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The Effect of the 2019 Minimum Wage Increase on Youth Employment in Hong Kong

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Abstract. This paper estimates the employment effect of the May 2019 increase in Hong Kong's Statutory Minimum Wage on young workers aged 15 to 24, using workers aged 35 to 44 as the comparison group. A two-way fixed effects difference-in-differences design is applied to a balanced monthly panel of the General Household Survey from May 2017 to May 2021. The point estimate indicates a relative decline of about 3 percentage points in the youth employment rate after the adjustment. Newey-West standard errors that account for overlapping three-month survey windows place the estimate at the 5 percent level of significance, not the 1 percent level implied by group-clustered errors. When the specification allows the young group to respond differently to aggregate labour market conditions, the residual difference-in-differences estimate shrinks to nearly zero. The youth labour force contracts by roughly 20 percent relative to the mid-age group, and the largest employment decline begins around eight months after implementation, coinciding with the onset of the COVID-19 shock. Pre-treatment event-study coefficients are not flat, and the identifying assumptions are not satisfied cleanly. The evidence is consistent with modest downward pressure on youth employment but does not isolate the wage floor as its cause.

Keywords: Minimum wage, Youth employment, Difference-in-differences, Hong Kong, Labour force participation

1. Introduction

Minimum wages remain one of the most widely used instruments of labour market protection across high-income economies, yet their effect on young and low-skilled workers remains contested [1]. Young workers enter the labour market with limited work experience and scarce qualifications, and they are over-represented in entry-level positions paid close to the statutory floor. The OECD (2021) identifies workers under 25 as the group most exposed to minimum wage adjustments, because the wage floor binds for a larger share of their earnings than for older workers. Whether this exposure reduces employment depends on the structure of the relevant labour market rather than on theory alone.

Hong Kong introduced its Statutory Minimum Wage in May 2011 and reviews the rate on a regular cycle. The adjustment that took effect in May 2019 raised the statutory hourly floor from HK$34.5 to HK$37.5, an increase of 8.7%, at a point when the local economy was still expanding. Hong Kong's employment structure is concentrated in service industries such as catering, retail,

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accommodation and property security, sectors that hire a large share of young workers and operate on narrow margins. Workers aged between 35 and 44 hold accumulated experience and earn wages well above the floor, which makes the same adjustment bind for only a small fraction of that group. This contrast across age groups offers the basis for an age-based comparison.

This paper uses the May 2019 adjustment as a quasi-natural experiment and applies a difference-in-differences design to monthly labour force data spanning from May 2017 to May 2021. Workers aged 15 to 24 form the treatment group, and workers aged 35 to 44 form the control group. The analysis asks whether the adjustment changed the youth employment rate relative to the mid-age benchmark, and whether the estimates survive standard specification checks.

The paper contributes evidence on a setting where age-specific evaluation remains sparse. Existing Hong Kong studies examine the overall low-wage workforce or household-level outcomes, and estimates that isolate workers aged 15 to 24 after the 2019 adjustment are absent. A territory-wide floor in one service-dominated labour market also avoids the cross-jurisdiction heterogeneity present in multi-state studies. The findings indicate a relative decline in the youth employment rate of about 3 percentage points after the adjustment. The timing of that decline, together with weak pre-treatment evidence, cautions against attributing it wholly to the wage floor, and the paper frames the estimates accordingly.

2. Literature review and theoretical background

2.1. Competing theoretical predictions

The competitive model treats the minimum wage as a price floor imposed on a market where firms face upward-sloping labour supply and wages equal marginal productivity. A floor above the competitive level raises hiring costs for low-productivity workers and predicts lower employment among groups whose productivity lies closest to the floor, including labour market entrants. The predicted loss grows with the size of the increase and with the share of workers affected.

Models of monopsonistic and search-frictional labour markets reverse or weaken that prediction. Where employers possess wage-setting power, an appropriately set minimum wage can raise wages without reducing employment and can increase hiring by compressing the gap between marginal product and pay [1]. Search and matching models generate a similar range of outcomes, because the wage floor alters the distribution of job offers rather than moving a single market-clearing price. The two frameworks predict different signs for moderate increases, and the dispute is resolved through empirical evidence rather than theoretical deduction.

