# Online Appendix A
# Additional Figures and Tables

Figure A.1 shows all minimum wage increases between 1979 and 2016. We use the time series of state-level minimum wage changes from Vaghul and Zipperer (2016). Blue circles show the minimum wage events that are used in the event study analysis. The light orange triangles represent small minimum wage changes that we do not analyze (but control for). For these changes, the minimum wage increased either by less than \$0.25 (the size of our wage bins) or by less than 2 percent of the workforce earned between the new and the old minimum wage. Finally, the green circles indicate federal changes, which we also exclude from our primary sample of treatments because the change in missing number of jobs, $\Delta b$, is identified only from time-series variation for these events as there are no "control states" with wage floors lower than the new minimum wage. The figure highlights that around 70% (99/138) of the minimum wage changes in our sample occurred after 2000.

Some wages in the CPS are imputed. In most of our analysis we use only non-imputed wages. This might be of concern if the imputation rate changes in response to the minimum wage, or is correlated with minimum wage changes for some other reason. Figure A.2 shows event study estimates where the outcome is the state-level imputation rate. The figure shows that minimum wage events studied here have no apparent effect on the imputation rate.

Our definition of "overall employment" does not include self-employed workers, who are not covered by the minimum wage. (Note that QCEW does not include self-employed either). The exclusion of self-employed can be problematic if minimum wages shift employees to self-employment. Figure A.3 ("Impact of Minimum Wages on the Self- Employment") shows that the self-employment rate (i.e., self employed workers divided by wage and salary plus self-employed workers) is not affected by the minimum wage. This confirms that there is not any shift to self-employment induced by the minimum wage.

Figure A.4 plots the evolution of wage and total employment change for affected workers over annualized event time using our baseline specification with wage-bin-period and wage-bin-state fixed effects. The upper graph in Figure A.4 illustrates the clear, statistically significant rise in the average wage of affected workers at date zero, which persists over the five year post-intervention period. In contrast, the lower panel in Figure A.4 shows that there is no corresponding change in employment over the five years following treatment. Moreover, employment changes were similarly small during the three years prior to treatment.

Figure A.5 shows the effect of the minimum wage on the wage distribution when we take into account that sometimes minimum wage increases are phased in over multiple events. In 65% of the cases we study, a primary minimum wage increase is followed by a secondary one within 5 years, on average at \$0.56 above the minimum for the primary event. In contrast to the main results of the paper, where we show the partial effect of each event, here we show the cumulative effect of both primary and secondary events by taking into account the incidence and size of secondary increases averaged across our sample of events. The cumulative effect of primary and secondary events on missing jobs is 2.5%, which is larger than the partial effect of the primary events, which is 1.8% (see Figure 2). Therefore, the presence of multiple events can explain some of the difference between the jobs below the new minimum wage—which is around 8.6%—and the missing jobs below the new minimum wage—which is around 1.8%— in the main analysis.

Figure A.6 compares our main estimates of own wage elasticity of employment to the estimates in the previous literature. The estimates from the previous literature are obtained from Harasztosi and Lindner (2016), using studies that reported both employment and wage estimates. We report the benchmark estimates from Column 1 in Table 1 and the Card and Krueger high probability groups from Column 6 in Table 2. The dashed line shows the lower bound estimates of our benchmark specification. The Figure A.6 points out that our benchmark estimates can rule out 7 out of 11 negative estimates in the literature. When we additionally focus on the Card and Krueger high-probability group, our estimates rule out 8 of those 11 negative estimates.

Panel (a) in Figure A.7 plots the relationship between missing jobs below (multiplied by -1) and the excess jobs above the new minimum wage for the various subgroups in Table 2. While there is large variation in the missing jobs across various demographic groups, they are matched closely by excess jobs above the new minimum wage. The dashed line is the 45-degree line and depicts the locus of points where the missing and excess jobs are equal in magnitude ($\Delta a = -\Delta b$). In all cases, except for the black or Hispanic group, the excess jobs are larger than the missing jobs indicating a positive albeit statistically insignificant employment effect. For black or Hispanic individuals, the difference between excess and missing jobs is negligible.

Panel (b) in Figure A.7 plots the relationship between missing jobs below (multiplied by -1) and the excess jobs above the new minimum wage for fully partitioned education-age groups. We use 4 education categories and 6 age categories, yielding a total of 23 education-by-age groups.[37] For each of these 23 groups, we separately estimate a regression using our baseline specification, and calculate changes in missing ($\Delta b_g$) and excess jobs ($\Delta a_g$) for each of them. Each grey circle represents one age-education group, while the blue squares show the binned scatterplot. We also report the linear fit (red line) and the 45-degree (dashed) line that depicts the locus of points where the missing and excess jobs are equal in magnitude ($\Delta a = -\Delta b$). The figure can be used to assess labor-labor substitution across various demographic groups. If there is no employment effect in any of the groups, the slope coefficient $\mu_1$ from regressing $\Delta a_g = \mu_0 + \mu_1 \times (-\Delta b_g)$ should be close to one; under this scenario, differences across groups in the number of excess jobs at or above the minimum wage exactly mirrors the difference in the number of missing jobs below. In contrast, if employment declines are more severe for lower skilled groups—for whom the bite (-$\Delta b$) is expected to be bigger—then we should expect the slope to be less than one, especially for larger values of $-\Delta b$. As shown in in Figure A.7, the slope of the fitted line is very close to one, with $\hat{\mu}_1 = 1.070$ (s.e. 0.075). The binned scatter plot shows that there is little indication of a more negative slope at higher values of $-\Delta b$. While some specific groups (e.g., individuals with less than high school education between 30 and 40 years of age) are above the 45 degree line, others (e.g., individuals with less than high school education between 40 and 50 years of age) are below the line. Overall, these findings provide little evidence of heterogeneity in the employment effect by skill level.

Figure A.8 shows the event-by-event relationship between missing jobs, excess jobs,

[37] Education categories are less than high school, high school graduate, some college and college graduate. Age categories are teens, [20, 30), [30, 40), [40, 50), [50, 60), and 60 and above. We exclude teens with college degrees from the sample.

employment change and the minimum to median wage (Kaitz index). We plot the bin-scattered non-parametric relationship without controlling for other characteristics of the event. The figure is very similar to our benchmark estimates in Figure 5 where we do control for observable characteristics including urban share, decade dummies and whether the state leans Republican.

Figure A.9 shows the event-by-event relationship between the change in employment and the minimum to median wage ratio (the Kaitz index). Here we show the raw (and not binned) scatter plots, where each dot represents one of the 138 events studied in the event study. The red circles show the 8 minimum wage changes in Washington DC, while the green circles show the remaining 130 events. The figure highlights that events from Washington DC are often outliers, which is not surprising given that the Washington DC sample sizes are very small in the CPS. To alleviate the influence of outliers when comparing across events, we decided to drop Washington DC from our event-by-event analysis in Figure 5 and in Figure A.8. However we keep those events in the rest of the paper where we report the event study estimates.

Figure A.10 shows the impact of minimum wages on the wage distribution in *weighted* and *unweighted* TWFE-logMW specifications. Panel (a) reports Figure 6 from the main text estimated using (level) fixed effects. Panel (b) reports the *unweighted* version of Figure 6. The use of weights has a modest impact on the results.

Obtaining a meaningful "first stage" effect of the minimum wage on average wages is essential for interpreting the estimated employment effects of the minimum wage. Table A.1 compares the t-statistics obtained from estimates of wage elasticities using the preferred bunching estimator, which infers the wage changes from the bin by bin estimates from equation 1, and the estimator that runs equation 1 at the state-level and uses log of average state level wage as the outcome variable. Both sets of estimates use the paper's same underlying 138 events for the minimum wage increases. In nearly every demographic group, the bunching estimator's wage effects are much more precisely estimated and the aggregated estimator's wage effects are often not distinguishable from zero at conventional levels of statistical significance. For all workers, the t-statistic for the bunching estimator is 12 times as large as the t-statistic from the aggregated estimator. Only in the smaller subgroup of teens does the aggregated estimator's precision modestly outperform that of the bunching estimator. In almost all cases, the bunching estimator is able to estimate a wage effect statistically different from zero at the 1 percent level of significance. The only exception is for the low probability CK group, in which the bunching estimator estimates a positive wage effect statistically distinguishable from zero at the 5 percent level, and where the aggregated estimator obtains a negative and highly imprecise wage effect estimate.

The bunching method infers job losses from employment changes around the minimum wage. This has a potential advantage even in the absence of large upper tail employment changes: filtering out random shocks to jobs in the upper part of the wage distribution can improve precision of the estimates. Table A.2 compares the point estimates and standard errors of the bunching estimator and an estimator that uses equation 1 at the state-level, and specified group's aggregate employment as the outcome variable for calculating the elasticity of employment with respect to the minimum wage. For almost all the groups, the bunching estimator is at least as precise as the aggregate estimator, sometimes substantially more so in the case of smaller demographic groups. Row 1 shows that, for all workers, the point estimates of both approaches are rather similar when estimating the policy's employment elasticity, with the standard error of the bunching approach modestly smaller, at 88% of the aggregate estimator. In the cases of workers with lower education, the bunching estimator's employment elasticity standard errors are between 65% and 76% of those from the aggregate estimator. The last three rows of the table examine the the high probability, middle, and low probability groups described in section 2.2. Only for the middle group does the aggregate estimator largely outperform the bunching estimator's precision. (As we discuss in the paragraph above, however, for this middle group there is no significant wage effect detectable using the aggregate approach, which makes the precision meaningless.)

As a further check on the correlation between minimum wages and the imputation rate of wages, Table A.3 shows the effect of the minimum wage on the imputation rate using various alternative specifications. All specifications confirm that minimum wages have no impact on the imputation rate.

Table A.4 explores the robustness of the benchmark analysis shown in Column 1 of Table 1. In column (1) of Table A.4, we focus on the effect for events that take place in the 7 states without a tip credit, where the same minimum wage is applied to tipped and non-tipped employees.[38] Even if the share of the workforce earning below the new minimum wage (9.9%) in these states are similar to those in the primary sample, the bite of the policy is larger in the no-tip-credit states: missing jobs are 2.7% of pre-treatment employment in the no-tip-credit sample as compared to 1.8% in the full sample. However, the larger number of missing jobs is almost exactly compensated by an excess number of jobs above the minimum wage, which amount to 2.6% of pre-treatment employment. The resulting employment elasticity with respect to own wage is $-0.139$ (s.e. 0.530).

In the second column of Table A.4, we expand the event definition to include (nontrivial) federal minimum wage increases, which produces a total of 369 events. Here we find the missing jobs ($\Delta b$) to be slightly larger in magnitude at 2.0% of pre-treatment employment. The wage effect for affected workers is 6.7% and statistically significant. The employment elasticities with respect to the minimum wage and own wage are both close to zero at -0.009 (s.e. 0.019) and -0.157 (s.e. 0.32), respectively. For federal increases, the change in the number of missing jobs below, $\Delta b$, is identified only using time series variation, since there are no covered workers earning below the new minimum in control states. However, $\Delta a + \Delta b$ is identified using cross-state variation, since at least for the 1996-1997 increase and especially for the 2007-2009 increase there are many control states with covered employment \$4 above the new federal minimum wage. Overall, we find it reassuring that the key finding of a small employment elasticity remains even when we consider federal increases.

In column (3) of Table A.4, we consider the number of hours employed and estimate the effect of the minimum wage on full-time equivalent (FTE) workers. These estimates are not very different from Table 1. The actual number of FTE jobs below the minimum wage (relative to the pre-treatment employment) is lower ($\bar{b}_{-1}$ =6.7% as opposed to 8.6% in Table 1), indicating that low-wage workers work fewer hours. Consistent with this, missing jobs estimate is also smaller in magnitude when we use an FTE measure (-1.3% instead of -1.8%). The average wage change for affected workers accounting for hours is 7.3% (s.e 1.2%), while the employment change is 4.4% (s.e. 3.3%). After accounting for hours, the employment

[38] These states are Alaska, California, Minnesota, Montana, Nevada, Oregon and Washington.

elasticity with respect to the minimum wage and the own wage are 0.029 (s.e. 0.022) and 0.601 (s.e. 442), respectively. The analogous estimates for headcount employment in Table 1 were 0.024 (s.e. 0.025) and 0.411 (s.e. 0.43).

In column (4) of Table A.4, we restrict the sample to hourly workers; we expect these workers to report their hourly wage information more accurately than our calculation of hourly earnings (as weekly earnings divided by usual hours) for salaried workers. Although the actual number of workers below the new minimum wage is close to our benchmark sample (10.4% vs. 8.6% in Table 1) the missing jobs estimate almost doubles (3.3.% vs. 1.8% in Table 1). As a result, the wage effects are more pronounced for this subset of workers than the overall sample (9.4% versus 6.8% in Table 1), which is consistent with measurement error in wages being smaller for those who directly report their hourly wages. Nevertheless, the employment elasticities with respect to the minimum wage (0.029, s.e. 0.035) and with respect to the own wage (0.306 s.e. 0.392) are very similar to our benchmark estimates.

In column (5), we exclude workers in tipped occupations, as defined by Autor, Manning and Smith (2016). Tipped workers can legally work for sub-minimum wages in most states, and hence may report hourly wages below the minimum wage (as tips are not captured in the reported hourly wage). As we explained in Section 2.3, such imperfect coverage creates a discrepancy between the actual level ($\bar{b}_{-1}$) and the change ($\Delta b$) in the number of workers below the new minimum wage; however, it does not create a bias in the bunching estimate for the change in employment ($\Delta a + \Delta b$). Excluding tipped workers reduces the average bite, $\bar{b}_{-1} = 6.1\%$, while the estimate of missing jobs of -1.6% is close to our benchmark estimate of -1.8% in Table 1. Consequently, estimated wage effects are larger by around 20% (8.2% versus 6.8% in Table 1). However, excluding tipping workers has a negligible impact on the employment estimates: the own-wage employment elasticity is 0.337 as opposed to 0.41 in Table 1.

In column (6), we present estimates using the raw CPS data instead of the QCEW benchmarked CPS. The missing jobs estimate of -1.8% is essentially the same as the baseline estimate. The wage (7.7%) and employment (4.6%) estimates as well as the employment elasticities with respect to the minimum wage (0.039) and own wage (0.590) are slightly more positive. The benefit of using the QCEW benchmarked CPS is the increased precision of the estimates. Without benchmarking, the standard errors for the minimum wage and the own-wage elasticities are 44% and 25% larger than those in column (1) of Table 1.

In column (7) we provide estimates without using population weights. These results are virtually identical to our benchmark estimates (Column (1) of Table 1). For instance, the employment elasticity with respect to the minimum wage is 0.401 (s.e. 0.418), which is virtually identical to the weighted estimate of 0.411 (s.e. 0.430). The similarity of the weighted and unweighted estimates is reassuring, since a substantial difference between the two could reflect potential misspecification (Solon, Haider and Wooldridge 2015).

In column (8), we limit the sample to 1993-2016. The similarity of the employment elasticity with respect to the minimum wage estimates obtained from post-1992 sample and from the baseline sample (0.006 (0.026) instead of 0.024 (0.025)) is used below in Online Appendix F to explain differences between the findings of the event-based approach and the TWFE-logMW specification.

Our data is in 25-cent bins and the baseline specification treatment indicators are in 1-dollar increments. To allay any concerns, in column (9), we also check the robustness of our results where the treatment indicators are also in 25 cent increments. In other words, there are 4 times as many regression coefficients for this specification as in our benchmark specification. Obviously, the specific \$0.25 wage bins estimates are noisier than the \$1 bin estimates. However, once we sum up these more nosily estimated coefficients, we obtain estimates that are highly similar to our baseline results (0.023 (0.026) and 0.401 (0.447) instead of 0.024 (0.025) and 0.411 (0.430), respectively).

Table A.5 explores the sensitivity of the results using alternative thresholds, $\overline{W}$, for calculating the excess jobs at and above the minimum wage. In our baseline specification, we calculate the excess jobs by adding up the impact in the interval between $MW$ and $\overline{W} = MW + \$4$. In the table we report results using values for $\overline{W} - MW$ between \$2 and \$6. The table shows that the excess jobs estimate increases when the threshold is increased from \$2 (column 2) to \$3 (column 3), but beyond that the estimates remain stable. Therefore, our results are not sensitive to the particular value of $\overline{W}$ once we take into account the presence of spillovers up to \$3 above the minimum wage.

In Table A.6, we consider the robustness of our results to using alternative event windows. Column 1 repeats our baseline results using a window between event dates -3 and 4 (i.e., the 3rd year before the minimum wage increase and 4th year after). Columns 2 and 4 show that reducing the post-treatment window end-date to 2, or extending it to 6 has little impact on the wage or employment estimates. Similarly, columns 4 and 5 show that extending the pre-treatment start date to -5 or reducing it to -1 also has very limited impact on the estimates. For example, across all 5 columns, the employment elasticity with respect to the minimum wage varies between 0.008 and 0.025; the associated standard errors vary between 0.021 and 0.027. Overall, these estimates show that our findings are not driven by our specific choice of the event window.

Table A.7 reports estimated wage and employment effects of the aggregate event-based (panel A), and the bunching (panel B) estimators for the Card and Krueger predicted probability groups. While the aggregate event-based approach considers wage and employment of the full group, the bunching approach looks locally at wage and employment changes of affected workers near the minimum wage. Note that the percentage change in overall average wage will be considerably smaller than the percentage change in wage at the bottom of the distribution. Take the case where both employment fell by 5% and wages rose by 5% for affected workers, but affected workers were only half of total employment. Then aggregate employment would fall by 2.5%, but average wage will rise by even less, since unaffected workers have higher wages than affected workers. As a result the common way of calculating employment elasticity–that takes the ratio of the employment effects and wage effect–will be biased using the aggregate approach; and the smaller the share of affected workers in the group (so that the average wage of the group is much larger than the wage of affected workers), the bigger is the bias.

Column 1 of Table A.7 shows the estimates for the high probability group. Both approaches estimate a sizable and statistically significant wage effects with no indication of disemployment. The wage and employment elasticities with respect to the minimum wage are 0.187 (s.e. 0.062) and 0.081 (s.e. 0.084) in panel A, respectively, using the aggregate approach; these are consistent with the findings in panel B using the bunching estimator. However, the former approach fails to detect a statistically significant wage effect of the policy for the middle and the low probability groups in columns 2 and 3. The wage elasticity estimates in columns

2 and 3 are 0.065 (s.e. 0.057) and -0.005 (s.e. 0.038). This limits the ability of using the CK probability group approach by itself to examine the employment effects of the minimum wage. Since the "first stage" wage effect is missing for the latter two groups, it is difficult to assess the size of the estimated employment effects (0.057 (s.e. 0.047) and 0.001 (s.e. 0.023) for the middle and low probability groups, respectively). On the other hand, the bunching estimator captures a sizable and statistically significant wage effect for all of the groups (0.051 (s.e. 0.013) and 0.060 (s.e. 0.032) for the middle, and low probability groups). By examining changes in the frequency distribution for wages around the minimum wage, the bunching estimator enables us to establish a causal relationship between the policy and the employment effects for each of the groups.

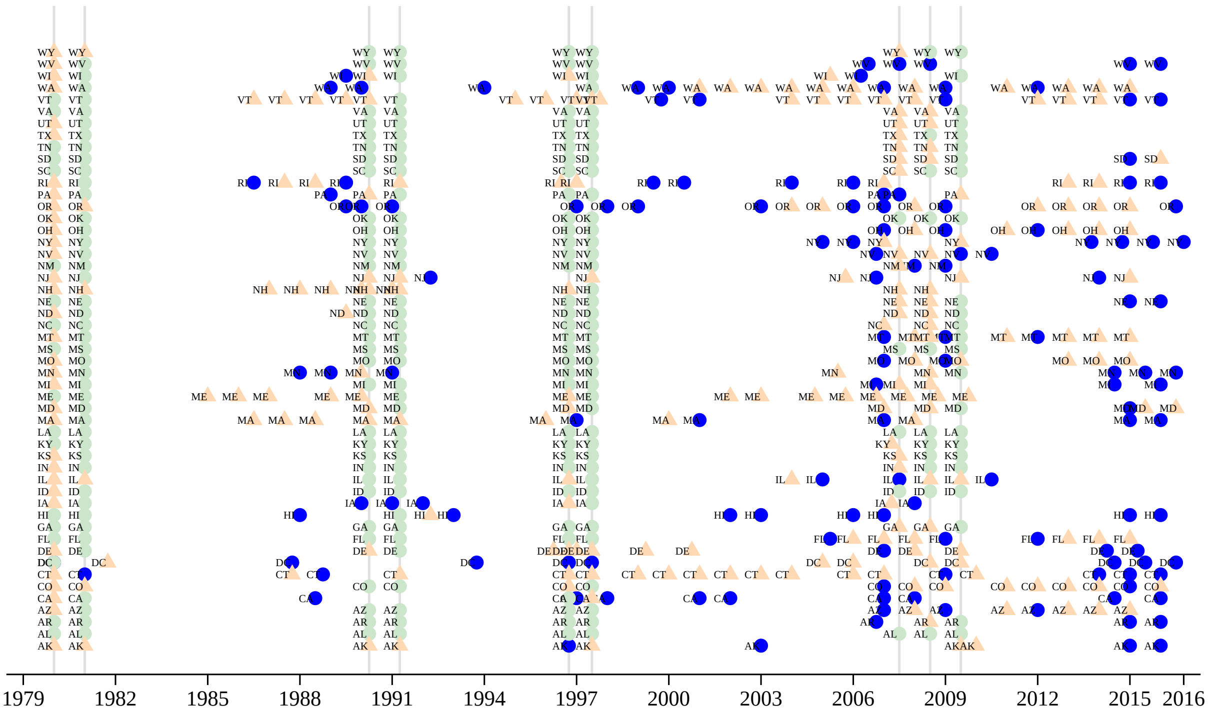
Table A.8 shows the impact of the minimum wage for incumbents and for new entrants to the labor force. Since CPS interviews individuals twice (one year apart), we can only assess a short term impact of the minimum wage for these two subgroups. However, columns (1) and (2) highlight that the short term and the long term impact of the minimum wage is very similar for the overall sample. By matching the CPS over time, we lose observations either because matching is not possible, or because there are "bad" matches (see Online Appendix D for details). Finally, we can only observe past employment status in the second period, so we can only use half of the observations in the matched sample. This shrinks our primary sample size from 4,694,104 to 1,505,192. The results from this matched sample is shown in column (3). The missing jobs are exactly the same as in the baseline (column 1), however, the excess jobs are slightly lower (1.8% in column 3 vs. 2.1% in baseline). As a result, the change in affected jobs is slightly smaller than in the baseline estimate, but it is still statistically insignificant and positive in sign. Columns (4) and (5) decompose these changes by incumbents and new entrants. Two thirds of the missing jobs come from incumbents, while one third from new entrants. However, the change in missing jobs matches the change in excess jobs in both groups, so the employment effects are very similar (0.9% for incumbents and 0.8% for new entrants). At the same time, the wage effects are different, since new entrants do not experience any spillover effects (see Figure 4).

Table A.9 shows estimates for the event-by-event analysis presented in Figure 5 using alternative specifications. The estimated relationship between the Kaitz index on the jobs below, on the missing jobs, on the excess jobs, and on the employment change are similar across various specifications, which underlines the robustness of the results presented in Figure 5.

Table A.10 shows the estimated employment elasticities using our event-based approach, as well as distributed lag specifications in log minimum wage (with 4 years of lags, contemporaneous, and 3 years of leads) estimated in both TWFE-logMW and in first differences (FD) specifications (see the details in Online Appendix F). We report employment estimates on aggregate employment in (columns 1, 2 and 5) and employment under \$15 (columns 3, 4 and 6) in Panel A. There is a wide range of estimates for aggregate employment, as we pointed out in Figure 6. When we exclude employment variation in the upper tail and focus on employment in jobs under \$15, the range of estimates narrows considerably. For example, for the weighted estimates, the employment elasticity with respect to the minimum wage is -0.020 (s.e. 0.028) in the fixed effect specification, -0.005 (s.e. 0.019) in first difference specification, and 0.027 (s.e. 0.022) in the event-based specification. These estimates cannot be distinguished statistically from each other, or from zero. This highlights that variability in the estimates is mainly driven by variation in employment above \$15, which is unlikely to reflect the causal effect of the minimum wage. Column 6 estimates event-based regressions of the minimum wage on jobs below \$15. We refer to this specification as the "simpler method" in Section 2.3 and we report the estimates in Column 7 of Table 1. (The slight difference between Column 6 in Table A.10 and Column 7 in Table 1 is that the former is based on annual data while the latter is based on quarterly data.) Column 7 shows our baseline estimates where we estimate the effect of the minimum wage on job counts in each wage bin, calculate the missing and excess jobs and then add them up. Both the point estimates and the standard errors are very close to each in other in the "simpler method" and in our baseline regressions.

