SECTOR-SPECIFIC WAGE FLOORS AND YOUNG ADULT LABOR MARKET OUTCOMES: EVIDENCE FROM CALIFORNIA'S AB 1228

A Thesis By

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Abstract:
California Assembly Bill 1228, effective April 1, 2024, established a $20 per hour minimum wage for fast-food chains, the first statewide sector-specific wage floor in United States history. Using Current Population Survey data from January 2022 through September 2025 and multiple identification strategies, this study finds that few workers may have lost jobs, but many got fewer hours, and less-educated workers bore the brunt of the adjustment. The all-state two-way fixed effects specification estimates a 0.80 percentage point employment decline among 16- to 24-year-olds, but this estimate fails pre-trend tests and the synthetic control robustness check yields a statistically insignificant gap (permutation p = 0.647), so the aggregate employment effect is best read as not robust. Hourly wages rose 2.1 percent and weekly hours fell 3.8 percent, with the hours reduction approximately offsetting the wage gain in weekly earnings at the sample mean. Employment effects vary sharply by education: workers with less than a high school diploma or only a high school diploma experienced employment declines of about 3.2 percentage points each, while those with some college education saw employment increases of 2.4 percentage points. Establishment-level Quarterly Census of Employment and Wages data show a 2.7 percent decline in California limited-service restaurant employment alongside a 5.4 percent wage increase, while a five-pair contiguous-county border design yields a null employment estimate (p = 0.650) with wages up 8.3 percent. The relevant policy question is less whether aggregate employment fell than whose employment changed, and the answer points to compositional reallocation away from the least-educated workers the policy was designed to help.

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CHAPTER 1

INTRODUCTION

On April 1, 2024, California became the first state to implement a statewide sector-specific minimum wage, requiring fast-food chains with 60 or more national locations to pay employees at least $20 per hour. The policy, enacted through Assembly Bill 1228, represented a structural departure from the conventional approach to minimum wage regulation, in which federal and state establish uniform floors applicable to all covered employment. By targeting a single industry, AB 1228 created a natural experiment in whether wage floors can be designed to sector-specific labor market conditions rather than applied uniformly across an entire economy. The $20 rate represented a 25 percent increase above California's general minimum wage of $16, a treatment intensity substantially larger than the incremental increases that dominate the existing empirical literature.

The employment consequences of AB 1228 are an important question for labor economics and public policy. California's fast-food sector employs more than 700,000 workers (Bureau of Labor Statistics, 2025), with young adults disproportionately represented (Figure 1): nationally, workers under age 25 account for roughly 42 percent of fast-food employment, and in this study's pre-policy CPS sample 87 percent of California workers aged 16 to 24 with a high school diploma or less earned below $20 per hour. Industry groups predicted widespread job losses and restaurant closures, while labor advocates argued that higher wages would improve worker welfare with minimal-to-no displacement. The outcome of this debate carries implications beyond California: multiple states and cities have considered or enacted sector-specific wage legislation in recent years, and the California experience will inform the design of future policies. Whether AB 1228 reduced employment, raised wages, or altered the composition of the workforce provides direct evidence on the relative merits of the competitive and monopsony models of low-wage labor markets at a policy-relevant scale.

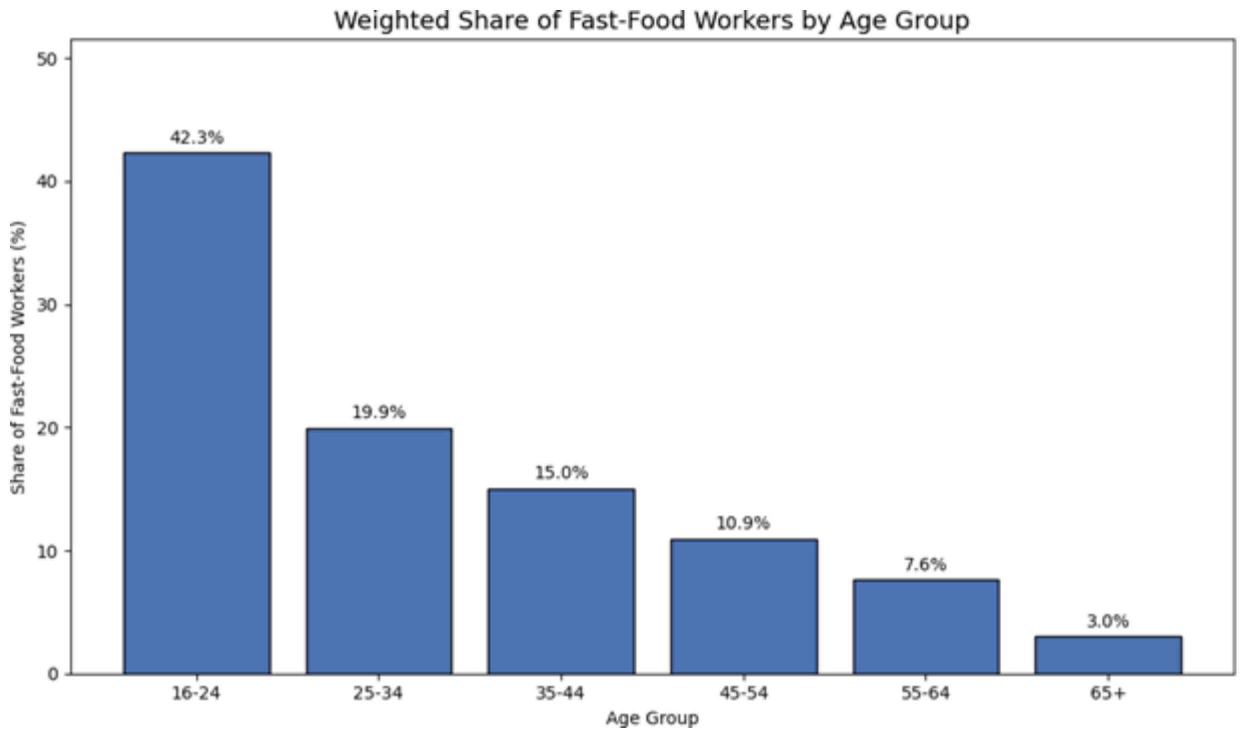

Figure 1. Fast Food Share by Age Group, Source: Calculations from CPS-ORG, January 2022–September 2025.

The central question is whether California's sector-specific minimum wage reduced employment among young adults aged 16 to 24, a group heavily exposed to the $20 floor. Hourly wages, weekly hours, and labor force participation are examined as mechanism checks to interpret the employment findings rather than as independent research questions.

The primary finding is that AB 1228 produced at most a modest aggregate decline in youth employment, and even that estimate is not robust. The all-state two-way fixed effects specification detects an employment decline of 0.80 percentage points with demographic controls, but this estimate fails pre-trend tests, and the synthetic control robustness check yields a statistically insignificant gap (permutation $p = 0.647$). Hourly wages rose 2.1 percentage points and weekly hours fell 3.8 percentage points, with the hours reduction approximately offsetting the wage gain in weekly earnings at the sample mean. The clearest finding is distributional: workers with less than a high school diploma and high school graduates experienced employment declines of about 3.2 percentage points each, while workers with some college education saw employment increases of 2.4 percentage points, and the share of fast-food workers without a high school diploma fell sharply.

Establishment-level QCEW data show a 2.7 percent decline in California limited-service restaurant employment with a 5.4 percent wage increase at the state level, while a five-pair contiguous county border design yields a precise null on employment ($p = 0.650$) alongside an 8.3 percent wage increase. Read together, the evidence does not permit a definitive adjudication between the competitive and monopsony models: the aggregate employment effects are small or null relative to the 25 percent treatment intensity, the significant hours reduction and the price pass-through documented elsewhere fit competitive adjustment, but the positive young adult labor force participation response and the wage gains without matching aggregate disemployment fit a monopsonistic interpretation. The most policy-relevant finding is the compositional reallocation away from the least educated workers.

The central contribution of this paper is to reframe the policy question from whether aggregate employment fell to whose employment changed: the education sorting results document a sharp compositional reallocation away from less-educated workers and toward workers with some college, a distributional consequence the aggregate near-null obscures. Methodologically, the study uses the employment-to-population ratio as the primary outcome, avoiding the endogenous labor force participation selection inherent in unemployment-based measures that is particularly problematic for a student-heavy sample. The all-state TWFE with 51 clusters provides substantially greater statistical power than restricted regional comparisons, and the synthetic control method provides a data-driven robustness check with permutation-based inference valid for a single treated unit. The analysis triangulates household survey evidence from the Current Population Survey with administrative employer data from the Quarterly Census of Employment and Wages at both state and contiguous county levels, providing both an aggregate labor market perspective and a sector-specific estimate. The sample extends through September 2025, providing an 18-month post-treatment window that is longer than most existing studies.

CHAPTER 2

BACKGROUND AND RELATED LITERATURE

Policy Context

California Assembly Bill 1228, signed by Governor Newsom on September 28, 2023, and effective April 1, 2024, established a $20 per hour minimum wage for employees of "national fast-food chains," defined as limited-service restaurant brands operating 60 or more establishments nationally that share a common brand with standardized decor, marketing, packaging, products, and services. The legislation emerged from a protracted political process that began with AB 257, the FAST Recovery Act of 2022, which would have created a Fast-Food Council with broad authority to set wages, hours, and working conditions for the sector. After industry groups gathered sufficient signatures, the referendum was slated to appear on the November 2024 ballot. Prior to the actual ballot appearance, labor and business interests negotiated a compromise: AB 1228 replaced the council model with a fixed wage floor, established a reconstituted Fast-Food Council with more limited authority to recommend future wage increases, and the referendum was withdrawn. The law includes several notable exemptions. Restaurants that operate an on-premises bakery producing and selling bread as a standalone menu item are excluded, as are restaurants located within airports, hotels, theme parks, museums, event centers exceeding 20,000 square feet, and gambling establishments. Grocery store food service operations are also exempt. These exclusions narrow the scope of coverage to freestanding chain fast-food outlets, though the CPS industry data used in this study cannot distinguish covered from exempt establishments within the limited-service restaurant category.

Theoretical Framework

The standard competitive model of the labor market predicts that a binding minimum wage, set above the market-clearing equilibrium, reduces employment by creating an excess supply of labor. Firms facing a higher wage cost reduce their demand for workers along the labor demand curve (Figure 2, Panel A). In this framework, the magnitude of the disemployment effect depends on the elasticity of labor demand: if demand is relatively inelastic, even a large wage increase may produce only a modest reduction in employment, while elastic demand implies larger effects. A $4 increase from $16 to $20 per hour, representing a 25 percent increase, would be expected to reduce employment to some degree, though the predicted magnitude varies substantially across plausible demand elasticities. The competitive prediction is strongest for the least-skilled and least-experienced workers, who are most likely to have marginal products near or below the minimum wage (Stigler, 1946; Neumark & Wascher, 2008).

