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The Effect of Minimum Wages on Low-Wage Jobs*

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Abstract

We estimate the effect of minimum wages on low-wage jobs using 138 prominent state-level minimum wage changes between 1979 and 2016 in the U.S using a difference-in-differences approach. We first estimate the effect of the minimum wage increase on employment changes by wage bins throughout the hourly wage distribution. We then focus on the bottom part of the wage distribution and compare the number of excess jobs paying at or slightly above the new minimum wage to the missing jobs paying below it to infer the employment effect. We find that the overall number of low-wage jobs remained essentially unchanged over the five years following the increase. At the same time, the direct effect of the minimum wage on average earnings was amplified by modest wage spillovers at the bottom of the wage distribution. Our estimates by detailed demographic groups show that the lack of job loss is not explained by labor-labor substitution at the bottom of the wage distribution. We also find no evidence of disemployment when we consider higher levels of minimum wages. However, we do find some evidence of reduced employment in tradable sectors. We also show how decomposing the overall employment effect by wage bins allows a transparent way of assessing the plausibility of estimates.

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1 Introduction

Minimum wage policies have featured prominently in recent policy debates in the United States at the federal, state and local levels. California, Illinois, Massachusetts, New Jersey, and New York have passed legislation to eventually increase minimum wages to $15/hour, while at least five other states are on paths to raise their minimum wages of $12 or more. Over a dozen cities have also instituted city-wide minimum wages during the past three years, typically by substantial amounts above state and federal standards. Underlying much of the policy debate is the central question: what is the overall effect of minimum wages on low-wage jobs?

Even though nearly three decades have passed since the advent of "new minimum wage research" (see e.g. Card and Krueger 1995; Neumark and Wascher 2008), there is surprisingly little research on the effect of the policy on *overall* employment. This shortcoming is particularly acute given the importance policymakers place on understanding overall responses. For example, in its attempt to arrive at such an estimate, the 2014 Congressional Budget Office (CBO) report noted the paucity of relevant research and then used estimates for teen minimum wage elasticities to extrapolate the total impact on low-wage jobs.

In this paper we use a difference-in-differences design to estimate the impact of minimum wage increases on the entire frequency distribution of wages; and subsequently focus on changes at the bottom of the distribution to estimate the impact on employment and wages of affected workers. Our approach relies on the idea that the overall employment and wage effects of the policy can be inferred from the localized employment changes around the minimum wage. An increase in the minimum wage will directly affect jobs that were previously paying below the new minimum wage. The jobs shifted into compliance create a "bunching," and show up as "excess jobs" at and slightly above the minimum. The effect of the minimum wage on the wage distribution fades out and becomes negligible beyond a certain point. Therefore, the overall employment and wage effects of the policy can be inferred from the localized employment changes around the minimum wage. For instance, we can assess the changes in employment from the difference between the number of excess jobs at and slightly above the minimum wage and the number of missing jobs below the minimum.

To identify the effect of the minimum wage on the frequency distribution of wages, we implement an event study analysis that exploits 138 prominent state-level minimum wage increases between 1979 and 2016. We estimate employment changes in each dollar wage bin relative to the new minimum wage for three years prior to and five years following an event. Our empirical approach, therefore, disaggregates the total employment effect of the policy into constituent wage bins, and we use these bin-by-bin estimates locally around the minimum wage to assess the effect of the policy.

There are several advantages of our disaggregated approach relative to the more standard approach that estimates the disemployment effect using aggregate employment or wage changes (e.g. Meer and West, 2016). First, we focus on employment changes locally around wage levels where minimum wages are likely to play a role. When only a small fraction of aggregate workforce is affected by the minimum wage, such a localized approach is crucial for uncovering meaningful "first stage" wage effects of the minimum wage—something that is not possible with the standard approach except for subgroups like teens. Second, by decomposing the aggregate employment impact by wage bins, we are able to assess employment changes in the upper tail of the wage distribution. This can provide an additional falsification test, since large changes in the upper part of the wage distribution are unlikely to reflect a causal effect of the minimum wage. Third, our localized focus on jobs around the minimum wage gains precision by filtering out random shocks to jobs in the upper part of the wage distribution.

We use hourly wage data from the 1979-2016 Current Population Survey to estimate the effect of the minimum wage by wage bins. We find that an average minimum wage hike leads to a large and significant decrease in the number of jobs below the new minimum wage during the five years following implementation. At the same time, there is clear evidence for the emergence of excess jobs at or slightly above the minimum wage. However, as expected, we find no indication of any employment changes in the upper part of the wage distribution—providing further validation to the empirical design. We estimate that the number of excess jobs closely matched the number of missing jobs: the employment for affected workers rose by a statistically insignificant 2.8% (s.e. 2.9%). Our estimates also allow us to calculate the impact of the policy on the average wages of affected workers, which rose by around 6.8% (s.e. 1.0%). The significant increase in average wages of affected workers implies an employment elasticity with respect to own wage (or the labor demand elasticity in a competitive model) of 0.41 (s.e. 0.43), which rules out elasticities more negative than -0.45 at the 95 percent confidence level.

An additional advantage of estimating the effect of the minimum wage on the frequency distribution of wages is that we can directly assess the extent which the direct wage effects of the minimum wage are amplified by wage spillovers. We find that spillovers extend up to $3 above the minimum wage and represent around 40% of the overall wage increase from minimum wage changes. Interestingly, we also find that the benefits of wage spillovers are not equally shared: workers who had a job before the minimum wage increase (incumbents) experience significant wage spillovers, but we do not find any evidence of such spillovers for new entrants. This asymmetry suggests that spillovers may reflect relative pay concerns within the firm (Dube, Giuliano and Leonard 2018) and the value of outside options or reservation wages of non-employed workers is unlikely to play a key role in generating wage spillovers (e.g. Flinn 2006).

Our estimates are highly robust to a wide variety of approaches to controlling for time-varying heterogeneity that has sometimes produced conflicting results in the existing literature (e.g., Allegretto, Dube, Reich and Zipperer 2017; Neumark and Wascher 1992). Moreover, the shifts in the missing and excess jobs are strongly related to the timing of minimum wage change—providing further support that we are identifying the causal effect of the policy. Both missing jobs below the new minimum and excess jobs above were close to zero prior to the minimum wage increase, which suggests that the treatment and the control states were following a parallel trend. The drop in jobs below the minimum wage is immediate, as is the emergence of the excess jobs at and slightly above. Over the five year post-treatment period, the magnitude of the missing jobs below the new minimum wage decreases only slightly, underscoring the durability of the minimum wage changes studied here.

To go beyond our overall assessment of the 138 case studies used for identification, we also produce event-by-event estimates of the minimum wage changes. While we find substantial heterogeneity in the bite of the events, the distribution of employment effects are consistent with a sharp null of no effect anywhere. For example, our event-by-event analysis finds that the estimated missing jobs rose substantially in magnitude with the minimum-to-median wage (Kaitz) index. At the same time, the number of excess jobs also rose for these events to a nearly identical extent. As a consequence, there is no relationship between the employment estimate and the Kaitz index up to around 59 percent, confirming that minimum wage changes in the U.S. we study have yet to reach a level above which significant disemployment effects emerge.

The lack of responses in overall employment might mask some heterogeneity in response across types of workers. Our localized approach around the minimum wage can be easily applied to various sub-groups, including those where only a small fraction of workers are affected by the minimum wage. As a result, we can provide a more complete picture of how various groups are affected by the minimum wage.

We examine whether there is a shift from low-skill to high-skill workers at the bottom of the wage distribution by partitioning workers into groups based on education and age. Comparing the number of excess jobs at and above the new minimum wage and missing jobs below it across age-by-education groups shows no evidence that low-skilled workers are replaced with high-skilled workers following a minimum wage increase. We also use demographics to predict the probability of being exposed to the minimum wage increase, and then assign workers to high, medium and low probability groups along the lines of Card and Krueger (1995). While there is considerable variation in the bite of the policy, the employment effects in these sub-groups are mostly close to zero and not statistically significant. The similar responses across demographic groups also suggests that the benefit of minimum wage policies were shared broadly.

Our approach also allows us to provide a more comprehensive picture on responses across various sectors of the economy. We show that the minimum wage is likely to have a negative effect on employment in tradable sector, and manufacturing in particular—with an employment elasticity with respect to own-wage of around -1.4—although the estimates are imprecise. At the same time, the effect of the minimum wage is close to zero in the non-tradable sectors (such as restaurants or retail), which employ most minimum wage workers in the U.S. today. This evidence suggests that the industry composition of the local economy is likely to play an important role in determining the disemployment effect of the minimum wage (Harasztosi and Lindner 2016).

This paper makes several contributions to the existing literature on minimum wages. First, our paper relates to the large and controversial literature on the employment and wage effects of the minimum wages. The debate has often been concentrated on the impact on teen employment (Card, 1992; Neumark and Wascher, 1992; Allegretto et al., 2017; Neumark et al., 2014), workers in specific sectors (Card and Krueger, 1994; Dube et al., 2010; Katz and Krueger, 1992; Lester, 1964), or workers earning low wages prior to the minimum wage increase (Abowd et al., 2000; Clemens and Wither, 2016; Currie and Fallick, 1996), while the evidence on the impact on overall employment is scant. By disaggregating the standard difference-in-differences estimates by wage bins, we can identify the effects of the minimum wage on overall employment and obtain meaningful first stage wage effects at the same time.

A notable exception studying overall employment changes is Meer and West (2016), who examine the relationship between aggregate employment at the state-level and minimum wage changes without assessing the wage effects. Meer and West (2016) find a large negative employment estimate using variants of the classic two-way fixed effects regression on log minimum wage. To highlight the importance of disaggregating the aggregate employment effects into wage bins, we calculate the bin-by-bin employment effects in such a specification. This exercise produces a striking finding: the specifications that indicate a large negative effect on aggregate employment tend to be driven by an unrealistically large drop in the number of jobs at the upper tail of the wage distribution, which is unlikely to be a causal effect of the minimum wage. We also provide an explanation for why the classic two-way fixed effect and our event study approach produces different results. We show that the large negative effects on employment is driven entirely by inclusion of the 1980s and the early 1990s in the sample—a period with very few minimum wage changes. However, aggregate employment changes in the 1980s turn out to be correlated with minimum wage changes in the 2000s. While inclusion of the 1980s biases the estimation in the two way fixed effect approach, it does not affect our event study approach that focuses on employment changes locally around the event window. It is worth noting that the disagreement on the choice of specification for estimating the impact of minimum wages on teen employment is also driven by these early period confounding shocks (Allegretto et al., 2017; Neumark et al., 2014). We find that in the post-1992 period, there is little evidence of disemployment for teens across any of the standard specfications.

Our paper also contributes to the literature on the effect of the minimum wage on overall wage inequality (Autor, Manning and Smith 2016; DiNardo, Fortin and Lemieux 1996; Lee 1999). These papers examine shifts in the wage density and assume away any possible disemployment effect. In contrast, we focus on the frequency distribution of wages instead of the wage density, which allows us to assess the effect on wage inequality and employment at the same time.[1] We show that the measured wage spillovers are not an artifact of disemployment, which would truncate the wage distribution. Additionally, we provide a wide range of evidence that these spillovers are unlikely to be an artifact of measurement error. Our spillover estimates are similar to the findings of Autor, Manning and Smith (2016) and Brochu et al. (2017), and more limited than those in Lee (1999).

Finally, our paper is also loosely related to the literature that uses bunching to elicit behavioral responses to public policies (Kleven 2016). At the same time, while most bunching analyses estimate the counterfactual distribution from purely cross sectional variation (Chetty et al., 2013; Saez, 2010), here we use a difference-in-differences strategy to construct the counterfactual frequency distribution of wages.

The rest of the paper is structured as follows. Section 2 explains the conceptual approach and the empirical implementation, Section 3 presents the main empirical findings on overall employment effects, wage spillovers and heterogenous responses to the minimum wage. Section 4 demonstrates the importance of assessing employment changes far above the minimum wage and highlights problems with the classic two-way fixed effects estimation. Section 5 concludes. Finally, all the Appendix materials can be found in the online Appendix to the paper.

[1] In a recent working paper, Brochu et al. (2017) use the hazard rate for wages to estimate spillover effects in the presence of disemployment effects.

2 Methodology and Data

2.1 The Conceptual Framework

In this paper, we infer the effect of the minimum wage from the employment changes at the bottom of the wage distribution. We illustrate our approach using Figure 1, which summarizes the effect of the minimum wage on the wage distribution. The red line shows a hypothetical (frequency) distribution of wages in the absence of the minimum wage. The blue line depicts the actual wage distribution with a minimum wage at MW.

In the presence of a binding minimum wage, there should be no jobs below MW. In practice, however, some jobs observed in the data will be sub-minimum wage because of imperfect coverage, imperfect compliance, or measurement error. Therefore, the number of missing jobs below MW, given by $\triangle b = Emp^1\left[w < MW\right] - Emp^0\left[w < MW\right]$, reflects the bite of the minimum wage.[2] Here $Emp^1[.]$ and $Emp^0[.]$ are the actual and counterfactual frequency distributions of wages, respectively.

Not all missing jobs below the minimum wage are destroyed. Some or all of the jobs below the minimum wage may be preserved—with their hourly pay raised to the minimum wage, creating a spike at MW. Some jobs may be pushed slightly above the minimum wage in order to maintain wage hierarchy within the firm, or because the minimum wage raises the bargaining power of workers (e.g. Flinn, 2011). Moreover, a minimum wage increase might induce low-wage workers to participate in job search, some of whom may find a job above the minimum wage. However, the ripple effects of the minimum wage are likely to fade out at certain point, which we denote by $\overline{W}$ in Figure 1. In models with labor market friction, wage spillovers also typically fade out, because workers and firms in the upper tail of the wage distribution are operating in different labor market segments (see Van den Berg and Ridder 1998 and Engbom and Moser 2017 for examples of such models).

The neoclassical model suggests that there may be some positive employment effects in the upper tail of the wage distribution caused by labor-labor substitution. However, as we discuss in Appendix B, because the minimum wage workers' share in the overall production is very small (around 2% in the U.S.), reasonable calibrations of a neoclassical model would suggest very small upper tail effects. For example, if we consider an elasticity of substitution between high and low-wage workers of around 1.4 based on Katz and Murphy (1992), and an output demand elasticity of around 1 based on Aaronson and French (2007), the implied

[2] When we refer to the "bite" of the minimum wage, or to the extent to which the minimum wage is "binding," we mean how effective the minimum wage is in raising wages at the bottom. Therefore, the bite is a function of (1) how many workers are earning below the new minimum wage, (2) how many of those workers are legally covered by the policy, and (3) the extent of compliance.

upper tail employment elasticity with respect to the minimum wage would be around 0.006. In Appendix Table B.1, we show that reasonable variations in the key parameters uniformly suggest that plausible estimate of minimum wage impact on upper-tail employment should be very small. Moreover, any theoretical upper tail effects would be positive, so ignoring them will overstate the measured job losses.

We assess the employment effect of the minimum wage on low-wage workers by summing the missing and excess jobs, $\triangle b + \triangle a$, which is equal to the employment change below a wage threshold $\overline{W}$: $\triangle b + \triangle a = Emp^1\left[w < \overline{W}\right] - Emp^0\left[w < \overline{W}\right]$. Such an estimator has broad similarity to the "bunching" method developed in the recent public finance literature, which uses bunching around points that feature discontinuities in incentives to elicit behavioral responses (Kleven, 2016). While the estimation of the the overall effect on low-wage jobs does not require a decomposition by wage bins relative to the minimum (e.g., into excess and missing jobs), such a decomposition does help assess both the bite of the policy, and exactly how the policy affects jobs and wages at the bottom. For example, the shape of the excess jobs can tell us about the extent of spillover.

2.2 Empirical Implementation

The key empirical challenge is to estimate the counterfactual wage frequency distribution in absence of a minimum wage increase. Instead of using either *ad hoc* functional forms (Meyer and Wise 1983, Dickens, Machin and Manning 1998) or the distribution prior to the minimum wage increase (Harasztosi and Lindner 2016) we exploit state-level variation in the minimum wage and identify the counterfactual distribution using a differences-in-differences event study design. Our event-based approach uses a similar framework as Autor et al. (2006) and examine employment changes within a 8 year window around 138 prominent state-level minimum wage events, where states increased their minimum wage by at least \$0.25, and where at least 2% of the workers were directly affected by the increase.[3] By focusing on employment changes around the event window, we incompletely capture long-run effects of the minimum wage. Nevertheless, as we show below we find no evidence of change in employment up to 5 years after the minimum wage hike, and so it strikes us as unlikely that our empirical design misses important long-term employment changes. Appendix Table A.6 shows the robustness of estimates to alternative window lengths, including allowing for up to a 7-year post-treatment period.

[3]We exclude federal increases from our primary sample of events because for these events, the change in missing jobs, Δb, is identified only from time-series variation—as there are no "control states" with a wage floor lower than the new minimum wage. However, we show in Appendix Table A.4 that our employment and wage estimates are similar when we include federal events as well.

We estimate the effect of the minimum wage not just on aggregate employment, but also on employment in every \$0.25 wage bins. Our basic regression specification is the following:

$$\frac{E_{sjt}}{N_{st}} = \sum_{\tau=-3}^{4} \sum_{k=-4}^{17} \alpha_{\tau k} I_{sjt}^{\tau k} + \mu_{sj} + \rho_{jt} + \Omega_{sjt} + u_{sjt} \tag{1}$$

where E_{sjt} is the employment in \$0.25 wage bin j in state s and at quarter t, while N_{st} is the size of the population in state s and quarter t. The treatment dummy $I_{sjt}^{\tau k}$ equals to 1 if the minimum wage was raised τ years from date t and for the \$0.25 wage bins j that fall between k and $k+1$ dollars of the new minimum wage. This definition implies that $\tau = 0$ represents the first year following the minimum wage increase (i.e., the quarter of treatment and the subsequent three quarters), and $\tau = -1$ is the year (four quarters) prior to treatment. Moreover, the $I_{sjt}^{\tau k}$ treatment variables are not only a function of state and time, but also of the wage bins. For instance, $k = 0$ represents the four \$0.25 bins between MW and $MW + \$0.99$ and $k = -1$ is a "below" bin with wages paying between $MW - \$0.01$ and $MW - \$1.00$. Our benchmark specification also controls for state-by-wage bin and period-by-wage bin effects, μ_{sj} and ρ_{jt}. This allows us to control for state-specific factors in the earnings distribution and also the nation-wide evolution of wage inequality. Finally, Ω_{sjt} include controls for small or federal increases.[4] We cluster our standard errors by state, which is the level at which policy is assigned. Our standard errors, therefore, account for the possibility that employment changes at different parts of the wage distribution may be correlated within a state.

As with any difference-in-difference design our approach identifies the causal effect of the minimum wage under the assumption that the entire frequency distribution of wages in the treated and untreated states would move in parallel in the absence of the policy change. While this assumption cannot be tested directly, we conduct a variety of checks whose results will be reported below. As is standard, we use the leading terms to assess pre-existing trends. As an added check, when we calculate event-by-event estimates in section 3.3, we test whether the distribution of leading effects is consistent with a sharp null of zero effects everywhere.

Additionally, since our approach locates the source of the employment effects within the wage distribution, we can use the upper tail employment changes as an added falsification

[4] Our primary minimum wage events exclude very small increases. To ensure they do not confound our main effects, we include controls for these small events. We also separately control for federal minimum wages. In particular, separately for small events and federal events, we construct a set of 6 variables by interacting $\{BELOW, ABOVE\} \times \{EARLY, PRE, POST\}$. Here $BELOW$ and $ABOVE$ are dummies equal to 1 for all wage bins that are within \$4 below and above the new minimum, respectively; $EARLY$, PRE and $POST$ are dummies that take on 1 if $-3 \leq \tau \leq -2$, $\tau = -1$, or $0 \leq \tau \leq 4$, respectively. These two sets of 6 variables are included as controls in the regression (Ω_{sjt} in the equation 1).

test.[5] Since large positive or negative changes in jobs paying above, say, \$15 are unlikely to reflect the causal effect of the minimum wage, reporting such employment changes in the upper tail can be highly informative about model validity. Moreover, the potential bias from the confounding factors affecting the upper tail can be especially large when only a small fraction of the workforce is directly affected by the minimum wage (as is true in the U.S.). The contribution of these omitted variables may be sizable compared to the relatively small expected effect of the minimum wage on aggregate employment. As a result, the bias arising from shocks to the upper tail can be particularly severe when we are interested in estimating the overall employment effect of the minimum wage.

There are numerous advantages of decomposing the aggregate employment changes by wage bins. First, such a decomposition allows us to focus on employment changes locally around the new minimum wage—the part of the wage distribution where we expect the policy to play a role. This variation is highly informative, yet rarely exploited. Second, and more importantly, our localized approach allows us to estimate the effects on overall employment and on subgroups where the standard approaches often fail to provide meaningful estimates on employment and wages. When only a small fraction of workers are directly affected by the minimum wage, the effect on the average wage of such subgroups will be very small. Without a clear wage effect, it is not clear how to interpret the size of any employment effect found for those groups.[6] Third, the localized focus around the minimum wage often improves the precision of estimates by filtering out random shocks to jobs in the upper part of the wage distribution.[7]

We use the estimated $\alpha_{\tau k}$ from equation 1 to calculate the change in employment throughout the wage distribution in response to the policy. The change in the number jobs (per capita) paying below the new minimum wage between event date -1 and τ can be calculated as: $\sum_{k=-4}^{-1} \alpha_{\tau k} - \sum_{k=-4}^{-1} \alpha_{-1k}$. To be clear, this is a difference-in-differences estimate, as it nets out the change in the counterfactual distribution implicitly defined by the regression equation 1. Analogously, the change in the number of jobs (per capita) paying

[5]This idea is similar to Autor et al. (2016) who use unrealistically large spillover effects to validate the empirical model in use.

[6]In Appendix Table A.1 we demonstrate that the standard approach, which looks at the wage and employment effects aggregated over the entire wage distribution, fails to produce positive and statistically significant wage effects in most cases. This indicates that the standard approach fails to capture the program effect of the minimum wage for these subgroups. At the same time, our estimates focused on low-wage jobs always produce sizable and significant wage effects. The own-wage elasticity of employment estimated using minimum wage variation is effectively a Wald-IV estimate; hence the lack of a strong "first stage" means estimates are biased towards the OLS estimate obtained by naively regressing employment on wages (Bound, Jaeger and Baker, 1995).

[7]Appendix Table A.2 confirms that the standard errors tend to be lower when we consider counts of low-wage jobs compared to an approach using total number of jobs.

between the minimum wage and $\overline{W}$ is $\sum_{k=0}^{\overline{W}-MW}\alpha_{\tau k}-\sum_{k=0}^{\overline{W}-MW}\alpha_{-1k}$. For our baseline estimates, we set $\overline{W}=MW+4$.[8] We define the excess jobs at or above the minimum wage as $\Delta a_\tau=\frac{\sum_{k=0}^{4}\alpha_{\tau k}-\sum_{k=0}^{4}\alpha_{-1k}}{\overline{EPOP}_{-1}}$, and the missing jobs below as $\Delta b_\tau=\frac{\sum_{k=-4}^{-1}\alpha_{\tau k}-\sum_{k=-4}^{-1}\alpha_{-1k}}{\overline{EPOP}_{-1}}$. By dividing the employment changes by $\overline{EPOP}_{-1}$, the sample average employment-to-population ratio in treated states during the year (four quarters) prior to treatment, we normalize the excess and missing jobs by the pre-treatment total employment. The Δa_τ and Δb_τ values plot out the evolution of excess and missing jobs over event time τ. We also report the excess and missing employment estimates averaged over the five years following the minimum wage increase, $\Delta b=\frac{1}{5}\sum_{\tau=0}^{4}\Delta b_\tau$ and $\Delta a=\frac{1}{5}\sum_{\tau=0}^{4}\Delta a_\tau$.

Given our normalization, $\Delta e=\Delta a+\Delta b$ represents the estimate for the percentage change in total employment due to the minimum wage increase. We refer to this estimate as "event-based bunching" or EB-bunching estimates to highlight that we are: 1) using an event-based difference-in-difference design, and 2) estimating the excess and missing jobs locally around the bunching in the distribution at the minimum wage.

If we divide Δe by the percentage change in the minimum wage averaged across our events, $\%\Delta MW$, we obtain the employment elasticity with respect to the minimum wage:

$$\frac{\%\Delta\text{Total Employment}}{\%\Delta MW}=\frac{\Delta a+\Delta b}{\%\Delta MW}$$

We define the percentage change in affected employment as the change in employment divided by the (sample average) share of the workforce earning below the new minimum wage the year before treatment, $\bar{b}_{-1}$:[9]

$$\%\Delta\text{Affected Employment}=\%\Delta e=\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$$

We also use the estimated coefficients to compute the percentage change in the average hourly wage for affected workers. We calculate the average wage by taking the ratio of the total wage bill collected by workers below the new minimum wage to the number of such workers. Prior to treatment, it is equal to $\overline{w}_{-1}=\overline{wb}_{-1}/\bar{b}_{-1}$. Here the wage bill, $\overline{wb}_{-1}$, and the number of workers earning below the new minimum wage just prior to the increase, $\bar{b}_{-1}$, are averages for the full sample of events. The minimum wage increase causes both the wage bill

[8] Appendix Table A.5 shows the results are robust to higher cutoffs.

[9] Notice that we divide by the actual share of the workforce and not by the change in it. As we pointed out earlier, these two are not the same if there is imperfect compliance, imperfect coverage, or measurement error in wages. While both divisions are meaningful, dividing by the actual share is the more policy relevant elasticity. This is because policy makers can calculate the actual share of workers at the new minimum wage and use the estimates presented in this paper. However, the change in the jobs below the new minimum wage is only known after the minimum wage increase, and so it cannot be used for a prospective analysis of the policy's impact.

and employment to change. The new average wage in the post-treatment period is equal to $w = \left(\overline{wb}_{-1} + \Delta wb\right) / \left(\overline{b}_{-1} + \Delta e\right)$.[10] Therefore, the percentage change in the average wage of affected workers is given by:

$$\%\Delta w = \frac{w}{\overline{w}_{-1}} - 1 = \frac{\dfrac{\overline{wb}_{-1} + \Delta wb}{\overline{b}_{-1} + \Delta e}}{\dfrac{\overline{wb}_{-1}}{\overline{b}_{-1}}} - 1 = \frac{\%\Delta wb - \%\Delta e}{1 + \%\Delta e} \tag{2}$$

The percentage change in the average wage is obtained by taking the difference in percentage change in wage bill and employment, and dividing by the retained employment share. This formula implicitly assumes the average wage change of those workers exiting or entering due to the policy is the same as the wage of affected workers those who remain employed.