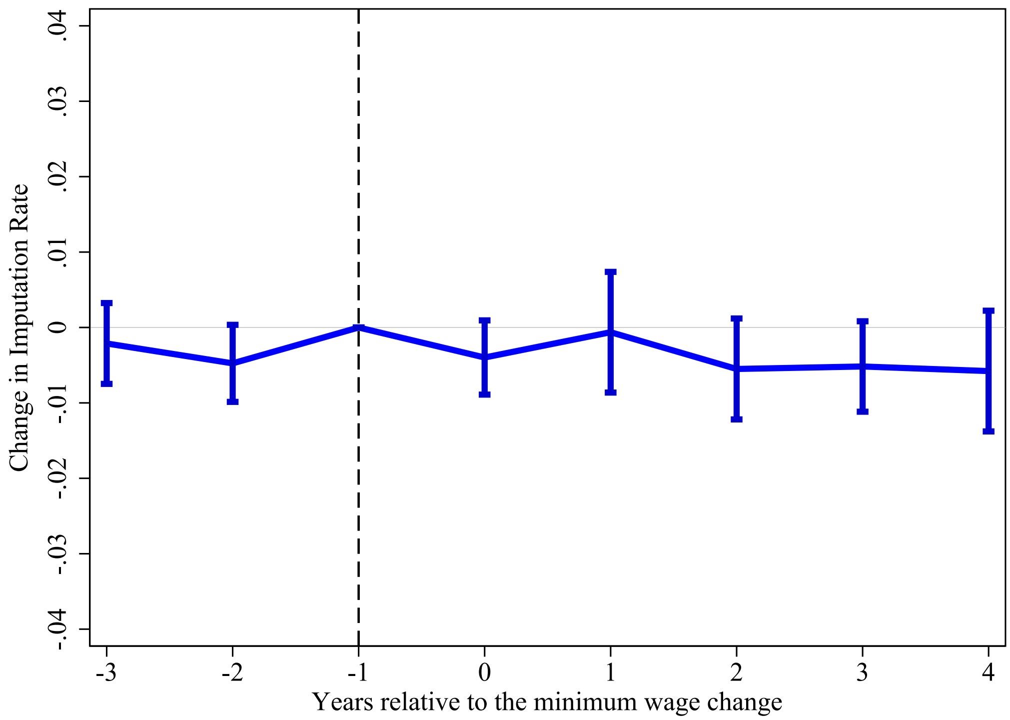
Panel B of Table A.10 shows the TWFE-logMW, first difference (FD), and event-based (EB) regressions for teens (see the details in Online Appendix F). The variability in the estimates for teens is not driven by changes in employment in the upper tail. This is not surprising, since most teens earn below \$15, and so variation in the upper tail can only have limited impact on the estimates. Column 6 estimates event based regression of the minimum wage on jobs below \$15. Column 7 shows our baseline estimates where we estimate the effect of the minimum wage on job counts in each wage bin, calculate the missing and excess jobs and then add them up. The estimates with the "simpler method" (column 6) and with our baseline method (column 7) are very similar. In general, we find that the teen estimates from fixed effects models tend to be more negative than the first difference ones—similar to Allegretto et al. (2017), and to the estimates for overall employment. Moreover, event-based estimates are much closer to those using first differencing, again mirroring the findings for overall employment.

Figure A.1: Minimum Wage Increases between 1979 and 2016



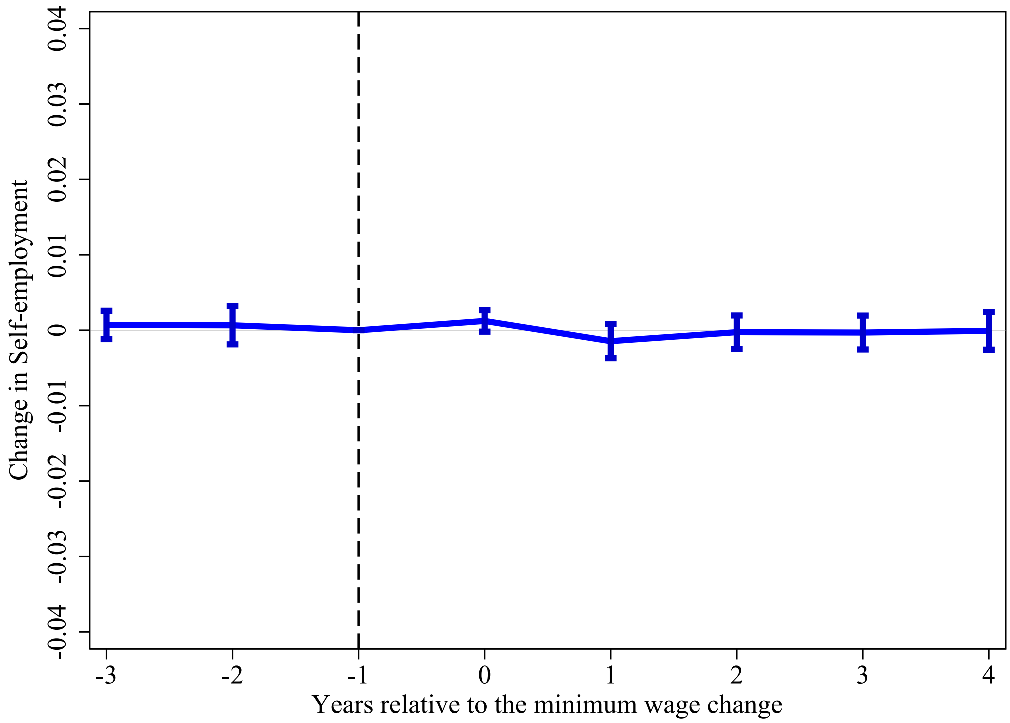

*Notes*: The figure shows all minimum wage increases between 1979 and 2016. There are a total of 623 minimum wage increases. The blue circles show the primary minimum wage events used in estimating equation 1; the light orange triangles highlight small minimum wage changes where minimum wage increased less than $0.25 (the size of our wage bins) or where less than 2 percent of the workforce earned between the new and the old minimum wage. The green circles indicate federal changes, which we exclude from our primary sample of treatments because the change in missing number of jobs, $\Delta b$, is identified only from time-series variation for these events as there are no "control states" with wage floors lower than the new minimum wage (see the text for details).

Figure A.2: Impact of Minimum Wages on the Imputation Rate



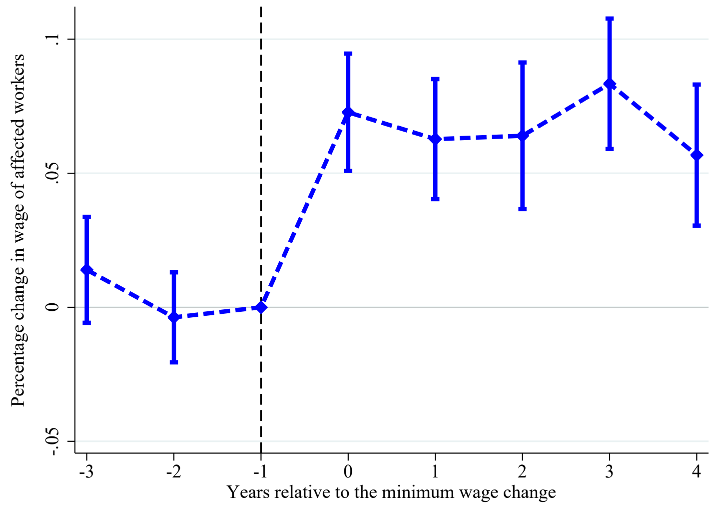

*Notes*: The figure shows the effect of the minimum wage on the imputation rate. In our event study analysis we only use non-imputed hourly wages. To alleviate the concern that imputation has an effect on our estimates, we implement an event study regression where the outcome variable is state-level imputation rate. Events are the same 138 state-level minimum wage changes between 1979-2016 that we use in our benchmark specification. Similarly to our benchmark specification we include state and time fixed effects in the regression. In the Online Appendix Table A.3 we report results with other specifications. The blue line shows the evolution of the imputation rate (relative to the year before the treatment). We also show the 95% confidence interval based on standard errors that are clustered at the state level.

Figure A.3: Impact of Minimum Wages on the Self-Employment Rate

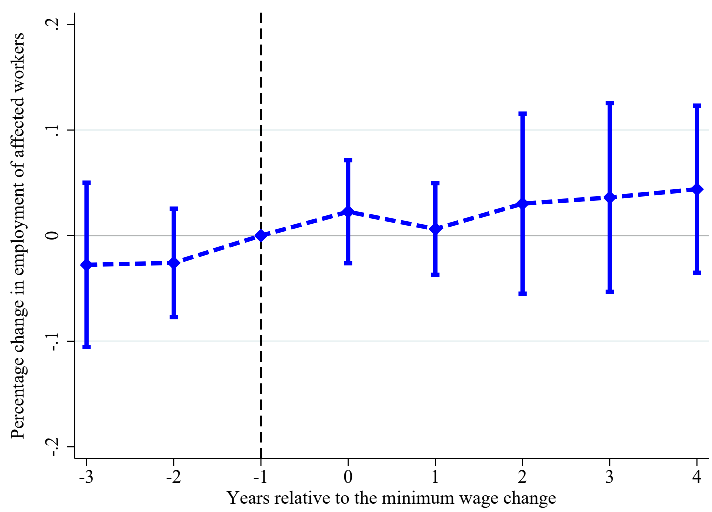



*Notes*: The figure shows the effect of the minimum wage on the self-employment rate. In our event study analysis we only use wage workers. To alleviate the concern that changes in self-employment rate have effects on our estimates, we implement an event study regression where the outcome variable is state-level self-employment rate. Events are the same 138 state-level minimum wage changes between 1979-2016 that we use in our benchmark specification. Similarly to our benchmark specification we include state and time fixed effects in the regression. The blue line shows the evolution of the self-employment rate (relative to the year before the treatment). We also show the 95% confidence interval based on standard errors that are clustered at the state level.

Figure A.4: Impact of Minimum Wages on Average Wage and on Employment Over Time



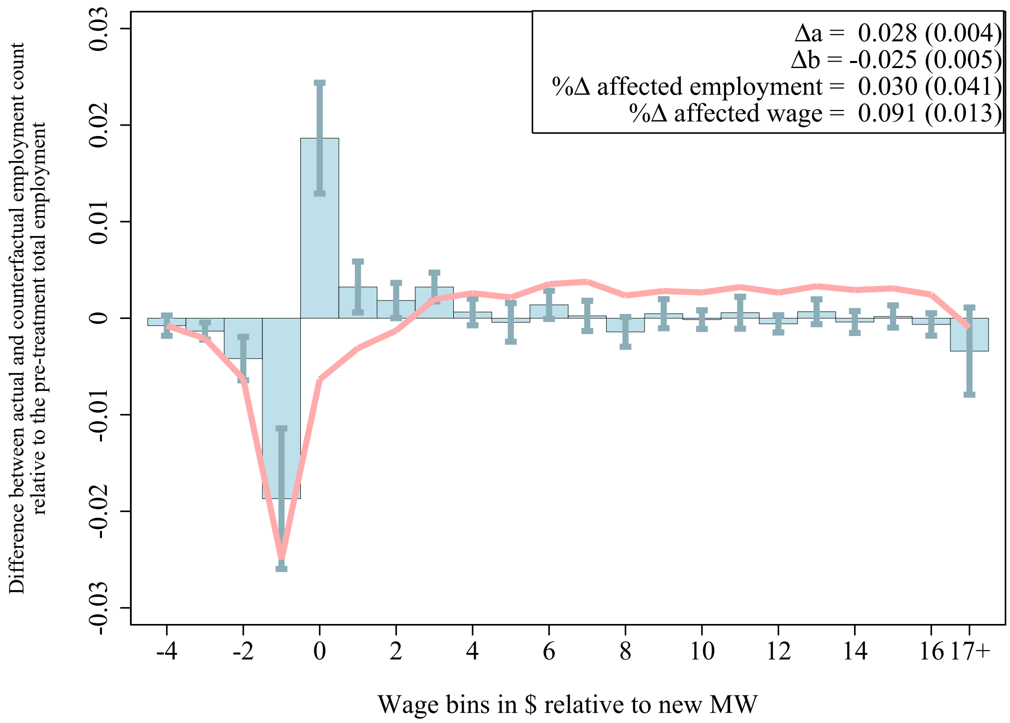

(a) Evolution of the average wage of the affected workers




(b) Evolution of the employment of the affected workers

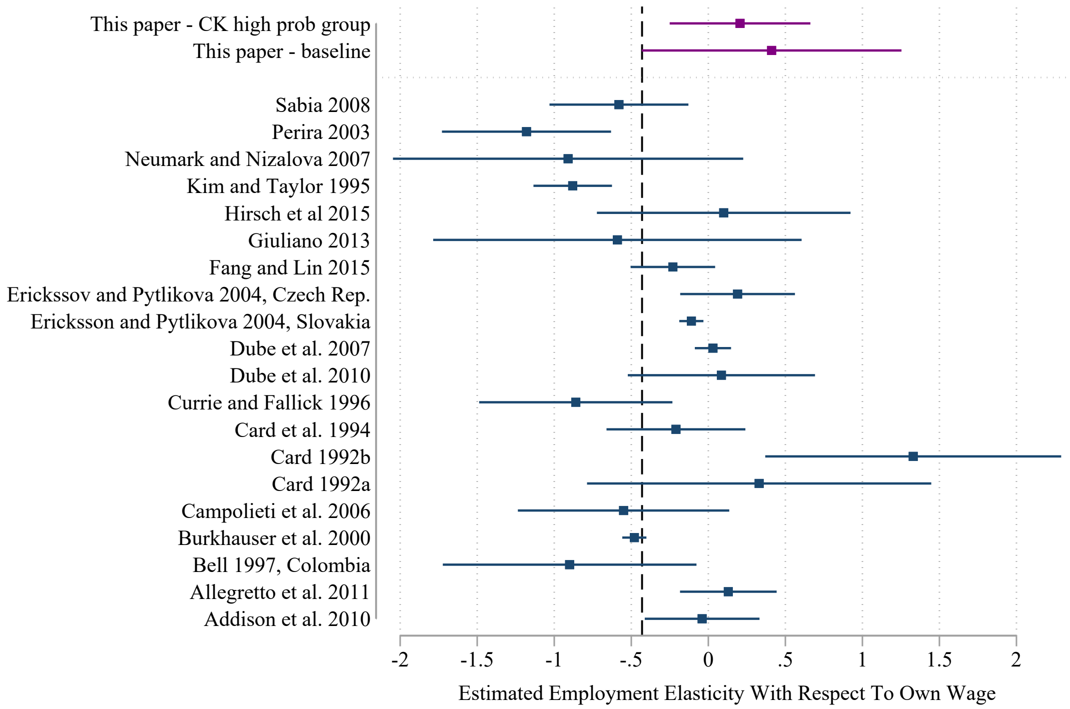
*Notes*: The figure shows the main results from our event study analysis (see equation 1) exploiting 138 state-level minimum wage changes between 1979-2016. Panel (a) shows the effect on the average wage over time, which is calculated using equation 2. Panel (b) shows the evolution of employment between \$4 below the new minimum wage and \$5 above it (relative to the total employment 1 year before the treatment), which is equal to the sum of missing jobs below and excess jobs at and slightly above the minimum wage, $\Delta b + \Delta a$. The figure highlights that minimum wage had a positive and significant effect on the average wage of the affected population, but there is no sign of significant disemployment effects.

Figure A.5: Change in Employment by Wage Bins after Aggregating Multiple Treatment Events



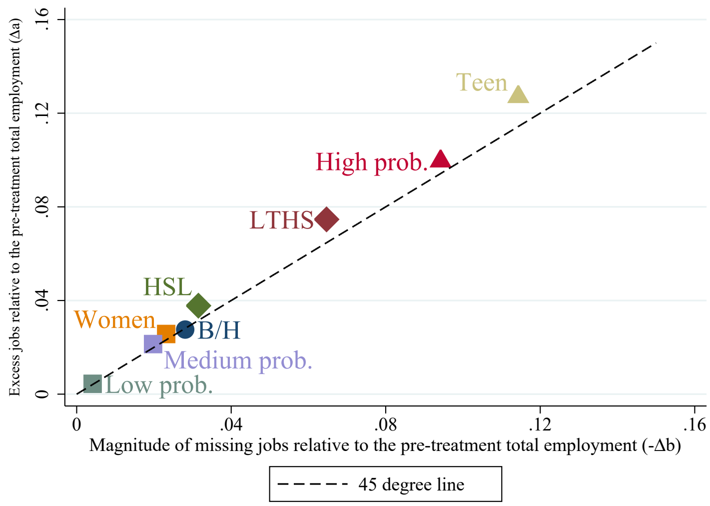

*Notes*: This figure replicates Figure 2 in the main text, but calculates a cumulative effect when there are multiple events in the 5-year post-treatment window. Overall, 65% of the time, a primary minimum wage increase is followed by a secondary one within 5 years, on average at $0.56 above the minimum for the primary event. Figure 2 shows the partial effect of each event. Here we show the cumulative effect of all events within a 5-year post-treatment window by taking into account the incidence and size of secondary increases averaged across our sample of events. The blue bars show for each dollar bin (relative to the minimum wage) the estimated average employment changes in that bin during the 5-year post-treatment relative to the total employment in the state one year before the treatment. The red line is the running sum of the bin-specific impacts. Adjusting for multiple events increases the estimate for missing jobs below the new minimum from 1.8% to 2.5%. Therefore, some of the difference between jobs below the new minimum wage, which is around 8.6%, and the missing jobs below the new minimum wage can be explained by multiple events following each other.

Figure A.6: Employment Elasticity with Respect to Own Wage in the Literature and in this Paper

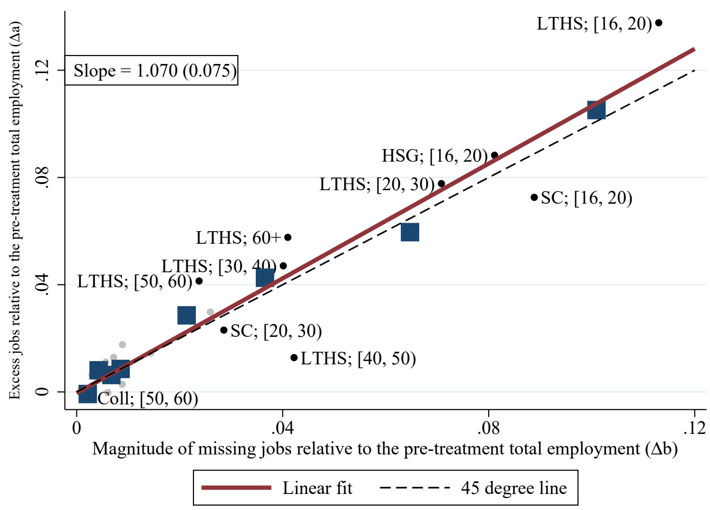



*Notes*: This figure summarizes the estimated employment elasticity with respect to wage and compares it to the previous estimates in the literature. The estimates in the literature are collected by Harasztosi and Lindner (2016). The two estimates from our paper is the benchmark estimate on overall employment (Column 1 in Table 1) and the estimates for the Card and Krueger high probability group Column 6 in Table 2. The dashed vertical line shows the lower bound of our benchmark estimates. The benchmark estimates can rule out 7 out of the 11 negative estimates provided in the previous literature.

Figure A.7: Impact of the Minimum Wage by Demographic Groups



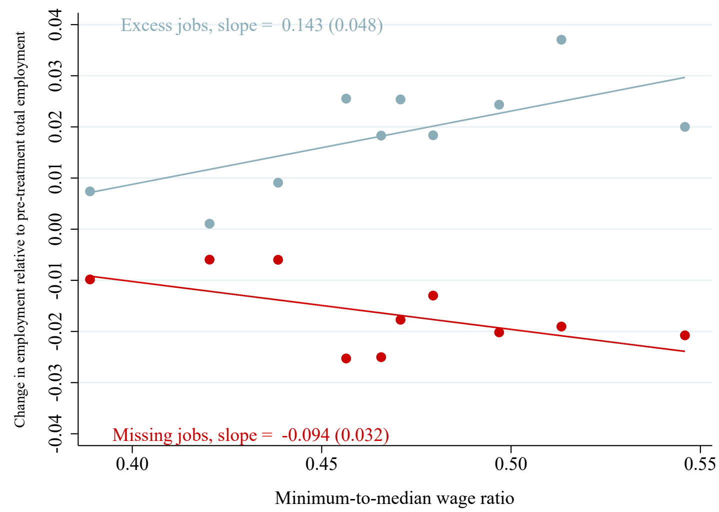

(a) Effect of the minimum wage by demographic groups



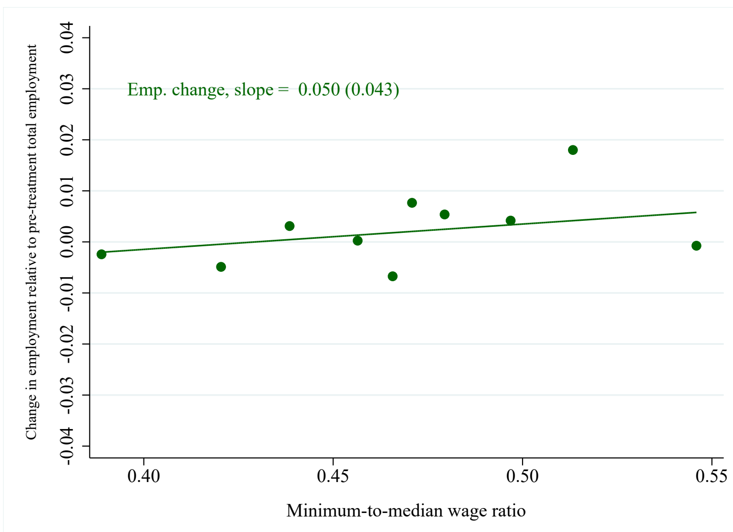

(b) Effect of the minimum wage by age-education groups

*Notes*: Both figures show the excess jobs (relative to the pre-treatment total employment in that group) above the new minimum wage ($\Delta a$) and magnitude of missing jobs below it ($-\Delta b$) for various demographic groups. The black dash line in both of the graphs are the 45 degree line indicating the locus of points where the excess number of jobs above and the missing jobs below the new minimum wage are exactly the same, and so the employment effect is zero. Estimates above that line indicate positive employment effects, and estimates below the line indicate negative ones. Panel (a) shows the estimates for demographic groups in Table 2: those with less than high school (LTHS) education, high school or less (HSL) education, women, teen, black or Hispanic workers (B/H), and groups with low, medium and high probability of being exposed to the minimum wage increase. Panel (b) shows the estimates for education-by-age groups generated from 6 age and 4 education categories. The small light gray and black points correspond to each of the groups, while the large blue squares show the non-parametric bin scattered relationship between the excess jobs ($\Delta a$) and the magnitude of missing jobs ($-\Delta b$). The red line shows the linear fit. A slope of that line below one would indicate the presence of labor-labor substitution across age and eduction groups.

Figure A.8: Relationship between Excess Jobs, Missing jobs, Employment Change and the Minimum-to-Median Wage Ratio Across Events (Replicating Figure 5 in the Main Text without using Controls)



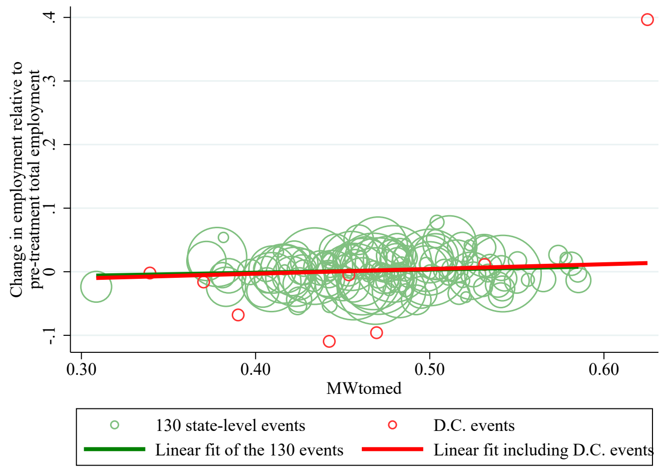

(a) Missing and excess jobs



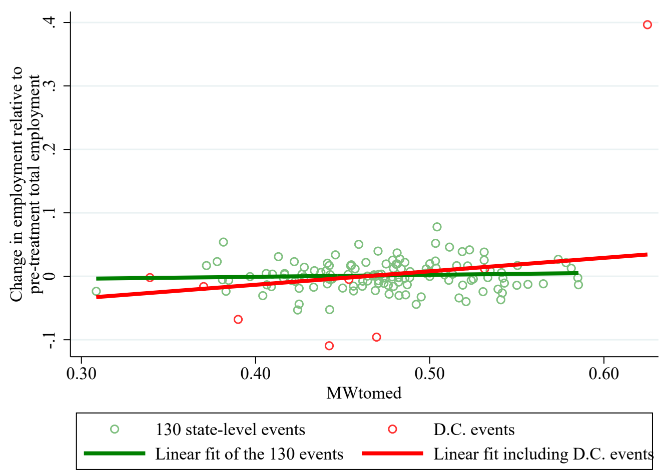

(b) Employment change

*Notes*: This figure replicates Figure 5 in the main text without using controls in the regression. The figure shows the binned scatter plots for missing jobs, excess jobs, and total employment changes by value of the minimum-to-median wage ratio (Kaitz index) for the 130 event-specific estimates. The minimum-to-median wage ratio is the new minimum wage $MW$ divided by the median wage at the time of the minimum wage increase (Kaitz index). The 130 events exclude 8 minimum wage raising events in the District of Columbia, since those events are very noisily estimated in the CPS. The bin scatters and linear fits plot the relationship without any control variables. Estimates are weighted by the state populations. The slope (and robust standard error in parentheses) is from the weighted linear fit of the outcome on the minimum-to-median wage ratio.

