

Young Adult Employment and Wage Floors: Evidence from California's AB 1228

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Abstract

This study evaluates the early labor-market effects of California's Assembly Bill 1228 (AB 1228), which established a \$20 per-hour minimum wage for large fast-food chains beginning in April 2024. Because fast food employs a large share of young adults, the policy provides a quasi-experimental setting to assess how a high, sector-specific wage floor influences employment outcomes of young adults. Using microdata from the Current Population Survey (CPS) Outgoing Rotation Group (ORG) from 2022 through mid-2025, a difference-in-differences (DiD) approach compares California to a control group of Nevada, Arizona, and Oregon. Results across multiple specifications show no statistically significant change in young-adult employment. These findings support the monopsony interpretation of low-wage labor markets, suggesting that even sizable wage floors can raise pay without reducing job opportunities for early-career workers.

1. Introduction

Assembly Bill 1228 established the nation's first sector-specific minimum wage of \$20 per hour, applying to large fast-food chains with more than 60 locations. Implemented in April 2024, the policy raised wages for hundreds of thousands of workers in a single industry that employs a disproportionately large share of teenagers and young adults. Because fast-food jobs often serve as entry points into the labor market, particularly for individuals in early career stages, AB 1228 provides a valuable setting to examine how a high and narrowly targeted wage floor affects employment outcomes for young workers. As shown in **Figure 1**, which reports the age distribution of workers in limited-service establishments using CPS data, teenagers and young adults comprise a substantial share of employment in this sector.

