Are We There Yet? The Search for a Turning Point in the Effect of Minimum Wage on Jobs*

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Abstract

At what point do higher wage floors begin to reduce employment? Imperfect competition on the labor market suggests the presence of a turning point: employment may rise or remain unchanged at low levels of the wage floor but declines once it becomes sufficiently high. We investigate the empirical relevance of this prediction using a comprehensive sample of 60 U.S. state-level minimum wage increases between 1980 and 2019. Exploiting variation across seven measures of minimum wage bite—including the Kaitz index, real minimum wage levels, and the workforce share below the new minimum—we test for heterogeneous employment effects consistent with a turning point. Across specifications, we find no evidence that existing policy changes have reached such a threshold. Even among the most binding increases, where the minimum wage ranges from 55 to 60 percent of the median wage, estimated employment effects remain positive, with an elasticity of 0.32 (s.e. 0.15). To interpret these findings, we calibrate a model of monopsonistic competition in labor markets and use it to quantify the location of the turning point. The model implies a threshold Kaitz index of approximately 0.62-0.72, suggesting that historical U.S. minimum wage policies have operated below the range where negative employment effects are predicted to emerge.

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1 Introduction

Minimum wage increases have been shown, on average, to have modest overall employment effects (Dube and Lindner, 2024), with most estimated own-wage elasticities of employment being either positive or "small". But much less is known about whether there is a level at which the minimum wage begins to cause substantial job losses. Standard models of imperfectly competitive labor markets predict nonlinear outcomes: small or even positive employment effects at low minimum wage levels, but increasingly negative effects as the wage floor rises. A missing piece in the literature is a systematic analysis of heterogeneity in minimum wage effects—a search for the "turning point."

This paper provides systematic evidence on such nonlinearities, and then uses this evidence to calibrate a structural model and conduct out-of-sample counterfactual analysis. The empirical component uses an event-based approach to study 60 U.S. state-level minimum wage increases from 1980 to 2019. We study the impact of these events on low-skilled workers identified using the machine learning based approach developed in Cengiz et al. (2019). We take 25% of the population that is most likely to be predicted to be a low-wage workers, covering about 75% of all minimum wage workers. We estimate event-specific treatment effects using a difference-in-differences estimator, and test for heterogeneity by splitting events into terciles of minimum wage "bite." We consider seven measures of bite along two dimensions—levels versus changes, and absolute dollar values versus the Kaitz index (the minimum-to-median wage ratio)—as well as direct measures of treatment intensity such as the share of workers earning below the new minimum.

Overall, consistent with prior findings in the literature, we estimate that the employment effects of minimum wage increases are small and close to zero. Across all 60 events, the employment elasticity with respect to wage changes is 0.13 (SE = 0.15). We next examine heterogeneity by the level of the minimum wage, measured by the Kaitz index (the ratio of the minimum to the median wage). Minimum wage increases in the top tercile—corresponding to Kaitz indices between 0.55 and 0.60—generate substantially larger wage effects: wages raised by 7.2% (SE = 1.2%), whereas the bottom 20 minimum wage events leads to a modest and statistically insignificant increase of 2.1% (SE = 1.2%).

Strikingly, employment effects become somewhat more positive at the higher end of the range of minimum wages. For the upper tercile of changes, we estimate an employment increase of 2.4% (SE = 1.1%), corresponding to an own-wage elasticity of 0.33 (SE = 0.16). The 95% confidence interval excludes negative values, providing no indication of employment losses even among the most binding policy changes. Taken together, these results offer no evidence of a turning point; if anything, the existing evidence suggests that current minimum wage levels remain below the range at which negative employment effects would emerge.

This absence of a turning point is robust across a wide range of specifications. We consider alternative measures of minimum wage bite, including levels and changes in the Kaitz index, real minimum wage levels and changes, the share of workers initially below the new minimum, and the compliance gap. We also examine more exposed subpopulations and alternative definitions of low-skilled workers (e.g., less than high school), and control for additional covariates such as log income per capita. Across all specifications, we confirm the lack of turning point: there is no indication that the top tercile events lead to negative disemployment effects.

While we find no empirical evidence of a turning point, a natural question is whether the existing evidence can nonetheless inform its location. To address this, we develop a model of monopsonistic competition with heterogeneous workers and firms. Workers choose among firms and non-employment in a nested logit framework, generating firm-specific labor supply elasticities that vary by skill and allow for an elastic aggregate labor supply. Firms produce with decreasing returns and set wages subject to these upward-sloping labor supply curves, resulting in equilibrium markdowns of wages below marginal products that differ across worker types. Introducing a binding minimum wage gives rise to three regimes: for some firm-skill pairs the constraint is slack; for others it binds and increases employment by raising wages toward marginal products; and for sufficiently high minimum wages it binds with rationing, reducing employment. This structure implies a natural turning point in the employment effects of minimum wages, and the location of the turning point will depend on the key parameters.

We calibrate the model to match the 60 event-by-event estimates on employment and wage changes. The model is able to broadly replicate the observed heterogeneity patterns in wages and employment, and the calibration suggests a turning point—where the marginal effect on employment turns negative—around a Kaitz index of 0.62 to 0.72, somewhat above the current U.S. range.

Finally, we compare our empirical findings to the calibrated oligopsonistic competition model of Berger et al. (2025). That framework predicts substantially larger negative employment effects, driven by the assumption that small, less productive firms face more elastic labor supply and therefore have limited wage-setting power, causing minimum wage increases to bind most strongly at those firms. Our results are difficult to reconcile with this form of firm-level heterogeneity in labor supply. Instead, we find that heterogeneity in labor supply across skill groups within firms, combined with labor-labor substitution, better matches the observed patterns in the data. This suggests that models of low-wage labor markets should incorporate these features to accurately capture the effects of minimum wage policies.

This paper contributes to the large literature on employment effects of minimum wages, where

evidence on heterogeneity remains limited (Dube and Lindner, 2024). It builds on the shift to event-based approaches in Cengiz et al. (2019), Cengiz et al. (2022), and Clemens and Strain (2024). Studies of large increases or high minimum wage levels have reached mixed conclusions: Godoey and Reich (2021) and Wiltshire et al. (2023) find small employment effects even for large state-level increases, while Jardim et al. (2017) and Clemens and Strain (2024) find larger disemployment effects for specific subgroups or localities. Our systematic treatment of heterogeneity across a wide range of bite measures and worker groups is designed to adjudicate between these findings.[1]

Second, we contribute to the literature that studies the effects of minimum wages using structural approaches and derives optimal policy (e.g., Hurst et al. (2026); Karabarbounis et al. (2025); Berger et al. (2025)). While these frameworks incorporate monopsonistic or oligopsonistic labor markets, our key contribution is to discipline the model using credibly estimated moments from the full set of state-level minimum wage changes. This approach leads to markedly different conclusions. Existing structural models typically predict turning points at much lower levels of the minimum wage—for example, Berger et al. (2025) imply negative employment effects at minimum wages below 30 percent of the median wage. In contrast, our framework predicts positive employment effects up to a Kaitz index of approximately 65 percent, in line with the empirical evidence.

Third, our calibrated monopsonistic model sheds new light on the key features of labor markets and provides useful inputs on the emerging literature on monopsony (Azar and Marinescu, 2024; Kline, 2025). We show that allowing for heterogeneity in labor supply elasticities across skill groups substantially improves the model's fit, highlighting the importance of this dimension for understanding minimum wage effects. These findings suggest that future research should place greater emphasis on skill-based heterogeneity in labor supply.

The rest of the paper proceeds as follows. Section 2 describes the data, the construction of worker groups and minimum wage bite measures, and the empirical methodology. Section 3 shows the empirical results. Section 4 presents the theoretical framework and the calibration results. Section 5 concludes.

[1] In Appendix B, we also provide a detailed comparison of our findings to Clemens and Strain (2024), who studied the implication of 6 large (the dollar change in the real minimum wage) recent minimum wage changes and find large negative employment elasticity with respect to own wage for teenagers or young low skilled adults. We demonstrate that these estimates are fragile: it is specific to one narrow worker group definition and one particular measure of "large". The result is further attenuated when restricting to U.S.-born workers.

2 Data and Empirical Methodology

2.1 Data

We combine several data sources. Our main source is the Current Population Survey (CPS) over 1980–2019. We use the Outgoing Rotation Group (ORG) files for hourly wages, restricting to workers aged 16–64 and excluding observations with imputed earnings.[2] From the Basic Monthly CPS we construct state-level employment rates for each worker group using final person weights.[3]

We complement our analysis with the Quarterly Census of Employment and Wages (QCEW) over 1980–2019, which provides administrative data on employment and earnings by sector. Moreover, we employ minimum wage data which come from the Vaghul and Zipperer (2016) dataset, using both federal and state-level variation.

We aggregate all individual-level data to the state-year-skill level. The key outcomes are log average wages and log employment rates, computed separately for each skill group.

2.2 Treatment Group Definitions and Events

Skill groups. We apply a machine learning procedure developed in Cengiz et al. (2022) to classify the individuals into low-wage and non-low wage workers. We employ a gradient boosted model (GBM) to predict whether a worker is low wage (earns less than 1.25 times the prevailing minimum wage) using demographics (race, sex, Hispanic origin, marital status, rural status, education category, and veteran status).[4] We classify workers as low-wage/low-skill comprising 28 percent of the overall workforce, which capture approximately 75 percent of minimum wage workers.[5] The remaining 78% workforce is classified as non-low wage workers, and we will refer to those workers throughout the text as Skill 3. We can analyze changes in wages and employment for this skill group to assess whether any unusual patterns emerge that might indicate the influence of other shocks occurring alongside the minimum wage change

In certain specifications (and for the purpose of model calibration), we also divide the low-wage workers into two groups: Skill 1, referring to the most likely minimum wage workers, compromising 8% of the workforce and covering 35% of the minimum wage workers. Skill 2 referring to workers who have predicted probabilities between 0.124 and 0.347, making up approximately 20% of the workforce.

[2] We weight wage observations using the CPS earnings weight (`earnwt`).

[3] Employment is defined as being employed at work or employed but absent (`empstat` codes 10, 12), with armed forces members classified as employed.

[4] The GBM uses 4,000 trees with a shrinkage parameter of 0.005 and an interaction depth of 6. We evaluate performance using AUC-ROC and the area under the precision-recall curve on a held-out test set.

[5] Cengiz et al. (2022) refers to that sample as high-recall sample.

We also report results for several worker groups commonly used in the minimum wage literature: teens (aged 16–19), workers aged 21 and under, workers with a high school diploma or less (HSL), and workers with less than a high school diploma aged 25 and under (LTHS under 26). We also examine sector-based groups using the Quarterly Census of Employment and Wages (QCEW): restaurant workers (NAICS 722), and a broader aggregate low-wage sector which combines food services, retail trade (NAICS 44–45), and accommodation (NAICS 721).[6] Figure A2 plots the precision and recall of each group definition.[7] The low-wage worker sample dominates standard demographic definitions, capturing 75 percent of minimum wage workers (recall) with 36 percent precision. Narrower groups such as LTHS under 26 achieve higher precision (67 percent) but much lower recall (19 percent). The HS-or-less group has the highest recall among demographic definitions (69 percent) but the lowest precision (25 percent). The figure also shows sector-based groups, identified using Census industry codes in the CPS; these measure the share of *jobs* (rather than *workers*) in low-wage sectors, so precision and recall are not directly comparable to the person-based groups but provide a complementary perspective.[8] Among sectors, restaurants have high precision (61 percent) but low recall (26 percent); the aggregate low-wage sector (restaurants, retail, and accommodation) achieves 50 percent recall with 39 percent precision.

Previous literature, such as Berger et al. (2025), calibrates structural models across three education groups: less than high school, high school diploma or some college, and college degree or higher. We also report estimates for these subgroups and also evaluate the model's predictions for each of them.

Minimum wage events. Our analysis studies 60 combined state-level minimum wage events over 1980–2019. We require each event to raise the nominal minimum wage by at least \$0.25 and 5 percent. We exclude events that coincide with federal minimum wage increases (1980, 1990, 1996, and 2007), and we combine multi-year phased-in increases within a state into a single event, measuring outcomes at the horizon when the minimum wage reaches its maximum level. Across the 60 events, the average log increase in the real minimum wage is 0.24 and the average post-window length is 2.9 years. Figure A1 shows the distribution of the Kaitz index—the minimum wage relative to the state median wage—across events. Appendix Figure A4 shows the geographic and temporal distribution of these

[6] For the pre-1990 period, we use SIC equivalents (SIC major groups 52–57 and 59 for retail, SIC 70 for accommodation, SIC 58 for eating and drinking places) and align series using the 1990 overlap between SIC and NAICS classifications.

[7] We define a minimum wage worker as one earning less than 1.25 times the prevailing state minimum wage. *Precision* for a group is the share of group members who are minimum wage workers; *recall* is the share of all minimum wage workers who belong to the group. Both are computed from the CPS ORG 1979–2019, pooling across all years and states, weighted by earnings weights. The reference population is workers aged 16–65 with non-missing wages and ML prediction probabilities.

[8] Sector-based precision and recall are computed from the 2000–2019 CPS sample, which uses NAICS-based Census 2002 industry codes. We restrict to this period to avoid bridging between SIC and NAICS industry classifications in the CPS microdata.

events.

For each event, treated states are those that raised their minimum wage, while *clean controls* are states that did not experience any minimum wage change during the pre- or post-treatment window. This ensures that comparisons are not contaminated by overlapping policy changes.

2.3 Defining the Bite of the Minimum Wage

We consider several measures of minimum wage bite, each capturing a distinct economic channel.

A first set of measures captures the *level* of the minimum wage relative to the wage distribution. The Kaitz index—the ratio of the minimum wage to the median wage—is a natural summary of how far the wage floor reaches into the distribution.[9] The real minimum wage level (in 2019 dollars) provides a complementary measure: two states may have the same Kaitz index but different real minimum wages if their median wages differ. The absolute level matters if production costs or product market competition depend on the dollar value of the wage floor rather than its position in the local distribution.

A second set of measures captures the *change* in the minimum wage. The log change in the real minimum wage and the change in the Kaitz index both measure how large the policy shock is. These measures are relevant for a distinct reason: if minimum wage effects involve slow-moving adjustments—firm entry and exit, capital-labor substitution, or gradual price pass-through—then a larger discrete increase may cause these adjustments to materialize faster, making them easier to detect within the event-study horizon. The size of the change therefore provides information about the *speed* of adjustment, not only the long-run magnitude.

A third set of measures comes closer to a direct measure of treatment intensity. The share of workers earning below the new minimum wage in the pre-reform period is the most literal measure of the "complier share"—the fraction of workers whose wages must rise mechanically for the employer to comply with the law. We also construct a "total bite" measure that combines this share with the average log wage gap between current wages and the new minimum among affected workers. This product captures both how many workers are affected (the extensive margin) and how much their wages must rise (the intensive margin), providing a summary of the total labor cost shock imposed by the policy.[10]

These measures are correlated but not interchangeable. The Kaitz index and the share below both

[9] The Kaitz index was originally proposed by Kaitz (1970). See Dube and Lindner (2024) for a discussion of its use in the modern minimum wage literature.

[10] The gap measure is computed from pre-reform CPS microdata. Let w_i denote the real wage of worker i and $\overline{m}$ the new real minimum wage. The share below is $s = \Pr(w_i \leq \overline{m})$, the average log gap conditional on being below is $g = E[\log \overline{m} - \log w_i \mid w_i \leq \overline{m}]$, and the total bite is $s \times g$.

proxy for the biteness of the policy, but the Kaitz index is available without microdata and is more commonly used in international comparisons. The change measures capture variation that the level measures could miss: two events ending at the same Kaitz index may differ in the size of the increase. By examining heterogeneity along all of these dimensions, we can assess whether the presence or absence of a turning point is robust or an artifact of any single measure of bite.

2.4 Empirical Specification

Baseline estimator. We implement the stacked event study estimation of Cengiz et al. (2019). We do this by estimating the following (local projections) difference-in-differences (LP-DiD) estimator:

$$y_{s,t+k} - y_{s,t-1} = \beta D_{s,t} + \delta_t + \nu_{s,t}, \tag{1}$$

where $y_{s,t}$ is a log outcome (wages or employment) in state s at time t, $D_{s,t}$ is an indicator for a minimum wage increase, δ_t are year fixed effects, and $\nu_{s,t}$ is the error term. The estimation sample contains newly treated observations ($D_{s,t} = 1$) and clean controls ($D_{s,t} = 0$) that are not treated in the pre- or post-treatment window. For each event, we set the horizon k to the year at which the minimum wage reaches its maximum level, capping k at 5 years after the onset of the event. Regressions are weighted by state population and standard errors are clustered by state. This estimator is numerically equivalent to the stacked difference-in-differences approach of Cengiz et al. (2019).

Joint estimation and own-wage elasticities. To relate wage and employment effects, we estimate the two equations jointly using a seemingly unrelated regression (SUR) system with population weights and state-clustered standard errors:

$$\Delta \log w_{s,e} = \alpha^w D_{s,e} + \delta_t^w + \nu_{s,e}^w \tag{2}$$

$$\Delta \log e_{s,e} = \alpha^e D_{s,e} + \delta_t^e + \nu_{s,e}^e, \tag{3}$$

where s indexes states and e indexes events, $\Delta \log w_{s,e}$ is the change in log average wages and $\Delta \log e_{s,e}$ the change in log employment for low-wage workers workers. This joint estimation allows us to compute the own-wage elasticity of employment as $OWE = \hat{\alpha}^e / \hat{\alpha}^w$, with standard errors obtained via the delta method.[11]

[11] Because the regressors are identical in both equations, SUR point estimates coincide with equation-by-equation OLS; the value of joint estimation is that it provides the cross-equation covariance matrix needed for delta-method inference on the ratio $\hat{\alpha}^e / \hat{\alpha}^w$.

Testing for heterogeneity by minimum wage bite. To assess whether employment effects vary with the bite of the minimum wage, we split the 60 events into terciles of a given bite measure X (e.g., the Kaitz index) and estimate tercile-specific effects. For each bite measure, we construct a stacked dataset in which each tercile's treated events are paired with the set of clean controls active in the same years. The regression interacts the treatment indicator with tercile dummies:

$$\Delta y_{s,e} = \sum_{j=1}^{3} \alpha_j \left(D_{s,e} \times \mathbf{1}[e \in T_j] \right) + \sum_{j=1}^{3} \delta_{t,j} + \nu_{s,e}, \tag{4}$$

where T_j denotes the j-th tercile of the bite measure, and each tercile has its own set of year fixed effects $\delta_{t,j}$. The coefficients α_1, α_2, and α_3 give the average treatment effect within each tercile. We test for heterogeneity using the difference $\alpha_3 - \alpha_1$ between the top and bottom terciles. With 60 events, terciles provide approximately 20 events per bin, balancing resolution against statistical power. The same tercile structure is applied within the SUR system to obtain tercile-specific OWEs.

We apply this framework to seven bite measures, defined in Section 2.3: the change in the Kaitz index, the log change in the Kaitz index, the real minimum wage level, the change and log change in the real minimum wage, the share of workers below the new minimum, and the total bite (share below times average wage gap). The Kaitz index level serves as the primary bite measure in the baseline analysis.