Figure A.9: Relationship between Employment Change and the Minimum-to-Median Wage Ratio Across Events, Scatterplot



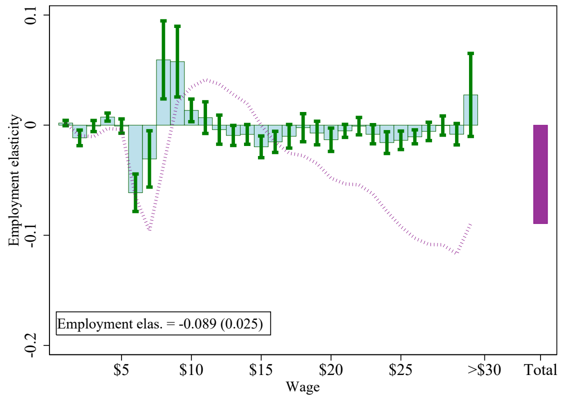

(a) Population weighted



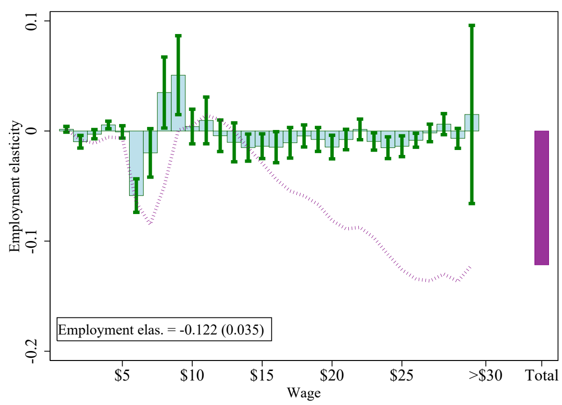

(a) Unweighted

*Notes*: The figure shows the population weighted and unweighted scatter plots of the estimated percentage change in employment in $[MW - \$4, MW + \$5)$ bins of each of the 138 events during the 5-year post-treatment relative to the 1-year pre-treatment period against the minimum-to-median wage ratio. The estimated employment change of each event is created from 138 regressions corresponding to each event, as explained in Section 3.3. The red circles indicate D.C. events, and the green circles the remaining 130 events. The lines are linear fits. The green line employs the 130 events; while the red one all events.

Figure A.10: Impact on Employment throughout the Wage Distribution in the Two-Way Fixed Effects Model on log Minimum Wages - Weighted and Unweighted Estimates




(a) Weighted




(b) Unweighted

*Notes*: The figure shows the effect of the minimum wage on the wage distribution using fixed effects specifications (TWFE-logMW), with and without population weights. Both panels estimate two-way (state-bin and year) fixed effects regressions on contemporaneous as well as 2 annual leads, and 4 annual lags of log minimum wage (panel (a) is the same as Figure 6 in the main text). For each wage bin we run a separate regression, where the outcome is the number of jobs per capita in that state-wage bin. The cumulative response for each event date 0, 1, ..., 4 is formed by successively adding the coefficients for the contemporaneous and lagged log minimum wages. The green bars show the mean of these cumulative responses for event dates 0, 1, ..., 4, divided by the sample average employment-to-population rate —and represents the average elasticity of employment in each wage bin with respect to the minimum wage in the post-treatment period. The 95% confidence intervals around the point estimates are calculated using clustered standard errors at the state level. The dashed purple line plots the running sum of the employment effects of the minimum wage up until the particular wage bin. The rightmost purple bar in each of the graphs is the elasticity of the overall state employment-to-population rate with respect to minimum wage, obtained from regressions where outcome variables are the state level employment-to-population rate. In the bottom left corner we also report the point estimate on this elasticity with standard errors that are clustered at the state level. Regressions in panels (a) are weighted by state population; whereas the ones in panels (b) are not weighted.

Table A.1: T-statistics for the Wage Effects of the Minimum Wage - Bunching and Aggregate Approaches

| | Bunching | Aggregated |
|---|---|---|
| All workers | 6.942 | 0.577 |
| Less than high school | 5.526 | 1.359 |
| High school or less | 5.487 | 0.549 |
| Teens | 4.603 | 4.965 |
| Women | 6.261 | 0.796 |
| Black or Hispanic | 3.585 | 0.584 |
| High prob. group | 6.822 | 3.003 |
| Middle group | 3.973 | 1.140 |
| Low prob. group | 1.866 | –0.136 |

*Notes.* Each cell reports the t-statistic from the estimated wage effect with respect to the minimum wage for various demographic groups. The bunching approach is the preferred specification in this paper, estimating the wage effect from bin-specific employment changes near the relevant minimum wage. The aggregated approach uses as the outcome overall aggregate employment. For the bunching case, the wage effect is the estimated percentage change of affected workers. For the aggregated case, the wage effect is the elasticity of the wage with respect to the minimum wage. Regressions are weighted by state averaged population of the demographic groups. T-statistics are obtained by dividing the estimated wage effects by robust standard errors clustered by state.

Table A.2: Precision of the Employment Elasticities with Respect to the Minimum Wage - Bunching and Aggregate Approaches

| | Bunching | Aggregated | Ratio of bunching to aggregated standard errors |
|---|---|---|---|
| All workers | 0.024 | 0.016 | 0.878 |
| | (0.025) | (0.029) | |
| Less than high school | 0.097 | 0.178* | 0.654 |
| | (0.061) | (0.094) | |
| High school or less | 0.061 | 0.041 | 0.756 |
| | (0.042) | (0.055) | |
| Teens | 0.125 | 0.128 | 1.011 |
| | (0.134) | (0.132) | |
| Women | 0.025 | –0.006 | 0.825 |
| | (0.027) | (0.033) | |
| Black or Hispanic | –0.005 | –0.004 | 0.716 |
| | (0.058) | (0.082) | |
| High prob. group | 0.052 | 0.081 | 0.876 |
| | (0.062) | (0.071) | |
| Middle group | 0.016 | 0.057* | 1.443 |
| | (0.049) | (0.034) | |
| Low prob. group | 0.003 | 0.001 | 0.558 |
| | (0.014) | (0.026) | |

*Notes.* Columns 1-2 report the separately estimated employment elasticity with respect to the minimum wage for the bunching and aggregate approaches, for various demographic groups. Column 3 reports the ratio of the bunching to aggregate approach standard errors. The bunching approach is the preferred specification in this paper, using wage-bin-specific employment per capita changes as the outcome. The aggregate approach uses overall employment per-capita as the outcome. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table A.3: Impact of Minimum Wages on the Imputation Rate in Various Regression Specifications

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| $\Delta$ imputation rate | -0.000 | 0.001 | 0.001 | 0.002 | -0.004 | -0.002 | -0.002 | -0.001 |
| | (0.004) | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| # observations | 7,242 | 7,242 | 7,242 | 7,242 | 7,242 | 7,242 | 7,242 | 7,242 |
| Mean of the dep. var | 0.249 | 0.249 | 0.249 | 0.249 | 0.280 | 0.280 | 0.280 | 0.280 |
| *Controls* | | | | | | | | |
| State trends | | Y | | Y | | Y | | Y |
| Division-by-year FE | | | Y | Y | | | Y | Y |
| Weighted | | | | | Y | Y | Y | Y |

*Notes.* The table reports 5-year averaged change in the imputation rate of the CPS from 1979 to 2016 after the primary 138 events. The dependent variable is the imputation rate, defined as the number of imputed observations divided by the number of employed observations. The estimates are calculated by employing an event based approach, where we regress state imputation rates on quarterly leads and lags on treatment spanning 12 quarters before and 19 quarters after the policy change. All specifications include state, and quarter fixed effects. Columns 2, 4, 6, and 8 controls for state linear trends; whereas columns 3, 4, 7, and 8 allow census divisions to be affected differently by macroeconomic shocks. The regressions are not weighted in columns 1-4; and they are population weighted in columns 5-8. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table A.4: Robustness of the Impact of Minimum Wages to Alternative Workforce, Treatment and Sample Definitions

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Missing jobs below new MW (Δb) | -0.027*** | -0.020*** | -0.013*** | -0.033*** | -0.016*** | -0.018*** | -0.017*** | -0.019*** | -0.016*** |
| | (0.003) | (0.003) | (0.003) | (0.008) | (0.004) | (0.004) | (0.003) | (0.004) | (0.004) |
| Excess jobs above new MW (Δa) | 0.026*** | 0.019*** | 0.016*** | 0.036*** | 0.017*** | 0.022*** | 0.019*** | 0.020*** | 0.019*** |
| | (0.002) | (0.003) | (0.003) | (0.007) | (0.003) | (0.003) | (0.002) | (0.003) | (0.002) |
| %Δ affected wages | 0.065*** | 0.067*** | 0.073*** | 0.094*** | 0.082*** | 0.077*** | 0.070*** | 0.058*** | 0.069*** |
| | (0.007) | (0.012) | (0.012) | (0.020) | (0.014) | (0.011) | (0.010) | (0.011) | (0.010) |
| %Δ affected employment | -0.009 | -0.010 | 0.044 | 0.029 | 0.028 | 0.046 | 0.028 | 0.007 | 0.028 |
| | (0.034) | (0.021) | (0.033) | (0.035) | (0.039) | (0.042) | (0.030) | (0.029) | (0.030) |
| Employment elasticity w.r.t. MW | -0.010 | -0.009 | 0.029 | 0.029 | 0.017 | 0.039 | 0.022 | 0.006 | 0.023 |
| | (0.036) | (0.019) | (0.022) | (0.035) | (0.024) | (0.036) | (0.024) | (0.026) | (0.026) |
| Emp. elasticity w.r.t. affected wage | -0.139 | -0.157 | 0.601 | 0.306 | 0.337 | 0.590 | 0.401 | 0.122 | 0.401 |
| | (0.530) | (0.326) | (0.442) | (0.392) | (0.496) | (0.536) | (0.418) | (0.495) | (0.447) |
| Jobs below new MW ($\bar{b}_{-1}$) | 0.099 | 0.083 | 0.067 | 0.104 | 0.061 | 0.087 | 0.079 | 0.087 | 0.086 |
| %Δ MW | 0.093 | 0.096 | 0.101 | 0.101 | 0.101 | 0.101 | 0.100 | 0.097 | 0.101 |
| Number of events | 44 | 369 | 138 | 138 | 138 | 138 | 138 | 116 | 138 |
| Number of observations | 847,314 | 847,314 | 847,314 | 847,314 | 847,314 | 847,314 | 847,314 | 531,063 | 847,314 |
| Number of workers in the sample | 4,694,104 | 4,694,104 | 4,561,684 | 2,824,287 | 4,402,488 | 4,694,104 | 4,694,104 | 2,503,803 | 4,694,104 |
| Set of events | No tip credit states | State & Federal | Primary | Primary | Primary | Primary | Primary | Primary | Primary, treatment in 25-cent increments |
| Sample | All workers | All workers | FTE | Hourly workers | Non-tipped occupations | CPS-Raw | Unweighted | All workers, post-1992 | All workers |

*Notes.* The table reports robustness checks for the effects of a minimum wage increase based on the event study analysis (see equation 1) exploiting minimum wage changes between 1979 and 2016. All columns except column (2) are based on state-level minimum wage changes. The table reports five year averaged post-treatment estimates on missing jobs up to $4 below the new minimum wage, excess jobs at and up to $5 above it, employment and wages. Column (1) reports estimates for the 44 events which occured in states that do not allow tip credit. Column (2) reports estimates using 369 state or federal minimum wage increases. Column (3) uses full time equivalent job counts and so takes changes in hours worked into account. Column (4) uses workers who directly reported being hourly workers in the survey. Column (5) uses workers in non-tipped occupations only. Column (6) does not use the QCEW benchmarking, and instead reports the estimates calculated using the raw CPS counts (see Section 4.2 for details). All regressions are weighted by state-quarter aggregated population except Column (7), where we report unweighted estimates. Column (8) only considers minimum wage events that happened on or before 2012q1 to ensure a full five year post-treatment period. Column (9) shows the results for the post-1992 sample. Column (10) defines treatment indicators in 25 cent increments. All specifications include wage bin-by-state and wage bin-by period fixed effects. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

*Line-by-line description.* The first two rows report the change in number of missing jobs below the new minimum wage (Δb), and excess jobs above the new minimum wage (Δa) relative to the pre-treatment total employment. The third row, the percentage change in average wages in the affected bins, (%ΔW), is calculated using equation 2 in Section 2.2. The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by jobs below the new minimum wage ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). The fifth row, employment elasticity with respect to the minimum wage is calculated as $\frac{\Delta a+\Delta b}{\%\Delta MW}$ whereas the sixth row, employment elasticity with respect to the wage, reports $\frac{1}{\%\Delta W}\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$. The line on the number of observations shows the number of quarter-bin cells used for estimation, while the number of workers refers to the underlying CPS sample used to calculate job counts in these cells.

Table A.5: Impact of Minimum Wage Increase on the Average Wage and Employment of Affected workers - Robustness to Alternative Wage Windows

| | Alternative wage window | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Missing jobs below new MW (Δb) | -0.018*** | -0.018*** | -0.018*** | -0.018*** | -0.018*** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Excess jobs above new MW (Δa) | 0.018*** | 0.021*** | 0.021*** | 0.020*** | 0.021*** |
| | (0.003) | (0.002) | (0.003) | (0.003) | (0.002) |
| %Δ affected wages | 0.046*** | 0.064*** | 0.068*** | 0.068*** | 0.081*** |
| | (0.009) | (0.008) | (0.010) | (0.013) | (0.012) |
| %Δ affected employment | -0.002 | 0.029 | 0.028 | 0.024 | 0.033 |
| | (0.025) | (0.031) | (0.029) | (0.031) | (0.034) |
| Employment elasticity w.r.t. MW | -0.001 | 0.025 | 0.024 | 0.020 | 0.028 |
| | (0.021) | (0.027) | (0.025) | (0.026) | (0.029) |
| Emp. elasticity w.r.t. affected wage | -0.038 | 0.452 | 0.411 | 0.349 | 0.410 |
| | (0.539) | (0.479) | (0.430) | (0.443) | (0.390) |
| Jobs below new MW ($\bar{b}_{-1}$) | 0.086 | 0.086 | 0.086 | 0.086 | 0.086 |
| %Δ MW | 0.101 | 0.101 | 0.101 | 0.101 | 0.101 |
| Number of event | 138 | 138 | 138 | 138 | 138 |
| Number of observations | 847,314 | 847,314 | 847,314 | 847,314 | 847,314 |
| Number of workers in the sample | 4,694,104 | 4,694,104 | 4,694,104 | 4,694,104 | 4,694,104 |
| Upper endpoint of wage window ($\overline{W}$): | MW+$2 | MW+$3 | MW+$4 | MW+$5 | MW+$6 |

*Notes.* The table reports the effects of a minimum wage increase based on the event study analysis (see equation 1) exploiting 138 state-level minimum wage changes between 1979-2016. The table reports five year averaged post-treatment estimates on missing jobs up to $4 below the new minimum wage, excess jobs, employment and wages. The different columns explore the robustness of the results to alternative upper end points, $\overline{W}$, for calculating excess jobs. The first column limits the range of the wage window by setting the upper limit for calculating the excess jobs to $\overline{W} = \$2$, and the last column expands it until $\overline{W} = \$6$. All specifications include wage bin-by-state and wage bin-by period fixed effects. Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

*Line-by-line description.*The first two rows report the change in number of missing jobs below the new minimum wage (Δb), and excess jobs above the new minimum wage (Δa) relative to the pre-treatment total employment. The third row, the percentage change in average wages in the affected bins, (%ΔW), is calculated using equation 2 in Section 2.2. The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by jobs below the new minimum wage ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). The fifth row, employment elasticity with respect to the minimum wage is calculated as $\frac{\Delta a+\Delta b}{\%\Delta MW}$ whereas the sixth row, employment elasticity with respect to the wage, reports $\frac{1}{\%\Delta W}\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$. The line on the number of observations shows the number of quarter-bin cells used for estimation, while the number of workers refers to the underlying CPS sample used to calculate job counts in these cells.

Table A.6: Impact of Minimum Wage Increase on the Average Wage and Employment of Affected workers - Robustness to Alternative Time Windows

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Missing jobs below new MW (Δ b) | -0.018*** | -0.021*** | -0.018*** | -0.018*** | -0.018*** |
| | (0.004) | (0.004) | (0.002) | (0.002) | (0.003) |
| Excess jobs above new MW (Δ a) | 0.021*** | 0.021*** | 0.019*** | 0.021*** | 0.019*** |
| | (0.003) | (0.003) | (0.002) | (0.002) | (0.003) |
| %Δ affected wages | 0.068*** | 0.065*** | 0.064*** | 0.068*** | 0.067*** |
| | (0.010) | (0.010) | (0.009) | (0.009) | (0.009) |
| %Δ affected employment | 0.028 | 0.010 | 0.022 | 0.029 | 0.013 |
| | (0.029) | (0.025) | (0.031) | (0.032) | (0.029) |
| Employment elasticity w.r.t. MW | 0.024 | 0.008 | 0.018 | 0.025 | 0.011 |
| | (0.025) | (0.021) | (0.027) | (0.027) | (0.025) |
| Emp. elasticity w.r.t. affected wage | 0.411 | 0.148 | 0.335 | 0.427 | 0.197 |
| | (0.430) | (0.380) | (0.461) | (0.445) | (0.436) |
| Jobs below new MW ($\bar{b}_{-1}$) | 0.086 | 0.086 | 0.086 | 0.086 | 0.086 |
| %Δ MW | 0.101 | 0.101 | 0.101 | 0.101 | 0.101 |
| Number of events | 138 | 138 | 138 | 138 | 138 |
| Number of observations | 847,314 | 847,314 | 847,314 | 847,314 | 847,314 |
| Number of workers in the sample | 4,694,104 | 4,694,104 | 4,694,104 | 4,694,104 | 4,694,104 |
| Time window | [-3, 4] | [-3, 2] | [-3, 6] | [-5, 4] | [-1, 4] |

*Notes.* The table reports the effects of a minimum wage increase based on the event study analysis (see equation 1) exploiting 138 state-level minimum wage changes between 1979-2016. The table reports averaged post-treatment estimates on missing jobs up to $4 below the new minimum wage, excess jobs at and up to $5 above it, employment and wages. The different columns explore the robustness of the results to alternative time windows. The first column reproduces our baseline estimate in Table 1 column 1. Compared to the baseline specification, columns 2 and 3 change the post-treatment period to 2 and 6 years, respectively. Similarly, in columns 4 and 5, we start the pre-treatment window from 5 years and one year prior to the event. All specifications include wage-bin-by-state and wage-bin-by-period fixed effects. Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

*Line-by-line description.*The first two rows report the change in number of missing jobs below the new minimum wage (Δb), and excess jobs above the new minimum wage (Δa) relative to the pre-treatment total employment. The third row, the percentage change in average wages in the affected bins, (%ΔW), is calculated using equation 2 in Section 2.2. The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by jobs below the new minimum wage ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). The fifth row, employment elasticity with respect to the minimum wage is calculated as $\frac{\Delta a+\Delta b}{\%\Delta MW}$ whereas the sixth row, employment elasticity with respect to the wage, reports $\frac{1}{\%\Delta W}\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$. The line on the number of observations shows the number of quarter-bin cells used for estimation, while the number of workers refers to the underlying CPS sample used to calculate job counts in these cells.

Table A.7: Impact of Minimum Wages on Employment and Wages for Card and Krueger Probability Groups - Bunching and Aggregate approaches

| | (1) | (2) | (3) |
|---|---|---|---|
| Panel A: Aggregate | | | |
| %Δ average wage | 0.020*** | 0.007 | -0.001 |
| | (0.007) | (0.006) | (0.004) |
| %Δ employment | 0.008 | 0.006 | 0.000 |
| | (0.009) | (0.005) | (0.002) |
| Employment elasticity wrt wage | 0.435 | N/A | N/A |
| | (0.371) | | |
| Panel B: Bunching | | | |
| %Δ affected wages | 0.073*** | 0.051*** | 0.060* |
| | (0.011) | (0.013) | (0.032) |
| %Δ affected employment | 0.015 | 0.015 | 0.011 |
| | (0.018) | (0.048) | (0.055) |
| Emp. elasticity w.r.t. affected wage | 0.206 | 0.304 | 0.184 |
| | (0.233) | (0.904) | (0.841) |
| Jobs below new MW ($\bar{b}_{-1}$) | 0.358 | 0.104 | 0.027 |
| %Δ MW | 0.102 | 0.102 | 0.101 |
| Number of events | 138 | 138 | 138 |
| Number of observations | 847,314 | 847,314 | 847,314 |
| Group: | High prob. | Middle prob. | Low prob. |

*Notes.* The table reports the wage and employment elasticities with respect to the minimum wage for the high , middle, and the low probability groups using the Card and Krueger predictive model of exposure to minimum wage changes. Both panels A and B are based on the 138 state level events and an event-based approach with five year post-treatment period. Panel A reports the estimates for aggregate employment and wages for the three groups. Panel B reports the estimated employment and wage effect for affected workers using the bunching approach. Regressions are weighted by state averaged population of the relevant demographic group. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table A.8: Impact of Minimum Wage Increase by Pre-Treatment Employment Status: New Entrants and Incumbents

| | | | Matched CPS | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Missing jobs below new MW (Δb) | -0.018*** | -0.023*** | -0.018*** | -0.012*** | -0.005*** |
| | (0.004) | (0.004) | (0.003) | (0.002) | (0.001) |
| Excess jobs above new MW (Δa) | 0.021*** | 0.025*** | 0.018*** | 0.013*** | 0.006*** |
| | (0.003) | (0.004) | (0.002) | (0.002) | (0.001) |
| %Δ affected wages | 0.068*** | 0.073*** | 0.059*** | 0.095*** | 0.019 |
| | (0.010) | (0.011) | (0.013) | (0.020) | (0.013) |
| %Δ affected employment | 0.028 | 0.023 | 0.009 | 0.009 | 0.008 |
| | (0.029) | (0.024) | (0.046) | (0.068) | (0.034) |
| Employment elasticity w.r.t. MW | 0.024 | 0.019 | 0.006 | 0.003 | 0.003 |
| | (0.025) | (0.021) | (0.032) | (0.026) | (0.011) |
| Emp. elasticity w.r.t. affected wage | 0.411 | 0.311 | 0.145 | 0.094 | 0.431 |
| | (0.430) | (0.320) | (0.747) | (0.704) | (1.682) |
| Jobs below new MW ($\bar{b}_{-1}$) | 0.086 | 0.086 | 0.072 | 0.042 | 0.384 |
| %Δ MW | 0.101 | 0.101 | 0.103 | 0.103 | 0.103 |
| Number of events | 138 | 138 | 137 | 137 | 137 |
| Number of observations | 847,314 | 847,314 | 733,941 | 733,941 | 733,941 |
| Number of workers in the sample | 4,694,104 | 4,694,104 | 1,505,192 | 1,373,696 | 131,496 |
| Sample: | All workers | All workers | All matched workers | Incumbents | New entrants |
| Time window: | 5 years | 1 year | 1 year | 1 year | 1 year |

*Notes.* The table reports 1 year post-treatment estimates of employment and wages of the affected bins for all workers (incumbents and new entrants) using state-quarter-wage bin aggregated CPS data from 1979-2016, and matched CPS data from 1980-2016. Incumbent workers are employed in the 4th interview month of CPS, and new entrants are not employed in the 4th interview month. The first column replicates column 1 in Table 1 for comparability. The second column includes all workers in the primary CPS sample and employs the baseline specification, but reports only the first year effects. The third and fourth columns use matched CPS and consider only the first year effects on incumbent, and new-entrant workers. Specifications include wage bin-by-state, wage bin-by period, and state-by-period fixed effects. Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

*Line-by-line description.*The first two rows report the change in number of missing jobs below the new minimum wage (Δb), and excess jobs above the new minimum wage (Δa) relative to the pre-treatment total employment. The third row, the percentage change in average wages in the affected bins, (%ΔW), is calculated using equation 2 in Section 2.2. The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by jobs below the new minimum wage ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). The fifth row, employment elasticity with respect to the minimum wage is calculated as $\frac{\Delta a+\Delta b}{\%\Delta MW}$ whereas the sixth row, employment elasticity with respect to the wage, reports $\frac{1}{\%\Delta W}\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$. The line on the number of observations shows the number of quarter-bin cells used for estimation, while the number of workers refers to the underlying CPS sample used to calculate job counts in these cells.

Table A.9: Robustness of the Relationship Between Employment Changes and the Minimum-to-Median Wage Ratio (Kaitz Index) Across Events

| | Jobs below new MW ($\bar{b}_{-1}$) | | Missing jobs ($\Delta b$) | | Excess jobs ($\Delta a$) | | Employment change ($\Delta a + \Delta b$) | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Panel A: Main estimates | | | | | | | | |
| Minimum-to-median ratio | 0.314*** | 0.361*** | -0.094*** | -0.133*** | 0.143*** | 0.139** | 0.050 | 0.006 |
| | (0.063) | (0.056) | (0.032) | (0.034) | (0.048) | (0.057) | (0.043) | (0.048) |
| Panel B: With D.C. | | | | | | | | |
| Minimum-to-median ratio | 0.312*** | 0.358*** | -0.075** | -0.111*** | 0.149*** | 0.148** | 0.074 | 0.037 |
| | (0.061) | (0.055) | (0.035) | (0.037) | (0.048) | (0.057) | (0.049) | (0.055) |
| Panel C: Unweighted | | | | | | | | |
| Minimum-to-median ratio | 0.275*** | 0.286*** | -0.112*** | -0.128*** | 0.142*** | 0.134*** | 0.031 | 0.006 |
| | (0.035) | (0.035) | (0.024) | (0.026) | (0.037) | (0.041) | (0.038) | (0.042) |
| *Number of observations* | | | | | | | | |
| Panels A, C | 130 | 130 | 130 | 130 | 130 | 130 | 130 | 130 |
| Panel B | 138 | 138 | 138 | 138 | 138 | 138 | 138 | 138 |
| Controls | | Y | | Y | | Y | | Y |

*Notes.* The table reports the effect of the minimum-to-median wage ratio (Kaitz index) on four outcomes: jobs below the new minimum wage, missing jobs, excess jobs, and the total employment change. The minimum-to-median wage ratio is the new minimum wage divided by the state-level median wage. Odd columns reports simple linear regression estimates. Even columns include the controls in Table A.9. Regressions are weighted by state-populations. Robust standard errors are in parentheses; significance levels are * 0.10, ** 0.05, *** 0.01.

