Who Pays for Higher Wages? Evidence from Consumer Spending after California's Fast-Food Minimum Wage Increase

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Abstract

This paper examines how California's 2024 fast-food minimum wage ($20 under AB-1228) affected consumer behavior. Using high-frequency transaction data from ten national chains and a difference-in-differences design, I find that customer visits and transactions fell by 6–7 percent, while revenue declined by about 5 percent, implying higher spending per visit and partial price pass-through. Income-resolved data show sharp redistribution: higher-income consumers spend 2–3 percent more per visit despite visiting less, whereas low-income customers show little change. Distinguishing composite from income-specific effects reveals that consumer-side incidence is heterogeneous and borne disproportionately by higher-income households.

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1 Introduction

Minimum wage policy is one of the most widely used and contested labor market interventions in the United States. A large empirical literature has asked whether raising the wage floor reduces employment, compresses wage distributions, or reshapes firm behavior (Allegretto et al., 2017; Dube, 2019; Neumark & Wascher, 2007). Much less is known about how these policies affect the product market on the demand side. A higher minimum wage is not only a change in labor market institutions; it is also a targeted cost shock to covered firms. When those firms respond by adjusting prices, product quality, or service intensity, the final incidence of the policy depends critically on how consumers react.

Existing work on the price channel finds that minimum wage increases are partially passed through to consumers. Using restaurant and retail prices, studies typically estimate that a 10 percent increase in the minimum wage raises prices by roughly 0.4–0.7 percent (Aaronson, 2001; Aaronson & French, 2007; Aaronson et al., 2008), with the degree of pass-through varying by market structure, expectations, and policy design (MacDonald & Nilsson, 2016). More recent theoretical and empirical contributions emphasize that wage hikes can also shift demand. In Leung (2021), higher earnings for low-wage workers reduce demand elasticities and induce firms to raise markups, generating "demand-induced" feedbacks that amplify effective pass-through in markets where the minimum wage binds more tightly.

These insights highlight a central gap. Price changes alone do not tell us whether, and for whom, higher wages translate into higher costs of consumption. If menu prices rise and low-income customers substantially reduce visits while higher-income customers absorb the increase, the consumer-side incidence is regressive even if low-wage workers benefit on the earnings margin. Conversely, if low-income workers who gain from the minimum wage increase their restaurant demand, firms may be able to raise prices without substantial reductions in quantity, changing the welfare calculus. Understanding who pays for higher wages therefore requires evidence on consumer behavior and its distribution across income groups.

This paper brings consumer behavior to the center of minimum wage incidence by studying California's 2024 fast-food minimum wage law (AB-1228), which raised the minimum wage to $20 per hour for workers at large, chain-affiliated limited-service restaurants. This reform was both large and highly targeted, creating a clear shock to labor costs within a narrow, labor-intensive sector. I combine this institutional setting with high-frequency transaction microdata from SafeGraph (2022)'s *Spend*, which aggregates anonymized debit and credit card purchases at brick-and-mortar outlets across the United States. For each restaurant, the data report monthly total spending, transaction counts, and the number of unique customers, as well as the distribution of spending and customers across seven household income brackets. I construct a balanced panel of outlets belonging to ten major national fast-food chains including McDonald's, Taco Bell, Burger King, and others with operations in California and throughout the rest of the United States.

Empirically, I implement a difference-in-differences design comparing treated outlets in California to outlets from the same chains in other states. All specifications include establishment and calendar time fixed effects, and preferred versions control for county-level median wages and unemployment rates, as well as city fixed effects. I estimate both *composite effects*, which summarize changes in overall demand at treated outlets, and *income-specific effects*, which disaggregate spending and customer responses by household income group. Event-study estimates validate the parallel-trends assumption and reveal no evidence of anticipatory behavior at the announcement date.

Four main findings emerge. First, aggregate activity at treated fast-food outlets contracts meaningfully: relative to comparable outlets in other states, customer counts and transaction volumes fall by 6–7 percent after the April 2024 implementation date, and total card-present spending declines by roughly 5 percent. Second, because revenue falls less than visits and transactions, average spending per remaining customer increases in the wake of the wage hike, consistent with partial pass-through of higher labor costs to prices. Third, these composite effects mask substantial heterogeneity across the household income distribution. Spending by consumers in middle- and higher-income households (above \$75,000) increases by 2–3 percent, while customer counts for these groups fall by 4–6 percent. Low-income customer counts, by contrast, remain roughly unchanged, with modest spending gains. Finally, the pattern of fewer customers, lower transactions, and only modestly lower total spending implies that the composition of demand shifts toward higher-spending, higher-income consumers.

These findings motivate a distinction between two objects that are often conflated in empirical work. The *composite effect* captures the net change in total spending or visits at treated outlets, aggregating across all income groups. The *income-specific effect* isolates how each income group adjusts its spending and visitation, holding the composition of income groups fixed. In this setting, the composite effect points to moderate declines in overall demand, while the income-specific effects reveal a redistribution of consumption opportunities and burdens across the income distribution.

To interpret these patterns, I develop a simple imperfect-competition framework in which firms set markups over marginal cost and face consumers with heterogeneous incomes and demand elasticities. Building on pass-through theory from Weyl and Fabinger (2013) and demand-induced feedbacks from Leung (2021), the framework highlights two channels. First, a *cost-based pass-through* channel raises prices as higher wages increase marginal costs. Second, a *demand-induced* channel operates when higher wages raise the incomes of low-wage consumers, shifting the composition of buyers toward less price-sensitive households and increasing optimal markups. Both channels operate simultaneously, and both are mediated by the composition of demand.

By connecting a well-defined sectoral wage shock to rich, income-resolved transaction data, this paper contributes to the broader literature on minimum wage incidence and pass-through. It shows that consumer-side incidence is heterogeneous and mediated by demand composition: in this setting, higher-income households bear a larger share of the consumer burden, while low-income households are partially insulated on the product-market side. More broadly, the results underscore that a complete incidence analysis of minimum wage policies requires information on product-market responses, not just employment and prices.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting of AB-1228 and reviews related research on minimum wages, price pass-through, and consumer incidence. Section 3 presents a conceptual framework linking cost shocks, demand curvature, and income heterogeneity in demand. Section 4 describes the SafeGraph data, sample construction, and descriptive statistics. Section 5 outlines the empirical strategy, including difference-in-differences and event-study designs, and clarifies the distinction between composite and income-specific effects. Section 6 presents the main results. Section 7 reports robustness checks and falsification exercises. Section 8 discusses mechanisms and welfare implications. Section 9 concludes the findings and limitations..

2 Background

Effective April 1, 2024, California's AB-1228 raised the minimum wage to $20 per hour for workers at fast-food restaurants affiliated with national chains of at least 60 outlets. The law applied primarily to limited-service restaurants (NAICS 722513) often termed "quick-service" or "fast-food" restaurants and exempted full-service restaurants, small independent outlets, and other retail sectors. The change represented a roughly 25 percent increase over the statewide minimum wage of $16, which had itself risen steadily in previous years.