2.2. International evidence and the rise of quasi-experimental designs

Early empirical work relied on aggregate time-series variation and produced a wide range of elasticities, with estimates sensitive to the controls and periods included. Dube et al. show that national-level panel designs confound minimum wage variation with differences in local employment trends, and they use contiguous counties across state borders to compare labour markets exposed to different wage floors [2]. Their border design yields an estimate close to zero for restaurants and younger workers, and traces the negative estimates in earlier specifications to unobserved regional trends.

The modern literature extends this comparison-based logic through event-study and bunching estimators applied to national reforms. Cengiz et al. pool hundreds of state-level changes in the United States and trace the wage distribution around each floor, finding that affected jobs are

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preserved as the wage rises rather than destroyed [3]. Similarly, Harasztosi and Lindner examine a large, sustained Hungarian increase and show that higher labour costs pass into prices and are absorbed through firm reallocation, with limited aggregate job loss [4]. Dustmann et al. reach a related conclusion for Germany, where the national minimum wage reallocated workers toward higher-paying establishments without a sizeable aggregate employment decline [5]. In the United Kingdom, Giupponi et al. likewise document small employment effects alongside wage gains for the lowest-paid workers [6].

Evidence specific to young workers follows the same age-based logic used in this paper. Hyslop and Stillman study a New Zealand reform that extended the adult minimum wage to younger ages and compare affected teenagers with workers in their early twenties, finding no robust negative effect on youth employment and some evidence of increased hours for the youngest groups [7]. van Bezooijen et al. examine age-specific minimum wages in the Netherlands through a comparison across adjacent age cohorts and report concentrated wage effects with limited adjustment along the employment margin [8]. However, large local increases can still produce measurable losses. Jardim et al. analyse the Seattle wage hikes and find reductions in hours and entry-level employment when the floor rises to a high level relative to the local wage distribution [9]. Overall, Manning synthesises this body of evidence and concludes that the employment effect of moderate minimum wages is close to zero on average, with larger effects where the floor is set high or where affected workers can be substituted [1].

2.3. Hong Kong evidence and the research gap

Hong Kong-based evidence is comparatively thin and concentrates on the initial 2011 introduction. Lau and Wong examine the statutory minimum wage through a family impact lens and document wage gains for low-income households alongside limited aggregate employment consequences, without separating young workers from the broader low-wage population [10]. Studies of later adjustments focus on the overall workforce and do not exploit the differential exposure of age groups. No identified study isolates workers aged 15 to 24 around the May 2019 increase, even though young workers are the group for whom the floor binds most frequently. This paper addresses that gap with an age-stratified difference-in-differences design and reports the identifying evidence, including pre-treatment trends, alongside the point estimates.

3. Research design and data

3.1. Quasi-natural experiment and identification

Because the statutory minimum wage in Hong Kong is set territory-wide, the city contains no geographically untreated market. Identification instead exploits differential exposure across age groups. The treatment group comprises workers aged 15 to 24, whose entry-level wages cluster near the statutory floor and whose employers face the largest increase in mandated pay. The control group comprises workers aged 35 to 44, whose median wages lie well above the floor and for whom the adjustment changes few existing contracts. The two groups participate in the same territorial labour market and face common macroeconomic conditions, which isolates the age-specific component of the policy under the parallel trends assumption.

The causal interpretation rests on two conditions. First, absent the adjustment, the employment rates of the two groups would have followed parallel paths. Second, no other shock occurring after May 2019 affects young and middle-age workers differentially. An event-study specification and

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visual trend evidence assess the first condition, and the empirical sections report what these checks show rather than assuming the condition holds.

3.2. Data sources and variables

The analysis uses a balanced monthly panel covering 49 months from May 2017 to May 2021, with 24 months before and 25 months after the adjustment, for two age groups and 98 observations in total. Employment and labour force counts are drawn from the General Household Survey published by the Hong Kong Census and Statistics Department, using the department's tables on labour force and employment by age. The survey publishes rolling three-month figures rather than independent monthly observations, and the value assigned to each closing month covers the preceding interval, a feature revisited below.