Table A.10: Employment Elasticities of Minimum Wage from Alternative Approaches

| | Continuous treatment - ln(MW) | | | | Event based | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Fixed Effects | First Difference | Fixed Effects | First Difference | | | |
| Panel A: Overall | | | | | | | |
| Employment elasticity wrt MW | -0.089*** | 0.027 | -0.020 | -0.005 | 0.016 | 0.027 | 0.024 |
| | (0.025) | (0.031) | (0.028) | (0.019) | (0.029) | (0.022) | (0.025) |
| Panel B: Teen | | | | | | | |
| Employment elasticity wrt MW | -0.238*** | 0.094 | -0.210** | 0.080 | 0.163 | 0.152 | 0.125 |
| | (0.088) | (0.122) | (0.091) | (0.120) | (0.115) | (0.107) | (0.134) |
| Aggregate | Y | Y | | | Y | | |
| Under $15 | | | Y | Y | | Y | |
| $[MW–\$4, MW+\$5)$ | | | | | | | Y |
| Data aggregation | State-year | State-year | State-year | State-year | State-year | State-year | Wage-bin-state-quarter |

*Notes.* The table reports estimated overall (panel A) and teen (panel B) employment elasticities of minimum wage from alternative approaches. All columns show average post-treatment elasticities calculated from regressions of state-level employment to population rate on contemporaneous and 4 annual lags and 2 annual leads of log minimum wages. We use state-by-year aggregated CPS data from 1979-2016. Columns (1) and (3) estimate two-way (state and year) fixed effect regresions, while in columns (2) and (4) we employ first differences. Column (3) and (4) exclude workers with hourly wages greater than $15. Columns (5)-(7) report estimates employment elasticities using an event study framework where we exploit the same 138 events as in our benchmark specifications. In column (5), we use state by quarter aggregated CPS data. In column (6) we directly estimate the effect of the minimum wage on jobs below $15. We refer to this specification as simpler method in Section 4.2., since it directly estimate the sum of missing and excess jobs. Finally, column (7) shows estimates from the bunching approach (same as in column 1 of Table 1, and column 3 of Table 2). In all cases we show estimates with and without population weighting. Standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

# Online Appendix B
# Washington State Case Study

In this Appendix, we report estimates using administrative data on hourly wages for a case study of a large state-level minimum wage increase. The state of Washington increased its real hourly minimum wage by around 22% from \$7.51 to \$9.18 (in 2016 dollars) in two steps between 1999 and 2000. Moreover, this increase in the real minimum wage was persistent, since subsequent increases were automatically indexed to the rate of inflation. In addition to the size and permanence of this intervention, Washington is an attractive case study because it is one of the few states with high quality administrative data on hourly wages.[39] Using hourly wage data, we can easily calculate the actual post-reform wage distribution (blue line in Figure 1). However, the key challenge implementing the bunching method is that we do not directly observe the wage distribution in the absence of the minimum wage increase (red line in Figure 1). To overcome this challenge, the previous literature constructed the counterfactual by imposing strong parametric assumptions (Meyer and Wise 1983) or simply used the pre-reform wage distribution as a counterfactual (Harasztosi and Lindner 2016).[40] Here we improve upon these research designs by implementing a difference-in-differences style estimator.

In particular, we discretize the wage distribution, and count per-capita employment for each dollar wage bin $k$. For example, the \$10 wage bin includes jobs paying between \$10 and \$10.99 in 2016\$. We normalize these counts by the pre-treatment employment-to-population rate in Washington,

$$e_{WA,k,Post} = \frac{1}{\frac{E_{WA,Pre}}{N_{WA,Pre}}} \frac{E_{WA,k,Post}}{N_{WA,Post}}$$

where $\frac{E_{WA,k,t}}{N_{WA,t}}$ is per-capita employment for each dollar wage bin $k$ in state Washington at time $t$, and $N_{WA,t}$ is the size of the population. We use administrative data on hourly wages from Washington State to calculate $e_{WA,k,Post}$.

We calculate the post treatment counterfactual wage distribution for each wage bin, $e^{CF}_{WA,k,Post}$, by adding the (population-weighted) average per capita employment change in the 39 states that did not experience a minimum wage increase during the 1998-2004 time period to the Washington state's pre-treatment per-capita wage distribution. After the appropriate normalization, this leads to the following expression:

[39] The state of Washington requires all employers, as part of the state's Unemployment Insurance (UI) payroll tax requirements, to report both the quarterly earnings and quarterly hours worked for all employees. The administrative data covers a near census of employee records from the state. One key advantage of the bunching method proposed here is that there is no need for confidential or sensitive individual-level data for implementation. Instead, we rely here on micro-aggregated data on employment counts for 5-cent hourly wage bins. Workers with hourly wages greater than \$50 are censored for confidentiality purposes. We deflate wages to 2016 dollars using the CPI-U-RS.

[40] As shown in Dickens, Machin and Manning (1998), estimates using the Meyer and Wise 1983 approach is highly sensitive to the parameterization of the wage distribution.

$$e^{CF}_{WA,k,Post} = \underbrace{\frac{1}{\frac{E_{WA,Pre}}{N_{WA,Pre}}}}_{\text{normalization}} \times \left[ \underbrace{\frac{E_{WA,k,Pre}}{N_{WA,Pre}}}_{\substack{\text{Pre-treament} \\ \text{in WA}}} + \underbrace{\sum_{s \in Control} \frac{1}{39} \left( \frac{E_{s,k,Post}}{N_{s,Post}} - \frac{E_{s,k,Pre}}{N_{s,Pre}} \right)}_{\substack{\text{Change in control} \\ \text{states}}} \right]$$

where $\frac{E_{skt}}{N_{s,t}}$ is per-capita employment for each dollar wage bin $k$ in state $s$ at time $t$, and $N_{st}$ is the size of the population (age 16 or over) in state $s$ at time $t$. To calculate the third part of this expression, the change in control states, we use hourly wage data from the Outgoing Rotation Group of the Current Population Survey (CPS). We will discuss the data in more detail in Section 2.3. For the second part of the expression, the pre-treatment Washington wage distribution, we use administrative data on hourly wages. However, in Appendix Figure B.4 we show that when we use the CPS, we get very similar results. Finally, the first part of this expression, the normalization, is to express the counterfactual employment counts in terms of pre-treatment total employment in Washington. It is worth highlighting that our normalization does not force the area below the counterfactual wage distribution to be the same as the area below the actual wage distribution—in other words, the minimum wage can affect aggregate employment.

In Figure B.1, panel (a) we report the actual (blue filled bar) and the counterfactual (red empty bars) frequency distributions of wages, normalized by the pre-treatment total employment in Washington. We define the pre-treatment period as 1996-1998, and the post-treatment period as 2000-2004. The post-treatment actual wage distribution in Washington state (blue filled bars) shows that very few workers earn less than the mandated wage, and there is a large spike at the new minimum wage at \$9. The post-treatment counterfactual distribution differs considerably. That distribution indicates that in the absence of the minimum wage increase, there would have been more jobs in the \$7 and \$8 bins, but fewer jobs at the \$9 bin and above. Compared to the counterfactual wage distribution, the actual distribution is also elevated \$1 and \$2 above the minimum wage, which suggests that minimum wages induce some modest spillover effects. At the same time, the ripple effect of the minimum wage fades out above \$12, and no difference is found between the actual and counterfactual distribution above that point. Such a relationship between the actual and counterfactual distributions closely resembles the illustration of the bunching method shown in Figure 1.

The difference between the actual, $e_{WA,k,Post}$, and the counterfactual, $e^{CF}_{WA,k,Post}$, frequency distributions of wages represents the causal effect of the minimum wage on the wage distribution. This difference can be expressed as:

$$e_{WA,k,Post} - e^{CF}_{WA,k,Post} = \underbrace{\frac{1}{\frac{E_{WA,Pre}}{N_{WA,Pre}}}}_{\text{normalization}} \times \Bigg[ \underbrace{\frac{E_{WA,k,Post}}{N_{WA,Post}} - \frac{E_{WA,k,Pre}}{N_{WA,Pre}}}_{\text{Change in treatment}} - \underbrace{\sum_{s \in Control} \frac{1}{39} \left( \frac{E_{WA,k,Post}}{N_{s,Post}} - \frac{E_{WA,k,Pre}}{N_{s,Pre}} \right)}_{\text{Change in control}} \Bigg] \tag{B.5}$$

which is the classic difference-in-differences estimator underlying the core estimates in the paper. Standard errors are calculated using the procedure proposed by Ferman and Pinto (forthcoming). Appropriate for a single treated unit, their procedure extends the cluster residual bootstrap by correcting for sample-size based heteroskedasticity—an important issue given the very different sample sizes across states in the CPS, and because Washington is based on administrative data.

The blue bars in Panel (b) of Figure B.1 report the differences in job counts for each wage bin. The difference-in-differences estimate shows a clear drop in counts for wage bins just below the new minimum wage. In the upper part of the table we report our estimate of missing jobs, $\triangle b$, which is the sum of employment changes, $\sum_{k=\$5}^{\$8} e_{WA,k,Post} - e^{CF}_{WA,k,Post}$, between \$5 and \$8—i.e., under the new minimum wage. These missing jobs paying below \$9 represent around 4.6% of the aggregate pre-treatment Washington employment. We also calculate the number of excess jobs paying between \$9 and \$13, $\triangle a$, which is equal to $\sum_{k=\$9}^{\$13} e_{WA,k,Post} - e^{CF}_{WA,k,Post}$. The excess jobs represent around 5.4% of the aggregate pre-treatment Washington employment.

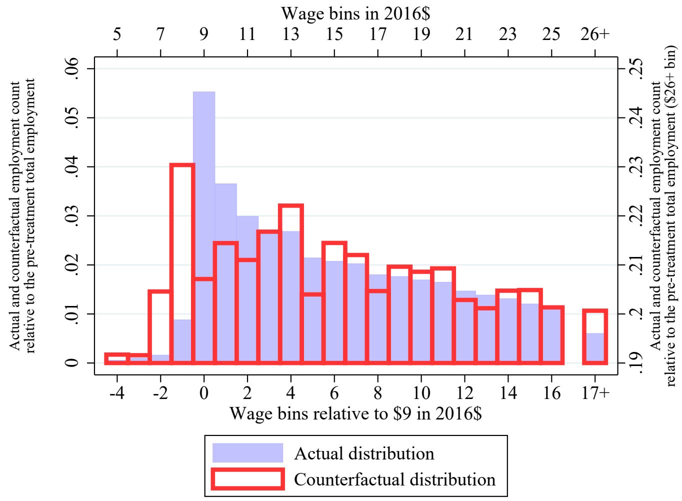
As we explained in the previous section, the effect of the minimum wage on low-wage jobs is equal to the sum of the missing jobs below and the excess jobs above the new minimum wage of \$9. We find that the net employment change is positive—the increase amounted to 0.8% of the pre-treatment aggregate employment in Washington. This reflects a 6.1% (s.e. 10.9%) increase in employment for the workers who earned below the new minimum wage in 1998. We also find that average wages of affected workers at the bottom of the wage distribution increased by around 9.0% (s.e.18.8%) Coming from a single case study, the precision of these estimates is much lower than in the pooled event study estimates presented in the main paper.

In Panel (b) of Figure 2, the red line shows the running sum of employment changes up to each wage bin. The running sum drops to a sizable, negative value just below the new minimum wage, but returns to around zero once the minimum wage is reached. By around \$2 above the minimum wage, the running sum reaches a small positive value and remains flat thereafter—indicating little change in upper tail employment. This strengthens the case for a causal interpretation of these results.

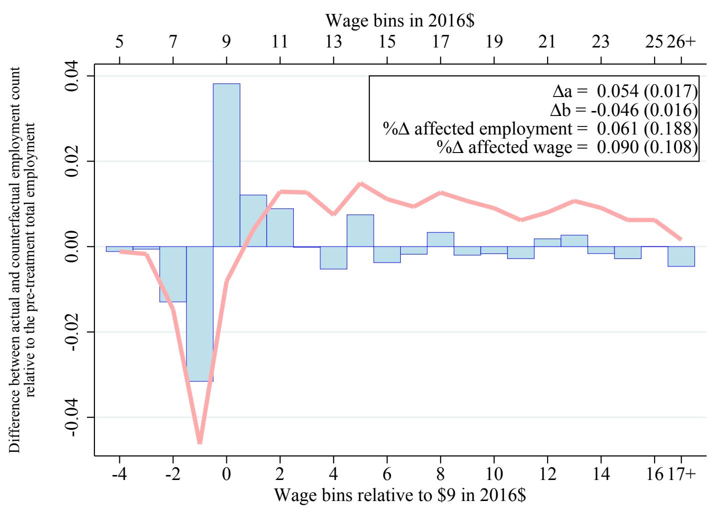
Finally, we also explore the evolution of missing jobs (red line) and excess jobs (blue line) over time in panel (a) in Online Appendix Figure B.3. The figure shows that excess and missing jobs are close to zero before 1999, and there are no systematic pre-existing trends.[41] Once the minimum wage is raised in two steps between 1999 and 2000, there is a clear and sustained drop in jobs below the new minimum wage (relative to the counterfactual). Since the minimum wage is indexed to inflation in Washington, the persistence of the drop is not surprising. The evolution of excess jobs after 2000 closely matches the evolution of missing jobs. As a result, the net employment change—which is the sum of missing and excess jobs—is close to zero in all years following the minimum wage increase (see panel (b) in Figure B.3).

[41] There is a one-time, temporary, drop in excess jobs and an increase in missing jobs in 1996, which likely reflects the fact that the 1996 federal minimum increase from $4.25 to $4.75 only affected control states, since Washington's minimum wage was already at $4.90 (in current dollars). However, the 1997 federal minimum wage increase to $5.15 affected both Washington and controls states and hence restored the difference in excess and missing jobs prior to Washington's state minimum wage increase in 1999 and 2000.

Figure B.1: Employment by Wage Bins in Washington between 2000-2004



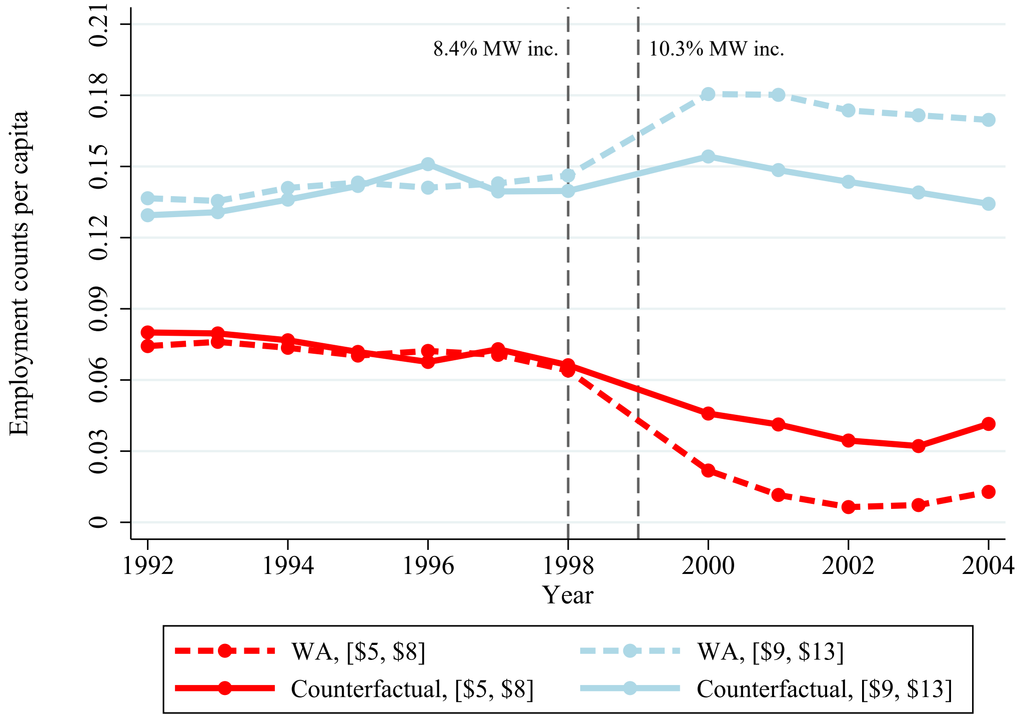

(a) The actual and counterfactual frequency distribution of wages




(b) The difference between the actual and counterfactual frequency distribution of wages

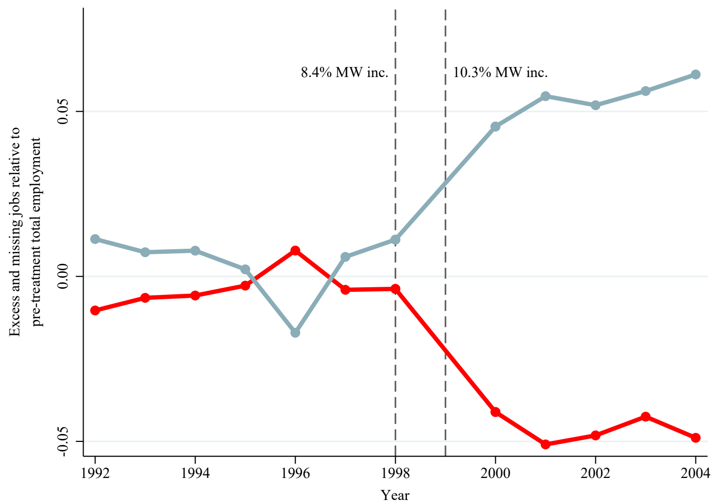
*Notes*: We examine the effect of the 1999-2000 minimum wage change in Washington state on the frequency distribution of wages (aggregated in $1 bins), normalized by the 1998 level of employment in Washington. The minimum wage was raised from $7.51 to $9.18 (in 2016 values) and it was indexed by inflation afterwards. Panel (a) shows the actual (purple solid bars) and counterfactual (red outlined bars) wage frequency distribution after the minimum wage increases in Washington. The actual distribution (post treatment) plots the average employment between 2000 and 2004 by wage-bin relative to the 1998 total employment in Washington using administrative data on hourly wages between 2000-2004. The counterfactual distribution adds the average change in employment between 2000 and 2004 in states without any minimum wage change to the mean 1996-1998 job counts (see the text for details). The $26+ bin (the bin that is $17+ above the new minimum wage) contains all workers earning above $26, and its values shown on the right y-axis. Panel (b) depicts the difference between the actual and the counterfactual wage distribution. The blue bars show the change in employment at each wage bin (relative to the 1998 total employment in Washington). The red line shows the overall employment changes up to that wage bin. The upper right panel shows the estimates on missing jobs below $9, $\triangle b$; on the excess jobs between $9 and $13, $\triangle a$, and on the estimated employment and wage effects. The standard errors are calculated using the method proposed by Ferman and Pinto (forthcoming).

Figure B.2: Comparison of Per-capita Employment Counts of Washington and the Counterfactual

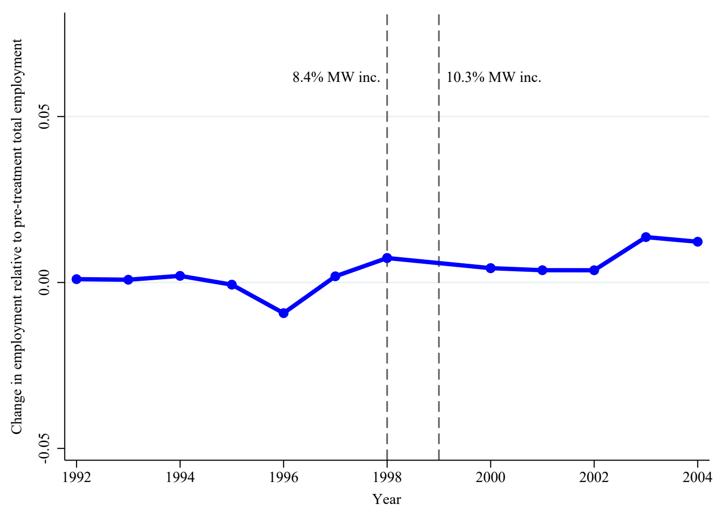



*Notes*: The figure shows the evolution of the number of jobs per capita with hourly wages between $5 and $8, and $9 and $13 in Washington and in the counterfactual, with data aggregated in $1 bins. The counterfactual jobs are calculated using states without any minimum wage change during the 1998-2004 time period. In particular, we add the average change in per capita employment between $5 and $8 (and between $9 and $13) in the control states to the mean 1996-1998 job counts in Washington state (see the text for details). The two vertical dashed black lines at 1998 and 1999 show the that the minimum wage was raised in 1999 and 2000 in two steps from from $7.51 to $9.18 (in 2016 values). The minimum wage was indexed to inflation after 2001. We exclude all observations with imputed wages in the CPS in forming the counterfactual employment counts, except for years 1994 and 1995. Since determining imputed wages is not possible for those years, we use all observations in 1994 and 1995.

Figure B.3: Impact of Minimum Wages on Missing and Excess Jobs, and Employment Change Over time in the Washington Case Study



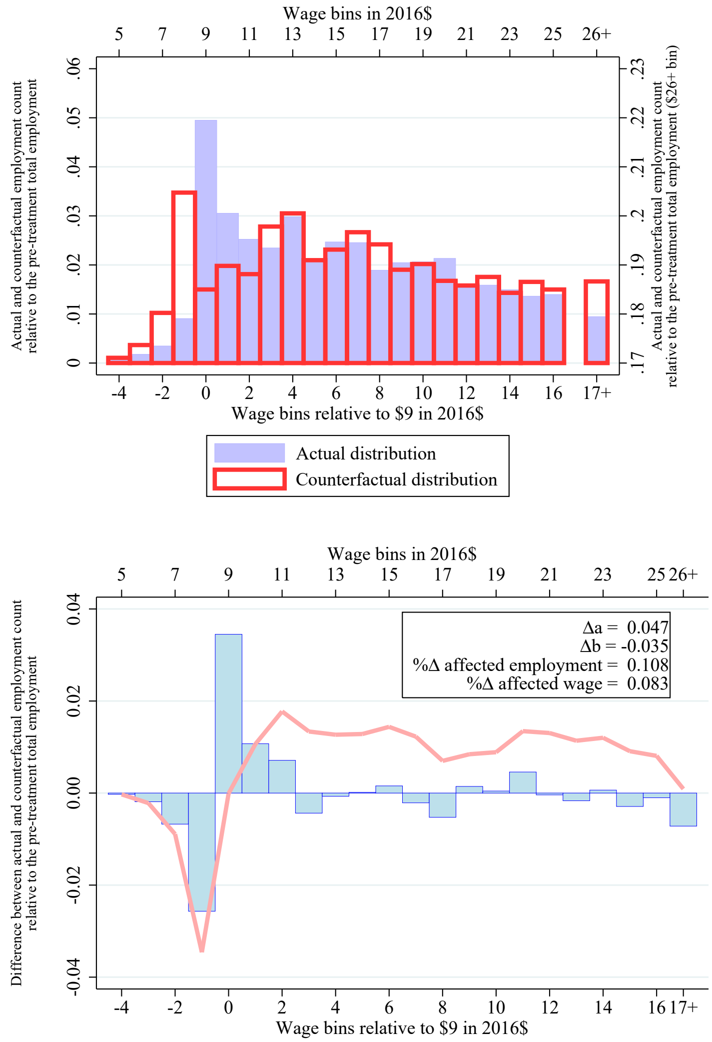

(a) Missing and excess jobs over time




(b) Employment change over time

*Notes*: The figure shows the evolution of missing jobs, excess jobs, and total employment change over time in Washington state, with data aggregated in $1 bins. In Panel (a), the red line represents the missing jobs—the difference between the actual and counterfactual wage distribution between $5 and $8; while the light blue line shows the excess jobs that is the difference between the actual and counterfactual frequency distributions for wages between $9 and $13. In Panel (b), we report the employment change over time (the sum of excess jobs and missing jobs). The counterfactual distribution is calculated by adding the average job change in the control states to the mean 1996-1998 job counts in Washington (see the text for details). The two vertical dashed black lines at 1998 and 1999 show the that the minimum wage was raised in 1999 and 2000 in two steps from from $7.51 to $9.18 (in 2016 values). The minimum wage was indexed to inflation after 2001. We exclude all observations with imputed wages in the CPS in forming the counterfactual employment counts, except for years 1994 and 1995. Since determining imputed wages is not possible for those years, we use all observation in 1994 and 1995.