Two features of AB-1228 are particularly important for identification. First, treatment is defined at the intersection of sector and chain status rather than geography alone: only chain-affiliated fast-food outlets in California are directly affected. This allows for comparisons both within California (treated versus non-treated sectors) and across states (treated chains in California versus the same chains in other states). Second, the policy was announced well before implementation. The law was signed in 28th September 2023, with substantial public discussion and industry lobbying. Nonetheless, as I show in event-study analyses, there is little evidence of anticipatory adjustments in customer flows or transactions prior to the implementation date.

The fast-food sector provides a particularly useful setting for studying the incidence of minimum wage policies. First, it is labor-intensive: wages and benefits constitute a large share of total costs, so minimum wage increases directly raise marginal costs. Second, products are relatively standardized, menu prices are frequently observed, and consumers can substitute across outlets and chains at low cost, making pass-through empirically observable. Third, fast-food restaurants serve consumers across the income distribution, from budget-constrained households to higher-income consumers seeking convenience. As a result, changes in prices or service intensity can differentially affect demand by income.

This paper connects to several strands of research. First, a vast literature studies the employment effects of minimum wage increases, leveraging state and local variation to estimate quasi-experimental impacts on employment, hours, and labor force participation (Neumark & Shirley, 2022). While early work often found negative employment effects, more recent studies show smaller or zero average effects, with heterogeneity by sector, age, and labor market conditions (Cengiz et al., 2019). This literature establishes the labor-market side of minimum wage incidence but typically leaves consumer behavior implicit. A second literature investigates how firms adjust prices and other margins in response to minimum wage hikes. Aaronson (2001), Aaronson and French (2007), and Aaronson et al. (2008) use restaurant and retail price data to estimate wage price elasticities, finding partial pass-through concentrated in the month of implementation. MacDonald and Nilsson (2016) show that pass-through is attenuated when minimum wage increases are more predictable or indexed. Other work highlights firm adjustments in product quality, service intensity, and worker scheduling.

More recent theoretical work emphasizes that minimum wages can alter demand as well as costs. Leung (2021) embeds a minimum wage into an imperfect-competition model with heterogeneous consumers and shows that higher low-wage earnings can reduce demand elasticity for some consumers, inducing firms to raise markups and amplifying effective pass-through. This mechanism depends on the distribution of incomes among consumers and on the curvature of demand. The empirical evidence in this paper speaks directly to these demand-side channels by documenting how spending and visit patterns by income group change after a wage hike.

This paper relates to the broader literature on tax and cost pass-through in imperfectly competitive markets. Weyl and Fabinger (2013) show that pass-through depends on the cost share of the taxed input and on demand curvature, and they decompose pass-through into mechanical and strategic components. Minimum wage policies can be viewed as a specific form of input cost shock whose pass-through depends on labor shares and demand curvature, and whose incidence depends on the distribution of demand across consumers.

By bringing rich, income-resolved transaction data to bear on a well-defined sectoral wage shock, this paper advances these literatures in two ways. First, it provides direct evidence on changes in consumer demand and the composition of customers at treated outlets, linking wage policy to demand-side incidence. Second, it quantifies how these changes differ by income, illuminating the distribution of consumer burdens across the income distribution.

3 Theoretical Framework

This section develops a partial-equilibrium framework that rationalizes the empirical patterns documented later in the paper namely, (i) a decline in total customer visits and transactions, (ii) a smaller decline in total spending, and (iii) heterogeneous responses across the income distribution. The objective is not to structurally estimate demand or markups, but to clarify how a sector-specific minimum wage shock affects pricing, quantities, and incidence in an imperfectly competitive market with heterogeneous consumers. The framework synthesizes insights from Weyl and Fabinger (2013), Bulow and Pfleiderer (1983), and the "demand-induced pass-through" channel emphasized by Leung (2021).

Consider a representative fast-food outlet operating in an imperfectly competitive market. Let P denote the posted price (or bundle of menu prices), and let total quantity demanded be

$$Q(P, \theta) = \sum_{g \in \mathcal{G}} Q_g(P),$$

where $\mathcal{G}$ indexes household income groups and θ denotes the income distribution of customers. Each group g faces its own demand function $Q_g(P)$ and price elasticity $\varepsilon_g(P)$, with lower-income households typically exhibiting higher elasticities. The composite elasticity faced by the firm is

$$\varepsilon(P, \theta) = \frac{\sum_g PQ'_g(P)}{\sum_g Q_g(P)}.$$

A key feature, consistent with empirical work such as Kaplan and Schulhofer-Wohl (2017), is that $Q(P, \theta)$ and $\varepsilon(P, \theta)$ depend on the composition of customers: if the share of inframarginal, low-elasticity consumers rises, the firm faces a less elastic residual demand curve.

Each consumer makes two decisions: (i) whether to visit the outlet (extensive margin: C_g customers), and (ii) how much to spend conditional on visiting (intensive margin: E_g spending per visit). Total revenue is therefore

$$S(P, \theta) = \sum_{g \in \mathcal{G}} S_g(P) = \sum_{g \in \mathcal{G}} C_g(P) \cdot E_g(P).$$

3.1 Marginal Cost and the Minimum Wage Shock

Let marginal cost be

$$c = c_L(w, Q) + c_N(Q),$$

where $c_L(w, Q)$ is labor cost and $c_N(Q)$ captures non-labor costs. Labor cost rises mechanically with the statutory wage w. The April 2024 minimum wage increase in California raises w to w', shifting marginal cost from c to $c' > c$.

Crucially, this structure does not assume that non-labor cost is fixed; $c_N(Q)$ may vary with output or with menu adjustments. All that is required is that the wage enters marginal cost through $c_L(w, Q)$, consistent with Harasztosi and Lindner (2019) and the large literature on cost pass-through.

Under a wide class of imperfect competition models including single-product monopoly, multiproduct firms, Dixit–Stiglitz monopolistic competition (Stiglitz, 2017), or differentiated oligopoly the optimal price can be written as a markup over marginal cost:

$$P = \mu(P,\theta)\, c, \qquad \mu(P,\theta) = \frac{\varepsilon(P,\theta)}{\varepsilon(P,\theta) - 1}.$$

This follows the unified "conduct parameter" representation in Bulow and Pfleiderer (1983) and the pass-through decomposition of Weyl and Fabinger (2013). The key implication is that both marginal cost c and the composite elasticity $\varepsilon(P,\theta)$ determine optimal pricing.

3.2 Cost-Based Pass-Through

The first channel through which the minimum wage affects prices is mechanical:

$$\frac{\partial P}{\partial w} = \underbrace{\frac{\partial P}{\partial c}}_{\text{markup response}} \times \underbrace{\frac{\partial c}{\partial w}}_{\text{labor share}} .$$

Following Weyl and Fabinger (2013), the pass-through rate depends on: Demand curvature (via $\partial P/\partial c$), and the labor share in marginal cost (via $\partial c/\partial w$).