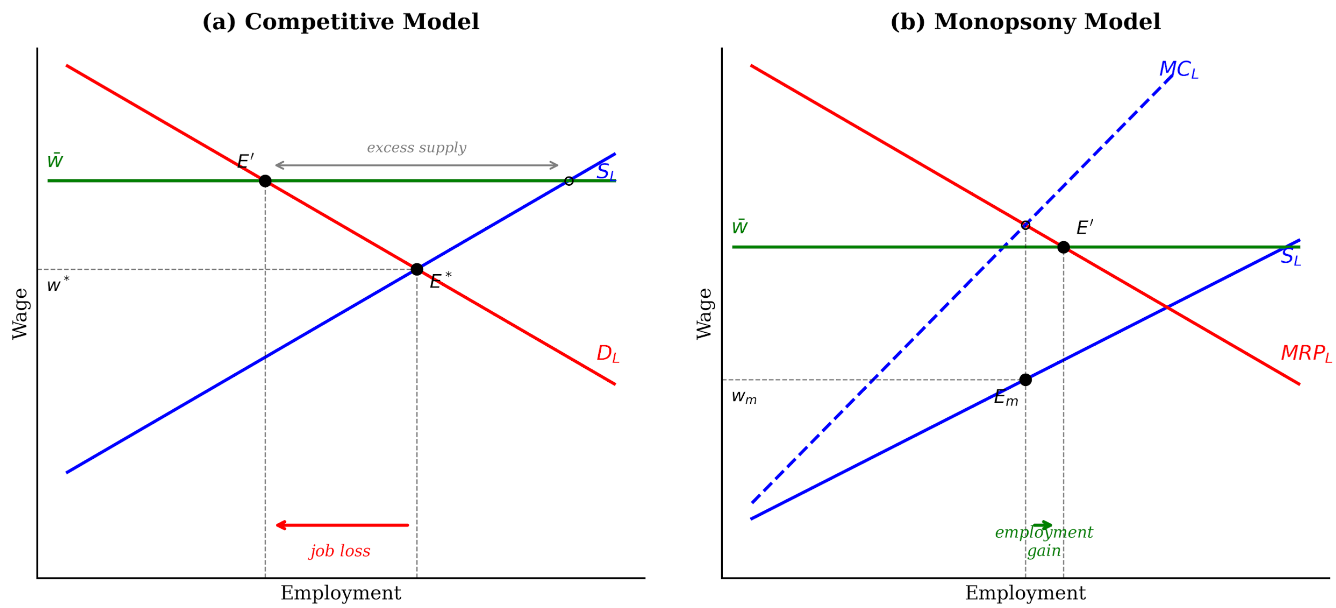

Figure 2. Competitive vs. Monopsony Models of the Labor Market

The monopsony model offers an alternative prediction. If employers possess wage-setting power, either because workers face mobility costs, information frictions, or a small number of local employers, the firm's profit-maximizing wage is below the competitive equilibrium. In the static textbook case depicted in Figure 2, Panel B, a moderate minimum wage can simultaneously raise wages and increase employment by compressing the wedge between the marginal revenue product of labor and the wage paid (Manning, 2003, 2021). Note that Figure 2 depicts the static monopsony case; the dynamic case discussed below generates qualitatively similar but less sharp predictions. In dynamic monopsony models with search frictions, the prediction is not necessarily a positive employment effect but rather a smaller negative effect than the competitive model predicts, because the wage floor reduces the cost of recruiting and retaining workers (Manning, 2003). The empirical implication is that the employment elasticity with respect to the minimum wage should be close to zero rather than the negative values predicted by the competitive model. The monopsony framework also predicts that hourly wages should increase and that labor force participation may rise as higher wages attract marginal workers into job search.

Both models allow for adjustment along the intensive margin. Rather than eliminating positions outright, firms may reduce hours per worker in response to a higher wage floor, lowering labor costs while retaining headcount. This hours reduction is a weaker form of the competitive prediction and is consistent with monopsony if firms partially offset the higher wage through scheduling adjustments. Neither the competitive nor the monopsony model pins down whether the reduction in labor input comes through fewer workers (the extensive margin) or fewer hours per worker (the intensive margin); both channels are available to cost-minimizing firms in either framework. The empirical distinction between the two models therefore lies primarily in the magnitude of the total employment effect relative to the size of the wage increase, not in its form. A binding wage floor is also expected to compress the wage distribution from below, raising wages at the bottom while leaving wages above the floor unaffected. The degree of compression depends on the share of workers earning between the old and new minimum wage, measured by the Kaitz index or the fraction of workers below the new floor.

Related Literature

The empirical literature on minimum wage employment effects has evolved substantially since the early consensus that binding wage floors reduce employment. Card and Krueger (1994) established the landmark finding in a study of fast-food employment in New Jersey and Pennsylvania, detecting no employment reduction following New Jersey's minimum wage increase and, in some specifications, finding a small positive effect. Dube, Lester, and Reich (2010) extended this approach using contiguous county pairs along state borders to control for local economic conditions, finding employment elasticities indistinguishable from zero alongside significant earnings increases in the restaurant sector. Allegretto, Dube, and Reich (2011) demonstrated that the negative teen employment effects reported in earlier panel studies were artifacts of heterogeneous regional trends, and that controlling for spatial heterogeneity rendered the employment elasticity insignificant. Cengiz, Dube, Lindner, and Zipperer (2019), using a bunching estimator applied to 138 state-level minimum wage changes, estimated a near-zero employment elasticity of approximately -0.04 for low-wage jobs, with affected workers bunching just above the new minimum wage rather than losing employment.

Not all evidence supports the monopsony interpretation. Neumark and Wascher (2008) provide a comprehensive review finding that most studies, particularly those focusing on teenagers and less-skilled workers, detect negative employment effects. More recently, the debate has been characterized by Dube (2019) as converging toward a consensus that "the most precise estimates suggest employment elasticities close to zero" (p. 2), while acknowledging that large minimum wage increases remain understudied. The AB 1228 increase of 25 percent above the prior California minimum wage is substantially larger than the incremental increases studied in much of the prior literature, raising the possibility that effects may be nonlinear at higher treatment intensities.

A small but growing literature evaluates the effects of AB 1228 specifically. Reich and Sosinskiy (2024) provided the first systematic evaluation using administrative records from the UC Berkeley Institute for Research on Labor and Employment, finding no reduction in fast-food employment and wage increases of 8 to 9 percent. Hamdi and Sovich (2025) report near-zero net employment effects using payroll data from large employers but document a substantial reduction in turnover of 2.2 percentage points per month, consistent with efficiency wage or monopsony mechanisms. Wiltshire, McPherson, Reich, and Sosinskiy (in press) extend the IRLE analysis to 47 California counties, finding no disemployment over a seven-year run-up in fast-food wages and a 13 to 36 percent reduction in worker separations. Schneider et al. (2024), using worker-level survey data from The Shift Project, found that reported wages rose substantially while hours and benefits were largely unchanged, with higher pay associated with improvements in worker well-being.

Against this backdrop, Clemens, Edwards, and Meer (2025) report a divergent result: using Quarterly Workforce Indicators matched to fast-food establishments, they estimate that AB 1228 reduced fast-food employment by 2.7 to 3.2 percent, or approximately 18,000 positions. In a companion paper, Clemens, Edwards, Meer, and Nguyen (2026) document that food-away-from-home prices in California MSAs rose 3.3 to 3.6 percent relative to control MSAs, with implied limited-service restaurant price increases of 4.9 to 5.1 percent. The price pass-through rate exceeds what would be expected from a simple cost increase, a finding they attribute partly to compositional turnover: drawing on Luca and Luca (2019), who show that minimum wage increases disproportionately drive exit among lower-rated restaurants, they estimate that removal of the bottom 3.2 percent of the price distribution mechanically raises the average price by approximately 2.5 percent even without any individual firm changing its prices. Pandit (2026), using high-frequency transaction data from ten national fast-food chains, corroborates the demand-side response: customer visits and transactions fell 6 to 7 percent following AB 1228, while revenue declined approximately 5 percent, implying higher spending per visit consistent with partial price pass-through. The Clemens et al. employment and price findings and Pandit (2026) transaction data are jointly consistent with competitive adjustment: higher labor costs passed to consumers reduced demand, concentrated establishment exits among lower quality firms and produced a modest aggregate employment decline.

The discrepancy between the Clemens et al. fast-food-specific employment decline — approximately 18,000 positions at the 2.7 to 3.2 percent range — and the near-zero aggregate effects found in Reich and Sosinskiy (2024) and Hamdi and Sovich (2025) may therefore reflect a genuine sectoral employment decline that is diluted or offset through cross-sector reallocation in broader labor market measures. Alternatively, the difference may arise from the measurement level (establishment counts versus household employment status), the sample period (Clemens et al. end in September 2024, while most other studies extend further), or the comparison group construction. This study's QCEW state level analysis, which also uses establishment-level administrative data for the limited-service restaurant industry, provides a direct point of comparison with the Clemens et al. findings and yields estimates of similar magnitude.

CHAPTER 3

DATA

Data Source

The primary data for this study comes from the Current Population Survey Outgoing Rotation Group (CPS ORG), accessed through the Integrated Public Use Microdata Series (IPUMS CPS) harmonized microdata platform (Flood et al., 2025). The CPS is a monthly household survey administered jointly by the U.S. Census Bureau and the Bureau of Labor Statistics, designed to measure labor force participation, employment status, earnings, and more for the civilian population. The Outgoing Rotation Group subsample consists of respondents in their fourth and eighth months of CPS participation, who receive an extended set of questions on earnings, hours worked, and wage rates in addition to the standard labor force items. The ORG design produces a nationally representative monthly sample with individual-level earnings information, making it the standard data source for estimating the wage and employment effects of minimum wage policies (Autor, Katz & Kearney, 2008; Dube, Lester & Reich, 2010).

The sample covers January 2022 through September 2025, spanning 27 months of pre-policy observations and 18 months following the April 2024 implementation of AB 1228. The start date of January 2022 was chosen to provide a sufficiently long pre-period to test the parallel trends identifying assumption while avoiding the period of elevated COVID-19-related labor market volatility in 2020 and 2021, during which youth unemployment rates in California and the comparison states diverged for reasons unrelated to minimum wage policy. The raw extract contains 5,051,768 individual-month observations across all states and age groups, drawn from 33 harmonized variables. Supplementary establishment-level data from the Bureau of Labor Statistics Quarterly Census of Employment and Wages (QCEW) are used in Chapter 6 to triangulate the household survey findings with administrative employer records.