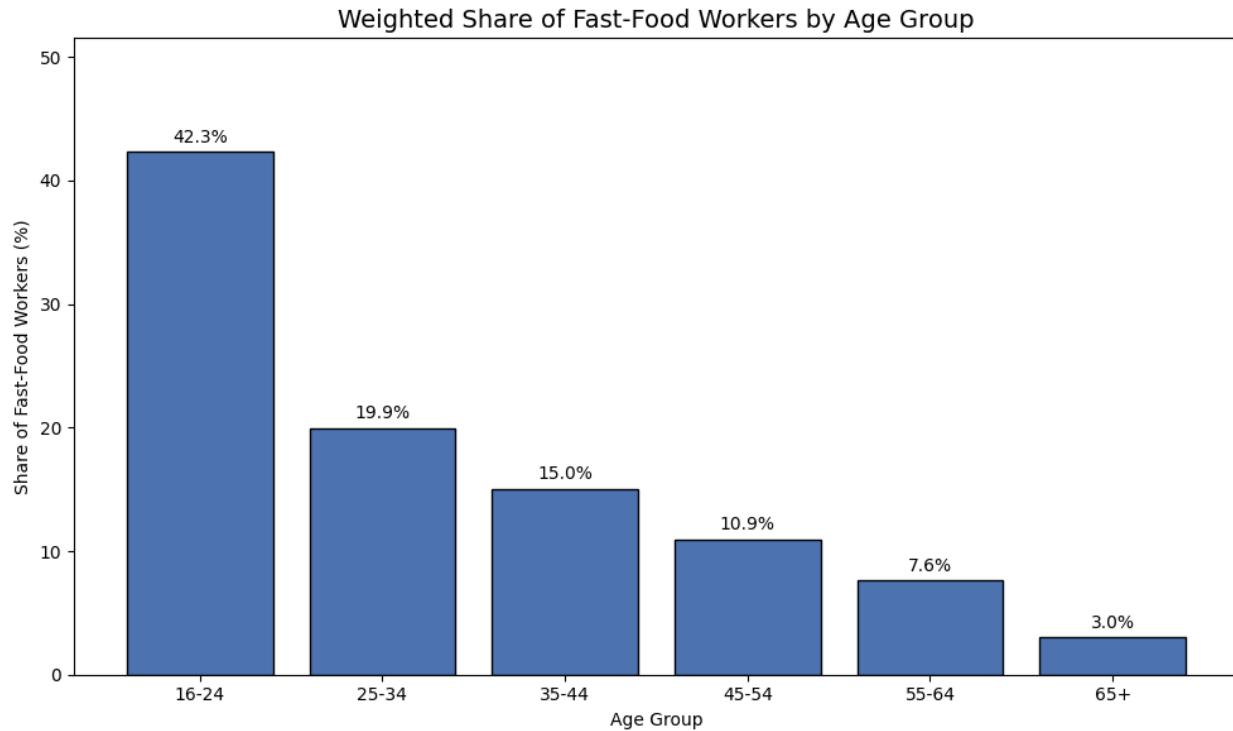


Figure 1: Weighted Age Share of Limited-Service Establishments

Young workers occupy a transitional phase between education and long-term employment, and jobs in restaurants and retail provide early exposure to workplace norms, scheduling, and earnings. Changes in the availability of these jobs may therefore have lasting implications for labor-market attachment and human-capital accumulation. At the same time, higher wages could increase job retention or encourage labor-force participation among young adults. Whether a large wage floor reduces employment opportunities for this group or instead improves labor-market outcomes remains an open empirical question.

Economic theory offers competing predictions regarding the effects of binding wage policies. In a competitive labor market, a wage floor set above the equilibrium level raises labor costs and leads firms to reduce employment or hours, particularly for less experienced workers whose productivity is closer to the minimum wage. In contrast, monopsony models emphasize labor-market frictions and employer wage-setting power, under which firms pay wages below workers' marginal product (Manning, 2021). In such settings, moderate increases in the minimum wage can raise both wages and employment by increasing labor supply and reducing monopsonistic distortions. The magnitude of these effects depends on the size of the wage increase and firms' ability to adjust along margins such as hours, staffing composition, or prices. AB 1228 represents a sharp and well-timed intervention, substantially raising the wage floor within a single sector, making it a natural setting to test the contrasting predictions of competitive and monopsonistic models.

Empirical evidence on minimum wages has increasingly challenged the predictions of the competitive model. Card and Krueger's (1994) seminal study of fast-food restaurants found no evidence of job losses following a minimum wage increase, a result later reinforced by research using improved identification strategies such as border-county designs (Dube, Lester, and Reich,

2010). More recent work emphasizes heterogeneity across worker groups, with teenagers shown to be more responsive to minimum wage changes than adults (Allegretto, Dube, and Reich, 2011). However, most studies of AB 1228 to date have focused on aggregate employment and wages, leaving age-specific effects largely unexplored.

Shepherd (2000) provides a theoretical and empirical interpretation of the Card and Krueger fast-food results that is explicitly grounded in monopsony. He argues that fast-food labor markets are characterized by upward-sloping labor supply to individual firms, driven by search friction, high turnover costs, limited outside options for low-skill workers, and geographic immobility. Under these conditions, employers possess wage-setting power, allowing them to pay wages below workers' marginal revenue product. Shepherd shows that when a binding minimum wage is introduced in such a market, employment need not fall and can even rise, because the policy constrains firms' ability to exploit monopsony power and moves wages closer to the competitive level. The absence of employment losses in the New Jersey fast-food sector following the minimum-wage increase is therefore not paradoxical but instead consistent with monopsonistic competition, where higher wage floors are absorbed through reduced turnover, improved recruitment, and modest price adjustments rather than layoffs (Shepherd, 2000).

Weekly earnings also provide a natural summary measure of labor-market outcomes because they combine both the wage rate and labor supply decisions. Policies that affect labor costs or worker compensation may influence weekly earnings through multiple channels. Firms may respond by adjusting hourly wages, altering hours worked, or both, depending on labor demand elasticities, contractual rigidities, and the extent to which workers can substitute across margins. For example, increases in mandated labor costs may be passed through to higher wages for some workers, while firms may simultaneously reduce hours to offset higher per-hour

expenses. Alternatively, if wages are rigid in the short run, earnings effects may operate primarily through changes in hours worked. Examining weekly earnings alongside hourly wages and hours worked therefore allows for a more complete understanding of how policies shape worker compensation and labor supply, and helps distinguish whether observed earnings changes reflect price effects, quantity adjustments, or a combination of both.

This paper contributes to the literature by examining how AB 1228 affected employment outcomes among 16–24-year-olds in California relative to neighboring states. Using a difference-in-differences design, the analysis compares quarterly unemployment rates in California to those in Nevada and Arizona, which serve as the control group. By focusing on young adults, the study sheds light on whether a large, sector-specific wage increase altered early-career employment opportunities. The results show no evidence that California’s April 2024 \$20 fast-food minimum wage increased unemployment among 16–24-year-olds. While youth unemployment rose in the post-policy period, this increase appears common to both California and comparison states and is not statistically attributable to the policy itself. Across the main specification and multiple robustness checks, the estimated treatment effects remain small, statistically insignificant, and economically negligible. These results suggest that observed post-period increases in unemployment reflect broader labor-market conditions rather than a policy-induced employment response.