The dependent variable, emp_rate, is the ratio of employed persons to the labour force within each age group, and the robustness analysis uses the group unemployment rate unemp_rate_group, defined as one minus emp_rate. Three controls capture aggregate conditions that move city-level employment: the seasonally adjusted overall unemployment rate unemp_rate_sa, measured in percent on the same three-month basis; the year-on-year growth rate of real GDP gdp_growth, reported quarterly and held constant across the three months of each quarter; and the employment share of the retail, accommodation and food services sector food_retail_share, reported annually and held constant across the year. The Census and Statistics Department combines these three industries in a single category, so separate catering and retail components are not available.

3.3. Econometric specification

The baseline model is a two-way fixed effects difference-in-differences regression:

$$emp_rate(g,t) = beta\ did(g,t) + gamma\ X(t) \tag{1}$$

$$+alpha(g) + lambda(t) + epsilon(g,t)$$

where g indexes the age group and t indexes the month. The term did(g,t) equals one for young workers in months from May 2019 and zero otherwise, X(t) contains the aggregate controls, alpha(g) is a group fixed effect, and lambda(t) is a month fixed effect. The coefficient beta measures the change in the youth employment rate relative to the mid-age group after the adjustment. Group-clustered standard-error inference is unreliable in this setting given only two age-group clusters, cluster-robust variance estimators tend to collapse and produce artificially small standard errors. Therefore, all regression specifications employ Newey-West standard errors with six lags to account for serial correlation induced by the survey's overlapping three-month rolling-window design.

Pre-treatment dynamics are assessed through an event-study specification that replaces did(g,t) with interactions between the treatment indicator and indicators for months relative to the adjustment, using the month immediately before implementation as the base period. Estimation uses the reghdfe command in Stata, absorbing group and month fixed effects.

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4. Empirical results

4.1. Descriptive statistics

Table 1 reports summary statistics for the pooled panel. The employment rate averages 0.933 across groups and months, with a standard deviation of 0.050 and a range from 0.795 to 0.984, while the group unemployment rate averages 0.067. Aggregate conditions vary widely over the window, as the overall unemployment rate ranges from 2.8% to 7.2% and real GDP growth from -9.1% to 8.0%.

Table 1. Descriptive statistics

Variable	N	Mean	SD	Min	Max
Employment rate (emp_rate)	98	0.933	0.050	0.795	0.984
Group unemployment rate	98	0.067	0.050	0.016	0.205
Overall unemployment rate, SA (%)	98	3.933	1.542	2.800	7.200
Real GDP growth, year on year (%)	98	0.341	4.862	-9.100	8.000
Retail, accommodation and food share	98	0.155	0.011	0.141	0.167
DID interaction	98	0.255	0.438	0.000	1.000

Source: Author's calculations using General Household Survey data.

Group means around the adjustment show the contrast driving the estimates. The youth employment rate averages 0.9141 before May 2019 and 0.8699 afterward, a decline of 4.42 percentage points, while the youth labour force contracts from 288,900 to 239,100 persons. Over the same period, the mid-age employment rate moves from 0.9809 to 0.9663, a decline of 1.46 percentage points, against a broadly stable labour force. The simple difference of these changes is about -2.96 percentage points, which previews the regression estimate without adjusting for aggregate conditions.

4.2. Baseline estimates

Table 2 reports the baseline regressions. Model 1 includes only the difference-in-differences term alongside group and month fixed effects. The coefficient is -0.030 with a Newey-West standard error of 0.011, significant at the 5 percent level. This indicates a relative decline of 3.0 percentage points in the youth employment rate after May 2019.

Model 2 adds interactions between the treatment indicator and each aggregate series. The coefficient on the difference-in-differences term falls to -0.005 and is no longer statistically distinguishable from zero. To deep dive the variables interaction, firstly, the treatment-by-unemployment interaction is negative and precisely estimated: young workers' employment moves more sharply with the overall unemployment rate than does mid-age employment. Secondly, the treatment-by-sector-share interaction is also negative and sizeable. The R-squared rises from 0.28 to 0.68, so the interactions absorb most of the residual variation that Model 1 attributes to the policy.

The contrast between the two models is the central result. The raw difference-in-differences estimate of -3 percentage points is statistically meaningful but largely reflects differential cyclical exposure of the young group, rather than a discrete downward shift at implementation. Once the

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model allows young workers to respond differently to the business cycle, the residual policy effect is close to zero.