Even under constant elasticity of demand, cost-based pass-through is positive and incomplete, consistent with evidence from Aaronson et al. (2008), Harasztosi and Lindner (2019), MacDonald and Nilsson (2016), and Sosinskiy and Reich (2025).

A second channel operates through changes in θ i.e., the composition of buyers. If the minimum wage raises incomes of low-wage workers (who disproportionately patronize fast-food restaurants), then: some low-income consumers face higher disposable income, potentially making them less price elastic; some high-income consumers remain nearly inelastic and adjust mainly on the extensive margin; visiting customers become a more infra-marginal group.

In the model of Leung (2021), these changes induce:

$$\frac{\partial \mu}{\partial \theta} > 0, \qquad \frac{\partial \varepsilon}{\partial \theta} < 0,$$

meaning that the residual demand curve becomes less elastic as the buyer mix shifts. This increases optimal markups even if marginal cost were held constant.

This mechanism rationalizes the empirical pattern in Table 3: higher-income groups reduce visits but increase spending per visit, while low-income consumers exhibit little change in visitation. The composition of revenue therefore shifts toward groups with higher E_g, generating a lower composite elasticity even if individual elasticities ε_g do not change.

3.3 Composite vs. Income-Specific Effects

Summing revenue across income groups gives

$$S = \sum_g C_g E_g, \qquad C = \sum_g C_g.$$

A first-order decomposition yields:

$$dS = \sum_g E_g \, dC_g + \sum_g C_g \, dE_g.$$

Thus, the composite effect (changes in total spending or customers) reflects a weighted aggregation of income-specific adjustments. A modest decline in S may mask: large decreases in visits among higher-income consumers, small or no declines among low-income consumers, and positive intensive-margin responses ($dE_g > 0$) for all groups.

This decomposition explains why total spending falls by about 5 percent, while spending by each income group increases on the intensive margin. This theoretical structure provides a conceptual foundation for interpreting both the level and distribution of consumer-side incidence associated with California's fast-food minimum wage in rest of the paper.

4 Data

This study uses establishment-level transaction data from SafeGraph (2022), which aggregates anonymized debit and credit card purchases made at brick-and-mortar locations across the United States. For each establishment, identified by a persistent unique ID (*placekey*), the data provide monthly measures of: total dollar spending (card-present transactions); total card-present transaction counts; the number of distinct customers, and the distribution of both spending and customers across seven household income brackets (<\$25k, \$25–45k, \$45k-60k, \$60k-75k, \$75k-100k, \$100k-150k, \$150k <). The data do not contain individual-level identifiers but represent a high-frequency, establishment-level panel of demand-side activity linked to a well-defined set of outlets.

4.1 Sample and Chain Selection

The empirical analysis focuses on limited-service restaurants (NAICS 722513) operated by ten major national fast-food chains with large footprints both inside and outside California: McDonald's, Taco Bell, Panda Express, Domino's Pizza, Burger King, Chipotle Mexican Grill, Jack in the Box, El Pollo Loco, Jersey Mike's Subs, and Subway. These chains offer standardized menus and similar operational structures nationwide, making them particularly suitable for cross-state comparisons.

I construct a balanced monthly panel spanning January 2023 through December 2024. I retain locations with continuous observation of all core outcome variables over this period. California outlets belonging to these chains constitute the treated group because they are directly affected by AB-1228. All outlets from the same chains located outside California serve as comparison establishments. Because these chains operate across many regions, the control group reflects a broad cross-section of U.S. product-market conditions, mitigating concerns that nearby states may share unobserved shocks with California.

Table 1, summarizes descriptive statistics for log spending and log customers by income group and treatment status. The table reports means and standard deviations separately for control (non-California) and treated (California) outlets.

Table 1: Descriptive Statistics for Spending and Customers by Income Group and Treatment Status

	Spending (log)		Customers (log)	
Income Group	Control	Treated	Control	Treated
Composite	8.103	7.071	4.962	3.923
	(1.332)	(1.052)	(1.312)	(1.026)
<$25k	3.022	3.029	3.194	2.082
	(0.357)	(0.421)	(1.322)	(0.953)
$25–45k	3.116	3.118	3.382	2.271
	(0.346)	(0.392)	(1.316)	(0.999)
$45–60k	3.137	3.130	3.022	2.030
	(0.377)	(0.425)	(1.221)	(0.906)
$60–75k	3.155	3.135	2.709	1.803
	(0.407)	(0.474)	(1.130)	(0.808)
$75–100k	3.186	3.178	2.950	2.011
	(0.394)	(0.442)	(1.180)	(0.875)
$100–150k	3.214	3.208	2.984	2.094
	(0.405)	(0.438)	(1.173)	(0.883)
>$150k	3.351	3.263	3.028	2.322
	(0.525)	(0.461)	(1.153)	(0.902)
N	38,864	5,098	38,864	5,098

Notes: Values are means with standard deviations in parentheses. All variables are log-transformed. Income groups reflect household-level income categories. "Control" outlets are chain fast-food restaurants outside California; "Treated" outlets are the same chains in California, which are directly affected by AB-1228.

Two patterns are noteworthy. First, composite spending and customers are somewhat higher in control outlets than in treated outlets, reflecting larger store footprints and broader catchment areas in many non-California markets. Second, within each treatment status, average log spending rises monotonically with customer income, while variation in log customer counts is particularly large among lower-income groups. The distributions for treated and control stores overlap substantially across income bins, suggesting that the composition of customers served by these chains is broadly comparable across states.

5 Empirical Strategy

To estimate the causal effect of California's fast-food minimum wage on consumer outcomes, I employ a two-way fixed-effects difference-in-differences (DiD) design. Let Y_{ist} denote an outcome for store i of chain s in month t. I study three aggregate outcomes: log number of distinct customers; log number of transactions; and log total spending. All outcomes are transformed as $\log(1 + Y)$ to accommodate zeros and reduce the skewness of the right. [1]

$$\log(1 + Y_{ist}) = \beta\,(\text{Treated}_i \times \text{Post}_t) + \mu_i + \lambda_t + X_{ct}\gamma + \varepsilon_{ist}, \tag{1}$$

where Treated_i is an indicator for outlets located in California, Post_t equals one for months on or after April 2024 (the implementation of AB-1228), μ_i are store fixed effects, and λ_t are month fixed effects common to all stores. The vector X_{ct} contains fixed effect for time-varying county-level controls, including log median wages and the unemployment rate in county c, as well as City where the establishment is located that flexibly capture time-invariant local demand and cost conditions. Standard errors are clustered at the store level to allow for arbitrary within-establishment serial correlation.

The parameter of interest, β, captures the average log change in the outcome for treated outlets relative to comparison outlets after the policy, conditional on fixed effects and local controls. I report estimates from specifications both with and without X_{ct} to show that the results are not sensitive to including local economic controls.

