Simply Can't Wait: Evaluating the Effect of California's Fast-Food Minimum Wage Increase

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October 2025

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Abstract

This paper estimates short-run employment effects of California's sectoral minimum wage for fast-food restaurants. On April 1, 2024, covered limited-service chains faced a $20 hourly floor while the statewide minimum wage was $16 (up from $15.50 in 2023). Using establishment-level mobility data and a difference-in-differences design restricted to "clean" counties without local wage ordinances, I proxy on-site staffing with weekly long-duration device visits (¿4 hours). Baseline estimates indicate an average 8% decline in on-site employment at treated outlets relative to comparable full-service and retail establishments. An event-study shows no pre-trends and a gradual, persistent post-announcement decline, consistent with anticipatory staffing adjustments. Placebo estimates for exempt "enclosed" venues are null, and effects are similar across urban and rural locations. The results imply that a sectoral wage floor induces meaningful reductions in labor utilization on intensive margins (hours, shift density) rather than immediate large-scale job loss.

Keywords— Minimum wage; Fast food; Labor demand; Employment; California; Difference-in-differences

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1 Introduction

In April 2024, as California implemented a $20 minimum wage for fast-food workers, national media described an industry bracing for change. The *New York Times* reported franchise owners trimming hours and experimenting with automation, while workers expressed both optimism about higher pay and concern over lost shifts.[1] Yet much of the adjustment was already underway. When California lawmakers announced Assembly Bill (AB) 1228 on September 28th, 2023 establishing a sectoral wage floor for fast-food employees, the legislation immediately created a binding future cost shock for a narrowly defined set of firms.

AB-1228 applied only to limited-service restaurants that are part of chains with 60 or more establishments nationwide, deriving at least half of their revenue from food or beverages prepared for immediate consumption. Smaller chains, in-store bakeries selling bread as a standalone item, and outlets operating inside grocery stores/hotels/airports exceeding 15,000 square feet were explicitly exempt[2]. The law also created the *California Fast Food Council*, authorized to set annual wage adjustments (up to 3.5 percent) and additional employment standards. These provisions make the policy unusually sharp and heterogeneous in its coverage: large franchises faced a predictable cost increase, while nearby exempt outlets did not.

Because of its relative novelty, this policy has already attracted empirical attention. Reich and Sosinskiy (2024) examine AB-1228's effects on fast-food wages and prices using early CES data, reporting no adverse employment impact, though later CES revisions suggest more negative patterns. Sovich and Hamdi (2025) use anonymized Equifax payroll data covering about 5,000 large U.S. employers and find offsetting declines in turnover and hiring that yield modest net effects on employment. Clemens et al. (2025), also presents an interesting analysis with a negative effect on employment. Together, these studies rely on aggregate or firm-level administrative data and focus primarily on post-implementation outcomes.

This paper extends that evidence by analyzing firms' anticipatory labor-market responses following the 2023 announcement of AB-1228. Using establishment-level mobile-device location data, I track on-site staffing through long-duration mobile-device visits to capture daily

[1] Greenhouse, S. (2024, March 28). "California's $20 fast-food wage: A turning point for low-wage work." *The New York Times*. Retrieved from https://www.nytimes.com/2024/03/28/business/economy/fast-food-minimum-wage-california.html.

[2] California - Assembly Bill No. 1228 https://leginfo.legislature.ca.gov/faces/billNavClient.xhtml?bill_id=202320240AB1228

employment activity. Because the law's treatment definition depends on chain affiliation and business model rather than geography alone, high-frequency, location-specific data are essential for distinguishing treated from exempt outlets and observing real-time adjustments in labor inputs. I further benchmark these responses against other low-wage, high-employment sectors such as full-service dining and retail trade that employ many minimum-wage workers but were unaffected by AB-1228. This approach offers new insight into how employers adjust staffing not only after a wage increase, but in anticipation of one, highlighting the value of mobility-based data in evaluating sector-specific labor policies.