Sample Construction

The analytic sample was constructed through a sequence of restrictions applied to the raw CPS extract. Table 1 documents the observation count at each stage. The full extract of 5,051,768 individual-month observations across all states is first restricted to individuals aged 16 to 24, yielding 520,242 observations across all 50 states and the District of Columbia. This all-state youth sample is used for the synthetic control and TWFE specifications and is further divided into 245,464 teenagers (16 to 19) and 274,778 young adults (20 to 24) for age heterogeneity analysis. For the restricted West-Coast difference-in-differences robustness checks, the sample is limited to California, Nevada, Arizona, and Oregon, producing 70,386 observations. Individuals classified as Not in the Labor Force are retained in all samples, so that the primary outcome captures employment-to-population rather than unemployment among labor force participants. Subsamples used for secondary outcomes and mechanism checks are strict subsets: 291,606 labor force participants for the unemployment rate analysis (36,595 in the West-Coast sample), 50,281 employed individuals with valid hourly wages for the wage analysis (6,255 West-Coast), and 258,152 employed individuals with valid weekly hours for the hours analysis (31,798 West-Coast). No earnings availability restriction is imposed on the main employment sample.

Table 1. Sample Construction

Panel A: Main samples

Restriction	All States	West-Coast	Used for
Raw CPS ORG extract (all ages, Jan 2022 – Sep 2025)	5,051,768	—	—
Ages 16–24	520,242	70,386	SC, TWFE / DiD robustness
Teens (16–19)	245,464	—	Age heterogeneity
Young adults (20–24)	274,778	—	Age heterogeneity

Panel B: Outcome-specific subsamples

Restriction	All States	West-Coast	Used for
Labor force participants	291,606	36,595	Unemployment rate
Employed with valid HOURWAGE2	50,281	6,255	Wage analysis
Employed with valid AHRSWORKT	258,152	31,798	Hours analysis

Panel C: Education subsamples (all states)

Education group	N	Used for
Less than high school	177,899	Education sorting
High school diploma	151,428	Education sorting
Some college	140,659	Education sorting
Bachelor's degree or higher	50,256	Education sorting

Panel D: Supplementary data

Source	N	Used for
QCEW border county-quarters (5 pairs, 10 counties, 15 quarters)	150	Border county DiD

Notes: CPS ORG = Current Population Survey Outgoing Rotation Group. West-Coast sample: CA, NV, AZ, OR. All samples span January 2022 through September 2025. NILF individuals retained in all main samples. QCEW = Quarterly Census of Employment and Wages, NAICS 722513 (limited-service restaurants).

The restriction to ages 16 through 24 follows standard practice in the minimum wage literature. Card and Krueger (1994), Allegretto, Dube, and Reich (2011), and the Bureau of Labor Statistics all define "youth" as individuals in this age range. The lower bound of 16 reflects the minimum legal working age in California; individuals younger than 16 are ineligible for most employment and do not appear in the CPS labor force questions. The upper bound of 24 captures workers most likely to hold minimum-wage-proximate jobs: in the pre-policy period, 87 percent of workers aged 16 to 24 with a high school diploma or less earned below $20 per hour, indicating high exposure to the AB 1228 wage floor. Workers aged 25 and older are increasingly likely to have completed college and to earn well above the minimum wage, making them poor candidates for identifying disemployment effects of the sectoral wage floor. The 16 to 24 range also spans two economically distinct subgroups, teenagers (16 to 19) who are predominantly enrolled in school and working part-time, and young adults (20 to 24) who are more likely to be working full-time, allowing heterogeneity analysis across these margins. Age is measured as reported in the CPS at the time of the interview; no partial-year adjustment is applied. Individuals near the age boundaries who turn 16 or 25 during the sample period enter or exit the eligible sample at the month of their birthday, but this compositional rotation is symmetric across treatment and control states and does not bias the difference-in-differences estimates.

The main sample pools students and non-students because both groups participate actively in the low-wage labor market targeted by AB 1228; school enrollment status is included as a demographic control in specifications with individual-level covariates, and heterogeneity by age group and education level is examined in Chapter 5.

Control Group Construction

The primary specification employs a two-way fixed effects (TWFE) design that exploits variation across all 50 states. The synthetic control method of Abadie, Diamond, and Hainmueller (2010) constructs an optimally weighted counterfactual for California from all 50 states as potential donors rather than relying on a researcher-selected control group. The data-driven procedure assigns the majority of weight to New York (46 percent) and Texas (42 percent), with none of the West-Coast comparison states receiving meaningful weight; the synthetic counterfactual closely tracks California's pre-treatment employment trajectory (Figure 3), with results presented in Chapter 6. A restricted difference-in-differences comparison using Nevada, Arizona, and Oregon is reported as a robustness check in Chapter 6; these states were selected for geographic proximity and similar labor market structures, though all three enacted modest minimum wage increases during the sample period (Arizona to $14.35 in January 2024, Nevada to $12.00 in July 2024, Oregon to $14.70 in July 2024). Figure 4 plots the minimum wage schedules for all four states over the sample period.

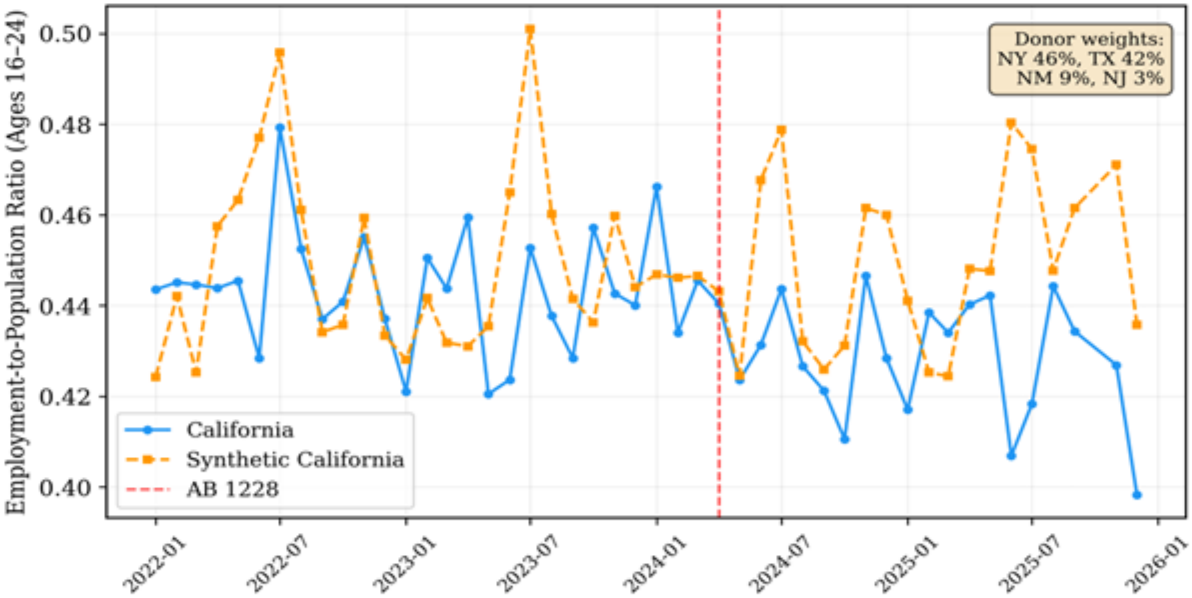

Figure 3. CA vs. Synthetic CA

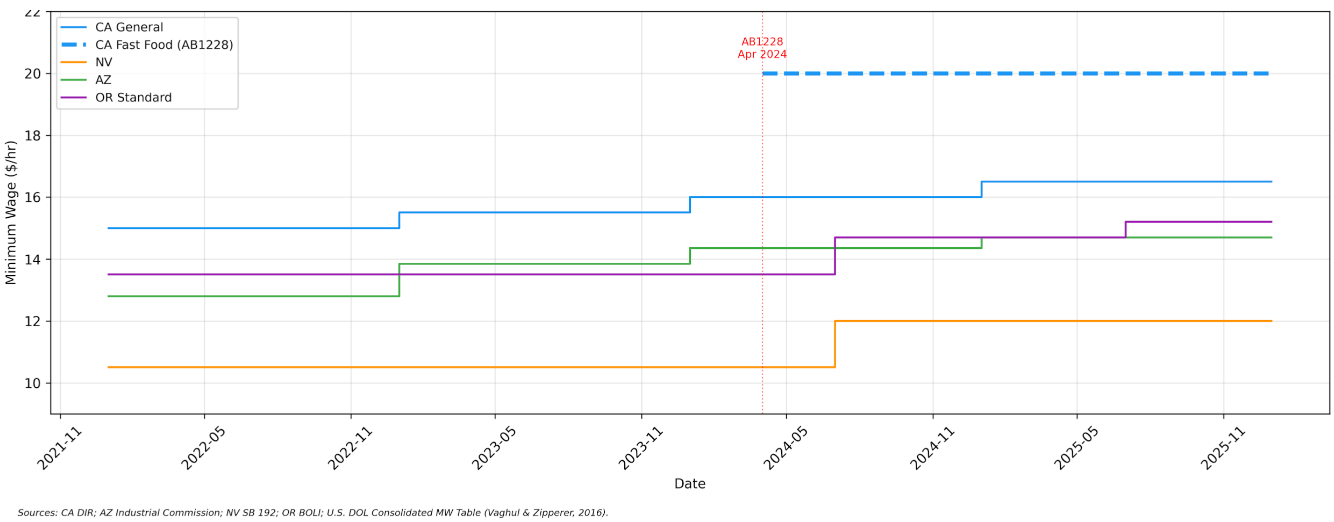

Figure 4. Minimum Wage Schedules - West Coast

Variables

The primary outcome in this study is a binary indicator for employment, defined among all individuals aged 16 to 24 regardless of labor force status. This employment-to-population measure departs from the unemployment rate used in much of the earlier minimum wage literature but is better suited to the sample. Among 16- to 24-year-olds, the distinction between "unemployed" and "not in the labor force" is often ambiguous: a full-time college student who stops looking for part-time work after failing to find a job would be classified as NILF rather than unemployed, even though her labor market withdrawal was driven by reduced hiring. In a student-heavy sample, this margin of adjustment is quantitatively important and would cause an unemployment-rate-based estimator to misstate the true disemployment effect. Retaining NILF individuals in the denominator captures both the unemployment and non-participation margins in a single measure. A limitation of this approach is that the denominator is affected by population composition changes, including migration and college enrollment decisions, that may not be labor market outcomes of the policy; however, such compositional shifts are unlikely to be large enough over 18 months to materially affect the estimates. The unemployment rate among labor force participants is reported as a secondary outcome on the restricted subsample of 36,595 observations for comparability with the existing literature.