5.1 Event-Study Design

To examine dynamics and assess the plausibility of the parallel-trends assumption, I estimate a dynamic event-study specification that replaces the single post indicator with

[1] For small changes, the log point estimate β can be interpreted as an approximate percentage change; using $\exp(\beta) - 1$ yields nearly identical magnitudes.

a full set of leads and lags:

$$\log(1 + Y_{ist}) = \sum_{k \neq -1} \beta_k \, \mathbf{1}\{t - t_0 = k\} \times \text{Treated}_i + \mu_i + \lambda_t + X_{ct}\gamma + \varepsilon_{ist}, \tag{2}$$

where t_0 indexes April 2024 and k is measured in months relative to that date. I omit $k = -1$ so that all coefficients are interpreted relative to the month immediately before the minimum wage increase. Plots of $\hat{\beta}_k$ and associated confidence intervals reveal whether treated and control outlets followed parallel trends before the policy and how outcomes evolved afterwards.

5.2 Income-Specific Specifications

To study distributional effects across the income distribution, I estimate analogues of equations (1) for income-specific outcomes. Let Y^g_{ist} denote either spending or customer counts attributable to income group g. Then

$$\log(1 + Y^g_{ist}) = \beta^g \, (\text{Treated}_i \times \text{Post}_t) + \mu_i + \lambda_t + X_{ct}\gamma + \varepsilon^g_{ist}. \tag{3}$$

The resulting β^g trace out the *income-specific effect* of the policy on spending and customer flows for each income group, holding the classification by income fixed. Comparing these to the composite effects for total spending and customers allows us to distinguish between aggregate changes in demand and shifts in its composition.

6 Results

Table 2, reports the main difference-in-differences estimates for the three composite outcomes: log customers, log spending, and log transactions. Each row corresponds to a different outcome. Columns (1) and (2) report estimates from specifications without and with county-level controls, respectively.

Across all three outcomes, the estimated effects of California's fast-food minimum wage increase are negative, precisely estimated, and highly stable across specifications.

Table 2: Effect of California Fast-Food Minimum Wage on Customer Counts, Spending, and Transactions

	(1) Without Controls	(2) With Controls
Panel A: Customers (log)		
Treated × Post	-0.0660***	-0.0662***
	(0.0038)	(0.0038)
Panel B: Spending (log)		
Treated × Post	-0.0499***	-0.0502***
	(0.0045)	(0.0045)
Panel C: Transactions (log)		
Treated × Post	-0.0645***	-0.0647***
	(0.0041)	(0.0042)
Establishment FE	Yes	Yes
Month FE	Yes	Yes
Median wage	No	Yes
Unemployment rate	No	Yes
City region FE	No	Yes
Observations	1,051,541	1,051,041

Notes: Each cell reports the estimated coefficient on the treatment indicator (Treated × Post) from equation (1). The dependent variable is log(1+outcome), where outcomes are number of distinct customers (Panel A), total spending (Panel B), and number of transactions (Panel C). "With controls" includes county-level median wages, unemployment rate, and city-by-region fixed effects. Standard errors clustered at the establishment level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

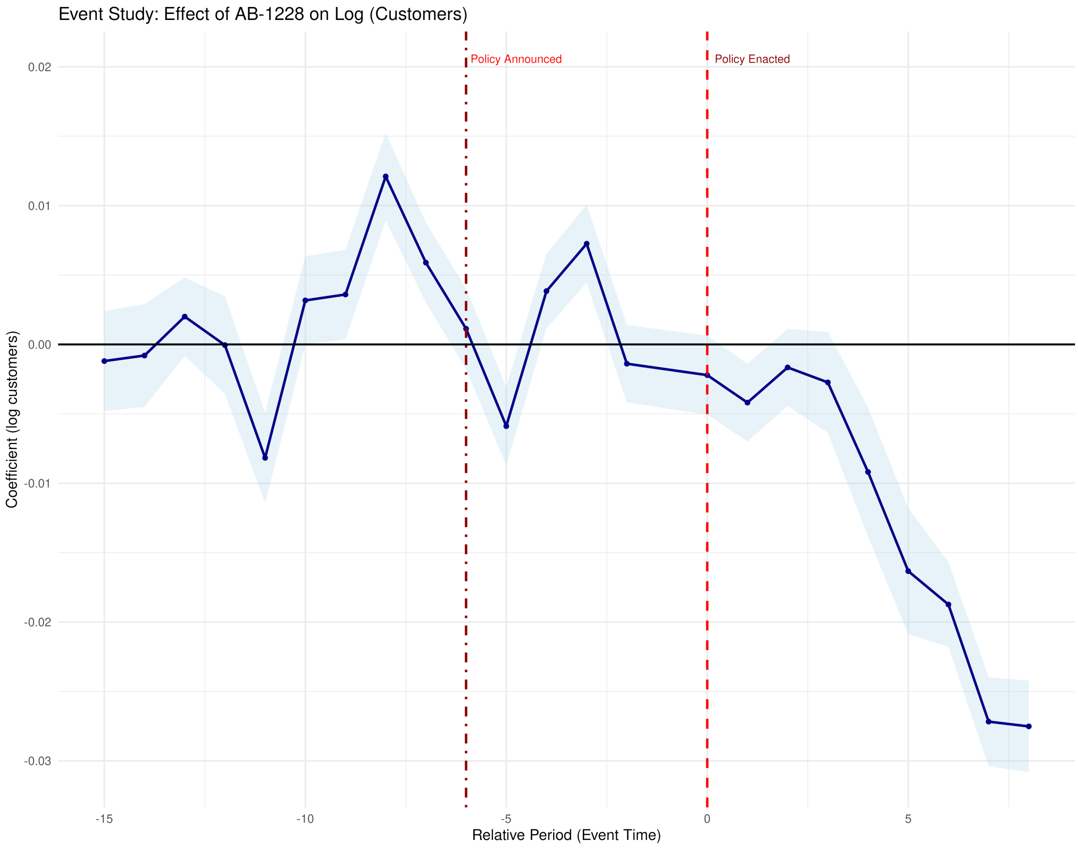
In the preferred specification with controls (column 2), the log number of customers declines by 0.0662 , implying a 6.4 – 6.8 percent decrease in distinct customers at treated outlets relative to comparison outlets. The log number of transactions declines by 0.0647, also around 6–7 percent. Total spending decreases more moderately, with an estimated coefficient of -0.0502, corresponding to roughly a 5 percent drop in revenue.

Taken together, these composite results indicate that California's fast-food minimum wage led to a measurable contraction in total customer flows and transaction volumes at treated outlets, but a somewhat smaller decline in total spending. This pattern in which revenue falls less than visits or transactions implies an increase in spending per customer or per transaction among remaining buyers, consistent with partial pass-through of higher labor costs into prices.