2 Data

The analysis uses establishment-level mobility records from Advan Research (2025), which aggregates anonymized mobile-device GPS pings from over five million commercial Points of Interest (POIs) across the United States. Each establishment-week observation reports total visit counts, average dwell durations, and identifiers including brand, NAICS classification, and geographic coordinates. These data enable near real time measurement of customer and employee presence at specific business locations.

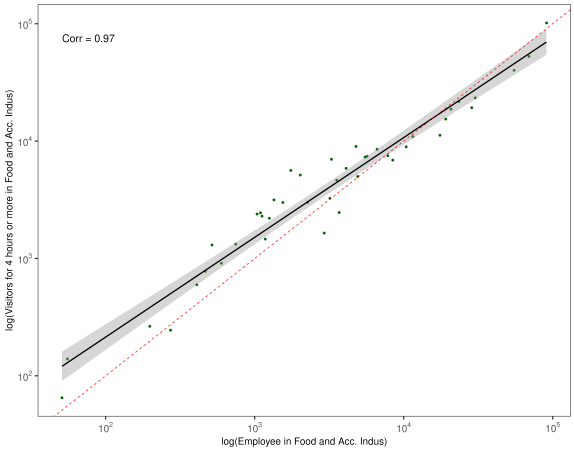
The focus sample isolates standalone fast-food restaurants (NAICS 722513), the segment directly affected by California's fast-food wage mandate under AB-1228. For comparison, I also construct panels for full-service restaurants (NAICS 722511) and retail establishments (NAICS 445110, 445120 and 445310), which employ a large share of minimum-wage workers but were not subject to the law. I use balanced panel from January 2023 until Decmber 2024 of Advan's temporal reporting.

2.1 Employment Proxy Validation

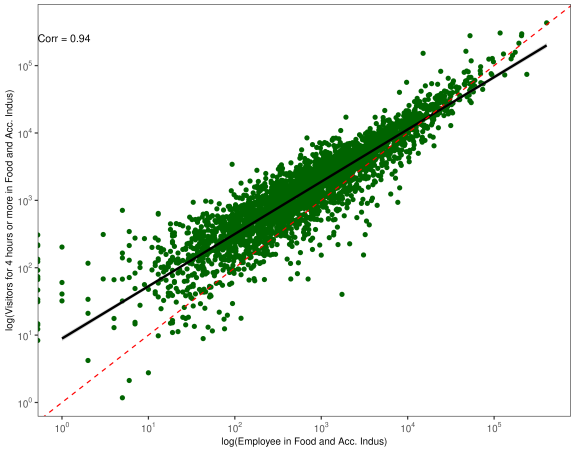
Following Pandit (2025), I use long-duration visits stays exceeding four hours as a proxy for on-site employment activity. To validate this measure, I correlate 2022 county-level long-duration visits with official employment counts from the U.S. Census Bureau's Longitudinal Employer–Household Dynamics (LEHD) Workplace Area Characteristics (WAC) data. Across the accommodation, food service, and retail sectors, correlations exceed 0.97, indicating that long-duration visits closely track local employment levels and provide a reliable, high-frequency

proxy for labor presence at the establishment level.

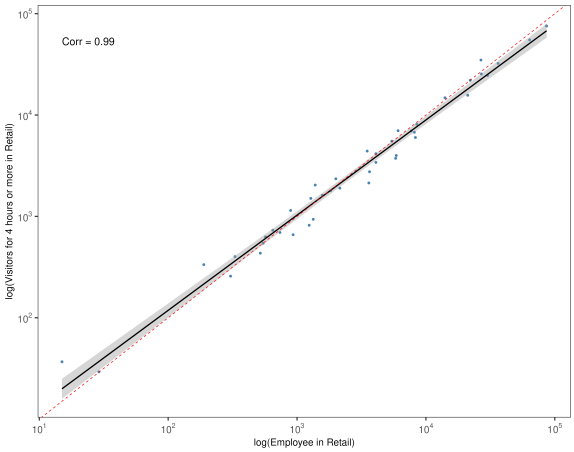
Figure 1: Validation of Long-Duration Visits as Employment Proxy: Accommodation & Food Services and Retail Trade