In contrast, the policy is associated with a statistically significant increase in weekly earnings among employed workers in California relative to control states. Difference-in-differences estimates indicate an average increase of approximately \$82 per week following implementation. Further analysis shows no statistically significant differential changes in hourly wages or weekly hours worked, suggesting that the earnings gains are not driven by clear

adjustments along either margin. Taken together, the findings indicate that the \$20 minimum wage raised earnings for those who remained employed without generating measurable increases in youth unemployment, consistent with monopsony theory predictions.

2. Data

The data for this project comes from the Current Population Survey (CPS) Outgoing Rotation Group (ORG), accessed through IPUMS CPS. The CPS is a monthly household survey conducted by the U.S. Census Bureau and the Bureau of Labor Statistics (BLS). It provides nationally representative microdata on labor market outcomes, wages, demographics, and household information. Each month, about 60,000 U.S. households are surveyed using a rotating panel design in which households remain in the sample for four months, exit for eight, and then return for another four months (IPUMS CPS). The Outgoing Rotation Group (ORG) supplement collects detailed information on wages, earnings, and hours worked, making it a key source for analyzing labor market policies like minimum wage laws.

This study focuses on individuals between the ages of 16 and 24, since younger workers are typically more responsive to minimum wage changes. The analysis compares California, which is treated under AB 1228, with Nevada, Arizona, and Oregon which serves as the control group. The period covers January 2022 through August 2025, spanning both the pre-policy and post-policy periods around April 1, 2024, when AB 1228 took effect.

CPS offers several strengths for this analysis. It is a large, nationally representative dataset that collects information at a monthly frequency, which allows for precise tracking of labor market trends around policy implementation dates. The main variables of interest include demographic characteristics such as age, sex, and race; labor market measures such as employment status and labor force participation; and wage-related variables such as hourly wage and weekly earnings.

Figure 2 compares the gender composition of the treated and control groups prior to the policy. The control states exhibit a nearly balanced distribution, with men accounting for 50.9 percent of individuals and women 49.1 percent. California shows a slightly higher share of men at 54.2 percent and a correspondingly lower share of women at 45.8 percent. While this indicates a modest baseline difference in gender composition between the treated and control groups, the overall distributions remain broadly comparable. Importantly, because gender composition is largely time-invariant over the study period, these level differences are unlikely to bias the difference-in-differences estimates, which rely on changes over time rather than cross-sectional differences.