Finally, armed with the changes in employment and wages for affected workers, we estimate the employment elasticity with respect to own-wage (or the "labor demand elasticity" in a competitive market):

$$\frac{\%\Delta \text{Affected Employment}}{\%\Delta \text{Affected Wage}} = \frac{1}{\%\Delta w} \frac{\Delta a + \Delta b}{\overline{b}_{-1}}$$

We calculate the standard errors for this elasticity using the delta method.

While our use of wage-bin-by-state-by-quarter data is useful for decomposing the employment changes by bins relative to the minimum wage, our employment and wage elasticities do not rely on this binning. To clarify this point, we additionally show results from a simpler method that estimates a regression using state-by-quarter data, where the outcomes are the (per capita) number of jobs or total wage bill under, say, \$15/hour, and the event indicators are just by state and quarter. We show below that the resulting employment and wage estimates (and standard errors) are very similar when using this simpler method.

2.3 Data and sample construction

We use the individual-level NBER Merged Outgoing Rotation Group of the Current Population Survey for 1979-2016 (CPS) to calculate quarterly, state-level distributions of hourly wages. For hourly workers, we use the reported hourly wage, and for other workers we define the hourly wage to be their usual weekly earnings divided by usual weekly hours. We do not

[10]The change in wage bill can be written as a function of our regression coefficients as follows. Averaging the coefficients over the 5 year post-treatment window, $\alpha_k = \frac{1}{5}\sum_{\tau=0}^{4} \alpha_{\tau,k}$, we can write $\Delta wb = \sum_{k=-3}^{4} \left(k + \overline{MW}\right) \cdot (\alpha_k - \alpha_{-1k})$, where $\overline{MW}$ is (approximately) the sample average of the new minimum wage. We say approximately because k is based on \$1 increments, and so $\overline{MW}$ is calculated as the sample mean of $[MW, MW + 1)$.

use any observations with imputed wage data in order to minimize the role of measurement error.[11] There are no reliable imputation data for January 1994 through August 1995, so we exclude this entire period from our sample. Our available sample of employment counts therefore spans 1979q1 through 1993q4 and 1995q4 through 2016q4.[12]

We deflate wages to 2016 dollars using the CPI-U-RS and for a given real hourly wage assign its earner a \$0.25 wage bin w running from \$0.00 to \$30.00.[13] For each of these 117 wage bins we collapse the data into quarterly, state-level employment counts E_{swt} using the person-level ORG sampling weights. We use estimates for state-level population aged 16 and over, N_{st}, from the CPS-MORG (which in turn is based on the Census), as the denominator for constructing per-capita counts. Our primary sample includes all wage earners and the entire state population, but below we also explore the heterogeneity of our results using different subgroups, where the bite of the policy varies.

The aggregate state-quarter-level employment counts from the CPS are subject to sampling error, which reduces the precision of our estimates. To address this issue, we benchmark the CPS aggregate employment-to-population ratio to the implied employment-to-population ratio from the Quarterly Census of Employment and Wages (QCEW), which is a near universe of quarterly employment (but lacks information on hourly wages). Appendix F explains the QCEW benchmarking in detail. As we discuss below, the QCEW benchmarking has little effect on our point estimates, but substantially increases their statistical precision.

Our estimation of the change in jobs paying below and above a new minimum wage requires us to specify minimum wage increasing events. For state-level minimum wage levels, we use the quarterly maximum of the state-level daily minimum wage series described in Vaghul and Zipperer (2016).[14] For the 138 minimum wage events, on average, 8.6% of workers were below the new minimum wage in the year before these 138 events and the mean real minimum wage increase was 10.1%.[15]

[11] The NBER CPS merged ORG data are available at http://www.nber.org/morg/. Wage imputation status markers in the CPS vary and are not comparable across time. In general we follow Hirsch and Schumacher (2004) to define wage imputations. During 1979-1988 and September 1995-2016, we define wage imputations as records with positive BLS allocation values for hourly wages (for hourly workers) and weekly earnings or hours (for other workers). For 1989-1993, we define imputations as observations with missing or zero "unedited" earnings but positive "edited" earnings (which we also do for hours worked and hourly wages).

[12] In general, there has been an increase in the rate of imputation over time. However, in Appendix Table A.3 and Appendix Figure A.2, we show that minimum wage raises are not systematically related to changes in the imputation rate. Event study estimates for the effect of minimum wages on the imputation rate show no substantial or statistically significant change 3 years before and 5 years after the treatment.

[13] We assign all wages between \$0 and \$1 to a single bin and all wages above \$30 to the \$30 bin. The resulting 117 wage bins are $(0.00, 1.25), [1.25, 1.50), \ldots, [29.75, 30.00), [30, \infty)$.

[14] The minimum wage series is available at https://github.com/benzipperer/historicalminwage/releases.

[15] All minimum wage increases including our events are shown in Appendix Figure A.1

One concern when using \$0.25 bins and CPS data is that some of the bins may be sparse with very few or no workers. However, we stress that our employment estimate is based on the *sum* of employment changes in 36 cells covering a \$9 range $[MW - \$4, MW + \$4]$, summed over at least four quarters (typically twenty quarters). As a result, small or zero employment in particular cells is not a major concern. In each state, there are, on average, around 7 workers each quarter in each of the \$0.25 bins between \$5 and \$15/hour in our sample.[16] Since the coefficients for our event dummies are estimated at a \$1-bin-year-state level, on average, for each of these we use around 112 individual-level observations per event. Moreover, when we assess the total employment effects, we calculate the sum of the \$1-bin estimates between \$4 below and \$4 above the minimum wage, and we consider 5 year averages. This implies that, on average, we use approximately 5,040 individual worker observations per event. This is a well-sized sample which allows a reliable estimate of the true counts of employment for each event. Consistent with this point, we note again that results from our approach are very similar to those from a simpler method that uses state-by-quarter data, and where the outcomes are the (per capita) number of jobs or total wage bill under \$15/hour.

Another potential concern with the data is that misreporting of wages in the CPS may bias our estimates. If reported wages contain some measurement error, some workers earning above the minimum wage will appear to earn below it, which could attenuate the estimate for Δb. However, this does not affect the consistency of the estimate for $\Delta a + \Delta b$ as long as the the minimum wage only affects reported wages below $\overline{W}$. The reason is straightforward. Assume that 1% of the workforce mistakenly report earning below the new minimum wage in the post-treatment period. This would lead our estimate of the missing jobs to be too small in magnitude: $\hat{\Delta b} = \Delta b + 0.01$. However, this misreporting would also lead to an equal reduction in the number of excess jobs above, producing the estimate $\hat{\Delta a} = \Delta a - 0.01$; this will be true as long as these misreported workers are coming from the range $[MW, \overline{W})$, which is likely to be satisfied for a wide variety of classical and non-classical measurement error processes where the support of the measurement error is contained in $[MW - \overline{W}, \overline{W} - MW]$. Therefore, the employment estimate $\hat{\Delta a} + \hat{\Delta b}$ is likely to be unaffected by measurement error in reported wages. We also directly assess how misreporting of wages in the CPS may affect our results in Appendix E, where we compare the CPS hourly wage distribution to micro-aggregated administrative data on hourly wages from three U.S. states that collect this information. Reassuringly, the evolution of the number of jobs paying below the minimum wage, and the

[16] Overall, we have 847,314 wage bin-state-period observations, which we obtained from 4,694,104 individual level observations, producing a count of 5.5 workers per \$0.25 bin. However, the count per bin is higher in the \$5-to-\$15/hour range because the upper tail wage bins are more sparse. The \$5-to-\$15/hour range is the relevant one since it contains the $[MW - \$4, MW + \$4]$ windows for all of our events.

number of jobs paying up to $5 above the minimum wage in the CPS data from these three states match quite well with their counterparts using administrative data.[17]

3 Results

We begin our analysis by estimating the effect of the minimum wage on the frequency distribution of hourly wages. Figure 2 shows the results from our baseline specification (see equation 1). We report employment changes averaged over the five year post-treatment period, $\frac{1}{5}\sum_{\tau=0}^{4}\alpha_{\tau k}$, for each dollar wage bin (k) relative to the minimum wage. Recall that all employment changes are normalized to pre-treatment total employment in the state. Several points should be noted.

First, there is a clear and significant drop in the number of jobs below the new minimum wage, amounting to 1.8% (s.e. 0.4%) of the total pre-treatment employment.[18] Around $\frac{3}{4}$ of this reduction occurs in the $1 wage bin just under the new minimum. Second, there is also a clear and significant increase in jobs just at the new minimum wage (at the $0 wage bin). Third, there is also a statistically significant increase in employment in the wage bin $3 above the new minimum and modest, statistically insignificant increases in the $1 and $2 bins. This pattern of employment changes is consistent with limited wage spillovers resulting from the minimum wage increase, as suggested in Autor, Manning and Smith (2016).[19] Fourth, the excess jobs between the new minimum and $4 above it represents 2.1% (s.e. 0.3%) of the total pre-treatment employment. Fifth, the employment changes in the upper tail wage bins,

[17] In Appendix F, we also structurally estimate a model of measurement error in reported wages proposed by Autor, Manning and Smith (2016), and show that the contribution of misreporting error to the overall variance in wages in the CPS and in administrative data on hourly wages from three U.S. states are very similar. Furthermore, we semi-parametrically deconvolve the CPS wage distribution using the estimated measurement error model and show that our estimates using this measurement error corrected distribution are very similar to the baseline estimates (Appendix Table F.3). In Appendix C we implement our approach using administrative data from Washington, and find estimates to be similar when using the CPS. While each piece of evidence has limitations, together they suggest that our employment and wage results are not likely to be biased substantially due to measurement error. At the same time, more precise wage data could help better discern the exact nature of the wage effects including the extent of spillovers, the size of the spike, and the extent of non-compliance.

[18] The discrepancy between the actual number of jobs below the new minimum, which is 8.6% of total pre treatment employment on average, and the change in the number of jobs below it, which is 1.8% on average, can be explained by the following factors. First, some of the jobs below the minimum wage (e.g. tipped workers) are exempted from the minimum wage in most states. Second, there are often multiple changes in the minimum wage in a relatively short period. In these cases, the cumulative effect of the various treatments should be considered: when we adjust for this we find the change in the number of jobs below the minimum rises in magnitude from 1.8% to 2.5%. Third, there is some wage growth even in the absence of a minimum wage increase, and our event study design controls for these changes.

[19] The $3 above the minimum wage is around the 23^{rd} percentile of the wage distribution on average. Autor, Manning and Smith (2016) finds the wage spillovers are effectively zero at around the 25^{th} percentile.

from \$5 above the minimum wage to \$17 or more (the final bin), are all small in size and statistically insignificant—both individually as well as cumulatively as shown by the red line, which represents the running sum of employment changes. Finally, it is worth emphasizing that drop in employment just below the new minimum, the equal sized increase just above it, and the lack of employment change in the upper tail is exactly what we expect if employers are complying with the law and adjusting wages but not employment.

We estimate the employment change by adding the missing jobs below and excess jobs above the minimum wage: $\triangle a + \triangle b$. We divide this change by the jobs below the new minimum wage ($\bar{b}_{-1} = 8.6\%$) to obtain a change in the affected employment of 2.8% (s.e. 2.9%), which is positive but statistically insignificant. We can also divide the employment change $\Delta a + \Delta b$ by the sample-averaged minimum wage increase of 10.1% to calculate the employment elasticity with respect to the minimum wage of 0.024 (s.e. 0.025). This estimate is statistically insignificant, and the 95% confidence interval rules out substantial reductions in the aggregate employment, including the baseline aggregate employment elasticity of -0.074 in Meer and West (2016) (see their Table 4). The most common minimum wage employment elasticities are from teens; for example, Neumark and Wascher (2008) argue this falls between -0.1 and -0.3, while Allegretto et al. (2017) argue that it is closer to zero. However, the directly affected share of teens (43.2%) is much larger than the workforce overall (8.6%). Therefore, to make our estimates on overall employment comparable to the estimates for teens we can multiply our estimate and standard errors by the ratio of the shares 0.432/0.086=5.02. This leads to an affected-share-adjusted 95% confidence interval of [-0.13, 0.37], which rules out most of the -0.1 to -0.3 range.

Second, using the formula in equation 2 we can also calculate the change in the average wage and the employment elasticity with respect to own wage (i.e., the labor demand elasticity in the competitive model). We estimate that the effect of the minimum wage on average wages is 6.8% (s.e. 1.0%), which is statistically significant. The estimate for the elasticity of employment with respect to own wage is 0.411 (s.e. 0.430). The confidence intervals rule out any own-wage elasticities more negative than -0.450 at the 95 percent confidence level. Such a lower bound rules out many estimates in the literature that found negative employment elasticity (see Appendix Figure A.7; also, Neumark and Wascher (2008) argue that the own-wage employment elasticity can easily be -1 or even -2). .

Figure 3 shows the changes in the missing jobs paying below the new minimum wage (Δb_τ), and the excess jobs paying up to \$4 above the minimum wage (Δa_τ) over annualized event time using our baseline specification. All the estimates are expressed as changes from event date $\tau = -1$, or the year just prior to treatment, the estimates for which are normalized to zero. There are four important findings that we would like to highlight. First, we find a very clear reduction in the jobs paying below the new minimum wage (shown in red) between the year just prior to treatment ($\tau = -1$) and the year of treatment ($\tau = 0$)—this shows that the minimum wage increases under study are measurably binding. Second, while there is some reduction in the magnitude of the missing jobs in the post-treatment window, it continues to be very substantial and statistically significant five years out, showing that the treatments are fairly durable over the medium run.[20] Third, the response of the excess jobs at or above the new minimum (Δa) exhibits a very similar pattern in magnitudes, with the opposite sign. There is an unmistakable jump in excess employment at $\tau = 0$, and a substantial portion of it persists and is statistically significant even five years out. Fourth, for both the changes in the excess and missing jobs there is only a slight indication of a pre-existing trend prior to treatment. The $\tau = -2$ leads are statistically indistinguishable from zero and although there is some evidence of changes three years prior to treatment, the leading effects are very small relative to the post-treatment effect estimates. Moreover, the slight downward trend in excess jobs, and the slight upward trend in missing jobs is consistent with falling value of the real minimum wage prior to treatment. The sharp upward jump in both the excess and missing jobs at $\tau = 0$, the lack of substantial pre-treatment trends, and the persistent post-treatment gap between the two shares all provide strong validation of the research design. Appendix Figure A.5 shows analogous time paths for wages and employment showing sharp and persistent wage effect at $\tau = 0$ coupled with little change in employment over the event window–either before or after treatment.

Robustness Checks. In Table 1, we assess the robustness of the main results to including additional controls for time-varying, unobserved heterogeneity. This is particularly important since results in the existing literature are often sensitive to the inclusion of various versions of time varying heterogeneity (e.g., Neumark, Salas and Wascher 2014 and Allegretto et al. 2017). In Column (1) we report the five-year-averaged post-treatment estimates for the baseline specification shown in Figures 2 and 3. Columns (2) and (3) add wage-bin-by-state specific linear and quadratic time trends, respectively. Note that in the presence of 3 pre-treatment and 5 post-treatment dummies, the trends are estimated using variation outside of the 8 year window around the treatment, and thereby unlikely affected by either lagged or anticipation effects. Columns (4)-(6) additionally allow the wage-bin-period effects to vary by the 9 Census divisions. Column (6) represents a highly saturated model allowing for state-specific quadratic time trends and division-period effects for each $0.25 wage bin.

Overall, the estimates from the additional specifications are fairly similar to the baseline estimate. In all cases, there is a clear bite of the policy as measured by the reduction in jobs

[20] The durability of the treatment can also be seen in Appendix Figure A.4 which plots the progression of the minimum wage using our event study design.

paying below the minimum, Δb. Consistent with the presence of a substantial bite, there is statistically significant increase in real wages of affected workers in all specifications: these range between 5.7% and 6.9% with common wage-bin-period effects (columns 1, 2, 3,), and between 4.3% and 5.0% with division-specific wage-bin-period effects (columns 4, 5 and 6). In contrast, the proportionate change in employment for affected workers is never statistically significant, and is numerically smaller than the wage change, ranging between -1.9% and 3.6% across the 8 specifications. For most part, the employment estimates are small or positive; the only exception is column (5) with state-specific linear trends and bin-division-specific period effects. The employment elasticities with respect to wage are -0.449 (s.e. 0.574) . However, adding quadratic trends to the former specification (column 6) substantially reduces the magnitude of the employment elasticity with respect to the wage to -0.003 (s.e. 0.455).

Finally, column (7) provides employment and wage estimates using a state-by-period panel, where we regress either per-capita wage bill or employment under an absolute wage threshold ($\overline{W}$), and then estimate the change in affected wage and employment using the same formulae as our baseline.[21] The estimates and standard errors for affected employment (0.025, se 0.029) and wage (0.063, se 0.011) are virtually identical to column 1, clarifying that use of wage bins or choices around those have no impact on our key estimates. At the same time, unlike our baseline specification, this simpler method using an absolute wage threshold cannot provide separate estimates for excess and missing jobs.

Appendix Table A.4 shows that our results are robust to focusing only on the events occurring in the states that do not allow tip credits; dropping occupations that allows tipping; using full-time equivalent job counts; restricting the sample to hourly workers; additionally using federal-level minimum wage changes for identification; using the raw CPS data instead of the QCEW benchmarked CPS; without using population weights; focusing on the post-1992 period. We also show robustness to alternative event window lengths (Appendix Table A.6), and alternative values of the upper end point of the wage window, $\overline{W}$ (Appendix Table A.5).

3.1 Heterogenous Responses to the Minimum Wage

We can use our approach focused on low wage jobs to estimate the effect of the minimum wage on specific subgroups.

By demographic groups. We assess the presence of labor-labor substitution at the bottom of the wage distribution by examining employment responses across various demographic groups.[22] In Table 2 we report estimates for workers without a high school degree,

[21] The threshold is $W = 15$, which is at least \$4 above the new minimum wage in all of our events but one.

[22] Existing evidence on labor-labor substitution has typically focused on specific groups like teens (Giuliano, 2013), individual case studies (Fairris and Bujanda, 2008), or specific segments like online labor platforms

those with high school or less schooling, women, black or Hispanic individuals, and teens using our baseline specification (see equation 1).

As expected, restricting the sample by education and age produces a larger bite. For example, for those without a high school degree, the missing jobs estimate, Δb, is -6.5% while for those with high school or less schooling it is -3.2%. These estimates for the missing jobs are, respectively, 261% and 78% larger than the baseline estimate for the overall population (-1.8%, from column 1 in Table 1). Nevertheless, the large variation in the missing jobs across various demographic groups matched closely by excess jobs above the new minimum wage.[23] 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. As a result, the employment elasticities with respect to own wage range between -0.086 and 0.570 for the first five demographic groups of the table. In all cases but one, the elasticities are statistically indistinguishable from zero. The sole exception is those without a high school degree, for whom the employment elasticity with respect to the wage is 0.475 (s.e. 0.268) and is marginally significant at the ten percent level. The minimum wage elasticity for teens is 0.125, which is more positive than some of the estimates in the literature, though we note that it is not statistically significant given a standard error of 0.134.[24]

In addition, we examine the effects on groups of workers with differential probability of being exposed to the minimum wage changes. To determine the likelihood of exposure, we construct a prediction model analogous to Card and Krueger (1995). We use observations from three years prior to the 138 events that also lie outside any of the 5-year post-treatment windows and estimate a linear probability model of having a wage less than 125% of the statutory minimum wage on a rich set of demographic predictors.[25] We use the estimated

(Horton, 2018).

[23]In Appendix A we also show that the close match between excess jobs and missing jobs holds also if we fully partition the workforce into 23 age-education cells.

[24]We note that the teen estimates are unrelated to a focus on low-wage jobs, since the benefits of focusing on employment changes around the minimum wage is small for groups where most workers are low wage ones. In Appendix Table A.10 we show that our event study estimates are close to zero for teens even if we use *overall* teen employment. At the same time, the classic two-way fixed effect specification with log minimum wage (TWFE-logMW) generates a sizable negative estimate for teens and for overall employment as well. In Section 4 we discuss this discrepancy and argue that the difference between our approach and TWFE-logMW are driven by how the two empirical model affected by employment shocks in the 1980s and early 1990s. Appendix Table G.7 shows that in the post-1992 period, there is little divergence in teen elasticity across standard specifications (including the TWFE-logMW); none of the specifications suggest noticeable losses to teen employment, and the elasticities are no more negative than -0.03.

[25]We use the exact same predictors as in Card and Krueger (1995): all three-way interactions of non-white, gender, and teen indicators; all three-way interactions of non-white, gender, and age 20-25 indicators; an indicator for having less than high school education; continuous highest grade completed variable; a third order polynomial in labor-market experience; Hispanic ethnicity indicator; interactions of the education and

model to obtain predicted probabilities of being exposed to minimum wage increases for all individuals in the sample regardless of their actual employment status. We then use the predicted probabilities to place individuals in three groups: a "high probability" group that contains individuals in the top 10 percent of the predicted probability distribution; a "low probability" group that contains workers in the bottom 50 percent of the predicted probabilities; and a middle group containing the rest.

As expected, the high probability group shows a considerably larger bite ($\Delta b = -9.4\%$) than the middle group ($\Delta b = -2.0\%$), and the low probability group ($\Delta b = -0.4\%$). At the same time, the employment elasticities are very similar across the three Card and Krueger probability groups. It is worth mentioning that the most precise estimate of the own-wage employment elasticity reported in this paper appears in column (6) of Table 2, where we look only locally around the minimum wage and also focus on the high probability group: the confidence interval rejects any value smaller than -0.251 and larger than 0.663. Such a confidence interval is quite narrow and rejects many estimates in the literature—highlighting the gains from combining the demographic profiling approach of Card and Krueger with the approach based on low-wage jobs advanced in this paper. The employment elasticities for the other groups are similar in magnitude, though less precise.[26]

Overall, these findings provide little evidence of heterogeneity in the employment effect by skill level; the lack of a reduction in overall low-wage jobs does not appear to mask a shift in employment from low-skill to high-skill workers.

By industrial sectors. Much of the literature has focused on specific sectors like restaurants where the minimum wage is particularly binding—therefore making it easier to detect a clear effect on the sectoral wage. In contrast, by focusing on changes at the bottom of the distribution, our approach can recover employment and wage responses even in industries where only a small fraction of workers are directly affected by the minimum wage increase. This allows us to provide a more comprehensive assessment of the effect of the policy across a range of industries.

In Table 3 we report estimates for various sectors in the economy. We assign workers to tradable and non-tradable sectors following Mian and Sufi (2014).[27] The table shows that the

experience variables with gender. Cengiz (2018) shows the predictions using this Card and Krueger model compare favorably with those from more sophisticated machine learning based methods.

[26]In Appendix A we show that if we estimate the impact of the events on the aggregate wage and employment outcomes for each of the three probability groups, we can obtain a clear wage effect only for the high probability group—capturing only around 36% of all minimum wage workers. This highlights the value of focusing at the bottom of the wage distribution which allows us to get an overall estimate for all low wage workers.

[27]Mian and Sufi (2014) define "tradable" industries as having either the sum of imports and exports exceeding \$10,000 per worker or \$500 million total; their "non-tradable" sector consists of a subset of restaurant and retail industries; "construction" consists of construction, real estate or land development-

bite of the minimum wage varies a lot across industries. The minimum wage is highly binding in the restaurant sector with a missing jobs estimate of 10.1%, while it does not appear to be binding in the construction sector. The minimum wage is more binding in the non-tradable sector (6.6%) than in the tradable sector (1.6%) or in the manufacturing sector (1.7%).

The effect of the minimum wage on employment also varies by sector. We find that the number of excess jobs at or above the minimum wage is smaller than the missing jobs in the tradable sector, and so the employment effect is negative (-11.1%, s.e. 13.6%), albeit not statistically significant. Similarly, the point estimate in the manufacturing sector suggests that around 10.1% (s.e. 14.5%) of the jobs directly affected by the minimum wage are destroyed. The implied employment elasticity with respect to own wage is quite large in magnitude in both sectors (-1.910 in the tradable and -1.385 in manufacturing), although the estimates are imprecise and statistically insignificant.

At the same time, we find no indication for negative disemployment effects in the non-tradable, restaurant, and retail sectors where most minimum wage workers are employed in the U.S. The employment elasticity with respect to own wage in the non-tradable sector is positive (0.387, s.e. 0.597), which is in stark contrast to the tradable sector, where we find a large negative elasticity. Harasztosi and Lindner (2016) find similar sectoral patterns in Hungary and argue, using revenue data, that the larger job losses for tradable reflect a more elastic consumer demand in that sector.

By pre-treatment employment status. We consider the effect of the minimum wage separately on workers who were employed prior to the minimum wage increase (incumbent workers) and for new entrants into the labor market. We partition our sample of wage earners into incumbent workers and new entrants by exploiting the fact that the CPS interviews each respondent twice, exactly one year apart.[28] The partition limits our sample to the 1980-2016 time period covering 137 eligible minimum wage-raising events and also restricts our time window to one year around the minimum wage increase rather than the five years in our baseline sample.

Figure 4 shows the event study estimates for new entrants (panel a) and incumbents (panel b) for each k-dollar wage bin relative to the new minimum wage. For both subgroups,

related industries. We use the list in Mian and Sufi (2014) of 4-digit NAICS industries and Census industry crosswalks to categorize all the industries in the CPS for 1992-2016. In our sample the shares of employment are 13%, 14%, 10%, for tradable, non-tradable, construction, respectively. See more details in Appendix E. Since consistent industrial classifications limit our sample to the 1992-2016 period, we first replicate our benchmark analysis using all industries for this restricted sample in column (1) in Table 3. The estimated employment and wage effects on this restricted sample are similar to the full 1979-2016 sample.

[28]All CPS respondents are interviewed for four months in the first interview period, then rotated out of the survey for eight months, and then rotated back into the survey for a final four months of interviews. In the fourth month of each interview period (the "outgoing rotation group"), respondents are asked questions about wages. Appendix Appendix E explains how we match workers across rotation groups.

new minimum wages clearly bind, with significantly fewer jobs just below and significant more at the new minimum. This highlight that studies that restricts its sample to incumbent workers (e.g. Abowd et al. 2000; Currie and Fallick 1996; Clemens and Wither 2016) can only provide a partial characterization of the full effects of the minimum wage increase, since new entrants are also affected by the policy.

For both groups the excess jobs closely match the missing jobs (for incumbents $\Delta a = 1.3\%$ and $\Delta b = -1.2\%$ and for new entrants $\Delta a = 0.6\%$ and $\Delta b = -0.5\%$) and so the net employment changes are approximately zero. The green and blue solid lines show the running sums of employment changes up to the corresponding wage bin for each group. The lines show that in both cases there is little change in upper tail employment. We note that if employers are replacing lower skilled workers with higher skilled ones, we should expect to see some reduction in jobs for previously employed workers, perhaps offset by high skilled entrants; the lack of job loss for incumbents provides additional evidence against such labor-labor substitution. The affected wage increase for incumbents (9.5%, s.e. 2.0%) is significantly larger than it is for new entrants (1.9%, s.e. 1.3%) and some of these differences can be explained by the lack of spillover effects for the new entrants. In the next section we return to this issue.