Table 2. Baseline difference-in-differences estimates

Variable	Model 1: no controls	Model 2: with controls
DID (treatment x post)	-0.030** (0.011)	-0.005 (0.013)
Overall unemployment rate, SA		-0.015 (0.008)
Real GDP growth, year on year		0.011 (0.004)
Retail and food services share		0.804 (0.381)
Constant	0.949*** (0.008)	0.818* (0.078)
Observations	98	98
R-squared	0.28	0.68
Group fixed effects	Yes	Yes
Month fixed effects	Yes	Yes

Notes: Newey-West standard errors with six lags appear in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

4.3. Parallel trend evidence

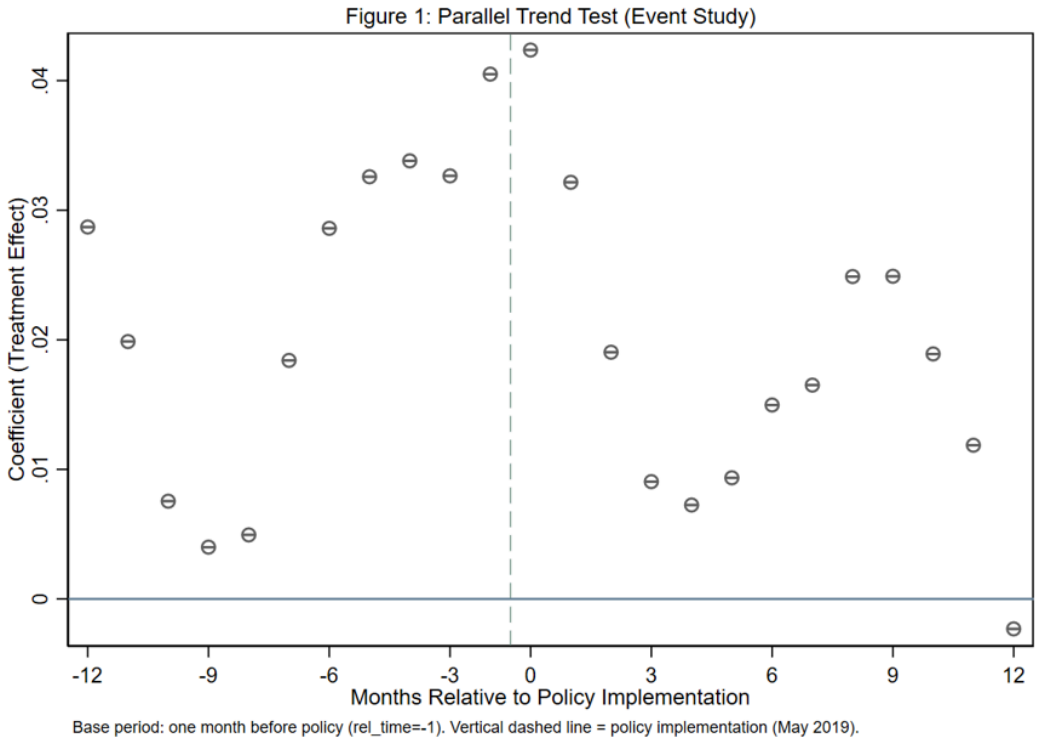


Figure 1. Parallel trend test, event study coefficients

Figure 1 plots the event-study coefficients for 12 months before and after the adjustment. Pre-treatment point estimates do not lie on a flat line around zero. The coefficients range from about 0.004 to 0.041 and rise toward the implementation date, so the visual evidence does not show a

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stable pre-treatment relationship between the two groups. Post-treatment coefficients decline from the levels near implementation, reaching about 0.009 three months after and -0.003 twelve months after, tracing a widening gap rather than an immediate step at May 2019.

Figure 2 plots the underlying employment rates. The mid-age rate remains within a narrow band between roughly 0.95 and 0.98 across the full window. The youth rate fluctuates between about 0.89 and 0.93 before the adjustment and begins a pronounced decline around the first months of 2020, reaching a low near 0.81 later that year before recovering to about 0.87. The largest fall starts roughly eight months after the adjustment rather than at implementation.