Covariates. Our baseline specification includes no covariates beyond year fixed effects. As robustness, we add three controls: (i) the change in log employment for Skill 3 workers, which proxies for state-level labor demand shocks among workers unlikely to be bound by the minimum wage; (ii) the change in log wages for S_3 workers, which serves a similar purpose for wage trends; and (iii) the change in log income per capita, which controls for broad macroeconomic conditions. We report results without controls, with each control separately, and with all controls jointly.

Pre-trends. We assess the validity of the parallel trends assumption by estimating equation (1) over the pre-treatment window ($k < 0$). We report pre-trend estimates alongside the main results and flag specifications where pre-trends are not parallel.

3 Empirical Results

3.1 Baseline Results and Heterogeneity

We present results in five steps: aggregate effects, heterogeneity by Kaitz index, robustness across alternate bite measures and worker groups, and an assessment of what predicts cross-event variation in effects.

Aggregate effects. Figure A5 reports the baseline event-study estimates for the low-wage workforce (left panels) and the narrower Skill 1 subgroup (right panels). Both groups show wages rising steadily after the minimum wage increase, with no pre-trend. Employment effects are small and centered on zero throughout the post-treatment horizon. The Skill 1 subgroup shows larger wage effects—as expected for a more narrowly targeted sample—but noisier employment estimates. Pooling across all 60 events, the average wage effect is 0.046 (SE $= 0.010$) and the average employment effect is 0.006 (SE $= 0.008$), yielding an overall pooled own-wage elasticity of $OWE = 0.13$ (SE $= 0.15$)—positive but imprecisely estimated. Table A1 shows that this pattern holds across alternative worker groups: Skill 1 workers, the restaurant sample, the low-wage sectors, teens, workers with a high school diploma or less, and the full workforce all show positive wage effects and employment effects that are small and statistically indistinguishable from zero.[12]

Heterogeneity by Kaitz index. Does the employment response become more negative when the minimum wage bites harder? Table 1 address this question by splitting events into terciles of the Kaitz index. The wage effect rises monotonically with the Kaitz: 0.021 (SE $= 0.012$) in the bottom tercile (mean Kaitz $= 0.41$) to 0.072 (SE $= 0.012$) in the top tercile (mean Kaitz $= 0.54$). Employment effects, however, do not follow: the point estimate in the top tercile is 0.024 (SE $= 0.011$), slightly *positive*, with an implied tercile three own-wage elasticity of 0.33 (SE $= 0.16$). We can also calculate the the employment elasticity by comparing the wage and employment responses at the lower minimum wage events (tercile 1) to the higher minimum wage regime events (tercile 3). The implied own-wage elasticity is 0.54 (SE $= 0.24$)—positive and statistically significant. The 95 percent confidence intervals for these estimates excludes negative values entirely, ruling out a turning point within this range of the Kaitz index.

In Figure 2, we present more disaggregated evidence by plotting the wage effects (panel A) and employment effects (panel B) for all 60 events as a function of the Kaitz index. The figure also includes

[12] Appendix Table **??** reports robustness across controls, weighting schemes, and aggregation methods for the low wage sample. Results are stable.

a local polynomial fit (red line) to summarize the underlying relationship. At lower levels of the minimum wage, we observe relatively modest wage effects, accompanied by slightly negative—but imprecisely estimated—employment effects. As the Kaitz index increases, wage effects rise clearly and monotonically. Moreover, for the most binding events, we find evidence of positive employment effects, with no instances of negative disemployment events. Overall, this figure reinforces our main conclusion: even at a highly disaggregated level, there is no systematic evidence of adverse employment effects from minimum wage changes at the Kaitz level between 55-60% of the minimum wage changes.

Pre-trend. We examine the evolution of employment and wages across pre-treatment periods separately for each tercile on Figure 1. Across all terciles, there are no statistically significant differences in employment or wages between treated and control locations prior to the policy change, supporting the absence of differential pre-trends. At the same time, we observe clear post-treatment wage effects in each tercile. While the estimated wage increases are somewhat smaller in the lowest tercile, the primary distinction across terciles reflects differences in composition. In particular, tercile 3 includes events observed over longer post-treatment horizons (higher event time k), which mechanically capture larger cumulative wage changes. Thus, tercile 3 reflects not only larger minimum wage increases but also longer-term effects.

Turning to employment, pre-treatment trends are similarly flat across terciles. Following the minimum wage increase, we observe a slight and noisily estimated disemployment effect in the lowest tercile, particularly in periods 3-5. However, these estimates are based on a limited number of observations, as most events in tercile 1 are only observed for up to two years post-treatment. In contrast, for tercile 3, we find little evidence of disemployment effects; if anything, the estimates are slightly positive. Overall, these patterns reinforce that our main findings are not driven by pre-existing trends.

Beside, the event study figures, we also present event by event estimates for pre-trends (see Appendix Figure A6) and also we report the pre-trend estimates in Table A4).

Alternate bite measures. Table 4 extends the heterogeneity analysis to all seven bite measures defined in Section 2.3. The qualitative pattern is consistent across measures: wage effects rise with bite, while employment effects remain small or mildly positive. For example, splitting by the share of workers below the new minimum—the most direct measure of treatment intensity—yields a tercile 3 wage effect of 0.082 (SE $= 0.010$), a tercile 3 employment effect of 0.032 (SE $= 0.010$), and an employment wage elasticity of 0.39 (SE $= 0.13$). The difference between the tercile 3 and tercile 1

employment change is 0.038 (SE $= 0.016$), positive and marginally significant. In In Appendix Figures A8 we show that for the share of employment below the new minimum wage leads to the most striking wage gradient (from close to zero wage effects at the lowest events to 12% wage change for the highest share events). In addition to that, we find a similar disemployment effect. There is a close to zero or negative estimates for the lowest wage events where the wage effects are not positive, suggesting that those negative effects unlikely to reflect the effect of the minimum wage. Furthermore, similarly to the findings with the Kaitz index, for larges events we find a clear and positive employment effects. [13]

Table 4 also shows the estimates where we classify events to terciles based on the *change* instead of the *level* (log change in real MW, change in Kaitz). We find a similar absence of negative employment effects in the top tercile.

Different worker groups. Table 3 examines whether the absence of a turning point extends beyond the low wage worker sample by decomposing effects across skill subgroups. The most exposed skill group, Skill 1, experience larger wage effect of 0.105 (SE $= 0.020$). Still the tercile 3 employment effect is positive with 0.042 (SE $= 0.023$), This implies an elasticity with respect to wage of 0.40 (SE $= 0.25$). Meanwhile, Skill 2 workers show a slightly lower tercile 3 wage effect of 0.057 (SE $= 0.011$) and a tercile 3 employment effect of 0.016 (SE $= 0.010$), with a tercile 3 OWE of 0.27 (SE $= 0.18$). The non-low wage (Skill 3) workers show near-zero wage and employment effects throughout, confirming that the estimated effects are concentrated among workers likely to be bound by the minimum wage.

We also report the effects by education groups (Appendix Table A2). Table A1 confirms that the pattern holds across additional demographic groups—teens, restaurant workers, the low-wage sectors aggregate, and the full workforce—all of which show positive wage effects and employment effects that are statistically indistinguishable from zero.

Robustness to covariates and event selection. Table 2 shows that adding controls for log income per capita reduces the tercile 3 wage effect modestly (from 0.072 to 0.052) and attenuates the tercile employment effect to near zero (0.009, SE $= 0.013$). The employment elasticity with respect to wage remains small and positive throughout. Adding all covariates jointly yields a similar pattern (tercile 3 OWE $= 0.19$, SE $= 0.22$). Appendix Figure A7 report the full Kaitz heterogeneity analysis with all covariates. Restricting to long events (those with at least three post-treatment years) or events with long post-windows does not change the conclusions: the tercile 3 own wage elasticity ranges from 0.33 to 0.37 across event selection criteria, always positive and never significantly negative (Table 2).

[13] Appendix Figures A8, and A9 and Appendix Tables A3, and A4 report the full set of heterogeneity exercises.

What predicts minimum wage effects. Table A7 asks which event characteristics predict the cross-event variation in wage and employment effects. Minimum wage bite—summarized by a composite of the Kaitz index, the real minimum wage, and the size of the increase—strongly predicts wage effects (coefficient $= 1.00$, SE $= 0.23$), confirming that higher-bite events produce larger first-stage wage increases. The same composite has a positive but less precise relationship with employment effects (coefficient $= 0.66$, SE $= 0.28$), consistent with the absence of a turning point. Local labor market conditions—the unemployment rate and establishment concentration—do not significantly predict either wage or employment effects.

Effects by disaggregated treatment groups, and multiple testing. Table A1 reports pooled effects across the full set of demographic groups used in the analysis: the low-wage worker samples, teens, workers with a high school diploma or less, restaurant workers, the low-wage sectors aggregate, and the full workforce. All groups show positive wage effects and employment effects that are small and statistically indistinguishable from zero. Appendix Table B1 extends this comparison to the top Kaitz tercile, reporting tercile 3 own-wage employment elasticities for nine worker groups across four bite measures. The broad pattern is consistent: all groups except LTHS under 26 show OWEs that are small or positive when events are ranked by the Kaitz index. The LTHS-under-26 group—the basis of the Clemens and Strain (2024) finding—shows a large negative OWE only with the dollar change measure (-0.65, SE $= 0.21$), not with the Kaitz index (-0.20, SE $= 0.36$); we return to this fragility in Section 3.2.

Because we test for heterogeneity across many worker groups and bite measures, we assess whether any individual finding could be a false discovery. We apply the Romano and Wolf (2005) stepdown procedure to control the familywise error rate, using a cluster bootstrap with 999 replications.[14] We consider two hypothesis families: the T3–T1 employment contrast across all 14 worker groups (fixing the Kaitz bite measure), and across all seven bite measures (fixing the low-wage worker sample). No T3–T1 employment contrast is significantly negative in any group or bite measure—either before or after the Romano-Wolf adjustment. The adjusted p-values reinforce the conclusion that the absence of a turning point is not an artifact of selective reporting.

Summary. The central finding is the absence of a detectable turning point. Wage effects rise monotonically with the bite of the minimum wage, confirming that higher-bite events generate larger

[14] For each bootstrap replication, we resample state panels with replacement and re-estimate the SUR system. We test the difference between tercile 3 and tecrile 1 employment (a linear combination) rather than the OWE ratio, which can be numerically unstable. The bootstrap coefficients are re-centered and scaled by observed standard errors to form the step-down test statistics. Details are in Appendix Table B6. We also apply the procedure to the full grid of worker groups and bite measures tested in the Clemens and Strain (2024) comparison (Table B5).

first-stage effects. Employment effects, however, remain small across all measures of bite, all worker groups, and all specifications. In the top tercile by Kaitz—where the minimum wage reaches approximately 54 percent of the median wage—the own-wage employment elasticity is positive, and the 95 percent confidence interval for the T3–T1 contrast OWE excludes negative values. These bounds are informative: they are inconsistent with the large negative employment effects predicted by the calibrated Berger et al. (2025) model, as we show in Section 4.

3.2 Comparison with Clemens and Strain (2024)

The most prominent claim of nonlinear minimum wage effects in recent U.S. data comes from Clemens and Strain (2024). They report a large negative own-wage elasticity ($OWE = -1.01$) for workers with less than a high school diploma aged below 26 in states with "large" minimum wage increases, defined by the dollar change in the real minimum wage. Using our tercile framework, we can replicate a large negative T3 employment effect for this group ($OWE = -0.65$, SE $= 0.21$), but it is fragile along three dimensions.

First, the result is specific to one narrow worker group: the LTHS-under-26 sample, which comprises only 8 percent of the workforce and 19 percent of minimum wage workers. For low wage workers in general, the tercile $OWE = 0.08$ (SE $= 0.21$); for Skill 1 workers, $OWE = -0.19$ (SE $= 0.25$). Second, the result is specific to one bite measure: the dollar change in the real minimum wage. Using the Kaitz index—which more directly captures the bindingness of the minimum wage—yields $OWE = -0.20$ (SE $= 0.36$) even for their preferred group. It is also smaller in magnitude and not significant using the level of minimum wage, or the GAP measure. Third, restricting to U.S.-born workers reduces the OWE from -0.65 to -0.38 (SE $= 0.24$), which is no longer statistically significant.

We formalize this fragility using the Romano and Wolf (2005) stepdown procedure applied to the full grid of nine worker groups and four bite measures (Appendix Table B1). Of the 36 tercile 3 employment coefficients, none is significantly negative after Romano-Wolf correction for multiple testing (Appendix Table B5). The Clemens and Strain result, which appears significant in isolation ($p = 0.004$), has a Romano-Wolf adjusted p-value of 0.29. Appendix B provides a detailed comparison.

4 Theoretical Framework

4.1 Our Model

In this section, we propose a simple partial-equilibrium model of the labor market to rationalize our empirical results. The model features monopsonistic competition between firms for heterogeneously-

skilled workers, with firm-facing labor supply elasticities that vary by skill, and a potentially elastic aggregate labor supply. [15]

We use a Card et al. (2018)-type monopsonistic labor market with nested logit labor supply. Worker i of skill s has utility over M firms and an outside option, unemployment:

$$U_{ij} = \ln w_{j,s} + \epsilon_{ij}, \tag{5}$$

where ϵ_{ij} follows a multivariate GEV distribution. The model has two nests, employment and unemployment. Let $1/\beta_s$ denote the scale of taste shocks across firms inside the employment nest, and let $1/\alpha_u$ denote the scale of taste shocks across nests. The nesting parameter is $\mu_s = \alpha_u/\beta_s$, with $\mu_s < 1$ implying that firms are closer substitutes for each other than for unemployment, and $\mu_s = 1$ collapsing to a flat logit.

Conditional on employment, worker i chooses firm j with probability

$$\Pr(j \mid \text{emp}) = \frac{w_{j,s}^{\beta_s}}{\Omega_s^{\beta_s}}, \qquad \Omega_s \equiv \left(\sum_j w_{j,s}^{\beta_s} \right)^{1/\beta_s}, \tag{6}$$

where Ω_s is the inclusive value, or wage index, from the firm-choice nest. Participation in employment is determined in the upper nest:

$$1 - u_s = \frac{\Omega_s^{\alpha_u}}{\omega_{0,s}^{\alpha_u} + \Omega_s^{\alpha_u}}, \tag{7}$$

where $\omega_{0,s}$ is the value of the outside option and ϕ governs the aggregate labor supply elasticity through $\alpha_u = \phi/(1 - p_0)$, where p_0 is the baseline non-employment rate. Combining participation and firm choice, labor supply of skill s to firm j is

$$\ell_s(w_{j,s}) = N_s(1 - u_s)\frac{w_{j,s}^{\beta_s}}{\Omega_s^{\beta_s}}. \tag{8}$$

Firms hire labor and produce with decreasing returns to scale and no capital[16]:

$$q_j = \overline{Z} z_j L_j^{\alpha}, \qquad \alpha < 1, \tag{9}$$

with CES labor aggregator

$$L_j = \left(\sum_s \varphi_s^{\frac{1}{\sigma}} \ell_{j,s}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}, \tag{10}$$

[15] Appendix Figure A3 illustrates the employment effects in the standard monopsonistic competition framework for low, intermediate, and high minimum wage levels.

[16] Or with capital optimized out; the two approaches are equivalent given appropriate choice of other parameters.

and $\ln z_j \sim N(0, \sigma_z^2)$ in the firm population. Firms post wages $w_j \equiv [w_{j,1}, \ldots, w_{j,S}]'$ and choose labor demand to maximize

$$\max_{w_j} \ \overline{Z} z_j L_j(w_j)^{\alpha} - w_j' \ell(w_j), \tag{11}$$

subject to the labor supply schedule above. The firm's first order condition for $w_{j,s}$ is given by:

$$\alpha \overline{Z} z_j L_j^{\alpha-1} \varphi_s^{\frac{1}{\sigma}} \ell_{j,s}^{\frac{-1}{\sigma}} \frac{\partial \ell_{j,s}(w_{j,s})}{\partial w} = \ell_{j,s}(w_{j,s}) + w_{j,s} \frac{\partial \ell_{j,s}(w_{j,s})}{\partial w} \tag{12}$$

Rearranging, we obtain the familiar formula:

$$w_{j,s} = \frac{\beta_s}{\beta_s + 1} MPL_{j,s} \tag{13}$$

Where $MPL_{j,s} = \alpha \overline{Z} z_j L_j^{\alpha-1} \varphi_s^{\frac{1}{\sigma}} \ell_{j,s}^{\frac{-1}{\sigma}}$ is the marginal product of labor type s at firm j. Since $\ell_{j,s}$ depends on $w_{j,s}$ directly and L_j depends on all of the firm's labor inputs $\ell_{j,s}$, 13 is an implicit function of the wages that firm j pays to all skill types, giving a system of S equations in S unknowns for each firm to solve. An equilibrium is a set of wages $w_{j,s}$ satisfying (12) given labor supply (8) for all (j, s).

With a binding minimum wage $\underline{w}$, we follow Berger et al. (2025) and introduce shadow wages to handle rationing at constrained firms. Each firm–skill pair (j, s) falls into one of three regions. In *Region I*, the firm's optimal wage exceeds the minimum ($w_{j,s}^* > \underline{w}$), so the constraint does not bind and the firm sets its unconstrained optimal wage. In *Region II*, the minimum wage binds ($w_{j,s} = \underline{w}$) but the firm's marginal product of labor at the resulting employment level still exceeds the markdown-adjusted wage, so the firm would prefer to hire *more* workers at the minimum wage than it attracts. In this region the minimum wage is employment-increasing: it raises wages above the monopsony optimum and expands labor supply to the firm. In *Region III*, the minimum wage binds and the firm would prefer to hire *fewer* workers than it attracts at $\underline{w}$. To model rationing in this region, we introduce a shadow multiplier $v_{j,s} \in (0, 1]$ that discounts the posted wage in workers' utility:

$$\ell_{j,s} = N_s(1 - u_s) \frac{(\underline{w} v_{j,s})^{\beta_s}}{\Omega_s^{\beta_s}}, \tag{14}$$

where $v_{j,s} = 1$ for firms in Regions I and II, and $v_{j,s} < 1$ for firms in Region III. Labor supply to firm j thus behaves as if the wage offered by firm j to type s workers is equal to $w_{j,s} v_{j,s} \leq w_{j,s}$. Berger et al. (2025) show that in equilibrium $v_{j,s}$ can be derived from the firm's maximization problem when one imposes that the firm's employment of type s labor does not exceed an optimally-chosen rationing constraint: the shadow multiplier is pinned down by the condition that the firm's marginal product

of labor equals the posted wage at the rationed employment level:

$$MPL_{j,s} = \underline{w}. \tag{15}$$

The inclusive value is computed over shadow wages: $\Omega_s = \left(\sum_j (w_{j,s} v_{j,s})^{\beta_s}\right)^{1/\beta_s}$, so that rationing at constrained firms endogenously reduces the attractiveness of the employment nest and affects both participation and the allocation of workers across firms.

An equilibrium with minimum wage $\underline{w}$ is a set of wages $w_{j,s}$ and shadow multipliers $v_{j,s}$ such that: (i) unconstrained firms set wages according to (12); (ii) constrained firms pay $w_{j,s} = \underline{w}$; (iii) for Region III firms, $v_{j,s}$ equates marginal product to the minimum wage; and (iv) the labor market clears for all types s.