6.1 Dynamic Event-Study Results

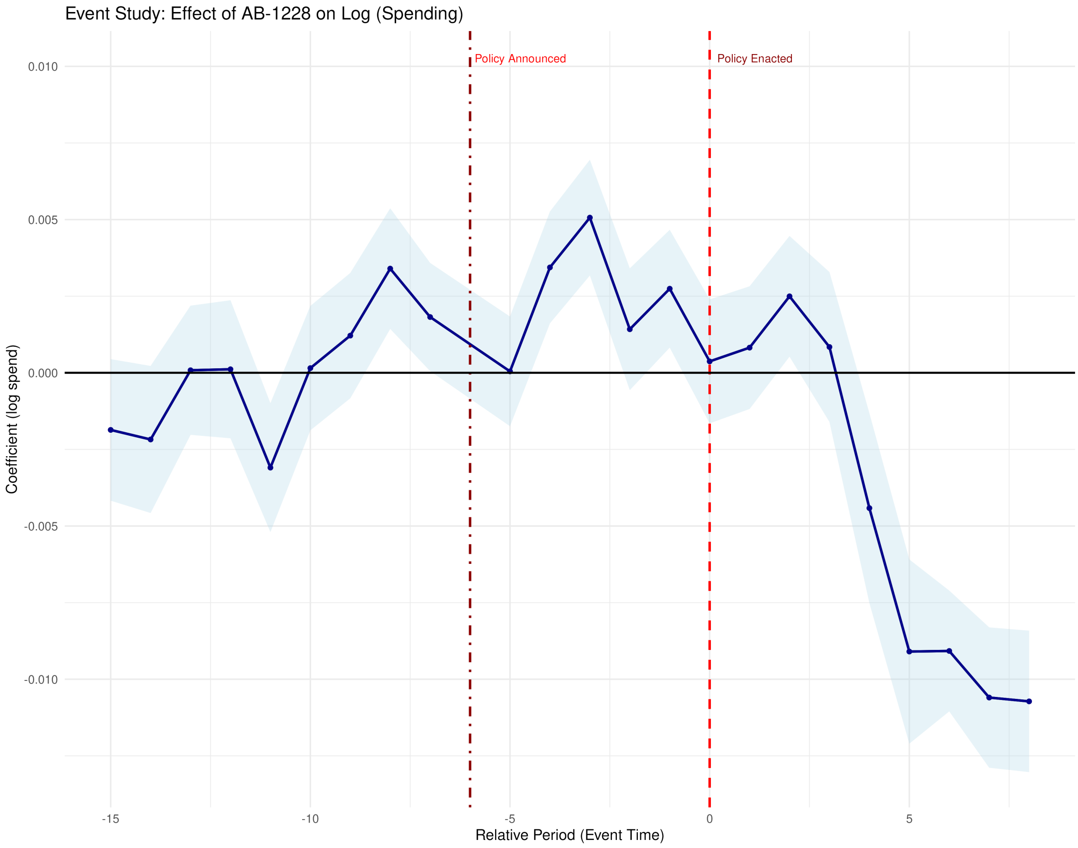
Event-study estimates provide visual evidence on pre-trends and the timing of the policy response. Figures 1–3 plot the coefficients $\hat{\beta}_k$ from equation (2) for log customers, log spending, and log transactions, respectively.

Figure 1: Dynamic Event Study: Effect of AB-1228 on Log Customers



In the pre-period, the leads of treatment are centered around zero with no discernible trend for any of the three outcomes, providing support for the parallel-trends assumption. Following the April 2024 implementation date, the coefficients move sharply into negative territory and remain negative, showing persistent declines in customers, transactions, and spending at treated outlets relative to controls. The declines are somewhat steeper for customers and transactions than for total spending, foreshadowing the composition effects documented below. There is no evidence of a gradual buildup or anticipatory decline following the 28 September 2023 announcement, suggesting that behavioral adjustments are concentrated around the date when wages actually rose.

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

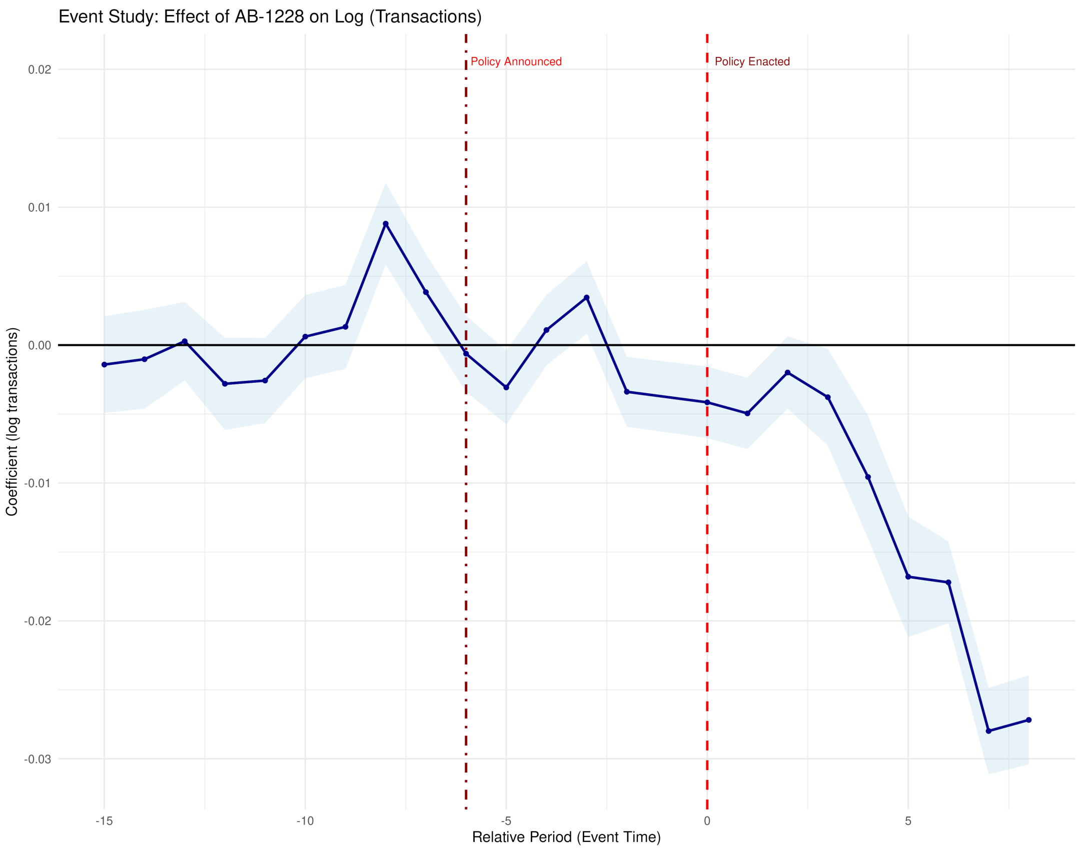


6.2 Heterogeneous Effects by Customer Income

I now turn to heterogeneity by income. Table 3, reports the estimated effects of the minimum wage increase on income-specific spending and customer counts. Each row corresponds to a different household income bracket, and columns report coefficients from specifications without and with county-level controls for both log spending and log customers.