3.2 Wage spillovers

So far we have focused on the employment effects of the minimum wage. However, an equally important question is understanding the nature of the wage effects. In this section, we quantify the direct effect of the minimum wage and the indirect effect that comes from wage spillovers.

We calculate the direct (or "no spillover") wage increase by moving each missing job under the new minimum wage exactly to the new minimum wage:

$$\%\Delta w_{\text{no spillover}} = \frac{\sum_{k=-4}^{-1} k \left(\alpha_k - \alpha_{-1k}\right)}{\overline{wb}_{-1}} \tag{3}$$

The total wage increase of affected workers, $\%\Delta w$, in equation 2 incorporates both this direct effect as well as the add-on effect from wage spillovers. Therefore, the difference between the two measures, $\%\Delta w - \%\Delta w_{\text{no spillover}}$, provides an estimate of the size of the wage spillovers.

Note that our spillover estimates use the frequency distribution of wages, which contrasts with the earlier literature relying on the density of wages (see e.g. Card and Krueger 1995; DiNardo, Fortin and Lemieux 1996; Lee 1999; Autor, Manning and Smith 2016). As a result, changes in employment—which could create an artificial spillover effect when using the wage density—do not affect our estimates.

We report our estimates of wage spillovers in Table 4, where the columns show estimates of the total wage effect $\%\Delta w$, the "no spillover" wage effect $\%\Delta w_{\text{no spillover}}$, and the spillover share of the total wage increase calculated as $\frac{\%\Delta w - \%\Delta w_{\text{no spillover}}}{\%\Delta w}$. The first row shows the estimated effects for the entire workforce. Column (1) repeats the estimated total wage effect from Column (1) in Table 1, which is 6.8% (s.e. 1.0%). Column (2) shows that in the absence of spillovers, wages would increase by 4.1% (s.e. 0.9%). Column (3) shows that 39.7% (s.e. 11.9%) of the total wage effect is caused by the ripple effect of the minimum wage.

In Table 4 we also report estimates for several subgroups. The share of spillovers in the total wage increase is relatively similar for several key demographic groups, such as those without a high school degree (37.0%), teens (34.7%), those without a college degree (40.2%), and women (35.9%). In most cases, the spillover share is statistically significantly different from zero at the 5 percent level. One exception is Black or Hispanic individuals, for whom the estimated share of wage spillover is much smaller at 17.9% (s.e. 26.5%), which is less than half of the 39.7% (s.e. 11.9%) spillover share for all workers. Although the difference is not statistically significant, this finding nonetheless suggests that the wage gains at the bottom may be more muted for some disadvantaged groups.[29]

We also find a substantially smaller change in wages due to spillovers in the tradable sector, though the estimates here are a bit imprecise. This highlights that wage affects are small in the tradable sector, and some of it may be undone by clawbacks from higher wage workers. The combination of this evidence and the disemployment effects suggest that there may be more unintended consequences of minimum wages when the tradable sector constitutes a more sizable share of the affected workforce.

We also find a stark difference in the spillover shares of wage increases for incumbents versus new entrants. Incumbents receive a larger total wage increase (9.5%) than the overall workforce (6.8%), but the spillover share for incumbents and all workers is relatively similar (42.2% and 39.7%, respectively). In contrast, the spillover share for entrants is -17.8%, suggesting that essentially all of the wage increase received by new entrants is through the creation of jobs at or very close to the new minimum. Larger spillovers for incumbents relative to entrants can also be seen in Figure 4. Two points should be noted.

First, the stark differences in the size and scope of spillovers for the incumbent and for the new entrants are inconsistent with a simple measurement error process common to both groups. This suggest that spillover effects found are likely to reflect real responses and not measurement error in CPS-based wages, a possibility that is raised by Autor, Manning and

[29]The smaller spillover for Black/Hispanic workers is not due to sectoral or incumbency composition, which are very similar to other workers (results not reported).

Smith (2016).[30]

Second, since we find that essentially none of the wage spillovers accrue to workers who were not employed prior to the minimum wage increase, it is unlikely that our estimates of spillovers primarily reflect an increase in the value of the outside options or reservation wages of non-employed workers (e.g. Flinn 2006). In contrast, the spillovers may reflect some "optimization friction" that firms face when they set incumbent workers' wages. Kleven (2016) discusses a range of optimization frictions in the context of bunching at kink points. Moreover, our results also consistent with Dube, Giuliano and Leonard (2018) who argue that firms are constrained by relative-pay norms inside the firm.

3.3 Event-specific estimates

So far, most of our evidence has come from averaging the effects across all 138 events. In this section, we estimate treatment effects for each of the events separately, and assess how these impacts vary when we consider minimum wage increases that are more binding.

For this purpose, we create 138 data sets, one for each event h. The data sets include the state of event j and all clean control states for 8 year panel by event time. Clean control states are those that do not have any non-trivial state minimum wage increases in the 8 year panel around event h; other states are dropped from data set h. We calculate event-specific per-capita number of jobs in \$1 wage bins relative to the minimum wage for each state-by-year. Then, the regression equation is,

$$Y_{skth} = \sum_{\tau=-3}^{4} \alpha_{\tau kh} I_{sth}^{\tau} + \mu_{skh} + \rho_{kth} + \Omega_{skth} + u_{skth}, \tag{4}$$

where k indicates the k^{th} dollar bin relative to the minimum wage. Then, Y_{skth} is the per-capita number of jobs in state s time t, and k^{th} bin relative to the minimum wage in data set h. The calculation of event-specific change in excess jobs above (Δa_h), change in missing jobs below (Δb_h), and employment change ($\Delta e_h = \Delta a_h + \Delta b_h$) are similar to the ones described in section 2.2. Ω_{jst} controls for other primary, federal, and small events whose 5-year post-treatment periods take place within the data set h. It takes the value of 1 for all post-treatment periods of these events.[31]

[30]In Appendix C we implement our approach using administrative data from Washington. In that data we find similar spillover effects which provides additional evidence that the spillovers are not primarily caused by CPS-specific misreporting by survey respondents. In addition, as shown in Appendix Table F.3, our wage estimates are similar using a deconvolved distribution which purges the type of measurement error proposed in Autor, Manning and Smith (2016).

[31]Figure D.1 reports event-specific estimates for excess, missing jobs and employment effect, along with (Ferman and Pinto, forthcoming) confidence intervals that are appropriate for a single treated unit and

Figure 5 panel (a) shows the non-parametric bin-scattered relationship between the event by event estimates on missing jobs and the new minimum wage.[32] To calculate the former we use the ratio of the minimum wage to the median wage, also known as the $Kaitz$ index (e.g., Lee 1999, Dube 2014, Autor, Manning and Smith 2016, Manning 2016). When the minimum wage is high relative to the median, it is expected to have a larger bite. Consistent with that expectation, we find that events h with a higher minimum-to-median wage ratio had substantially more missing jobs — the coefficient on $Kaitz_h$ is sizable and statistically significant at -0.133 (s.e. 0.034). At the same time, when we consider excess jobs, we find that the coefficient on $Kaitz_h$ has a very similar magnitude at 0.139 (s.e. 0.057). In other words, when the minimum wage is high relative to the median, the events have a bigger bite and a greater number of missing jobs below the new minimum, but also have a nearly equally sized number of excess jobs at or above the new minimum. As a consequence, the employment effect is virtually unchanged (slope = 0.006 (s.e. 0.048)) as we consider minimum wages that range between 37% and 59% of the median wage, as shown in the bottom panel of Figure 5. Overall, these findings suggest that that the level of the minimum wage increases in the U.S. that we study have yet to reach a point where the employment effects become sizable.

4 Employment Changes along the Wage Distribution in the Classic Two-Way Fixed Effect Regression on log Minimum Wage

In the previous section, we estimated the impact of minimum wages on the wage distribution using our event study specification. We found that the effect of the minimum wage was concentrated at the bottom of the wage distribution, and reassuringly we found no indication of considerable employment changes in the upper tail of the wage distribution (see Figure 2). The lack of responses $4 above the minimum wage or higher also implies that the effect of

heteroscedasticity. 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 5.3% of estimates are statistically significant at the 5 percent level. In addition, the stacked event-by-event estimates can be also used to estimate the average effect of the minimum wage across events. Table D.1 we report estimates using that approach and show that estimates are very similar to our panel regression based event study. This shows that issues about negative weighting using staggered treatments (e.g., Abraham and Sun, 2018) are unlikely to be driving our results. Finally, the event-by-event estimates in Figure D.2 confirm that the lack of leading effects and upper tail employment changes hold event-by-event, and not just on average: only 5.4% of the events experience a statistically significant upper tail effects at the 5 percent level, while 7.7% the events experience statistically significant leading effects. For additional details, see Appendix D.

[32] We control for state-level unemployment rate at the time of the minimum wage increase, political orientation of the state, urban share of the state, and the decade of the minimum wage increase. However, the results are very similar if we leave out controls see Appendix Figure A.9.

the minimum wage on aggregate employment is close to the estimated employment effect at the bottom of the wage distribution. Such stability of upper-tail employment is consistent with the observation that the share of workers affected by the minimum wage changes we study is too small to affect upper tail employment to a noticeable degree.

In this section, we estimate the effect of the minimum wage on employment throughout the wage distribution using alternative identification strategies to illustrate the advantage of the distributional approach in diagnosing research designs. Recent empirical literature using the classic two-way fixed effect specification with log minimum wage (TWFE-logMW), has found large aggregate disemployment effects in the U.S. context (see Meer and West 2016).

We decompose the classic two-way fixed effects estimate of log minimum wage on the state level employment-to-population rate. In Figure 6 we divide the total wage-earning employment in the 1979-2016 Current Population Survey into inflation-adjusted $1-wage bins by state and by year. Then, for each wage bin, we regress that wage bin's employment per capita on the contemporaneous, 4 annual lags, and 2 annual leads of log minimum wage, along with state and time fixed effects.[33] This distributed lags specification is similar to those used in numerous papers (e.g., Meer and West 2016, Allegretto et al. 2017).[34] The histogram bars show the average post-treatment effect divided by the sample average employment-to-population rate,[35] while the dashed purple line plots the running sum of the employment effects of the minimum wage up to the particular wage bin. The final purple bar represents the estimated effect on aggregate employment to population rate.

Figure 6 panel (a) shows that, on average, minimum wage shocks are associated with large employment changes in the real dollar bins in the $6 to $9/hour range. There is a sharp decrease in employment in the $6/hour and $7/hour bins, likely representing a reduction in jobs paying below new minimum wages; and a sharp rise in the number of jobs in the $8/hour and $9/hour wage bins, likely representing jobs paying above the new minimum. At the same time, the figure also shows consistent, negative employment effects of the minimum wage for levels far above the minimum wage: indeed, the aggregate negative employment elasticity (e.g. -0.137) accrues almost entirely in wage bins exceeding $15/hour.

[33] In the TWFE-logMW model, the point estimates for the leads and lags show the impact relative to the employment in the 3rd year or earlier. Once we normalize the TWFE-logMW estimates to the first lead, we can report 3 leads and 4 lags, similarly to our benchmark estimates.

[34] Meer and West (2016) present unweighted results on the total employment effect of the minimum wage. Here we present estimates weighted by the population size as it is more standard in the literature and also closer to our event study estimates. However, as we show in the Appendix Figure A.11, the unweighted estimates are similar.

[35] We construct the cumulative response over event dates 0, 1, ..., 4 relative to event date -1 by successively summing the coefficients for contemporaneous and lagged minimum wages. We then average the cumulative responses over dates 0,1, ... , 4. This average post-treatment effect is analogous to what we did in our event-based analysis in the previous sections.

It strikes us as implausible that a minimum wage increase in the \$8 to \$9/hour range causally leads to losses of jobs mostly at or above the median wage, even though the minimum wage is binding far lower in the wage distribution. More plausibly, this suggests that the specification is confounded by negative employment shocks to the upper part of the wage distribution (possibly much earlier than the actual treatment dates), and these shocks are not fully absorbed by the simple two-way fixed effect specifications estimated using a long panel. For instance, as shown in Appendix Table G.2, the negative employment changes shown in Figure 6 arise only for the Card and Krueger low probability group, which should not be affected by the minimum wage. At the same time, the high and medium probability groups exhibit no negative disemployment effect.

How is it possible that our benchmark specification that focuses on employment changes around the event window leads to such different results compared to the TWFE-logMW specification? The differences in the estimates suggest that the negative employment changes in upper part of the wage distribution in TWFE-logMW must come from outside of the event window. Indeed, we find that the employment losses are driven by the 1980s expansion and the 1990-91 recession, even though most of the minimum wage changes in our sample occurred after 2000. When we restrict the regression to the 1993-2016 period—the period where 86% (118/138) of all our events occurred—we indeed find very similar estimates across the two specifications: the TWFE-logMW specification in this sample suggests small employment effects and little upper tail employment changes (Appendix Figure G.5). Moreover, even if we limit our sample to the 39 states with no state minimum wage increases until the late 1990s, the negative disemployment effects are driven entirely by the inclusion of a period (1980s) long before any cross-state variation in treatment occurred in these states (see Appendix Table G.5).

We also show in Appendix Figure G.4 that the overall employment-to-population rate evolved very similarly between the early 1990s and 2016 in high minimum wage states (those that instituted a minimum exceeding the federal standard after the early 1990s) and low minimum wage states (where the federal standard was always binding). This is noteworthy because the period between the early 1990s and 2016 is when much of the cross-sectional variation in minimum wages emerged. Importantly, however, the employment-to-population rate had diverged between these two groups of states during the 1980s, at least a decade before most high minimum wage states started to raise their minimum wage. This creates a spurious correlation between employment changes in the 1980s and minimum wage changes in the early 2000s that confounds the TWFE-logMW specification in the full sample, which is sensitive to shocks occurring long before the event window. However, this does not affect our event study estimates, since these only consider employment changes within the event window.

This also explains why we do not find any pre-existing trends in our event based analysis, while the TWFE-logMW in the full sample exhibits sizable and statistically significant leads. At the same time, TWFE-logMW estimates restricted to the post-1992 sample produces neither sizable leads nor sizable employment effects (see further details in Appendix G). [36]

The above example illustrates that showing the effect of the minimum wage throughout the wage distribution can provide additional falsification tests and therefore be an useful tool for model selection. This type of model selection tool can be particularly helpful in the context of minimum wages, where the literature has often grappled with figuring out the "right" empirical model.[37]

5 Discussion

In this paper we infer the employment effects of the minimum wage from the change in the frequency distribution of wages. The key advantage of this approach is that it allows us to assess the overall impact of the minimum wage on low-wage workers, who are the primary target of minimum wage policies. We use an event study analysis exploiting 138 prominent minimum wage increases and provide a robust and comprehensive assessment of how minimum wages affect the frequency distribution of wages. Second, we calculate the number of missing jobs just below the minimum wage, the number of excess jobs at or slightly above the minimum wage, and also the job changes in the upper tail of the wage distribution. Our main estimates show that the number of excess jobs at and slightly above the minimum wage closely matches the number of missing jobs just below the minimum wage, while we find no evidence for employment changes at or more than $4 above the minimum wage. A similar pattern obtains for low-skilled workers, suggesting labor-labor substitution is unlikely

[36] Why are the expansion in the 1980s and the downturn in the 1990-1991 recession related to future minimum wage changes? Because the expansion and downturn were more pronounced for states that would be more Democratic-leaning in the 2000s. One possibility is that the 1990-1991 recession was so severe in some states that it changed the political landscape and opened the door for candidates supporting minimum wages. However, another explanation is that the 1990-1991 recession just happened to be more pronounced for Democratic-leaning states—states that would also be more inclined to raise the minimum wage starting in the early 2000s following a long period of federal inaction. In Appendix G we show that this latter explanation fits the data better. In particular, we show that the predictive power of the severity of recession on future minimum wage increases disappears once we control for the partisan voting index (PVI) in the 2000s and instrument that variable with the PVI in 1988 (i.e., prior to the 1990-1991 recession).

[37] Appendix G also relates these findings to other parts of the literature. We show in Table G.7 that the expansion in the 1980s and the downturn during 1990-1991 is also responsible for the sensitivity of teen employment estimates to specification that has plagued the literature, such as controls for trends. In the post-1992 period, there is little divergence in teen elasticity across standard specificaitons (including the TWFE-logMW): they all suggest any losses to teen employment are small, with elasticities no more negative than -0.03. Use of trend controls also matters little in the post-1992 sample which is where most minimum wage variation is.

to be a factor in our setting. Moreover, we find that the level of the minimum wages that we study—which range between 37% and 59% of the median wage—have yet to reach a point where the job losses become sizable. However, the employment consequences of a minimum wage that surpass the ones studied here remain an open question. Furthermore, if minimum wage increases affect tradable sectors more, our findings suggest employment effects may be more pronounced.

A key advantage of tracking job changes throughout the wage distribution is that we can transparently show the source of any disemployment effects. As a result, we can detect when an empirical specification suggests an unrealistic impact on the shape of the wage distribution. More importantly, the relationship between minimum wages and the wage distribution can also be used to infer the structure of low-wage labor markets. While providing a unified theoretical framework is beyond the scope of this paper, our empirical results on the wage distribution together with the estimates on labor-labor substitution across demographic groups and the heterogenous responses across sectors provide new empirical findings which can be used to test and distinguish various theories of the low-wage labor market.

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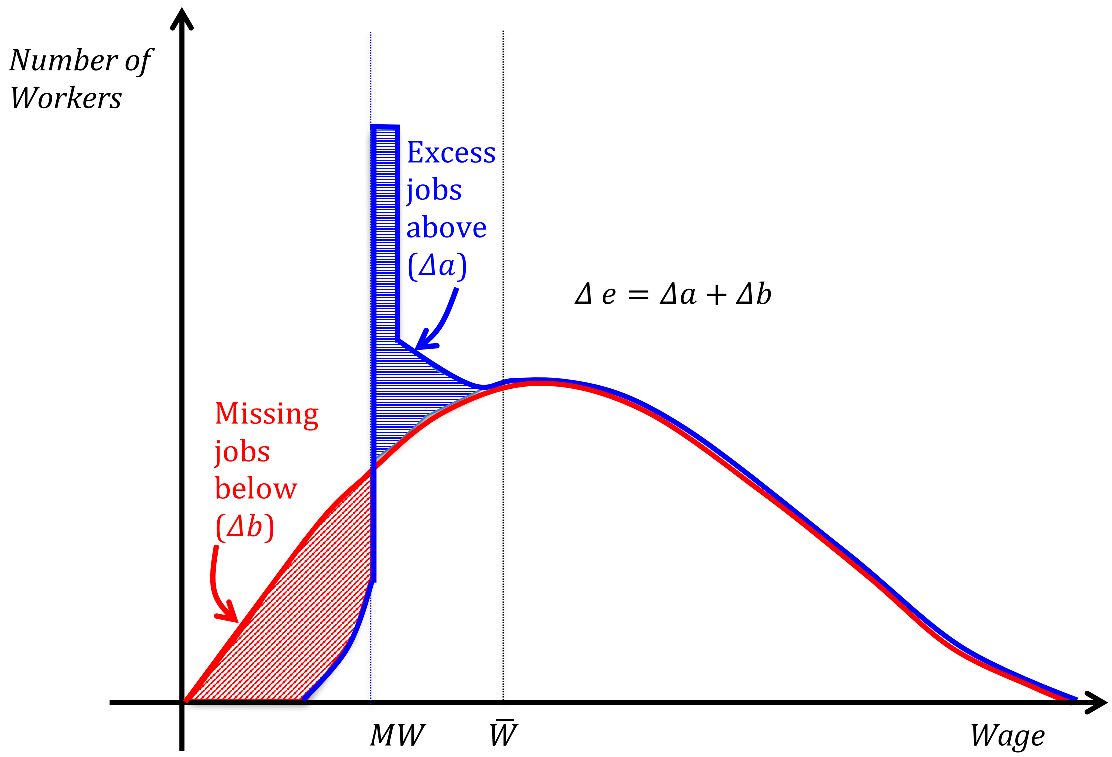
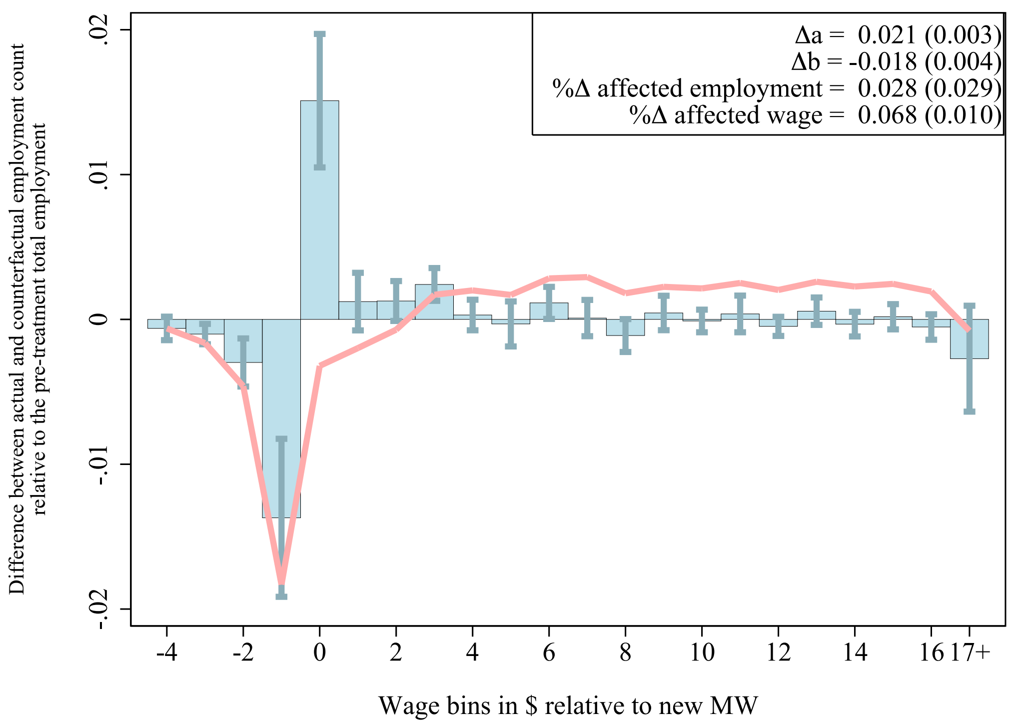
Figure 1: An Illustration of the Impact of Minimum Wages On the Frequency Distribution of Wages



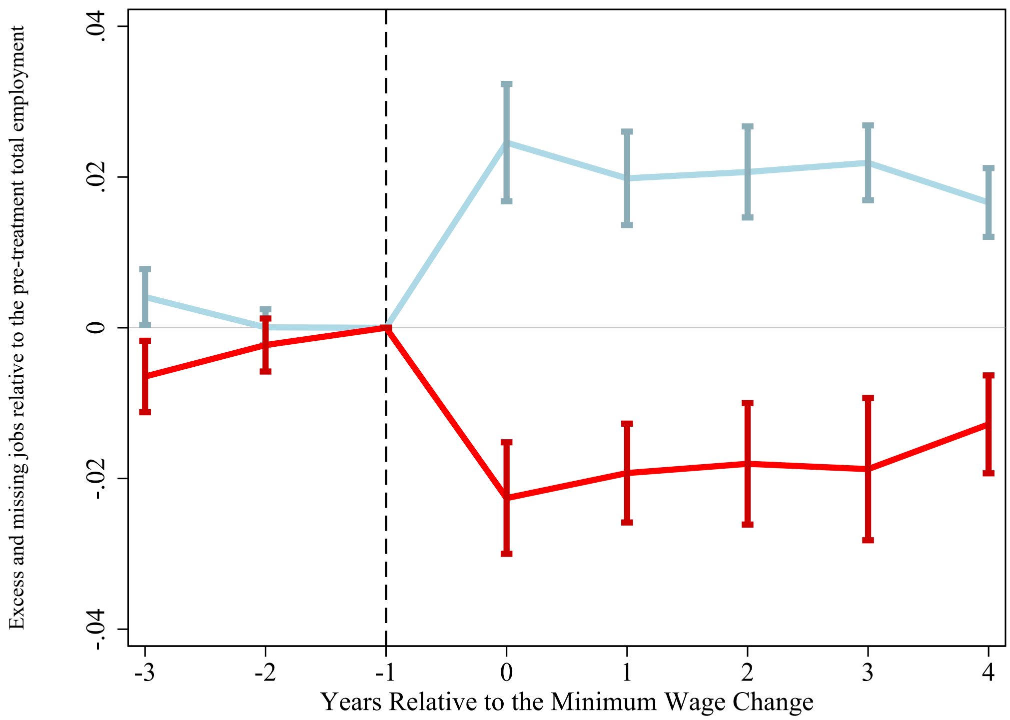
Notes: The figure shows the effect of the minimum wage on the frequency distribution of hourly wages. The red solid line shows the wage distribution before, and the blue solid line after the introduction of the minimum wage. Since compliance is less than perfect, some earners are uncovered and the post-event distribution starts before the minimum wage. For other workers, shown by the red shaded area between origin and MW (Δb), introduction of minimum wage may increase their wages, or those jobs may be destroyed. The former group creates the "excess jobs above" (Δa), shown by the blue shaded area between MW and $\overline{W}$, the upper limit for any effect of minimum wage on the earnings distribution. The overall change in employment due to the minimum wage (Δe) is the sum of the two areas ($\Delta a + \Delta b$).

Figure 2: Impact of Minimum Wages on the the Wage Distribution



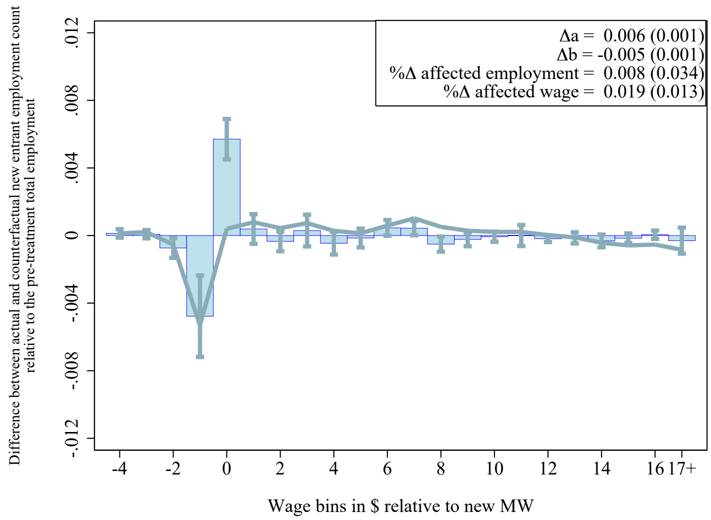
Notes: The figure shows the main results from our event study analysis (see equation 1) exploiting 138 state-level minimum wage changes between 1979-2016. The blue bars show for each dollar bin (relative to the minimum wage) the estimated average employment changes in that bin during the 5-year post-treatment relative to the total employment in the state one year before the treatment. The error bars show the 95% confidence interval using standard errors that are clustered at the state level shown using the error bar. The red line shows the running sum of employment changes up to the wage bin it corresponds to.