4.2 Model Calibration and Estimation

To assess whether our relatively simple monopsony model can fit our empirical estimates of the employment and wage effects of US minimum wage changes by skill group, we need to parameterize the model. Define the set of model parameters $\{\overline{Z}, \omega_0, \varphi\}$ as the model "constants"—these parameters are the aggregate productivity shifter $\overline{Z}$, a vector of skill-specific worker outside options $\omega_{0,s}$, and a vector of relative productivities φ_s, with φ_S normalized to 1. These constants pin down the scale and location of the economy, and the relative wages of each skill group. Our internal calibration procedure chooses values for these parameters to match the mean wage, the relative wages of the bottom two skill groups, and the employment rate of each skill type when the minimum wage is zero. Then, for each parameterization over a coarse grid in α, ϕ, σ_z, β_s, and σ, we carry out the internal calibration procedure above to pin down the model constants, mapping our notion of worker "skill" to the probabilistic skill groups from our machine learning approach in the empirical section. Our chosen parameterization is the one that minimizes the mean squared error between the empirical and simulated wage and employment effects from each of our reform events for the low wage workers (Skill 1 and Skill 2 combined).

Column 1 of Table 5 reports our baseline parameterization that we obtain from this grid search procedure.[17] We defer discussion of these parameter values until Section 4.3, where we compare with the proposed model in Berger et al. (2025), another recent attempt to present a theoretical framework for analyzing the employment, wage, and welfare effects of a minimum wage. An extended discussion of the differences between BHM25 and DLMP can be found in section 4.4, which conducts a step-by-

[17] We are in the process of implementing a formal indirect inference estimation of model parameters.

step decomposition of the differences between the two models, revealling which modelling choices drive BHM25's large predicted disemployment effects relative to ours, which are a better fit to the data.

Figure 3 compares the predictions of our preferred specification with the empirical estimates for the low wage workers by simulating each reform event in the DLMP model, calculating wage and employment effects, and then plotting a local polynomial fit line for these simulated effects along with our empirical estimates. We get a reasonable fit to the data, including persistently positive employment effects into the upper tail of the Kaitz distribution. Table 5 reports the standardized mean squared error for the event-specific wage and employment effects; the parameterization we choose from our grid search minimizes this statistic over our coarse grid. Figure A10 shows the same comparison, for aggregate wages and employment.

Figure 4 plots employment and wages in the simulated DLMP labor market for different levels of the Kaitz index. Subplot (a) plots these quantities separately by skill group, while subplot (b) shows them for the low-skill groups only. Reassuringly, the employment and wage effects for Skill 3 workers are zero for almost the entire range of Kaitz indices we simulate. Reading off subplot (b), low-skill employment peaks at a Kaitz of around 0.62, but MW reforms up to a Kaitz of around 0.72 can potentially raise employment relative to a world with no minimum wage. Allowing for firm-level labour supply elasticities that are heterogeneous by skill and market-level labor supply responses seems to move the predictions of theory much closer to the empirical evidence. This level of the minimum wage is well above what the previous literature, such as Berger et al. (2025), has identified as the turning point for minimum wage effects.

4.3 Berger et al. (2025) Model

Berger et al. (2025) (henceforth BHM25), is a well-known recent attempt to derive the socially optimal level of the minimum wage in a rich model of the labor market where employers compete within and across local labor markets for workers that have nested CES preferences over firm-specific employment. The oligopsonistic structure of competition gives firms heterogeneous degrees of market power; larger firms face a lower elasticity of labor supply and therefore have larger wage markdowns. The model allows for the standard monopsony motive for a minimum wage (since firms face a finite elasticity of labor), as well as rich patterns of worker reallocation across employers due to the size-dependent elasticity of labor that each firm faces. Purely on efficiency grounds, the optimal minimum wage in their framework is quite low: around $7.35 in 2019 dollars, corresponding to a Kaitz index of 0.37. Even with redistributive concerns, the optimum is around $11, corresponding to a Kaitz index of 0.55. As

the authors point out, a key driver of this result is the oligopsonistic structure: high-productivity firms face relatively inelastic labor supply while low-productivity firms face elastic labor supply, closer to the competitive benchmark. A minimum wage increase therefore hits low-productivity, low-markdown firms hardest, leaving high-productivity, high-markdown firms untouched, which leaves little scope for positive employment effects of a minimum wage in their framework.

BHM25's model validation for employment effects relies on a single case study of the Seattle minimum wage (Jardim et al., 2022). But that exercise uses the nominal level of the minimum wage, does not account for Seattle's low Kaitz. Moreover, the findings from that paper are highly sensitive to the method used, and exhibits upper-tail effects as described in Dube and Lindner (2021). We therefore compare the model directly to the evidence from 60 state-level events. We calculate the calibrated model's wage and employment predictions for the non-college workforce in those events, and realign national predictions to states using the Kaitz index associated with a federal dollar minimum. This misses cross-state mobility, but that does not appear empirically relevant according to Dube and Lindner (2024).

To assess the empirical plausibility of BHM25's model, we simulate each of the 60 events in our sample using their model, and calculate the implied wage and employment effects. Since BHM25's baseline broad skill groups are defined by educational attainment (less than high school, high school, and college workers), we calculate these simulated effects among the LHS group and the bottom two income quintiles of the HS group, which together make up around 30% of the worker population, in order to make them comparable to our empirical results (which define skill based via the machine learning approach discussed in the empirical section and find around 30% of the sample in the low-skill group). Figure 5 plots our event-by-event empirical estimates (with a local polynomial fit line in red) overlaid with a green local polynomial fit line for BHM25's event-by-event simulated results. It is clear from the figure that the model predicts substantially more negative employment effects than the empirical estimates imply. At the bottom of Table 5 we report BHM25's MSE for the event-specific wage and employment effects; it is around 70% larger than the MSE for our preferred parameterization of DLMP.

Table 6 reports model-implied OWEs for both DLMP and BHM25, along with our empirical estimates for OWEs by tercile for the low-skill group; BHM25 delivers negative OWEs for the entire range of changes that we consider, including at the very bottom, with post-reform Kaitz of only 0.30. In the next section 4.4, we explore what drives the discrepancy between our and the BHM25 model.

4.4 BHM25 to DLMP: a Decomposition of Differences

Why is DLMP able to match the wage and employment effects that we see in the data, while BHM25 is not? It isn't simply insufficient monopsony power for employers: Table 7 contains the results of a sensitivity analysis of BHM25, wherein we change the value of the within- and between-market substitution parameters and investigate whether shifting from oligopsony to monopsony can produce predicted OWEs that are closer to the data. It is evident that a more monopsonistic environment does raise the model's implied own-wage elasticity substantially for MW changes in our second and third terciles, but it takes an unrealistic degree of monopsony to get close to the data, and even in the range of our model's labor supply elasticities (between 1.5 and 3, depending on worker skill) still results in disemployment effects where the data suggests positive effects.

Table 5 lists the parameters of our preferred model in column 1, and those of BHM25 in column 2. Our model allows for skill-specific labor markets that differ in terms of how much wage-setting power employers have, with the lowest-skill workers facing the least competitive labor markets, which generates scope for markdown-reducing and employment-increasing binding minimum wages for these workers. In BHM25's oligopsonistic environment, LS elasticities are heterogeneous along a different dimension: firm size. The fact that the firms with the biggest markdowns are at the top of the wage distribution means that in order to target the firms with the most market power, the minimum wage must be set at such a high level that disemployment effects at other firms offset the potential gains. We find that a monopsonistic environment where LS elasticities vary by skill, not by firm, is better able to fit the data.

Another difference is in our returns to scale parameters, α. Lower returns to scale steepens the marginal product of labor curve, which although it decreases the desired increase in employment at Region II firms, also reduces the scope for decreased employment at Region III firms, due to the steep rise in MPL_s when employment of type s falls.

Our elasticity of substitution between worker types is also much lower than BHM25's value of over 13, implying rapid substitution away from low skill workers as the MW begins to bind for them. Finally, we allow for market-level labor supply responses by raising the aggregate labor supply elasticity above BHM25's level of effectively zero [18].

In addition to these higher-level choices about key parameters, other modelling and calibration choices contribute to BHM25's predictions of strong disemployment effects and negative OWEs even for moderate levels of the minimum wage, and the divergence between the predictions of our preferred

[18] They use additively separable, isoelastic preferences, with consumption curvature equal to 1.05 and a Frisch elasticity of 0.62, which corresponds to a Marshallian elasticity of labor supply of essentially 0.

model and theirs. To illustrate the point, Table 8 presents a "bridge" from BHM25 to our model, DLMP. Starting with BHM25's preferred version of their model in the first column, each subsequent column changes a feature of the model or calibration strategy, and then simulates the model-predicted employment and wage effects, as well as the implied OWE, of a minimum wage change from a starting Kaitz index of 0.48 to a post-reform Kaitz of 0.54, which corresponds to the average minimum wage change in the upper tercile of our sample of events. The second column (BHM monop. GE) removes oligopsony from BHM25 by imposing a constant labor supply elasticity of 3.02 facing each firm, which is (Berger et al., 2025)'s preferred value for this parameter in the monopsony-only version of their model. Moving to a monopsonistic version of their model has little effect, raising the magnitude of the predicted disemployment effect as well as the wage effect. Turning off the general equilibrium element of the model in the next column (BHM25 monop. PE) again has little effect, as does raising the elasticity of aggregate labor supply (more discussion of this below). Moving to a nested logit formulation of the model is without loss, as we can qualitatively replicate BHM25's predicted employment and wage effects up to that point in the model bridge, given the same choice of aggregate labor supply elasticity ϕ and LS elasticity to the firm β.

The next change matters substantially for the model's predicted impacts. For simplicity, consider the homogenous workers case. In both BHM25 and DLMP, aggregate labor supply is given by an isoelastic function, $C_m \mathbf{W}_m^{\phi}$, where C_m is a constant specific to model m, and $\mathbf{W}_m$ is the aggregate wage index implied by model m. In BHM25, workers' nested CES preferences over firms implies that $\mathbf{W}_{BHM25} = \left(\sum_j w_j^{1+\beta} \right)^{\frac{1}{1+\beta}}$. DLMP's wage index (the inclusive value of employment) is given by $\mathbf{W}_{DLMP} = \left(\sum_j w_j^{\beta} \right)^{\frac{1}{\beta}}$. Even with the same value of labor supply to the firm β and the same LS elasticity (ϕ) with respect to the aggregate wage index ($\mathbf{W_m}$), minimum wage changes in the two models have very different effects on total employment. Because wages in the BHM25 index are powered to a larger value than in DLMP, the BHM25 index is less sensitive to wage changes at the bottom of the wage distribution, exactly where supply-constrained firms, where labor demand is increasing with the MW, are located. Thus, because the BHM25 aggregate wage index is relatively insensitive to MW changes at levels where both the data and our model indicate positive employment effects, any employment gains among low- and medium-productivity firms can virtually only be at the expense of employment up further up the firm ladder. This fact also explains why raising the aggregate labor supply elasticity does not generate larger positive employment effects before the MW starts to move firms into Region II: the aggregate value of employment simply does not rise enough with the MW to generate substantial increases in employment on the extensive margin.

A second issue concerns the baseline distribution of employment. BHM25's preferred calibration of their monopsony model with heterogeneous workers model implies extremely counterfactual employment rates in the zero minimum wage economy. The model implies that less than high school workers' employment rate are about 5% of the college graduates employment rate; for high school graduates the figure is about 12%. In the CPS 2015-2019, the employment rates of less than high school, high school, and college workers aged 16-65 are 43%, 70%, and 83%, respectively. BHM25 therefore produces large proportional falls in employment of the lowest two skill groups in part because baseline employment, the denominator, is so low in levels. Together, changing the aggregate wage index to the one implied by our nested logit model and calibrating $\overline{Z}$, $\omega_{0,s}$, and φ_s to target the baseline employment rate directly (along with mean wages for each skill group) erodes the negative employment effect by around 70%.

The next four columns go the rest of the way to DLMP. Lowering the elasticity of substitution between worker types σ from BHM25's 13 prevents firms from rapidly substituting away from exposed workers. Lower returns to scale steepens the marginal product of labor curve, which although it decreases the desired increase in employment at Region II firms, also reduces the scope for decreased employment at Region III firms, due to the steep rise in MPL_s when employment of type s falls. Lower firm productivity dispersion, helps to raise the employment effect, but perhaps counterintuitively it does not result in a fall in the wage effect: because the MW induces some reallocation of labor down the productivity ladder, making the ladder shallower can offset some of the negative wage effect that comes from changing the firm composition of employment. Finally, heterogeneous labor supply elasticities by skill type generate the requisite amount of wage-setting power for the minimum wage to be markup-reducing (and employment-increasing) in the correct Kaitz range.

5 Conclusion

This paper asks whether a "turning point" has been reached—a level of the minimum wage at which employment effects become substantially negative. We study 60 U.S. state-level minimum wage increases from 1980 to 2019, using a machine learning model to identify affected workers, and test for heterogeneity across seven measures of minimum wage bite.

The answer, through the range of U.S. experience, is no. Employment effects remain small and statistically indistinguishable from zero across all measures of bite, all worker groups, and all specifications. In the top tercile by Kaitz index—where the minimum wage reaches approximately 60 percent of the median wage the own-wage elasticity is positive, and the 95 percent confidence interval

excludes negative values. Wage effects, by contrast, rise monotonically with bite, confirming that higher-bite events generate genuine first-stage increases.

These findings have implications for both theory and policy. The calibrated Berger et al. (2025) model predicts substantially more negative employment effects than we observe, suggesting that its parameterization—particularly the low aggregate labor supply elasticity and the reliance on a single case study for validation—may understate the degree of labor market power. Our alternative model, which features monopsonistic competition with a more elastic market-level labor supply, fits the observed heterogeneity better and implies a turning point around a Kaitz index of 0.62 to 0.72.

Several mechanisms not captured could help explain why employment effects remain small at high bite levels, including worker reallocation across firms, price pass-through to consumers, and productivity responses. Rao and Risch (2024) finds evidence for both price and productivity channels in the 2010–2019 period. Recovering the structural parameters that rationalize the observed heterogeneity—through indirect inference or other methods—is an important next step.

For policy, the absence of a turning point in our data suggests there is still room for further experimentation with higher minimum wage levels in U.S. states. At the same time, our model calibration implies that such a turning point is likely to emerge beyond the range observed so far, at a Kaitz index of roughly 0.62 to 0.72. This creates a familiar exploration-versus-exploitation tradeoff for policymakers: the evidence to date supports continued increases, but at sufficiently high levels disemployment effects are likely to appear. In that context, the theoretical predictions from our calibrated model—anchored in credible empirical estimates—offer a useful, if necessarily imperfect, guide for policymakers moving into less-charted territory.

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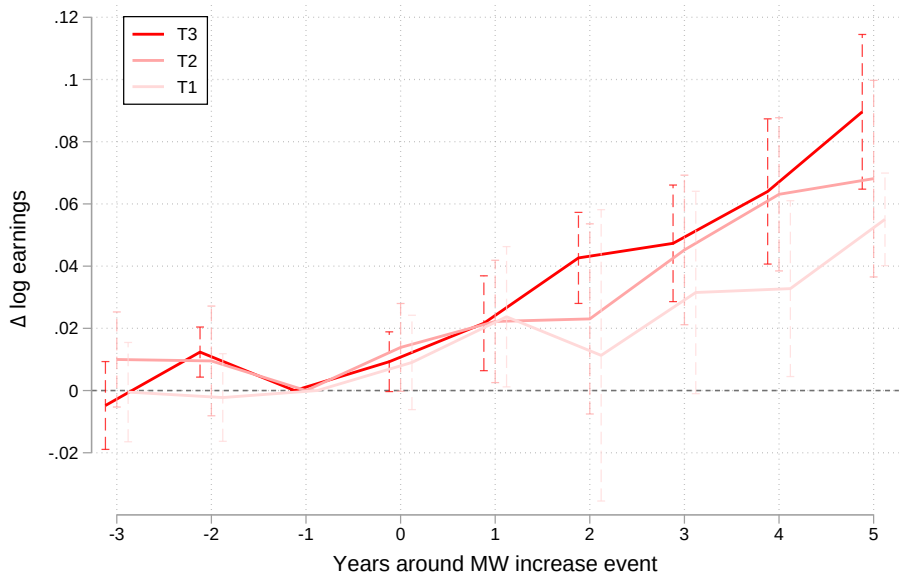
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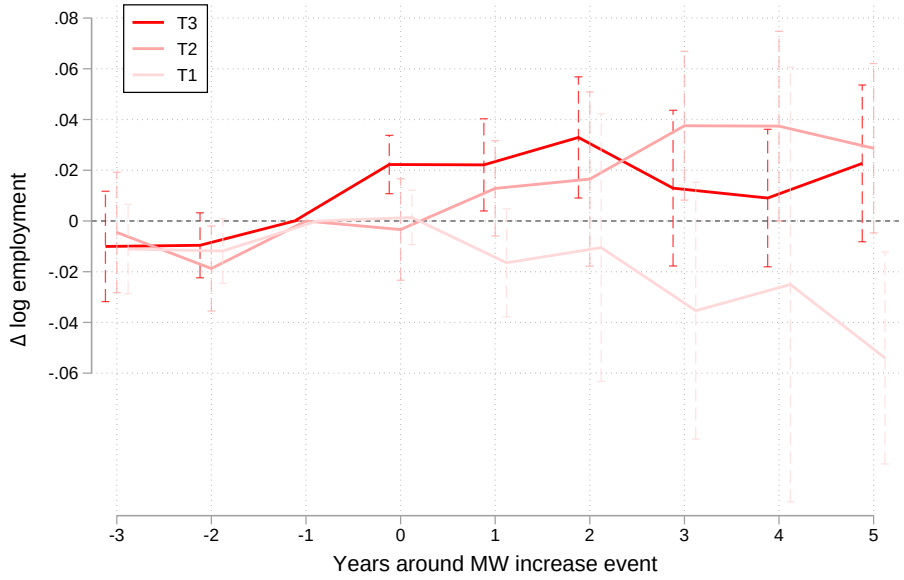
Main Figures

Figure 1: Baseline Event Study Estimates by Kaitz Index: Low-Wage Workers

(a) Wage Effects



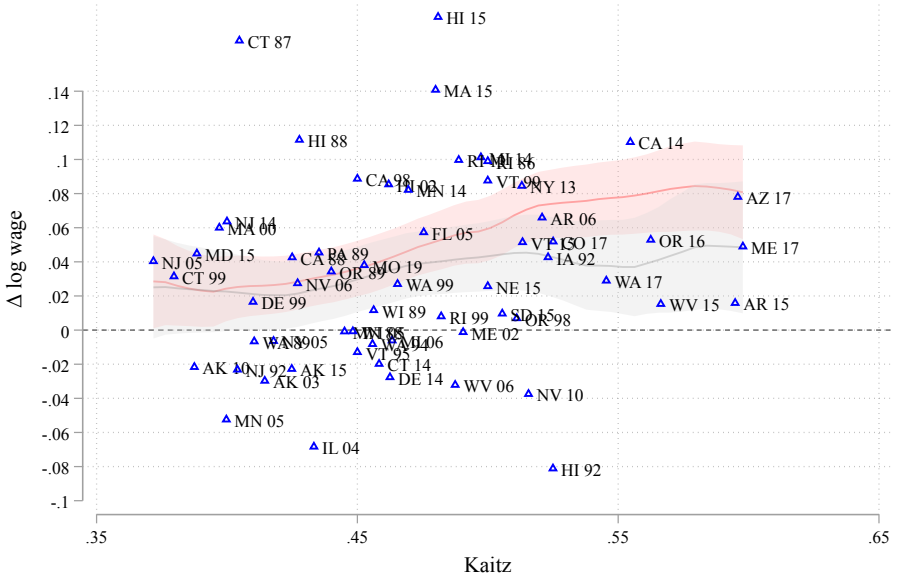
(b) Employment Effects



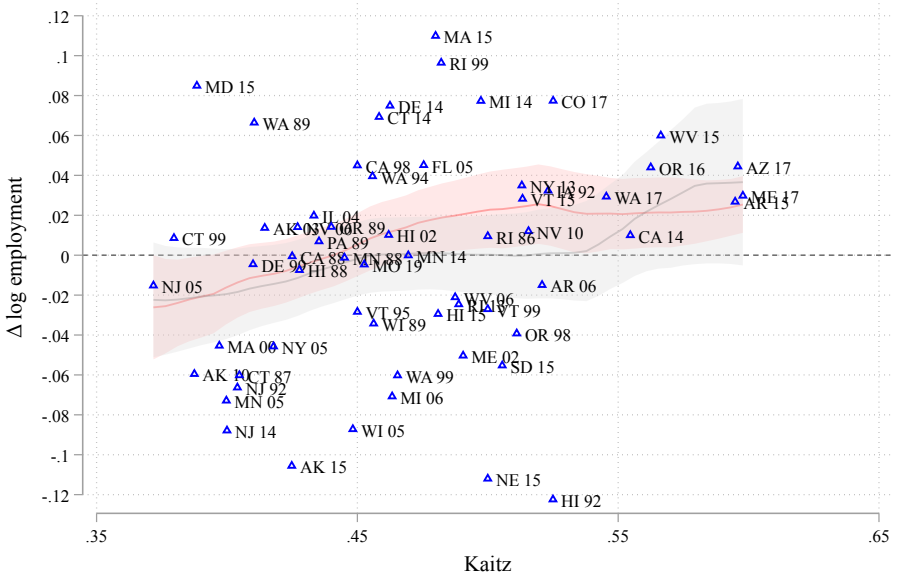
Notes: The figures plot event-study estimates separately for the bottom, middle, and top terciles (T1, T2, and T3) of state minimum wage events ranked by the Kaitz index . Panel (a) reports log wage effects and Panel (b) reports log employment effects for the broad low-wage workforce of low-wage workers. The horizontal axis shows years relative to the minimum wage increase event.