Several patterns stand out. First, the effects on spending are positive and statistically significant for all income groups. In the specification with controls, spending by customers from households earning less than $25,000 increases by about 1 percent, while spending by customers in the $75,000–$100,000 and $100,000–$150,000 ranges increases by roughly 2.4 and 2.2 percent, respectively. Even the highest-income group (above $150,000) shows significant spending increases of around 1.7 percent. These patterns indicate that conditional on visiting a fast-food restaurant, consumers in all income groups tend to spend more after the policy, with the largest percentage increases observed among middle- and

Figure 3: Dynamic Event Study: Effect of AB-1228 on Log Transactions



upper-middle-income groups.

Second, the effects on customer counts are small at the bottom of the income distribution and become increasingly negative for higher-income groups. The number of low-income customers (below \$25,000) is essentially unchanged: the estimated coefficients are near zero and statistically insignificant. In contrast, customer counts among households in the \$45,000–\$60,000, \$60,000–\$75,000, \$75,000–\$100,000, \$100,000–\$150,000, and above \$150,000 ranges decline significantly, with magnitudes ranging from about 4 to 6 percent. These declines in customer counts mirror the composite reductions in visits and transactions, but they are concentrated among middle- and higher-income households.

Combining the spending and customer-count results, we find that the policy induces *intensive-margin* increases in spending per visit within each income group, alongside *extensive-margin* reductions in visitation rates for higher-income customers. The net effect is a redistribution in the composition of consumers purchasing from treated outlets: the set of customers tilts toward those who continue to visit and now spend more per visit,

Table 3: Effect of California Fast-Food Minimum Wage on Spending and Customer Visits by Income Group

	Spending (log)		Customers (log)	
Income Group	Without Controls	With Controls	Without Controls	With Controls
<$25k	0.0101***	0.0100***	0.0010	0.0008
	(0.0029)	(0.0029)	(0.0039)	(0.0039)
$25–45k	0.0074**	0.0074**	-0.0057	-0.0061
	(0.0027)	(0.0027)	(0.0039)	(0.0039)
$45–60k	0.0125***	0.0124***	-0.0364***	-0.0365***
	(0.0029)	(0.0029)	(0.0038)	(0.0038)
$60–75k	0.0153***	0.0153***	-0.0449***	-0.0450***
	(0.0032)	(0.0032)	(0.0035)	(0.0036)
$75–100k	0.0240***	0.0240***	-0.0408***	-0.0408***
	(0.0029)	(0.0029)	(0.0036)	(0.0036)
$100–150k	0.0217***	0.0217***	-0.0551***	-0.0551***
	(0.0029)	(0.0029)	(0.0037)	(0.0037)
>$150k	0.0169***	0.0167***	-0.0509***	-0.0510***
	(0.0030)	(0.0031)	(0.0036)	(0.0036)
Establishment FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
City FE	No	Yes	No	Yes
Median wage	No	Yes	No	Yes
Unemployment rate	No	Yes	No	Yes
Observations	1,033,081	1,017,180	1,033,081	1,017,180

Notes: Each cell reports the estimated coefficient on the treatment indicator (Treated × Post) from equation (3) for the indicated income group. The dependent variables are log(1+spending) and log(1+customers) for each group. "With controls" includes county-level median wages, unemployment rate, and city-by-region fixed effects. Standard errors clustered at the establishment level are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

and away from higher-income customers who reduce their visitation frequency. Because low-income customer counts do not decline, while their spending rises slightly, the data suggest that income gains from the wage increase may offset or dominate the impact of price increases for these households.

From the perspective of incidence, the composite decline in total spending (around 5 percent) aggregates these income-specific adjustments. But the income-specific effects reveal a more nuanced picture: the product-market burden of higher wages is borne disproportionately by remaining, higher-spending consumers, many of whom are in middle- and higher-income households. This composition effect is a central incidence margin that would be invisible in price indices alone.

7 Robustness and Falsification

This section presents a set of robustness checks and falsification exercises designed to assess the sensitivity of the estimates and to evaluate alternative explanations for the observed post-policy declines in fast-food demand. Across all exercises, the results consistently reinforce the baseline findings.

7.1 Alternative Control Groups

I first investigate whether the baseline estimates are driven by minimum-wage dynamics outside California rather than by AB–1228 itself. To do so, I re-estimate equation (1) restricting the comparison group to states that have not changed their minimum wage since the 2009 federal adjustment. These "policy-stable" states provide a cleaner counterfactual by eliminating potential contamination from contemporaneous state-level wage reforms. Columns (1)–(2) of Table 4 show that the estimated treatment effects are nearly identical to the baseline: customer visits and transactions fall by approximately 4.7–5.2 percent, and total spending declines by roughly 3.5 percent, with or without county-level controls. The similarity of these estimates confirms that cross-state minimum-wage variation is not driving the results.

I next impose a restriction to the treated group to "clean" California counties those without any local minimum-wage ordinances prior to AB–1228 while maintaining the same set of no-change outside states. This restriction ensures that treated and control areas share a more homogeneous pre-policy wage environment. As shown in Columns (3)–(4) of Table 4, the estimates remain stable: customers and transactions decline by roughly 5 percent, and spending falls by 4–4.5 percent. The tight correspondence across

Table 4: Alternative Control Groups: States with No Minimum Wage Changes Since 2009

	All CA Counties		Clean CA Counties	
	(1)	(2)	(3)	(4)
Customers (log)				
Treated × Post	-0.0516***	-0.0519***	-0.0519***	-0.0526***
	(0.0040)	(0.0040)	(0.0066)	(0.0067)
Spending (log)				
Treated × Post	-0.0351***	-0.0354***	-0.0432***	-0.0439***
	(0.0047)	(0.0047)	(0.0079)	(0.0080)
Transactions (log)				
Treated × Post	-0.0474***	-0.0476***	-0.0491***	-0.0500***
	(0.0044)	(0.0044)	(0.0073)	(0.0074)
Establishment FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Median wage	No	Yes	No	Yes
Unemployment rate	No	Yes	No	Yes
City–region FE	No	Yes	No	Yes
Observations	539,145	538,949	460,555	463,085

