The Effect of Minimum Wages on Low-Wage Jobs: Evidence from the United States Using a Bunching Estimator*

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Doruk Cengiz§
University of Massachusetts Amherst

Arindrajit Dube‡
University of Massachusetts Amherst, NBER, IZA

Attila Lindner§§
University College London, CEP, IFS, IZA, MTA-KTI

Ben Zipperer†
Economic Policy Institute

Abstract

We propose a novel method that infers the employment effect of a minimum wage increase by comparing the number of excess jobs paying at or slightly above the new minimum wage to the missing jobs paying below it. Using state-level variation in U.S. minimum wages, we implement our method by providing new estimates on the effect of the minimum wage on the frequency distribution of hourly wages. First, we present a case study of a large, indexed minimum wage increase using administrative data on hourly wages from Washington state. Then we implement an event study analysis pooling 138 minimum wage increases between 1979 and 2016. In both cases, we find that the overall number of low-wage jobs remained essentially unchanged over 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. In contrast to our bunching-based estimates, we show that some conventional studies can produce misleading inference due to spurious changes in employment higher up in the wage distribution.

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§dcengiz@econs.umass.edu, ‡adube@econs.umass.edu, §§a.lindner@ucl.ac.uk, †bzipperer@epi.org

1 Introduction

Minimum wage policies have featured prominently in recent policy debates in the United States at the federal, state and local levels. In the past two years, two large states (California and New York) have passed legislation to increase minimum wages to $15/hour by 2022 or sooner. 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," the effect of minimum wage on employment remains a controversial topic among economists (Card 1992; Neumark and Wascher 1992; Card and Krueger 1995; Neumark and Wascher 2008; Dube, Lester and Reich 2010). Moreover, the debate has often been concentrated on the impact on teen employment or on workers in specific sectors (Allegretto et al. 2017, Dube, Lester and Reich 2010, Manning 2016, Neumark, Salas and Wascher 2014, Totty 2017) while the evidence on the impact on total employment remains limited. This shortcoming is particularly acute given the importance policymakers place on understanding the overall employment effect on low-wage workers. For example, in its attempt to arrive at such an estimate, a 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 propose a novel method to assess the overall employment effect of the minimum wage together with its effect on the shape of the frequency distribution of wages. Our method infers the disemployment effect of the minimum wage by tracking the changes in the number of jobs throughout the wage distribution following a minimum wage increase. The changes at the bottom of the wage distribution—in particular the missing jobs below the minimum, and the excess jobs at or just above the minimum—reflect the effect of the minimum wage on low-wage workers. Therefore, our approach allows us to jointly estimate the effect of minimum wages on the wages and employment of low-wage workers, the primary target of the policy.

The basic idea behind our approach is captured in Figure 1, which shows a hypothetical frequency distribution of wages with and without a statutory minimum wage. The binding minimum wage will directly affect jobs that were previously paying below the new minimum wage. These jobs may either be destroyed or shifted into compliance with the mandated minimum. The number of jobs shifted into compliance will create a spike at the minimum wage. In practice, firms may sometimes shift pay at affected jobs somewhat above the minimum (Dube, Giuliano and Leonard 2015). However, they are unlikely to shift such jobs to the very top of the wage distribution. Moreover, to the extent that the new minimum wage increases labor market entry, some additional workers may end up finding jobs close to the minimum. Hence, the amount of "bunching" in the wage distribution at and slightly above the minimum wage is a nonparametric indicator that jobs are being preserved or created. The difference between the number of excess jobs at and slightly above the minimum wage and the number of missing jobs below the minimum provides an estimate for the overall effect of the policy on low-wage workers.

There are several advantages of using the bunching method to estimate disemployment effects of the minimum wage. First, the bunching approach focuses on employment changes locally around wage levels where minimum wages are likely to play a role. Second, the localized comparisons of the missing and excess jobs in the wage distribution allows us to estimate the effect on overall employment and on subgroups where only a small fraction of workers are directly affected by the minimum wage. This is particularly important because the standard approaches often fails to find a meaningful "first stage" wage estimates for these workers, which renders any employment elasticities unreliable.

Third, our approach also highlights that estimates with large upper tail employment changes are likely to reflect problems with the empirical model and not the causual effect of the minimum wage. As a result, reporting employment changes throughout the whole distribution provides an additional falsification test of the empirical model. Since, the academic debates in the minimum wage literature often focuses on which specification is the "right" one, this additional falsification test is partiuclarly relevant in our context. Finally, the bunching approach gains precision by filtering out random shocks to jobs in the upper part of the wage distributon.

We implement the bunching estimator proposed above by comparing the actual frequency distribution of wages observed in the data to our estimates of the counterfactual distribution. We deviate from previous literature which has constructed this counterfactual 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). Instead, here we exploit state-level variation in the minimum wage and compare states with minimum wage changes to states without—taking advantage of a difference-in-differences style estimation.