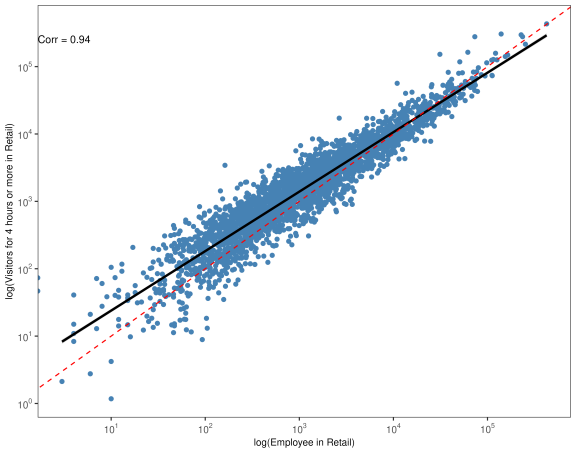
(a) CA Counties with No MW ordinance



(b) All the U.S. counties



(c) CA Counties with No MW ordinance



(d) All the U.S. counties

Note: *Each point represents a county. The x-axis shows the log of LEHD employment, and the y-axis shows the log of long-duration visits (>4 hours). The solid black line shows the fitted regression; the red dashed line is the 45° line. (a) and (b) uses the LEHD-WAC, Food & Accommodation (CNS18) Employment numbers and (c) and (d) uses the LEHD-WAC Retail Trade (CNS07) Employment numbers. Correlations exceed 0.97 in both samples.*

2.2 Descriptive Statistics

Table 1, summarizes weekly log long-duration visits a proxy for establishment-level employment activity—across major service-sector industries in clean counties that follow only state-level minimum wage laws. The sample includes over 3 million store-week observations, predominantly from full-service (NAICS 722511) and limited-service (NAICS 722513) restaurants.

Average logged employment activity is highest in grocery stores and limited-service restaurants, with mean log values of 1.31 and 0.71, respectively. Across space, establishments located in Census Block Groups (CBGs) classified as urban, using the U.S. Census Bureau's urban area boundaries via the `tigris` package, show higher log employment intensity than those in rural areas in most sectors. For instance, grocery stores average 1.41 in urban CBGs versus 1.10 in rural CBGs, and full-service restaurants average 0.52 versus 0.49. The exception is limited-service restaurants, where rural outlets exhibit higher logged employment activity (0.91) than urban outlets (0.64), potentially reflecting more stable staffing levels relative to customer density in rural areas.

Overall, the descriptive evidence indicates that weekly on-site employment activity, as captured by log long-duration visits, is more concentrated in urban markets but varies meaningfully across sectors. These differences motivate the subsequent difference-in-differences analysis, which isolates the causal effect of state-level minimum wage adjustments on establishment employment.

Table 1: Employee Visitors Summary Statistics by Urban and Rural Areas

NAICS	**Description**	**Total**	**Urban**	**Rural**
445110	Grocery Stores	1.31	1.41	1.10
		(2.15)	(2.19)	(2.04)
		488,460	333,585	154,875
445120	Convenience Stores	1.08	1.01	1.26
		(1.87)	(1.82)	(1.97)
		200,760	146,685	54,075
445310	Beer, Wine & Liquor Stores	0.555	0.566	0.512
		(1.46)	(1.47)	(1.42)
		67,410	53,025	14,385
722511	Full-Service Restaurants	0.515	0.523	0.487
		(1.43)	(1.44)	(1.40)
		1,322,790	1,006,215	316,575
722513	Limited-Service Restaurants	0.705	0.635	0.908
		(1.60)	(1.53)	(1.78)
		1,055,250	785,295	269,955