Figure 3: Impact of Minimum Wages on the Missing and Excess Jobs Over Time

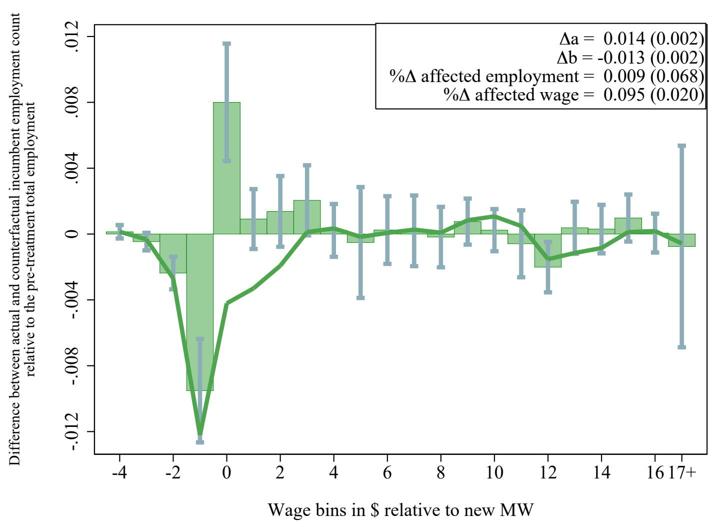


Notes: The figure shows the main results from our event study analysis (see equation 1) exploiting 138 state-level minimum wage changes between 1979-2016. The figure shows the effect of a minimum wage increase on the missing jobs below the new minimum wage (blue line) and on the excess jobs at and slightly 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 (Δb); and the red lines show the number of jobs between the new minimum wage and \$5 above it (Δa). We also show the 95% confidence interval based on standard errors that are clustered at the state level.

Figure 4: Impact of Minimum Wages on the Wage Distribution by Pre-Treatment Employment Status: New Entrants and Incumbents



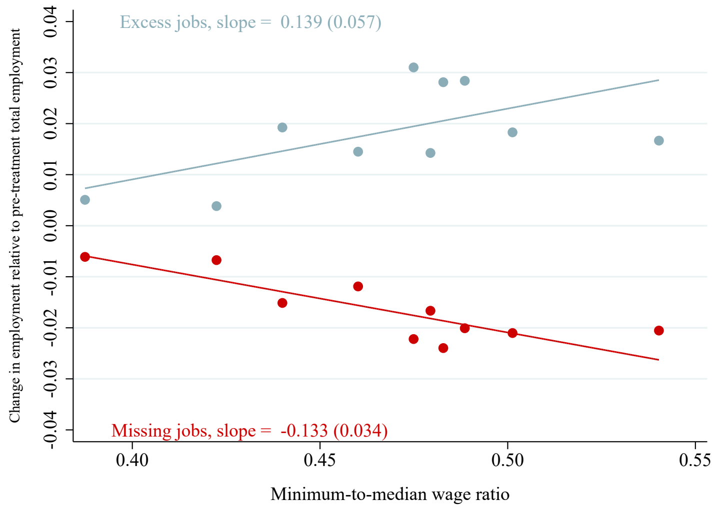
(a) New entrants



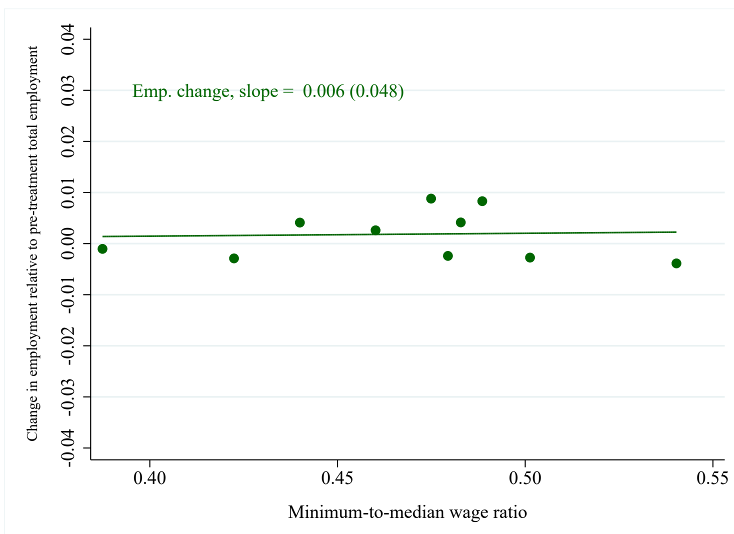
(a) Incumbents

Notes: The figure shows the main results for new entrants (panel a) and for incumbents (panel b) from our event study analysis (see equation 1) exploiting 138 state-level minimum wage changes between 1979-2016. The blue bars show for each dollar bin the estimated change in the number of new entrants in that bin 1-year post-treatment relative to the total employment of the new entrants 1 year before the treatment. The green bars show the equivalent for incumbents. Incumbent workers were employed a year prior to the minimum wage increase, whereas new entrants were not. The error bars show the 95% confidence interval calculated using standard errors that are clustered at the state level. The green and blue lines show the running sum of employment changes up to the wage bin they correspond to for new entrants and incumbents, respectively. The figures highlight that the ripple effect of the minimum wage mainly comes from incumbent workers.

Figure 5: Relationship between Excess Jobs, Missing Jobs, Employment Change and the Minimum-to-Median Wage Ratio Across Events



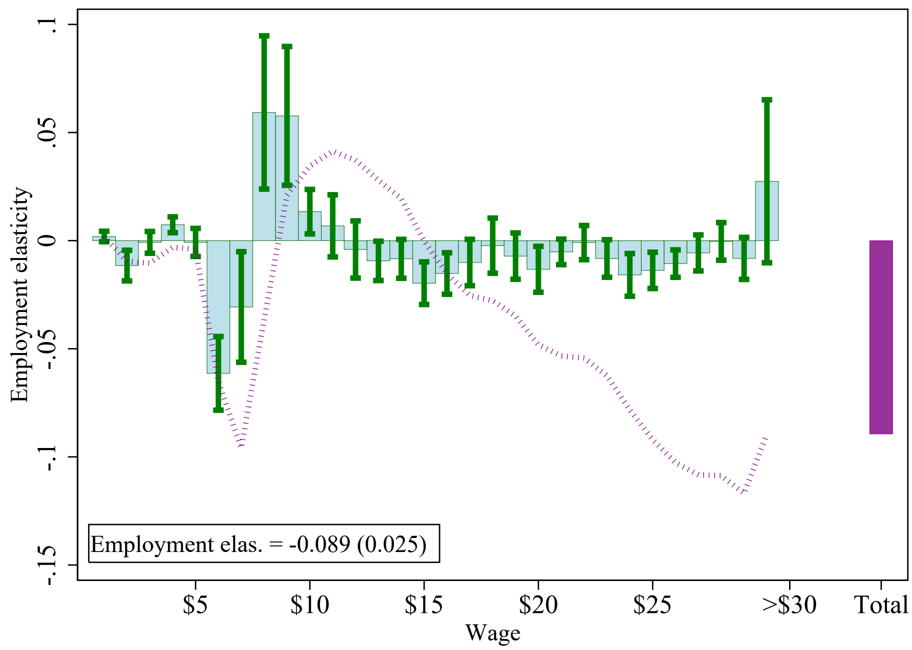
(a) Missing and excess jobs



(b) Employment change

Notes: 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 130 events exclude 8 minimum wage raising events in the District of Columbia, since individual treatment effects are very noisily estimated for those events. (See Appendix Figure A.10 for a raw scatterplot including the 8 events in DC.) 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 bin-scatters and linear fits control for decade dummies, state-specific unemployment rate at the time of the minimum wage increase, the urban share of the state's population, and an indicator for being a Republican-leaning state. 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 6: Impact on Employment throughout the Wage Distribution in the Two-Way Fixed Effects Model on log Minimum Wages



Notes: The figure shows the effect of the minimum wage on the wage distribution in fixed effects (TWFE-logMW) specification. We estimate two-way (state and year) fixed effects regressions 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 is the elasticity of the overall state employment-to-population with respect to minimum wage, obtained from regressions where the outcome variable is 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 are weighted by state population. The figure highlights that large aggregate disemployment effects are often driven by shifts in employment at the upper tail of the wage distribution.

Table 1: Impact of Minimum Wages on Employment and Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Missing jobs below new MW (Δb)	-0.018***	-0.018***	-0.018***	-0.016***	-0.016***	-0.015***	
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	
Excess jobs above new MW (Δa)	0.021***	0.018***	0.020***	0.016***	0.014***	0.015***	
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	
%Δ affected wages	0.068***	0.057***	0.068***	0.049***	0.043***	0.050***	0.065***
	(0.010)	(0.010)	(0.012)	(0.010)	(0.010)	(0.011)	(0.010)
%Δ affected employment	0.028	0.000	0.022	-0.002	-0.019	-0.000	0.027
	(0.029)	(0.023)	(0.021)	(0.021)	(0.021)	(0.023)	(0.028)
Employment elasticity w.r.t. MW	0.024	0.000	0.019	-0.001	-0.016	-0.000	0.023
	(0.025)	(0.020)	(0.018)	(0.018)	(0.018)	(0.019)	(0.024)
Emp. elasticity w.r.t. affected wage	0.411	0.006	0.326	-0.032	-0.449	-0.003	0.410
	(0.430)	(0.402)	(0.313)	(0.439)	(0.574)	(0.455)	(0.421)
Jobs below new MW ($\bar{b}_{-1}$)	0.086	0.086	0.086	0.086	0.086	0.086	0.086
%Δ MW	0.101	0.101	0.101	0.101	0.101	0.101	0.101
Number of events	138	138	138	138	138	138	138
Number of observations	847,314	847,314	847,314	847,314	847,314	847,314	14,484
Number of workers in the sample	4,694,104	4,694,104	4,694,104	4,694,104	4,694,104	4,694,104	4,694,104
Controls							
Bin-state FE	Y	Y	Y	Y	Y	Y	
Bin-period FE	Y	Y	Y	Y	Y	Y	
Bin-state linear trends		Y	Y		Y	Y	
Bin-state quadratic trends			Y			Y	
Bin-division-period FE				Y	Y	Y	
State FE							Y
Year FE							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) shows the benchmark specification while Columns (2)-(6) explore robustness to bin-state time trends and bin-division-period fixed effects. Column (7) reports the simpler methodology estimates where we calculate changes in affected wage and employment by using state-by-quarter data, where the outcomes are the number of jobs or total wage bill under $15 per hour. 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 2: Impact of Minimum Minimum Wages on Employment and Wages by Demographic Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Missing jobs below new MW (Δb)	-0.065***	-0.032***	-0.114***	-0.023***	-0.028***	-0.094***	-0.020***	-0.004***
	(0.010)	(0.007)	(0.010)	(0.005)	(0.008)	(0.010)	(0.005)	(0.001)
Excess jobs above new MW (Δa)	0.075***	0.038***	0.127***	0.026***	0.028***	0.100***	0.021***	0.004***
	(0.011)	(0.006)	(0.020)	(0.004)	(0.006)	(0.012)	(0.003)	(0.001)
%Δ affected wages	0.080***	0.076***	0.083***	0.072***	0.044***	0.073***	0.051***	0.060*
	(0.014)	(0.014)	(0.018)	(0.011)	(0.012)	(0.011)	(0.013)	(0.032)
%Δ affected employment	0.038	0.043	0.030	0.025	-0.004	0.015	0.015	0.011
	(0.024)	(0.030)	(0.032)	(0.027)	(0.044)	(0.018)	(0.048)	(0.055)
Employment elasticity w.r.t. MW	0.097	0.061	0.125	0.025	-0.005	0.052	0.016	0.003
	(0.061)	(0.042)	(0.134)	(0.027)	(0.058)	(0.062)	(0.049)	(0.014)
Emp. elasticity w.r.t. affected wage	0.475*	0.570	0.356	0.343	-0.086	0.206	0.304	0.184
	(0.268)	(0.386)	(0.317)	(0.362)	(1.005)	(0.233)	(0.904)	(0.841)
Jobs below new MW ($\bar{b}_{-1}$)	0.264	0.145	0.432	0.102	0.133	0.358	0.104	0.027
%Δ MW	0.103	0.103	0.102	0.101	0.100	0.103	0.103	0.103
Number of events	138	138	138	138	138	138	138	138
Number of observations	847,314	847,314	847,314	847,314	846,729	847,314	847,314	847,314
Number of workers in the sample	660,771	2,248,711	287,484	2,277,624	781,003	469,226	1,830,393	2,349,485
Sample	Less than high school	High school or less	Teen	Women	Black or Hispanic	High probability	Medium probability	Low probability

Notes. The table reports effects of a minimum wage increase by demographic groups 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 for individuals without a high school degree (Column 1), for individuals with high school degree or less schooling (Column 2), for teens (Column 3), for women (Column 4), for black or Hispanic workers (Column 5). Columns (6)-(8) report the results for groups of workers with differential probability of being exposed to the minimum wage changes. We use the Card and Krueger (1995) demographic predictors to estimate the probability of being exposed (see the text for details). Column 6 shows the results for the workers who have a high probability of being exposed to the minimum wage increase, Column (7) for the middle probability group, and Column (8) for the low probability group. All specifications include wage bin-by-state and wage bin-by period fixed effects. Regressions are weighted by state-quarter aggregated population of the demographic groups. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.5, *** 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 3: Impact of Minimum Minimum Wages on Employment and Wages by Sectors (1992-2016)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Missing jobs below new MW (Δb)	-0.019***	-0.016*	-0.066***	-0.003	-0.011***	-0.101***	-0.033***	-0.017**
	(0.004)	(0.008)	(0.007)	(0.002)	(0.003)	(0.015)	(0.003)	(0.008)
Excess jobs above new MW (Δa)	0.020***	0.011	0.072***	0.005	0.011***	0.101***	0.041***	0.011
	(0.003)	(0.008)	(0.011)	(0.006)	(0.002)	(0.015)	(0.010)	(0.009)
%Δ affected wages	0.058***	0.058	0.056***	0.097	0.056***	0.049***	0.060***	0.073
	(0.011)	(0.073)	(0.014)	(0.086)	(0.013)	(0.012)	(0.021)	(0.078)
%Δ affected employment	0.008	-0.111	0.022	0.051	0.009	-0.001	0.062	-0.101
	(0.031)	(0.136)	(0.037)	(0.163)	(0.044)	(0.026)	(0.080)	(0.145)
Employment elasticity w.r.t. MW	0.007	-0.056	0.060	0.019	0.005	-0.002	0.086	-0.052
	(0.027)	(0.069)	(0.103)	(0.059)	(0.026)	(0.117)	(0.111)	(0.074)
Emp. elasticity w.r.t. affected wage	0.140	-1.910	0.387	0.530	0.166	-0.011	1.040	-1.385
	(0.523)	(3.922)	(0.597)	(1.311)	(0.763)	(0.542)	(1.058)	(2.956)
Jobs below new MW ($\bar{b}_{-1}$)	0.087	0.050	0.270	0.036	0.057	0.434	0.136	0.050
%Δ MW	0.098	0.098	0.098	0.098	0.098	0.098	0.098	0.098
Number of events	118	118	118	118	118	118	118	118
Number of observations	554,931	554,931	554,931	554,931	554,931	554,931	554,931	554,931
Number of workers in the sample	2,652,792	358,086	384,498	274,812	1,504,643	156,634	315,397	349,749
Sector:	Overall	Tradable	Nontradable	Construction	Other	Restaurants	Retail	Manufacturing

Notes. The table reports the effects of a minimum wage increase by industries based on the event study analysis (see equation 1) exploiting 118 state-level minimum wage changes between 1992 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 for all sectors (Column 1), tradable sectors (Column 2), non-tradable sectors (Column 3), construction (Column 4), other sectors (Column 5), restaurants (Column 6), retail (Column 7), and manufacturing industries (Column 8). Our classification of tradable, non-tradable, construction and other sectors follows Mian and Sufi (2014) (see Appendix D for the details). 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 4: The Size of the Wage Spillovers

	%Δ affected wage		Spillover share of wage increase
	$\%\Delta w$	$\%\Delta w_{\text{No spillover}}$	$\frac{\%\Delta w - \%\Delta w_{\text{No spillover}}}{\%\Delta w}$
Overall	0.068***	0.041***	0.397***
	(0.010)	(0.009)	(0.119)
Less than high school	0.077***	0.048***	0.370***
	(0.013)	(0.009)	(0.078)
Teen	0.081***	0.053***	0.347***
	(0.015)	(0.007)	(0.059)
High school or less	0.073***	0.043***	0.402***
	(0.013)	(0.011)	(0.100)
Women	0.070***	0.045***	0.359***
	(0.011)	(0.010)	(0.120)
Black or Hispanic	0.045***	0.037***	0.179
	(0.012)	(0.010)	(0.265)
Tradable	0.058	0.065**	-0.114
	(0.073)	(0.028)	(1.157)
Non-tradable	0.056***	0.043***	0.237
	(0.014)	(0.006)	(0.191)
Incumbent	0.095***	0.055***	0.422**
	(0.020)	(0.011)	(0.181)
New entrant	0.019	0.023***	-0.178
	(0.013)	(0.006)	(0.748)

Notes. The table reports the effects of a minimum wage increase on wages based on the event study analysis (see equation 1) exploiting 138 state-level minimum wage changes between 1979 and 2016. The table reports the percentage change in affected wages with (Column 1) and without (Column 2) taking spillovers into account for all workers, workers without a high school degree, teens, individuals with high school or less schooling, women, black or Hispanic workers, in tradable industries, in non-tradable industries, those who were employed 1 year before the minimum wage increase (incumbents); and those who did not have a job 1 year before (new-entrants). The first column is the estimated change in the affected wages calculated according to the equation 2 in Section 2.2, and the second column assumes no spillovers (see equation 3 in Section 3.2). In the last column, the spill-over share of the wage effect is calculated by subtracting 1 from the ratio of the estimates in the second to the first column. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

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, Δb, 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 shows the average change in the minimum wage after the 138 events we use in our baseline specification. The figure depicts a sizable, statistically significant and persistent increase in the minimum wage starting from the first year of events ($\tau = 0$) compared to the controls. On average, states with minimum wage events experience an increase of 8.4% (0.7%) in the 5 post-treatment years, supporting our definition of minimum wage event.

Figure A.5 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.5 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.5 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.6 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.7 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.7 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.8 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.8 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.[38] For each of these 23 groups, we separately estimate a regression using our baseline specification, and calculate changes in missing (Δb_g) and excess jobs (Δa_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 μ_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 (-Δb) 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.8, 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

[38]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.

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.9 shows the event-by-event relationship between missing jobs, excess jobs, 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.10 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.9. However we keep those events in the rest of the paper where we report the event study estimates.

Figure A.11 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 our preferred estimator focusing locally around the minimum wage using 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 local 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 local 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 local estimator. In almost all cases, the local 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, for which our estimator obtains 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.

In this paper we infer 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 local 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 local 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 local approach modestly smaller, at 88% of the aggregate estimator. In the cases of workers with lower education, the local 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 local 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.[39] 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 (Δb) 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, Δb, 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

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

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 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 (Δb) in the number of workers below the new minimum wage; however, it does not create a bias in our 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 Appendix G 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 local (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 local 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 local 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 local 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 local estimator enables us to establish a causal relationship between the policy and the employment effects for each of the groups.

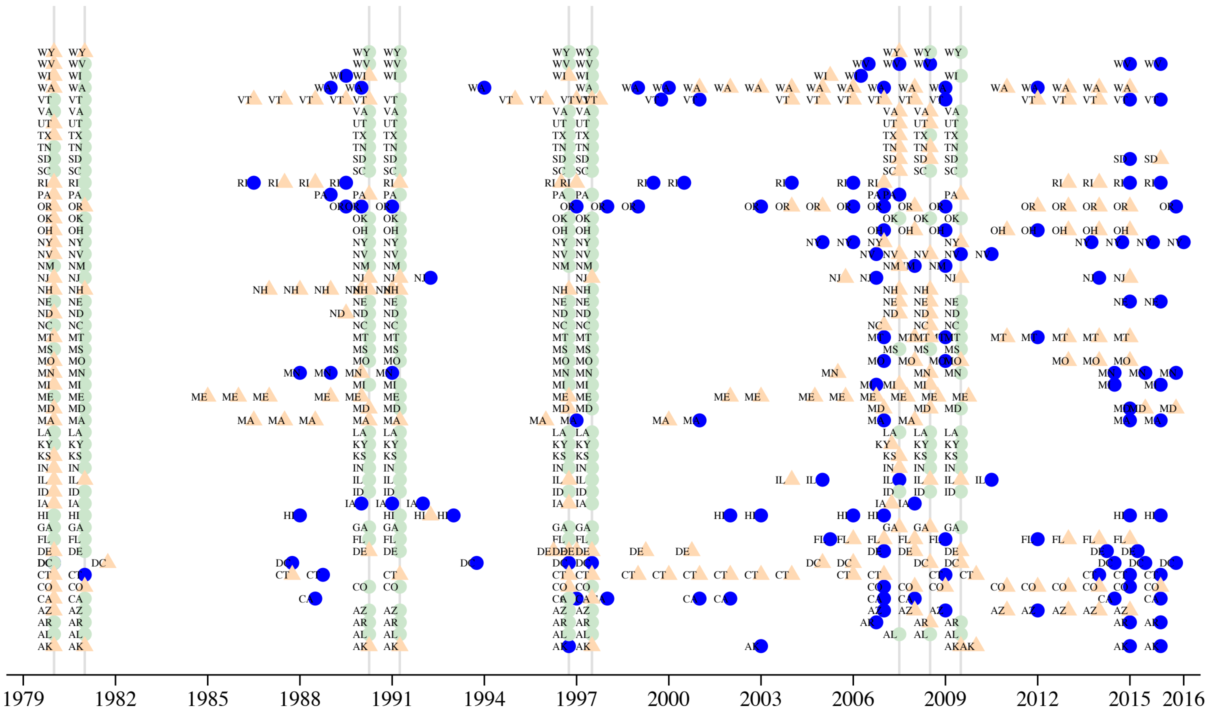
Table A.8 shows the impact of the minimum wage for incumbents and for new entrants to the labor force. Since CPS interviews individuals twice (one year apart), we can only assess a short term impact of the minimum wage for these two subgroups. However, columns (1) and (2) highlight that the short term 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 Appendix E 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 Appendix G). 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.