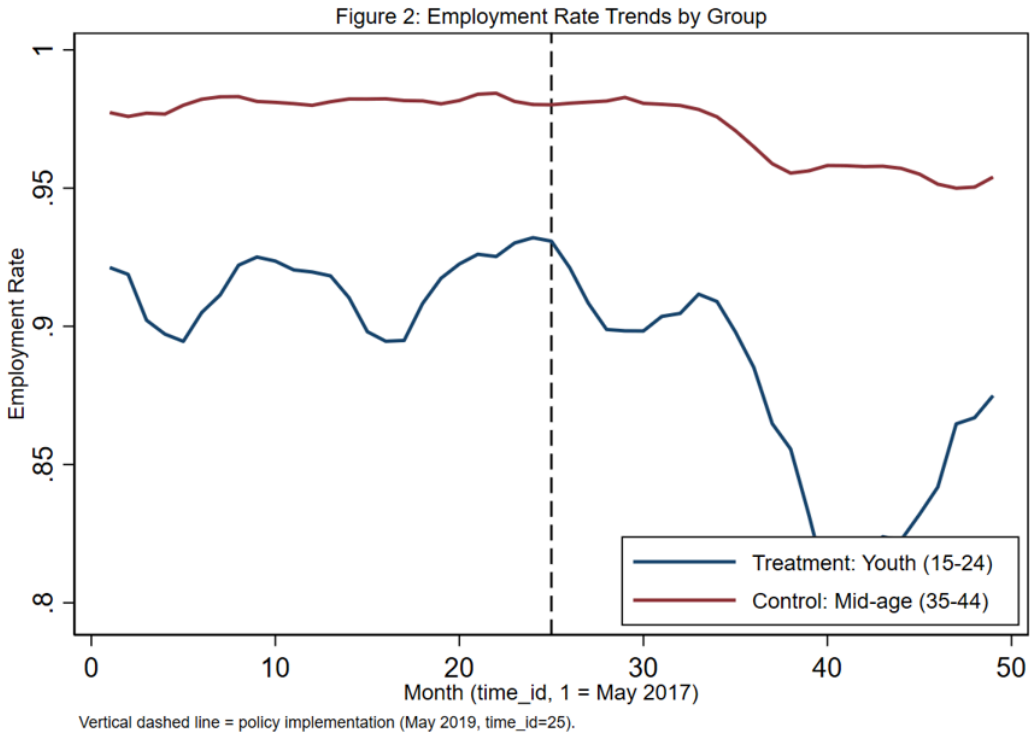


Figure 2. Employment rate trends by age group

4.4. Robustness and labour force outcomes

Table 3 replaces the employment rate with the group unemployment rate. The difference-in-differences coefficient is +0.030 with the same Newey-West standard error of 0.011, the exact mirror of the baseline estimate, and the two specifications describe the same movement from opposite sides of the labour force identity. This is not an independent check but confirms that the result is driven by the arithmetic identity.

Table 4 reports results for the log of the labour force and the log of employment. Both outcomes show large and precisely estimated declines: the youth labour force contracts by about 20 percent relative to the mid-age group, and youth employment contracts by about 23 percent. These magnitudes are larger than the rate change because the denominator of the employment rate also falls. The result indicates that the adjustment window coincided with a substantial withdrawal of young people from the labour force, not merely a shift among the already employed. Without population-level data, the analysis cannot separate labour force exit from population change, and the participation margin remains open.

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Table 3. Robustness with the group unemployment rate

Variable	Unemployment rate
DID (treatment × post)	0.030** (0.011)
Observations	98
Within R-squared	0.278
Group fixed effects	Yes
Month fixed effects	Yes

Notes: Newey-West standard errors with six lags appear in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table 4. Labour force and employment levels

Variable	ln (labour force)	ln (employment)
DID (treatment × post)	-0.196*** (0.038)	-0.231*** (0.045)
Observations	98	98
Within R-squared	0.182	0.201
Group fixed effects	Yes	Yes
Month fixed effects	Yes	Yes

Notes: Newey-West standard errors with six lags appear in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