Two secondary outcome variables supplement the primary employment measure. The unemployment rate, defined as the share of labor force participants who are unemployed, is estimated on the restricted subsample of labor force participants (291,606 in the all-state sample, 36,595 in the West-Coast sample). This measure is reported for comparability with the existing minimum wage literature, which has traditionally focused on unemployment rather than employment-to-population ratios. Labor force participation, equal to one if the respondent is either employed or unemployed, is examined separately to isolate the non-participation margin from the employment margin.

Hourly wages and weekly hours are examined as mechanism checks to interpret the employment finding. Hourly wages are measured using HOURWAGE2, the IPUMS-constructed variable that applies post-April 2023 rounding rules retroactively to ensure comparability across the full sample period. For workers paid by the hour, HOURWAGE2 records the reported hourly wage directly; for salaried workers, it computes an hourly equivalent from weekly earnings divided by usual hours, creating a mechanical linkage between the wage and hours variables for this group. The wage sample includes both hourly and salaried workers; results are qualitatively similar when restricted to hourly workers only. Wage and earnings variables are collected only from Outgoing Rotation Group respondents, who constitute approximately one-quarter of the CPS monthly sample, substantially reducing the available observations for these outcomes. In the West-Coast sample, the wage subsample contains 6,255 employed individuals with valid wage observations and the earnings subsample contains 7,201 observations; in the all-state sample used for the TWFE, the corresponding counts are 50,281 (wage) and 59,260 (earnings). Weekly hours worked are measured using AHRSWORKT, which records the total hours the respondent worked at all jobs in the reference week and is available for all employed respondents (31,798 in the West-Coast sample, 258,152 in the all-state sample). Log transformations of both variables are used in the regression specifications. These outcomes are examined to interpret the employment result rather than as independent research questions.

An important limitation of the CPS for studying AB 1228 is the absence of an industry code that cleanly identifies fast-food workers covered by the policy. The finest available classification, IND code 8680 captures all limited-service restaurants rather than only the chains with 60 or more national locations to which AB 1228 applies. The main specifications therefore estimate the effect of the policy across all industries among 16-to 24-year-olds, capturing both direct effects on covered workers and any spillover or reallocation effects across sectors. This measurement dilution implies that the estimated coefficients should be interpreted as the broad youth labor market effect of AB 1228 rather than the fast-food-specific effect. QCEW establishment-level data for NAICS 722513 provides a complementary industry-specific estimate in Chapter 6.

Individual-level demographic controls include age, sex, race (White, Black, Asian or Pacific Islander, Two or More Races), Hispanic origin, educational attainment (Some High school, High school diploma or equivalent, Some College, Bachelors+) marital status, and school enrollment status. School enrollment status is drawn from SCHLCOLL, which distinguishes full-time and part-time high school students, full-time and part-time college students, and individuals not enrolled in school; these categories are collapsed into three groups (high school student, college student, not enrolled) for use as controls and for subsample analysis. California is the treated unit in all specifications. For the difference-in-differences robustness checks, a binary post-period indicator equals one for observations dated April 2024 or later, corresponding to the effective date of AB 1228. Each state-month observation is also assigned the prevailing effective minimum wage for use in the elasticity specification described in Chapter 4. State-level minimum wage schedules follow the methodology of Vaghul and Zipperer (2016), extended through 2025 using the U.S. Department of Labor Consolidated Minimum Wage Table. For California, the AB 1228 fast-food rate of $20 per hour is assigned beginning in April 2024; in all other months, the general state minimum wage applies. This assignment overstates the effective minimum wage facing a randomly drawn California youth worker, since the $20 rate applies only to covered fast-food establishments while most employers face the $16 general minimum wage.

The main specifications are estimated without survey weights, following Solon, Haider, and Wooldridge (2015), who show that unweighted regression is consistent and more efficient when the model is correctly specified and the weighting variable is not correlated with the error term. Weighted robustness checks using WTFINL, the CPS final person weight, for the employment outcome and EARNWT, the earner study weight, for wage and hours outcomes are reported in Chapter 6. Results are substantively unchanged under weighting.

Descriptive Statistics

The California youth sample consists of 29,846 individual-month observations in the pre-period (January 2022 through March 2024). The sample is 47.7 percent female and 52.1 percent Hispanic, reflecting California’s demographic composition. School enrollment is high: 25.2 percent are high school students, 31.1 percent are college students, and 43.3 percent are not enrolled. The education distribution is 28.0 percent with a high school diploma, 26.0 percent with some college, and 9.6 percent with a bachelor’s degree, consistent with the age composition of a 16 to 24 sample in which most respondents have not yet completed postsecondary education. The pre-period employment rate is 44.4 percent, with 49.5 percent participating in the labor force; the gap reflects the large share of full-time students classified as not in the labor force. Among employed California youth with valid wage data, mean hourly wages were $17.72 in the pre-period, and 83.2 percent earned at or below $20 per hour, indicating high exposure to the AB 1228 wage floor. Top-coded weekly earnings affect 59 of 7,201 earnings observations (0.82 percent), a negligible share for this age group. Covariate balance between California and the synthetic control unit is reported in Chapter 6 alongside the synthetic control results. All descriptive statistics are shown in Table 2.

Table 2. California Descriptive Statistics: Pre vs. Post AB 1228 (Ages 16–24)

	Pre-Period			Post-Period				
	Mean	SD	N	Mean	SD	N	t	p
Employment Rate	0.444	0.497	29,846	0.431	0.495	16,851	2.71	0.007***
Unemployment Rate	0.047	0.212	29,846	0.063	0.244	16,851	-7.17	<0.001***
NILF Rate	0.505	0.500	29,846	0.500	0.500	16,851	0.87	0.386
Labor Force Part.	0.491	0.500	29,846	0.494	0.500	16,851	-0.65	0.516
Female Share	0.477	0.499	29,846	0.465	0.499	16,851	2.44	0.015**
Hispanic Share	0.521	0.500	29,846	0.532	0.499	16,851	-2.42	0.016**
Student Share	0.562	0.496	29,846	0.573	0.495	16,851	-2.15	0.032**
HS Student Share	0.252	0.434	29,846	0.254	0.436	16,851	-0.67	0.502
College Student Share	0.311	0.463	29,846	0.318	0.466	16,851	-1.66	0.097*
Not Enrolled Share	0.433	0.496	29,846	0.422	0.494	16,851	2.37	0.018**
Mean Age	19.9	2.602	29,846	19.9	2.646	16,851	-0.12	0.901

Notes: Pre = Jan 2022–Mar 2024; Post = Apr 2024–Sep 2025. P-values from Welch's t-test (unequal variances). * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

California's labor code restricts minors under age 18 to three hours of work per day on school days and 18 hours per week during the school year, with similar though not identical restrictions in other states. These constraints create a ceiling on the hours adjustment available to employers hiring teenagers and may amplify any disemployment effect if employers respond to a higher wage floor by shifting toward fewer, longer-hour workers who are not subject to minor labor restrictions.

CHAPTER 4

EMPIRICAL STRATEGY

Two-Way Fixed Effects

The empirical strategy is designed to distinguish between the competitive and monopsony predictions: if competitive forces dominate, the AB 1228 increase should reduce employment and hours among covered workers; if monopsony forces dominate, the employment response should be small or positive, with wage gains as the primary outcome

The primary identification strategy is a two-way fixed effects (TWFE) specification that exploits the full CPS microdata across all 50 states. The TWFE regression takes the form

$$Y_{ist} = \alpha_s + \gamma_t + \tau(CA_s \times Post_t) + X_{ist}\delta + \varepsilon_{ist} \tag{1}$$

where Y_{ist} is the labor market outcome for individual i in state s at time t, $\alpha_s$is a state fixed effect, γ_t is a year-month fixed effect, CA_s is an indicator equal to one for California, $Post_t$ is an indicator equal to one for April 2024 onward, and X_{ist} is a vector of individual-level demographic controls. The coefficient τ captures the differential change in the outcome for California youth relative to all other states after the implementation of AB 1228. Standard errors are clustered at the state level, yielding 51 clusters for reliable inference. For the QCEW quarterly county-level specifications in Chapter 6, where the panel has few clusters and serial correlation is a more immediate concern, HAC standard errors with two lags are reported instead. Because only one state (California) is treated, the effective number of treated clusters is one, which could affect the size of cluster-robust tests. The TWFE sample contains 520,242 individual-month observations across all states for 16- to 24-year-olds, providing substantially more statistical power than the restricted West-Coast comparison. The recent TWFE literature has documented potential bias under staggered treatment adoption (Goodman-Bacon, 2021; Callaway & Sant'Anna, 2021). The setting involves a single treatment event (AB 1228 in April 2024) rather than staggered adoption, but other states enacted minimum wage increases during the sample period. Because these control-state increases are modest relative to the $4 California fast-food increase, and because the TWFE identifies the differential California effect conditional on year-month fixed effects that absorb common shocks, the staggered-adoption concern is attenuated though not eliminated. The TWFE is the preferred specification because it exploits the full sample of 520,242 individual-level observations across 51 clusters, providing substantially greater statistical power than the synthetic control's state-level aggregates. Consistency between the two approaches strengthens confidence in the main findings.

Minimum Wage Elasticity

Following Allegretto, Dube, Reich, and Zipperer (2017), the employment elasticity with respect to the minimum wage is estimated directly by replacing the binary treatment indicator with the log of the effective minimum wage. The specification takes the form

$$Y_{ist} = \alpha_s + \gamma_t + \epsilon \, log \, (MW_{st}) + X_{ist}\delta + u_{ist} \qquad (2)$$

where $MW_{st}$is the effective minimum wage in state s at time t, $\alpha_s$absorbs permanent cross-state differences, and $\gamma_t$absorbs seasonality. The coefficient ϵ is identified from within-state variation in minimum wages over time and is interpreted as the elasticity of the employment rate: a 10 percent minimum wage increase is associated with an ϵ times 10 percentage point change in the employment rate. Each state-month is assigned its prevailing effective minimum wage, with California assigned the AB 1228 fast-food rate of $20 per hour beginning in April 2024 and the general state minimum wage in prior months. This framework makes the results directly comparable to the Allegretto and Dube literatures, which use the same log specification to estimate employment elasticities across policy episodes.

Identifying Assumptions

The TWFE specification relies on the parallel trends assumption: conditional on state and year-month fixed effects, California youth employment would have followed the same trajectory as the national average absent treatment. The state fixed effects absorb permanent cross-state differences in employment levels, while the year-month fixed effects absorb aggregate shocks common to all states, including national business cycle fluctuations and seasonal patterns in youth employment. The remaining identifying variation is the differential change in California relative to all other states coinciding with the April 2024 implementation of AB 1228. This assumption does not hold cleanly. Anticipatory effects tests on the all-state specification yield negative pre-treatment coefficients at every monthly lead from October 2023 through the actual treatment date (Appendix Table A1), and a linear pre-trend test on the all-state all-young adult sample fails both including and excluding the potential anticipatory period (Appendix Table A2). This pre-existing downward drift in California youth employment relative to comparison states means the all-state TWFE point estimate of −0.80 percentage points should be read as an upper bound on any AB 1228 employment effect, not as a clean causal estimate. The synthetic control method addresses this concern through a data-driven control group with permutation-based inference, and its null finding (Chapter 6) suggests that the TWFE significance is partly driven by pre-existing trends rather than treatment. Pre-trend diagnostics are reported in the Appendix, and the synthetic control fit is documented in Chapter 6.