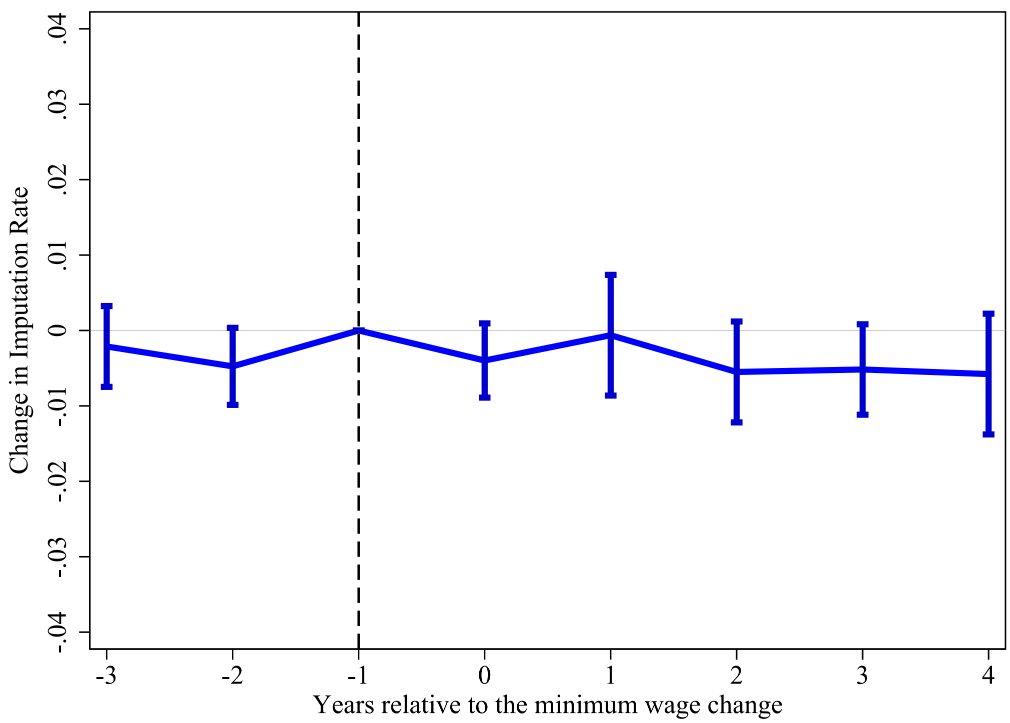
Panel B of Table A.10 shows the TWFE-logMW, first difference (FD), and event-based (EB) regressions for teens (see the details in Appendix G). The variability in the estimates for teens is not driven by changes in employment in the upper tail. This is not surprising, since most teens earn below $15, 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



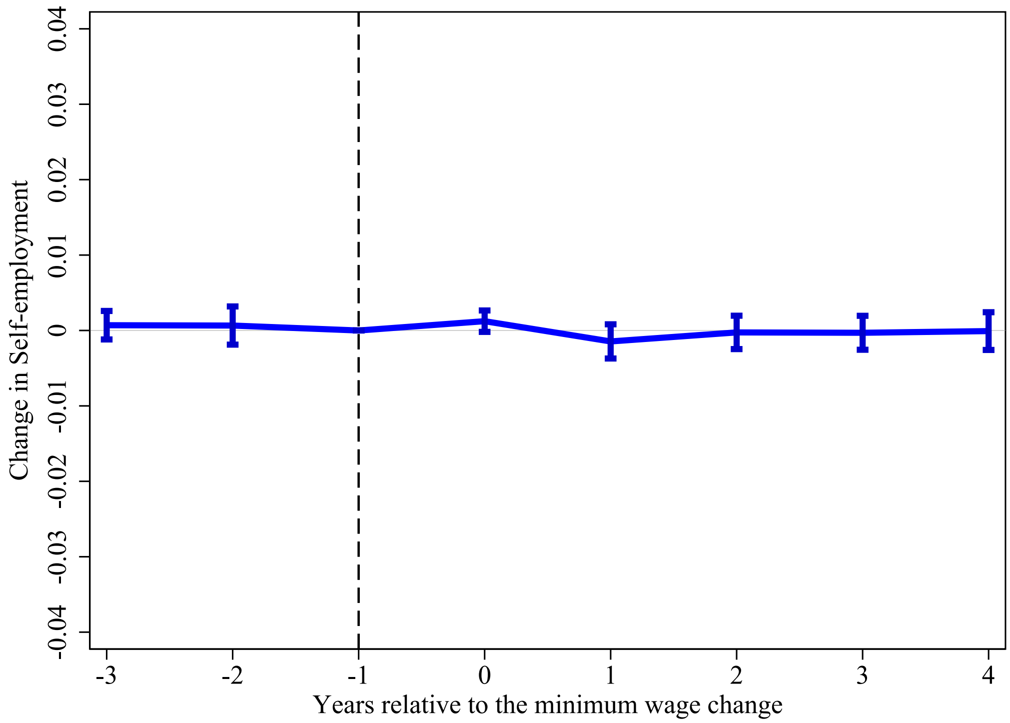
Notes: The figure shows all minimum wage increases between 1979 and 2016. There are a total of 623 minimum wage increases. The blue circles show the primary minimum wage events used in estimating equation 1; the light orange triangles highlight small minimum wage changes where minimum wage increased 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, Δb, 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



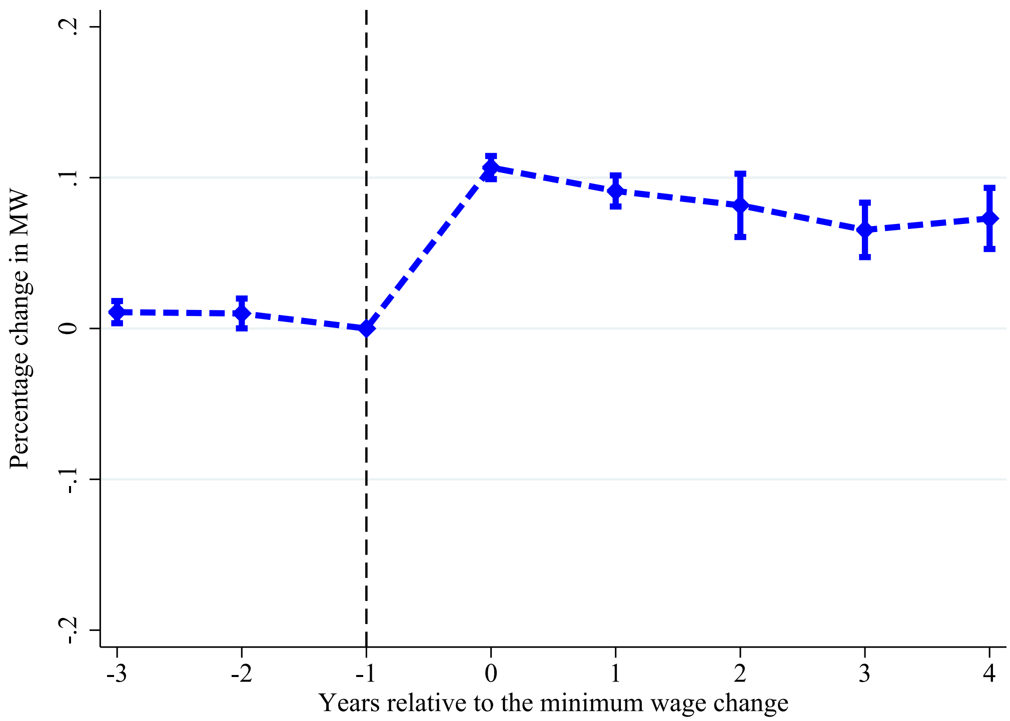
Notes: The figure shows the effect of the minimum wage on the imputation rate. In our event study analysis we only use non-imputed hourly wages. To alleviate the concern that imputation has an effect on our estimates, we implement an event study regression where the outcome variable is state-level imputation 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 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



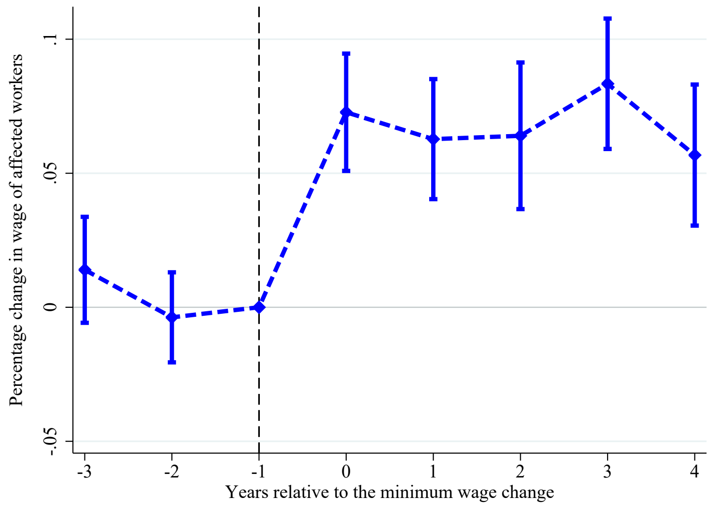
Notes: The figure shows the effect of the minimum wage on the self-employment rate. In our event study analysis we only use wage workers. To alleviate the concern that changes in self-employment rate have effects on our estimates, we implement an event study regression where the outcome variable is state-level 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: Average Progression of Minimum Wages Around 138 Events

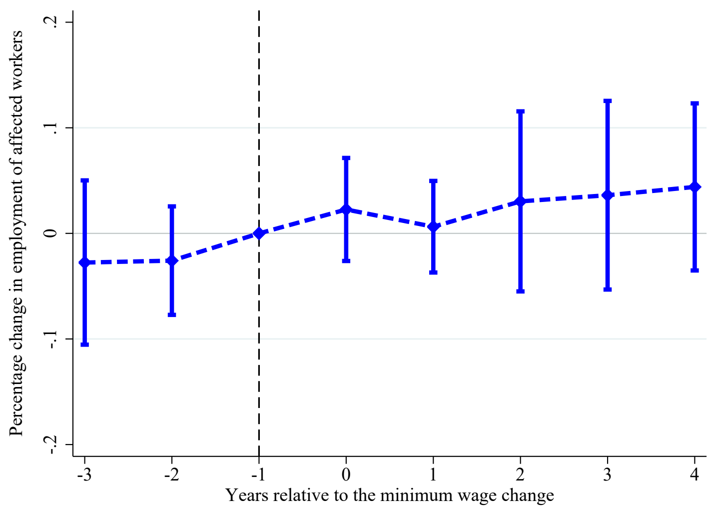


Notes: The figure shows the average increase of the minimum wage in the event window. Events are the 138 state-level minimum wage changes between 1979-2016 that we use in our benchmark specification. Similarly to our benchmark specification we include state and time fixed effects in the regression. The blue line shows the evolution of the minimum wage (relative to the year before the treatment) compared to the counterfactual. We also show the 95% confidence interval based on standard errors that are clustered at the state level.

Figure A.5: Impact of Minimum Wages on Average Wage and on Employment Over Time



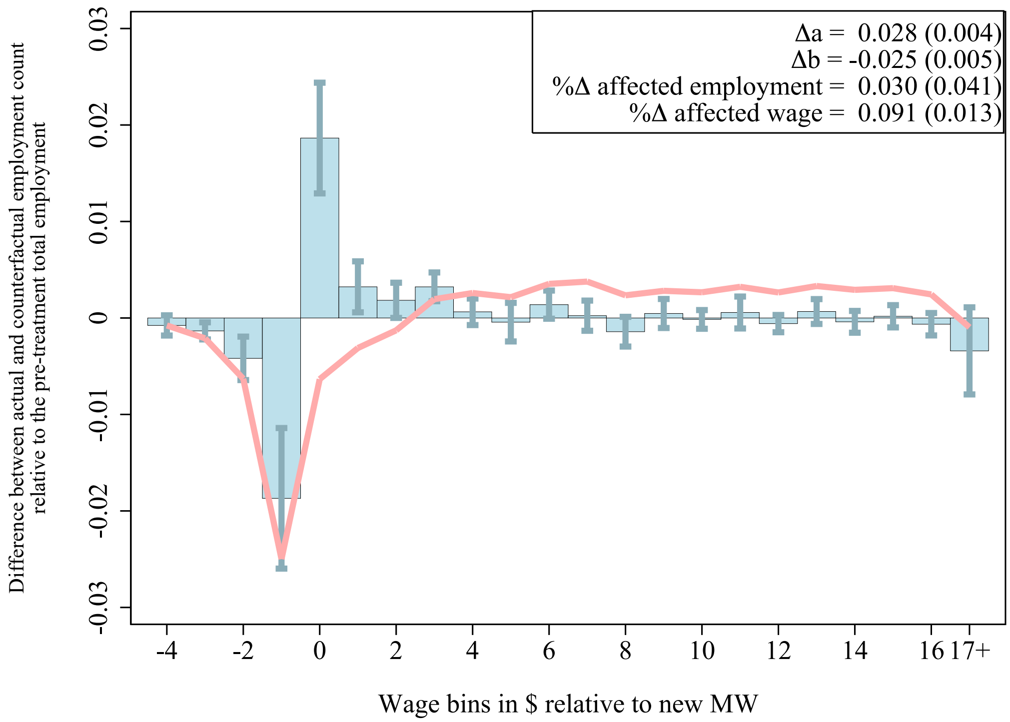
(a) Evolution of the average wage of the affected workers



(b) Evolution of the employment of the affected workers

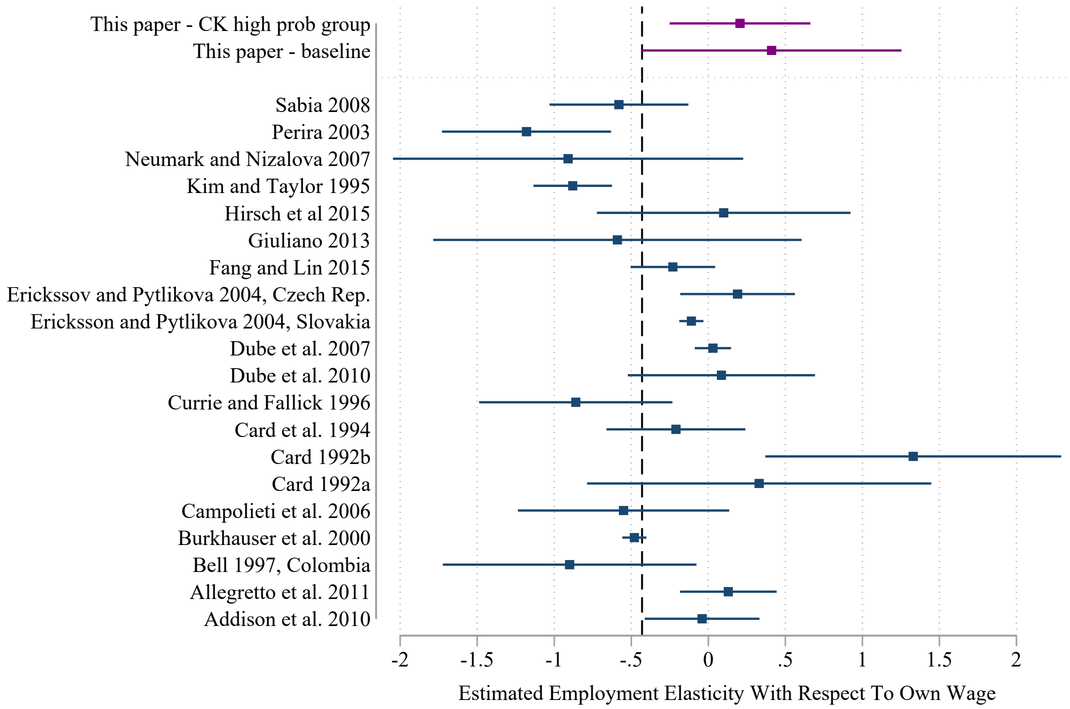
Notes: The figure shows the main results from our event study analysis (see equation 1) exploiting 138 state-level minimum wage changes between 1979-2016. Panel (a) shows the effect on the average wage over time, which is calculated using equation 2. Panel (b) shows the evolution of employment between \$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.6: Change in Employment by Wage Bins after Aggregating Multiple Treatment Events



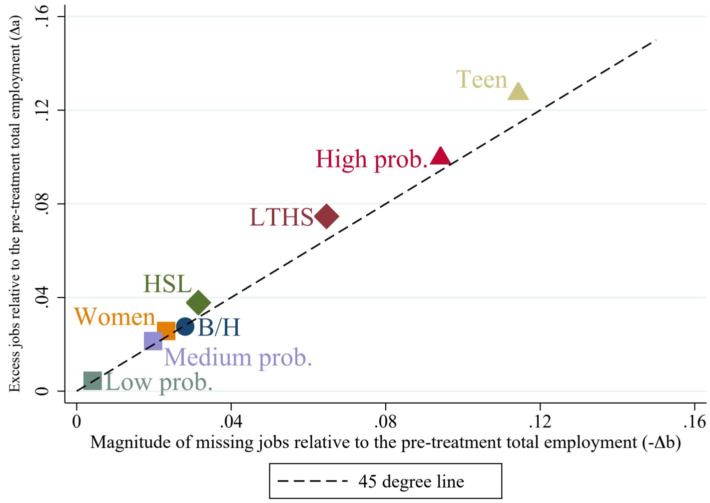
Notes: This figure replicates Figure 2 in the main text, but calculates a cumulative effect when there are multiple events in the 5-year post-treatment window. Overall, 65% of the time, a primary minimum wage increase is followed by a secondary one within 5 years, on average at $0.56 above the minimum 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.7: Employment Elasticity with Respect to Own Wage in the Literature and in this Paper

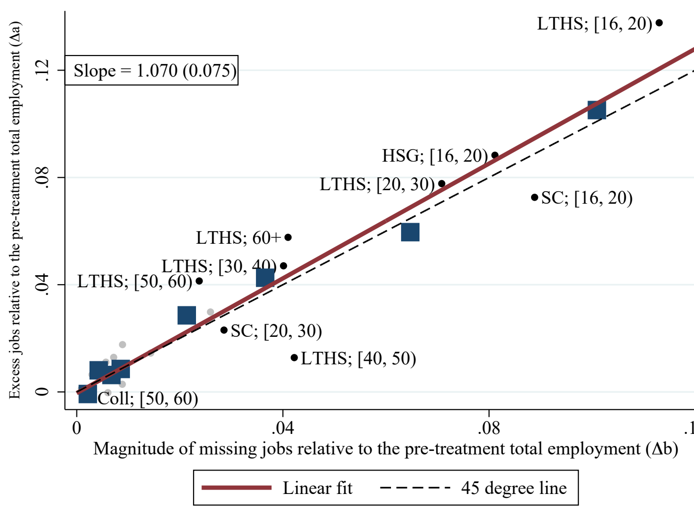


Notes: This figure summarizes the estimated employment elasticity with respect to wage and compares it to the previous estimates in the literature. The estimates in the literature are collected by Harasztosi and Lindner (2016). The two estimates from our paper is the benchmark estimate on overall employment (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.8: Impact of the Minimum Wage by Demographic Groups



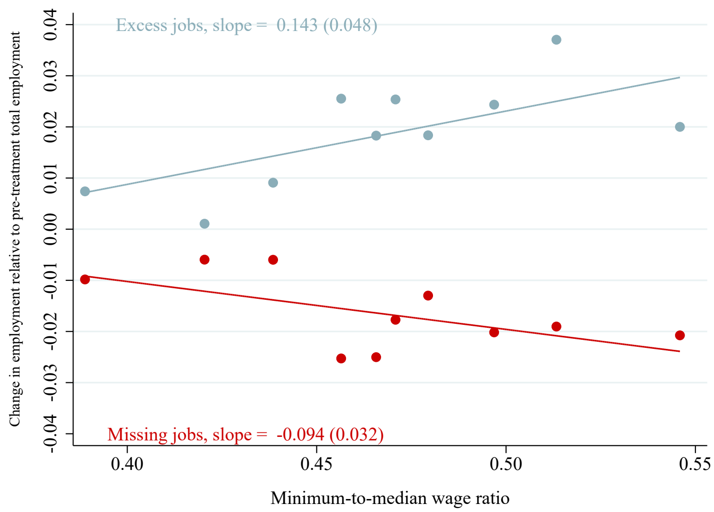
(a) Effect of the minimum wage by demographic groups



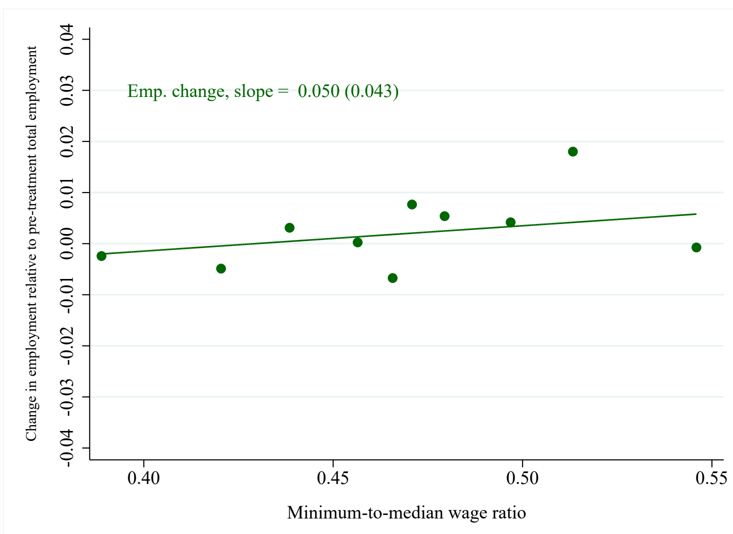
(b) Effect of the minimum wage by age-education groups

Notes: Both figures show the excess jobs (relative to the pre-treatment total employment in that group) above the new minimum wage (Δa) 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 (Δa) 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.9: 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)



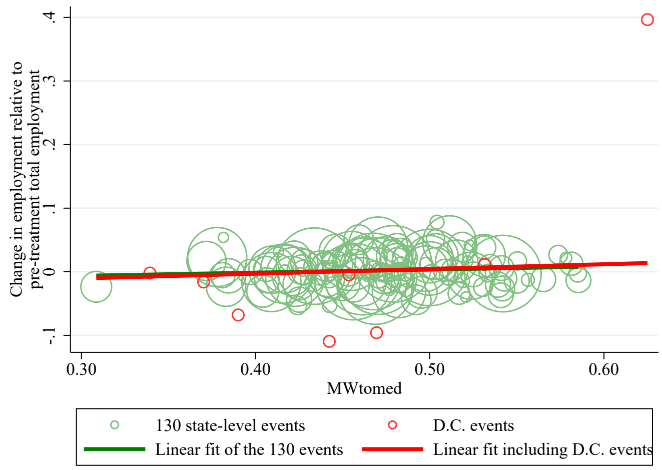
(a) Missing and excess jobs



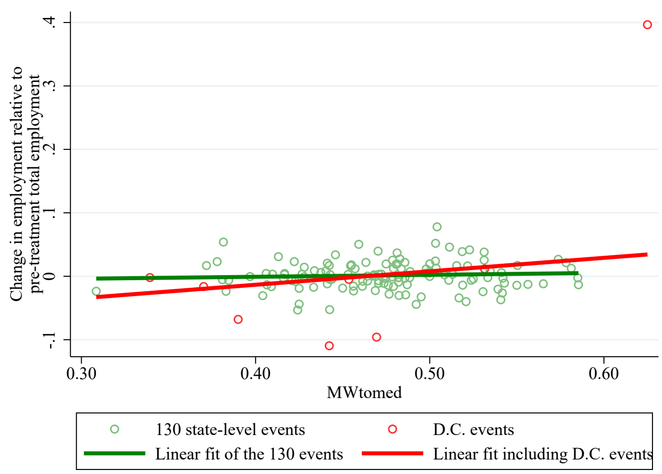
(b) Employment change

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



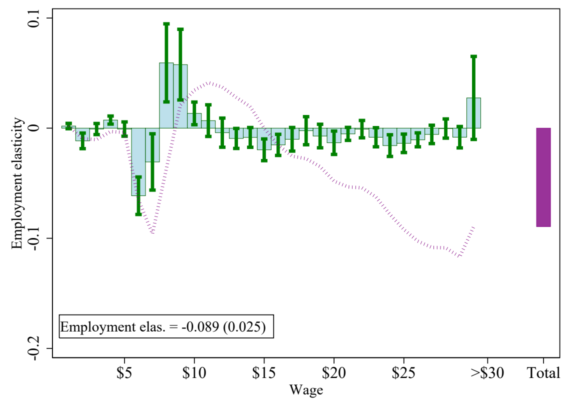
(a) Population weighted



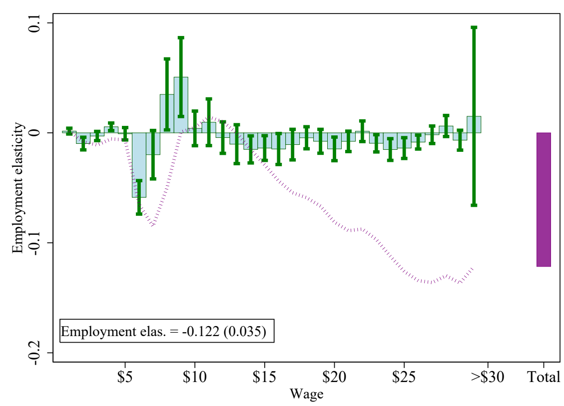
(a) Unweighted

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.11: 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 - local and Aggregate Approaches

	EB-bunching (1)	EB-aggregate (2)
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 local 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 local 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 - Local and Aggregate Approaches

	EB-bunching (1)	EB-aggregate (2)	Ratio of bunching to aggregated standard errors (3)
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 local and aggregate approaches, for various demographic groups. Column 3 reports the ratio of the local to aggregate approach standard errors. The local 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)
Δ 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)	(6)
Missing jobs below new MW (Δ b)	-0.018***	-0.021***	-0.018***	-0.018***	-0.018***	-0.021***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.004)
Excess jobs above new MW (Δ a)	0.021***	0.021***	0.019***	0.021***	0.019***	0.020***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
%Δ affected wages	0.068***	0.065***	0.064***	0.068***	0.067***	0.066***
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
%Δ affected employment	0.028	0.010	0.022	0.029	0.013	-0.011
	(0.029)	(0.025)	(0.031)	(0.032)	(0.029)	(0.022)
Employment elasticity w.r.t. MW	0.024	0.008	0.018	0.025	0.011	-0.009
	(0.025)	(0.021)	(0.027)	(0.027)	(0.025)	(0.019)
Emp. elasticity w.r.t. affected wage	0.411	0.148	0.335	0.427	0.197	-0.163
	(0.430)	(0.380)	(0.461)	(0.445)	(0.436)	(0.335)
Jobs below new MW ($\bar{b}_{-1}$)	0.086	0.086	0.086	0.086	0.086	0.086
%Δ MW	0.101	0.101	0.101	0.101	0.101	0.101
Number of events	138	138	138	138	138	138
Number of observations	847,314	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	4,694,104
Time window	[-3, 4]	[-3, 2]	[-3, 6]	[-5, 4]	[-1, 4]	[-1, 1]

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 (Δb)		Excess jobs (Δa)		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 local 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.

Appendix B
Upper Tail Employment Effects in the Neoclassical Model

In this Appendix, we quantitatively assess the plausible magnitudes of upper tail employment effects of a minimum wage increase in a competitive neoclassical model of the labor market.

Consider a three-factor economy where output is a function of low-skilled, minimum wage labor (L); higher skilled, non-minimum wage labor (H); and capital (K). The elasticity of substitution between high and low-skilled labor is σ_{HL}.

In this setup, the effect of a change in low-skilled wage, w^L, on higher skilled labor demand is given by the well-known formula Hicks-Marshall rule of derived demand:

$$\frac{\partial \ln L^H}{\partial \ln w^L} = s_L(\sigma_{HL} - \eta)$$

where s_L is the share of minimum-wage labor in total production, and η is the output demand elasticity.

In the United States, averaged over our sample (1979-2016), minimum wage workers' share of the wage bill was around 3%. During this same time, labor's share of output was roughly 2/3, which implies a low-skilled share of production of $s_L \approx 2\%$.

In terms of the the elasticity of substitution between high and low skilled workers, Katz and Murphy (1992) estimates $\sigma_{HL} \approx 1.4$. For output elasticity of demand in low-wage intensive sectors, η is often assumed to be 1 (Aaronson and French, 2007).

Overall, these parameter estimates imply a cross-wage elasticity of $\frac{\partial \ln L^H}{\partial \ln w^L} \approx 0.008$. To get a minimum wage elasticity, we note that we find that for a 10% increase in the minimum wage, hourly wages of affected workers increase by 7%, or $\frac{\partial \ln w^L}{\partial \ln MW} \approx 0.7$. Putting all of these estimates together implies a very small minimum wage elasticity for higher-skilled employment of $\frac{\partial \ln L^H}{\partial \ln MW} = \frac{\partial \ln L^H}{\partial \ln w^L} \times \frac{\partial \ln w^L}{\partial \ln MW} \approx 0.008 \times 0.7 = 0.0056$.

How sensitive are these estimates to reasonable variations in the key parameters? Here we vary σ_{HL} between 0.5 and 2, and η between 0.5 and 2. The table below shows that the relevant minimum wage elasticity for upper tail employment, $\frac{\partial \ln L^H}{\partial \ln MW}$, falls between -0.024 and 0.024. When the output elasticity exceeds the elasticity of substitution in magnitude, the upper tail effect is negative, as the scale effect dominates. When the elasticity of substitution is larger in magnitude, the effect on upper tail is positive as the substitution effect dominates. Either way, however, given the small output share of minimum wage workers, a plausible estimate of minimum wage impact on upper-tail employment should be quite small in the neoclassical model. Indeed, these bounds for the upper tail are smaller in magnitude than the standard error for the elasticity of minimum wages for aggregate employment and employment above $15/hour, as shown in Table G.1.

We also empirically show the absence of an effect on the upper tail of the distribution using our event-based design in section 3.

Table B.1: Sensitivity Analysis of the Neoclassical Model for the Upper Tail Employment

	$\eta = 0.5$	$\eta = 1$	$\eta = 1.5$	$\eta = 2$
$\sigma_{HL} = 0.5$	0	-0.008	-0.016	-0.024
$\sigma_{HL} = 1$	0.008	0	-0.008	-0.016
$\sigma_{HL} = 1.5$	0.016	0.008	0	-0.008
$\sigma_{HL} = 2$	0.024	0.016	0.008	0

Notes. The table shows predicted minimum wage elasticities for upper tail employment for alternative output elasticity of demand (η), and elasticity of substitution between high and low skilled workers (σ_{HL}) values.