Figure B.4: Employment by Wage Bins in Washington between 2010-2004 (Replication of Figure B.1 using CPS data)




*Notes*: The figure replicates Figure B.1 that examine the effect of the 1999-2000 minimum wage change in Washington on the frequency distribution of wages (aggregated in $1 bins), normalized by the 1998 level of employment in Washington. The minimum wage was raised from $7.51 to $9.18 (in 2016 values) and it was indexed by inflation. Panel (a) shows the actual (purple solid bars) and counterfactual (red outlined bars) frequency wage distribution after the minimum wage increases in Washington. The actual distribution (post treatment) plots the average employment between 2000 and 2004 by wage-bin relative to the 1998 total employment in Washington using CPS data on hourly wages between 2000-2004 (instead of using administrative data as in Figure B.1. The counterfactual distribution adds the average change in employment between 2000 and 2004 in states without any minimum wage change to the mean 1996-1998 job counts (see the text for details). The 26+ bin contains all workers earning above $26, and its values shown on the right y-axis. Panel (b) depicts the difference between the actual and the countefactual wage distribution. The blue bars shows the change in employment at each wage bin (relative to the 1998 total employment in Washington). The red line shows the overall employment changes up to that wage bin. The upper left panel shows the estimates on missing number of jobs between $5 and $8, $\triangle b$; on the excess number of jobs between $9 and $13, $\triangle a$, and on the estimated employment and wage effects.

# Online Appendix C
# Event-by-event analysis

While the baseline estimates in this paper are an average effect across 138 events estimated by equation (1), our event-by-event analysis estimates separate treatment effects for each of the events. To do so, we first create event-specific annual state panel datasets using the same real wage bin-state-specific employment counts as before. Then we calculate event-specific estimates using separate regressions for each event.

Each event $h$-specific dataset includes the treated state and all other clean control states for an 8-year panel by event time ($t = -3, ..., 4$) with the minimum wage increase at $t = 0$. Clean controls are those without any non-trivial minimum wage increase within the 8-year event window. With these data we calculate event-specific per-capita state outcomes over time $Y_{sth}$: missing jobs $b_{sth}$, between the new minimum and \$4.00 below; excess jobs $a_{sth}$, between the minimum and \$4.00 above; total affected employment $e_{sth} = a_{sth} + b_{sth}$; and upper tail jobs more than \$4.00 above the new minimum. For each event, we have a similar regression equation to the one used in our baseline estimates

$$Y_{sth} = \sum_{\tau=-3}^{4} \alpha_{\tau hk} I_{sth}^{\tau} + \mu_{sh} + \rho_{th} + \Omega_{sth} + u_{sth}, \tag{C.6}$$

where $\Omega_{jsh}$ is an indicator that controls for other primary, federal, and small events whose 5-year post-treatment periods take place within the data set $h$. ($\Omega_{sth} = 1$ for post-treatment periods of these events.) Just like our baseline estimates, we calculate the event-specific change in excess jobs above ($\Delta a_j$), change in missing jobs below ($\Delta b_h$), and employment change ($\Delta e_h = \Delta a_h + \Delta b_h$) relative to the first year prior to treatment. For instance, the change in the excess number of jobs is given by $\Delta a_h = \frac{1}{5}\sum_{\tau=0}^{4} \Delta a_{h\tau} = \frac{1}{5}\sum_{\tau=0}^{4} \frac{\sum_{k=0}^{4} \alpha_{h,\tau} - \sum_{k=0}^{4} \alpha_{h,-1}}{EPOP_{-1}}$.

Figure C.1 shows the resulting estimated employment changes for each event, along with 95% confidence intervals obtained according to the procedure proposed by Ferman and Pinto (forthcoming). Appropriate for a single treated unit, their procedure extends the cluster residual bootstrap by correcting for heteroskedasticity—an important issue given the very different sample sizes across states in the CPS. Given the very small sample sizes for Washington D.C. in the CPS, we exclude these minimum wage increases from the event-by-event analysis, for a total of 130 events. The figure shows estimates for missing, excess, and total employment changes, where filled in markers represent statistically significant employment changes at the 5 percent level. There is clear evidence of sizable but heterogeneous bites across events: 83% (108) of the missing jobs estimates are negative, and 25% (32) of the events are statistically significant at the 5 percent level. At the same time 21% (27) of the excess jobs estimates are statistically significant, while 78% (100) are positive in sign. Therefore, while there is considerable heterogeneity in the bite of the policy, the distribution of employment estimates is consistent with the sharp null of zero effect everywhere: only 7 (or 5.3%) of events yield statistically significant overall employment changes: 1 is negative and 6 are positive, and the median estimate is very close to zero.

We can also use the event-by-event estimates to assess whether the lack of leading effects and upper tail employment changes hold event-by-event, and not just on average. Figure

C.2 shows leading and upper tail employment changes for 129 events; here one event from Connecticut in 1981 is dropped because it lacks a third leading term. Only 5.4% (7) of the events experience a statistically significant upper tail effects at the 5 percent level, while 7.7% (10) the events experience statistically significant leading effects. Overall, these results are reassuring as they show that the lack of upper tail or leading effects in aggregate is not driven by a mix of unusual positive and negative individual effects. Rather, our findings are consistent with the sharp null of zero upper tail and zero leading effects everywhere.

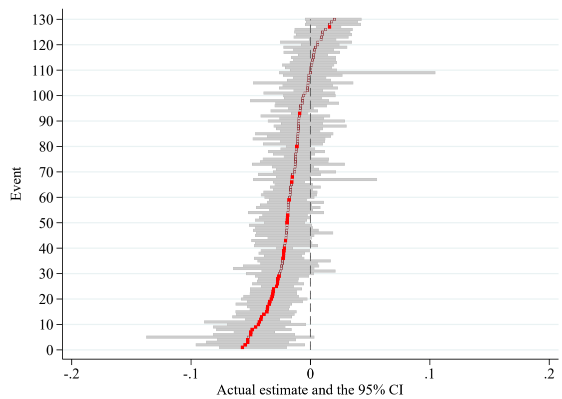
We also stack all of the event-specific data to calculate an average effect across all the events using the a single set of treatment effects $\alpha_{\tau k}$

$$Y_{hkst} = \sum_{\tau=-3}^{4} \alpha_{\tau k} I_{hst}^{\tau} + \mu_{hks} + \rho_{hkt} + \Omega_{hkst} + u_{hkst}. \tag{C.7}$$

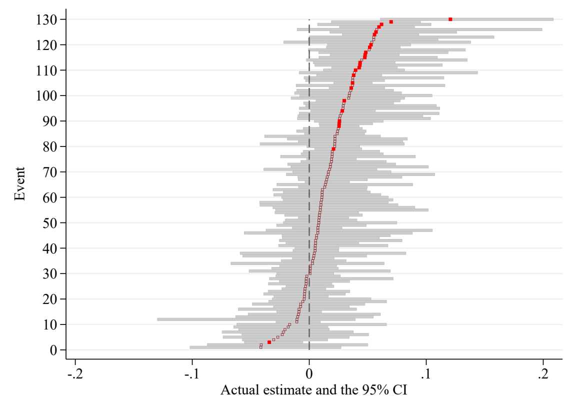
This provides an alternative to our baseline panel specification that uses a more stringent criteria for admissible control groups, and is not more robust to possible problems with a staggered treatment design in presence of heterogeneous treatment effects. In particular, by aligning events by event-time (and not calendar time), it is equivalent to a setting where the events happen all at once and are not staggered; this prevents negative weighting of some events that may occur with a staggered design (Abraham and Sun, 2018). Moreover, by dropping all states with any events within the 8 year event window, we further guard against bias due to heterogeneous treatment effects. Moving to the stacked-by-event approach (column 2 in Table C.1) continues to produce a sizable and statistically significant positive wage effect, but an employment effect that is statistically indistinguishable from zero. The minimum wage employment elasticity using the stacked-by-event approach (column 2) is 0.001 (s.e. 0.002) which is fairly similar to the estimate of 0.024 (0.25) in the baseline panel specification (column 1). The own wage elasticity is 0.018 (s.e. 0.546) in column 2 as opposed to 0.411 (s.e., 0.430) in the baseline column 1; here the more stringent stacked-by-event approach is somewhat less precise, though it still rules out an own wage elasticity more negative than -0.88 at the 90% confidence level. The time paths for missing jobs, excess jobs and employment are reported in Figure C.3; again these are quite similar to the baseline Figures 3 and A.4 and show a sharp and persistent change in missing and excess jobs on the event date, and a flat employment time path before and after the event.

In column (3) we consider the case were we manually average the 138 estimates where each event is weighted by population. The point estimates are very similar to column (2), providing further assurance against the problematic (e.g., negative) implicit weights in the panel estimate. Finally, in column (4) we further refine the sample by only considering events that have a full five year post-treatment sample (i.e., events that occurred on 2012q1 or earlier). The point estimates are quite close to column (2), even though, as expected, the standard errors are now somewhat larger. This shows that the small size of our estimates in columns (1) - (3) is not driven by a lack of a sufficiently long post-treatment period in some of the events.

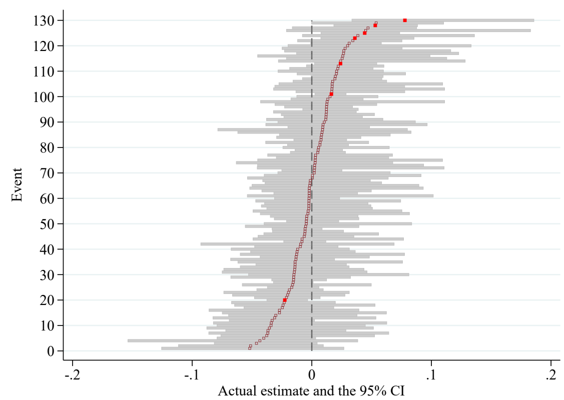
Figure C.1: Event-specific Excess Jobs Above, Missing Jobs Below, and Employment Change Estimates




(a) Missing jobs below ($\Delta b$)



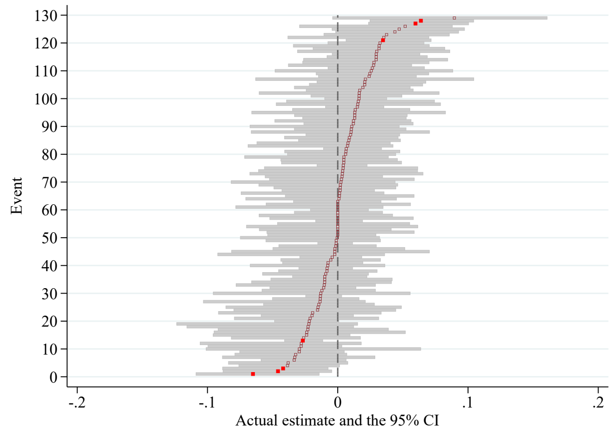

(b) Excess jobs above ($\Delta a$)



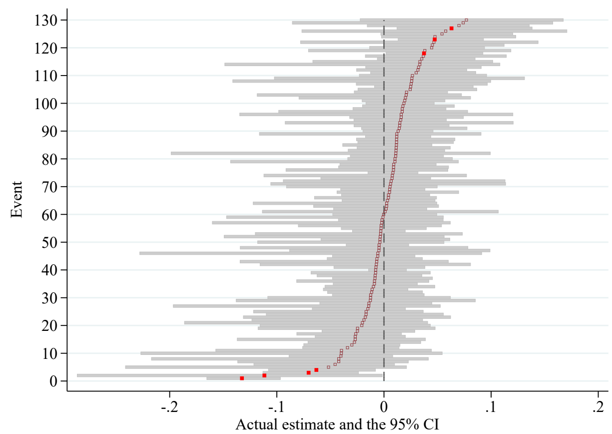

(c) Employment change ($\Delta a + \Delta b$)

*Notes*: The figure shows the event-specific point (square markers) and confidence interval (gray horizontal bars) estimates for missing jobs below ($\Delta b$), excess jobs above ($\Delta a$), and employment change ($\Delta a + \Delta b$). The point estimates are calculated using equation C.6, and the confidence intervals are obtained according to the procedure proposed by Ferman and Pinto (forthcoming). The vertical gray dash line indicates the null hypothesis of no effect, and it is rejected with 95% confidence if the confidence intervals do not contain 0. There are 130 events (D.C. events are dropped due to the measurement error concerns). 44/130, 25/130, and 7/130 events yield statistically significant estimates (filled markers) for missing jobs below, excess jobs above, and employment change.

Figure C.2: Leading estimates and upper tail falsification tests for event-specific estimates



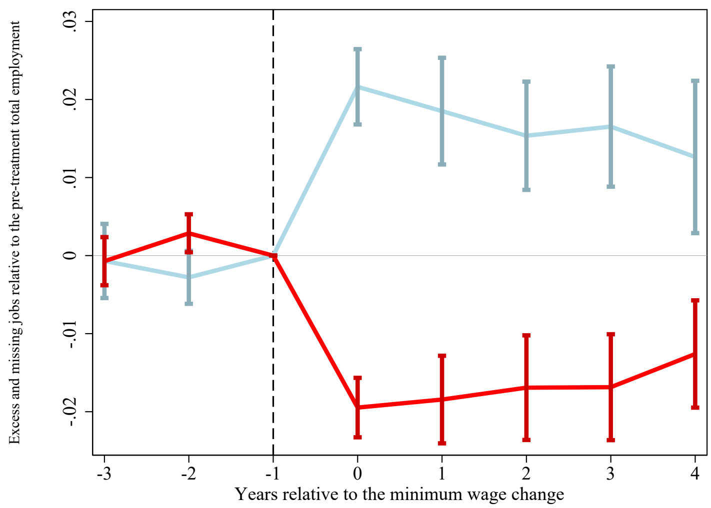

(a) Leading employment change $(\Delta a_{-3} + \Delta b_{-3})$



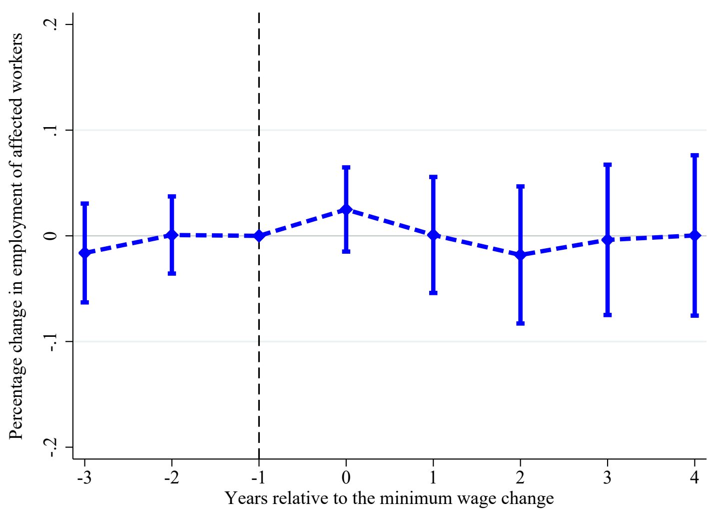

(b) Upper tail $(\frac{\sum_{\tau=0}^{\tau=4}\sum_{k\geq 5}\alpha_{\tau k}-\alpha_{-1k}}{\overline{EPOP}_{-1}})$

*Notes*: The figure shows the event-specific point (square markers) and confidence interval (gray horizontal bars) estimates for leading $(\Delta a_{-3} + \Delta b_{-3})$, and upper tail $(\frac{\sum_{\tau=0}^{\tau=4}\sum_{k\geq 5}\alpha_{\tau k}-\alpha_{-1k}}{\overline{EPOP}_{-1}})$ employment change. The point estimates are calculated using equation C.6, and the confidence intervals are obtained according to the procedure proposed by Ferman and Pinto (forthcoming). The vertical gray dash line at 0 indicates the null hypothesis of no effect, and it is rejected with 95% confidence if confidence intervals do not contain 0. There are 129 events (D.C. events are dropped due to the measurement error concerns and the minimum wage event that takes place in Connecticut in 1981 does not have the third leading term.). 7/129, and 7/129 events yield statistically significant estimates (filled markers) for leading, and upper tail employment change.

Figure C.3: Impact of Minimum Wages on the Missing and Excess Jobs, and Employment Over Time; Stacked Analysis Missing and Excess Jobs




(a) Evolution of the missing and excess jobs




(b) Evolution of the employment of the affected workers

*Notes*: The figure shows the main results from our stacked analysis (see equation 7) exploiting 138 state-level minimum wage changes between 1979-2016. Panel (a) shows the effect of a minimum wage increase on the missing jobs below the new minimum wage (blue line) and on the excess jobs at and slightly above it (red line) over time. The blue line shows the evolution of the number of jobs (relative to the total employment 1 year before the treatment) between \$4 below the new minimum wage and the new minimum wage ($\Delta b$); and the red lines show the number of jobs between the new minimum wage and \$5 above it ($\Delta a$). Panel (b) shows the evolution of employment between \$4 below the new minimum wage and \$5 above it (relative to the total employment 1 year before the treatment), which is equal to the sum of missing jobs below and excess jobs at and slightly above the minimum wage, $\Delta b + \Delta a$. We also show the 95% confidence intervals based on standard errors that are clustered at the state level.

Table C.1: Stacked Data Estimates

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Missing jobs below new MW (Δb) | -0.018*** | -0.017*** | -0.017 | -0.015*** |
| | (0.004) | (0.002) | (0.002) | (0.003) |
| Excess jobs above new MW (Δa) | 0.021*** | 0.017*** | 0.019 | 0.015*** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| %Δ affected wages | 0.068*** | 0.048*** | 0.060 | 0.042*** |
| | (0.010) | (0.012) | (0.014) | (0.013) |
| %Δ affected employment | 0.028 | 0.001 | 0.022 | -0.001 |
| | (0.029) | (0.026) | (0.041) | (0.030) |
| Employment elasticity w.r.t. MW | 0.024 | 0.001 | 0.019 | -0.001 |
| | (0.025) | (0.022) | (0.035) | (0.024) |
| Emp. elasticity w.r.t. affected wage | 0.411 | 0.018 | 0.367 | -0.017 |
| | (0.430) | (0.546) | (0.613) | (0.713) |
| Jobs below new MW ($\bar{b}_{-1}$) | 0.086 | 0.086 | .086 | 0.086 |
| %Δ MW | 0.101 | 0.101 | .101 | 0.108 |
| Number of events | 138 | 138 | 138 | 98 |
| Number of observations | 847,314 | 983,934 | 983,934 | 838,584 |
| Set of events | Primary | Primary | Primary | Primary, until 2012q1 |
| Data | All workers, state-by-wage-bin | All workers, stacked | All workers, stacked | All workers, stacked |
| Specification | Baseline | Pooled stacked | Manual averaging | Pooled stacked |

*Notes.* The table reports the effects of a minimum wage increase based on the event study analysis (see equation 1) and alternative variants of stacked analysis (see equations 6 and 7) exploiting 138 state-level minimum wage changes between 1979 and 2016. The table reports five year averaged post-treatment estimates on missing jobs up to \$4 below the new minimum wage, excess jobs at and up to \$5 above it, employment and wages. The first column reproduces column (1) of Table 1 for comparison purposes. Column (2) uses equation 7. Column (3) uses equation 6 and manually averages each event-by-event estimates. Column (4) uses the same regression equation as column (2), but uses only events that have occurred on or before 2012q1 to ensure a full five year post-treatment sample. Robust standard errors in parentheses are clustered by event-by-state in columns (1), (2), and (4). In column (3), we employ the procedure proposed by Ferman and Pinto (forthcoming) to obtain the standard errors. Significance levels are * 0.10, ** 0.05, *** 0.01.

# Online Appendix D
# Data Appendix

The primary data set we use in the event study analysis is the individual-level NBER Merged Outgoing Rotation Group of the Current Population Survey for 1979-2016 (CPS). We use variables EARNHRE (hourly wage), EARNWKE (weekly earnings), and UHOURSE (usual hours) to construct our hourly wage variable. For the period after 1995q4, we exclude observations with imputed hourly wages (I25a>0) among those with positive EARNHRE values, and exclude observations for which usual weekly earnings or hours information is imputed (I25a>0 or I25d>0) among those with positive EARNWKE values. There is no information on the imputation between 1994q1 and 1995q3 so we exclude these observations entirely. For the years 1989-1993, we follow the methodology of Hirsch and Schumacher (2004) to determine imputed observations.

The CPS is a survey, where only a subset of workers is interviewed each month; therefore, there is sampling error in the dataset. In addition, as we do not use observations with imputed hourly wages in most of our analysis, the employment counts of the raw CPS data are biased downwards. To reduce the sampling error and also address the undercounting due to dropping imputed observations, our primary sample combines the CPS wage densities with the true state-level employment counts from the QCEW ($E$). Specifically, in the QCEW benchmarked CPS, the employment counts for a wage bin $w$ is calculated as $\widehat{\frac{E_w}{N}}^{QCEW} = \widehat{f_w}^{CPS} \times \frac{E}{N}$, where $\widehat{f_w}^{CPS}$ is the (discretized) wage density estimated using the CPS: $\hat{f_w}^{CPS} = Prob(w \leq wage < w + 0.25)$. We also do a similar benchmarking of NAICS-based industry-and-state-specific QCEW employment (between 1990 and 2016) when we conduct sectoral analysis.

In addition, we use micro-aggregated administrative data on hourly wages from Washington state for the case study in Section B.5. This data was provided to us as counts of workers in (nominal) \$0.05 bins between 1992 and 2016 by the state's Employment Security Department. We convert this data into \$0.25 (real 2016 USD) hourly wage bins for our analysis using the CPI-U-RS. We also use similar micro-aggregated administrative data from Minnesota and Oregon for conducting comparison of data quality and measurement error in Online Appendix E.

### Matched CPS

The CPS outgoing rotation groups are structured so that an individual reports her wage twice, one year apart, in 4th and 8th sample months. We employ the longitudinal aspect of the CPS when separately estimating the impacts of the minimum wage on new entrant and incumbent workers. This requires matching two CPS files. We exactly follow the procedure proposed by Madrian and Lefgren (2000), and use household id (HHID), household number (HHNUM), person line number in household (LINENO), month in sample (MINSAMP), and month and state variables to match observations in two consecutive CPS files. We confirm the validity of matches by evaluating reported sex, race, and age in the two surveys. If sex or race do not match, or if individual's age decreases by more than 1 or increases by more than 2, we declare them as "bad matches" and exclude from the matched sample. Additionally, since matching is not possible from July to December in 1984 and 1985, from January to

September in 1985 and 1986, from June to December in 1994 and 1995, or from January to August in 1995 and 1996, we exclude these periods. On average, 72% of the observations in the CPS are matched: around 25% of the individuals in are absent in the 8th sample month, while an additional 3% are dropped because they are bad matches. We determine the incumbency of individual from employment status information in the 4th sample month. Similar to our primary CPS sample, we drop observations with imputed wages in the 8th sample month. Overall, the number of worker-level observations is smaller in the matched sample because we only use the 8th sample month in the matched sample, as opposed to both 4th and 8th sample months in the baseline sample.

**Industry classifications**

Following Mian and Sufi (2014), we use an industry classification with four categories (tradable, non-tradable, construction, and other) based on retail and world trade. According to the classification, an industry is "tradable" if the per worker import plus export value exceeds \$10,000, or if the sum of import and export values of the NAICS 4-digit industry is greater than \$500 million. The retail sector and restaurants compose "non-tradable" industries, whereas the "construction" industries are industries related to construction, land development and real estate. Industries that do not fit in either of these three categories are pooled and labeled as "other". We merge the CPS with Mian and Sufi (2014) industry classification using the IND80 and IND02 variables in the CPS.

# Online Appendix E
# Comparison of Administrative Data to CPS

In our event study analysis, we use the Current Population Survey (CPS), which provides information on wages for a large sample of individuals, after benchmarking to aggregate state-level employment counts in the QCEW. There is therefore sampling error in our estimated job counts in each wage bin. In this section we assess the accuracy of CPS based jobs counts by comparing administrative data on job counts from three states with reliable information on hourly wages (Minnesota, Oregon, and Washington).

In Section E.1, we compare the performance of the raw CPS and the QCEW-benchmarked CPS in predicting the counts of workers earning less than $15 in the administrative data from Minnesota, Oregon and Washington. We show that counts from the QCEW-benchmarked CPS are much closer to the counts from the administrative data than those from the raw CPS: the mean squared prediction error is substantially smaller when we use QWEW-benchmarked CPS data. In Section E.2, we show that the wage distribution from the QCEW-benchmarked CPS closely matches the distribution from the administrative data from the three states. In particular, we show that the number of workers reporting earnings under the state minimum wage is similarly small in both the administrative data and the CPS, which is an important indication of the degree of misreporting in the CPS. In section E.3 we implement structural estimation to further assess the importance of wage misreporting in the administrative data and in the QCEW-benchmarked CPS along the lines of Autor, Manning and Smith (2016). Our estimates show that the implied misreporting is of a similar magnitude in the two data sources. In section E.4 we deconvolve the QCEW-benchmarked CPS using the estimated measurement error model of Autor, Manning and Smith (2016), and provide estimates using this measurement-error-corrected frequency distribution.

