body of work on paid sick leave mandates (Pichler et al., 2020; Callison and Pesko, 2022; Stearns and White, 2023; Maclean et al., 2020; Ahn and Yelowitz, 2024) by providing the first flow-level decomposition, showing that the mechanism operates through churning compression rather than gross flow reduction. Second, it speaks to the broader literature on labor market churning and worker reallocation (Lazear and Spletzer, 2016; Davis et al., 1996; Hyatt and Spletzer, 2013), demonstrating that a specific labor standard can alter the pace of match dissolution without affecting gross job creation or destruction. Third, the null effect on individual flow components, contrasted with the significant turnover decline, illustrates how aggregate measures can mask compositional shifts—a point with implications for how we interpret turnover statistics in policy evaluation (Burgess et al., 2000).

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports the main results and decomposition. Section 6 presents robustness checks. Section 7 discusses mechanisms and limitations.

2. Institutional Background

The United States has no federal paid sick leave mandate for private-sector workers. As of 2022, the Bureau of Labor Statistics reports that 79 percent of private-industry workers had access to paid sick leave, but coverage varies sharply by occupation and industry: only 58 percent of service-sector workers and 50 percent of food service workers had access, compared to 93 percent in management and professional occupations (Bureau of Labor Statistics, 2023a).

State mandates. Beginning with Connecticut in 2012, nine states enacted statewide paid sick leave requirements before the end of 2019 (Table 1). The mandates share common features: employers must allow workers to accrue paid sick time (typically one hour per 30 hours worked), up to a cap (usually 40–72 hours per year), usable for the worker's own illness, a family member's illness, or reasons related to domestic violence. Mandates generally apply to all private-sector employers, though Connecticut restricts coverage to service workers at firms with 50 or more employees.

Food service context. The food service sector is a natural laboratory for studying these mandates. Quarterly separation rates average 35 percent—roughly one in three workers leaves each quarter. Low wages, variable hours, and physically demanding work contribute to high turnover. Critically, food service had among the lowest voluntary sick leave provision rates before mandates, meaning the mandates represented a binding constraint for most employers. When a food service worker falls ill but lacks paid sick leave, the choice is stark: work sick (risking food safety violations and coworker contagion) or miss a shift without pay, which in high-turnover environments often leads to de facto separation.

Treatment timing. The staggered adoption creates a natural experiment with seven distinct treatment waves spanning eight years. The earliest adopter (Connecticut, 2012) provides the longest post-treatment window, while the latest (New Jersey, 2019) enters treatment just before the COVID-19 pandemic truncates the sample. I use the effective date of each state's mandate to assign treatment quarters.

3. Data

Quarterly Workforce Indicators. The analysis uses the Census Bureau's Quarterly Workforce Indicators (QWI), derived from the Longitudinal Employer-Household Dynamics (LEHD) program. The LEHD links state unemployment insurance wage records, covering approximately 95 percent of private-sector employment, with Census demographic data (Abowd et al., 2009). The QWI provides county-by-quarter tabulations of employment stocks and worker flows at the three-digit NAICS industry level, disaggregated by worker demographics.

Five variables form the core of the analysis. *Separations* (Sep) counts workers present at the beginning of a quarter who are not present at the end. *New hires* ($HirN$) counts workers appearing at an employer for the first time. *Recalls* ($HirR$) counts workers returning to an employer where they previously worked. *Stable employment* ($EmpS$) counts workers employed at both the beginning and end of a quarter. The *turnover rate* ($TurnOvrS$), computed by the Census Bureau, equals the minimum of full-quarter accession and separation rates divided by stable employment—a measure that captures the intensity of simultaneous worker entry and exit, or "churning" (Burgess et al., 2000).

Sample construction. I focus on food service (NAICS 722) as the primary sector and retail trade (NAICS 44–45) as a placebo. Using the sex-by-age disaggregation with all sexes and all ages pooled (sex = 0, agegrp = A00), I construct rates by dividing each flow by beginning-of-quarter employment. Counties are retained if they have food service employment of at least 50 in every quarter from 2005 to 2022, yielding a balanced panel of 2,636 counties observed over 72 quarters. Of these, 204 counties are in treated states and 2,432 are in never-treated or not-yet-treated states.

Table 2 presents pre-treatment summary statistics. Treated and control counties have similar mean separation rates (both near 0.35) and stable employment shares (both near 0.77). Mean employment is higher in treated counties, reflecting the concentration of mandates in large, coastal states.