Additional Specifications

Several additional specifications are reported in Chapters 5 and 6 to probe the robustness of the main findings. Two triple-difference estimators test for differential effects along the age margin: the first compares teenagers (16 to 19) to young adults (20 to 24) within the youth sample, and the second compares the full youth group (16 to 24) to workers aged 25 to 35 as a within-state control group to difference out state-specific shocks that affect all age groups equally. A restricted difference-in-differences comparison using only Nevada, Arizona, and Oregon tests sensitivity to the choice of control group, with pre-trend diagnostics and event study figures reported alongside the estimates. A trend-adjusted specification adds a group-specific linear time trend for the teen subsample to address the pre-treatment divergence observed in that age group. Quantile regressions test the distributional pattern of wage effects across the wage distribution. Finally, all main results are re-estimated with CPS survey weights to verify that the unweighted estimates are not driven by differential sampling probabilities.

CHAPTER 5

RESULTS

Two-Way Fixed Effects

The all-state TWFE specification, which exploits monthly individual-level data across all 50 states with 51 clusters, detects a statistically significant employment decline. Without demographic controls, the Treat × Post coefficient is −1.30 percentage points (SE = 0.0028, $p < 0.001$). Adding age, gender, and education controls attenuates the estimate to −0.80 percentage points (SE = 0.0024, $p = 0.001$), indicating that a substantial portion of the raw decline reflects compositional differences between California youth and the national average rather than a causal policy effect. Table 3 reports both specifications. The 0.80 percentage point employment decline is statistically significant ($p = 0.001$) but modest in economic terms: applied to the approximately 4.5 million Californians aged 16 to 24 (U.S. Census Bureau, 2024 American Community Survey), a −0.80 percentage point decline implies roughly 36,000 fewer employed youth statewide. While the all-industry measurement approach attenuates the estimated percentage effect (as discussed in Chapter 3), the headcount translation is unaffected by this dilution. The number of displaced workers is the same whether measured as a small percentage of a large population or a large percentage of a small one.

Table 3. Two-Way Fixed Effects: Employment (All States, Ages 16–24)

	employed	
	(1)	(2)
	State + Month FEs	+ Demographics
CA × Post	−0.0130***	−0.0080***
	(0.0028)	(0.0024)
Demographics	No	Yes
Observations	520,242	520,242
R^2	0.018	0.216

Notes: Dependent variable is a binary indicator for employment (including NILF in denominator). All specifications include state and year-month fixed effects. Demographics include age, sex, race, Hispanic origin, education, marital status, and school enrollment. Standard errors clustered at state level (51 clusters) in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Age Heterogeneity

The aggregate employment result masks substantial age heterogeneity. The all-state TWFE with demographic controls shown in Table 4 estimates a 3.17 percentage point employment decline for teenagers aged 16 to 19 (SE = 0.004, $p < 0.001$) and a 1.18 percentage point employment gain for young adults aged 20 to 24 (SE = 0.004, $p = 0.003$). The opposing signs indicate that the aggregate TWFE coefficient of −0.80 percentage points reflects a weighted average of a larger teen decline and a smaller young adult gain, with the teen effect dominating because teenagers have lower baseline employment rates and are more exposed to the minimum wage floor.

Table 4. Employment by Age Group (All States, TWFE with Demographics)

	employed	
	(1)	(2)
	Teens (16–19)	Young Adults (20–24)
CA × Post	−0.0317***	+0.0118***
	(0.0035)	(0.0040)
Observations	245,464	274,778
Demographics	Yes	Yes

Notes: Dependent variable is a binary indicator for employment (including NILF in denominator). State and year-month FEs and demographics. Teen estimate subject to pre-trend concerns. Standard errors clustered at state level (51 clusters) in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

The teen employment decline should be interpreted with caution. The West-Coast event study for teenagers (Figure 5) reveals a clear downward drift in California teen employment relative to controls throughout the pre-period, predating the April 2024 policy implementation. When a group-specific linear time trend is added to the West-Coast specification to absorb this pre-existing divergence, the teen employment estimate flips to +2.73 percentage points (SE = 0.027, $p = 0.304$), rendering it statistically insignificant (Appendix Table A3). The all-state TWFE partially addresses this concern by using 50 control states rather than three, but it cannot fully resolve a California-specific pre-trend in teen employment. The teen results are therefore suggestive of disemployment but not robust to trend adjustment.

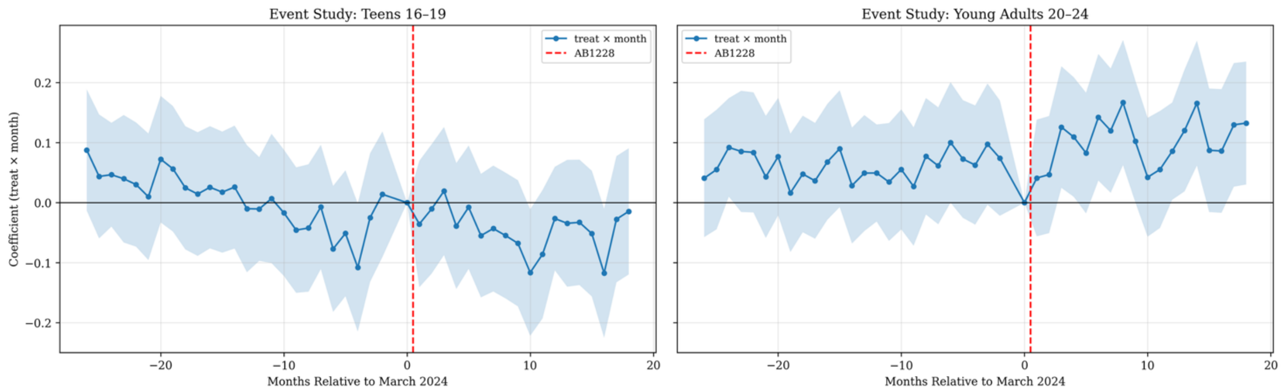

Figure 5. Event Study by Age Group

The young adult employment gain of 1.18 percentage points is statistically significant in the all-state TWFE ($p = 0.003$) and is accompanied by a 1.95 percentage point increase in labor force participation ($p < 0.001$), suggesting that the higher wage floor attracted workers who would otherwise have remained out of the labor force. The West-Coast pre-trend test for this age group is not significant ($p = 0.230$), the event study coefficients show no pre-period drift (Figure 5, right panel), and the all-state TWFE absorbs national trends through year-month fixed effects, supporting a causal interpretation.

The secondary outcomes (Table 5) show the same age group pattern. Teenagers experience a 6.61 percentage point increase in unemployment ($SE = 0.003$, $p < 0.001$) and a 1.95 percentage point decline in labor force participation ($p < 0.001$), while young adults show a smaller unemployment increase of 0.92 percentage points ($SE = 0.002$, $p < 0.001$) alongside a 1.95 percentage point increase in labor force participation ($p < 0.001$). The teen unemployment increase is subject to the same pre-trend concerns documented above. The young adult labor force participation increase indicates that the $20 wage floor attracted marginal workers into the labor force.

Table 5. Secondary Outcomes by Age Group

	(1)	(2)	(3)	(4)
	Teen Unemp.	Teen LFP	YA Unemp.	YA LFP
CA × Post	+0.0661***	−0.0195***	+0.0092***	+0.0195***
	(0.0030)	(0.0037)	(0.0022)	(0.0038)
Observations	94,828	245,464	196,778	274,778
Demographics	Yes	Yes	Yes	Yes

Notes: Cols (1) and (3) restrict to LF participants. Cols (2) and (4) include the full sample. All specifications include state and year-month FEs and demographic controls. Teen estimates subject to pre-trend concerns. Standard errors clustered at state level (51 clusters) in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Mechanism Checks

I examine hourly wages and weekly hours as mechanism checks on the employed subsamples to interpret the employment finding. Table 6 presents the all-state TWFE estimates for log hourly wages and log weekly hours, estimated with state and year-month fixed effects and a full set of demographic controls.

The all-state TWFE estimates a 2.1 percent increase in hourly wages (SE = 0.004, $p < 0.001$, N = 50,281) and a 3.8 percent decline in weekly hours (SE = 0.004, $p < 0.001$, N = 258,152), with all dependent variables in logs so the coefficients are interpreted as approximate percentage changes. All effects are precisely estimated with 51 state clusters. The wage effect is smaller than the 8 to 9 percent reported by Reich and Sosinskiy (2024) using administrative fast-food data, as expected given measurement dilution from the all-industry youth sample; the QCEW border wage estimate of +8.3 percent is closer to their finding.

Table 6. Log Wage and Log Hours Estimate (All States, Ages 16–24)

	(1)	(2)
	Log Hourly Wage	Log Weekly Hours
CA × Post	+0.0210***	−0.0380***
	(0.0043)	(0.0038)
Observations	50,281	258,152
Demographics	Yes	Yes

Notes: All specifications include state and year-month FEs and demographic controls. Wage regression esimated using employed individuals with valid hourly wage. Hours regression estimated using employed individuals with AHRSWORKT < 999. Standard errors clustered at state level (51 clusters) in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Quantile regressions (Table 7) on the all-state sample with demographic controls reveals that wage gains are concentrated at the median and above. The treat-by-post coefficient is −\$0.15 (p = 0.414) at Q10, +\$0.18 (p = 0.225) at Q25, +\$0.50 (p = 0.013) at Q50, +\$0.79 (p < 0.001) at Q75, and +\$0.88 (p = 0.012) at Q90. The null effects at Q10 and Q25 indicate that wages at the bottom of the distribution did not increase significantly relative to controls, while wages at the median and above rose substantially. The Q50 effect of +\$0.50 represents approximately 2.8 percent of the pre-period mean hourly wage of \$17.72, comparable in magnitude to the 2.1 percent mean wage increase from the TWFE log specification but estimated in levels rather than logs. Because the California general minimum wage of \$16 is relevant for most young workers in the sample (the pre-period mean wage is \$17.72), the additional effect of AB 1228 may naturally concentrate above the median, where the \$20 fast-food floor is most binding relative to the prevailing wage. A back-of-envelope calculation at the pre-period mean illustrates the tension: a 2.1 percent wage increase implies approximately +\$0.37 per hour, while the 3.8 percent hours reduction implies a loss of 1.15 hours per week (based on the mean hours of 30.3), or approximately \$23.00 at the new \$20 fast-food floor. Although the hourly wage rose, the back-of-envelope calculation suggests the hours reduction dominated at the mean, leaving average weekly earnings roughly flat or slightly lower for a typical worker.