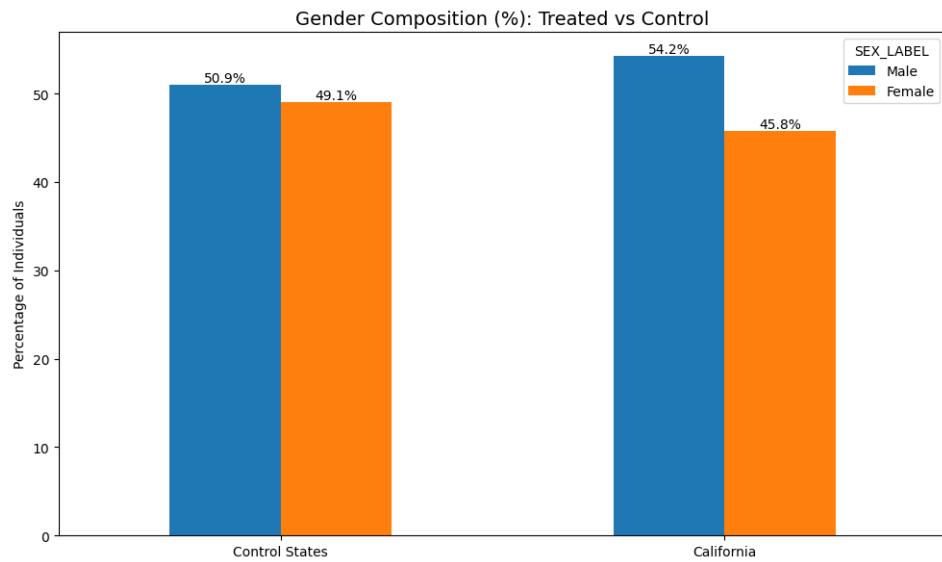


Figure 2: Gender Composition

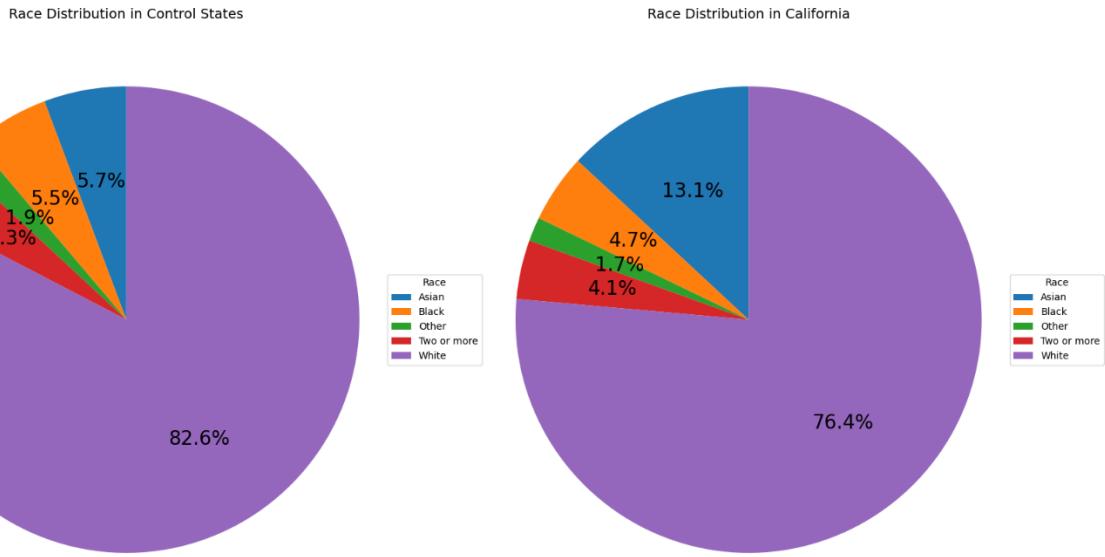


Figure 3: Racial Demographics

Baseline racial composition differs modestly between the treated and control groups, as shown in **Figure 3**. The control states are predominantly White, comprising 82.6 percent of the sample, while California is somewhat more diverse, with Whites accounting for 76.4 percent. California has a notably higher share of Asian individuals (13.1 percent) relative to the control states (5.7 percent), while the shares of Black individuals are similar across groups (4.7 percent in California versus 5.5 percent in control states). The proportion identifying as two or more races is also comparable, at roughly 4 percent in both groups, and other racial categories represent a small share of the sample. These differences reflect California's distinct demographic profile rather than policy-induced sorting. As with gender composition, these baseline level differences do not threaten difference-in-differences identification, which relies on parallel trends over time.