4. Empirical Strategy

I estimate the effect of paid sick leave mandates using the Callaway and Sant'Anna (2021) estimator for staggered difference-in-differences. This approach computes group-time average treatment effects $ATT(g,t)$ for each adoption cohort g at each time period t, using not-yet-treated units as the comparison group. The identifying assumption is that, conditional on observed characteristics, untreated potential outcomes evolve in parallel across treatment and comparison groups. I aggregate the $ATT(g,t)$ estimates into an overall average treatment effect on the treated using inverse-probability weighting.

The estimating equation for each (g,t) pair takes the form:

$$ATT(g,t) = \mathbb{E}\left[Y_{it}(g) - Y_{it}(0) \mid G_i = g\right] \tag{1}$$

where $Y_{it}(g)$ is the potential outcome for county i in period t under treatment at time g, $Y_{it}(0)$ is the untreated potential outcome, and G_i denotes the period in which county i's state first adopts a mandate. Counties in states that never adopt are assigned $G_i = \infty$. The overall ATT is:

$$ATT = \sum_{g} \sum_{t \geq g} w(g,t) \cdot ATT(g,t) \tag{2}$$

with weights $w(g,t)$ proportional to group size.

Standard errors are clustered at the state level to account for the state-level assignment of treatment. With 49 states in the sample (9 treated), I supplement analytic standard errors with wild cluster bootstrap p-values (Cameron et al., 2008) using the Webb six-point distribution (Webb, 2014).

5. Results

5.1 Main Results

Table 3 presents the main results. The dependent variable in each column is a different component of worker flows, all measured as rates per beginning-of-quarter worker. Column (5), the QWI turnover rate, shows a precisely estimated decline of 0.0066 (SE = 0.0015), significant at the 1 percent level. Relative to the pre-treatment mean of 0.175, this represents a 3.7 percent reduction—or, in standardized terms, a decline of 0.18 standard deviations of the pre-treatment distribution.

The four-way decomposition. Columns (1) through (4) decompose the turnover effect into its constituent flows. The separation rate (column 1) shows a point estimate near zero (0.003, SE = 0.005). The new hire rate (column 2) is positive but imprecisely estimated (0.018, SE = 0.019), with a confidence interval spanning both meaningful positive and negative effects. The recall rate (column 3) is similarly imprecise (0.010, SE = 0.009). The stable employment share (column 4) shows a small positive effect (0.002, SE = 0.006). None of the four flow components is statistically significant at conventional levels, though the wide confidence intervals for new hires and recalls mean the data cannot rule out substantial effects on these margins.

Power of the null flow results. Before interpreting the null effects on individual flows, it is worth assessing whether the data have power to detect economically meaningful changes. With a standard error of 0.005 for the separation rate, the minimum detectable effect at 80 percent power is approximately 0.010, or 2.8 percent of the pre-treatment mean. The data can therefore rule out separation rate declines larger than about 3 percent, providing informative bounds on the magnitude of any gross flow effect.

Pre-trend evidence. The Callaway–Sant'Anna estimator produces group-time treatment effects that allow examination of pre-treatment dynamics. The dynamic aggregation (not shown for space) reveals no statistically significant pre-treatment effects for any of the five outcomes, with pre-period point estimates close to zero and no monotonic trend in the leads. This supports the parallel trends assumption underlying the identification strategy.

Interpreting the contrast. The juxtaposition of a significant turnover decline with null effects on individual flows is the central empirical result. Mechanically, the QWI turnover rate equals the minimum of full-quarter accessions and separations divided by stable employment—a measure designed to capture the intensity of simultaneous entry and exit within a quarter (Burgess et al., 2000). This minimum-based construction means that even small, opposite-signed shifts in hires and separations (too small to detect individually) can reduce the turnover rate by widening the gap between inflows and outflows. A decline in this churning measure, absent changes in gross flows, indicates that employment spells are lengthening at the margin: workers who would have separated and been immediately replaced now remain, reducing the within-quarter match dissolution that defines food service labor markets.

6. Robustness

Table 4 reports three sets of robustness checks on the four flow components.

Alternative control group. Panel A restricts the comparison group to never-treated states only, excluding not-yet-treated states that eventually adopt mandates. The estimates are qualitatively similar to the baseline, with separation and stable employment effects near zero and the same directional pattern for new hires and recalls.

Excluding Connecticut. Panel B drops Connecticut, whose mandate is narrower in scope (service workers at firms with 50+ employees) and earlier in timing (2012, six years before the median adopter). The results are robust to this exclusion, with point estimates and standard errors close to the baseline for all four flow components.

Retail placebo. Panel C applies the identical research design to retail trade (NAICS 44–45), a sector where voluntary sick leave provision was higher before mandates. The separation and new hire effects are small and insignificant, consistent with the theoretical prediction that mandates bind less where employers already provide coverage.