Table 7. Quantile Regression: Hourly Wage (All States, Ages 16–24)

			Hourly Wage (\$)		
	Q10	Q25	Q50	Q75	Q90
CA × Post	−0.15 (0.18)	+0.18 (0.15)	+0.50**(0.20)	+0.79***(0.18)	+0.88**(0.35)
Observations			50,281		
Demographics			Yes		

Notes: Regressions estimated with employed individuals with valid hourly wage. All specifications include state and year-month FEs and demographic controls. SE in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

The combination of a small or null aggregate employment effect, a positive mean wage effect, and a significant hours reduction indicates that the primary margin of employer adjustment was intensive, not extensive. The quantile results suggest that the wage gains reflect compositional shifts in the employed workforce not uniform wage compression, a finding the education sorting results also support. The substantial hours reduction and compositional reallocation mean that the welfare implications for workers are more complex than a simple comparison of employment and wage effects would suggest.

Secondary Outcomes

Table 8 shows the unemployment rate among labor force participants, estimated on the all-state TWFE with 291,606 observations and 51 state clusters, rises by 2.21 percentage points with demographic controls (SE = 0.0018, $p < 0.001$), indicating a modest but robust increase in unemployment among those actively seeking work. The unemployment increase (2.21 percentage points) is larger than the employment-to-population decline (0.80 percentage points) because the two measures have different denominators: the unemployment rate conditions on labor force participants, a smaller base, while the employment-to-population ratio includes all individuals aged 16 to 24. A given number of job losses produces a larger percentage point change in the smaller denominator. The two estimates are consistent and confirm that the aggregate employment decline reflects genuine labor market softening rather than measurement noise.

Table 8. Unemployment Rate and Labor Force Participation (All States, Ages 16–24)

	(1)	(2)
	Unemployment Rate	LF Participation
CA × Post	+0.0221***	+0.0017
	(0.0018)	(0.0024)
Observations	291,606	520,242
Demographics	Yes	Yes

Notes: Col (1) restricted to LF participants. Col (2) uses the full sample, including NILF. All specifications include state and year-month FEs and demographic controls. Standard errors clustered at state level (51 clusters) in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

The implied minimum wage employment elasticity, estimated by regressing the employment-to-population ratio on the log of the effective state minimum wage with state and month fixed effects and demographic controls following Allegretto, Dube, Reich, and Zipperer (2017), is +0.028 (SE = 0.030, $p = 0.356$), indicating that a 10 percent minimum wage increase is associated with a negligible 0.28 percentage point employment change (Appendix Table A4). This elasticity is computed with respect to overall youth employment across all industries, but the $20 wage floor applied only to a subset of fast-food employers; the within-sector employment elasticity is likely substantially larger in magnitude, as the QCEW all-state estimate of a 2.7 percent fast-food employment decline indicates, even though the contiguous county border specification yields a precise null. Teenagers show a negative but insignificant elasticity of −0.033 (SE = 0.045, $p = 0.473$), while young adults show a statistically significant positive elasticity of +0.080 (SE = 0.038, $p = 0.036$). These estimates are within the range reported by Dube, Lester, and Reich (2010) and Allegretto, Dube, and Reich (2011), and consistent with the consensus summarized in Dube (2019) that the most precise estimates suggest employment elasticities close to zero.

Education Sorting

The education gradient in employment effects, estimated on the all-state TWFE with demographic controls, documents compositional reallocation within the youth labor market (Table 9). Workers with less than a high school diploma experience a 3.25 percentage point employment decline (SE = 0.004, $p < 0.001$) and workers with a high school diploma show a similar decline of 3.21 percentage points (SE = 0.005, $p < 0.001$). In contrast, workers with some college experience have a 2.41 percentage point employment gain (SE = 0.004, $p < 0.001$), while those with a bachelor's degree or higher show a positive but insignificant coefficient of +0.98 percentage points (SE = 0.007, $p = 0.154$). Three of four education groups reach statistical significance, and the pattern — negative effects for the two lowest education groups and positive for the two highest — points toward sorting or displacement rather than uniform wage compression.

Table 9. Employment by Education Level (All States, Ages 16–24)

	employed			
	(1)	(2)	(3)	(4)
	Less than HS	HS Diploma	Some College	Bachelor's+
CA × Post	−0.0325***	−0.0321***	+0.0241***	+0.0098
	(0.0040)	(0.0050)	(0.0040)	(0.0070)
Observations	177,899	151,428	140,659	50,256
Demographics	Yes	Yes	Yes	Yes

Notes: Less than HS (EDUC ≤ 71), HS Diploma (73), Some College (81–92), Bachelor's+ (≥ 111). All specifications include state and year-month FEs and demographic controls. Standard errors clustered at state level (51 clusters) in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

A finer decomposition of the less-than-high-school group into high school dropouts (aged 18 and older with no diploma) and minors who have not yet graduated is not feasible with the CPS data: the former group contains only 559 observations and the latter only 145, precluding reliable estimation. The CPS education variable (EDUC) codes the highest degree completed but does not distinguish current enrollment status from dropout status; the SCHLCOLL variable captures school enrollment separately but cannot recover whether a 17-year-old without a diploma is on track to graduate or has left school. This limitation means the less-than-high-school employment decline of 3.25 percentage points should be interpreted as an average across both subgroups, though the concentration of this category among minors subject to labor restrictions suggests the effect may be driven primarily by teenagers who have not yet completed high school.

CHAPTER 6

ROBUSTNESS

Synthetic Control

As a robustness check on the TWFE results, the synthetic control method of Abadie, Diamond, and Hainmueller (2010, 2015) constructs a data-driven counterfactual for California by computing an optimally weighted average of all other U.S. states that best reproduces California's pre-treatment employment trajectory and observable characteristics. The method selects donor weights by minimizing the distance between the treated unit and its synthetic counterpart over the pre-treatment period, where distance is defined over both predictor variables and lagged outcomes. The donor pool consists of all 50 states plus the District of Columbia, and statistical significance is evaluated via a permutation test in which the procedure is repeated for each state as a placebo-treated unit. The data-driven weighting eliminates the need for researcher-selected control states, and permutation-based inference provides valid p-values for a single treated unit without relying on asymptotic cluster corrections.

The synthetic control estimates an average treatment effect on the treated (ATT) of −2.05 percentage points, indicating that the employment-to-population ratio among 16- to 24-year-olds in California fell slightly below its synthetic counterfactual following the implementation of AB 1228. However, this gap is not statistically significant: California ranks 33rd out of 51 states in the permutation distribution of post-to-pre RMSPE ratios, yielding a permutation p-value of 0.647. The synthetic California is constructed primarily from New York (46 percent weight), Texas (42 percent), New Mexico (9 percent), and New Jersey (3 percent), with all remaining states receiving zero or negligible weight. Notably, none of the three West-Coast comparison states (Nevada, Arizona, Oregon) receives meaningful weight, confirming that the data-driven procedure selects different comparators than the researcher-chosen control group. The pre-period RMSPE of 0.023 indicates adequate pre-treatment fit, and covariate balance is good on age, gender, education, and pre-period employment levels, though imperfect on Hispanic share. The SC point estimate (−2.05 pp) is directionally consistent with but larger than the TWFE estimate (−0.80 pp), while the permutation-based inference cannot reject the null, which is consistent with the lower statistical power inherent in state-level aggregation relative to the individual-level TWFE. Figure 6 presents the synthetic control inference: panel (a) displays the month-by-month employment gap; panel (b) overlays California's gap (black) against all 50 placebo state gaps (gray), showing that the post-treatment divergence is unremarkable relative to the cross-state distribution; and panel (c) shows the permutation distribution of post-to-pre RMSPE ratios, with California's ratio falling well within the distribution.

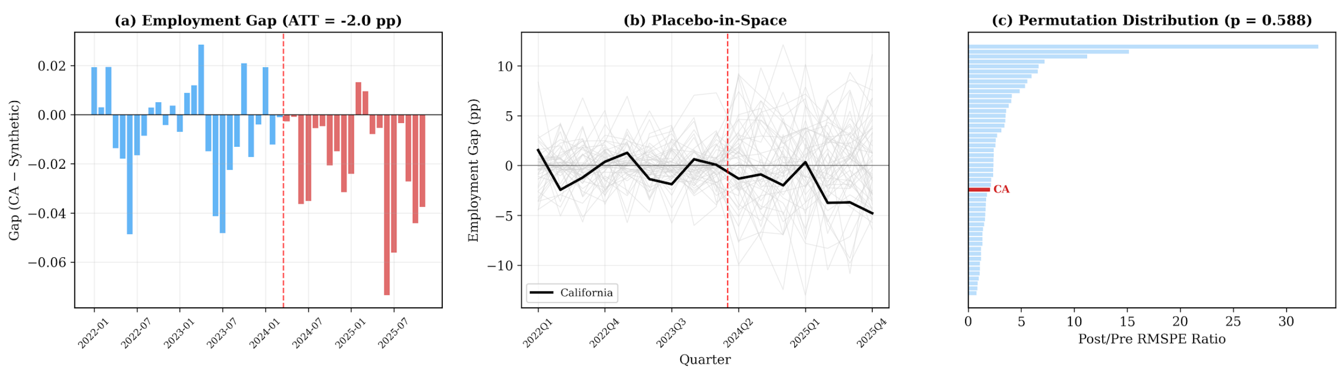

Figure 6. Synthetic Control Inference

This null finding aligns with Reich and Sosinskiy (2024), who find no reduction in fast-food employment using California administrative data, and with Hamdi and Sovich (2025), who report near-zero net employment effects accompanied by reduced turnover. It contrasts with Clemens, Edwards, and Meer (2025), who estimate employment losses of 2.7 to 3.2 percent using Quarterly Workforce Indicators data focused on the fast-food sector. Several factors may account for this discrepancy: Clemens et al. measure establishment-level employment in fast food specifically, while this study captures the broader youth labor market across all industries; this study's QCEW border county analysis suggests that sector-specific displacement is real but absorbed through cross-sector reallocation; and Clemens et al.'s sample ends in September 2024, while this study extends through September 2025.