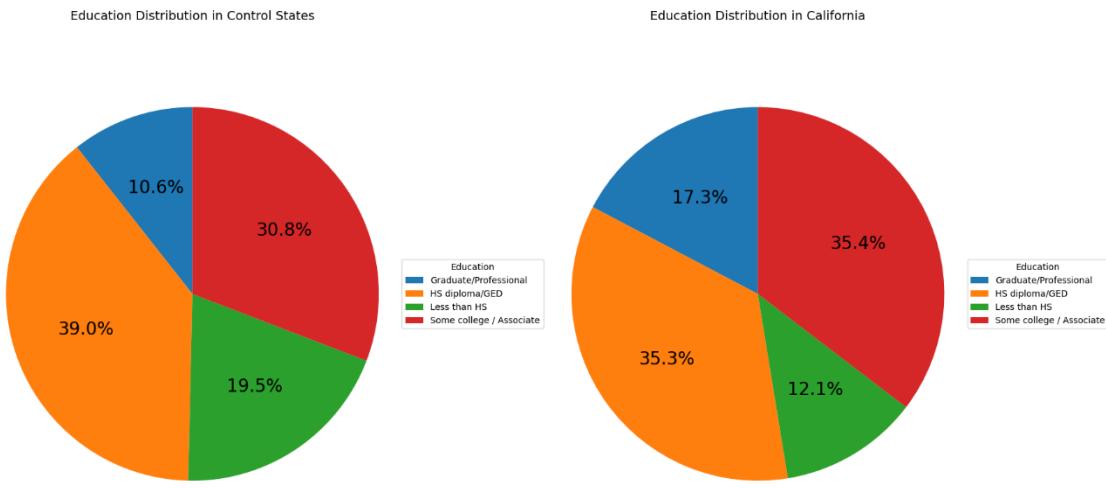


Figure 4: Educational Attainment Comparison

Educational attainment differs modestly between the treated and control groups. Control states have a higher concentration of individuals with a high school diploma or GED (39.0 percent) and a larger share with less than a high school education (19.5 percent) compared with California, where these shares are 35.3 percent and 12.1 percent, respectively. In contrast, California has a higher proportion of individuals with some college or an associate degree (35.4 percent versus 30.8 percent in control states) and a notably larger share with graduate or professional degrees (17.3 percent versus 10.6 percent). These patterns reflect California's relatively higher educational attainment at baseline. As with other demographic characteristics, these level differences are largely time-invariant and therefore do not threaten difference-in-differences identification.

Despite its advantages, the CPS has some limitations relative to an ideal dataset. Geographic precision is somewhat limited because certain metro and county identifiers are masked for confidentiality, thus rendering a contiguous-county approach ineffective. Although the CPS is

large overall, narrowing the sample to individuals aged 16–24 in only two metro areas can result in small sample sizes that reduce statistical power, so the study was expanded to the state level to accommodate this shortcoming. Wages and earnings are self-reported, which can introduce rounding errors or top-coding issues. Even with these challenges, the CPS remains one of the best available sources for evaluating the short-run employment and wage effects of California's AB 1228.

3. Empirical Methods

The empirical strategy is a difference-in-differences model, with California being the treatment group, and Nevada and Arizona as the Control group. The structural equation is as follows:

$$Y_t = \alpha + \beta_1 \text{Treat}_c + \beta_2 \text{Post}_t + \beta_3 (\text{Post}_t \times \text{Treat}_c) + \varepsilon_{it}$$

where Y is the calculated quarterly unemployment rate among 16-24 year old labor force participants at time t . Post equals 1 after April 2024, and $\text{Treat} = 1$ when the person of observation is in CA. The interaction term coefficient β_3 is the difference-in-differences estimate of AB 1228's impact.

The main assumption of the difference-in-differences model is the parallel trends assumption, or that the mean outcome of the treatment group would follow the same trend as the control group, had there not been an intervention. Visual inspection of the pre and post period trends is often the first step in analyzing the parallel trends assumption, as shown in **Figure 5**:

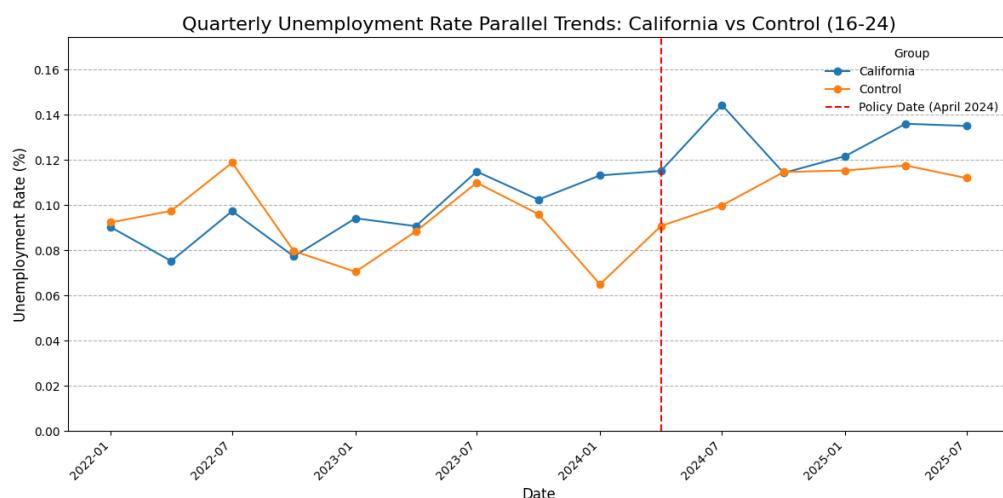


Figure 5: Parallel Trends Check for Unemployment among 16-24

Observing the time trends of quarterly unemployment among the treatment and control, we can see that although there are fluctuations, the trends are roughly similar, which is consistent with the expected parallel trends assumption.