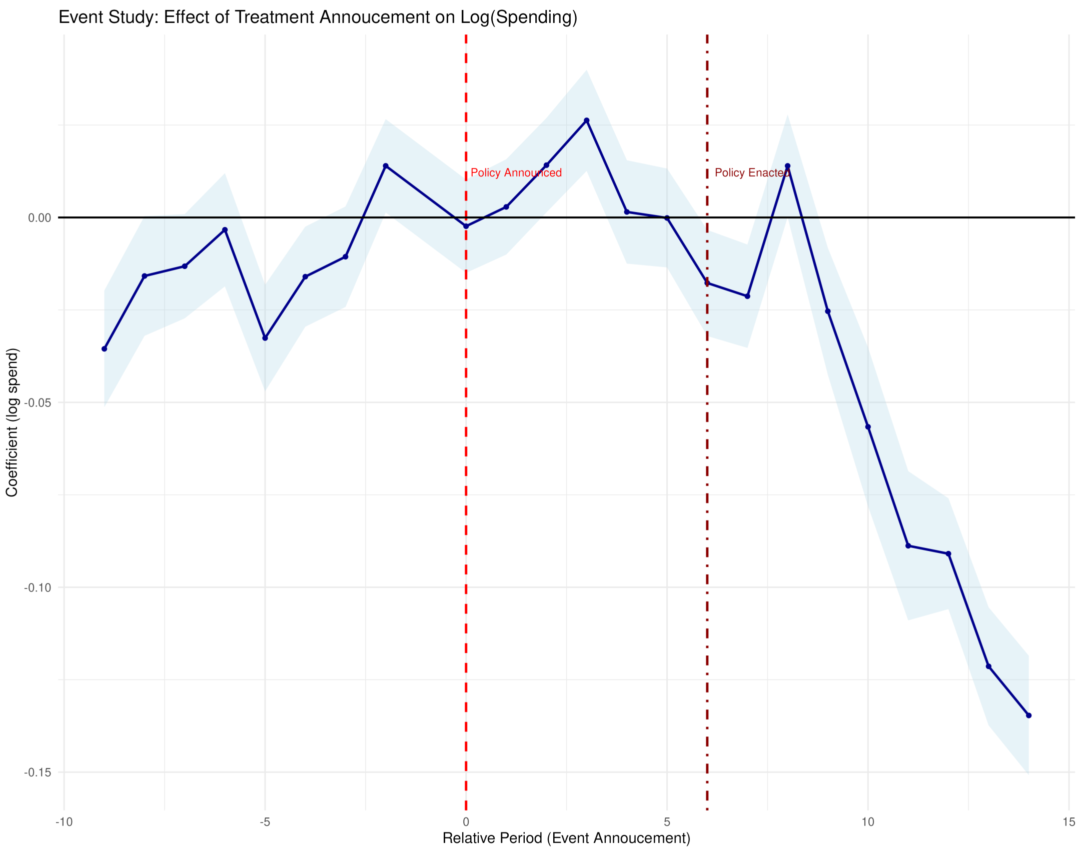
Notes: Each cell reports the estimated coefficient on Treated × Post from equation (1). Columns (1)–(2) restrict the comparison group to states with no minimum-wage changes since 2009 (AL, GA, ID, IN, IA, KS, KY, LA, MS, NH, NC, ND, OK, PA, SC, TN, TX, UT, WI, WY). Columns (3)–(4) further restrict the California sample to counties without local minimum-wage ordinances. Columns (2) and (4) include county-level median wages, unemployment rates, and city-by-region fixed effects. Standard errors are clustered at the establishment level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

all four specifications indicates that neither local pre-existing wage policies nor wage trends elsewhere meaningfully influence the results. Together, these tests provide strong evidence that the estimated effects capture the causal response to AB–1228 rather than artifacts of the control-group choice.

7.2 Time-Placebo Event Study Using the Policy Announcement Date

Finally, I conduct a falsification test that replaces the true treatment date with the 28th September 2023 policy announcement date. This "time-placebo" design evaluates whether the observed declines could reflect differential trends or unrelated shocks in late 2023. Figure 4 plots the dynamic treatment effects under this placebo timing. If the main results were driven by unrelated demand shocks, anticipation, or spurious breaks, I would observe negative post-period coefficients beginning in October 2023. Instead, the placebo event study shows that coefficients remain tightly centered around zero both before and after the announcement, with no evidence of a structural break.

Figure 4: Placebo Dynamic Event Study: Effect of AB-1228 on Log Spending



In sharp contrast, the true event-time specification exhibits a clear and sustained decline in customers, transactions, and total spending beginning precisely at the April 2024 implementation date. The absence of a detectable announcement response, combined with the strong break at the true implementation date, provides compelling evidence that the estimated effects are not driven by pre-trends, confounding shocks, or anticipation dynamics.

Taken together, the robustness and falsification exercises strongly support the interpretation that the documented reductions in fast-food demand across customers, transactions, and spending reflect the causal impact of California's fast-food minimum wage rather than spurious correlations or underlying trends.

8 Discussion

This section interprets the empirical patterns through the lens of established mechanisms of pass-through, demand adjustment, and incidence. The fast-food sector is a particularly informative setting for examining these channels: it is a high–labor-cost, highly standardized industry where minimum-wage policies have repeatedly generated clear and measurable product-market responses (Gittings & Schmutte, 2016; Meer & West, 2016). Building on the results in Section 6, I synthesize the main findings, relate them to existing theory and evidence, and discuss their distributional and welfare implications under AB–1228.

8.1 Price Pass-Through, Menu Adjustments, and Service Intensity

The decline in customer visits and transactions (6–7 percent) paired with a smaller decline in revenue (about 5 percent) implies a sizable increase in spending per visit. This divergence between quantity and revenue is consistent with partial price pass-through of the wage shock, a pattern widely documented in restaurant and retail sectors following minimum-wage hikes (Aaronson et al., 2008; Clemens, 2021; Harasztosi & Lindner, 2019; MacDonald & Nilsson, 2016). Interpreting the magnitudes in dollar terms, the estimated 5 percent decline in total spending corresponds to roughly a $300 reduction in monthly revenue for the average fast-food outlet in the sample, a modest adjustment consistent with partial but incomplete pass-through of AB-1228 [2].

Recent working paper by Sosinskiy and Reich (2025) also document that menu prices

[2] A back-of-the-envelope calculation uses the sample mean of card-present monthly spending ($6,615 per outlet). A 5 percent decline implies a revenue reduction of $6,615 × 0.05 $330 per store per month. With approximately 5,000 chain-affiliated limited-service restaurants in California represented in the data, this yields a statewide decline of about $1.6 million per month (roughly $20 million annually). These calculations are illustrative and reflect card-present transactions only; cash sales are not observed.

at major fast-food chains in California increased by 7–12 percent shortly after AB–1228 took effect. These price adjustments closely parallel the spending-per-visit increases in our transaction data and suggest that higher posted prices not changes in order composition account for most of the rise in average transaction values.

Firms may also adjust on non-price margins. Evidence from earlier minimum-wage episodes indicates that restaurants respond through changes in product bundles, reductions in promotional discounts, and shifts toward higher-margin items (Aaronson et al., 2008; Harasztosi & Lindner, 2019). Such "quality–price bundling" responses are consistent with the industrial-organization literature on cost pass-through and demand curvature (Weyl & Fabinger, 2013). While the SafeGraph data do not contain item-level detail, the consistency between external price evidence and the rising revenue-per-visit pattern in our data indicates meaningful but incomplete pass-through, with firms absorbing part of the wage shock through lower markups or reduced service intensity.