## E.1 Assessing the Accuracy of the Raw versus the QCEW- benchmarked CPS

We compare the administrative data with the raw CPS, and the QCEW-benchmarked CPS. Because the CPS is a survey, it has substantially greater sampling error than the QCEW which is a near-census of all workers in a state. Also, since we are not using observations with imputed hourly wages in our data sets, state-level employment counts of the raw CPS data are biased downwards. To address both these problems, our primary sample combines the CPS wage distribution with state-level employment counts in the QCEW. We label the data with the QCEW adjustment as the “QCEW-benchmarked CPS”, and the raw CPS as “CPS-Raw.”[42]

[42] We note that the QCEW and CPS have slightly different employment concepts. The CPS measures employment in a reference week while the QCEW measures employment at any time in a quarter. So CPS employment may be slightly lower than QCEW since some people work only parts of a quarter. Therefore, the QCEW-benchmarked CPS is closer to the QCEW employment concept. At the same time, any such gap is likely picked up by the state and time fixed effects. To confirm this, we we implement an event study regression where the outcome variable is the gap between CPS and QCEW employment. Events are the same 138 state-level minimum wage changes between 1979-2016 that we use in our benchmark specification. Similar to our benchmark specification we include state and time fixed effects in the regression. The blue line

First, we establish here that QCEW benchmarking of aggregate employment is likely to improve the accuracy of our counts by wage bin. The employment count for wage bin $w$, $E_w$, can be rewritten as the product of the (discretized) wage density, $f_w = Prob(w \leq wage < w + 0.25)$, and the employment , $E$, so $E_w = f_w \times E$. The raw CPS-based estimate for per-capita count is $E_w^{CPS} = f_w^{CPS} \times E^{CPS}$. The QCEW benchmarked CPS uses the state-level employment counts from the QCEW which has no measurement error given that includes the near universe of workers; so formally, $E_w^{QCEW} = f_w^{CPS} \times E$. It follows that the mean squared prediction error ($MSPE$) is lower for the QCEW benchmarked CPS than for the raw CPS, if the measurement errors for $f_w^{CPS}$ are uncorrelated with $E^{CPS}$. The latter condition holds if the source of the error is sampling.

Since the bunching approach proposed here mainly focuses on job changes at the bottom of the wage distribution, we assess whether the raw CPS or the QCEW-benchmarked CPS does a better job in predicting the number of workers earning less than \$15. For each quarter $t$, we calculate the average per-capita numbers of workers earning less than \$15 in the 20 subsequent quarters (i.e., between $t$and $t + 20$); we also calculate the average for the 4 preceding quarters (i.e., between $t$and $t - 4$). Then, we subtract the latter from the former and we refer to this as the transformed counts. The employment changes in Table 1 show the average employment changes in the 20 subsequent quarter after the minimum wage relative to the 4 preceding quarters. Therefore, the transformed counts are closely related to the employment estimates shown in Table 1.

In figure E.1 panels (a) and (b), we show the scatterplot of the transformed counts (per capita) from the administrative data against those from QCEW-benchmarked CPS and the raw CPS, respectively. In addition to a visual depiction, we also regress the transformed administrative counts on the transformed CPS-Raw, and QCEW-benchmarked CPS counts. To assess the accuracy of the data, we use two measures: $R^2$ and the slope ($\hat{\beta}$). A perfect match between the CPS and the administrative data would yield $R^2 = \hat{\beta} = 1$, or a zero mean-squared prediction error (MSPE). If the CPS correctly predicts the administrative counts on average, but each prediction possesses some error, then $R^2 < 1$ and $\hat{\beta} = 1$. On the other hand, if there is a bias in the CPS counts, then $\hat{\beta} \neq 1$. The QCEW-benchmarked counts are better predictors of the administrative counts than are the raw CPS counts: for the former, the estimated slope is 0.778 and the $R^2$ is 0.643. In contrast, the raw CPS has a larger bias ($\hat{\beta} = 0.564$) and variance ($R^2 = 0.322$).

In table E.1, we report the ratio of the MSPE using the raw CPS counts to the MSPE using the QCEW-benchmarked CPS. Besides reporting the MSPE for the transformed count (the 20 subsequent quarter average minus the 4 preceding quarter average) of workers under \$15, we also report the MSPEs for underlying components. Namely, we calculate the MSPEs using counts of workers earnings less than \$15/hour as well as counts of workers in each \$0.25 bins—each averaged over either 4 or 20 quarters. A MSPE ratio above one indicates that the QCEW-benchmarked CPS performs better in predicting the administrative data than the raw CPS. The table shows that this is indeed the case: QCEW-benchmarked CPS performs better in all cases, especially for the aggregated employment counts under \$15/hour.

shows the evolution of the gap in the employment rate (relative to the year before the treatment) between the CPS and QCEW. As Figure E.5 shows, there is no systematic change in the gap between CPS-QCEW employment following treatment.

## E.2 Comparison of the Wage Distribution in the CPS and in the Administrative Data

We assess the sampling and misreporting errors in the CPS by comparing the frequency distribution of hourly wages in the QCEW benchmarked CPS and in the administrative data. In Figure E.2 we plot 5-year averaged per-capita employment counts in \$3 bins relative to the minimum wage. We compare the distributions at this aggregation level, since our main estimates on excess and missing jobs in Table 1 show 5 year employment changes in \$3 to \$5 bins relative to the minimum wage. The red squares show the distribution in the administrative data while the blue dots show the distribution calculated using QCEW-adjusted CPS. We report the wage distributions in each each states separately, as well as in the three states together.

The distributions from the CPS closely match the distributions in the administrative data in all states and in all three five-years periods (2000-2004, 2005-2009, and 2010-2014). A similar number of jobs are present just below the minimum wage in the two data sources, albeit in some cases there are slightly more in the CPS (e.g. in WA 2005-2009). When we pool all three states, the CPS and the administrative data exhibit virtually the same distribution below the minimum wage. Note that in all three of these states, there is no separate tipped minimum wage, and nearly all workers are covered by the state minimum wage laws. Therefore, the presence of jobs paying below the minimum wage may reflect misreporting. If this is the case, then Figure E.2 suggests that the extent of misreporting is quite similar in the CPS and in the administrative data. We formally test this in the next section. At the same time, we should point out that some of the sub-minimum wage jobs may reflect true under-payment. Either way, it is encouraging that the extent of sub-minimum wage jobs in the CPS is very similar to what is found in high quality administrative wage data.

The figures also highlight that the [0,3) bin—which includes workers at and up to \$3 above the minimum wage—contains a somewhat larger number of workers in the administrative data than in the CPS for Washington state; however, for Oregon and Minnesota, the CPS closely matches the number of workers in that bin. As a result, when we pool all three states together, we find that the CPS tends to underestimate the number of jobs at and slightly above the minimum wage. However, this difference is quite stable over time, as further shown below in Figure E.3; as a result, our difference-in-difference estimates are unlikely to be affected by this gap between the two counts. Finally, the CPS tends to place slightly more workers in the middle-income bin ($[MW + \$6, MW + \$21)$), and fewer workers at the high-income bin ($[MW + \$21, \infty)$).

Figure E.3 plots the time paths of the number of jobs below the minimum wage $[MW - \$5, MW)$, and jobs at and above the minimum wage ($[MW, MW + \$5)$ relative to the state-level population from both the administrative data and the CPS. Consistent with the previous findings, the job counts below and above in both of the data sets follow very similar paths. When we pool the data across all three states, the evolution of the jobs below the minimum wage lines up perfectly across the two series. The level of jobs at and slightly above the minimum wage is slightly higher in the CPS, but again, the differences are quite stable over time. As a result, the difference-in-difference estimator implemented in this paper is unlikely to be affected by the small discrepancy between the administrative and the CPS data.

## E.3 Assessment of Misreporting of Wages Using Structural Estimation

To compare the potential measurement error in the CPS and in the administrative data for these states, we also implement a structural estimation approach developed by Autor, Manning and Smith (2016). Following Autor, Manning and Smith (2016), we assume that in the absence of the minimum wage, both the observed and the true latent wage distributions are log-normal.[43] A portion ($\gamma$) of the workers report their wages correctly, while others report it with some error. In the absence of a minimum wage, the observed (log) wage can be written as

$$v^* = w^* + D\epsilon$$

where $v^*$ is the observed and $w^*$ is the true latent (log) wage of the worker that would prevail in the absence of a minimum wage. $D$ is a binary variable that is equal to 1 when the wage is misreported, and 0 otherwise. Therefore, $P(D = 0) = \gamma$ measures the probability of reporting wages accurately. When the wage is misreported, the distribution of the (logged) error is again normal, with$\epsilon \sim N(0, \frac{1-\rho^2}{\rho^2})$, where $\rho^2 = \frac{cov(v^*,w^*)}{var(v^*)}$, reflects the correlation between the observed and true latent distributions. Both parameters $\rho$ and $\gamma$ determine how misreporting distorts the observed wage distribution. Here $1 - \gamma$ measures the rate of misreporting, while $\frac{1-\rho^2}{\rho^2}$ measures the variance of the error conditional on misreporting.

We can summarize the overall importance of misreporting by comparing the standard deviation of the true latent distribution ($\sigma_w$) and the observed latent distribution ($\sigma$). When $\frac{\sigma_w}{\sigma} = 1$, misreporting does not affect the dispersion in observed wages. But when $\frac{\sigma_w}{\sigma} = 0.5$, say, misreporting causes the observed wage distribution's standard deviation to be twice as large that it would if wages were always accurately reported. Autor, Manning and Smith (2016) notes that the ratio can be approximated by $\rho$ and $\gamma$ as follows:

$$\frac{\sigma_w}{\sigma} = \gamma + \rho(1 - \gamma)$$

We estimate the model parameters $\gamma$ and $\rho$ for both the administrative data and the CPS. One additional complication in the administrative data is that sometimes small rounding errors in hours can shift a portion of workers to the wage bin below the MW; this will tend to over-state the measurement error in the administrative data (at least in terms of estimating $1-\gamma$). For this reason, we present two sets of estimates. First we keep the data as is by using wage bins relative to the minimum wage, $[MW, MW + \$0.15)$. Second, we additionally show estimates using re-centered \$0.25 wage bins around the minimum wage. The re-centered \$0.25 bin that includes the minimum wage is now defined as $[MW - \$0.10, MW + \$0.15)$. The subsequent re-centered bins are defined as $[MW + \$0.15, MW + \$0.40)$, etc., while the preceding bins are defined as $[MW - \$0.35, MW - \$0.10)$, etc.

Our analysis covers the 1990-2015 period for Washington, and the 1998-2015 period for Minnesota and Oregon: the start dates reflect the earliest years the administrative data are

[43]The latent wage distribution refers to the distribution that would prevail in the absence of a minimum wage. The wage is called "observed" when it reflects both the true value as well as the reporting error. Note, however, that the "latent observed" wage distribution is only observed in practice in the absence of a minimum wage.

available for each state. Since none of these three states allow tip credits, we do not drop tipped workers from our sample, and use all workers in our analysis.

Table E.2 reports the misreporting rate $(1-\gamma)$, the variance of the error term, and the ratio of the true and observed standard deviations. In panel A, where we re-center the wage bins, and find that the misreporting rate $1-\gamma$ is slightly smaller in the CPS (.23) than in the administrative data (0.28).[44] However, conditional on misreporting, the variance of the errors $\left(\frac{1-\rho^2}{\rho^2}\right)$ is somewhat larger in the CPS (1.46) than in the administrative data (1.25). Putting these two parts together, we find that the ratios of the true to observed standard deviations $\frac{\sigma_w}{\sigma}$ are quite similar in the two datasets: 0.92 in the CPS and 0.91 in the administrative data. In panel B, where we use un-centered wage bins, the CPS estimates are virtually unchanged. However, due to the rounding errors in hours in the administrative data, the estimated misreporting rate (1-$\gamma$) increases while the variance of the error conditional on misreporting $\left(\frac{1-\rho^2}{\rho^2}\right)$ falls. Overall, the ratio of the true and observed standard deviations for administrative data in panel B (0.90) remains very similar to those reported in panel A (0.91) and to the CPS estimates (0.92).

Overall, the structural estimation results suggest that the extent to which there is misreporting of wages, they are of similar magnitude in the CPS and in high quality administrative wage data. This provides additional support for the validity of our bunching estimates using CPS data.

## E.4 Estimates using deconvolved, measurement-error corrected CPS-ORG

In the previous section, we obtained the functional form of the distribution of misreporting error ($D\epsilon$) in the CPS-ORG. Given an empirical distribution of the observed noisy wage $v = w + D\epsilon$, and an empirical distribution of the error $D\epsilon$, we can obtain an estimated distribution of the the error-free wage, $w$, using the non-parametric deconvolution procedure proposed by Comte and Lacour (2011). Given an empirical sample of errors $D\epsilon$ drawn from an arbitrary distribution (estimated in the previous section), and the sample of noisy observed wages $v$, the procedure recovers a measurement error corrected distribution. The deconvolution is based on the insight that the inverse-Fourier transform of the unknown distribution of $w$ is a function of the estimable characteristic functions of $v$ and $D\epsilon$. Estimation is performed using penalized deconvolution contrasts and data-driven adaptive model-selection, and implemented using the R package `deamer`.[45]

Figure E.4 plots the wage distributions of the CPS-ORG and the measurement error corrected (deconvolved) CPS-ORG (MEC-CPS) in $1 bins relative to the minimum wage averaged over time and states. We make three observations. First, the share of jobs paying below the current minimum wage is smaller in CPS-MEC. This is expected, since the Autor,

[44] The CPS estimate is largely in line with Autor, Manning and Smith (2016) who estimate the misreporting rate around 20% between 1979 and 2012 using 50 states.

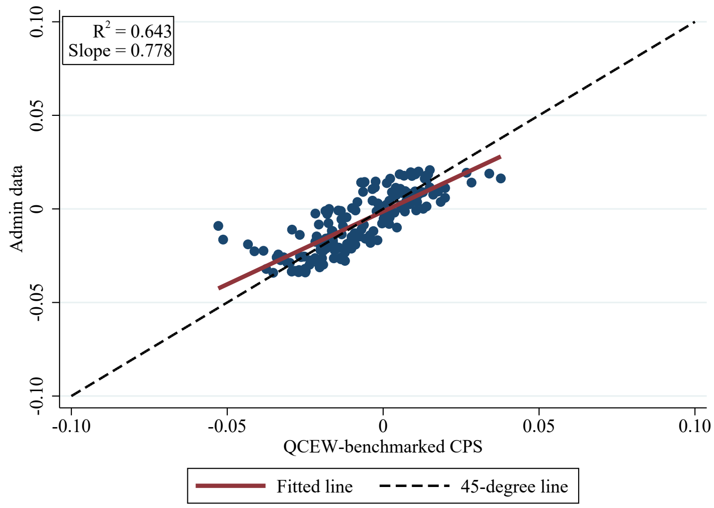
[45] We separately estimate the distribution of true wages for each state-by-quarter using the same distribution function for the measurement error. Estimating annual distribution functions for the error following Autor et al. (2016) produces virtually the same results.

Manning and Smith (2016) approach uses the share below the minimum wage to estimate the extent of measurement error; so a successful reduction in measurement error should reduce the share earning below the minimum. Numerically, while 2.67% of the workers report working below the minimum wage in the CPS, after the measurement error correction it decreases to 1.57%. Second, the share of workers in the dollar bin of the current minimum wage are similar in both samples, suggesting that the raw CPS performs relatively well in reporting the share of workers at or up to $0.99 above the minimum. Third, individuals in the raw CPS are more likely to report their wages as $17 higher than the current minimum wage. The CPS-MEC, on the other hand, find that there are more individuals with hourly wages between $1 and $16.99 above the minimum after taking the misreporting error into account.

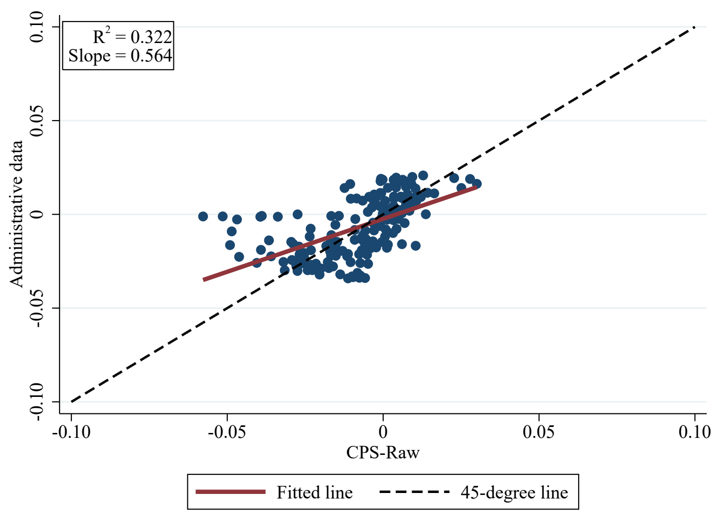
In Table E.3, we compare the baseline estimates with those obtained using the deconvolved data. Column (1) reproduces the baseline estimates reported in Table 1 column (1). Column (2) reports the results using deconvolved data[46] The missing and excess jobs estimates are quite similar across columns 1 and 2. The baseline missing jobs estimate of $-0.018$ (s.e. 0.004) in column 1 is very similar to the measurement error corrected estimate of -0.017(s.e. 0.004) in column 2. The baseline excess jobs estimates for both columns 1 and 2 are 0.021 (s.e. 0.003). This corroborates our argument that the employment estimates are not substantially affected by measurement error in reported wages. While the baseline employment elasticity with respect to the minimum wage is 0.028 (s.e. 0.029) in column 1, it is 0.037 (s.e. 0.031) after measurement error correction in column 2. The wage effect estimates are also quite similar when we use the deconvolved data. The baseline percentage change in affected wage is 0.068 (s.e. 0.010) in column 1, whereas it is 0.075 (s.e. 0.012) in column using deconvolved data. Overall, these findings underscore that our results are quite robust to the presence of misreporting error in wages..

[46]The deconvolved data uses a slightly different sample that excludes the quarters of events due to the existence of two spikes in those periods. By assumption, the latent wage distribution is log-normal and observed wage distribution can only have one mass point due to the minimum wage. However, if there is a minimum wage event in the quarter, then it is likely that observed wage distribution will have two mass points. In those cases, the deconvolution procedure does not perform well. However, in practice the estimates including the quarter of events are very similar (results not reported).

Figure E.1: Comparison of Administrative with QCEW-benchmarked CPS, and CPS-Raw Counts of Workers Earning less than $15




(a) Administrative data against QCEW-benchmarked CPS




(b) Administrative data against CPS-Raw

*Notes*: This figure plots per-capita counts of workers earning less than $15 in administrative data against QCEW-benchmarked CPS in panel A, and CPS-Raw in panel B. To construct a measure that is comparable to the baseline employment estimate, we transform the counts, and subtract the average number of workers earning less than $15 (per capita) in the 4 preceding quarters from that in the 20 subsequent quarters. The blue circles indicate each observation, the red straight line the fitted line, and the black dash line the 45-degree line. We report the estimated $R^2$ and slope from a simple linear regression in the box.

Figure E.2: Frequency Distributions in the Administrative and CPS data

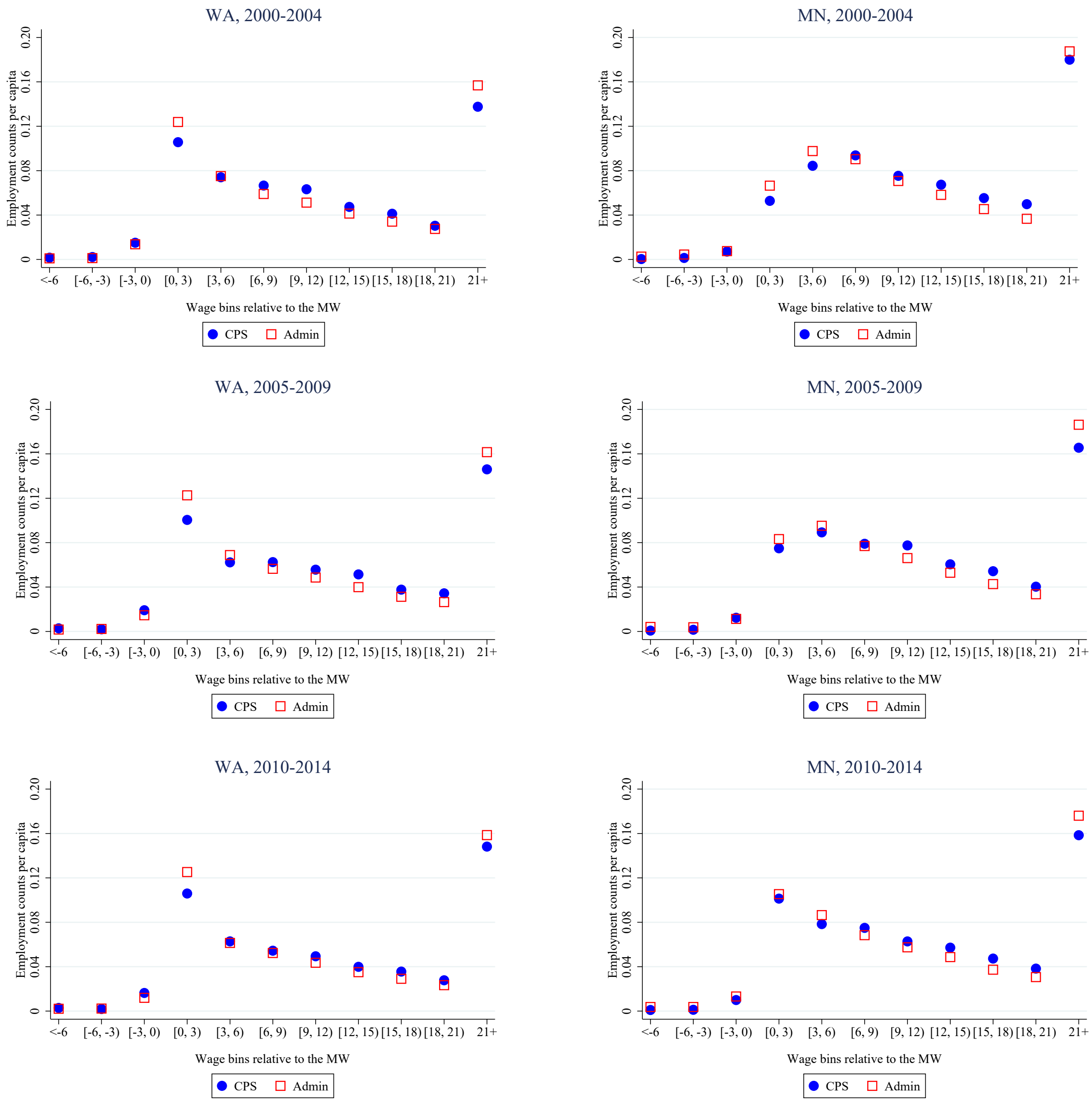
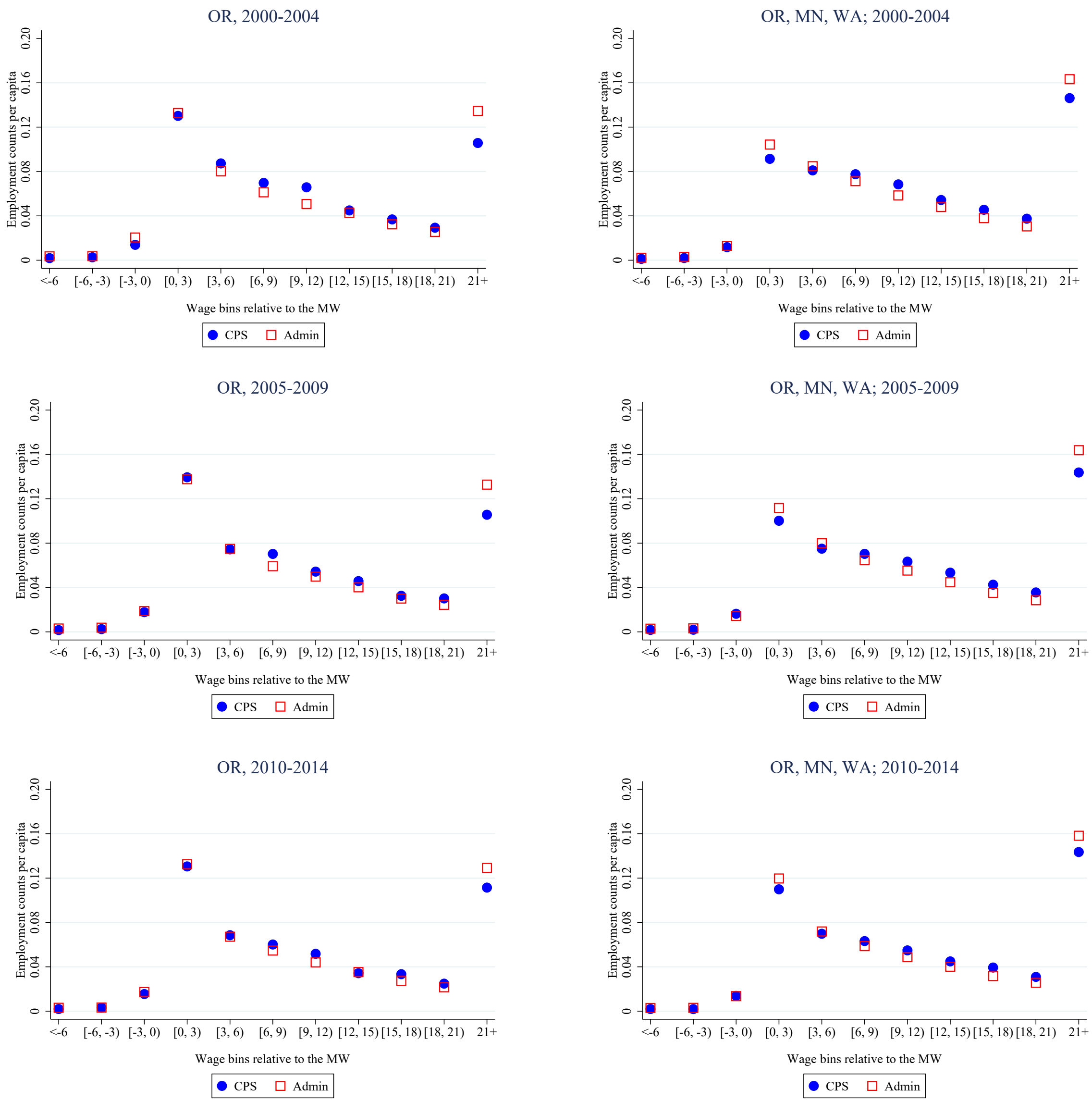
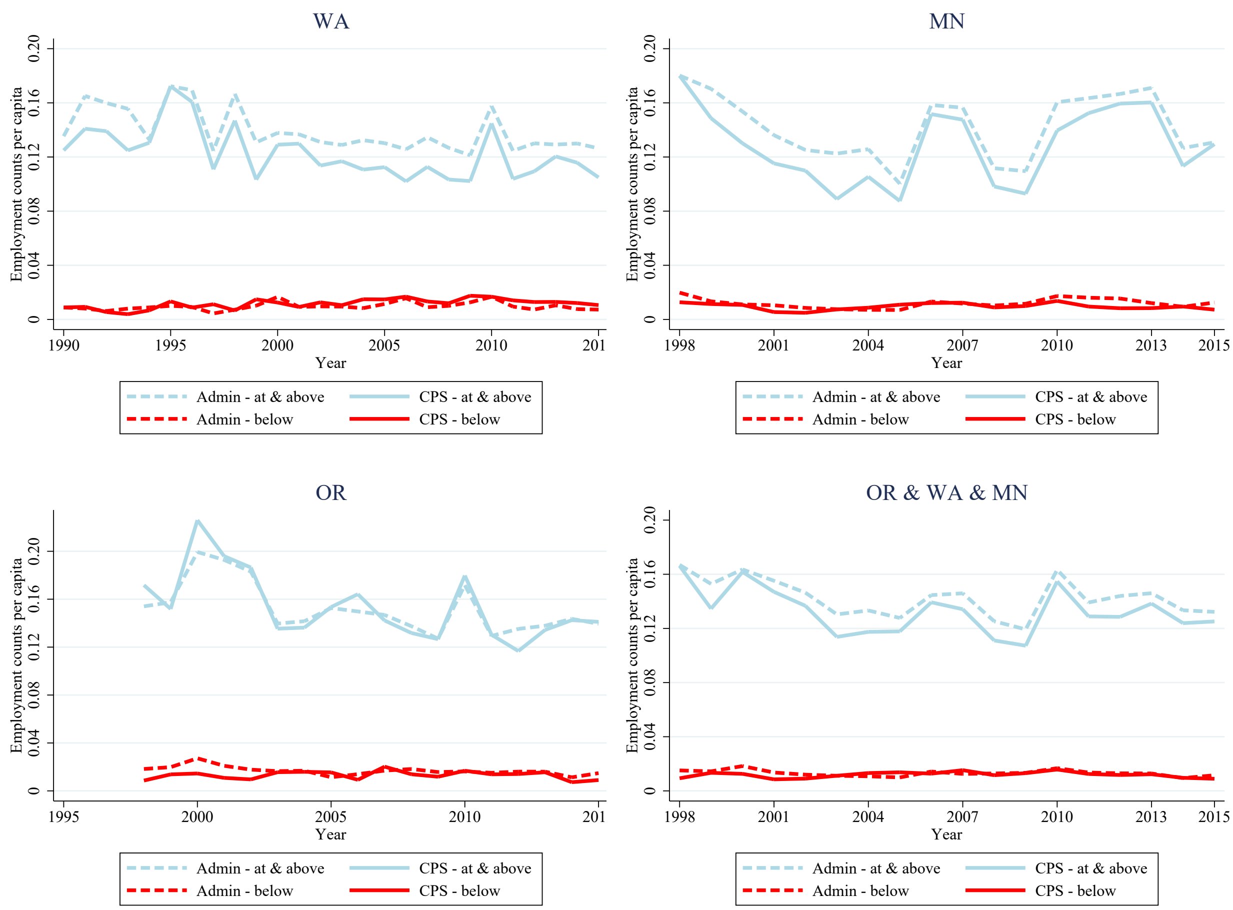