Note: *Each NAICS code category displays three rows. The first row shows the mean log employee visits; the second row shows the standard deviation in parentheses; the third row shows the sample size (N). The Total column includes all observations, while Urban and Rural columns represent stratified subsamples based on geographic location.*

3 Empirical Strategy

To estimate the causal effect of California's fast-food sectoral minimum wage on employment, I exploit cross-sector variation using a difference-in-differences (DiD) framework. The analysis is restricted to "clean" counties those that adhere strictly to state-level minimum wage schedules and did not enact local ordinances to ensure that policy exposure is driven solely by the statewide mandate. The baseline specification is:

$$\log(\text{Employee}_{ist}) = \alpha_i + \gamma_t + \beta_1 \text{Treatment}_{ist} + \varepsilon_{ist}, \tag{1}$$

where $\log(\text{Employee}_{ist})$ denotes the log of long-duration employee visits at establishment i in sector s and week t. The treatment indicator, Treatment_{ist}, equals one for limited-service restaurants (NAICS 722513) in weeks after the September 28, 2023, announcement of Assembly Bill 1228 (AB-1228), and zero otherwise. The coefficient β_1 captures the relative change in on-site employment among fast-food outlets compared with full-service restaurants and retail establishments located in the same counties over time. Establishment fixed effects (α_i) absorb time-invariant differences across stores such as brand or location, while week fixed effects (γ_t) control for aggregate shocks common to all sectors. Standard errors are clustered at the establishment level to account for serial correlation in weekly employment outcomes.

This "within-county, across-sector" comparison isolates the effect of the fast-food wage mandate from broader economic fluctuations or state-level trends. Because the sample excludes municipalities with their own wage ordinances, identification relies on the differential exposure of fast-food outlets to the AB-1228 policy shock relative to similar establishments unaffected by the sectoral law.

To assess the validity of the parallel-trends assumption and explore dynamic adjustment, I estimate an event-study version of Equation (1) in which the treatment indicator is replaced with a set of relative-time indicators for each week before and after the announcement of AB-1228:

$$\log(\text{Employee}_{ist}) = \sum_{k \neq -1} \delta_k \, \mathbf{1}\{\text{WeekRel}_{st} = k\} \times \text{Treatment}_{is} + \alpha_i + \gamma_t + \varepsilon_{ist}, \tag{2}$$

where WeekRel_{st} indexes the number of weeks relative to the announcement date, and $k = -1$ is the omitted reference week. The coefficients δ_k trace the dynamic evolution of

the treatment effect over time. Statistically insignificant estimates for $k < 0$ indicate the absence of pre-trends, supporting the identifying assumption. The post-treatment coefficients ($k \geq 0$) describe how establishment-level employment adjusts in the weeks following the wage announcement, capturing both the immediate and evolving responses to the policy shock.

4 Results

Table 2, presents the baseline estimates from the difference-in-differences specifications using log weekly long-duration employee visits as a proxy for on-site employment activity. Across all model variations, the estimated coefficients on the treatment indicator are negative and statistically significant at conventional levels, indicating a consistent reduction in employment following the introduction of California's sectoral minimum wage for fast-food establishments.

In the full sample (Column 1), the estimated coefficient of -0.083 implies that weekly on-site employment declined by roughly 7.9 percent relative to comparable establishments in the same counties after the AB-1228 announcement. Restricting the sample to retail and dining sectors (Column 2) yields nearly identical estimates, suggesting that the result is not driven by sectoral composition.

Column 3 provides a placebo-style check using a subset of fast-food outlets located within enclosed environments such as grocery stores, airports, and hotels venues exempt from AB-1228. The positive but insignificant coefficient (0.055) for this group supports the validity of the identification design, as no treatment effect should be expected among exempt locations.