Appendix C
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.[40] Using hourly wage data, we can easily calculate the actual post-reform wage distribution (blue line in Figure 1). However, the key challenge 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).[41] 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:

[40] 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 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.

[41] 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 C.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 C.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 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:

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

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 C.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.

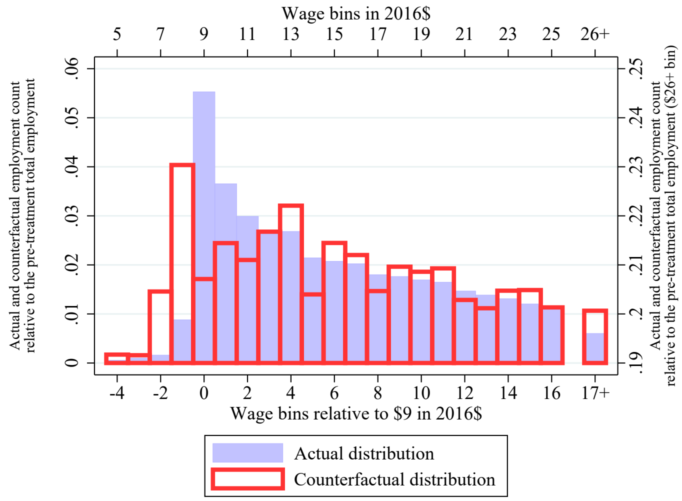
As we explained in the previous section, the effect of the minimum wage on low-wage jobs is equal to the sum of the missing jobs below and the excess jobs above the new minimum wage of \$9. We find that the net employment change is positive—the increase amounted to 0.8% of the pre-treatment aggregate 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.

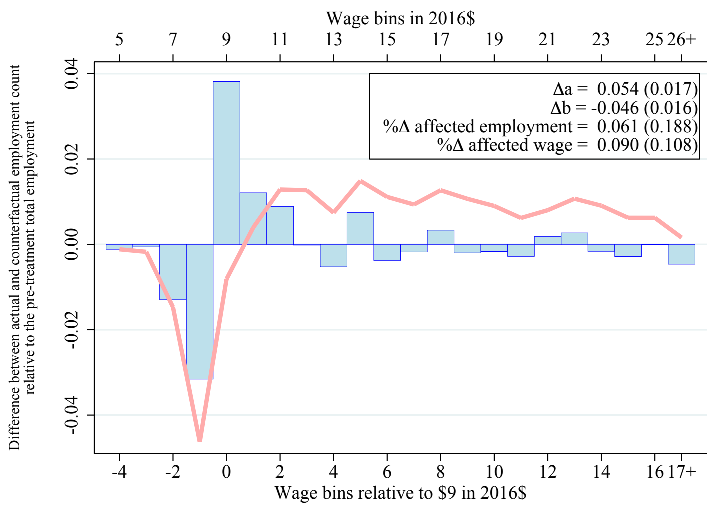
Finally, we also explore the evolution of missing jobs (red line) and excess jobs (blue line) over time in panel (a) in Figure C.3. The figure shows that excess and missing jobs are close to zero before 1999, and there are no systematic pre-existing trends.[42] Once the minimum wage is raised in two 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 C.3).

[42] 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 C.1: Employment by Wage Bins in Washington between 2000-2004



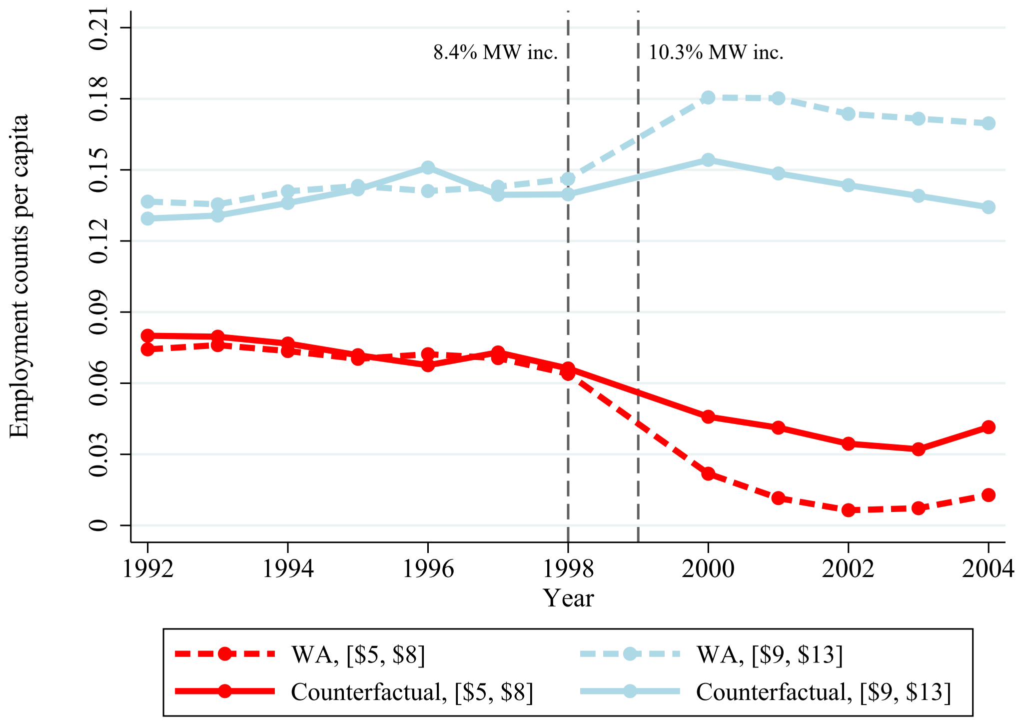
(a) The actual and counterfactual frequency distribution of wages



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

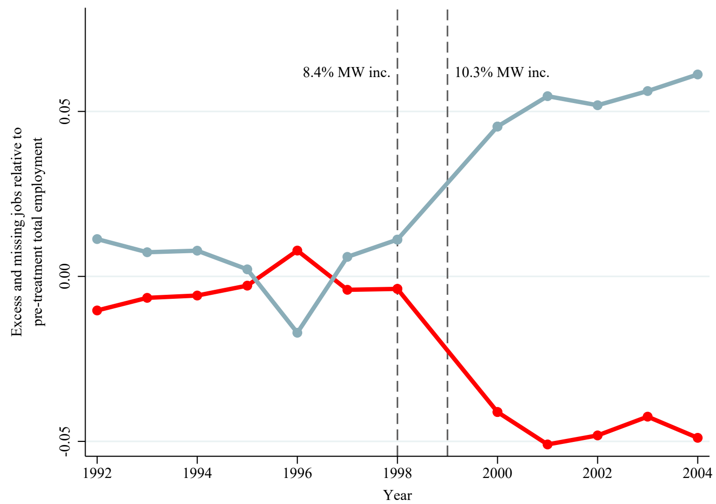
Notes: We examine the effect of the 1999-2000 minimum wage change in Washington state on the frequency distribution of wages (aggregated in $1 bins), normalized by the 1998 level of employment in Washington. The minimum wage was raised from $7.51 to $9.18 (in 2016 values) and it was indexed by inflation 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 C.2: Comparison of Per-capita Employment Counts of Washington and the Counterfactual

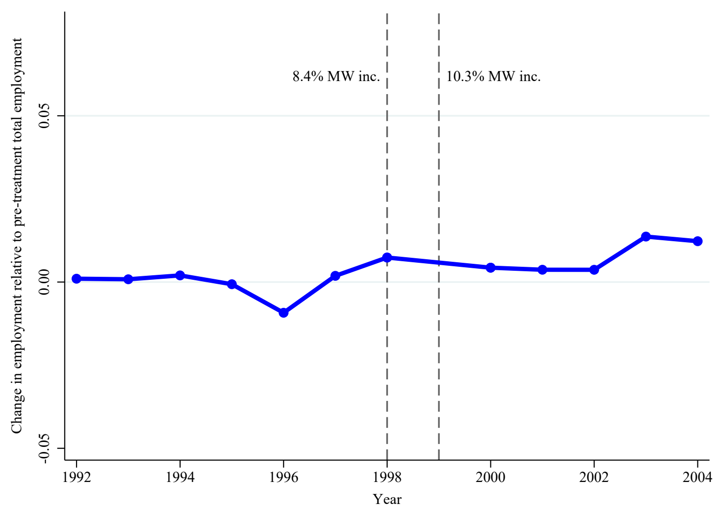


Notes: The figure shows the evolution of the number of jobs per capita with hourly wages between $5 and $8, and $9 and $13 in Washington and in the counterfactual, with data aggregated in $1 bins. The counterfactual jobs are calculated using states without any minimum wage change during the 1998-2004 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 C.3: Impact of Minimum Wages on Missing and Excess Jobs, and Employment Change Over time in the Washington Case Study



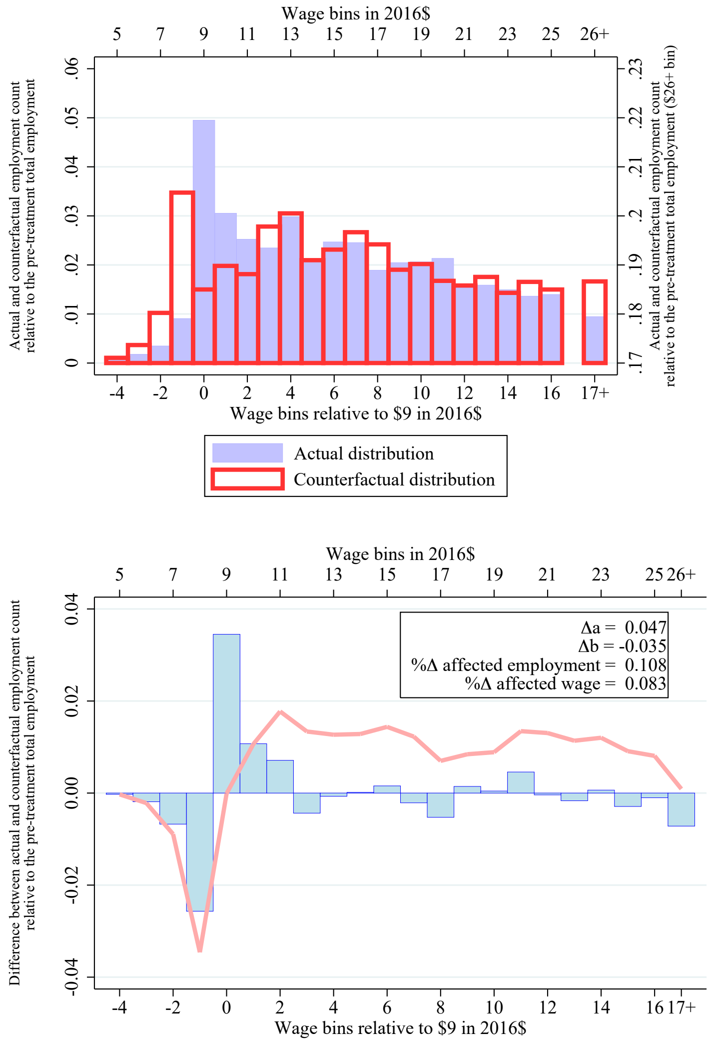
(a) Missing and excess jobs over time



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



Notes: The figure replicates Figure C.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 C.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 counterfactual 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.

Appendix D
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

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

where Ω_{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 (Δa_j), change in missing jobs below (Δb_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 D.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

D.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.

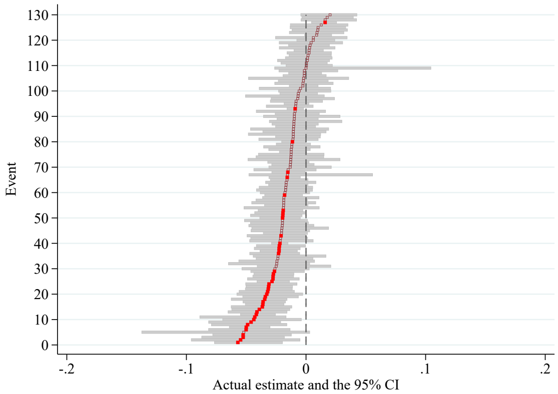
We also stack all of the event-specific data to calculate an average effect across all the events using the a single set of treatment effects $\alpha_{\tau k}$

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

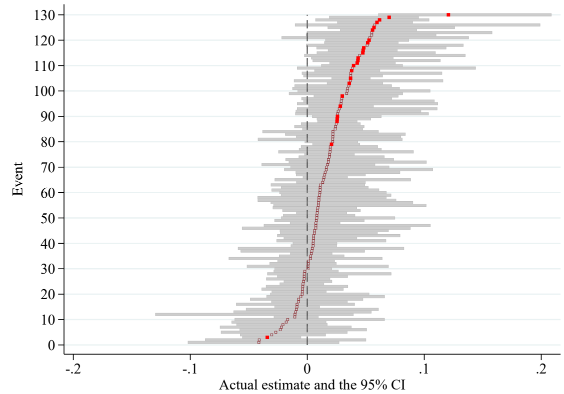
This provides an alternative to our baseline panel specification that uses a more stringent criteria for admissible control groups, and is not more robust to possible problems with a staggered treatment design in presence of heterogeneous treatment effects. In particular, by aligning events by event-time (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 D.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 D.3; again these are quite similar to the baseline Figures 3 and A.5 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.

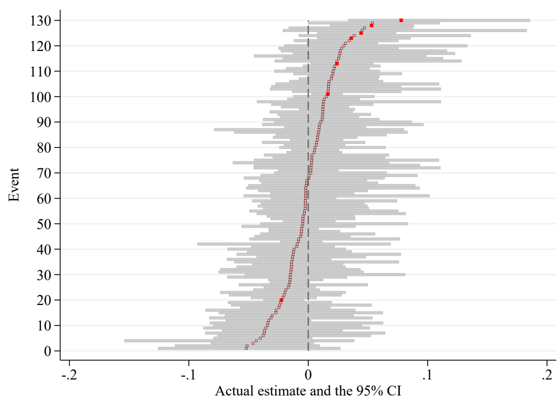
Figure D.1: Event-specific Excess Jobs Above, Missing Jobs Below, and Employment Change Estimates



(a) Missing jobs below (Δb)



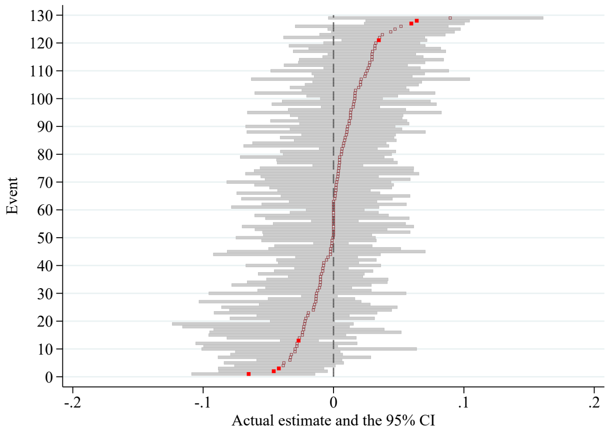
(b) Excess jobs above (Δa)



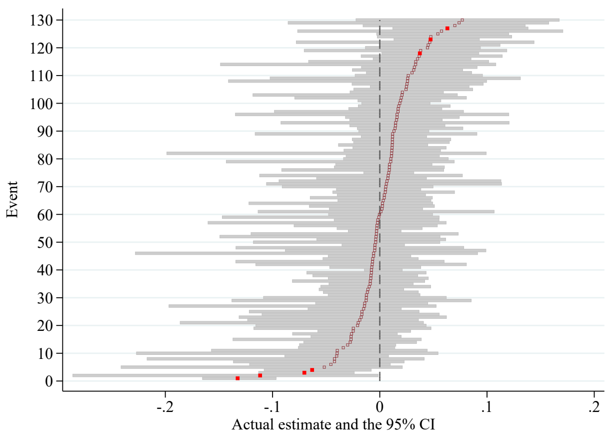
(c) Employment change ($\Delta a + \Delta b$)

Notes: The figure shows the event-specific point (square markers) and confidence interval (gray horizontal bars) estimates for missing jobs below (Δb), excess jobs above (Δa), and employment change ($\Delta a + \Delta b$). The point estimates are calculated using equation D.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 D.2: Leading estimates and upper tail falsification tests for event-specific estimates



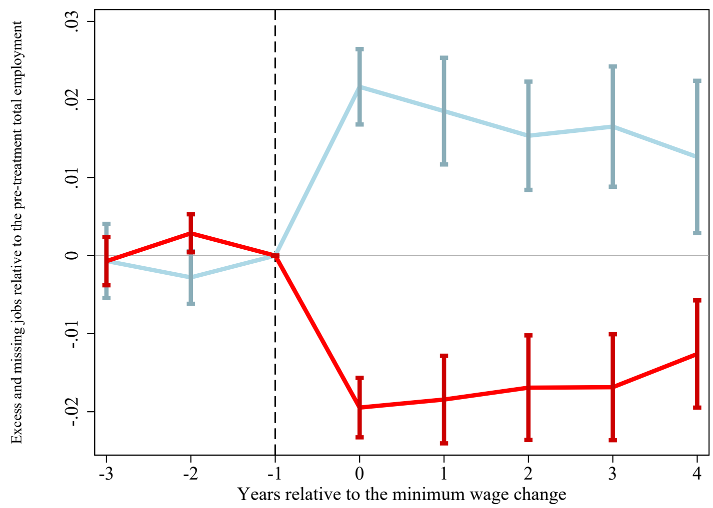
(a) Leading employment change $(\Delta a_{-3} + \Delta b_{-3})$



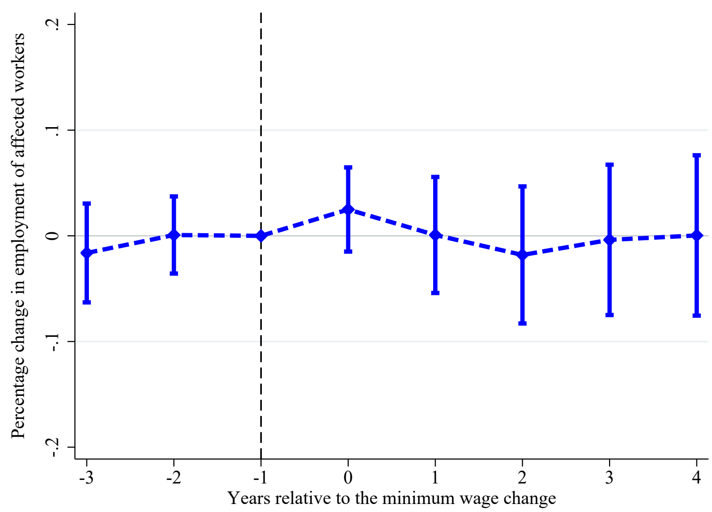
(b) Upper tail $(\frac{\sum_{\tau=0}^{\tau=4}\sum_{k\geq 5}\alpha_{\tau k}-\alpha_{-1k}}{\overline{EPOP}_{-1}})$

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 D.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 D.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 (Δb); and the red lines show the number of jobs between the new minimum wage and \$5 above it (Δa). 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 D.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.

Appendix E
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 C.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 Appendix F.

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.

Appendix F
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 F.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 F.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 F.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 F.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.

F.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."[43]

[43] 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 our approach 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 F.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 F.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 F.5 shows, there is no systematic change in the gap between CPS-QCEW employment following treatment.

F.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 F.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 F.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 F.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 F.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.

F.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.[44] A portion (γ) 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 ρ and γ 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 (σ_w) and the observed latent distribution (σ). 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 ρ and γ as follows:

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

We estimate the model parameters γ and ρ 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

[44] 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 F.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).[45] 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-γ) 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 estimates using CPS data.

F.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`.[46]

Figure F.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, Manning and Smith (2016) approach uses the share below the minimum wage to estimate

[45] 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.

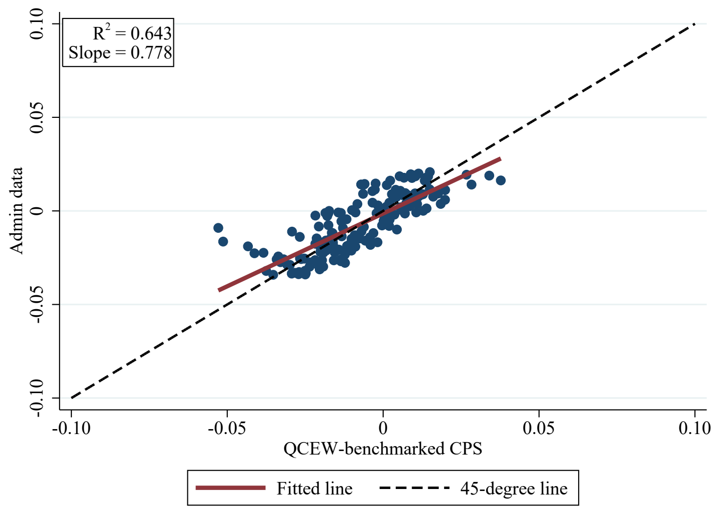
[46] We separately estimate the distribution of true wages for each state-by-quarter using the same distribution function for the measurement error. Estimating annual distribution functions for the error following Autor et al. (2016) produces virtually the same results.

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.

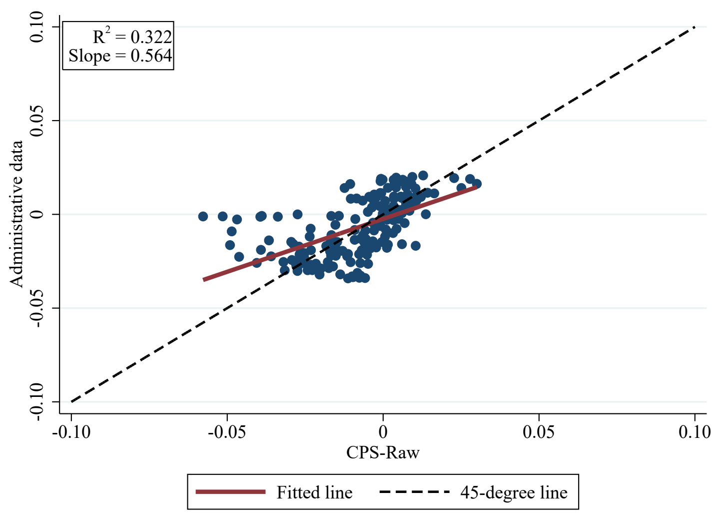
In Table F.3, we compare the baseline estimates with those obtained using the deconvolved data. Column (1) reproduces the baseline estimates reported in Table 1 column (1). Column (2) reports the results using deconvolved data[47] The missing and excess jobs estimates are quite similar across columns 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. While more precise wage data may uncover more accurate information on the exact size of the wage or spillover effects, the combination of the deconvolution-based estimates and comparisons of the CPS and administrative data suggests any bias due to measurement error is likely to be small.