Parallel Trends Check	
<i>Dependent variable: unemployed</i>	
(1)	
Time × Treatment	0.001 (-0.002 , 0.004)
Time	-0.001 (-0.004 , 0.003)
Constant	0.096*** (0.080 , 0.113)
Observations	18
R ²	0.046
Adjusted R ²	-0.081
Residual Std. Error	0.015 (df=15)
F Statistic	0.365 (df=2; 15)
Note:	* p<0.1; ** p<0.05; *** p<0.01 Standard errors in parentheses.
The dependent variable is the unemployment rate. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.	

Figure 6: Parallel Trends Estimate

To further verify this assumption, I estimate a linear pre-trend model using only pre-policy data, shown in **Figure 6**. The regression includes a common time trend and an interaction between time and treatment status. The interaction term is small and statistically insignificant, indicating no evidence of differential pre-trends between treated and control units prior to the policy. There are also additional robustness checks to ensure model validity, including different control groups (just AZ, just NV, just OR), as well as changing the treatment period to earlier, to see if there is a significant result. These will be discussed further in the results section.

4. Results

Figure 4 reports the main difference-in-differences estimates of the effect of the policy on the unemployment rate. The coefficient on the treatment–post interaction is positive but statistically insignificant, indicating no detectable differential change in unemployment in California relative to control states following the policy’s implementation. In contrast, the post-period indicator is positive and statistically significant, suggesting an overall increase in unemployment common to both treated and control groups during the post-policy period. The treatment indicator itself is small and insignificant, reflecting similar baseline unemployment levels across groups. Overall, these results imply that while unemployment rose in the post period, there is no evidence that this change was driven by the policy rather than broader labor market conditions.

<u>Difference-in-Differences Results</u>	
<i>Dependent variable: unemployed</i>	
	(1)
Treatment × Post	0.015 (-0.007 , 0.037)
Post	0.017** (0.002 , 0.033)
Treatment	0.004 (-0.010 , 0.018)
Constant	0.091*** (0.081 , 0.101)
Observations	30
R ²	0.520
Adjusted R ²	0.464
Residual Std. Error	0.014 (df=26)
F Statistic	9.373*** (df=3; 26)
Note:	* p<0.1; ** p<0.05; *** p<0.01
	Standard errors in parentheses.
The dependent variable is the unemployment rate.	
Significance levels: * p<0.10, ** p<0.05, *** p<0.01.	

Figure 7: Main Model Regression Results

<u>Nevada Only Robustness Check</u>	
<i>Dependent variable: unemployed</i>	
	(1)
Treatment × Post	0.006 (-0.020 , 0.031)
Post	0.027*** (0.009 , 0.045)
Treatment	0.007 (-0.009 , 0.023)
Constant	0.088*** (0.077 , 0.100)
Observations	30
R ²	0.490
Adjusted R ²	0.431
Residual Std. Error	0.017 (df=26)
F Statistic	8.331*** (df=3; 26)

Note: * p<0.1; ** p<0.05; *** p<0.01

Standard errors in parentheses.

The dependent variable is the unemployment rate.

Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Figure 8: Nevada only Robustness Check

The Nevada-only robustness check shown in **Figure 4** yields a similar pattern to the broader specification: the treatment indicator remains positive but statistically insignificant, and the post-period indicator shows a modest increase in unemployment that is statistically significant at all significance levels. The DiD interaction term remains small (0.006) and far from statistical significance, reinforcing the idea that, even when restricting the control comparison to Nevada, there is no detectable jump in unemployment among 16–24-year-olds in California relative to the counterfactual. Identical robustness checks were also conducted for Arizona (**Appendix A**) and Oregon (**Appendix B**), which yielded similar results, and the same interpretation. The point estimates were directionally consistent with the main specification but economically negligible, suggesting that the April 2024 \$20 fast-food minimum wage did not materially alter youth unemployment trends in California during the quarters immediately following implementation.