Columns 4 and 5 restrict the sample to fast-food and full-service restaurants only. The estimated employment response remains around -0.08, reinforcing that the decline in staffing intensity is concentrated within fast-food establishments directly subject to the policy. The interaction term between treatment and urban location in Column 5 is small and statistically insignificant, indicating no differential response between urban and rural establishments. Overall, the results reveal a consistent and economically meaningful decline in on-site employment intensity of 7–8 percent following the wage increase, with similar magnitudes across sectors and geographic contexts.

Table 2: Effect of the CA Fast-Food Minimum Wage on Log Employment

	(1)	(2)	(3)	(4)	(5)
Treatment	-0.0831***	-0.0817***	0.0548	-0.0838***	-0.0728***
	(0.0109)	(0.0139)	(0.0371)	(0.0115)	(0.0182)
Treatment × Urban					-0.0077
					(0.0199)
Fixed Effects:					
Establishment (α_i)	Yes	Yes	Yes	Yes	Yes
Week (γ_t)	Yes	Yes	Yes	Yes	Yes
Observations	3,134,670	1,811,880	739,186	2,378,040	2,378,040
R^2	0.498	0.519	0.695	0.420	0.420
Within R^2	0.00024	0.00023	0.00011	0.00029	0.00027

Note: *Dependent variable is* log*(weekly visitors with duration > 4 hours). Standard errors clustered at the establishment level. Columns: (1) all sectors; (2) retail and food; (3) exempt enclosed fast-food sample; (4) dining only; (5) urban–rural heterogeneity.* *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.1 Dynamic Event-Study Estimates

Figure 2, visualizes the dynamic treatment effects from the event-study specification estimated on the same sample as Columns 4 and 5. Each coefficient represents the estimated difference in log on-site employment between treated (fast-food) and control (full-service) establishments relative to one week before the policy announcement.

The pre-announcement coefficients fluctuate narrowly around zero, showing no systematic upward or downward trend prior to September 2023. This absence of pre-trends supports the parallel-trends assumption underlying the difference-in-differences framework. In the weeks immediately following the announcement, coefficients begin to decline modestly and become increasingly negative as the April 2024 implementation date approaches. After the law's enactment (marked by the second vertical line), the decline stabilizes around 0.07–0.10 log points, mirroring the average treatment effect reported in Table 2.

Overall, the dynamic estimates indicate that the employment effects emerged gradually rather than instantaneously consistent with firms adjusting staffing levels in anticipation of higher labor costs. The close alignment between the event-study path and the static difference-in-differences estimates suggests that the results are robust to alternative specifications and that the observed contraction in on-site employment reflects a genuine policy response rather

than spurious correlation.