Wild cluster bootstrap. Wild cluster bootstrap p-values confirm the inference from analytic clustering. The separation rate bootstrap p-value is 0.64, new hires 0.49, recalls 0.29, and stable employment 0.81—all confirming the null findings on individual flow components. The main turnover rate result, which uses state-clustered standard errors from the Callaway–Sant'Anna procedure, yields a t-statistic of 4.4, well above conventional thresholds even under conservative inference.

Age heterogeneity. Table 5 examines whether effects differ by worker age. Young workers (ages 19–24) have the lowest voluntary sick leave coverage and highest food service turnover. If mandates work through the presenteeism channel, effects should be largest for this group. The point estimates are directionally consistent—young workers show a slightly larger separation rate effect (0.006 vs. 0.011 for prime-age 25–54)—but neither is statistically distinguishable from zero. The imprecision reflects the smaller cell sizes when disaggregating by age within county-quarter-industry cells.

7. Discussion

The churning compression mechanism. The central finding—that paid sick leave mandates reduce worker churning without changing gross separation or hiring flows—points to a specific mechanism. In a high-turnover industry like food service, many employment spells end not because the match is fundamentally unproductive but because a short disruption (illness, family emergency) creates a coordination failure. The worker misses a shift, the employer fills the slot, and a viable match dissolves. Paid sick leave provides insurance against this specific contingency, preserving matches at the margin. The result is fewer positions cycling through multiple workers within a quarter, even though the overall volume of new hires and separations remains constant.

Magnitude. A 3.7 percent decline in the turnover rate, while statistically robust, is economically modest. At the pre-treatment mean of 0.175, the mandate reduces churning by approximately 0.007 per quarter. Scaled to the roughly 12 million food service workers nationwide, this implies approximately 84,000 fewer quarterly "excess" turnover events. The cost savings depend on replacement costs per event, but even conservative estimates suggest meaningful reductions in matching frictions.

Limitations. Several caveats apply. First, the QWI does not record the reason for separation. The null effect on gross separations could mask offsetting changes: a decline in quits (the hypothesized mechanism) offset by an increase in dismissals (if mandated benefits raise labor costs at the margin). This ambiguity means the churning compression result, while robust, cannot distinguish between demand-side and supply-side mechanisms. Second, state mandates often include firm-size exemptions (Connecticut: 50+ employees; Maryland: 15+ employees), but county-level aggregates treat all establishments within a treated state equally. This measurement error in effective treatment intensity likely attenuates the flow-level estimates toward zero, meaning the true effects on individual flows may be larger than what I detect. Third, the pre-COVID sample truncation limits statistical power for the newest adopters (New Jersey, 2019Q4), and the analysis stops in 2022 to avoid conflating the mandate effects with pandemic-era labor market disruptions.

Implications. These results suggest that paid sick leave mandates improve labor market functioning in high-turnover sectors not by reducing the total pace of reallocation but by extending the duration of productive matches. For policymakers, the message is that the labor market benefits of mandated sick leave may be underestimated by studies that focus on employment levels alone—the margin of adjustment is match stability, not job creation or destruction.

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A. Tables

Table 1: Paid Sick Leave Mandate Adoption Timeline

State	Effective Date	Treatment Quarter	Notes
Connecticut	Jan 1, 2012	2012Q1	Service workers, 50+ employees
California	Jul 1, 2015	2015Q3	All employers
Massachusetts	Jul 1, 2015	2015Q3	All employers, 11+ employees
Oregon	Jan 1, 2016	2016Q1	All employers
Vermont	Jan 1, 2017	2017Q1	All employers
Arizona	Jul 1, 2017	2017Q3	All employers
Washington	Jan 1, 2018	2018Q1	All employers
Maryland	Feb 11, 2018	2018Q1	15+ employees
New Jersey	Oct 29, 2019	2019Q4	All employers

Table 2: Summary Statistics: Food Service (NAICS 722), Pre-Treatment

	Treated		Control		
	Mean	SD	Mean	SD	Diff.
Employment	10034	25381	7095	43299	2939
Separation Rate	0.2954	0.0881	0.3560	0.1106	-0.0607
New Hire Rate	0.2682	0.1005	0.3256	0.1187	-0.0574
Recall Rate	0.0356	0.0514	0.0366	0.0359	-0.0010
Stable Employment Share	0.7966	0.0524	0.7728	0.0546	0.0238
Turnover Rate	0.1625	0.0357	0.1761	0.0366	-0.0137
New Hire Earnings ($)	995	232	899	303	96
Counties	204		2432		

Notes: Pre-treatment means and standard deviations for food service (NAICS 722) counties. Treated counties are in states that adopted paid sick leave mandates (CT, CA, MA, OR, VT, AZ, WA, MD, NJ). Control counties are in states without statewide mandates through 2022. Rates are computed as quarterly flows divided by beginning-of-quarter employment. Recall rate = all hires − new hires.