Long Differences

As a complement to the synthetic control and TWFE specifications, a long-difference estimator collapses the all-state youth sample to state-level pre-period and post-period means and regresses the change in employment on the California treatment indicator. This approach averages over monthly variation and provides a simple check on whether the main findings are driven by the particular functional form of the TWFE or the weighting procedure of the synthetic control. With 102 state-period observations (51 states times two periods), the raw long-difference estimate for employment is −1.57 percentage points (HC1 SE = 0.013, p = 0.228). A residualized specification that first removes the variation explained by age, sex, race, Hispanic origin, and education before collapsing to state means yields −1.54 percentage points (HC1 SE = 0.012, p = 0.179), confirming that demographic composition has little effect on the collapsed estimates. Both point estimates fall between the synthetic control (−2.05 percentage points) and the TWFE with controls (−0.80 percentage points), and like the synthetic control neither reaches statistical significance. The residualized long-difference wage estimate is +$0.40 per hour (HC1 SE = $0.182, p = 0.033), confirming a statistically significant positive wage effect. Heteroskedasticity-consistent standard errors are reported because each state contributes exactly two observations (one pre, one post), making cluster-robust inference unreliable. The consistency across the synthetic control, TWFE, and long-difference approaches strengthens confidence that the main findings are not artifacts of any single estimator (Appendix Table A5).

West-Coast DiD, Pre-Trend Diagnostics, and Placebo Tests

A restricted difference-in-differences comparison using only California, Nevada, Arizona, and Oregon (N = 70,386) is estimated as a descriptive benchmark. Across four specifications, the employment coefficient ranges from −0.23 to −0.72 percentage points, none statistically significant (p = 0.390 to 0.789), consistent in direction with the synthetic control and TWFE but with limited statistical power from four clusters (Appendix Table A6). The aggregate null holds when any single control state is dropped.

That said, this estimator does not support causal inference for the full sample or the teen subgroup. A linear pre-trend test yields $p = 0.014$ for the full 16 to 24 age group, with the divergence concentrated among teenagers ($p < 0.001$); young adults show no significant pre-trend ($p = 0.230$). The underlying regression coefficients are reported in Appendix Table A7. Placebo tests assigning false treatment dates in January, June, and September 2023 each produce significant negative coefficients, confirming that the West-Coast DiD detects a differential decline in California youth employment throughout the pre-period even absent the policy (Appendix Table A8). The significant placebo coefficients are not evidence of a spurious treatment effect; they reflect the pre-existing downward drift in California teen employment documented above, which caused the West-Coast estimator to detect differential declines even in the pre-period. The all-state TWFE is also subject to this concern. The West-Coast DiD is reported to confirm the direction of the main estimates rather than as a source of causal identification. The synthetic control, which constructs a counterfactual that reproduces California's pre-treatment trajectory by design and assigns zero weight to any West-Coast state, is not subject to this concern.

Triple-Difference: Youth Versus Older Workers

A triple-difference specification compares the full youth group aged 16 to 24 to workers aged 25 to 35 on the all-state sample with state and year-month fixed effects. The coefficient on the triple interaction (California times post times youth) is −1.50 percentage points ($SE = 0.006$, $p = 0.014$, $N = 1{,}207{,}393$), indicating that the policy differentially reduced employment for youth relative to older workers in California. The magnitude is modest (less than 2 percentage points) and broadly consistent with the TWFE findings. A complementary teen-versus-young-adult triple-difference, reported in Appendix Table A9, yields a triple interaction of −5.87 percentage points ($p < 0.001$), confirming the substantial age heterogeneity documented in Chapter 5.

QCEW Establishment-Level Evidence

Supplementary analysis using Bureau of Labor Statistics Quarterly Census of Employment and Wages data for NAICS 722513 (limited-service restaurants) triangulates the CPS household survey findings with administrative employer records. The all-state QCEW TWFE (Table 10), estimated with state and quarter fixed effects on 765 state-quarter observations across 51 clusters, detects a 2.7 percent decline in limited-service restaurant employment (SE = 0.003, $p < 0.001$) and a 5.4 percent increase in average weekly wages (SE = 0.004, $p < 0.001$). The number of establishments rose 2.1 percent (SE = 0.004, $p < 0.001$), suggesting net entry despite higher labor costs. Table 10 reports the full results. The employment decline of 2.7 percent is strikingly close to the 2.7 to 3.2 percent range reported by Clemens, Edwards, and Meer (2025) using Quarterly Workforce Indicators, providing independent confirmation of the sector-specific disemployment finding with a different administrative data source.

Table 10. QCEW All-State TWFE (NAICS 722513, Limited-Service Restaurants)

	Log Employment	Log Avg. Weekly Wage	Log Establishments
CA × Post	−0.027***	+0.054***	+0.021***
	(0.003)	(0.004)	(0.004)
State-quarters	765	765	765
States	51	51	51
State FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes

Notes: NAICS 722513 (limited-service restaurants). All 50 states plus DC. 2022Q1–2025Q3. Standard errors clustered at state level (51 clusters) in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

The contiguous county border design, motivated by Dube, Lester, and Reich (2010), compares QCEW outcomes in California border counties to geographically adjacent control counties across the state line. Five border pairs are pooled: San Bernardino County (CA)–Clark County (NV), Imperial County (CA)–Yuma County (AZ), Riverside County (CA)–Mohave County (AZ), Placer County (CA)–Washoe County (NV), and El Dorado County (CA)–Douglas County (NV). The pooled regression estimates log employment and log wages on 150 county-quarter observations (10 counties × 15 quarters, 2022Q1 through 2025Q3) with a treatment indicator for California counties, a post-period indicator for 2024Q2 onward, their interaction, and a pair fixed effect to absorb level differences across border pairs. Standard errors are HAC with two lags. The pooled border estimate (Table 11) yields a precise null on employment (−1.65 percent, $p = 0.650$) while wages rose 8.3 percent ($p < 0.001$). The wage pass-through matches the 8 to 9 percent reported by Reich and Sosinskiy (2024). The null employment estimate at the border tempers the all-state QCEW disemployment result and is consistent with the cross-sector reallocation interpretation: any fast-food displacement at the local level appears to have been substantially absorbed by neighboring sectors and counties rather than translating into lasting employment loss at the border. With only five border pairs, statistical power is limited and any county-specific shock could influence the point estimate. The QCEW border evidence therefore neither confirms nor refutes the all-state QCEW finding of fast-food disemployment, but it does argue against interpreting that finding as a clean causal estimate of localized job loss.

Table 11. QCEW Contiguous County Border DiD (NAICS 722513)

	(1)	(2)
	Log Employment	Log Avg. Weekly Wage
CA × Post	−0.0165(0.0360)	+0.0830***(0.0110)
County-quarters	150	150
Border pairs	5	5
Pair FE	Yes	Yes
HAC SE (2 lags)	Yes	Yes

Notes: Pairs: San Bernardino (CA)–Clark (NV), Imperial (CA)–Yuma (AZ), Riverside (CA)–Mohave (AZ), Placer (CA)–Washoe (NV), El Dorado (CA)–Douglas (NV). 10 counties × 15 quarters = 150 county-quarters, 2022Q1–2025Q3. Post = 2024Q2+. HAC SE with 2 lags. *p < 0.1, **p < 0.05, ***p < 0.01.

Weighted Estimates

All main employment and wage results are substantively unchanged when estimated with CPS survey weights (WTFINL for employment, EARNWT for wages and hours), confirming that the unweighted estimates are not driven by differential sampling probabilities (Appendix Table A10). The demographic composition of the California youth sample is stable between the pre and post periods, though the composition of employed workers shifts slightly toward older and more educated individuals, consistent with the education sorting mechanism documented in Chapter 5.

CHAPTER 7

DISCUSSION AND CONCLUSION

Using CPS Outgoing Rotation Group data from January 2022 through September 2025, this study estimates the effect of California's AB 1228, the first statewide sector-specific minimum wage in United States history, on youth employment, wages, and hours. The all-state TWFE detects a significant employment decline of 0.80 percentage points with demographic controls ($p = 0.001$). The synthetic control robustness check finds a directionally consistent but statistically insignificant gap of −2.05 percentage points (permutation $p = 0.647$), consistent with the lower power of state-level aggregation. Hourly wages rose 2.1 percent in the all-state TWFE, and weekly hours declined 3.8 percent ($p < 0.001$).

The central tension in the results is between the small-but-significant all-state CPS employment estimate, which fails pre-trend diagnostics, and the synthetic control null. Read together, the aggregate employment effect of AB 1228 on 16- to 24-year-olds is best characterized as not robust: the policy may have produced a small employment decline, but the available evidence does not support a confident statement that it did. The QCEW administrative data tells a parallel story. The all-state TWFE detects a 2.7 percent decline in limited-service restaurant employment, close to the 2.7 to 3.2 percent range reported by Clemens, Edwards, and Meer (2025), but the five-pair contiguous county border design yields a precise null on employment ($p = 0.650$) alongside an 8.3 percent wage increase. These pieces are consistent with the view that any fast-food displacement was localized, modest, and substantially absorbed through cross-sector or cross-county reallocation rather than translating into durable job loss. The reallocation interpretation is corroborated by the education sorting evidence in Chapter 5, which is the most robust finding in the study. Clemens, Edwards, Meer, and Nguyen (2026) provide complementary evidence on the price channel, documenting a 3.3 to 3.6 percent increase in food-away-from-home prices in California MSAs following AB 1228 and attributing the magnitude partly to compositional turnover, in which lower-priced, lower-rated restaurants were disproportionately likely to exit. This employer-side compositional change plausibly contributes to the labor-force compositional shifts documented in this study: if exiting establishments disproportionately employed less-educated workers, the observed education sorting in employment outcomes would emerge even without direct worker displacement.

The data does not definitively distinguish between the competitive and monopsony models, and the more honest reading of the evidence is agnostic between them. The competitive model predicts disemployment, hours reductions, and price pass-through; the all-state TWFE point estimate, the significant hours decline, and the 3.3 to 3.6 percent price increase documented by Clemens et al. (2026) all fit this prediction. The monopsony model predicts near-zero employment effects with wage gains and possible labor force participation increases; the synthetic control null, the positive young adult labor force participation response, and the near-zero implied employment elasticity all fit this prediction. The fast-food labor market plausibly sits somewhere between the two polar models, with different margins responding through different channels simultaneously. What the data do support is that the policy operated through multiple channels at once: modest, possibly null, aggregate employment effects; real but localized sectoral displacement; intensive-margin hours adjustment; compositional reallocation across education groups; and consumer price pass-through. This composite picture cannot be neatly assigned to either model and is best read as evidence that policy effects in real labor markets work through several margins simultaneously rather than through any single textbook mechanism.

Two features of the research design warrant emphasis when interpreting the magnitude of the estimated effects. First, the employment elasticity of +0.028 is computed with respect to overall youth employment, but the $20 wage floor applied only to a subset of employers in the fast-food sector; the within-sector employment effect is likely substantially larger, the within-sector QCEW all-state estimate of −2.7 percent provides a sector-specific magnitude, while the contiguous county border evidence is statistically null. Second, the effective treatment intensity may be understated because some California workers were already in jurisdictions with local minimum wages above $16, reducing the bite of the $20 floor, and because control states also enacted minimum wage increases during the sample period, attenuating the measured differential.