Figure 2: Heterogeneity of Effect by Kaitz Index: Low-Wage Workers

(a) Wage Effects



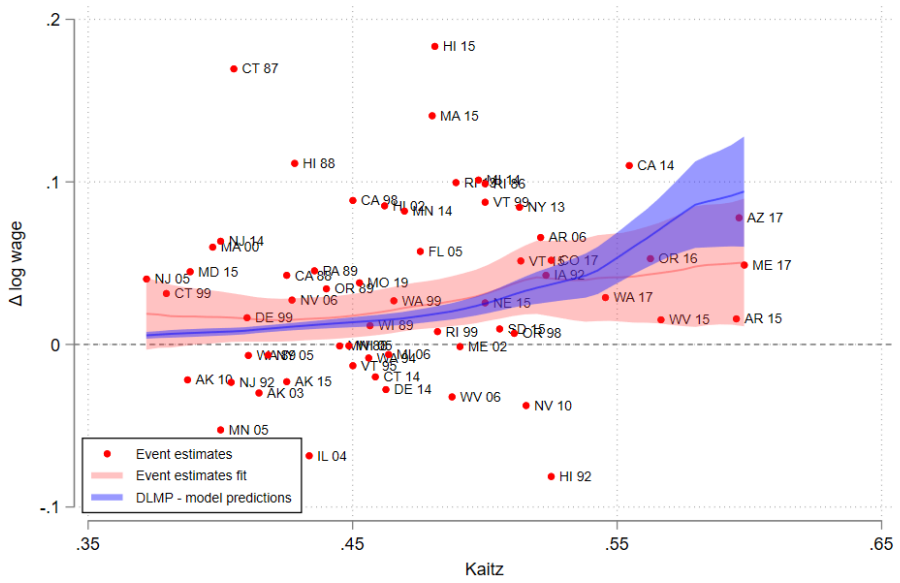
(b) Employment Effects



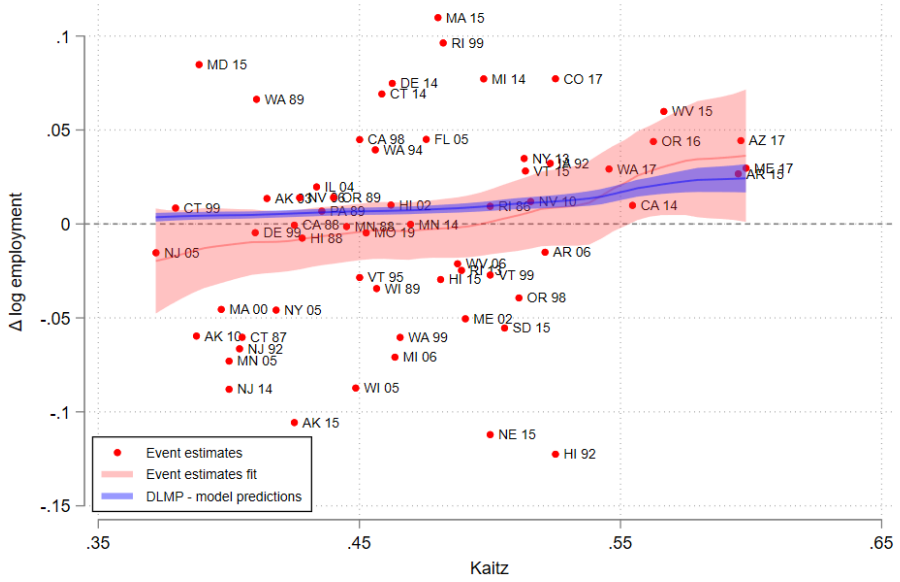
Notes: Panel (a) reports log wage effects and Panel (b) reports log employment effects for low-wage workers. Each labeled point correspond to an estimate for a given state minimum wage event, plotted against the event-level Kaitz index on the horizontal axis. The figure shows that wage effects tend to be larger in higher-Kaitz events, while employment effects do not become systematically more negative over the observed range. The red line and shaded band show a local polynomial fit weighting by population of the treated state, while the gray one shows the unweighted version.

Figure 3: Grid Search Results: Comparison to Empirical Estimates

(a) Wage Effects



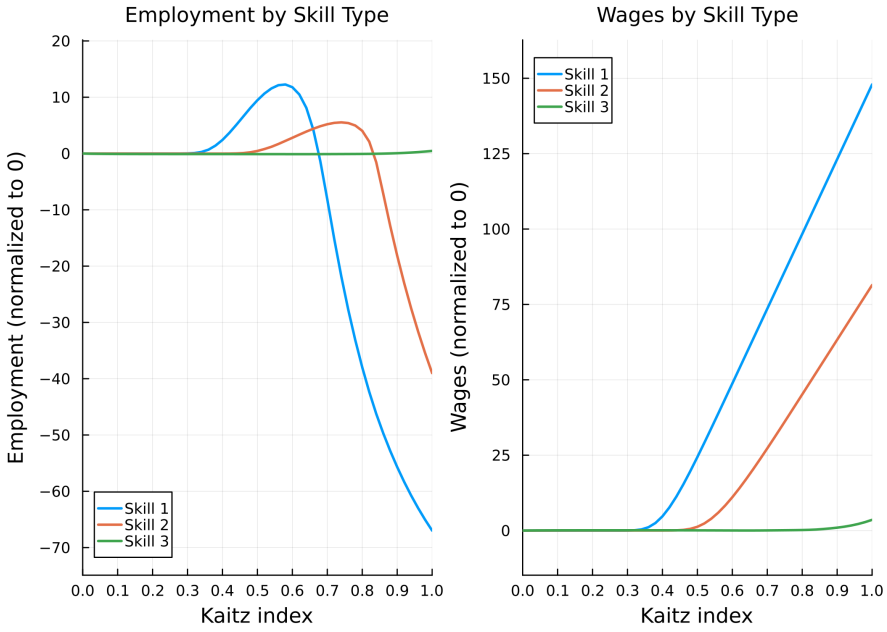
(b) Employment Effects



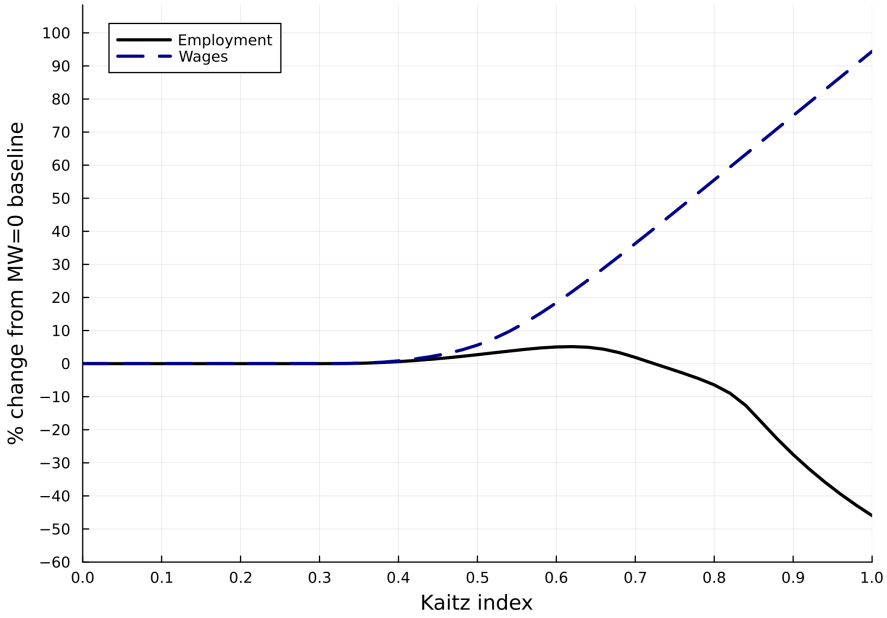
Notes: This figure plots the event-level wage and employment effects for the low-skill group from each of our 60 events, for both the empirical estimates (in red) and the predicted effects for each event from the DLMP model, captured by a local polynomial fit line and corresponding confidence interval in blue. The red line and confidence interval are a local polynomial fit line for the empirical estimates. Both fit lines are weighted by the inverse variance of the empirical estimate for that event.

Figure 4: DLMP: Employment and Wages by Kaitz Index

(a) By Skill Group



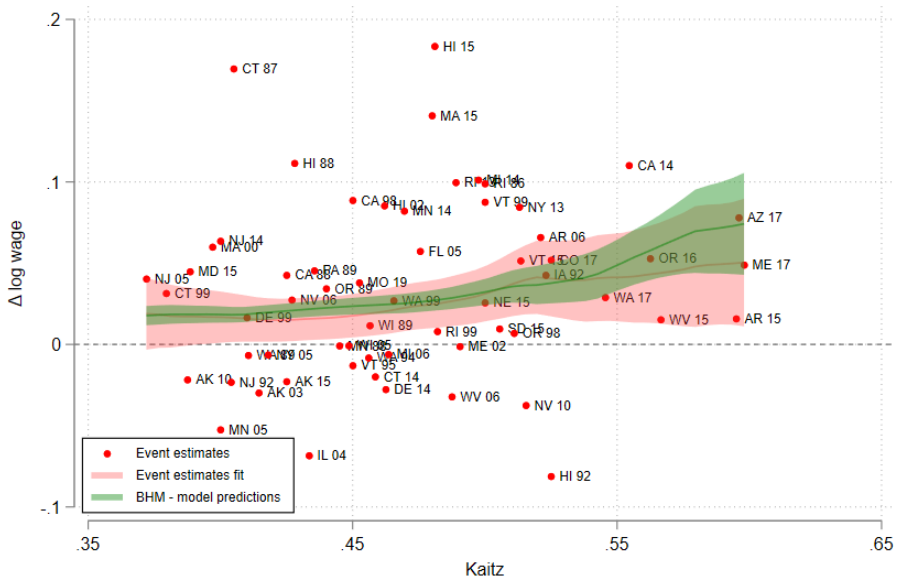
(b) Low Skill Only



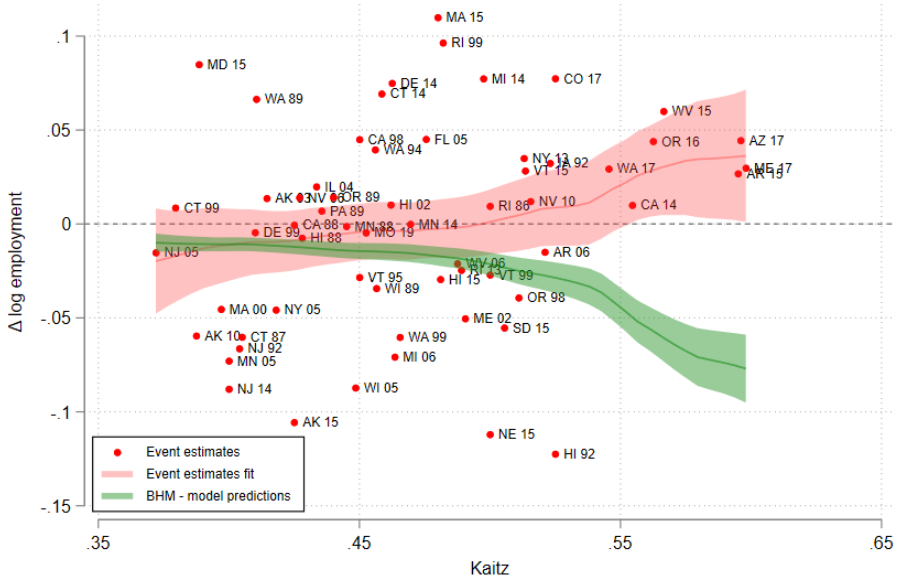
Notes: Panel (a) plots employment and wages as a function of the Kaitz index, relative to the zero minimum wage baseline in the DLMP economy, separately by worker skill level. The vertical axis represents percentage deviations from the baseline. Panel (b) plots the percentage deviations of employment and wages from the zero minimum wage baseline for low-skill (Skill 1 and Skill 2) workers only.

Figure 5: How Well Does Berger et al. (2025) Fit Evidence on Heterogeneity?

(a) Wage Effects



(b) Employment Effects



Notes: This figure plots the event-level wage and employment effects for the low-skill group from each of our 60 events, for both the empirical estimates (in red) and the predicted effects for each event from the BHM25 model, captured by a local polynomial fit line and corresponding confidence interval in green. The red line and confidence interval are a local polynomial fit line for the empirical estimates. Both fit lines are weighted by the inverse variance of the empirical estimate for that event.

Main Tables

Table 1: Impact of Minimum Wage on Low-Wage Workers, by Kaitz Tercile

	(1) Log Real MW	(2) Log Wage	(3) Log Emp.	(4) OWE
Overall	0.262***	0.046***	0.006	0.133
	(0.015)	(0.010)	(0.008)	(0.153)
T1	0.233***	0.021	-0.015	.
	(0.016)	(0.012)	(0.011)	
T2	0.246***	0.050***	0.014	0.278
	(0.017)	(0.014)	(0.015)	(0.246)
T3	0.317***	0.072***	0.024*	0.328*
	(0.032)	(0.012)	(0.011)	(0.162)
T3 - T1	0.084**	0.051***	0.039**	0.540*
	(0.033)	(0.014)	(0.015)	(0.237)

Notes: The table reports estimates for the broad low-wage workforce. Column (1) reports the effect on log real minimum wage, columns (2) and (3) report log wage and log employment effects, and column (4) reports the implied own-wage elasticity (OWE). Overall pools all 60 state minimum wage events. T1, T2, and T3 report estimates for the bottom, middle, and top terciles of events ranked by the Kaitz index; T3–T1 reports the difference between the top and bottom terciles. Standard errors are in parentheses. Mean Kaitz index by tercile: T1 = 0.41, T2 = 0.46, T3 = 0.54. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Impact of Minimum Wage by Kaitz Index: Robustness to Event Selection and Covariates

		Event Selection		Covariates	
	(1)	(2)	(3)	(4)	(5)
	Baseline	Long Events	Long Post Events	Log Income PC	All Covariates
Panel A: Overall					
Log wage	0.046***	0.067***	0.065***	0.038***	0.036***
	(0.010)	(0.014)	(0.012)	(0.007)	(0.007)
Log emp.	0.006	0.023	0.020*	0.001	-0.002
	(0.008)	(0.014)	(0.009)	(0.007)	(0.006)
OWE	0.133	0.345	0.307*	0.025	-0.051
	(0.153)	(0.213)	(0.147)	(0.171)	(0.179)
Panel B: T3					
Log wage	0.072***	0.080***	0.075***	0.052***	0.052***
	(0.012)	(0.011)	(0.012)	(0.008)	(0.008)
Log emp.	0.024*	0.029*	0.026*	0.009	0.010
	(0.011)	(0.013)	(0.012)	(0.013)	(0.011)
OWE	0.328*	0.366*	0.351*	0.183	0.186
	(0.162)	(0.174)	(0.171)	(0.252)	(0.222)
Panel C: T3 - T1					
Log wage	0.051***	0.063	0.050	0.029*	0.033*
	(0.014)	(0.038)	(0.031)	(0.014)	(0.014)
Log emp.	0.039**	0.039	0.040	0.023	0.031
	(0.015)	(0.031)	(0.024)	(0.020)	(0.018)
OWE	0.540*	.	.	0.447	0.605
	(0.237)			(0.381)	(0.348)

Notes: Panels A–C report results for low-wage workers. Panel A pools all 60 events, Panel B reports the top Kaitz tercile, and Panel C reports the Tercile 3 minus Tercile 1 contrast. Column (1) is the baseline specification; columns (2) and (3) restrict the sample to long events and long post-treatment windows; column (4) adds log income per capita; and column (5) adds all covariates. Rows report log wage effects, log employment effects, and implied OWEs. Standard errors are in parentheses. In Panel A, columns (1), (4), and (5) include 60 events, while columns (2) and (3) include 28 and 38 events, respectively. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Impact of Minimum Wage by Kaitz Index: Prediction-Based Skill Groups

	(1) Log Wage	(2) Log Emp.	(3) OWE
Panel A: Skill 1 (8%)			
Overall	0.066***	-0.007	-0.104
	(0.011)	(0.012)	(0.184)
T3	0.105***	0.042	0.397
	(0.020)	(0.023)	(0.254)
T3 - T1	0.058**	0.081*	0.776
	(0.021)	(0.033)	(0.405)
Panel B: Skill 2 (20%)			
Overall	0.035***	0.010	0.300
	(0.010)	(0.008)	(0.217)
T3	0.057***	0.016	0.274
	(0.011)	(0.010)	(0.179)
T3 - T1	0.048**	0.023	0.395
	(0.018)	(0.012)	(0.220)
Panel C: Skill 3 (72%)			
Overall	0.009	0.005*	.
	(0.006)	(0.002)	
T3	0.011	0.006	.
	(0.010)	(0.005)	
T3 - T1	0.005	-0.002	.
	(0.010)	(0.005)	