[47] 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 F.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 F.2: Frequency Distributions in the Administrative and CPS data

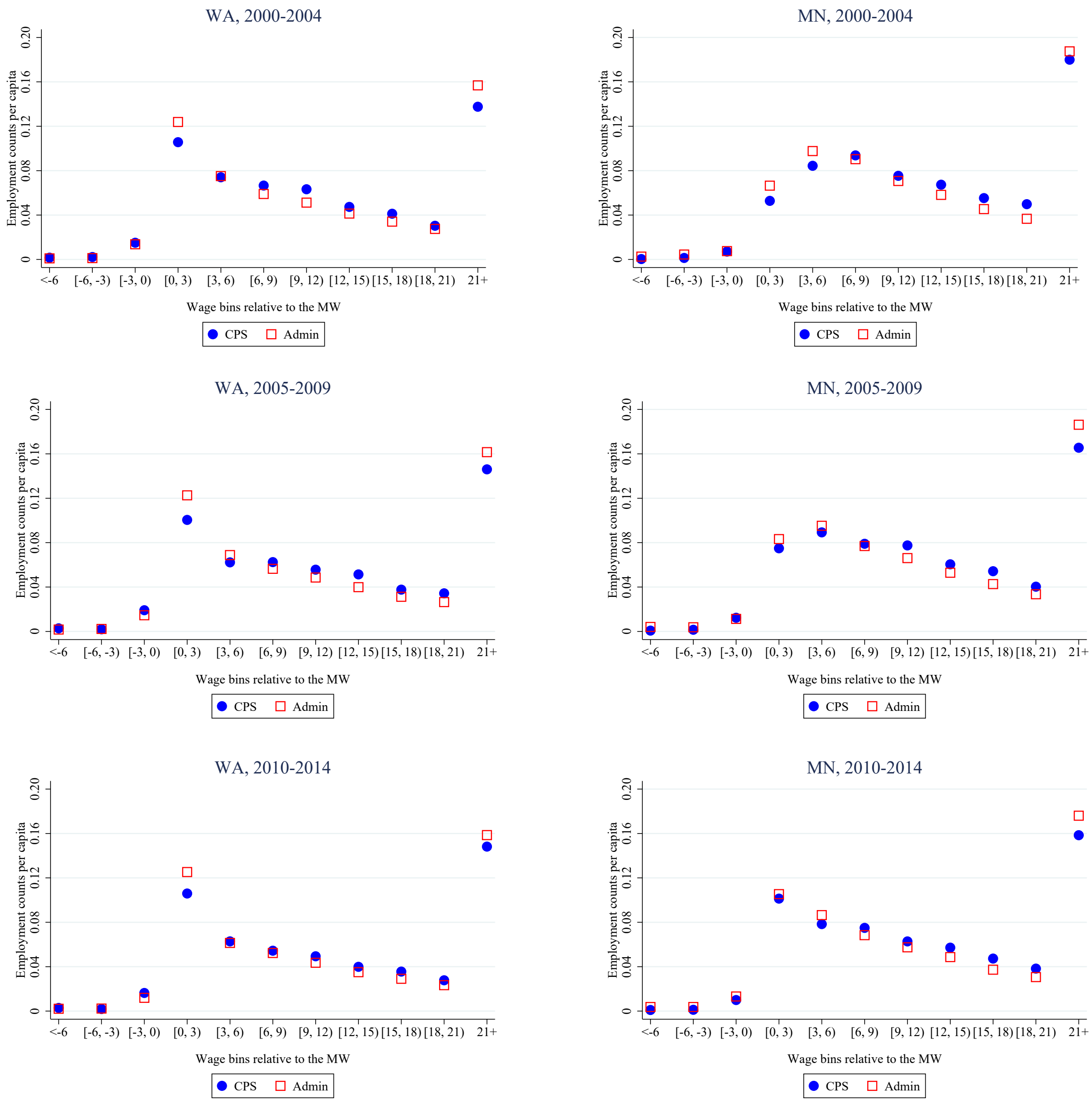
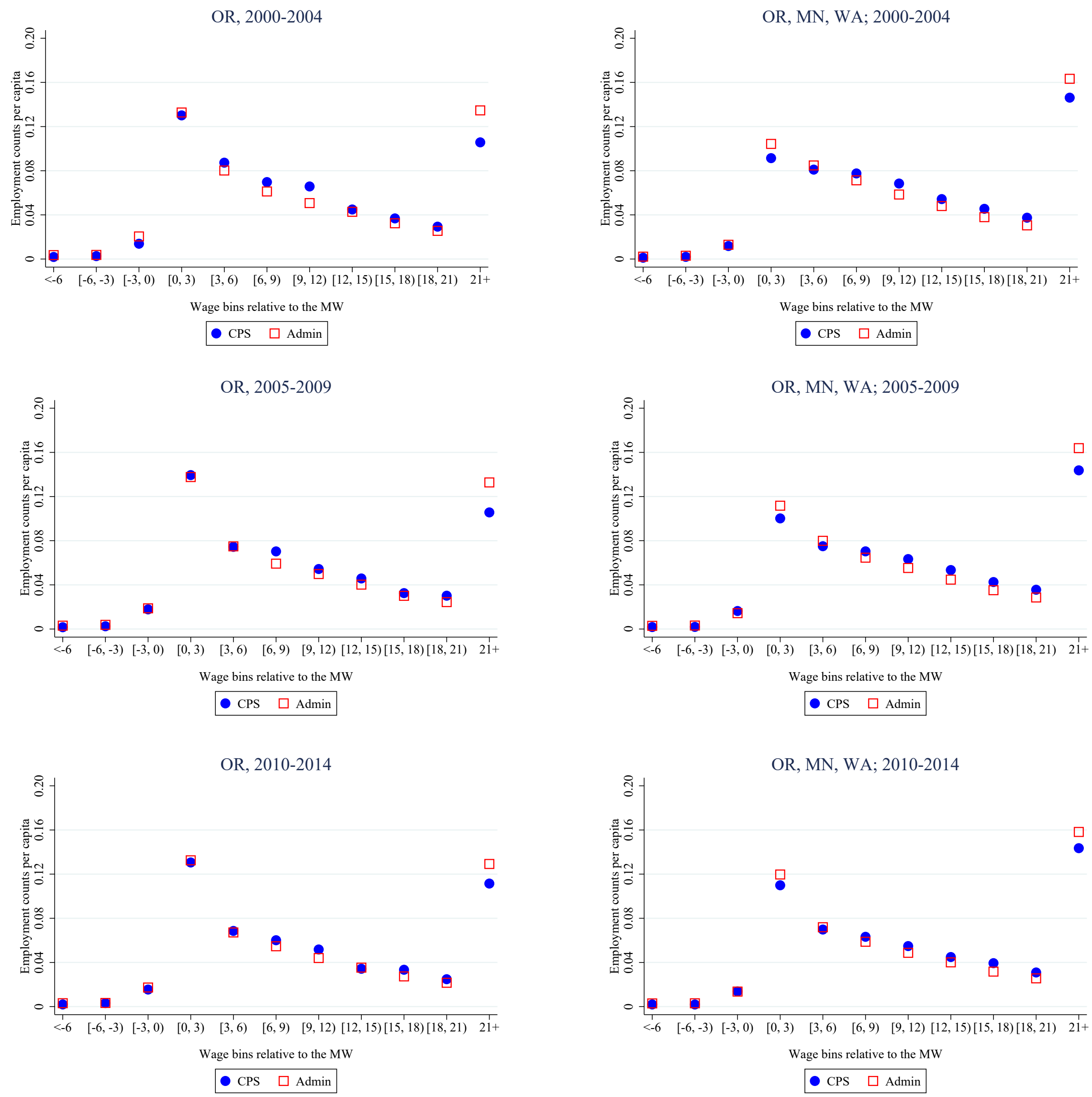
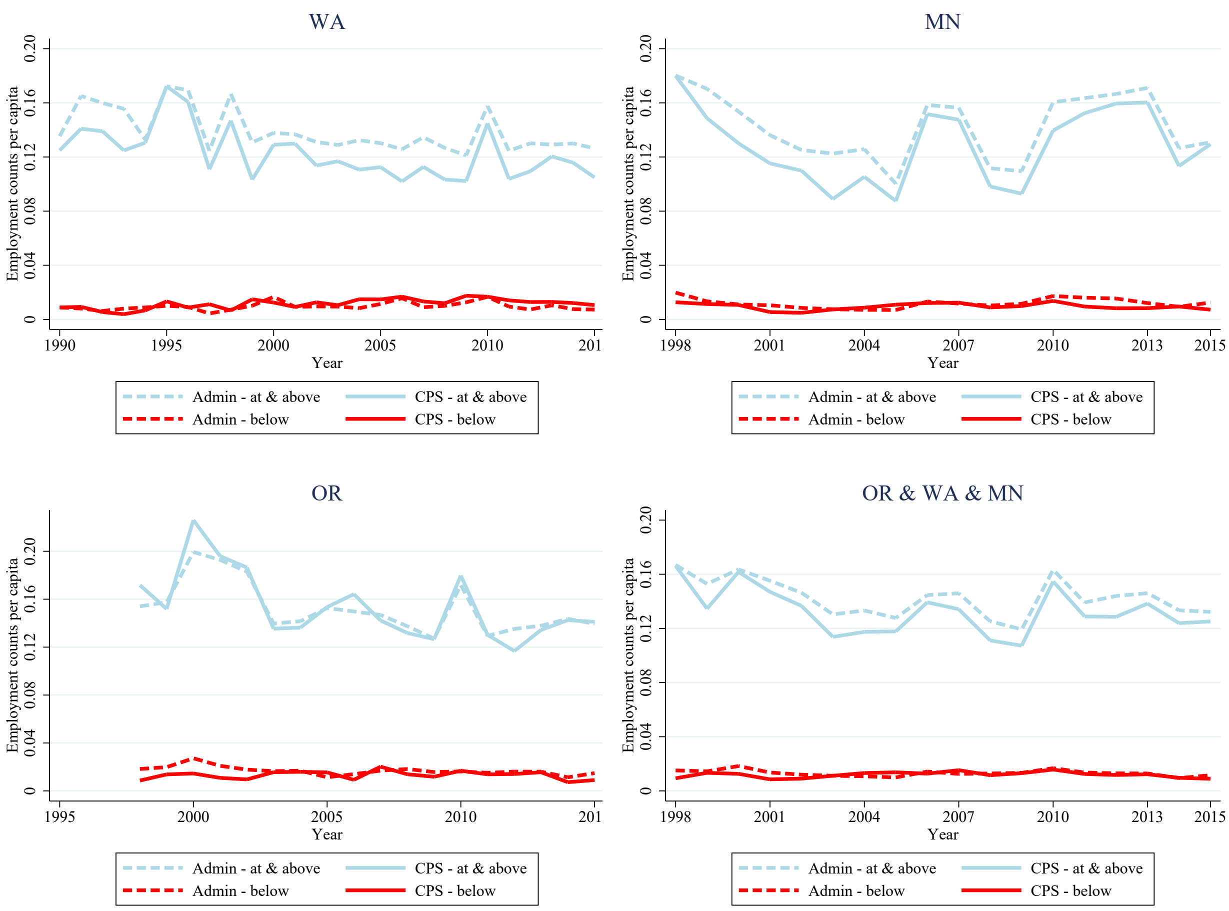


Figure cont'd: Frequency Distributions in the Administrative and CPS data



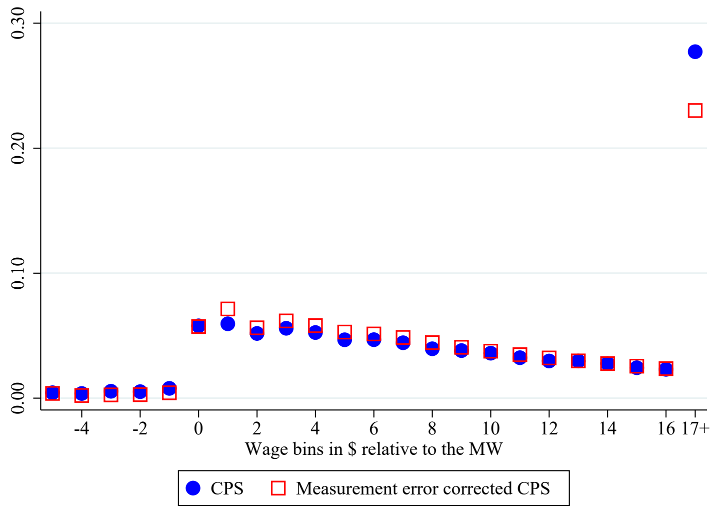
Notes: This figure plots 5-year averaged per-capita administrative and QCEW-benchmarked CPS employment counts of Washington, Minnesota, Oregon, and the three states combined from 2000 to 2014 in $3 bins relative to the minimum wage. The red squares indicate the administrative data, and the blue circles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

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



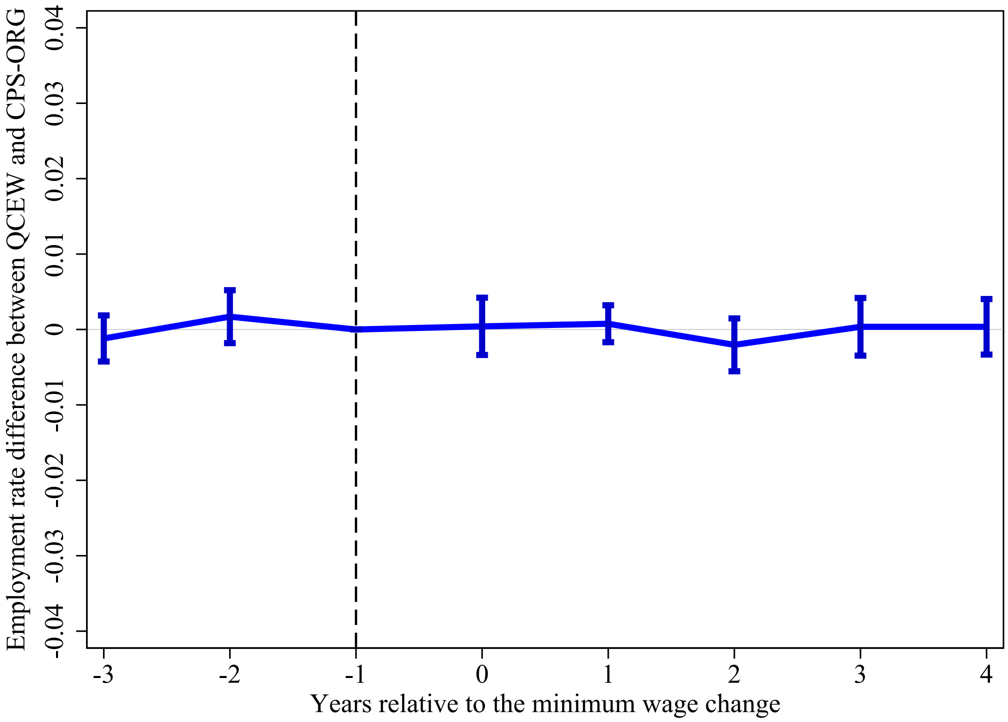
Notes: This figure plots the time paths of the number of jobs below the minimum wage $[MW - \$5, MW)$, and jobs at and above the minimum wage ($[MW, MW + \$5)$ relative to the state-level population from both the administrative data and the CPS in three states (MN, OR, WA) separately, and all together.

Figure F.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 F.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 F.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 F.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 F.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 $1\text{-}\gamma$	Conditional error variance $\frac{1-\rho^2}{\rho^2}$	Ratio of std. deviations of true to observed latent distribution $\frac{\sigma_w}{\sigma}$
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-γ, 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 F.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 47. To implement the procedure, we rely on the estimates in Table F.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.

Appendix G
Reconciling the Results from the Two-Way Fixed Effects Panel Regression with Log Minimum Wage and Event Based Regressions

This Appendix provides an explanation for the difference between the classic two-way fixed effects panel regression on log minimum wage (shown in Figure 6) and the event study used in our benchmark specification (shown in Figure 2). We first establish that the difference between the two estimates is not driven by using discrete versus continuous treatment meaasures, or any artifact of binning the wages. Rather, they differ in how they use variation in the outcome outside of the event windows. Then, we evaluate the credibility of the two specification by examining whether the parallel trend assumption holds before the reform; whether the results are sensitive to include different trends and control variables; whether the results are sensitive to the period chosen; and whether the employment changes are concentrated at the bottom of the wage distribution. This analysis highlights that the credibility of the two-way fixed effects estimator with log minimum wage (TWFE-logMW) can be questioned in our context: we show that the TWFE-logMW results are driven by pre-existing trends; are sensitive to inclusion of additional controls and to using different sample periods; and are driven by employment changes at the upper part of the wage distribution. At the same time, the event based specification (EB) provides plausible estimates in all cases. As a result we conclude that the EB approach is preferred to the TWFE-logMW in our context. We then provide an explanation for why the TWFE-logMW and EB estimates differ. The TWFE-logMW specification is sensitive to shocks to upper tail income in the 1980s and early 1990s in Democratic-leaning states contaminate the TWFE-logMW estimates using the full 1979-2016 period; even though most minimum wage variation comes from after 1992, these shocks affect the estimation of the fixed effects. In contrast, the TWFE-logMW specification in the 1993-2016 sample is not affected by these shocks. The EB specification is not affected by these shocks either because it uses variation locally within the event window around the minimum wage events. We finish this Appendix by providing additional insights into how the shocks from the 1980s and early 1990s, along with the use of the TWFE-logMW specificaiton, helps explain some of the controversies in the minimum wage literature—including estimates for teen employment.

G.1 Bridging the TWFE-logMW and the benchmark specification

We begin our analysis by assessing the contribution of various factors that drive the differences in estimates between our benchmark event-based bunching (EB-bunching) and the TWFE-logMW specifications. The benchmark specification in the paper estimates bin-by-bin employment changes relative to the minimum wage. The EB-bunching specification is calculated using the following regression (see the details in Section 2.2):

$$\frac{E_{sjt}}{N_{st}} = \sum_{\tau=-3}^{4} \sum_{k=-4}^{17} \alpha_{\tau k} I_{sjt}^{\tau k} + \mu_{sj} + \rho_{jt} + \Omega_{sjt} + u_{sjt} \tag{G.1}$$

where E_{sjt} is the employment in \$0.25 wage bin j in state s and at quarter t, while N_{st} is the size of the population in state s and quarter t. The treatment dummy $I_{sjt}^{\tau k}$ equals to 1 if the minimum wage was raised τ years from date t and for the \$0.25 wage bins j that fall between k and $k+1$ dollars of the new minimum wage In this regression we control for state-by-wage bin and period-by-wage bin effects, μ_{sj} and ρ_{jt}, and for for small or federal increases Ω_{sjt}. We use the estimates based on this specification to produce our key results (e.g. Figure 2).

In Section 4 we discuss the employment changes along the wage distribution using the TWFE-logMW specification (see Figure 6). For brevity, in this appendix we focus on the estimates for overall employment, E_{st}. We estimate the following specification:

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

where E_{st} is the employment in state s at time t; μ_s is state fixed effects and ρ_t are time effects.

There are two key differences between the EB-bunching specification shown in equation G.1 and the TWFE-logMW shown in equation G.2. First, the benchmark EB-bunching specification identifies the employment responses based on bin-by-bin employment changes around the minimum wage. To examine whether this makes a difference we estimate an event study where the outcome variable is the state level aggregate employment change (similarly to the TWFE-logMW). In particular, we estimate the following regression:

$$\frac{E_{st}}{N_{st}} = \sum_{\tau=-3}^{4} \alpha_\tau I_{st}^{\tau} + \mu_s + \rho_j + \Omega_{st} + u_{st} \tag{G.3}$$

Note that this is estimated using state-year data, the treatment variable, I_{st}^{τ}, is now defined at the state level and not at the wage-bin level, and we include state fixed effects, μ_s, and time effects, ρ_t, instead of bin-specific fixed effects. We refer to this specification as EB-state-discrete; in the main text, we also refer to this specification as EB-aggregate when the oucome is aggregate emplyoment.

The estimated employment elasticities with respect to the minimum wage are shown in Panel A of Table G.1.[48] In Column (1) we report the benchmark EB-bunching estimates (shown in equation G.1). Panel A in Column (2) show the event study estimates on state-level employment (the EB-state-discrete specification shown in equation G.3). The employment effects are virtually the same: 0.024 in the benchmark (shown in Column 1) and 0.016 in the state-level EB (shown in Column 2), which highlight that relying on bin-by-bin estimates and controlling for state-by-wage bin, μ_{sj}, and period-by-wage bin effects ρ_{jt} is not what drives the discrepancy between our benchmark EB-bunching estimate and the TWFE-logMW estimate.

The second key difference between the benchmark EB-bunching estimate and TWFE-logMW is that the former defines each treatment using a dummy variable, while the latter uses a continuous treatment definition. To bridge the two specifications, we report employment estimates based on two intermediate steps. First we examine whether using a continuous treatment measure, but keeping to a event-based (EB) specification makes a difference. In

[48]In this Section we focus on the the average employment changes 5 years after the minimum wage hike.

particular, we run the following regression:

$$\frac{E_{sjt}}{N_{st}} = \sum_{\tau=-3}^{4}\sum_{k=-4}^{17} \alpha_{\tau k} I_{sjt}^{\tau k} \triangle \log MW_{s,t-\tau} + \mu_{sj} + \rho_{jt} + \Omega_{sjt} + u_{sjt} \tag{G.4}$$

where we define the treatment as $I_{sjt}^{\tau k} \triangle \log MW_{s,t-\tau}$ instead of $I_{sjt}^{\tau k}$. In other words, instead of a dummy for treatment, now the treatment switches from 0 to $\triangle \log MW_{s,t-\tau}$ at event date $\tau = 0$. Column (3) in Table G.1 shows that the estimate using continuous treatment definition (0.024) is virtually the same as our benchmark estimate (0.024). We refer to this as the EB-bunching-continuous specification.

Second, we also explore whether a similar modification of the event based estimate on aggregate employment makes a difference, by estimating the following EB-state-continous specification:

$$\frac{E_{st}}{N_{st}} = \sum_{\tau=-3}^{4} \beta_{\tau} I_{st}^{\tau} \triangle \log MW_{s,t-\tau} + \mu_s + \rho_t + u_{st} \tag{G.5}$$

Column (4) in Panel A in Table G.1 show that redefining the treatment in that regression makes only a minor difference: the estimate of 0.024 in the benchmark EB-bunching specification (equation G.1) changes to 0.008 in the EB specification with aggregate employment and continuous treatment (equation G.5).

In Column (6) in Table G.1 we report the TWFE-logMW estimates. This produces a large disemployment estimate (-0.089) in line with the analysis in Section 4 in the main paper. As is clear from Table G.1, the discrepancy between our estimates is not driven by use of data by wage bins, or by the continuous-versus-discrete treatment definition. In Panel B of Table G.1 we also report estimates on state-level employment below \$15 and state-level employment above \$15. The results highlight that the below \$15 employment change is always close to zero even in the TWFE-logMW specification. At the same time the employment changes above \$15 are small and insignificant in the EB specifications, but large negative in the TWFE-logMW specification. So what drives the difference between the TWFE-logMW specification G.2 on the one hand, and specifications G.1, G.3, G.4, and G.5 on the other? Even though equation G.2 has 4 lags and 2 leads like the other specifications, it uses variation across observations throughout the sample period, including distant observations far away from event dates. This is because the first lead and the last lag are "binned up." In contrast, the other four observations specifically use variation within the event window.

We also estimate the distributed lag model estimated in first differences (FD):

$$\Delta\left(\frac{E_{st}}{N_{st}}\right) = \sum_{\tau=-2}^{4} \alpha_{\tau} \Delta \log MW_{s,t-\tau} + \rho_t + u_{st} \tag{G.6}$$

Unlike the TWFE-logMW estimates, FD estimator does not compare employment levels across observations that are decades apart, and compare within the lead/lag window. As shown in column (5), that the FD specification produces employment estimate of 0.031 (s.e. 0.031), which is similar to the EB specification. These results highlight that the key factor driving the difference in estimates from the empirical designs is the role of employment comparisons with distant observations outside of the event window.

G.2 Credibility of the TWFE-logMW and the event study designs

Since we have two empirical designs that provides very different estimates, it is important to assess the credibility of the two estimates. To simplify the discussion we will compare the TWFE-logMW specification (shown in equation G.2) to the event based estimates on aggregate employment (shown in equation G.3). As we documented above, the focus on the low-wage bins is not driving the difference between the two designs, since the aggregate employment estimates from the event-based design are similarly small as the benchmark event-based bunching estimates.

The crucial assumption made in all difference-in-differences style estimation is that the treated and untreated states would follow a parallel trend in absence of the policy change. While testing this directly is not possible, a standard way to assess the credibility of this assumption is to examine pre-existing trends. Figure G.1 plots the time path of employment elasticities with respect to the minimum wage for the TWFE-logMW (panel a) and for the EB-aggregate (panel b). Note that interpretation of the last lag and the first lead is different in the two empirical design. Since increases in nominal minimum wages, $log(MW)$, are always permanent, the last lag in the distributed lag model (such as TWFE-logMW 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. At the same time the event study estimates only focus on employment changes around the event window and so the last lag and first lead specifically reflect employment changes in that period.

The time path of the estimates shows that the TWFE-logMW estimator produces 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 are not present in our event based estimates (see panel b). Additionally, as shown in Figure G.3, the leading effect obtains only for high wage employment (above $15) in the TWFE-logMW model.

Another standard way to test the credibility of an estimate is to assess its robustness to alternative specifications. In Table G.3 we report estimates with additional controls such as state-specific linear trends (Column 2) or with average major industry and broad occupation shares from 1979-1980 interacted with time periods (Column 3). We also explore the effect of restricting the sample to the post 1992 periods when most minimum wage changes occurred in our sample (Column 4). In all these specifications, we find that both the TWFE-logMW and EB estimate induce close to zero disemployment effect, which highlight that the large negative employment estimates in the TWFE-logMW are not robust to small modification of the empirical design. In other words, the large negative TWFE-logMW estimates arise only from inclusion of the 1979-1992 period, even though most of the minimum wage variation occurs after 1992. In contrast, for the 1993-2016 period, the TWFE-logMW specification passes the credibility tests, including showing no spurious leads (see and no large upper tail effects (see Figure G.5); and precisely in the sample where it passes these credibility tests, it suggests little impact on aggregate employment, including in the long run.

Finally, we examine the source of disemployment effect to assess the credibility of the two empirical designs. In the main text, we already discussed that the employment changes in the EB design occurs where we expect the minimum wage should play a role. At the same time, the large negative estimates in the TWFE-logMW is driven by employment changes at the upper tail of the wage distribution. We extend this analysis by providing direct evidence on the Card and Krueger (CK) probability groups. In Figure G.2 we show the effect of the minimum wage for the low versus the medium/high probability groups. We estimate the effect of TWFE-logMW (panel a and b) and the EB model (panel c and d) for each wage bin. The results highlight that the overall effect in the TWFE-logMW model is not only concentrated in high wage jobs but also for the "wrong" workers: namely, the large employment change occurs for only the workers least demographically likely to be earning near the minimum wage. At the same time, the EB estimates show that minimum wage mainly affect the workers most demographically likely to be earning near the minimum wage change.

These results highlight that the TWFE-logMW specification, when it is implemented using the entire-sample between 1979-2016, produces a spurious negative estimate on employment. At the same time the EB design passes the standard credibility tests.

G.3 What drives the TWFE-logMW estimates

We see that TWFE-logMW when it is applied to the entire sample, produces a large negative employment effect, while when we restrict the sample to the 1993-2016 period, the TWFE-logMW estimates become small and statistically indistinguishable from zero. This is noteworthy because there were few state minimum wage changes prior to 1993.

We also perform an exercise to demonstrate the bias in the TWFE-logMW estimates using the 1979-2016 sample. As the first step, 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 EB 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 test.

To summarize, this exercise 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.

G.4 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 G.4. 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 G.4 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 the TWFE-logMW model covering 1979-2016. However, the divergence between low minimum wage and high minimum wage states ended quickly during the 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 is that 1990-1991 recession just 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 G.6 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 G.4. 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 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 from the full 1979-2016 sample 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.

G.5 Relation to other findings in the minimum wage literature

The argument that the TWFE-logMW specification can sometimes produce spurious findings is not new to this paper. However, we provide some new insights about the nature of the problem by highlighting how shocks from the 1980s and early 1990s—long before most minimum wage variation occurred—tend to drive estimates from this specification. Here we relate this point to some key findings in the minimum wage literature.

Teen employment

The existing literature finds teen employment estimates to be sensitive to specifications. Estimates using a TWFE-logMW specification has produced more negative estimates (Neumark et al., 2014) while inclusion of controls for state-specific trends or other controls for heterogeneity tend to suggest estimates close to zero (Allegretto et al., 2017). Since our estimates for teens (whether focused on low wage jobs or not) do not suggest disemployment effects–with or without any trend controls–here we provide a reconciliation with the existing teen literature. These estimates are based on the same distributed lag structure as before for TWFE-logMW, EB and FD specifications, with 2 annual leads, 4 annual lags, and the contemporaneous treatment measure.

The 1979-2016 estimates are large and negative in the TWFE-logMW specification, with an elasticity of -0.238 (s.e. 0.088). In contrast, the FD elasticity of 0.092 (s.e. 0.122) is positive in sign and not statistically significant. Both of these are consistent with findings reported in Allegretto et al. (2017). In addition, the EB estimate of 0.163 (s.e. 0.115) is also similarly positive and not statistically different from zero. Additionally, the TWFE-logMW specification in the full sample is highly sensitive to the inclusion of state-specific linear trends: inclusion of these trends produces an estimated elasticity of 0.065 (s.e. 0.128). This, too, is consistent with findings in Allegretto et al. (2017) and reflects disagreements about the right way to control for heterogeneity.

However, consistent with our findings on aggregate employment in this paper, when we consider the 1993-2016 period, none of the estimates are statistically different from zero across various specifications. The TWFE-logMW estimate from this sample of -0.024 (s.e. 0.153) is close to zero and not statistically significant. The FD estimate of 0.059 (s.e. 0.137) and EB estimate of 0.162 (s.e. 135) continue to be positively signed and not significant in this subsample. Therefore, if we consider the time period where most of the minimum wage increases have occurred, the estimates across all standard specifications suggest little teen dis-employment from minimum wage increases. This highlights how the same shock during the 1980s and early 1990s discussed above has also driven the sensitivity of the teen estimates. Moreover, the necessity to properly control for violations of the parallel trends assumption seems to arise from inclusion of a period with relatively little minimum wage variation (i.e., 1980s and early 1990s). And this primarily affects specifications like the TWFE-logMW which make distant comparisons. As far as we know, this point has not been recognized in the literature.