Figure cont'd: Frequency Distributions in the Administrative and CPS data



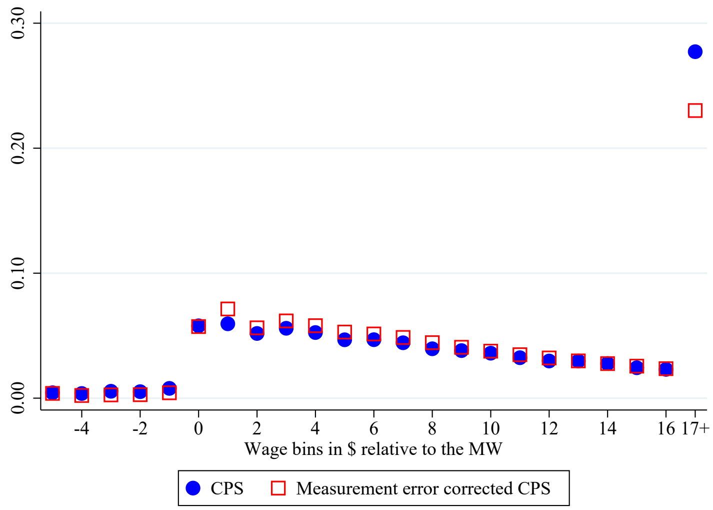

*Notes*: This figure plots 5-year averaged per-capita administrative and QCEW-benchmarked CPS employment counts of Washington, Minnesota, Oregon, and the three states combined from 2000 to 2014 in \$3 bins relative to the minimum wage. The red squares indicate the administrative data, and the blue circles the QCEW-benchmarked CPS counts.

Figure E.3: Comparing Administrative and CPS data; Time path



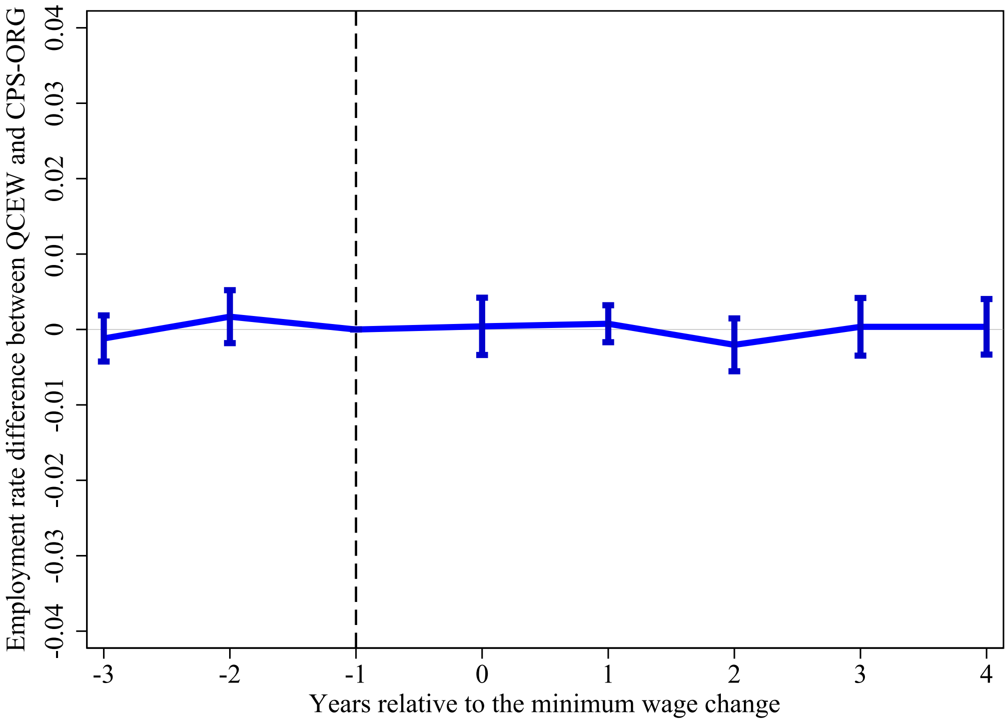

*Notes*: This figure plots the time paths of the number of jobs below the minimum wage $[MW - \$5, MW)$, and jobs at and above the minimum wage ($[MW, MW + \$5)$ relative to the state-level population from both the administrative data and the CPS in three states (MN, OR, WA) separately, and all together.

Figure E.4: Wage Distributions in the CPS and the Measurement Error Corrected CPS




*Notes*: This figure plots the national wage distributions of the CPS and measurement error corrected CPS combined from 1979 to 2016 in $1 bins relative to the minimum wage. The measurement error correction process uses the estimates in Table E.2, and the procedure described in Comte and Lacour (2011). The red squares indicate the share of workforce in the particular wage bin in the measurement error corrected CPS data, and the blue circles in the raw CPS.

Figure E.5: Impact of Minimum Wages on the Gap in Employment Between QCEW and CPS




*Notes*: The figure shows the effect of the minimum wage on the gap in employment rate between QCEW and CPS. In our event study analysis we use QCEW-benchmarked employment. The CPS and the QCEW have somewhat different employment concepts: the CPS asks about employment in a reference week, while the QCEW measures any employment during the quarter. To alleviate the concern that the differences in concepts has an effect on our estimates, we implement an event study regression where the outcome variable is the gap between CPS and QCEW employment. Events are the same 138 state-level minimum wage changes between 1979-2016 that we use in our benchmark specification. Similar to our benchmark specification we include state and time fixed effects in the regression. The blue line shows the evolution of the gap in the employment rate (relative to the year before the treatment) between the CPS and QCEW. We also show the 95% confidence interval based on standard errors that are clustered at the state level.

Table E.1: MSPE Ratios of CPS-Raw to QCEW-Adjusted CPS

| Data structure | MSPE ratio: Raw/Benchmarked |
|---|---|
| Employment count by \$0.25 bins, averaged across 4 quarters | 1.637 |
| Employment count by \$0.25 bins, averaged across 20 quarters | 3.875 |
| Employment count under \$15, averaged across 4 quarters | 7.212 |
| Employment count under \$15, averaged across 20 quarters | 7.394 |
| Transformed employment count under \$15: average of 20 subsequent quarters minus the average of 4 preceding quarters | 2.141 |

*Notes.* This table reports estimated mean squared prediction error (MSPE) ratios of the raw CPS to the QCEW-benchmarked CPS. For each dataset (raw and QCEW-benchmarked), the MSPE comes from predicting the (per-capita) administrative counts with the CPS based ones. The first two lines report the results from state-by-quarter-by-25-cent-wage-bin aggregated, and the last three lines state-by-quarter aggregated data. The transformed count is designed to be comparable to our baseline employment estimates, which compares employment in the 20 quarter following an event to the 4 quarter prior to the event. In all cases, we only consider wage bins under \$15/hour in real, 2016\$.

Table E.2: Structural Estimation of the Autor, Manning and Smith (2016) Model of Measurement Error in Wages: Evidence from CPS and Administrative Data

| Dataset | Misreporting rate<br>1-$\gamma$ | Conditional error variance<br>$\frac{1-\rho^2}{\rho^2}$ | Ratio of std. deviations of true to observed latent distribution<br>$\frac{\sigma_w}{\sigma}$ |
|---|---|---|---|
| A. Re-centered \$0.25 wage bins | | | |
| CPS | 0.232 | 1.462 | 0.916 |
| Administrative data | 0.277 | 1.251 | 0.908 |
| B. \$0.25 wage bins | | | |
| CPS | 0.218 | 1.484 | 0.920 |
| Administrative data | 0.343 | 1.076 | 0.895 |

*Notes.* We assess the misreporting in the CPS and in the administrative data by implementing Autor et al. (2016). To alleviate the effect of rounding of hours worked information in the administrative data we re-center the \$0.25 wage bins around the minimum wage in Panel A, while in Panel B we report estimates using wage bins that are not re-centered around the minimum wage. This latter is what we use in our main analysis. We report 1-$\gamma$, the misreporting rate, in Column 1; $(1-\rho^2)/\rho^2$, the variance of the error conditional on misreporting in Column 2; and the ratio of the standard deviation of the true latent distribution (w) and the observed latent distribution in Column 3.

Table E.3: Impact of Minimum Wages on Employment and Wages Using Deconvolved Data

| | (1) | (2) |
|---|---|---|
| Missing jobs below new MW (Δb) | -0.018*** | -0.017*** |
| | (0.004) | (0.004) |
| Excess jobs above new MW (Δa) | 0.021*** | 0.021*** |
| | (0.003) | (0.003) |
| | | |
| %Δ affected wages | 0.068*** | 0.075*** |
| | (0.010) | (0.012) |
| %Δ affected employment | 0.028 | 0.046 |
| | (0.029) | (0.038) |
| | | |
| Employment elasticity w.r.t. MW | 0.024 | 0.037 |
| | (0.025) | (0.031) |
| Emp. elasticity w.r.t. affected wage | 0.411 | 0.613 |
| | (0.430) | (0.502) |
| | | |
| Jobs below new MW ($\bar{b}_{-1}$) | 0.086 | 0.082 |
| %Δ MW | 0.101 | 0.101 |
| Number of events | 138 | 138 |
| Number of observations | 847,314 | 831,285 |
| | | |
| *Sample* | | |
| Measurement error corrected | | Y |

*Notes.* The table reports the effects of a minimum wage increase based on the event study analysis (see equation 1) exploiting 138 state-level minimum wage changes between 1979 and 2016. The table reports five year averaged post-treatment estimates on missing jobs up to $4 below the new minimum wage, excess jobs at and up to $5 above it, employment and wages. Column (1) reproduces the baseline estimates in Table 1 column (1). Column (2) estimates the same parameters, but uses the data deconvolved according to the procedure proposed by Comte and Lacour (2011). In column (2), we also exclude the quarters of events due to the existence of two spikes in those periods, as explained in footnote 46. To implement the procedure, we rely on the estimates in Table E.2. All specifications include wage-bin-by-state and wage-bin-by period fixed effects. Regressions are weighted by state-quarter aggregated population. Standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

*Line-by-line description.*The first two rows report the change in number of missing jobs below the new minimum wage (Δb), and excess jobs above the new minimum wage (Δa) relative to the pre-treatment total employment. The third row, the percentage change in average wages in the affected bins, (%ΔW), is calculated using equation 2 in Section 2.2. The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by jobs below the new minimum wage ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). The fifth row, employment elasticity with respect to the minimum wage is calculated as $\frac{\Delta a+\Delta b}{\%\Delta MW}$ whereas the sixth row, employment elasticity with respect to the wage, reports $\frac{1}{\%\Delta W}\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$. The line on the number of observations shows the number of quarter-bin cells used for estimation, while the number of workers refers to the underlying CPS sample used to calculate job counts in these cells.

# Online Appendix F
# Bias in the Classic Two-Way Fixed Effects Panel Regression with Log Minimum Wage

This Appendix establishes that the classic two-way fixed effects panel regression on log minimum wage provides biased estimates of the employment effect of the policy. When implemented over the same time period as our primary specification, the two-way fixed effects estimator with log minimum wage (TWFE-logMW) obtains a large negative estimate for overall employment changes, in contrast to the small positive and statistically insignificant estimate of the employment effect for low-wage workers obtained by the first difference (FD) specification, and the event based specifications (EB). In the main text, we showed that the negative employment changes in the TWFE-logMW specification mainly comes from the employment changes at the upper tail of the wage distribution, which suggests that the aggregate employment changes are unlikely to reflect the causal effect of minimum wages. Furthermore, this highlights how understanding the sources of disemployment throughout the wage distribution can serve as an useful tool for model selection. In this section we document additional problems with the TWFE-logMW specification and provide further evidence to understand the discrepancy between TWFE-logMW and the event study based benchmark (EB) estimates.

We begin our analysis by assessing the contribution of various factors that drive the differences in estimates between our benchmark EB and the TWFE-logMW specifications. We establish that the differences between the two estimates is not driven by using discrete versus continuous treatment, or any artifact of binning the wages. Table F.1 compares the employment elasticities with respect to the minimum wage of the benchmark EB (see Table 1 and Figure 2) and the TWFE-logMW estimator (Figure 6), as well as several intermediate forms that bridge the two specifications. Column 1 reproduces our baseline estimate of the policy elasticity which considers employment changes locally within a \$9 window around the new minimum wage using the regression specification in equation 1. In Column 2 we use the event study design, but estimate the effect on overall, below \$15, and above \$15 employment counts per capita using aggregated state-level treatment variation. The resulting employment elasticity of 0.027 for the below \$15 group is very similar to our baseline employment elasticity. Reassuringly, we see little employment change for higher wage workers earning above \$15. The resulting overall employment elasticity with respect to the minimum wage, 0.016, is very close to our benchmark estimate. In column 2 we also report results for the 3 Card and Krueger (CK) probability groups using demographic predictors (see Section 3.1 for the details). Here too, all of the point estimates are small, and in particular there is little change in employment for those workers predicted to have higher wages. There is also no sizable disemployment effect for predicted low-wage and and middle-wage groups, which capture most minimum wage workers,

The EB estimator uses a discrete, binary indicator for minimum wage changes, while the TWFE-logMW uses continuous treatment. In Column 3 and 4 we provide intermediate results that continue to use the 8-year event windows, but use a continuous treatment measure. In particular, we multiply the wage-bin-state-specific treatment indicators in EB by the change in log minimum wage; column 3 uses the \$9 window around the minimum wage, while column

4 simply considers employment under \$15, employment above \$15, overall employment, and the three CK groups. This latter EB-logMW specification can be written as follows:

$$\frac{E_{st}}{N_{st}} = \sum_{\tau=-3}^{4} \beta_\tau I_{st}^{\tau} \triangle \log MW_{s,t-\tau} + \mu_s + \rho_t + u_{st} \tag{F.1}$$

As in our benchmark specification, we report the 5 year averages in employment change from the year prior to treatment: $\frac{1}{5}\sum_{\tau=0}^{4}\beta_\tau - \beta_{-1}$. We find that moving from the discrete to continuous treatment measure has little effect on the estimated employment elasticity.

In contrast, Column 5 estimates a TWFE-logMW specification of the following form:

$$\frac{E_{st}}{N_{st}} = \sum_{\tau=-2}^{4} \alpha_\tau \log MW_{s,t-\tau} + \mu_s + \rho_t + u_{st} \tag{F.2}$$

The cumulative responses can be calculated as $\beta_\tau = \sum_{j=-3}^{\tau}\alpha_j$ by successively summing the coefficients. Again, we report the 5 year averages in employment change from the year prior to treatment: $\frac{1}{5}\sum_{\tau=0}^{4}\beta_\tau - \beta_{-1}$.[47] The TWFE-logMW estimate is the same as what is reported in Figure 6. The overall employment elasticity with respect to the minimum wage is large, negative, and statistically distinguishable from zero (-0.089., s.e. 0.025). In Figure F.1 we also show the effect of the minimum wage on the wage distribution by the Card and Krueger (CK) probability groups. The negative overall effect in the TWFE-logMW model is concentrated on the "wrong" workers: namely, the large employment change occurs for wage bins over \$15 per hour, and there is a large employment response for only the workers least demographically likely to be earning near the minimum wage. The TWFE-logMW estimate is driven entirely by high wage employment or employment likely to be high wage.

The main difference between equation F.1 and F.2 is that the EB-logMW focuses on employment changes within the 8-year event window around the minimum wage changes, while the TWFE estimates are more sensitive to underlying long-term trends or persistent shocks to employment, including those that are far away from the actual treatment events. We additionally estimate a first-difference (FD) specification:

$$\triangle\frac{E_{st}}{N_{st}} = \sum_{\tau=-3}^{4} \alpha_\tau \triangle \log MW_{s,t-\tau}^{\tau} + \mu_s + \rho_t + u_{st} \tag{F.3}$$

The cumulative responses can be calculated as $\beta_\tau = \sum_{j=-3}^{\tau}\alpha_j$ by successively summing the coefficients. Again, we report the 5 year averages in employment change from the year prior to treatment: $\frac{1}{5}\sum_{\tau=0}^{4}\beta_\tau - \beta_{-1}$. This latter specification is consistent under the same strict exogeneity assumptions as the TWFE estimated in levels; however, the small sample

[47] Because of this normalization, we report the "3rd year or earlier" estimate as $-\beta_{-1}$, even though there are 2 actual leads in the regression. This 3rd lead is analogous to the 5th lag, which refers to estimates the "5th year or later;" both of these are "binned up" and represent estimates at the end point or outside of the event window. Since we report estimates for 8 different event dates, we refer to this specification as having a 8-year event window. This is slightly different from the EB specification where the coefficient estimates are relative to all observations outside of the event window (before and after); this is why a 8-year event window in that specification requires estimating 3 leads and 4 lags. In practice, however, the inclusion of an additional lead in the TWFE-logMW specification makes very little difference.

properties differ substantially when the number of units ($N$) is small (Wooldridge, 2010). Our sample has a relatively small $N$ with 51 states and DC, and we have an outcome variable that is highly persistent (the raw autocorrelation in state EPOP is 0.97; conditional on state and year fixed effects it is 0.86). This opens up the possibility that small sample biases may be quite different for the TWFE-logMW model estimated in levels than the FD specification.

Figure A.10 shows that the impact of minimum wages on the wage distribution differs considerably in the fixed effects and first difference specifications. Panel (a) and (c) reports pre-reform effects. We do not expect the minimum wage changes to have an effect on the wage distribution one year before the minimum wage was increased. This is more or less the case in the first difference specifications; however, in the fixed effect specifications there are significant changes in the wage distribution even before the minimum wage was raised. This suggests that the fixed effect results are likely to reflect pre-existing trends and not the only the causal effect of the minimum wage. Panels (b) and (d) report the post reform effects. As we see in the main text, the drop in overall employment in the fixed effect specification is driven by changes in the upper part of the wage distribution. Contrary to that, the first difference specifications shows more muted employment responses in the upper tail of the wage distribution, and there is no indication for significant disemployment effects overall.

Table F.2 shows the fragility of the TWFE-logMW specification by using different methods of controlling for long-term trends and persistent shocks to employment confounded with minimum wage increases. Column 1 reports the EB-logMW specification, whose event window by definition minimizes the influence of long-term trends; as a result, the estimated employment elasticity with respect to the minimum wage is small, 0.008. Column 2 reports small employment effects using a first-differenced specification, which by focusing on higher-frequency variation reduces the role of long-term trends and persistent shocks.

Columns 3 through 6 of Table F.2 report the results of the TWFE-logMW specification with various control sets. Column 3 repeats for reference the baseline TWFE-logMW specification with no additional controls. Column 4 shows that simply controlling for long-term trends with state-specific linear trend terms shrinks the overall employment effect to close to zero. Column 5 instead adds average major industry and broad occupation shares from 1979-1980 (interacted with time periods) to the TWFE-logMW specification. These shares account for shocks to upper tail employment that are predicted by historical industrial/occupational patterns in the state. The employment estimate in column 5 is also close to zero and not statistically significant.

In column (6), we control for partisan leanings by including a variable that captures state-level differences in the presidential vote share of two major party candidates. This variable accounts for that state-level political leanings might affect both the minimum wage and employment in ways that are unrelated to each other (Wursten, 2017). In this case, the partisan voting index decreases the magnitude of the overall employment estimate by 70%, from -0.089 in column (3) to -0.027 in column (6). Overall, Table F.1 highlights the fragility of the two-way fixed estimate suggesting a large overall employment effect. Allowing for richer controls for time-varying heterogeneity produces results that are very similar to the baseline estimates.

Figure F.3 panel (a) shows another problem with the TWFE-logMW specification. This figure plots the time path of employment elasticities with respect to the minimum wage for the TWFE-logMW. Since increases in nominal minimum wages, $log(MW)$, are always permanent, the last lag in the distributed lag model reflects the "long term effect" - the weighted average of effect at or after 4 years following a minimum wage increase. Moreover, since we normalize the estimates relative to the one year before the minimum wage, $-\alpha_{-1}$ measures the average employment occurring at 3 (or more) years prior to the minimum wage increase. The time path of the estimates shows that the TWFE-logMW estimator obtains a spurious, positive leading effect three (or more) years prior to the minimum wage increase. This shows that there were large employment reductions substantially prior to minimum wage increases, which can impart a bias on the treatment effect estimated using the TWFE-logMW model; moreover, because we are "binning up" the leads and lags at -3 and +4, respectively, biases associated with these binned estimates can impart a bias on the estimated leads and lags, producing a spurious dynamic pattern even within the event window. These sizable and statistically significant pre-treatment and post-treatment effects disappear once we add state-specific linear trends (see panel b).

Figure F.4 plots the time path of the employment elasticities separately for workers earning below \$15 per hour (panel a) and above \$15 per hour (panel b). The pattern of positive leads and the overall downward trend in employment preceding the minimum wage change comes from workers earning above \$15 per hour. Lower wage workers, earning under \$15/ hour, see little change in employment before or after the minimum wage increase.

Finally, and importantly, Table F.3 provides a evidence that demonstrates that the disemployment detected by the TWFE-logMW estimator is not caused by the minimum wage changes, but rather is driven entirely by employment shocks in the 1980's (much earlier than the minimum wage changes). Column 1 uses the entire 1979-2016 period and shows the overall employment elasticities with respect to the minimum wage from the classic first difference estimator (panel A), and first-differenced version (panel B). Panel C reports estimates from our events-based approach. As before, the TWFE-logMW estimator stands out in estimating a large, statistically significant overall employment decline due to minimum wage changes, in contrast to the first-difference. However, when we restrict the sample to the 1993-2016 period, the TWFE-logMW estimates become small and stastitically indistinguishable from zero, and are quite similar to the first-difference and event-based estimates. This is noteworthy because there were few state minimum wage changes prior to 1993. At the same time, we note that our event based estimates are highly robust to the choice of sample (panel C), as are estimates using the first-differenced specification (panel B).