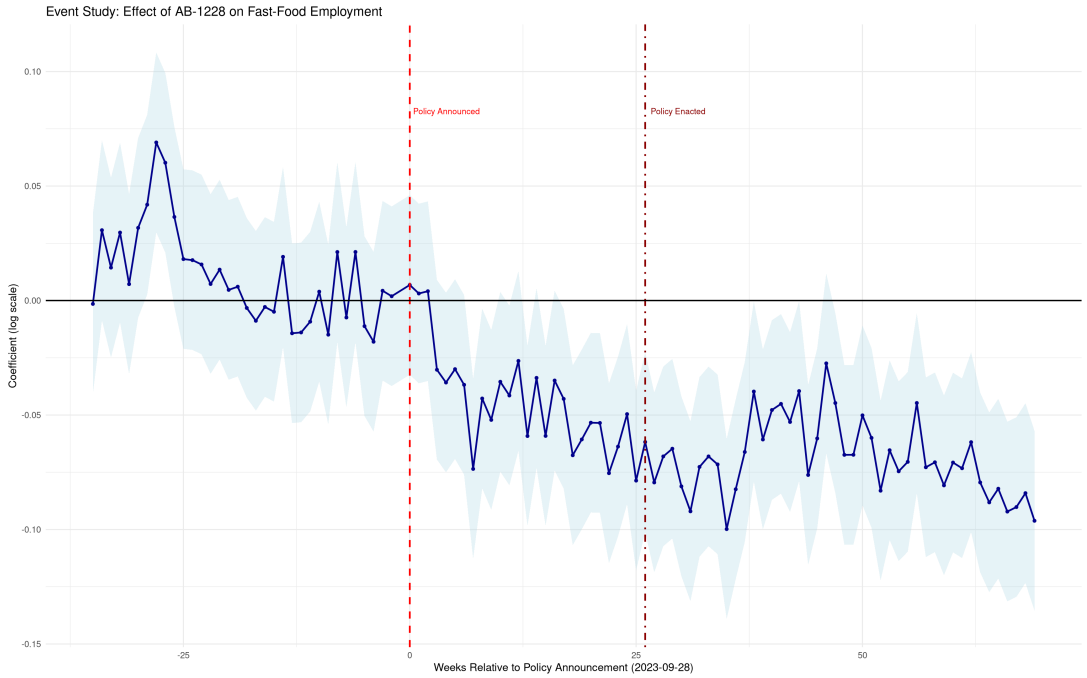


Figure 2: Dynamic Event Study: Effect of AB-1228 on Log Employment

Note: *Compared to the full-dinning restaurants establishments, The blue line represents estimated log-point changes in long-duration visits by week relative to the policy's implementation. Shaded bands denote 95% confidence intervals. The red dashed line indicates September 28, 2023 and Maroon dashed line indicates April, 2024.*

5 Discussion and Conclusion

The results show that California's fast-food sectoral minimum wage (AB-1228) led to a measurable contraction in on-site employment activity. Across specifications, treated outlets exhibit an average decline of about 8 percent in weekly long-duration employee visits following the September 2023 announcement, with similar magnitudes persisting through the April 2024 enactment. The effect is consistent across restricted samples and disappears for exempt (enclosed) venues, strengthening the causal interpretation.

Dynamic event-study estimates reveal no pre-trends and a gradual, sustained decline in staffing intensity beginning shortly after the announcement. This pattern points to anticipatory adjustment firms reorganized shifts and reduced staffing density in advance of the binding wage floor. Such behavior echoes findings by Clemens and Strain (2020), who show that employers respond to higher wage floors by altering scheduling intensity rather than by immediate layoffs.

The absence of urban–rural heterogeneity indicates that adjustments were widespread across market types, consistent with standardized franchise operations that face uniform labor-cost constraints. The null effect for exempt outlets within grocery stores or airports suggests limited spillover or substitution toward non-covered establishments. Together, these results imply that AB-1228's labor-market effects were both anticipatory and sector-specific, concentrated within large chain restaurants directly subject to the law.

In magnitude, the estimated 7–10 percent decline in on-site employment is moderate but economically meaningful given the short implementation window and the policy's narrow scope. It underscores that sectoral wage setting can generate targeted yet significant adjustments in labor utilization without broad displacement effects. Similar substitution and scheduling responses have been documented for other minimum-wage contexts Hsing (2000), and recent evidence on predictive scheduling laws Yelowitz (2022) likewise suggests that firms adjust labor inputs on intensive rather than extensive margins.

Overall, the findings suggest that employers respond to binding wage mandates by re-optimizing labor allocation, not through abrupt employment cuts. This anticipatory behavior implies that policy evaluations relying only on post-implementation data may underestimate total adjustment. Future work linking mobility-based staffing measures with payroll and price data could clarify whether these contractions reflect reduced hours, higher productivity, or automation investments.

By leveraging high-frequency mobility data, this study contributes new evidence on the short-run adjustment mechanisms in a sectoral minimum-wage regime a design that concentrates the policy shock in a single industry, offering a sharper test of firm-level responses. The results highlight both the efficiency of targeted wage policy and the importance of understanding real-time, anticipatory firm behavior in assessing its labor-market consequences.

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