Notes: Panels A–C report estimates separately for the three prediction-based skill groups. Skill 1, Skill 2, and Skill 3 samples comprise 8, 20, and 72 percent of the workforce, respectively; the first two groups are more likely to be bound by the minimum wage, while Skill 3 serves as a placebo group. Columns (1)–(3) report the log wage effect, log employment effect, and implied own-wage elasticity (OWE). Overall pools all 60 state minimum wage events, T3 reports estimates for the top tercile of events ranked by the Kaitz index, and T3–T1 reports the difference between the top and bottom terciles. Standard errors are in parentheses. The corresponding estimates for log real minimum wage are Overall $= 0.262^{***}$ (0.015), T3 $= 0.317^{***}$ (0.032), and T3–T1 $= 0.084^{**}$ (0.033). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Impact of Minimum Wage by Alternate Measures of Minimum Wage "Bite"

	(1) Log Real MW	(2) Log Wage	(3) Log Emp.	(4) OWE
Panel A: Δ Kaitz				
T3	0.317***	0.042**	0.007	0.167
	(0.022)	(0.016)	(0.011)	(0.245)
T3 - T1	0.117***	-0.016	-0.001	.
	(0.022)	(0.014)	(0.020)	
Panel B: Δ Log Kaitz				
T3	0.319***	0.042**	0.009	0.218
	(0.021)	(0.015)	(0.011)	(0.249)
T3 - T1	0.116***	-0.022	-0.004	.
	(0.023)	(0.015)	(0.020)	
Panel C: Real MW				
T3	0.342***	0.077***	0.024	0.314
	(0.023)	(0.012)	(0.014)	(0.172)
T3 - T1	0.157***	0.057***	0.025	0.327
	(0.020)	(0.016)	(0.017)	(0.226)
Panel D: Δ Real MW				
T3	0.341***	0.059***	0.005	0.080
	(0.014)	(0.013)	(0.013)	(0.205)
T3 - T1	0.197***	0.046***	0.017	0.289
	(0.019)	(0.012)	(0.017)	(0.274)
Panel E: Δ Log Real MW				
T3	0.340***	0.058***	0.002	0.027
	(0.014)	(0.013)	(0.012)	(0.208)
T3 - T1	0.197***	0.040***	0.007	0.127
	(0.019)	(0.011)	(0.017)	(0.274)
Panel F: Share Below New Real MW				
T3	0.325***	0.082***	0.032**	0.391**
	(0.017)	(0.010)	(0.010)	(0.129)
T3 - T1	0.126***	0.071***	0.038*	0.463*
	(0.021)	(0.017)	(0.016)	(0.194)
Panel G: GAP				
T3	0.332***	0.078***	0.021	0.276*
	(0.014)	(0.010)	(0.011)	(0.131)
T3 - T1	0.137***	0.067***	0.030**	0.384**
	(0.021)	(0.012)	(0.012)	(0.140)

Notes: Panels A–G report results for low-wage workers using the alternative bite measures defined in Section 2.3; panel titles identify the ranking variable. T3 is the top tercile of the 60 state minimum wage events ranked by that measure, and T3–T1 is the top-minus-bottom tercile contrast. Column (1) reports the log real minimum wage effect, columns (2) and (3) report log wage and log employment effects, and column (4) reports the implied OWE. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Baseline Parameterization

Parameter	DLMP	BHM25
α (returns to scale)	0.55	0.94
σ_z (firm prod. dispersion)	0.2	0.31
η (between-market labor sub.)	-	0.42
θ (within-market labor sub.)	-	10.85
β_1 (labor supply elasticity, skill 1)	1.5	-
β_2 (labor supply elasticity, skill 2)	2.0	-
β_3 (labor supply elasticity, skill 3)	3.0	-
ϕ (agg. LS elasticity)	0.2	~ 0
σ (labor type sub.)	3.0	13.7
φ_1 (relative prod. of low skill)	0.211	0.285
φ_2 (relative prod. of medium skill)	0.373	0.484
MSE (in SEs)	1029.71	1751.25

Notes: This table reports the preferred parameterization for our monopsony model of the labor market (DLMP) and the model from Berger et al. (2025) (BHM25). DLMP models labor supply slightly differently to BHM25: the former does not have within- and between-market substitution parameters (θ and η), but rather skill-specific labor supply elasticities to the firm (β_s). In the final row, we report the mean squared error of the predicted event-level wage and employment effects for low-skill workers (bottom two skill groups in DLMP, bottom skill group plus 20% of the middle group in BHM25) in the full sample of our 60 events. Each squared error is weighted by the inverse variance of our empirical estimate for that event-level effect.

Table 6: Model-Implied and Empirical OWEs (Low Skill)

Kaitz change	Empirical estimates	DLMP	BHM25
.30 $\rightarrow$ .35	-	0.933	-0.545
.37 $\rightarrow$ .41 (T1)	-	0.651	-0.633
.42 $\rightarrow$ .46 (T2)	0.278	0.491	-0.719
.48 $\rightarrow$ .54 (T3)	0.328	0.292	-0.880
.55 $\rightarrow$ .60	-	0.147	-1.033
.60 $\rightarrow$ .65	-	-0.039	-1.178
.65 $\rightarrow$ .70	-	-0.368	-1.363

Notes: This table reports the predicted own wage elasticities among the low-skill group for 7 simulated minimum wage changes for the DLMP and BHM25 models (final two columns), along with our empirical estimates of the OWE for the two representative upper tercile minimum wage changes (T2 and T3). For the representative change in the bottom tercile (T1), because we do not find a statistically significant wage effect for this group of workers, we do not report an OWE estimate.

Table 7: How Sensitive Are Berger et al. (2025)'s Results to Their Calibration?

	Parameter values		$7.69 → $9.06 (T2)	$8.57 → $10.91 (T3)
Empirical estimates	-		0.278	0.328
			(0.246)	(0.162)
	η	θ		
BHM25 baseline	0.42	10.85	-0.719	-0.880
Alternative 1	0.42	0.42	0.122	0.056
Alternative 2	1.0	1.0	0.023	-0.173
Alternative 3	2.0	2.0	-0.441	-0.623
BHM25 alt.	3.02	3.02	-0.701	-0.847

Notes: This table compares the own wage elasticity predicted by the BHM25 model for two representative minimum wage changes in the upper two terciles of our event sample to our empirical estimates of the OWEs in the upper terciles, for various choices of the within- and between-market substitution parameters that govern the degree of monopsony power possessed by firms in the model (θ and η, respectively). The top panel reports our empirical estimates of these OWEs, with standard errors in parentheses below. The bottom panel reports the simulated OWE for 5 different parameterizations of the BHM25 model: "BHM25 baseline" is their preferred specification, Alternatives 1 through 3 impose monopsony (instead of oligopsony) by setting $\eta = \theta$ for different values of the labor supply elasticity to the firm, and "BHM25 alt." is their preferred monopsony calibration of their model, with a labor supply elasticity to the firm of 3.02.

Table 8: Decomposition of Minimum Wage Effects: BHM25 to DLMP

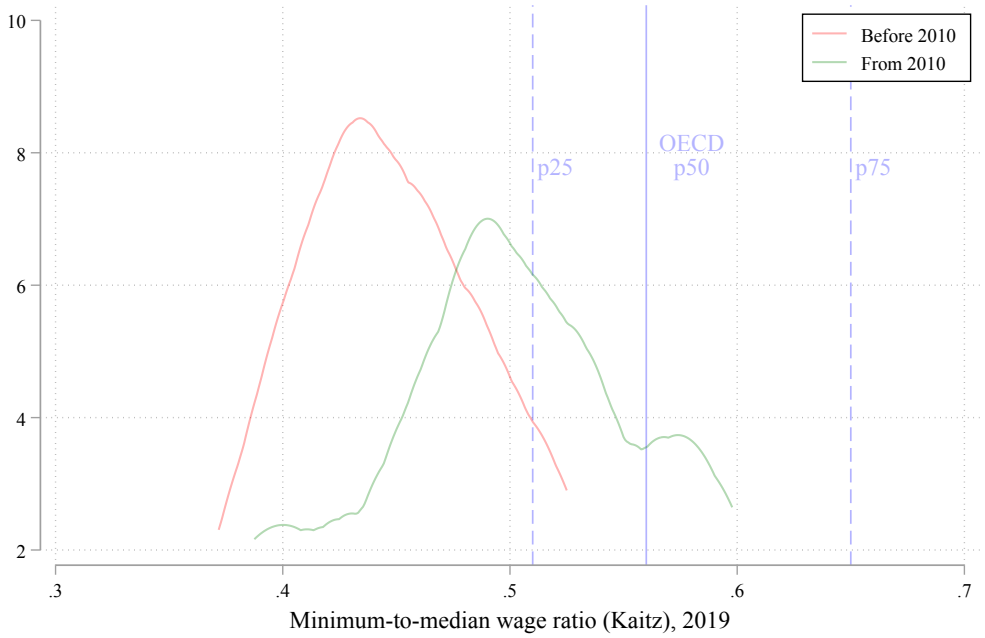
	BHM25 olig. GE	BHM25 monop. GE	BHM25 monop. PE	Higher ϕ	Nested Logit	DLMP W index	Calibrate earnings	Lower σ	Lower α	Lower σ_z	Het. β
Employment	-0.041	-0.044	-0.035	-0.038	-0.052	-0.017	-0.015	-0.011	0.018	0.023	0.016
Wages	0.047	0.052	0.043	0.044	0.052	0.043	0.044	0.040	0.036	0.036	0.054
OWE	-0.88	-0.85	-0.81	-0.85	-1.00	-0.40	-0.35	-0.26	0.48	0.65	0.29

Notes: All effects calculated for HS or less workers using a simulated MW change from a Kaitz of 0.48 to 0.54. Model features are changed one by one across the columns. BHM25 olig. GE: GE (representative agent) oligopsony version of BHM25. BHM25 monop. GE: GE (representative agent) monopsony version of BHM25. BHM25 monop. PE: PE version of previous model. Higher ϕ: aggregate labor supply elasticity raised to 0.62. Nested logit: outer participation decision via a nested logit with baseline aggregate LS elasticity=0.62, calibrated using probability groups as the measure of worker skill. DLMP W index: aggregate wage index changed to DLMP-implied index. Calibrate earnings: calibrating skill-specific productivities to relative wages and outside options to baseline employment rates. Lower σ: CES production function with $\sigma = 3.0$. Lower α: returns to scale reduced to $\alpha = 0.55$. Lower σ_z: firm productivity dispersion decreased to $\sigma_z = 0.2$. Het. β: β_1 set to 1.5, β_2 to 2.0, and aggregate LS elasticity ϕ set to 0.2.

Appendix A Additional Figures and Tables

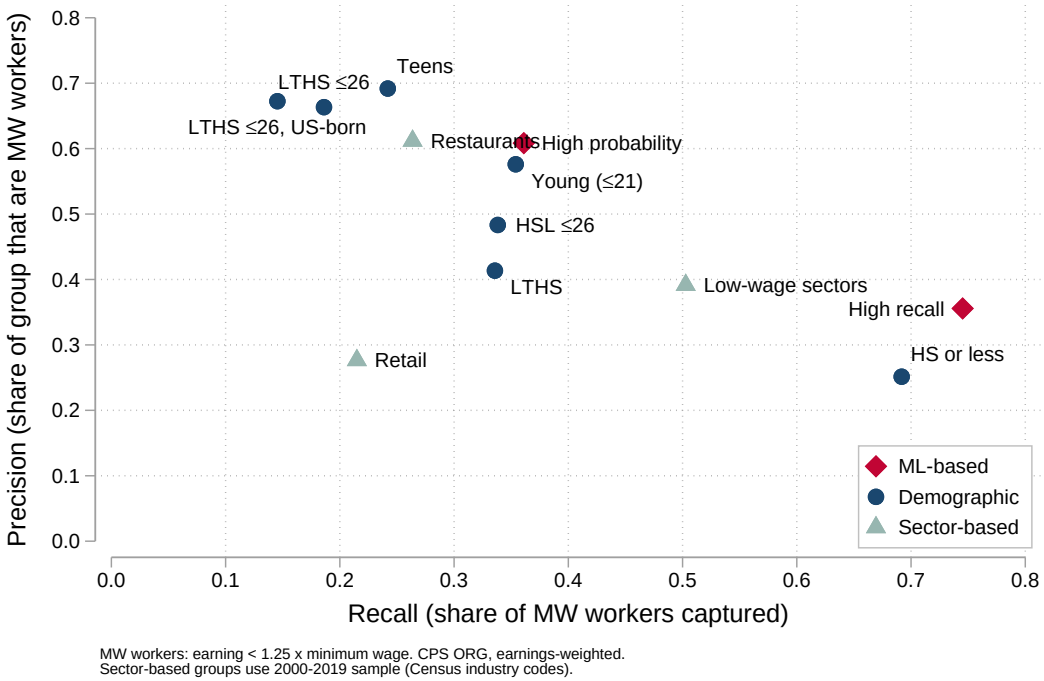
Figures

Figure A1: Distribution of the Minimum Wage Level Relative to Median Wage (Kaitz Index)



Notes: Figure shows the distribution of the Kaitz index, defined as the minimum wage relative to the state median wage, across the 60 state minimum wage events in our sample. The two histograms distinguish events before 2010 from events in 2010 or later; the latter are concentrated at higher Kaitz values. The vertical blue lines mark the 25th, 50th, and 75th percentiles of the distribution of Kaitz indices across OECD countries.

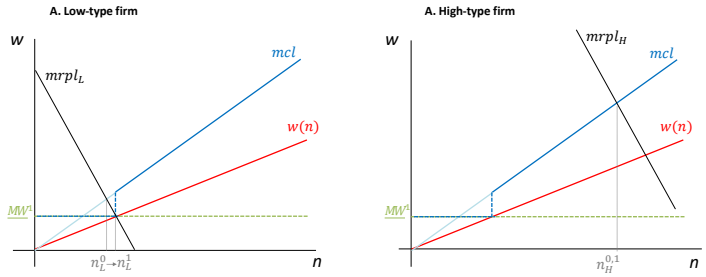
Figure A2: Precision and Recall of Worker Group Definitions



Notes: Each point represents a worker group used in the analysis. Precision is the share of group members who earn less than 1.25 times the minimum wage; recall is the share of all such workers captured by the group. ML-based groups (diamonds) are defined by predicted probability cutoffs from a gradient boosted model trained on demographic characteristics; demographic groups (circles) use standard age and education criteria. Both classify *workers* by individual characteristics. Sector-based groups (triangles) classify *jobs* by industry of employment, so precision and recall are not directly comparable to the person-based definitions. Computed from CPS ORG, earnings-weighted. Sector-based groups use the 2000–2019 sample with Census 2002 industry codes.

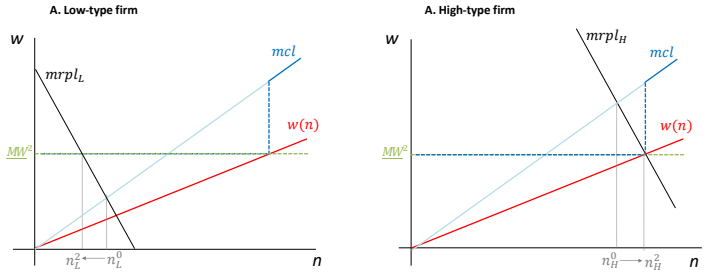
Figure A3: Employment Effects in a Model of Monopsonistic Competition

(a) Low minimum wage



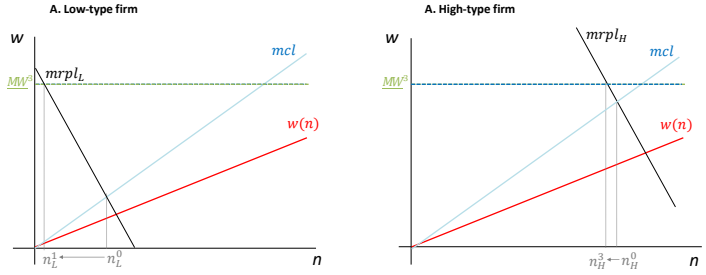
Low Minimum Wage (1)

(b) Intermediate minimum wage



Medium Minimum Wage (2)

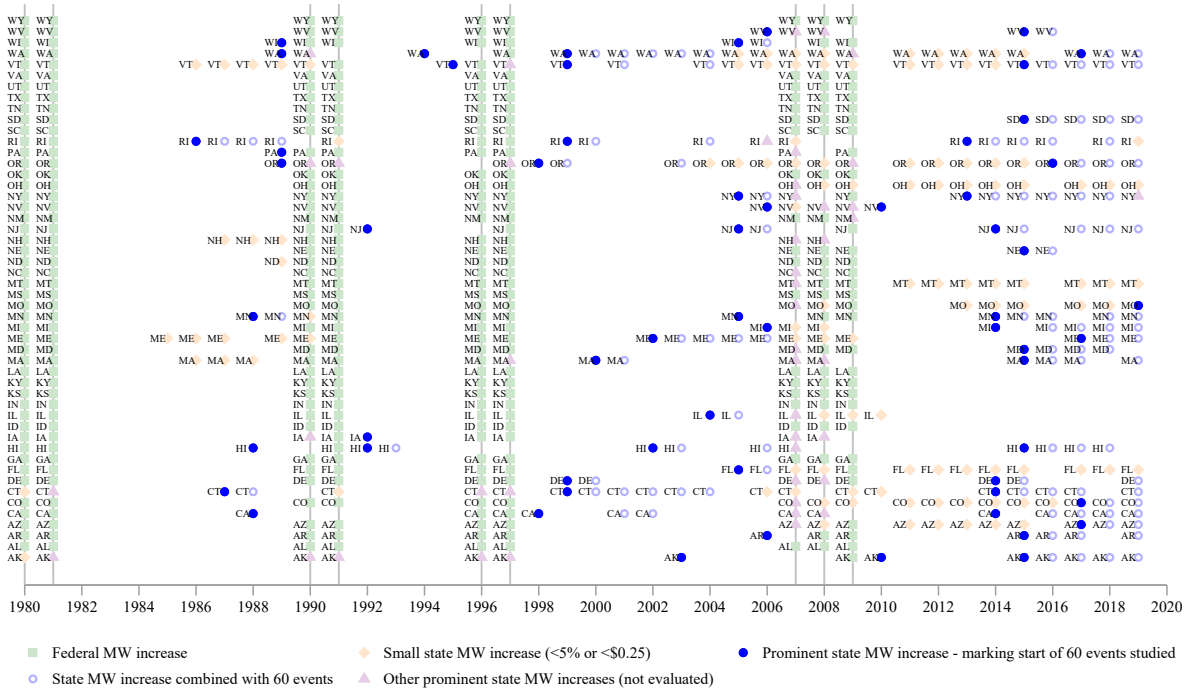
(c) High minimum wage



High Minimum Wage (3)

Notes: Low MW ($\underline{w}^1$) only binding for L: positive employment effect at L, and $\underline{w}^1$ not binding at H. Medium MW ($\underline{w}^2$) binding for both L, H: negative employment-effect at L, but positive at H, with reallocation. High enough MW ($\underline{w}^3$) leads to more negative employment effects, as $\underline{w}^3 > w_c$ in both L, H.