The sector-specific design itself is informative even under the more cautious reading of the aggregate employment evidence. The contrast between an arguably null aggregate employment effect, a measurable wage gain, a meaningful hours reduction, and a sharp education-based reallocation suggests that sector targeting concentrated adjustment within the covered industry while allowing the broader youth labor market to absorb the shock through hours and compositional margins. A uniform minimum wage increase of the same magnitude would not generate the same cross-sector and cross-education reallocation pattern. The principal policy lesson is that aggregate employment stability does not imply distributional neutrality. Higher hourly compensation came alongside fewer hours and a workforce that shifted away from less-educated workers, precisely the group the policy was nominally designed to help. Policymakers contemplating sector-specific or large minimum wage increases should consider complementary measures that mitigate the distributional consequences for the least-educated workers, including a graduated multi-year phase-in that gives employers time to adjust without sharp compositional substitution, paired training or wage subsidies targeted at workers without a high school diploma, and apprenticeship or work-experience programs in the covered sector. Without such complements, sector-specific wage floors risk raising hourly compensation for the workers who keep their jobs while reducing the employment opportunities of those most in need of an entry-level wage.

Several limitations of this analysis warrant acknowledgment. The CPS cannot precisely identify AB 1228-covered workers, so the main estimates capture the broad youth labor market effect rather than the fast-food-specific effect. The post-treatment period of 18 months may be insufficient for longer-run adjustments; Meer and West (2016) find effects grow over two years. The pre-trend concern prevents clean teen-specific identification. Control states enacted modest MW increases during the sample period. The composition of employed workers shifted slightly toward older and more educated individuals, so some wage gains may reflect selection. The post-treatment period coincided with California-specific disruptions from wildfires and technology sector layoffs that could independently affect youth employment.

Matched employer-employee data would permit establishment-level identification of AB 1228-covered workplaces. The cross-sector reallocation hypothesis could be tested directly using QCEW data for non-restaurant sectors in the same border counties. The education sorting finding warrants further investigation with data that can track individual workers across employers and industries.

APPENDIX A. ROBUSTNESS AND SUPPLEMENTARY TABLES

Table A1. Anticipatory Effects Tests (Monthly Lead Shifts)

		employed	
Treatment Date	Coefficient (β)	Std. Error	p-value
Apr 2024 (actual)	-0.0080***	(0.0024)	0.001
Mar 2024 (−1 mo)	-0.0074***	(0.0026)	0.005
Feb 2024 (−2 mo)	-0.0072***	(0.0026)	0.007
Jan 2024 (−3 mo)	-0.0056**	(0.0027)	0.039
Dec 2023 (−4 mo)	-0.0060**	(0.0027)	0.028
Nov 2023 (−5 mo)	-0.0064**	(0.0028)	0.020
Oct 2023 (−6 mo)	-0.0052*	(0.0027)	0.053
Sep 2023 (−7 mo)	-0.0071***	(0.0027)	0.008

Notes: All-state TWFE estimates of CA × Post on employed (binary), with the treatment date shifted backward by 0 to 7 months. Sample: ages 16–24, all 50 states + DC, January 2022 through September 2025; N = 520,242. Specification includes state fixed effects (51 clusters), year-month fixed effects, and individual demographic controls (age, sex, Hispanic origin, race, education, marital status, school enrollment). Standard errors clustered at the state level. Coefficients are negative and largely significant at every monthly lead, a pattern consistent with the pre-existing differential trend in California youth employment rather than discrete anticipation of AB 1228. * p<0.10, ** p<0.05, *** p<0.01.

Table A2. All-State TWFE Linear Pre-Trend Tests — Full Pre-Period vs. Excluding Anticipation Window

	Full Pre-Period (Jan 2022 – Mar 2024)			No Anticip. (Jan 2022 – Sep 2023)		
	Full (16–24)	Teens (16–19)	YA (20–24)	Full (16–24)	Teens (16–19)	YA (20–24)
treat × time_index	−0.00104***	−0.00188***	−0.00032	−0.00252***	−0.00424***	−0.00106**
	(0.00021)	(0.00026)	(0.00029)	(0.00030)	(0.00038)	(0.00045)
Observations	324,992	153,381	171,611	248,115	117,203	130,912
Pre-period months	27	27	27	21	21	21
p-value	<0.001	<0.001	0.268	<0.001	<0.001	0.018
Pre-trend verdict (α = 0.10)	Fail	Fail	Pass	Fail	Fail	Fail

Notes: OLS regression of employed on treat, time_index, treat × time_index, state fixed effects, and individual demographic controls (age, sex, Hispanic origin, race, education, marital status, school enrollment) estimated on the pre-period all-state sample. All 50 states plus DC, CPS ORG, ages 16–24 including NILF in the denominator. The left panel uses the full pre-period (January 2022 – March 2024, 27 months). The right panel restricts the sample to January 2022 – September 2023 (21 months), excluding the post-signing / pre-implementation window (October 2023 – March 2024) to rule out anticipatory effects. time_index is months from the start of each sample window. Standard errors clustered at the state level (51 clusters) in parentheses. Null: coefficient on treat × time_index equals zero (parallel trends). * p<0.10, ** p<0.05, *** p<0.01.

Table A3. Trend-Adjusted Teen Employment (West-Coast, Ages 16–19)

	employed		
	(1)	(2)	(3)
	Month FEs	+ Linear Trend	+ Quadratic Trend
treat × post	−0.0482***	+0.0273	+0.0198
	(0.0090)	(0.0266)	(0.0324)
Observations	32,996	32,996	32,996

Notes: West-Coast sample (CA vs NV/AZ/OR), teens only. Adding a group-specific linear time trend absorbs the pre-existing divergence and renders the teen estimate insignificant. Standard errors clustered at state level in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01

Table A4. Minimum Wage Employment Elasticity

	employed		
	(1)	(2)	(3)
	All (16–24)	Teens (16–19)	Young Adults (20–24)
Panel A: All-State (51 clusters)			
log(MW)	+0.0277	−0.0326	+0.0797**
	(0.0300)	(0.0454)	(0.0380)
Panel B: West-Coast (4 states)			
log(MW)	−0.0241	−0.1075**	+0.0442***
	(0.0211)	(0.0435)	(0.0108)
N (All-State)	520,242	245,464	274,778
N (West-Coast)	70,386	32,996	37,390
Demographics	Yes	Yes	Yes

Notes: Elasticity specification: employed = $\alpha\_s + \gamma\_m + \varepsilon \cdot \log(MW\_st) + X\delta + u$. State and month FEs. For CA, AB1228 $20 rate assigned from Apr 2024. Panel A: 51 state clusters. Panel B: 4 clusters (limited power). *p < 0.1, **p < 0.05, ***p < 0.01.

Table A5. Long Differences (All States, Ages 16–24)

	(1)	(2)	(3)	(4)
	Employment (raw)	Employment (residualized)	Wage ($) (raw)	Wage ($) (residualized)
CA	−0.0157	−0.0154	+0.40**	+0.39**
	(0.0130)	(0.0115)	(0.20)	(0.18)
State-periods	102	102	102	102

Notes: Collapses all-state youth sample to state-level pre/post means. HC1 standard errors. Residualized specs first remove demographic variation before collapsing. *p < 0.1, **p < 0.05, ***p < 0.01.

Table A6. West-Coast Employment DiD (CA vs NV/AZ/OR, Ages 16–24)

	employed			
	(1)	(2)	(3)	(4)
	Month FEs	+ Controls	Quarter FEs	+ Controls
treat × post	−0.0023	−0.0056	−0.0072	−0.0056
	(0.0085)	(0.0080)	(0.0070)	(0.0080)
Observations	70,386	70,386	70,386	70,386
Clusters	4	4	4	4

Notes: 4-state sample (CA, NV, AZ, OR). Limited statistical power with 4 clusters. All insignificant, consistent with TWFE direction. Standard errors clustered at state level in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Table A7. Linear Pre-Trend Tests by Age Group (West-Coast Sample)

	Full Sample (16–24)	Teens (16–19)	Young Adults (20–24)
treat×time	−0.00246**	−0.00366**	−0.00088
	(0.00101)	(0.00165)	(0.00080)
Observations	44,618	20,906	23,712
SE method	State-clustered	State-clustered	HC1 robust
p-value	0.014	0.027	0.230
Pre-trend verdict (α = 0.10)	Fail	Fail	Pass

Notes: OLS regression of employed on treat, time_index, and treat × time_index estimated on the pre-period West-Coast sample (California vs. NV, AZ, OR; CPS ORG, January 2022 through March 2024; ages 16–24 including NILF in the denominator). time_index is months from the start of sample. Standard errors clustered at the state level (4 clusters) for the full and teen subsamples; HC1 robust standard errors for the young adult subsample. Null: coefficient on treat × time_index equals zero (parallel trends). * p<0.10, ** p<0.05, *** p<0.01.

Table A8. Placebo Date Tests (False Treatment Dates)

	employed		
	β	SE	p-value
Jan 2023	−0.0332**	0.0132	0.012
Jun 2023	−0.0208***	0.0057	0.000
Sep 2023	−0.0134**	0.0056	0.016
Apr 2024 (actual)	−0.0023	0.0085	0.789
Observations		70,386	

Notes: Each row assigns a false treatment date. West-Coast sample, month FEs, clustered SE. Significant placebos reflect documented pre-trend (CA drifting down relative to controls). Actual treatment date yields smallest, most insignificant coefficient.

Table A9. Triple-Difference Estimates

	employed	
	(1)	(2)
	Teen vs YA (within youth)	Youth (16–24) vs 25–35 (within state)
Triple interaction	−0.0587***	−0.0150**
	(0.0059)	(0.0061)
Observations	520,242	1,207,393
Demographics	Yes	Yes

Notes: Col (1): CA × Post × Teen on all-state youth sample. Col (2): CA × Post × Youth(16–24) on combined 16–35 sample. State and year-month FEs. Standard errors clustered at state level in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.

Table A10. Weighted vs Unweighted Estimates (West-Coast, Ages 16–24)

	(1)	(2)
	Unweighted	Weighted
Panel A: Employment		
treat × post	−0.0023	−0.0004
	(0.0085)	(0.0093)
Panel B: Hourly Wage ($)		
treat × post	+0.59**	+0.61*
	(HC1)	(EARNWT)
Panel C: Weekly Earnings ($)		
treat × post	+92.20***	+88.57***
	(HC1)	(EARNWT)
N (employment)	70,386	70,386

Notes: Panel A uses WTFINL (CPS final person weight). Panels B–C use EARNWT (earner study weight). Results substantively unchanged under weighting. *p < 0.1, **p < 0.05, ***p < 0.01.

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