5. Discussion

5.1. Interpretation of the estimates

The raw difference-in-differences estimate indicates that young workers experienced a relative decline of about 3 percentage points in the employment rate after the May 2019 adjustment, and this estimate survives Newey-West inference at the 5 percent level. Once the treatment-by-macro interactions are included, however, most of this gap tracks the young group's greater cyclical sensitivity rather than a discrete shift at implementation. After allowing for differential cyclical exposure, the residual policy coefficient is -0.005, not distinguishable from zero. A competitive labour market interpretation treats the raw gap as the disemployment response to a higher wage floor, and the magnitude falls within the range of modest effects reported for moderate increases. The timing qualifies that interpretation. The event-study coefficients are not flat before treatment, and the raw youth rate begins its largest fall in early 2020, eight months after the floor changed. A labour demand response to higher mandated wages would appear around implementation rather than after a long lag, so the pattern fits a shock that hit young workers later and harder than the mid-age group.

The labour force results sharpen the concern. A 20 percent relative contraction in the youth labour force is large enough to reflect a participation response to deteriorating entry-level opportunities. The employment rate can fall either because employed workers lose jobs or because non-participating young workers fail to enter the labour force. The log-level estimates suggest both margins moved, and without age-specific population data the two cannot be separated.

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The structure of the Hong Kong labour market supplies a plausible reason for limited immediate disemployment. The adjustment raised the floor by 8.7%, within the range characterised as moderate in the international literature, and affected service sectors faced the same territory-wide cost increase without competition from uncovered firms. Under those conditions, employers can absorb higher costs through prices, compressed margins or reallocation across establishments, the channels documented by Harasztosi and Lindner and Dustmann et al. [4, 5], rather than immediate separations.

5.2. Comparison with prior evidence

These estimates align with the age-based studies closest to this design. Hyslop and Stillman and van Bezooijen et al. find little adjustment along the employment margin after youth-focused minimum wage changes [7, 8], and Cengiz et al. show that jobs near the floor are largely preserved across United States reforms [3]. In this respect, the negative point estimate here is closer to the Seattle evidence reported by Jardim et al. [9], where a high local floor coincided with reduced entry-level work. This contrast fits the synthesis of Manning [1], where employment consequences remain small on average for moderate floors and grow with the level of the floor and the scope for substitution. The Hong Kong estimates thus occupy an intermediate position, showing a measurable relative decline whose causal attribution is weakened by the surrounding shocks.

6. Limitations and future research

Four limitations bound the conclusions. First, because the panel contains two age groups, group-clustered standard errors are not reliable. Therefore, the analysis instead uses Newey-West errors that account for the overlapping three-month survey windows, although these also rest on a short time series. Secondly, the General Household Survey releases rolling three-month figures, which smooths month-to-month movement and, in turn, limits precise dating of any response. Similarly, the quarterly GDP and annual sector series repeat values within each period. A third limitation is the COVID-19 shock from early 2020, which affected young service workers more severely and overlaps the post-treatment window, so a pandemic indicator or an estimator robust to heterogeneous timing would help separate the two events. Finally, the treatment group is broad: not all workers aged 15 to 24 earn near the floor, and data on student status, contract type and industry of employment would permit a more granular exposure measure.

The pre-treatment event-study pattern is the most consequential of these issues, because it strains the parallel trends assumption on which the design depends. Future work could extend the panel in both directions, add adjacent age cohorts as comparison groups, and apply estimators built for staggered and age-based settings, together with wage and hours data that distinguish substitution from labour force withdrawal.

7. Conclusion

This paper applies a difference-in-differences design to monthly Hong Kong labour force data and reports a relative decline of about 3 percentage points in the employment rate of workers aged 15 to 24 after the May 2019 minimum wage increase, with mirroring evidence from the unemployment rate and stable estimates across specifications. The identifying evidence is less clean than the point estimates suggest. Pre-treatment trends are not flat, and the largest employment fall coincides with the pandemic eight months after implementation rather than with the adjustment itself. The estimates

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are therefore consistent with modest downward pressure on youth employment but do not establish the minimum wage as its cause. For policy, the findings support cautious, gradual rate reviews paired with monitoring of youth entry-level employment, and evaluations should report pre-treatment dynamics and overlapping shocks before assigning employment movements to the wage floor.

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