**Falsification test.** We also perform a falsification exercise to demonstrate the bias in the TWFE-logMW estimates using the 1979-2016 sample. As the first step of the falsification exercise, column 3 shows results using the full 1979-2016 sample of data, but excluding the ten states that experienced any minimum wage increases prior to 1993. The pattern of results remains the same in column 3, with the TWFE-logMW specification estimating large employment declines due to the minimum wage. In columns 4 and 5, we decompose the estimate in column 3. In column 4, we use actual employment data until 1992 for the forty states that did not have any minimum wage event 1979-1992. For 1993-2016, we set employment outcomes to exactly 0 in all states. Because there were no minimum wage events prior to 1993 in this sample, and because the employment outcomes are exactly constant after 1993, the causal employment effect should be zero. Yet, column 4 shows that the TWFE-logMW specification still estimates a sizable negative employment effect, in contrast to the first-differenced and event-based specifications. Put differently, minimum wage events in 1993 and onwards appears to affect employment changes in 1979-1992. Column 5 does the opposite, and replaces all employment outcomes before 1993 with 0, and uses the actual employment rate in 1993-2016. In this case, variations in the variable of interest and the dependent variable take place in the same time period, and both the TWFE-logMW and the FD specifications indicate no disemployment effect. Finally, in column 6 we show that the spurious negative results in column 4 are not due to anticipation effects: here we consider states without minimum wage events prior to 1996 (instead of 1993 as in column 4); this reduces the sample to 39 instead of 40 states but the results are similar. Finally, in contrast to the TWFE-logMW case (panel A), our EB estimates (panel C) easily pass this falsification test.

To summarize, our falsification test shows that estimated disemployment in the TWFE-logMW specification is entirely due to employment shocks in the 1980s that were correlated with future minimum wage increases decades later, thereby affecting the estimation of the state fixed effects. This is why the restriction to an explicit event window as in the EB specification guards against the bias afflicting the TWFE-logMW specification. This is also why the inclusion of state trends or controls for historical industry/occupation shares interacted with periods substantially reduces the likely bias in that specification.

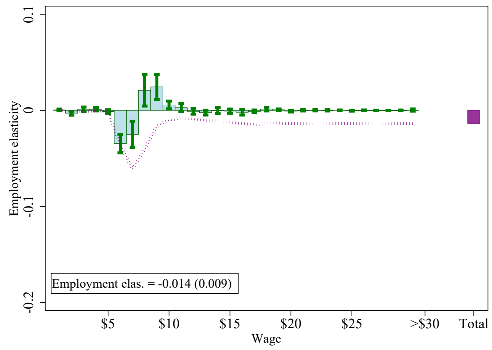
## The Partisan Tilt of the 1990-1991 Recession and the Confounder

Why are state-level employment rates in 1979-1992 are correlated with minimum wage events in post-1996? To understand what drives this correlation we plot the time paths of the minimum wage (Panel (a)) and employment rates (Panel (b)) of the low and high minimum wage states in Figure F.5. The 15 states where the federal minimum wage laws applies during 1996-2016 are classified as low minimum wage states, and the remaining 36 states as high minimum wage states. Figure F.5 shows that the employment rate of the latter states are elevated relative to the former between the mid-1980s and the early 1990s, even as the level of minimum wages were almost the same across the two set of states in this period. The elevated employment level in the mid-1980s affects the estimation of the state fixed effects in the TWFE-logMW model covering 1979-2016. However, the divergence between low minimum wage and high minimum wage states ended quickly by 1990-1991 recession. Since then the employment rates follow parallel trends, even though there is a clear divergence in the level of the minimum wage between low and high minimum wage states in the 2000's. The timing of divergence between high and low minimum wage states highlights that the bias in the TWFE-logMW estimates is related to the differential impact of the 1990-1991 recession on (future) low and high minimum wage states.

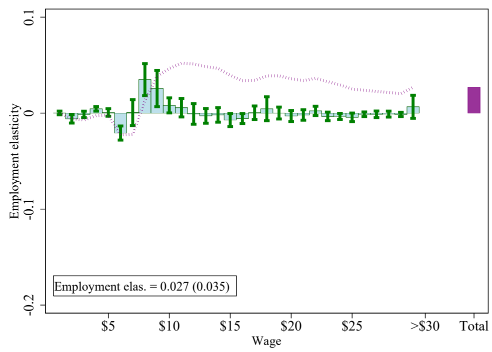
Why is the drop in employment in the 1990-1991 recession related to future minimum wage changes in the 2000s? It is possible that the 1990-1991 recession was so severe in some states that it changed the political landscape and opened up the door for parties supporting minimum wages. Another explanations could be that 1990-1991 recession happened to be more pronounced for Democratic-leaning states—states that would also be more inclined to raise the minimum wage in the early 2000s following a long period of federal inaction. Table **??** aims to test the empirical relevance of these explanations by examining the determinants of having a state-level minimum wage higher than the federal level in the post-1996s using a linear probability model. Column (1) shows that states that are harder hit by the 1990-1991 recession are more likely to have a state-level minimum wage after 1996, confirming our previous observation about Figure F.5. The model reports that for each percentage point decline in employment rate in 1990-1991, the probability of a state to be a high minimum wage state increases by 4.2% (s.e. 1.3%). However, including political leanings variables in columns (2) and (3) substantially decrease the estimate and renders it statistically indistinguishable from zero. In column (2), the unionization rate in the 1980s variable substantially decreases the size of the 1990s shock estimate and renders it statistically insignificant. In column (3), we include the average of the Partisan Voting Index (PVI) in 2000s. The PVI shows the shows the difference between Republican Party and Democratic Party candidates' vote shares in the state. To address potential concerns related to long-run effects of the recession on political leanings, we instrument the average PVI in the 2000s with that of the 1988. In this case, the coefficient of the severity of the recession has changed its sign, become negative and statistically insignificant. This suggests that the severity of the 1990-1991 recession did not have a causal impact on future state-level minimum wage changes.

Overall, these findings clarify that the large, negative TWFE-logMW estimate is driven by upper tail shocks in the 1980s—substantially prior to most minimum wage increases we study. Moreover, these shocks are predicted by a state's historical industrial/occupational structure. Importantly, these shocks died out substantially prior to most minimum wage changes we study: indeed, as we have shown, these shocks do not produce any pre-existing trends or upper tail employment changes within the 8-year window used in our event-based analysis. However, they do substantially bias the TWFE-logMW estimator that is sensitive to underlying long-term trends or persistent shocks occurring many years before the actual treatment events.

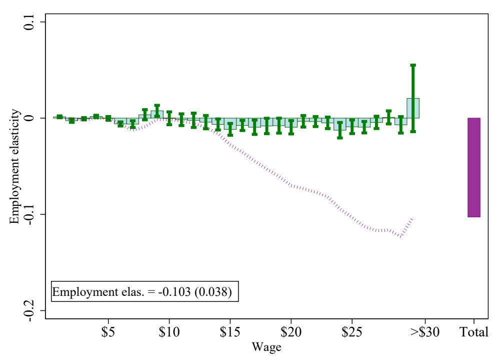
Figure F.1: Impact of Minimum Wages on the Wage Distribution by Predicted Probability Groups for Fixed Effects Specification




(a) High probability group (top 10%)



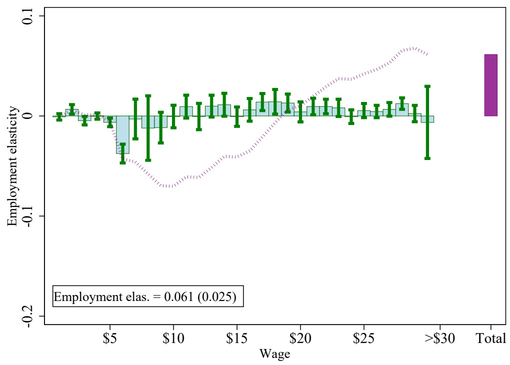

(b) Middle probability group (10%-50%)



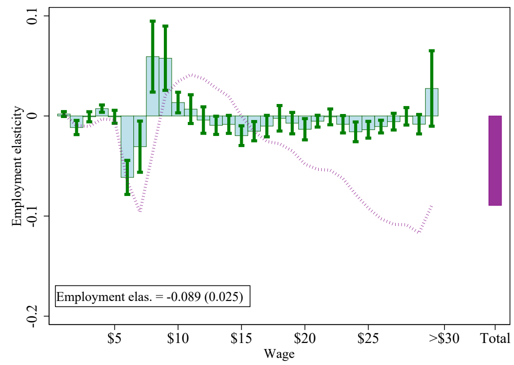

(c) Low probability group (bottom 50%)

*Notes*: The figure shows the effect of the minimum wage on the wage distribution of the three Card and Kruger probability groups in fixed effects specification (TWFE-logMW). All panels estimate the regression on the contemporaneous log minimum wage, as well as on 4 annual lags and 2 annual leads. For each wage bin we run a separate regression, where the outcome is the number of jobs per capita in that state-wage bin. The cumulative response for each event date 0, 1,...,4 is formed by successively adding the coefficients for the contemporaneous and lagged log minimum wages. The green histogram bars show the mean of these cumulative responses for event dates 0, 1,...,4, divided by the sample average employment-to-population rate —and represents the average elasticity of employment in each wage bin with respect to the minimum wage in the post-treatment period. The 95% confidence intervals around the point estimates are calculated using clustered standard errors at the state level. The dashed purple line plots the running sum of the employment effects of the minimum wage up until the particular wage bin. The rightmost purple bar in each of the graphs decomposes the post-averaged elasticity of the overall state employment-to-population with respect to minimum wage by the groups, where the latter is obtained from the regressions where outcome variable is the state level employment-to-population rate. All regressions are weighted by the sample average state population.

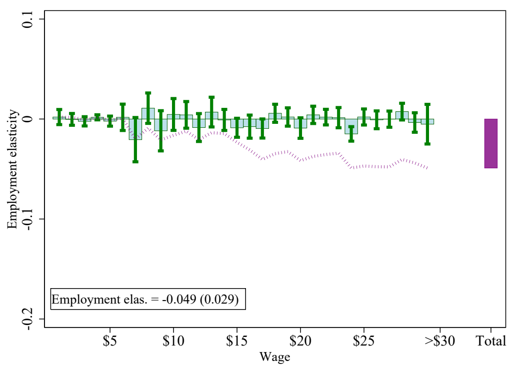
Figure F.2: Impact of Minimum Wages on the Wage Distribution for Fixed Effects and First Differences specifications




(a) Fixed Effect Model (Pre Reform)




(b) Fixed Effect Model (Post Reform)

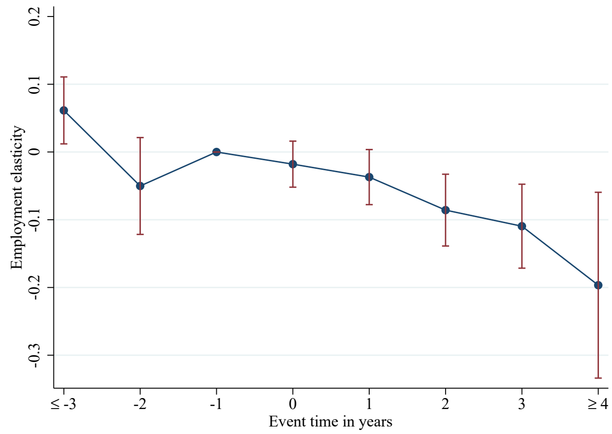



(c) First Differences Model (Pre Reform)

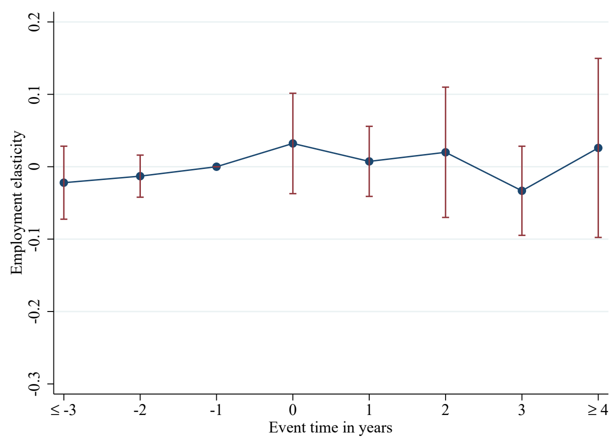

(d) First Differences Model (Post Reform)

*Notes*: The figure shows the three year the pre reform (panel a and c) and post reform effects (panel b and d) of the minimum wage hikes on the wage distribution in fixed effects (TWFE-logMW) and first difference (FD) specifications. Panels (a) and (b) estimate two-way (state-bin and year) fixed effects regressions on contemporaneous as well as 2 annual leads, and 4 annual lags of log minimum wage. In Panels (c) and (d) we employ first difference regressions on contemporaneous as well as 2 annual leads, and 4 annual lags of the log change in the minimum wage. For each wage bin we run a separate regression, where the outcome is the number of jobs per capita in that state-wage bin. The pre reform effect is formed by adding the first and the second leading coefficients and multiplying the sum by -1: when the cumulative response at $\tau = -1$ is normalized to 0, this represents the three year leading effect relative to the date $-1$. For the fixed effects specification (panel a), the three year leading effect estimate represents three or more years prior to treatment; for the first difference specification (panel c), this estimate represents exactly three years prior to treatment. The cumulative response for each event date 0, 1, ..., 4 is formed by successively adding the coefficients for the contemporaneous and lagged terms. The green bars show the post reform effects calculated by the mean of these cumulative responses for event dates 0, 1, ..., 4, divided by the sample average employment-to-population rate —and represents the average elasticity of employment in each wage bin with respect to the minimum wage in the post-treatment period. (panel (b) is the same as Figure 6 in the main text). The 95% confidence intervals around the point estimates are calculated using clustered standard errors at the state level. The dashed purple line plots the running sum of the employment effects of the minimum wage up until the particular wage bin. The rightmost purple bar in each of the graphs is the elasticity of the overall state employment-to-population rate with respect to minimum wage. In the bottom left corner we also report the point estimate on this elasticity with standard errors that are clustered at the state level. Regressions are weighted by state population.

Figure F.3: Estimated Impacts of Minimum Wages on Employment Over Time Using Alternative Specifications



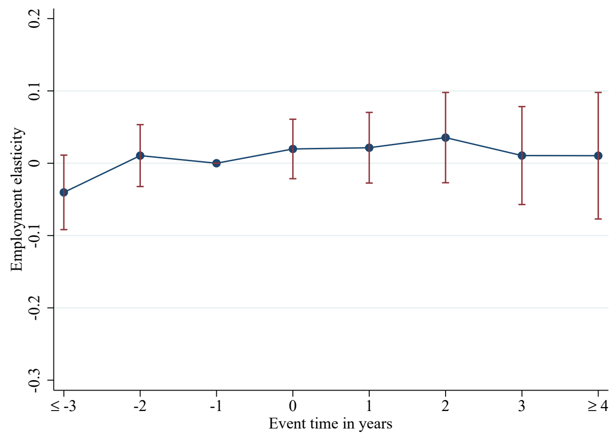

(a) Fixed effects



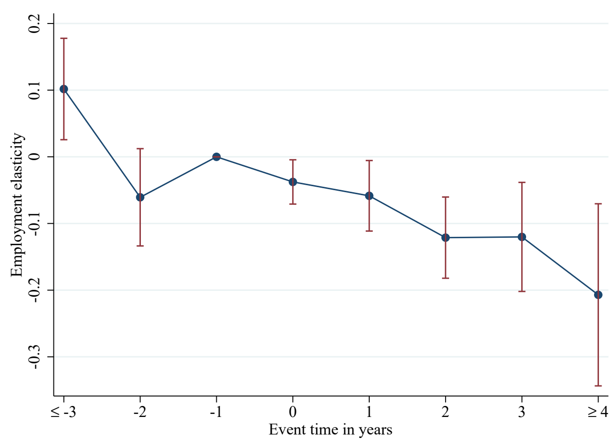

(b) Fixed effects with Linear trends

*Notes*: The figure shows the effect of the minimum wage on employment over time in the fixed effects (panel (a)) and in the fixed effects augmented with state-specific linear trends (panel (b)) models. All panels estimate regressions of state-level employment rate on the state-level contemporaneous log minimum wage, as well as on 4 annual lags and 2 annual leads. The blue markers show cumulative employment elasticities by event date. These cumulative effects are calculated by successively summing the coefficients on leads and lags of log minimum wage, and then dividing them by the sample average employment-to-population rate. Furthermore, the cumulative elasticity at event date -1 is normalized to 0; this is why the figure shows a 3rd year or earlier ($\leq -3$) estimate.The red error bars indicate the 95% confidence intervals around the point estimates, calculated using clustered standard errors at the state level. All regressions are weighted by sample average state population.

Figure F.4: Impact of Minimum Wages on Lower- and Upper-tail Employment Over Time for Fixed Effects Specification



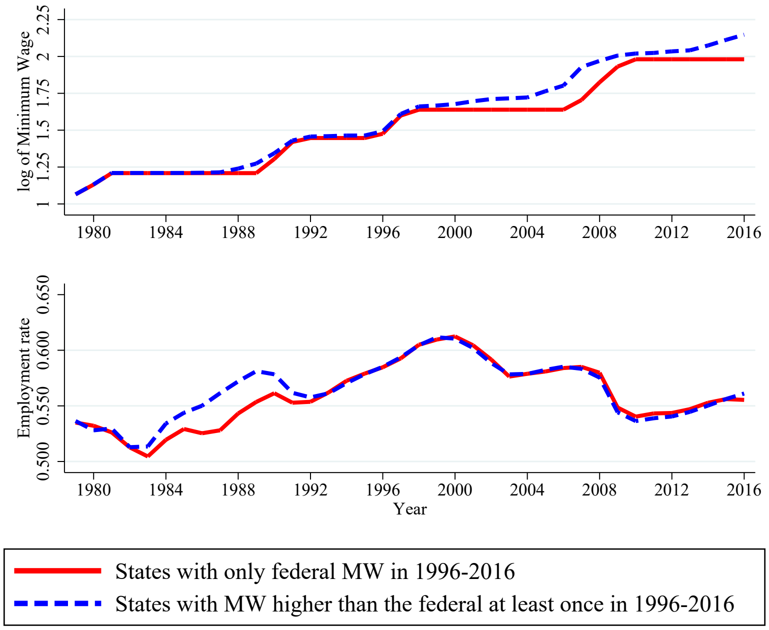

(a) Fixed Effects; Below $15




(b) Fixed Effects; At or above $15

*Notes*: The figure shows the effect of the minimum wage on the number of jobs below (panel (a)), and at or above $15 (panel (b)) over time in the fixed effects (TWFE-logMW) specification. Panels (a) and (b) estimate regressions of state-level total number of jobs below, and at or above $15 over state population on the state-level contemporaneous log minimum wage, as well as on 4 annual lags and 2 annual leads. The blue markers show cumulative employment elasticities by event date. These cumulative effects are calculated by successively summing the coefficients on leads and lags of log minimum wage, and then dividing them by the sample average employment-to-population rate. Furthermore, the cumulative elasticity at event date -1 is normalized to 0; this is why the figure shows a 3rd year or earlier (" $\leq -3$") estimate. The red error bars indicate the 95% confidence intervals around the point estimates, calculated using clustered standard errors at the state level. All regressions are weighted by sample average state population.

Figure F.5: Time Paths of the Statutory Minimum Wage and Employment Rate in High and Low Minimum Wage States




*Notes*: The figure shows the time paths of the average statutory log minimum wage (Panel (a)) and employment rate (Panel (b)) in 15 states where the federal minimum wage law applies in 1996 (low minimum wage states) and onward, and in 36 remaining states that had state-level minimum wages higher than the federal level at least once in 1996-2016 (high minimum wage states). In both graphs, the straight red lines correspond to the low minimum wage states, and the dash blue lines to the high minimum wage states.

Table F.1: Employment Elasticities with Respect to the Minimum Wage, Event-based and Continuous Variation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Bunching | 0.024 | | 0.024 | | |
| | (0.025) | | (0.020) | | |
| Overall | | 0.016 | | 0.008 | –0.089*** |
| | | (0.029) | | (0.025) | (0.025) |
| *By wage bin* | | | | | |
| Below $15 | | 0.027 | | 0.020 | 0.020 |
| | | (0.022) | | (0.016) | (0.028) |
| Over $15 | | –0.010 | | –0.012 | –0.109*** |
| | | (0.042) | | (0.033) | (0.030) |
| *By demographically predicted wage* | | | | | |
| Predicted low-wage workers | | 0.019** | | 0.022* | –0.014 |
| | | (0.009) | | (0.013) | (0.009) |
| Predicted middle-wage workers | | 0.026 | | 0.001 | 0.027 |
| | | (0.020) | | (0.014) | (0.035) |
| Predicted high-wage workers | | –0.029 | | –0.015 | –0.103*** |
| | | (0.023) | | (0.022) | (0.038) |
| Event-based | Y | Y | Y | Y | |
| Bin-state-specific treatment | Y | | Y | | |
| State-specific treatment | | Y | | Y | Y |
| Discrete treatment | Y | Y | | | |
| Continuous treatment | | | Y | Y | Y |
| Standard TWFE | | | | | Y |

*Notes.* The table reports estimated employment elasticities of minimum wage from alternative approaches. Column 1 reports our baseline estimates (Column 1 in Table 1) that is derived by using local employment changes within a $9 window around the new minimum wage. Column 2 use the same event study design as in Column 1 (see equation 2), but estimate the effect on below $15 employment counts, on above $15 employment counts, on overall employment counts, and on the three Card and Krueger probability groups. In Column 3 we use the 8-year event window around the minimum wage like in Column 1, but use a continuous treatment measure, where we multiply the wage-bin-state-specific treatment indicators by the change in log minimum wage. Column 4 reports the results using continuous treatment measure for the below $15 employment counts, above $15 employment counts, and overall employment counts (see equation F.1). For comparison, Column (5) report the results using two way fixed effects estimator with log minimum wage (equation F.2) shown in Figure 9. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table F.2: Robustness of the Two-way Fixed Effects-log(MW) Estimates to Alternative Controls

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| All workers | 0.008 | 0.031 | –0.089*** | 0.010 | –0.025 | –0.027 |
| | (0.025) | (0.031) | (0.025) | (0.036) | (0.029) | (0.022) |
| Event-based | Y | | | | | |
| First-differenced | | Y | | | | |
| Standard TWFE | | | Y | Y | Y | Y |
| State fixed effects | Y | | Y | Y | Y | Y |
| State-specific linear trends | | | | Y | | |
| Base industry/occupation shares | | | | | Y | |
| Partisan voting index | | | | | | Y |

*Notes.* The table reports estimated employment elasticities of minimum wage from alternative approaches and outcome groups. Each column and row is a separately estimated model specification and outcome group, respectively. Columns 1 shows the results using EB-logMW specification (see equation F.1), Column 2 the first differenced specification (equation F.3), while Columns 3 shows that two-way fixed effects specifications with log minimum wage (equation F.2). Column 4, 5, and 6 explores robustness of the two-way fixed effects specifications to various controls. Columns 4 add state-specific linear trends, Column 5 control for 1979-1980 major industry and occupation shares interacted with time fixed effects, and Column 6 control for the partisan voting index variable. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table F.3: Estimated Impacts of Minimum Wages on Actual, and Simulated Employment Using Alternative Specifications

| | Actual | Post 1992 sample | Excl. states with pre-1993 events | | | Excl. states with pre-1996 events |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Panel A: TWFE-logMW | | | | | | |
| Emp. elas. wrt MW | -0.089*** | -0.012 | -0.106*** | -0.091** | -0.015 | -0.090* |
| | (0.025) | (0.027) | (0.037) | (0.041) | (0.043) | (0.047) |
| Panel B: FD | | | | | | |
| Emp. elas. wrt MW | 0.031 | 0.021 | -0.034 | 0.006 | -0.040 | 0.019 |
| | (0.031) | (0.030) | (0.021) | (0.018) | (0.024) | (0.015) |
| Panel C: EB | | | | | | |
| Emp. elas. wrt MW | 0.016 | -0.009 | -0.027 | -0.001 | -0.026 | 0.001 |
| | (0.029) | (0.018) | (0.039) | (0.023) | (0.032) | (0.023) |
| Observations | 1479 | 1020 | 1160 | 1160 | 1160 | 1131 |
| Number of states | 51 | 51 | 40 | 40 | 40 | 39 |
| Period estimated | 1979-2016 | 1993-2016 | 1979-2016 | 1979-2016 | 1979-2016 | 1979-2016 |
| Outcome variable | Actual epop | Actual epop | Actual epop | Simulated epop (1993 onwards 0) | Simulated epop (0 until 1992) | Simulated epop (1993 onwards 0) |

*Notes.* The table reports the effect of a minimum wage increase on the actual and simulated employment using the fixed effects (Panel A), first-differences (Panel B), and the event-based specifications (EB). The first column reports the estimates using the actual employment data using the full sample from 1979 to 2016. The second column excludes all the pre-1993 years. The third column employs the entire time span of the data, yet leaves out the states that have a minimum wage event before 1993. The fourth column replaces the actual outcome variable in the previous column with the simulated employment data, where we use the actual data until 1992, and replace it with 0 from 1993 onwards. The fifth column uses an alternative simulated data, we use the actual data from 1992, but replace it with 0 before 1993. The sixth column replicates the analysis based on the same simulated data as in Column 4, but it excludes all the states that experience a minimum wage increase before 1996 to account for potential anticipation effects. Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.