Estimates using border county design

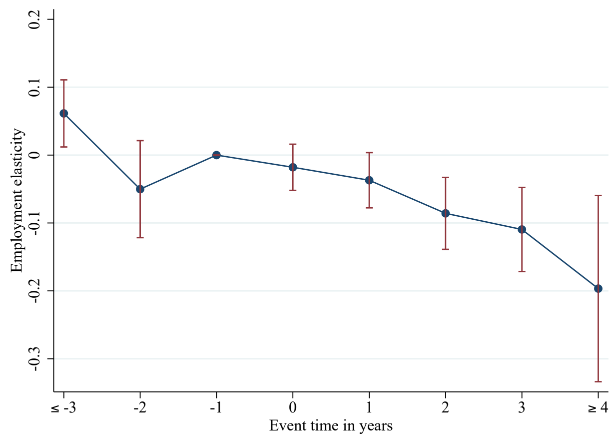
Dube et al. (2010) (hereafter DLR) also argue that the estimates from a TWFE-logMW specification in the 1990-2006 produces spurious negative employment effects for restaurant employment, based on the presence of large negative leading effects similar to what we find here. They propose using a border-discontinuity design that compares outcomes across contiguous border county pairs.

Here we use their county-level data to establish several findings on aggregate employment, in addition to restaurant employment reported in DLR. First, similar to this paper, Figure 1 in DLR demonstrates that aggregate employment growth was systematically lower in high minimum wage states the during the 1990-1991 recession, but quite similar afterwards. This highlights the same likely source of bias in their sample due to the early years that we have documented above. Using county-level QCEW dataset from the DLR replication package, we estimate distributed lag models 16 quarters of lags and 8 quarters of leads. We calculate the estimate for the long run (16th quarter) effect net of the 4-quarter average just prior to treatment, similar to those reported in this paper. We find the TWFE-logMW estimate for aggregate employment is large and negative at -0.139 (s.e. 0.091). However, when we estimate the model in first differences, the FD estimate is close to zero at 0.023 (s.e. 0.094), consistent with what we have found in this paper using state-level data. Similarly, when we use an event-based approach like in this paper, we also find a small EB estimate of -0.024 (s.e. 0.052). Second, we find that when we estimate the model using contiguous border county pairs, the border county pair (BCP) specification similarly produces estimates that are close to zero 0.001(s.e. 0.073). If comparing either highly similar areas (i.e, border counties) or looking locally around the time of the policy change avoids biases, combining both approach would have a “double robust” property. We find that the BCP-EB specification (column 5) produces an aggregate employment elasticity of 0.030 (s.e. 0.054), which is quite close to the baseline estimate in this paper, though less precise.

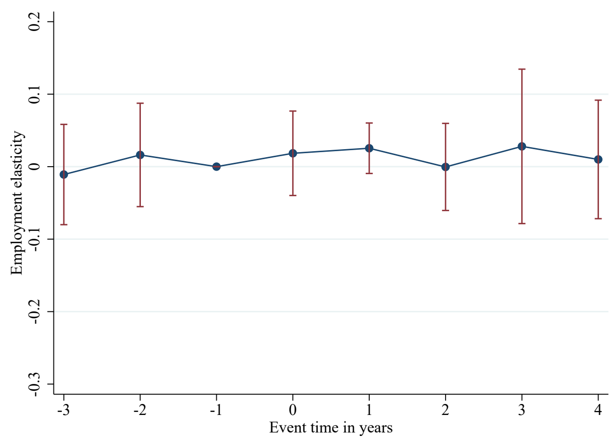
The estimates for restaurant employment follow a similar pattern. While the TWFE-logMW estimate is large and negative, -0.289 (s.e. 0.113), the EB, FD, BCP, and BCP-EB estimates are 0.002 (s.e. 0.050), -0.055 (s.e. 0.094), -0.016 (s.e. 0.082) and 0.043 (s.e., 0.055) respectively. An event-based approach looking within a window around the minimum wage increases and that allows for up to a 16 quarters post-treatment period finds no evidence of losses in resaturant jobs, even when using a panel of all-counties used in DLR.

Overall, these results confirm that using specifications like EB or FD that avoid making distant comparisons or specifications that compare across highly similar areas (like just across the state border as in BCP) appear to avoid a bias from the shocks far outside the event window. And the shocks in the 1980s and early 1990s recession seem to drive the key violation of parallel trends in this literature. This is true for aggregate employment as well as for highly affected groups like restaurants or teens.

Figure G.1: Estimated Impacts of Minimum Wages on Aggregate Employment Over Time Using Alternative Specifications



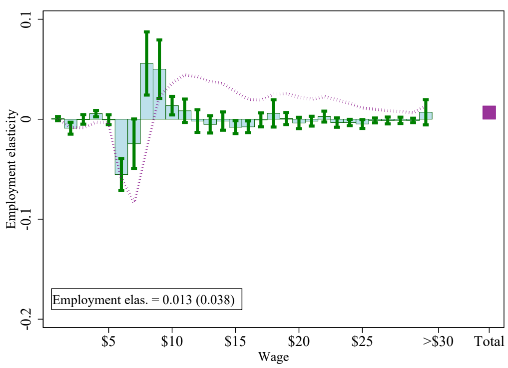
(a) TWFE-logMW



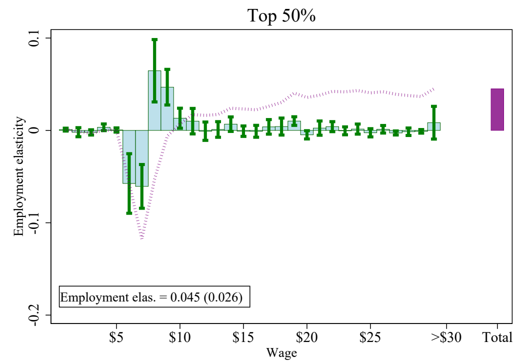
(b) Event Based Estimate

Notes: The figure shows the effect of the minimum wage on aggregate employment over time. Panel (a) uses the TWFE-logMW (equation G.2) regression of state-level aggregate employment rate on the state-level contemporaneous log minimum wage, as well as on 4 annual lags and 2 annual leads. Panel (b) uses the EB specification (equation G.3), and regresses 4 annual lags and 3 annual leads in the event dummies. 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 (panel a) or event dummies (panel b), and then dividing them by the sample average employment-to-population rate. Furthermore, the cumulative elasticity at event date -1 is normalized to 0, which is why the panel (a) shows a 3rd year or earlier (≤ -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.

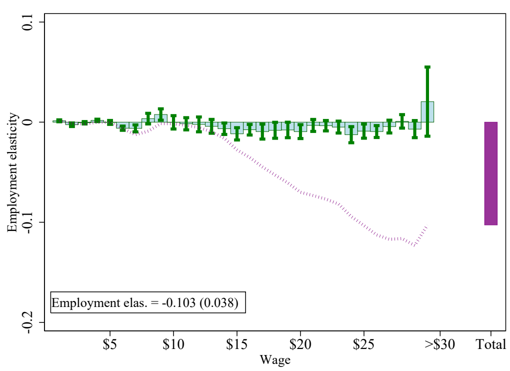
Figure G.2: Impact of Minimum Wages on the Wage Distribution by Predicted Probability Groups



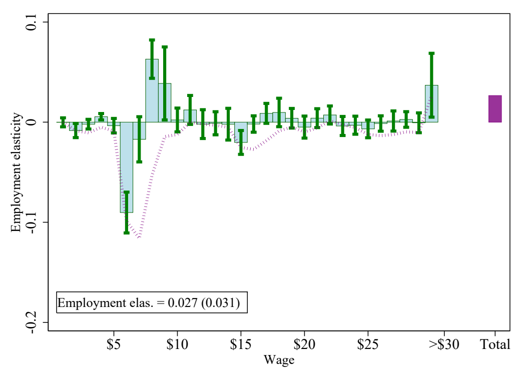
(a) High and middle probability groups for the TWFE-logMW



(b) High and middle probability groups for the EB



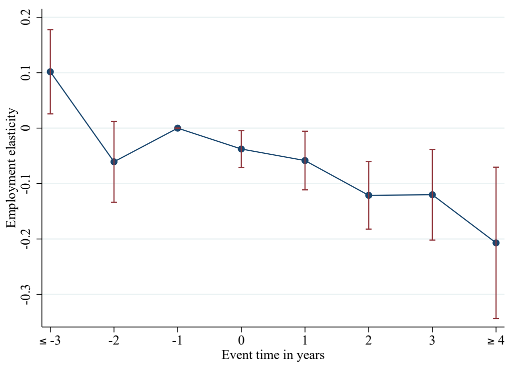
(c) Low probability for the TWFE-logMW



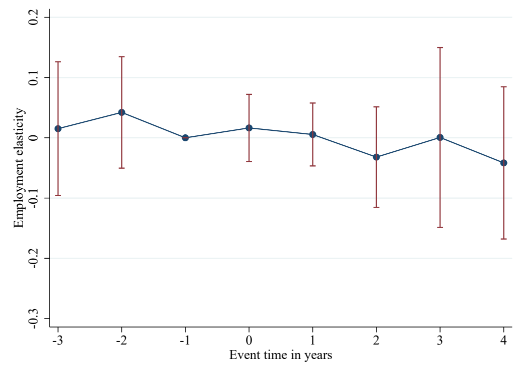
(d) Low probability for the EB

Notes: The figure shows the effect of the minimum wage on the wage distribution of the Card and Kruger probability groups in fixed effects (TWFE-logMW) and event-based specifications(EB). Panels (a) and (c) estimate the regression on the contemporaneous log minimum wage, as well as on 4 annual lags and 2 annual leads. Panels (b) and (d) use the EB specification (equation G.3), and regresses 4 annual lags and 3 annual leads in the event dummies. For each wage bin we run a separate regression, where the outcome is the number of jobs per capita in that state-wage bin. In panels (a) and (c) 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. In panels (b) and (d), the responses for each event date 0, 1,...,4 are captured by the corresponding α_τ. The green histogram bars show the mean of these cumulative responses for event dates 0, 1,...,4 relative to the event date -1, 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.

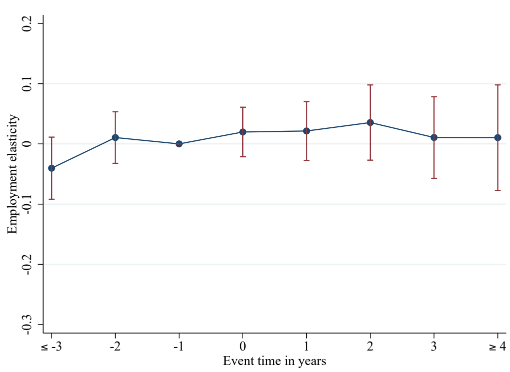
Figure G.3: Impact of Minimum Wages on Lower- and Upper-tail Employment Over Time for Fixed Effects Specification



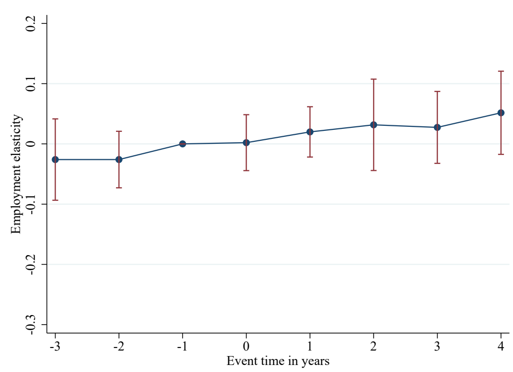
(a) TWFE-logMW, Employment Above $15



(b) EB, Employment Above $15



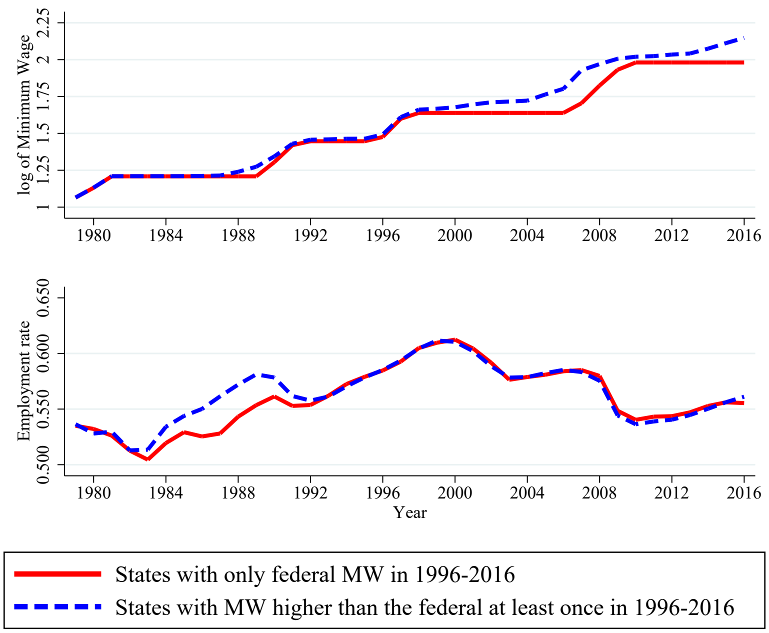
(c) TWFE-logMW, Employment Below $15



(d) EB, Employment Below $15

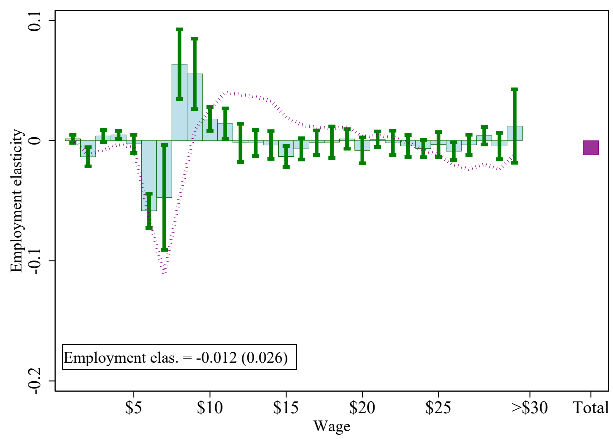
Notes: The figure shows the effect of the minimum wage on the number of jobs at or above (panels (a) and (b)), and below $15 (panel (c) and (d)) over time in the fixed effects (TWFE-logMW) and event-based specifications. Panels (a) and (c) estimate regressions of state-level total number of jobs below, 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. Panels (b) and (d) use the EB specification (equation G.3), and regresses 4 annual lags and 3 annual leads in the event dummies. The blue markers show cumulative employment elasticities by event date. In panels (a) and (c), the 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 (" ≤ -3") estimate. In panels (b) and (d), the responses for each event date are captured by the corresponding α_τ. 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 G.4: Time Paths of the Statutory Minimum Wage and Employment Rate in High and Low Minimum Wage States



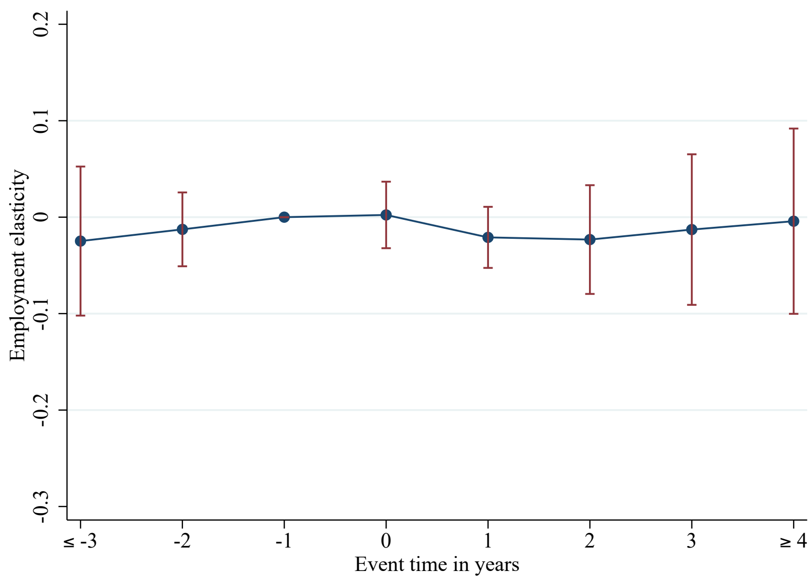
Notes: The figure shows the time paths of the average statutory log minimum wage (Panel (a)) and employment rate (Panel (b)) in 15 states where the federal minimum wage law applies in 1996 (low minimum wage states) and onward, and in 36 remaining states that had state-level minimum wages higher than 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.

Figure G.5: Impact of Minimum Wages on the Wage Distribution for TWFE-logMW Specification - 1993-2016 Sample



Notes: This figure is based on the same specification as Figure 6, but restricted to the 1993-2016 period. The figure shows the effect of the minimum wage on the wage distribution in fixed effects (TWFE-logMW) specification. We estimate two-way (state and year) fixed effects regressions 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 is the elasticity of the overall state employment-to-population with respect to minimum wage, obtained from regressions where the outcome variable is 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 are weighted by state population. The figure highlights that large aggregate disemployment effects are often driven by shifts in employment at the upper tail of the wage distribution.

Figure G.6: Impact of Minimum Wages on Employment Over Time for TWFE-logMW Specification - 1993-2016 Sample



Notes: This figure is comparable to panel (a) of Figure G.1, except that it restricts the sample to the 1993-2016 period. The figure shows the effect of the minimum wage on aggregate employment over time using the TWFE-logMW (equation G.2) regression of state-level aggregate 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, which is why the panel (a) shows a 3rd year or earlier (≤ -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.

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

	EB bunching	EB state discrete	EB bunching continuous	EB state continuous	FD	TWFE-log(MW)
	(1)	(2)	(3)	(4)	(5)	(6)
$[MW$-\$4, $MW+\$5)$	0.024		0.024			
	(0.025)		(0.020)			
Aggregate		0.016		0.008	0.031	-0.089***
		(0.029)		(0.025)	(0.031)	(0.025)
By wage bin						
Below $15		0.027		0.020	-0.005	0.020
		(0.022)		(0.016)	(0.020)	(0.028)
Over $15		-0.010		-0.012	0.035	-0.109***
		(0.042)		(0.033)	(0.023)	(0.030)
Number of observations	847,314	7,242	847,314	7,242	1,479	1,530
Period estimated	1979-2016	1979-2016	1979-2016	1979-2016	1979-2016	1979-2016
Equation	G.1	G.3	G.4	G.5	G.6	G.2

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, and on aggregate employment counts. 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 G.1). Column 5 uses a first-differences estimator with the change in the log minimum wage (equation G.6). For comparison, Column 6 reports the results using two way fixed effects estimator with log minimum wage (equation G.2) shown in Figure 9. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table G.2: Employment Elasticities with Respect to the Minimum Wage, Event-based and Continuous Variation - by Probability Groups

	EB-State Discrete (1)	EB-State Continuous (2)	FD (3)	TWFE-log(MW) (4)
By demographically predicted wage				
Predicted low-wage workers	0.019**	0.022*	0.015	-0.014
	(0.009)	(0.013)	(0.010)	(0.009)
Predicted middle-wage workers	0.026	0.001	0.019	0.027
	(0.020)	(0.014)	(0.024)	(0.035)
Predicted high-wage workers	-0.029	-0.015	-0.004	-0.103***
	(0.023)	(0.022)	(0.013)	(0.038)
Number of observations	7,242	7,242	1,479	1,530
Period estimated	1979-2016	1979-2016	1979-2016	1979-2016
Equation	G.3	G.5	G.6	G.2

Notes. Column 1 uses the same event study design our baseline approach (see equation 2) but estimates the effect on the aggregate employment of three Card and Krueger probability groups. Column 2 reports the results using continuous treatment measure. For comparison, Column 3 and 4 report the results using a first-differenced (equation G.6) or two way fixed effects estimator with log minimum wage (equation G.2) shown in Figure 9. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table G.3: Aggregate Employment Elasticities with Respect to the Minimum Wage: Robustness of Alternative Model Specifications to Controls

	Baseline (1)	State-specific linear trends (2)	Base period occ. & ind. shares (3)	Post-1992 sample (4)
Panel A: TWFE-log(MW)				
Emp. elas. wrt MW	-0.089***	0.010	-0.025	-0.012
	(0.025)	(0.036)	(0.029)	(0.027)
Number of observations	1,530	1,530	1,530	1,020
Panel B: EB				
Emp. elas. wrt MW	0.016	0.022	0.022	-0.009
	(0.029)	(0.023)	(0.028)	(0.018)
Number of observations	7,242	7,242	7,242	4,538
Panel C: FD				
Emp. elas. wrt MW	0.027	0.037	0.034	0.017
	(0.031)	(0.035)	(0.023)	(0.030)
Number of observations	1,479	1,479	1,479	1,020
Number of states	51	51	51	51
Period estimated	1979-2016	1979-2016	1979-2016	1993-2016

Notes. The table reports estimated aggregate employment elasticities of minimum wage from alternative approaches. Each column and panel is a separately estimated model specification. Panel A shows the results using TWFE-logMW specification (see equation G.2), Panel B the event-based specification (equation G.3), while Panel C shows the first-differenced specification (equation G.6). Column 1 reports the results obtained from the baseline specifications, while column 2 augments the model with state-specific linear trends. Column 3 additionally controls for 1979-1980 major industry and occupation shares interacted with time fixed effects. Column 4 is similar to the first column, but limits the time span of the sample to 1993-2016.Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table G.5: 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)
Number of observations	1,530	1,020	1,200	1,200	1,200	1,170
Panel B: 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)
Number of observations	7,242	4,538	5,680	5,680	5,680	5,538
Panel C: 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)
Number of observations	1,479	1,020	1,160	1,160	1,160	1,131
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), the event-based (Panel B), and first-differences specifications (Panel C). 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.

Table G.4: Aggregate Employment Elasticities with Respect to the Minimum Wage: Robustness of Alternative Model Specifications to Presidential Voting Index

	Baseline (1)	With continuous PVI control (2)
Panel A: TWFE-log(MW)		
Emp. elas. wrt MW	-0.089***	-0.027
	(0.025)	(0.022)
Number of observations	1,530	1,530
Panel B: EB		
Emp. elas. wrt MW	0.016	0.009
	(0.029)	(0.028)
Number of observations	7,242	7,242
Panel C: FD		
Emp. elas. wrt MW	0.027	0.029
	(0.031)	(0.033)
Number of states	51	51
Period estimated	1979-2016	1979-2016
Number of observations	1,479	1,479

Notes. The table shows the robustness of the estimated minimum wage elasticities for aggregate employment using TWFE-logMW (Panel A), EB (Panel B), and FD (Panel C) specifications to the presidential voting index control. The first column reproduces the first column of Table G.3, while column 2 augments it with presidential voting index variable. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table G.6: Determinants of Having State-level Minimum Wage in Post-1996

	(1)	(2)	(3)
Severity of the 1990-1991 shock	4.204***	1.473	-2.770
	(1.319)	(1.864)	(2.306)
Unionization rate in the 1980s		3.007**	
		(1.124)	
Average wage in the 1980s		0.001	
		(0.001)	
HSL share in the 1980s		-0.838	
		(1.496)	
PVI: Rep-Dem Vote Share in the 2000s			-2.035***
			(0.576)
IV specification			Y
First stage F-statistic			74.737
Number of observations	51	51	51

Notes. The table reports the probability of having a state minimum wage higher than the federal level in any year after 1996. The predictor "severity of the 1990-1991 shock" is the percentage point decline in state-level employment due to the 1990-1991 recession. "Unionization rate in the 1980s", "Average wage in the 1980s", and "HSL share in the 1980s" are the average unionization rate, wage, and the share of individuals with high school or less education in the 1980s, respectively. "PVI: Rep - Dem Vote Share in the 2000s" is a partisan voting index that shows the difference between Republican Party and Democratic Party candidates' vote shares in the state. The first two columns use least squares estimators, and column (3) a two-stage least squares (2SLS) regression. In the 2SLS regression we use the partisan voting index from 1988 as an instrumental variable. Regressions are weighted by state averaged population. Robust standard errors are in parentheses; significance levels are * 0.10, ** 0.05, *** 0.01.

Table G.7: Robustness of Alternative Model Specifications to Controls; Teen Sample

	Baseline	State-specific linear trends	Post-1992 sample	Post-1992 sample & State-specific linear trends
	(1)	(2)	(3)	(4)
Panel A: TWFE-log(MW)				
Emp. elas. wrt MW	-0.238***	0.065	-0.024	0.124
	(0.088)	(0.128)	(0.153)	(0.128)
Number of observations	1,530	1,530	1,020	1,020
Panel B: EB				
Emp. elas. wrt MW	0.163	0.162	0.162	0.152
	(0.115)	(0.100)	(0.135)	(0.121)
Number of observations	7,242	7,242	4,538	4,538
Panel C: FD				
Emp. elas. wrt MW	0.094	0.143	0.059	0.081
	(0.122)	(0.129)	(0.137)	(0.139)
Number of observations	1,479	1,479	1,020	1,020
Number of states	51	51	51	51
Period estimated	1979-2016	1979-2016	1993-2016	1993-2016

Notes. The table reports estimated teen employment elasticities of minimum wage from alternative approaches. Each column and panel is a separately estimated model specification. Panel A shows the results using TWFE-logMW specification (see equation G.2), Panel B the event-based specification (equation G.3), while Panel C shows the first-differenced specification (equation G.6). Column 1 reports the results obtained from the baseline specifications, while column 2 augments the model with state-specific linear trends. Column 3 is similar to the first column, but limits the time span of the sample to 1993-2016. Column 4 also limits the sample to 1993-2016, and includes state specific linear trends. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table G.8: Reconciliation with DLR (2010) Findings

Specification:	TWFE-logMW	Event-based	First-differences	BCP	BCP & EB
	(1)	(2)	(3)	(4)	(5)
Restaurant emp. elasticity	-0.289**	0.002	-0.055	-0.016	0.043
	(0.113)	(0.056)	(0.094)	(0.082)	(0.055)
Number of observations	88,320	91,080	86,940	40,448	41,316
Overall emp. elasticity	-0.131	-0.024	0.023	0.001	0.030
	(0.091)	(0.052)	(0.094)	(0.073)	(0.054)
Number of observations	197,631	203,807	194,542	144,768	148,896

Notes. This table shows estimated restaurant and aggregate employment elasticities of minimum wage using fixed effects (TWFE-logMW), event-based (EB), first differenced (FD), and border county-pairs (BCP) specifications. The BCP specifications exactly follows the proposed specification of Dube, Lester, and Reich (2010). Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.