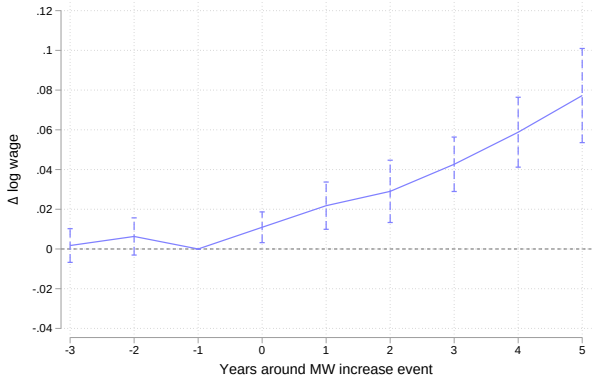
Figure A4: Minimum Wage Variation: Waves of State Minimum Wage Hikes



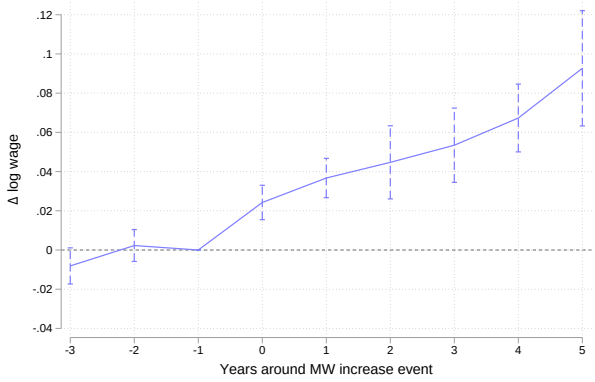
Notes: Figure shows the timing of minimum wage increases by state over 1979-2019. Each row corresponds to a state and each column to a year. The figure distinguishes federal minimum wage increases, small state increases (less than 5 percent or less than $0.25), other prominent state minimum wage increases, and the prominent state increases that mark the beginning of the 60 combined events analyzed in the paper.

Figure A5: Baseline Event Study Estimates on Wage and Employment

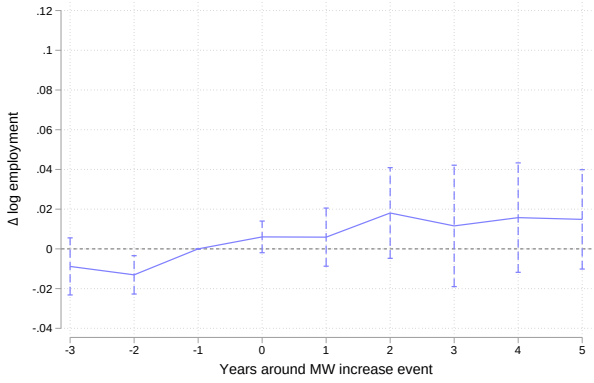
(a) Low-Wage Workers' Wage Change



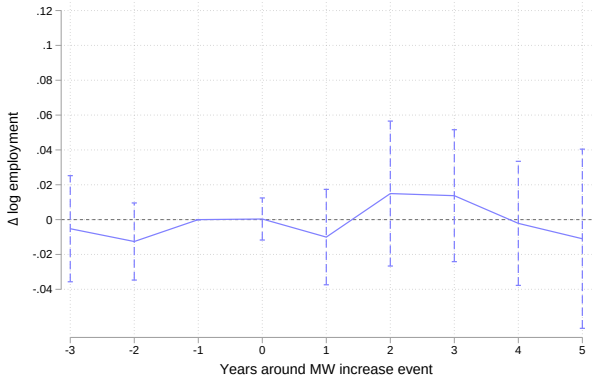
(b) Skill 1 Workers' Wage Change



(c) Low-Wage Workers' Employment Change



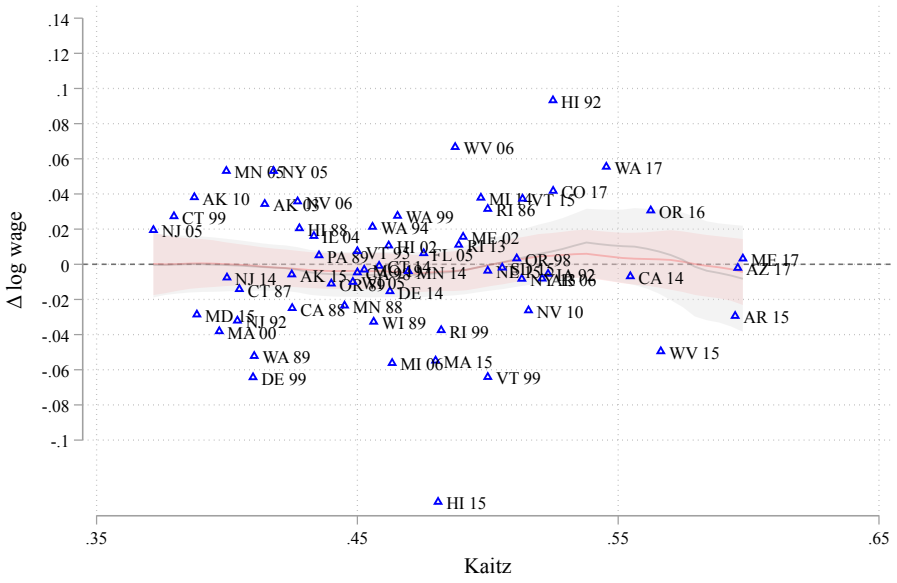
(d) Skill 1 Workers' Employment Change



Notes: Panels A and B report baseline event-study estimates of log wage effects for the broad low-wage workforce of low-wage workers and the narrower Skill 1 subgroup, respectively. Panels C and D report the corresponding log employment-rate effects for the same two groups. All Panels leverage the 60 state level minimum wage events described in the main text.

Figure A6: Heterogeneity of Effect by Kaitz Index: Low-Wage Workers – Pre-Reform ($t-1$ – $t-3$)

(a) Wage Effects



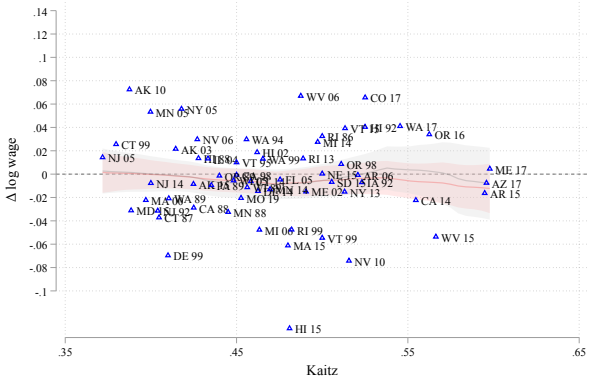
(b) Employment Effects



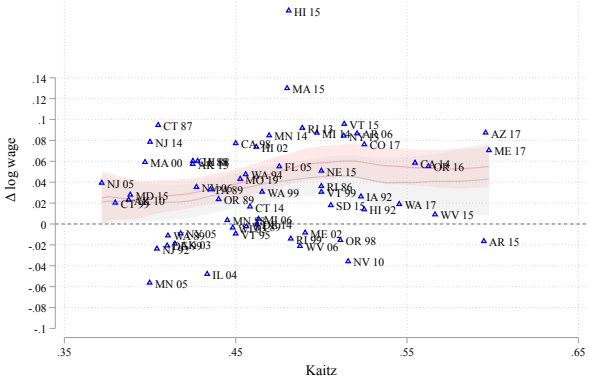
Notes: Panel A reports pre-reform log wage estimates over the $t-3$ to $t-1$ window and Panel B reports pre-reform log employment estimates over the same window for the low-wage workforce. Each labeled point is a state minimum wage event, plotted against the event-level Kaitz index on the horizontal axis. The red line and shaded band show a local polynomial fit weighting by population of the treated state, while the gray one shows the unweighted version.

Figure A7: Heterogeneity of Effect by Kaitz Index: Low-Wage Workers – All Covariates

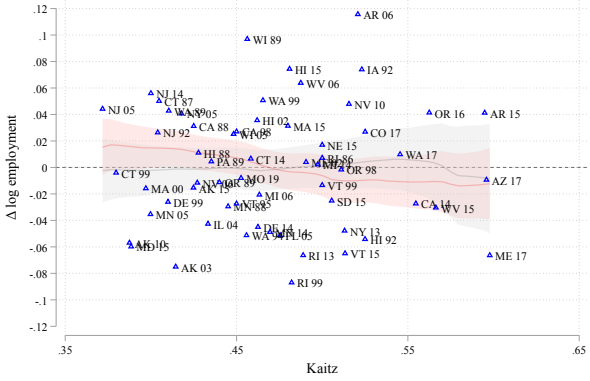
(a) Wage: Pre-Reform ($t-1$ – $t-3$)



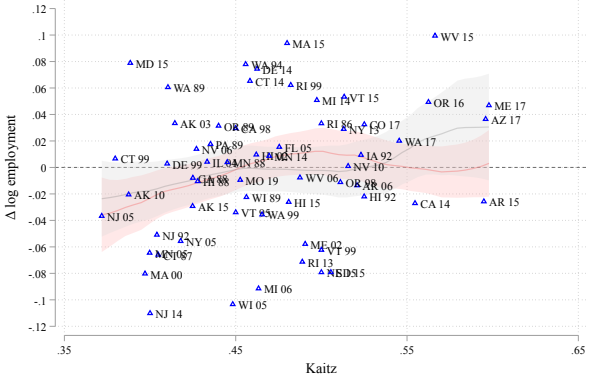
(b) Wage Effects



(c) Employment: Pre-Reform ($t-1$ – $t-3$)



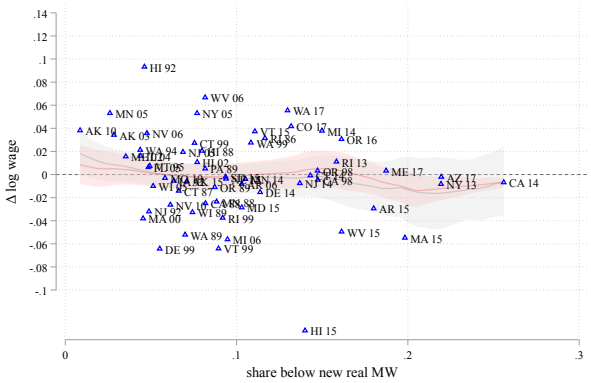
(d) Employment Effects



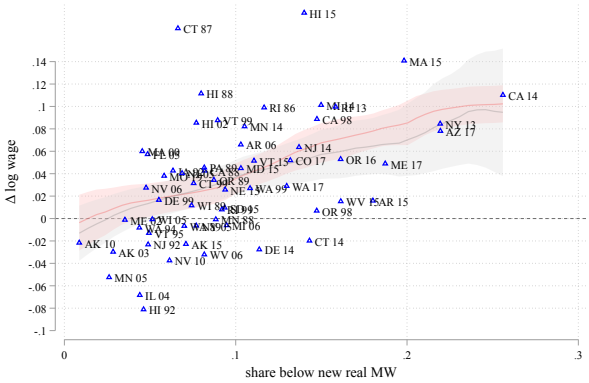
Notes: Panel A reports pre-reform log wage estimates over the $t-3$ to $t-1$ window, Panel B reports log wage effects, Panel C reports pre-reform log employment estimates over the same window, and Panel D reports log employment effects for the low-wage workforce. Each labeled point is a state minimum wage event, plotted against the event-level Kaitz index on the horizontal axis. The red line and shaded band show a local polynomial fit weighting by population of the treated state, while the gray one shows the unweighted version. These figures are obtained from the all-covariates specification.

Figure A8: Heterogeneity of Effect by Share Below New Real Minimum Wage: Low-Wage Workers

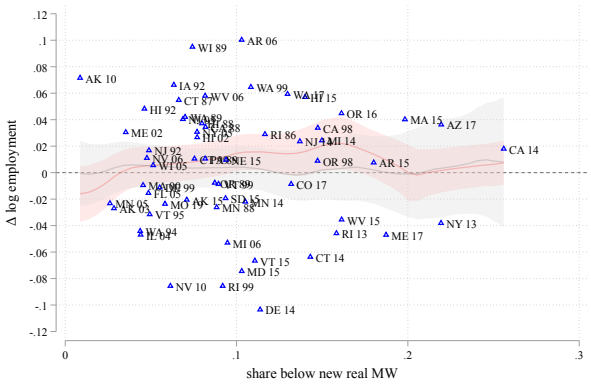
(a) Wage: Pre-Reform ($t-1$ – $t-3$)



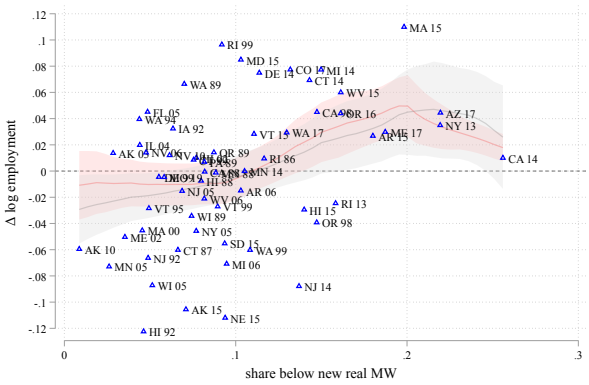
(b) Wage Effects



(c) Employment: Pre-Reform ($t-1$ – $t-3$)



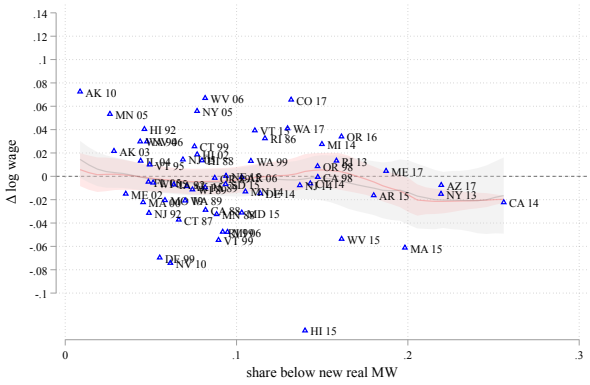
(d) Employment Effects



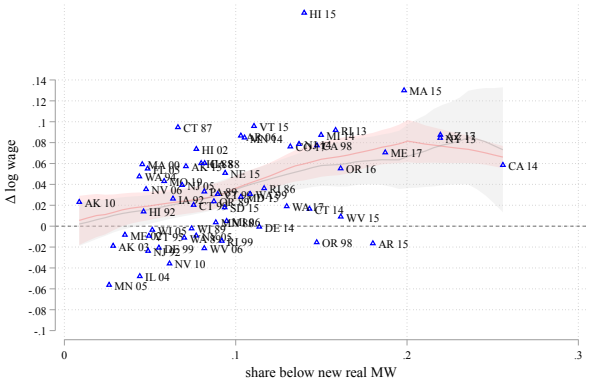
Notes: Panel A reports pre-reform log wage estimates over the $t-3$ to $t-1$ window, Panel B reports log wage effects, Panel C reports pre-reform log employment estimates over the same window, and Panel D reports log employment effects for the low-wage workforce. Each labeled point is a state minimum wage event, plotted against the share of workers below the new real minimum wage in the pre-reform period. The red line and shaded band show a local polynomial fit weighting by population of the treated state, while the gray one shows the unweighted version.

Figure A9: Heterogeneity of Effect by Share Below New Real Minimum Wage: Low-Wage Workers – All Covariates

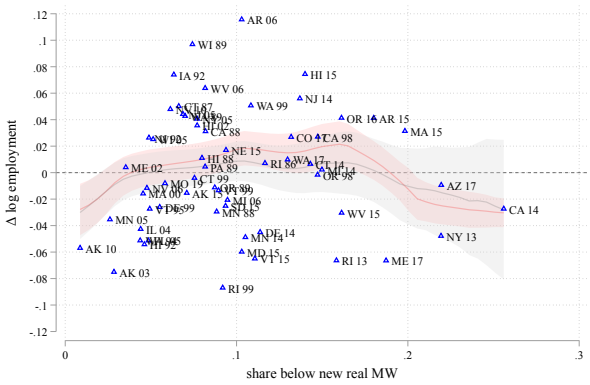
(a) Wage: Pre-Reform ($t-1$ – $t-3$)



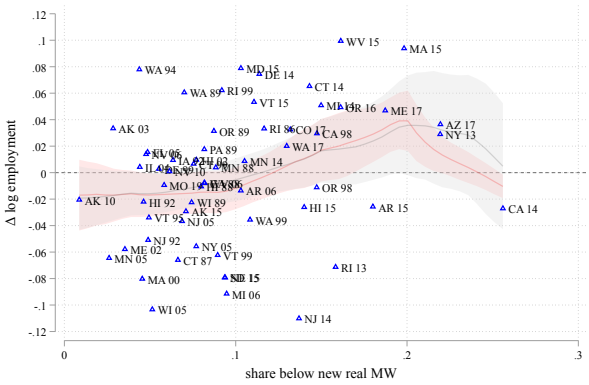
(b) Wage Effects



(c) Employment: Pre-Reform ($t-1$ – $t-3$)



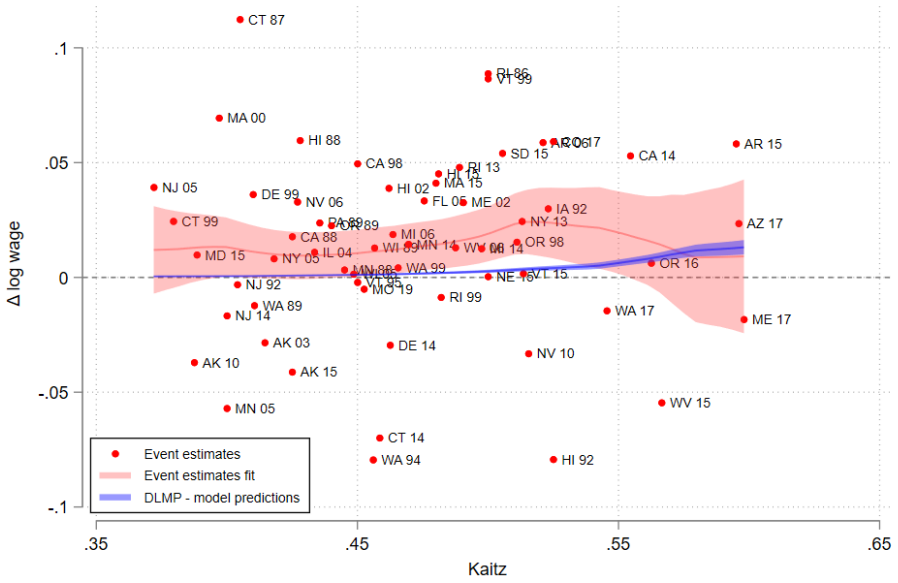
(d) Employment Effects



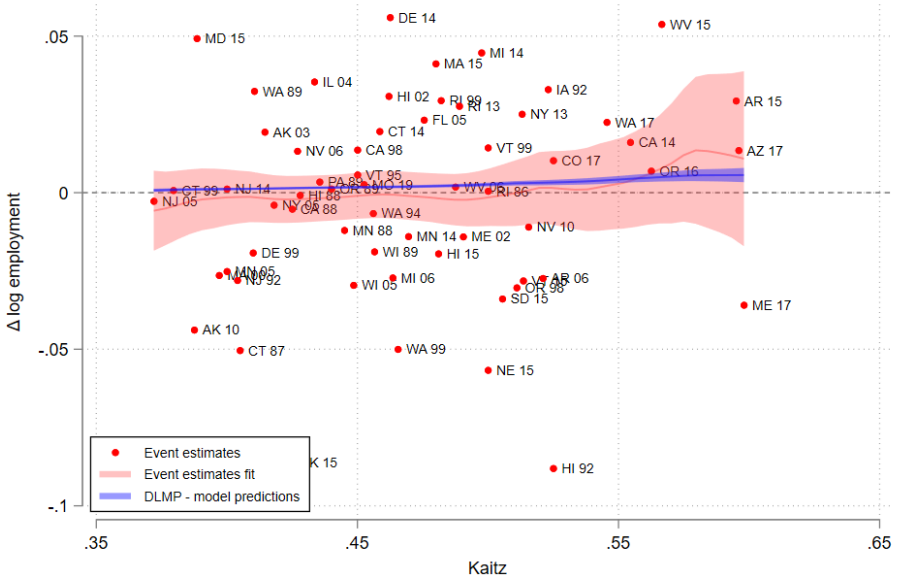
Notes: Panel A reports pre-reform log wage estimates over the $t-3$ to $t-1$ window, Panel B reports log wage effects, Panel C reports pre-reform log employment estimates over the same window, and Panel D reports log employment effects for the low-wage workforce. Each labeled point is a state minimum wage event, plotted against the share of workers below the new real minimum wage in the pre-reform period. The red line and shaded band show a local polynomial fit weighting by population of the treated state, while the gray one shows the unweighted version. These figures are obtained from the all-covariates specification.