Fake Treatment Date Robustness Check	
<i>Dependent variable: unemployed</i>	
	(1)
Treatment × Post	0.012 (-0.010 , 0.035)
Post	0.021** (0.005 , 0.037)
Constant	0.091*** (0.082 , 0.100)
Observations	30
R ²	0.538
Adjusted R ²	0.484
Residual Std. Error	0.014 (df=26)
F Statistic	10.072 *** (df=3; 26)
Note:	* p<0.1; ** p<0.05; *** p<0.01 Standard errors in parentheses.
The dependent variable is the unemployment rate. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.	

Figure 9: Fake Treatment Date Robustness Check

Shifting the treatment date out to June 6, 2024, as shown in **Figure 6**, provides another robustness check to ensure the main results are not driven by pre-existing noise or arbitrary cutoff choices. When assigning the policy shock a year earlier, the interaction term remains small and statistically insignificant, indicating no differential break in unemployment trends between California and the selected control when the placebo date is applied. The post coefficient is positive and significant, reflecting a broader upward movement in youth unemployment across both groups during this earlier period, but importantly the placebo DiD estimate does not detect any spurious jump tied to the fake policy date. This strengthens the argument that the actual April 2024 policy timing is what the model is testing, and the absence of an effect under the placebo further supports the conclusion that California's \$20 minimum wage did not generate a measurable unemployment response for 16–24-year-olds in the quarters surrounding implementation.

Weekly Earnings Extension	
<i>Dependent variable: EARNWEEK2</i>	
	(1)
Treatment × Post	82.108*** (23.957 , 140.259)
Post	0.971 (-44.740 , 46.681)
Constant	726.054*** (686.348 , 765.761)
Observations	6688
R ²	0.017
Adjusted R ²	0.016
Residual Std. Error	566.479 (df=6683)
F Statistic	28.964 *** (df=4; 6683)
Note:	* p<0.1; ** p<0.05; *** p<0.01 Standard errors in parentheses.
The dependent variable is the unemployment rate. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.	

Figure 10: Weekly Earnings Results

Table 7 presents difference-in-differences estimates of the policy's effect on weekly earnings, using a sample restricted to individuals who were employed and reported earnings. The coefficient on the treatment–post interaction is positive and statistically significant at the 1 percent level, indicating that weekly earnings increased in California relative to control states following the policy's implementation. The estimated magnitude suggests an increase of approximately \$82 per week, with a confidence interval that excludes zero. In contrast, the post-period indicator is statistically insignificant, implying no common shift in weekly earnings across both treated and control groups. Overall, these results provide evidence of a positive relative effect of the policy on weekly earnings among employed workers, though the interpretation is conditional on continued employment and reported earnings.

The weekly earnings results indicate a positive and statistically significant increase for employed workers in California relative to control states following the policy. Because weekly earnings reflect both the hourly wage rate and the number of hours worked, this finding alone does not reveal the underlying adjustment mechanism. To disentangle these channels, we examine hourly wages, as shown in **Appendix C**. The estimated treatment effect on hourly wages is positive but statistically insignificant, suggesting that the observed increase in weekly earnings is not driven by a clear increase in hourly pay. Taken together, these results imply that while weekly earnings rose for employed workers, this increase cannot be conclusively attributed to higher hourly wages, motivating further examination of potential adjustments along the hourly margin.

Given the absence of a statistically significant effect on hourly wages, we next turn to weekly hours worked to assess potential adjustment, as shown in **Appendix D**. The treatment–post interaction is small and statistically insignificant, indicating no evidence that weekly hours changed differentially in California relative to control states following the policy. Both the treatment and post indicators are negative and statistically significant, suggesting that California workers have lower baseline hours and that weekly hours declined over time across all states, likely reflecting broader labor-market or macroeconomic conditions. Taken together, these results imply that observed changes in earnings are unlikely to be driven by adjustments in hours worked and instead reflect other channels.

5. Conclusion

Taken together, the main specification and both robustness checks tell a consistent story: unemployment did rise after April 2024, but there is no evidence that California's \$20 fast-food minimum wage caused a differential increase in unemployment among 16–24-year-olds relative to reasonable counterfactuals. The primary regression shows a statistically significant post-policy increase in unemployment across both treatment and control areas, but the DiD interaction term is small and far from significant, indicating no detectable treatment-specific effect. The state-only robustness checks mirror this pattern, again showing that while overall unemployment moved up, California did not deviate from the controls in a way consistent with a policy shock. Finally, the placebo test using a June 2024 treatment date produces no spurious effect, confirming that the identification strategy is not mechanically generating false positives and that the main results are not driven by arbitrary timing choices. Across all three approaches, the evidence points toward the same conclusion: youth unemployment did not respond in a statistically or economically meaningful way to the April 2024 minimum wage increase.