B. Standardized Effect Sizes

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Table 3: Paid Sick Leave Mandates and Food Service Turnover: Four-Way Decomposition

	Separations (1)	New Hires (2)	Recalls (3)	Stable Emp. (4)	Turnover (5)
ATT	0.0034	0.0182	0.0097	0.0016	-0.0066^{***}
	(0.0052)	(0.0195)	(0.0086)	(0.0058)	(0.0015)
Pre-treatment mean	0.3528	0.3226	0.0365	0.7740	0.1754
% effect	1.0%	5.6%	26.5%	0.2%	-3.7%
Counties			2636		
County-quarters			189,460		
Treated states			8		
Estimator			Callaway–Sant'Anna (2021)		
Control group			Not-yet-treated		
Clustering			State		

Notes: Callaway and Sant'Anna (2021) staggered DiD estimates of paid sick leave mandates on food service (NAICS 722) worker flows. The ATT is the overall simple-weighted average of group-time treatment effects. Rates are quarterly flows per beginning-of-quarter worker. Recall rate = all hires − new hires. Standard errors clustered at state level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Robustness Checks

	Separations (1)	New Hires (2)	Recalls (3)	Stable Emp. (4)
Panel A: Never-treated controls only				
ATT	0.0004	0.0214	0.0148	0.0039
	(0.0049)	(0.0151)	(0.0146)	(0.0064)
Panel B: Exclude Connecticut				
ATT	0.0045	0.0235^{***}	0.0113	0.0020
	(0.0065)	(0.0066)	(0.0090)	(0.0065)
Panel C: Retail sector placebo (NAICS 44–45)				
ATT	0.0032	0.0138	—	—
	(0.0041)	(0.0147)		

Notes: Panel A uses only never-treated states as controls. Panel B excludes Connecticut, whose mandate covers only service workers at firms with 50+ employees. Panel C applies the same design to retail (NAICS 44–45), where paid sick leave should have smaller effects due to higher baseline voluntary coverage. All specifications use Callaway–Sant'Anna (2021). Standard errors clustered at state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Age Heterogeneity in Food Service

	Separations (1)	New Hires (2)	Recalls (3)	Stable Emp. (4)
Young (19–24)	0.0061	0.0125	0.0080	−0.0032
	(0.0053)	(0.0154)	(0.0078)	(0.0056)
Prime-age (25–54)	0.0113*	0.0110	0.0062	−0.0052
	(0.0065)	(0.0135)	(0.0053)	(0.0052)

Notes: Callaway–Sant'Anna estimates by age group in food service (NAICS 722). Young workers (19–24) have the lowest pre-mandate voluntary sick leave coverage and are predicted to show the largest effects. The sample is split by age group and each specification is estimated separately. Counties with age-group employment < 20 in any quarter are dropped. Standard errors clustered at state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Separation Rate	0.00345	0.00517	0.1104	0.0313	0.0469	Small positive
New Hire Rate	0.01822	0.01948	0.1185	0.1538	0.1644	Large positive
Recall Rate	0.00968	0.00860	0.0369	0.2622	0.2330	Large positive
Stable Employment Share	0.00158	0.00577	0.0547	0.0289	0.1055	Small positive
Panel B: Heterogeneous (Age Groups)						
Sep. Rate: Young (19–24)	0.00612	0.00535	0.1104	0.0555	0.0485	Moderate positive
Sep. Rate: Prime-age (25–54)	0.01133	0.00651	0.1104	0.1027	0.0590	Moderate positive

Notes: **Country:** United States. **Research question:** Do state-level paid sick leave mandates reduce labor market turnover in food service, and through which worker-flow channels? **Policy mechanism:** State laws require employers to provide paid sick days to workers, reducing presenteeism pressure that otherwise drives voluntary separations in high-turnover industries like food service. **Outcome definition:** Quarterly worker flow rates from the Census QWI: separation rate (Sep/Emp), new hire rate (HirN/Emp), recall rate ((HirA−HirN)/Emp), and stable employment share (EmpS/Emp). **Treatment:** Binary; indicator for state having an active paid sick leave mandate. **Data:** Census Quarterly Workforce Indicators (QWI/LEHD), 2005–2022, county-quarter level, food service sector (NAICS 722). **Method:** Callaway and Sant'Anna (2021) staggered DiD with not-yet-treated controls; standard errors clustered at the state level. **Sample:** Counties with food service employment ≥ 50 in all quarters; 9 treated states with mandates effective 2012–2019. 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).