Figure A10: DLMP Comparison to Empirical Estimates: Aggregate Results

(a) Wage Effects



(b) Employment Effects



Notes: This figure plots the event-level aggregate wage and employment effects from each of our 60 events, for both the empirical estimates (in red) and the predicted effects for each event from the DLMP model, captured by a local polynomial fit line and corresponding confidence interval in blue. The red line and confidence interval are a local polynomial fit line for the empirical estimates. Both fit lines are weighted by the inverse variance of the empirical estimate for that event.

Tables

Table A1: Baseline Estimates: Alternative Groups

	(1) High recall	(2) High prob.	(3) Teen	(4) HS or less	(5) Restaurant	(6) Low-wage sectors	(7) All	(8) Low recall
Log wages	0.046***	0.066***	0.086***	0.030**	0.045***	0.029***	0.020**	0.009
	(0.010)	(0.011)	(0.014)	(0.011)	(0.009)	(0.006)	(0.006)	(0.006)
Log Employment	0.006	-0.007	0.003	0.009	-0.003	0.012	0.005	0.005*
	(0.008)	(0.012)	(0.016)	(0.006)	(0.006)	(0.009)	(0.003)	(0.002)
OWE	0.133	-0.107	0.041	0.312*	-0.071	0.424	0.243	0.575
	(0.153)	(0.188)	(0.188)	(0.155)	(0.144)	(0.281)	(0.144)	(0.331)

Notes: Table reports pooled estimates across all 60 state minimum wage events for alternative worker-group definitions. Columns (1)–(8) correspond to the low-wage workers, the Skill 1 workers, teens, workers with a high school diploma or less, restaurant workers, the low-wage sectors aggregate, the full workforce, and the Skill 3 workers, respectively. Rows report log wage effects, log employment effects, and implied own-wage elasticities (OWE). Estimates use the baseline specification. Worker-based groups are constructed from CPS data; restaurant and low-wage sectors estimates use QCEW data, with low-wage sectors combining restaurants, retail trade, and accommodation. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A2: Impact of Minimum Wage by Kaitz Index: Education-Based Skill Groups

	(1) Log Wage	(2) Log Emp.	(3) OWE
Panel A: LTHS (13%)			
Overall	0.046***	-0.014	-0.309
	(0.012)	(0.013)	(0.281)
T3	0.076***	-0.013	-0.175
	(0.015)	(0.031)	(0.399)
T3 - T1	0.049**	0.014	0.186
	(0.015)	(0.039)	(0.537)
Panel B: HS (54%)			
Overall	0.018**	0.011*	0.627**
	(0.005)	(0.004)	(0.215)
T3	0.030***	0.020**	0.678**
	(0.007)	(0.007)	(0.221)
T3 - T1	0.027**	0.014	0.466*
	(0.010)	(0.007)	(0.233)
Panel C: COL (33%)			
Overall	0.014	0.004	.
	(0.009)	(0.003)	
T3	0.019	0.008	.
	(0.016)	(0.007)	
T3 - T1	0.008	0.006	.
	(0.014)	(0.008)	

Notes: Panels A–C report estimates separately for the three education-based skill groups. LTHS workers have less than a high school diploma, HS workers have a high school diploma or some college, and COL workers have a college degree or higher. LTHS, HS, and COL comprise 13, 54, and 33 percent of the workforce, respectively; the first two groups form the non-college workforce. Columns (1)–(3) report the log wage effect, log employment effect, and implied own-wage elasticity (OWE). Overall pools all 60 state minimum wage events, T3 reports estimates for the top tercile of events ranked by the Kaitz index, and T3–T1 reports the difference between the top and bottom terciles. Standard errors are in parentheses. The corresponding estimates for log real minimum wage are Overall $= 0.262^{***}$ (0.015), T3 $= 0.317^{***}$ (0.032), and T3–T1 $= 0.084^{**}$ (0.033). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Impact of Minimum Wage by Share Below New Real Minimum Wage

	(1) Log Real Min. Wage	(2) Log Wage	(3) Log Emp.	(4) OWE
Overall	0.262***	0.045***	0.006	0.139
	(0.015)	(0.010)	(0.008)	(0.153)
T1	0.199***	0.011	-0.006	.
	(0.015)	(0.015)	(0.014)	
T2	0.234***	0.027**	-0.016	-0.582
	(0.019)	(0.009)	(0.010)	(0.517)
T3	0.325***	0.082***	0.032**	0.391**
	(0.017)	(0.010)	(0.010)	(0.129)
T3 - T1	0.126***	0.071***	0.038*	0.463*
	(0.021)	(0.017)	(0.016)	(0.194)

Notes: Table reports estimates for the low-wage workforce, with events ranked by the share of workers below the new real minimum wage in the pre-reform period. Column (1) reports the effect on log real minimum wage, columns (2) and (3) report log wage and log employment effects, and column (4) reports the implied own-wage elasticity (OWE). Overall pools all 60 state minimum wage events. T1, T2, and T3 report estimates for the bottom, middle, and top terciles of events ranked by the share-below measure; T3–T1 reports the difference between the top and bottom terciles. This is the baseline specification. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A4: Impact of Minimum Wage by Kaitz Index: Pre-Trends of Low-Wage and Skill 3 Workers

	Low-Wage		Skill 3	
	(1)	(2)	(3)	(4)
	Log Wage	Log Employment	Log Wage	Log Employment
Panel A: Pre-trends, No Controls				
Overall	-0.002	0.009	-0.007	0.004
	(0.004)	(0.007)	(0.004)	(0.002)
T1	0.001	0.011	-0.007	0.000
	(0.008)	(0.009)	(0.005)	(0.003)
T2	-0.010	0.005	-0.002	0.007*
	(0.007)	(0.012)	(0.007)	(0.003)
T3	0.005	0.010	-0.012	0.004
	(0.007)	(0.011)	(0.007)	(0.004)
Panel B: Pre-trends, With Controls				
Overall	-0.001	0.009	-0.006	0.004
	(0.005)	(0.006)	(0.003)	(0.002)
T1	0.002	0.013	-0.006	0.000
	(0.008)	(0.009)	(0.004)	(0.003)
T2	-0.008	0.008	0.000	0.008*
	(0.006)	(0.011)	(0.006)	(0.003)
T3	0.002	0.006	-0.014*	0.003
	(0.007)	(0.009)	(0.007)	(0.003)

Notes: Table reports pre-trend estimates by terciles of the Kaitz index for the low-wage workforce and for SKill 3 workers, who serve as a placebo group. Columns (1) and (2) report log wage and log employment pre-trends for low-wage workers; columns (3) and (4) report the corresponding estimates for Skill 3 workers. Overall pools all 60 state minimum wage events, while T1, T2, and T3 report the bottom, middle, and top terciles of events ranked by the Kaitz index. Panel A reports estimates without controls, and Panel B reports the corresponding specification with controls. The outcomes correspond to differences over the pre-reform window between $t-1$ and $t-3$. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5: Impact of Minimum Wage by Alternate Measures of Minimum Wage "Bite": Low-Wage Workers – All Covariates

	(1) Log Real Min. Wage	(2) Log Wage	(3) Log Empl.	(4) OWE
Panel A: Δ Kaitz				
T3	0.315***	0.030**	-0.003	-0.114
	(0.020)	(0.010)	(0.010)	(0.335)
T3 - T1	0.116***	-0.013	-0.003	.
	(0.022)	(0.012)	(0.016)	
Panel B: Δ Log Kaitz				
T3	0.317***	0.030**	-0.002	-0.055
	(0.020)	(0.010)	(0.010)	(0.336)
T3 - T1	0.116***	-0.018	-0.004	.
	(0.023)	(0.012)	(0.016)	
Panel C: Real MW				
T3	0.341***	0.062***	0.011	0.173
	(0.022)	(0.009)	(0.013)	(0.207)
T3 - T1	0.156***	0.046**	0.016	0.258
	(0.020)	(0.017)	(0.018)	(0.278)
Panel D: Δ Real MW				
T3	0.340***	0.046***	-0.006	-0.126
	(0.013)	(0.008)	(0.011)	(0.244)
T3 - T1	0.196***	0.034***	0.005	0.099
	(0.018)	(0.010)	(0.016)	(0.341)
Panel E: Δ Log Real MW				
T3	0.338***	0.045***	-0.009	-0.197
	(0.013)	(0.008)	(0.011)	(0.244)
T3 - T1	0.196***	0.030**	-0.004	-0.084
	(0.018)	(0.010)	(0.015)	(0.336)
Panel F: Share Below New Real MW				
T3	0.323***	0.063***	0.017	0.270
	(0.016)	(0.007)	(0.009)	(0.156)
T3 - T1	0.125***	0.057***	0.027	0.420
	(0.020)	(0.015)	(0.015)	(0.226)
Panel G: GAP				
T3	0.330***	0.060***	0.008	0.135
	(0.014)	(0.006)	(0.010)	(0.162)
T3 - T1	0.135***	0.047**	0.016	0.277
	(0.021)	(0.014)	(0.011)	(0.182)

Notes: Panels A–G report results for low-wage workers using the alternative bite measures defined in Section 2.3; panel titles identify the ranking variable. T3 is the top tercile of the 60 state minimum wage events ranked by that measure, and T3–T1 is the top-minus-bottom tercile contrast. Column (1) reports the log real minimum wage effect, columns (2) and (3) report log wage and log employment effects, and column (4) reports the implied OWE. GAP denotes the share-below measure times the average log wage gap to the new minimum. Estimates use the all-covariates specification, adding the change in log employment for Skill 3 workers, the change in log wages for Skill 3 workers, and the change in log income per capita. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6: Impact of Minimum Wage by Alternate Measures of Minimum Wage "Bite": Skill 1 Workers, Baseline and All Covariates

	Baseline				All Covariates			
	(1) Log Real Min. Wage	(2) Log Wage	(3) Log Empl.	(4) OWE	(5) Log Real Min. Wage	(6) Log Wage	(7) Log Empl.	(8) OWE
Panel A: Δ Kaitz								
T3	0.317***	0.074***	0.002	0.025	0.315***	0.060***	-0.014	-0.227
	(0.022)	(0.019)	(0.012)	(0.158)	(0.020)	(0.013)	(0.014)	(0.220)
T3 - T1	0.117***	0.008	0.012	.	0.116***	0.009	0.010	.
	(0.022)	(0.020)	(0.028)		(0.022)	(0.017)	(0.024)	
Panel B: Δ Log Kaitz								
T3	0.319***	0.073***	0.004	0.060	0.317***	0.060***	-0.012	-0.199
	(0.021)	(0.018)	(0.013)	(0.175)	(0.020)	(0.012)	(0.015)	(0.245)
T3 - T1	0.116***	-0.006	0.008	.	0.116***	-0.004	0.009	.
	(0.023)	(0.022)	(0.030)		(0.023)	(0.018)	(0.025)	
Panel C: Real MW								
T3	0.342***	0.104***	0.012	0.114	0.341***	0.087***	-0.012	-0.137
	(0.023)	(0.018)	(0.028)	(0.274)	(0.022)	(0.014)	(0.029)	(0.332)
T3 - T1	0.157***	0.072***	0.022	0.212	0.156***	0.059***	0.001	0.010
	(0.020)	(0.016)	(0.034)	(0.332)	(0.020)	(0.018)	(0.036)	(0.416)
Panel D: Δ Real MW								
T3	0.341***	0.090***	-0.017	-0.193	0.340***	0.075***	-0.037	-0.494
	(0.014)	(0.017)	(0.021)	(0.245)	(0.013)	(0.012)	(0.019)	(0.280)
T3 - T1	0.197***	0.060**	0.003	0.033	0.196***	0.045**	-0.019	-0.255
	(0.019)	(0.018)	(0.029)	(0.317)	(0.018)	(0.014)	(0.028)	(0.389)
Panel E: Δ Log Real MW								
T3	0.340***	0.088***	-0.022	-0.249	0.338***	0.073***	-0.040*	-0.549
	(0.014)	(0.017)	(0.020)	(0.250)	(0.013)	(0.012)	(0.019)	(0.282)
T3 - T1	0.197***	0.056**	-0.007	-0.083	0.196***	0.044**	-0.026	-0.350
	(0.019)	(0.018)	(0.028)	(0.322)	(0.018)	(0.015)	(0.028)	(0.397)
Panel F: Share Below New Real MW								
T3	0.325***	0.099***	0.021	0.212	0.323***	0.078***	-0.004	-0.054
	(0.017)	(0.013)	(0.022)	(0.221)	(0.016)	(0.013)	(0.024)	(0.311)
T3 - T1	0.126***	0.074***	0.023	0.229	0.125***	0.057**	0.001	0.012
	(0.021)	(0.018)	(0.030)	(0.300)	(0.020)	(0.018)	(0.031)	(0.401)
Panel G: GAP								
T3	0.332***	0.089***	0.009	0.101	0.330***	0.068***	-0.015	-0.214
	(0.014)	(0.014)	(0.020)	(0.221)	(0.014)	(0.011)	(0.022)	(0.332)
T3 - T1	0.137***	0.051***	0.024	0.274	0.135***	0.028	-0.003	.
	(0.021)	(0.013)	(0.023)	(0.263)	(0.021)	(0.017)	(0.026)	

Notes: Panels A–G report results for the Skill 1 workers using the alternative bite measures defined in Section 2.3; panel titles identify the ranking variable. Columns (1)–(4) report the baseline specification without covariates and columns (5)–(8) report the all-covariates specification. Within each block, columns report the log real minimum wage effect, the log wage effect, the log employment effect, and the implied OWE. T3 is the top tercile of the 60 state minimum wage events ranked by the relevant bite measure, and T3–T1 is the top-minus-bottom tercile contrast. GAP denotes the share-below measure times the average log wage gap to the new minimum. The all-covariates specification adds the change in log employment for Skill 3 workers, the change in log wages for Skill 3 workers, and the change in log income per capita. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: What Predicts Wage and Employment Effects?

	Wage	Wage	Wage	Wage	Employment	Employment	Employment	Employment
Composite Min. Wage Bite	1.000***			1.160***	0.658*			0.737
	(0.229)			(0.277)	(0.276)			(0.429)
Unemp. rate		-0.002		-0.006*		-0.000		-0.003
		(0.004)		(0.002)		(0.003)		(0.004)
Concentration			-3.230	-1.834			-1.810	-0.745
			(1.732)	(1.516)			(1.791)	(2.035)

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix B Detailed Comparison with Clemens and Strain (2024)

This appendix provides a systematic comparison of our results with Clemens and Strain (2024). We organize the discussion around the three dimensions of fragility identified in Section 3.2: (i) the worker group definition, (ii) the definition of a "large" minimum wage increase, and (iii) the role of foreign-born workers.

Sensitivity to worker group and bite measure. Table B1 reports the T3 own-wage elasticity across nine worker group definitions and four bite measures: the Kaitz index, the real minimum wage level, the change in the real minimum wage, and the wage gap measure (the share of workers below the new minimum wage times the average log wage gap to the new minimum). Using the Clemens and Strain worker group definition—less than high school, aged below 26—and their definition of "large" (the dollar change in the real minimum wage), we find $OWE = -0.65$ (SE $= 0.21$), broadly consistent with their reported result. However, this finding does not generalize:

- *Across worker groups*: Holding fixed the dollar-change measure, the large negative OWE is specific to the less-than-high-school-under-26 cell. For low-wage workers, $OWE = 0.08$ (SE $= 0.21$); for Skill 1 workers, $OWE = -0.19$ (SE $= 0.25$); for teens and workers with a high school diploma or less under 26, the OWE is small and not significant. The LTHS-under-26 group comprises only 8 percent of the workforce and 19 percent of minimum wage workers.
- *Across bite measures*: Holding fixed the LTHS-under-26 group, the large negative OWE appears *only* with the dollar change measure. Using the Kaitz index yields $OWE = -0.20$ (SE $= 0.36$); using the real minimum wage level yields $OWE = -0.26$ (SE $= 0.34$); and using the wage gap measure yields $OWE = -0.40$ (SE $= 0.23$), which is not statistically significant. High-Kaitz events produce strong wage effects for this group (0.152, SE $= 0.024$) but employment effects that are small and not significant.

The Clemens and Strain result thus requires both a narrow worker group and a specific bite measure to produce a large negative employment effect. Table B2 decomposes the LTHS result by cumulative age window, showing how the negative effects evolve across all four bite measures.

Role of foreign-born workers. Tables B3 and B4 decompose the LTHS results by nativity using cumulative age windows comparable to Table B2. Even maintaining both the less-than-high-school-under-26 group and the dollar change definition, the result is further weakened by the composition of this cell. For the 16–25 cumulative window, restricting to U.S.-born workers reduces the OWE

from -0.65 (SE $= 0.21$) to -0.38 (SE $= 0.24$), and it loses statistical significance. The foreign-born component shows a larger negative point estimate ($OWE = -0.75$) but with a very large standard error (0.77), reflecting the small cell size. This pattern holds across all cumulative age windows: the U.S.-born OWEs are consistently smaller in magnitude than the pooled estimates, while the foreign-born estimates are too imprecise to draw conclusions from.