From a policy standpoint, these findings suggest that high statutory minimum wages, at least in the fast-food sector and for young workers, may not generate the large disemployment effects predicted by perfectly competitive labor market models. Instead, consistent with Shepherd (2000), the evidence is more indicative of a monopsonistic labor market in which fast-food employers possess wage-setting power, allowing higher wage floors to be absorbed through reduced monopsony rents rather than sharp employment losses. This aligns with a growing empirical literature showing muted or near-zero employment responses to minimum wage increases, particularly in sectors with high turnover, concentrated employers, or frictions that limit worker mobility. For future policy design, this implies that states considering substantial

wage increases may face fewer short-run employment trade-offs than traditionally assumed, though continued monitoring is crucial as effects may evolve over a longer horizon. Because the analysis covers only the first several post-policy quarters, the findings should be interpreted as short-run impacts; medium-run adjustments may differ as firms respond over time. More broadly, the results reinforce the importance of understanding market structure when evaluating labor policy: in settings where monopsony power is strong, higher minimum wages may raise earnings with limited employment loss, whereas in more competitive markets, the impacts could differ.

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Appendix

Appendix A: Arizona Robustness Check

Arizona Only Robustness Check

<i>Dependent variable: unemployed</i>	
(1)	
Treatment × Post	0.009 (-0.019 , 0.037)
Post	0.024 ** (0.004 , 0.043)
Treatment	0.012 (-0.005 , 0.030)
Constant	0.083 *** (0.071 , 0.095)
Observations	30
R ²	0.476
Adjusted R ²	0.415
Residual Std. Error	0.018 (df=26)
F Statistic	7.871 *** (df=3; 26)

Note: * p<0.1; ** p<0.05; *** p<0.01
Standard errors in parentheses.

The dependent variable is the unemployment rate.
Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Appendix B: Oregon Robustness Check

Oregon Only Robustness Check

<i>Dependent variable: unemployed</i>	
	(1)
Treatment × Post	0.035** (0.003 , 0.067)
Post	-0.002 (-0.025 , 0.020)
Treatment	-0.010 (-0.031 , 0.010)
Constant	0.105*** (0.091 , 0.120)
Observations	30
R ²	0.258
Adjusted R ²	0.172
Residual Std. Error	0.021 (df=26)
F Statistic	3.014 ** (df=3; 26)

Note: * p<0.1; ** p<0.05; *** p<0.01

Standard errors in parentheses.

The dependent variable is the unemployment rate.

Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Appendix C: Hourly Wage Regression

Hourly Wage Results

<i>Dependent variable: HOURWAGE2</i>	
	(1)
Treatment × Post	0.418 (-0.177 , 1.013)
Post	1.203*** (0.739 , 1.667)
Treatment	1.135*** (0.772 , 1.498)
Occupation Code	-0.000*** (-0.000 , -0.000)
Constant	17.242*** (16.815 , 17.669)
Observations	5794
R ²	0.032
Adjusted R ²	0.031
Residual Std. Error	5.428 (df=5789)
F Statistic	48.030*** (df=4; 5789)

Note: * p<0.1; ** p<0.05; *** p<0.01

Standard errors in parentheses.

The dependent variable is the unemployment rate.

Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Appendix D: Weekly Hours Results

Weekly Hours Results

<i>Dependent variable: AHRSWORKT</i>	
	(1)
Treatment × Post	-0.250 (-0.905 , 0.405)
Post	-0.701*** (-1.216 , -0.185)
Treatment	-0.590*** (-0.986 , -0.194)
Constant	31.938*** (31.625 , 32.251)
Observations	26980
R ²	0.002
Adjusted R ²	0.002
Residual Std. Error	12.820 (df=26976)
F Statistic	15.354*** (df=3; 26976)

Note: * p<0.1; ** p<0.05; *** p<0.01

Standard errors in parentheses.

The dependent variable is the unemployment rate.

Significance levels: * p<0.10, ** p<0.05, *** p<0.01.