8.2 Demand Composition and Heterogeneous Consumer Incidence

The income-specific estimates reveal substantial heterogeneity that aggregate quantities mask. Higher-income consumers reduce their visit frequency by 4–6 percent but increase spending per visit by 2–3 percent, while low-income consumers exhibit no statistically significant decline in visitation and only modest spending increases. This divergence indicates that the burden of AB–1228 is distributed unevenly across the income distribution, a pattern broadly consistent with the heterogeneous incidence documented in Harasztosi and Lindner (2019) and the dynamic adjustment margins emphasized by Meer and West (2016).

Several mechanisms rationalize these patterns. First, higher-income consumers face higher opportunity costs of time and are more sensitive to service-quality adjustments or menu simplifications, making the extensive margin of restaurant choice their primary response. Second, low-income consumers may experience offsetting income gains as beneficiaries of the wage increase, reducing their effective price elasticity; such demand-induced feedback effects are central in Leung (2021). Third, as visits becomes more concentrated among consumers who spend more per transaction, the residual demand faced by firms becomes less elastic, consistent with models of imperfect competition and endogenous markups (Berry et al., 1995; Weyl & Fabinger, 2013).

These mechanisms imply that AB–1228 alters both the level and the composition of demand. The policy reallocates product-market incidence toward middle- and higher-income households who reduce visits but spend more when they do visit while low-income households, including many direct beneficiaries of the wage increase, experience relatively smaller reductions in consumption opportunities. This asymmetric adjustment highlights an important distributional channel of sectoral minimum-wage policies that is often missed by studies focusing solely on employment effects (Cengiz et al., 2019; Gittings & Schmutte, 2016; Sabia et al., 2012).

9 Conclusion

This paper provides new evidence on how a large, sector-specific minimum wage affects consumer behavior in a highly standardized, high–labor-share industry. Using rich transaction microdata and a differences-in-differences design comparing national fast-food chains inside and outside California, I document that AB–1228 reduced customer visits and transactions by 6–7 percent but lowered total spending by only about 5 percent. The resulting rise in spending per visit is consistent with meaningful but incomplete pass-through of higher labor costs into prices, a pattern mirrored in documented menu-price increases following the policy by Sosinskiy and Reich (2025). These composite effects, however, hide substantial heterogeneity across the income distribution.

Income-resolved estimates show that low-income households many of whom likely benefited directly from the wage increase exhibit little change in visit frequency and modest increases in spending. In contrast, middle- and higher-income consumers reduce visits by 4–6 percent while increasing spending per visit by 2–3 percent. The resulting shift in the composition of demand toward higher-spending, less elastic customers aligns closely with the "demand-induced pass-through" mechanism developed by Leung (2021): when some consumers experience income gains, the effective elasticity of residual demand falls, allowing firms to raise markups even with moderate output reductions. In the context of AB–1228, this channel helps explain why prices and average spending increase even as total foot traffic declines.

Taken together, the results demonstrate that a sector-specific minimum wage can generate heterogeneous consumer-side incidence. Higher-income households absorb a disproportionate share of the product-market burden, while low-income households including many direct beneficiaries of the wage increase are comparatively insulated on the demand side. This pattern contrasts with the typical focus of the minimum wage literature on employment effects alone and complements evidence that pass-through varies with market structure, demand curvature, and consumer composition (Aaronson et al., 2008; Harasztosi & Lindner, 2019; Weyl & Fabinger, 2013). By documenting how a targeted wage shock reshapes both the level and composition of consumer demand, the analysis contributes a demand-side perspective that has been largely absent from incidence debates.

Several limitations temper the interpretation. The transaction data capture only card-present/online payments, omitting cash transactions that might be more common among certain group of consumers. Item-level detail is unavailable, preventing separation of pure price effects from changes in order bundles or service quality. Nonetheless, the consistency between documented menu-price increases and the rise in average spending per visit, combined with stable pre-trends and strong falsification tests, suggests that these limitations do not overturn the core conclusions.

The evidence from California's 2024 reform highlights that the consumer side of minimum wage incidence is neither uniform nor regressive by construction. In settings where higher-income households have lower elasticities of substitution and where low-wage workers patronize the same firms affected by the policy, price pass-through can be borne disproportionately by higher-income consumers while low-income households experience both wage gains and muted increases in consumption costs. This insight has broader implications for debates over sectoral wage regulation, the distributional consequences of labor standards, and the design of policies that aim to shift the incidence of labor-cost shocks. Future work combining consumer data with employment records or extending the analysis to cross-sector substitution and general equilibrium channels would help complete the welfare calculus, but the patterns documented here make clear that product-market responses are central to understanding who ultimately pays for higher wages.

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Appendix A: Robustness Using Raw Outcomes and Poisson Models

A1. Raw (Level) Outcomes

The raw-level specifications in Table 5 yield smaller and in some cases statistically insignificant coefficients particularly for total spending because OLS in levels and Poisson Pseudo Maximum Likelihood (PPML) estimate a fundamentally different parameter than the log-linear models emphasized in the main text. Level-based models implicitly weight establishments by their size: high-volume restaurants, which generate the largest dollar totals and exhibit the smallest proportional response to AB–1228, dominate the estimation. By contrast, medium and smaller establishments, which experience the largest percentage declines in customers, transactions, and spending, contribute little to the PPML likelihood when outcomes are measured in raw dollars. As a result, the PPML estimate for spending is attenuated toward zero even though log(1+spending) models show a clear and precise treatment effect. Importantly, the PPML coefficients for all three outcomes remain consistently negative across specifications, indicating directional agreement with the main results. The weaker precision in levels therefore reflects mechanical estimator weighting and the extreme right-skew of dollar spending rather than evidence against a spending response as shown in Figure 5. For this reason, and consistent with best practice in the minimum wage literature, percentage-based log specifications remain the preferred estimand for the composite consumer-demand response.

Table 5: Effect of California Fast-Food Minimum Wage on Raw Customers, Spending, and Transactions

	(1) Without Controls	(2) With Controls
Panel A: Customers (raw)		
Treated × Post	-0.0328**	-0.0330**
	(0.0133)	(0.0134)
Panel B: Total Spending (raw $)		
Treated × Post	-0.0114	-0.0115
	(0.0175)	(0.0176)
Panel C: Transactions (raw)		
Treated × Post	-0.0311**	-0.0312**
	(0.0149)	(0.0150)
Establishment FE	Yes	Yes
Month FE	Yes	Yes
Median wage	No	Yes
Unemployment rate	No	Yes
City–region FE	No	Yes
Observations	1,051,541	1,051,041

Notes: Each cell reports the coefficient on the treatment indicator (Treated × Post) using raw outcomes: number of distinct customers, total card-present spending, and number of transactions. "With controls" includes county median wage, county unemployment rate, and city-by-region fixed effects. All models include establishment and month fixed effects. Standard errors clustered at the establishment level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 5: log-OLS vs PPML Fast-food establishments weights