Multiple hypothesis testing. Table B5 applies the Romano and Wolf (2005) stepdown procedure to the full grid of nine worker groups and four bite measures in Table B1—36 hypotheses in total. Panel A reports the T3 employment coefficient, its standard error, and the Romano-Wolf adjusted p-value in brackets. No cell in the grid is significantly negative after the correction. The Clemens and Strain finding for LTHS under 26 with the dollar change measure—the only cell that is significant in isolation ($p = 0.004$)—has a Romano-Wolf adjusted p-value of 0.29, far from conventional significance. Panel B reports the corresponding T3 OWE for reference.

Table B1: Sensitivity of the T3 Own-Wage Elasticity to Worker Group and Bite Measure

	T3 Own-Wage Elasticity			
	(1)	(2)	(3)	(4)
	Kaitz	Real MW	Δ Real MW	GAP
Low Wage	0.328*	0.314	0.080	0.276*
	(0.162)	(0.172)	(0.205)	(0.131)
Skill 1	0.409	0.114	-0.193	0.101
	(0.262)	(0.274)	(0.245)	(0.221)
Teen	0.342	0.106	-0.110	0.214
	(0.186)	(0.229)	(0.207)	(0.177)
Young	0.198	0.062	-0.178	0.082
	(0.121)	(0.175)	(0.209)	(0.135)
HSL <26	0.259	0.135	-0.252	0.041
	(0.224)	(0.235)	(0.260)	(0.180)
LTHS <26	-0.199	-0.256	-0.653**	-0.402
	(0.361)	(0.342)	(0.212)	(0.232)
LTHS	0.003	-0.143	-0.567*	-0.180
	(0.329)	(0.278)	(0.257)	(0.185)
LTHS <26 – US Born	0.041	-0.009	-0.376	-0.133
	(0.261)	(0.283)	(0.236)	(0.169)
LTHS – US Born	-0.041	-0.044	-0.247	-0.019
	(0.223)	(0.222)	(0.215)	(0.160)

Notes: Each cell reports the T3 own-wage elasticity (OWE = employment effect / wage effect) for the given worker group, with events ranked by the bite measure indicated in the column header. GAP is the wage gap measure (share of workers below the new minimum $\times$ average log wage gap to the new minimum). Standard errors in parentheses, clustered by state. LTHS <26 – U.S.-born and LTHS – U.S.-born refer only to the period 1994+. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B2: Comparison to Clemens and Strain (2021): T3 Events, LTHS, No Controls, Cumulative Effect

	Log Wage				*Log Employment*				*OWE*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Kaitz	Real MW	Δ Real MW	GAP	Kaitz	Real MW	Δ Real MW	GAP	Kaitz	Real MW	Δ Real MW	GAP
16-18	0.157***	0.159***	0.144***	0.136***	0.007	-0.005	-0.052	-0.014	0.046	-0.034	-0.359	-0.100
	(0.026)	(0.024)	(0.024)	(0.019)	(0.043)	(0.050)	(0.046)	(0.036)	(0.272)	(0.315)	(0.351)	(0.272)
16-21	0.159***	0.154***	0.134***	0.127***	-0.001	-0.020	-0.068*	-0.036	-0.007	-0.132	-0.508*	-0.285
	(0.023)	(0.022)	(0.024)	(0.020)	(0.035)	(0.041)	(0.028)	(0.022)	(0.219)	(0.267)	(0.249)	(0.184)
16-25	0.152***	0.150***	0.134***	0.133***	-0.030	-0.039	-0.087**	-0.054	-0.199	-0.256	-0.653**	-0.402
	(0.024)	(0.023)	(0.022)	(0.017)	(0.057)	(0.053)	(0.030)	(0.033)	(0.361)	(0.342)	(0.212)	(0.232)
16-30	0.138***	0.138***	0.120***	0.119***	-0.025	-0.039	-0.077**	-0.049	-0.183	-0.281	-0.637**	-0.409
	(0.022)	(0.022)	(0.021)	(0.013)	(0.056)	(0.049)	(0.029)	(0.031)	(0.398)	(0.349)	(0.222)	(0.257)
16-35	0.114***	0.114***	0.091***	0.100***	-0.018	-0.032	-0.068*	-0.035	-0.156	-0.285	-0.748**	-0.354
	(0.020)	(0.020)	(0.021)	(0.015)	(0.054)	(0.046)	(0.028)	(0.027)	(0.461)	(0.401)	(0.287)	(0.257)
16-40	0.097***	0.098***	0.073***	0.092***	-0.003	-0.024	-0.048*	-0.019	-0.028	-0.248	-0.660*	-0.204
	(0.020)	(0.018)	(0.021)	(0.016)	(0.036)	(0.034)	(0.021)	(0.019)	(0.372)	(0.347)	(0.300)	(0.204)
16-45	0.080***	0.087***	0.064**	0.080***	-0.007	-0.016	-0.039*	-0.016	-0.087	-0.189	-0.610*	-0.196
	(0.020)	(0.018)	(0.019)	(0.015)	(0.030)	(0.027)	(0.018)	(0.015)	(0.363)	(0.316)	(0.300)	(0.191)
16-65	0.078***	0.085***	0.063***	0.081***	0.000	-0.012	-0.036*	-0.015	0.003	-0.143	-0.567*	-0.180
	(0.014)	(0.012)	(0.016)	(0.012)	(0.026)	(0.024)	(0.016)	(0.015)	(0.329)	(0.278)	(0.257)	(0.185)

Notes: Table reports T3 estimates for workers with less than a high school diploma (LTHS), using the cumulative age windows shown in the row labels. Columns (1)–(4) report log wage effects, columns (5)–(8) report log employment effects, and columns (9)–(12) report implied own-wage elasticities (OWE = employment effect / wage effect), with events ranked by the bite measures in the column headers: the Kaitz index, the real minimum wage level, the change in the real minimum wage, and GAP. GAP is the share of workers below the new minimum multiplied by the average log wage gap to the new minimum. T3 denotes the top tercile of events under the corresponding bite measure. The row labeled 16–25 corresponds most closely to the LTHS-under-26 group emphasized by Clemens and Strain (2024). Estimates use the no-controls specification. Standard errors are in parentheses and clustered by state. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table B3: Comparison to Clemens and Strain (2021): T3 Events, LTHS – U.S.-Born, No Controls

	Log Wage				*Log Employment*				*OWE*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Kaitz	Real MW	Δ Real MW	GAP	Kaitz	Real MW	Δ Real MW	GAP	Kaitz	Real MW	Δ Real MW	GAP
16-18	0.160***	0.158***	0.141***	0.132***	0.019	0.009	-0.042	0.001	0.120	0.059	-0.294	0.006
	(0.025)	(0.024)	(0.023)	(0.020)	(0.053)	(0.055)	(0.053)	(0.038)	(0.328)	(0.346)	(0.394)	(0.286)
16-21	0.159***	0.150***	0.132***	0.122***	0.007	-0.010	-0.063	-0.026	0.044	-0.067	-0.474	-0.212
	(0.020)	(0.021)	(0.022)	(0.019)	(0.041)	(0.043)	(0.037)	(0.025)	(0.258)	(0.289)	(0.311)	(0.201)
16-25	0.149***	0.145***	0.137***	0.131***	0.006	-0.001	-0.051	-0.017	0.041	-0.009	-0.376	-0.133
	(0.021)	(0.022)	(0.020)	(0.016)	(0.039)	(0.041)	(0.029)	(0.022)	(0.261)	(0.283)	(0.236)	(0.169)
16-30	0.125***	0.123***	0.117***	0.115***	0.009	-0.002	-0.040	-0.013	0.074	-0.014	-0.340	-0.117
	(0.018)	(0.018)	(0.016)	(0.011)	(0.034)	(0.035)	(0.024)	(0.018)	(0.274)	(0.287)	(0.223)	(0.157)
16-35	0.099***	0.099***	0.085***	0.096***	-0.011	-0.012	-0.049*	-0.016	-0.110	-0.120	-0.577	-0.168
	(0.015)	(0.016)	(0.015)	(0.014)	(0.040)	(0.037)	(0.024)	(0.020)	(0.413)	(0.378)	(0.311)	(0.206)
16-40	0.108***	0.104***	0.084***	0.106***	-0.008	-0.008	-0.035	-0.005	-0.076	-0.072	-0.413	-0.044
	(0.022)	(0.021)	(0.018)	(0.018)	(0.032)	(0.032)	(0.022)	(0.016)	(0.304)	(0.312)	(0.306)	(0.155)
16-45	0.099***	0.097***	0.082***	0.099***	-0.008	-0.004	-0.027	-0.005	-0.085	-0.044	-0.326	-0.048
	(0.019)	(0.018)	(0.016)	(0.015)	(0.026)	(0.026)	(0.019)	(0.015)	(0.262)	(0.268)	(0.264)	(0.148)
16-65	0.108***	0.109***	0.089***	0.110***	-0.004	-0.005	-0.022	-0.002	-0.041	-0.044	-0.247	-0.019
	(0.024)	(0.021)	(0.016)	(0.017)	(0.024)	(0.024)	(0.017)	(0.018)	(0.223)	(0.222)	(0.215)	(0.160)

Notes: Table reports T3 estimates for U.S.-born workers with less than a high school diploma (LTHS), using the cumulative age windows shown in the row labels. Columns (1)–(4) report log wage effects, columns (5)–(8) report log employment effects, and columns (9)–(12) report implied own-wage elasticities (OWE = employment effect / wage effect), with events ranked by the bite measures in the column headers: the Kaitz index, the real minimum wage level, the change in the real minimum wage, and GAP. GAP is the share of workers below the new minimum multiplied by the average log wage gap to the new minimum. T3 denotes the top tercile of events under the corresponding bite measure. Estimates use the no-controls specification. Because nativity is required, the sample is restricted to 1994 onward. Standard errors are in parentheses and clustered by state. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table B4: Comparison to Clemens and Strain (2021): T3 Events, LTHS – Non-U.S.-Born, No Controls

	Log Wage				*Log Employment*				*OWE*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Kaitz	Real MW	Δ Real MW	GAP	Kaitz	Real MW	Δ Real MW	GAP	Kaitz	Real MW	Δ Real MW	GAP
16-18	0.229**	0.187*	0.219***	0.218***	-0.121	-0.144	-0.130	0.010	-0.527	-0.771	-0.595	0.046
	(0.081)	(0.081)	(0.055)	(0.065)	(0.216)	(0.189)	(0.142)	(0.166)	(0.969)	(1.087)	(0.683)	(0.755)
16-21	0.215**	0.214***	0.140**	0.163***	-0.019	-0.076	-0.030	0.019	-0.086	-0.356	-0.213	0.116
	(0.067)	(0.057)	(0.050)	(0.047)	(0.206)	(0.183)	(0.129)	(0.141)	(0.953)	(0.846)	(0.903)	(0.863)
16-25	0.170***	0.167***	0.115**	0.124***	-0.115	-0.107	-0.086	-0.059	-0.676	-0.641	-0.754	-0.478
	(0.049)	(0.042)	(0.043)	(0.036)	(0.153)	(0.134)	(0.101)	(0.110)	(0.851)	(0.757)	(0.770)	(0.873)
16-30	0.197***	0.199***	0.138**	0.143***	0.023	0.014	0.003	0.017	0.118	0.068	0.019	0.122
	(0.053)	(0.050)	(0.046)	(0.036)	(0.079)	(0.068)	(0.044)	(0.058)	(0.409)	(0.345)	(0.323)	(0.401)
16-35	0.152***	0.140***	0.085*	0.099***	0.081	0.046	0.029	0.043	0.535	0.327	0.338	0.438
	(0.045)	(0.035)	(0.039)	(0.026)	(0.052)	(0.045)	(0.029)	(0.037)	(0.412)	(0.357)	(0.426)	(0.438)
16-40	0.071	0.081*	0.041	0.057	0.076*	0.039	0.029	0.031	1.080	0.479	0.699	0.548
	(0.044)	(0.035)	(0.037)	(0.030)	(0.036)	(0.037)	(0.028)	(0.029)	(1.007)	(0.554)	(0.974)	(0.704)
16-45	0.044	0.065	0.021	0.035	0.043	0.028	0.017	0.020	0.972	0.431	0.792	0.579
	(0.045)	(0.037)	(0.039)	(0.033)	(0.035)	(0.032)	(0.023)	(0.028)	(1.502)	(0.627)	(1.962)	(1.182)
16-65	0.047	0.058*	0.022	0.035	0.035	0.018	-0.007	0.001	0.741	0.315	-0.322	0.019
	(0.034)	(0.029)	(0.032)	(0.028)	(0.035)	(0.032)	(0.023)	(0.027)	(0.992)	(0.597)	(1.121)	(0.790)

Notes: Table reports T3 estimates for non-U.S.-born workers with less than a high school diploma (LTHS), using the cumulative age windows shown in the row labels. Columns (1)–(4) report log wage effects, columns (5)–(8) report log employment effects, and columns (9)–(12) report implied own-wage elasticities (OWE = employment effect / wage effect), with events ranked by the bite measures in the column headers: the Kaitz index, the real minimum wage level, the change in the real minimum wage, and GAP. GAP is the share of workers below the new minimum multiplied by the average log wage gap to the new minimum. T3 denotes the top tercile of events under the corresponding bite measure. Estimates use the no-controls specification. Because nativity is required, the sample is restricted to 1994 onward. Standard errors are in parentheses and clustered by state. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table B5: Romano-Wolf Adjusted P-values for T3 Employment Effects: Worker Groups × Bite Measures

	T3 Employment Coefficient			
	(1)	(2)	(3)	(4)
	Kaitz	Real MW	Δ Real MW	GAP
Panel A: T3 Employment Coefficient				
Low Wage	0.024*	0.024	0.005	0.021
	(0.011)	(0.014)	(0.013)	(0.011)
	[0.646]	[0.867]	[1.000]	[0.807]
Skill 1	0.042	0.012	−0.017	0.009
	(0.023)	(0.028)	(0.021)	(0.020)
	[0.834]	[1.000]	[1.000]	[1.000]
Teen	0.046	0.015	−0.013	0.025
	(0.024)	(0.032)	(0.024)	(0.022)
	[0.813]	[1.000]	[1.000]	[1.000]
Young	0.025	0.008	−0.018	0.009
	(0.016)	(0.022)	(0.019)	(0.015)
	[0.941]	[1.000]	[1.000]	[1.000]
HSL <26	0.027	0.015	−0.023	0.004
	(0.023)	(0.026)	(0.021)	(0.019)
	[1.000]	[1.000]	[1.000]	[1.000]
LTHS <26	−0.030	−0.039	−0.087**	−0.054
	(0.057)	(0.053)	(0.030)	(0.033)
	[1.000]	[1.000]	[0.289]	[0.935]
LTHS	0.000	−0.012	−0.036*	−0.015
	(0.026)	(0.024)	(0.016)	(0.015)
	[1.000]	[1.000]	[0.583]	[1.000]
LTHS <26 – US Born	0.006	−0.001	−0.051	−0.017
	(0.039)	(0.041)	(0.029)	(0.022)
	[1.000]	[1.000]	[0.870]	[1.000]
LTHS – US Born	−0.004	−0.005	−0.022	−0.002
	(0.024)	(0.024)	(0.017)	(0.018)
	[1.000]	[1.000]	[0.994]	[1.000]
Panel B: T3 Own-Wage Elasticity				
Low Wage	0.328*	0.314	0.080	0.276*
	(0.162)	(0.172)	(0.205)	(0.131)
Skill 1	0.409	0.114	−0.193	0.101
	(0.262)	(0.274)	(0.245)	(0.221)
Teen	0.342	0.106	−0.110	0.214
	(0.186)	(0.229)	(0.207)	(0.177)
Young	0.198	0.062	−0.178	0.082
	(0.121)	(0.175)	(0.209)	(0.135)
HSL <26	0.259	0.135	−0.252	0.041
	(0.224)	(0.235)	(0.260)	(0.180)
LTHS <26	−0.199	−0.256	−0.653**	−0.402
	(0.361)	(0.342)	(0.212)	(0.232)
LTHS	0.003	−0.143	−0.567*	−0.180
	(0.329)	(0.278)	(0.257)	(0.185)
LTHS <26 – US Born	0.041	−0.009	−0.376	−0.133
	(0.261)	(0.283)	(0.236)	(0.169)
LTHS – US Born	−0.041	−0.044	−0.247	−0.019
	(0.223)	(0.222)	(0.215)	(0.160)

Notes: Panel A reports the T3 employment coefficient from the SUR system for each worker group and bite measure combination. Standard errors in parentheses are clustered by state. Romano-Wolf adjusted p-values in brackets control the familywise error rate across all 36 hypotheses, using the stepdown procedure of Romano and Wolf (2005) with 999 cluster bootstrap replications. Panel B reports the T3 own-wage elasticity for reference (uncorrected). Stars on original (uncorrected) p-values: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B6: Romano-Wolf Adjusted P-values for T3–T1 Employment Contrasts

	Emp T3–T1	SE	p-val	RW p-val	OWE T3–T1	OWE SE
Panel A: Across Bite Measures (High-Recall Group)						
Kaitz index	0.039	(0.015)	0.009	[0.144]	0.540	(0.237)
Total bite (GAP)	0.030	(0.012)	0.010	[0.151]	0.384	(0.140)
Share below	0.038	(0.016)	0.016	[0.214]	0.463	(0.194)
Real MW	0.025	(0.017)	0.152	[0.779]	0.327	(0.226)
Δ Real MW ($)	0.017	(0.017)	0.327	[0.964]	0.289	(0.274)
Δ Log MW	0.007	(0.017)	0.656	[1.000]	0.127	(0.274)
Δ Kaitz	-0.001	(0.020)	0.945	[1.000]	-0.033	(0.476)
Panel B: Across Worker Groups (Kaitz Terciles)						
High recall	0.039	(0.015)	0.009	[0.182]	0.540	(0.237)
High probability	0.083	(0.033)	0.013	[0.228]	0.806	(0.418)
Young ($\leq$21)	0.049	(0.024)	0.044	[0.413]	0.388	(0.204)
Teens	0.066	(0.034)	0.055	[0.462]	0.503	(0.290)
HS or less	0.020	(0.011)	0.061	[0.484]	0.407	(0.270)
All	0.014	(0.007)	0.041	[0.399]	0.491	(0.268)
HSL $\leq$25	0.048	(0.034)	0.153	[0.738]	0.468	(0.342)
Restaurants	0.025	(0.018)	0.164	[0.766]	0.322	(0.197)
Low-wage sectors	-0.011	(0.010)	0.234	[0.874]	-0.245	(0.217)
LTHS	0.033	(0.034)	0.329	[0.950]	0.427	(0.483)
LTHS $\leq$25 US	0.034	(0.047)	0.460	[0.991]	0.230	(0.314)
LTHS US-born	0.023	(0.033)	0.490	[0.995]	0.213	(0.321)
Low recall	-0.003	(0.005)	0.569	[0.999]	-0.247	(0.518)
LTHS $\leq$25	0.007	(0.070)	0.918	[1.000]	0.047	(0.463)

Notes: This table reports T3–T1 employment contrasts (columns 1–4) and T3–T1 own-wage elasticities (columns 5–6) from the SUR system. Standard errors in parentheses are clustered by state. Romano-Wolf adjusted p-values in brackets control the familywise error rate within each panel, using the step-down procedure of Romano and Wolf (2005) with 999 cluster bootstrap replications. Panel A tests across seven bite measures for the low-wage workers. Panel B tests across 14 worker groups using Kaitz terciles.