We begin our empirical analysis by using administrative data on hourly wages from the state of Washington. We examine the effect of raising the minimum wage from $7.54 to $9.18/hour (in 2016 dollar value) in 1999, which is one of the largest indexed state-level minimum wage changes instituted in the U.S. to date. We calculate the counterfactual frequency distribution of hourly wages in Washington by adding the average change in per-capita employment in the control states to the pre-treatment per-capita employment count in Washington by each dollar wage bin.[1] When we compare this counterfactual distribution to the actual one, we find a sharp reduction in the number of jobs paying below $9/hour following the minimum wage

[1] We use the Current Population Survey to calculate the changes in employment in the control states.

increase. At the same time, we find an equally sized increase in jobs paying hourly wages between \$9 and \$14, implying a limited overall employment effect of the minimum wage increase. Reassuringly, the distribution of jobs paying above \$14/hour was quite stable compared to the counterfactual following the minimum wage increase, raising confidence in the comparability of the treatment and control groups.

The limitation of the Washington analysis is that it relies on one specific case study, and so inference is inherently problematic.[2] To overcome this challenge, we use hourly wage data from the 1979-2016 Current Population Survey (CPS) to estimate the impact of state-level minimum wage increases.[3] Pooling 138 such policy changes, we implement an event study analysis covering three years prior to and five years following each change. We find 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, the number of these missing jobs closely matches the number of excess jobs paying just above the minimum. Our baseline specification shows that in the five years following the minimum wage increase, 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.

We also track job changes throughout the wage distribution between three years before and five years after the minimum wage increases. 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. Following the minimum wage increase, the drop in the number of jobs below the new 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, making it unlikely that the lack of employment responses in our sample is driven by the non-durability of the minimum wage increase (e.g., Sorkin 2015). Moreover, the evolution of excess jobs

[2] A recent working paper by Jardim et al. (2017) estimates the employment effect of the 2015-2016 Seattle minimum wage increase by tracking the changes in employment at the bottom of the wage distribution. They cite an earlier version of our paper and remark on the similarity in the methods. Jardim et al. (2017) finds a large negative disemployment effect, which is in stark contrast with our finding on the effects of the large and indexed state-level minimum wage change instituted between 1999 and 2000 in Washington state. The differences in the findings are unlikely due to the greater bite of Seattle's minimum wage. Being a high wage city, the minimum-to-median wage ratio in 2016 for Seattle was 0.45, as compared to 0.49 for the state of Washington in 2000 after the minimum wage increase we study. Instead, the discrepancy in the estimated employment effects across the two case studies highlights the importance of using many events for inference instead of relying on one particular minimum wage change.

[3] One key concern with implementing the bunching method using CPS data is that small sample sizes and the presence of measurement error may make it difficult to detect any meaningful change in the shape of the wage distribution. However, as we show later, we indeed detect large shifts in the number of jobs at the bottom of the wage distribution using the CPS data, and we estimate a clear wage effect from the policies. Moreover, in Online Appendix C, we also use administrative data on hourly wages from three U.S. states that collect this information (Minnesota, Washington, Oregon) to show that the wage distributions in the CPS and in the administrative data are quite similar both in the cross section as well over time.

closely matches the evolution of missing jobs, which implies that the employment effect of the minimum wage is similar in the short and in the longer run—at least up to five years following the policy change.

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., Neumark, Salas and Wascher 2014 and Allegretto et al. 2017). We show that the inclusion of wage-bin-by-state-specific linear or quadratic trends or allowing the wage-bin-by-period effects to vary across the nine Census divisions does not affect our main conclusion. Moreover, estimates from a triple-difference specification that uses state-specific period effects to control for any state-level aggregate employment shocks are similar to our main results. We also show 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; and additionally using federal-level minimum wage changes for identification.

While we find no overall reduction in low-wage jobs, this could mask some shift in employment from low-skill to high-skill workers. To test for such labor-labor substitution directly, we partition workers into groups based on four education and six age categories. 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. In addition, we separately analyze those without a high school degree, those with high school or less schooling, women, black or Hispanic individuals, and teens. 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.

The pooled event study estimates may mask some heterogeneity in the responses to the minimum wage. 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. Our event-by-event analysis finds that the estimated missing jobs rise in magnitude substantially with the minimum-to-median wage (Kaitz) index. At the same time, the number of excess jobs also rise 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 55 percent, confirming that minimum wage changes in the U.S. we study have yet to reach a level above which significant disemployment effects emerge.

A key advantage of our bunching approach is that by focusing on employment changes at the bottom of the wage distribution, we can assess the disemployment effect even for groups where only a small fraction of workers are affected by the minimum wage. We use this feature to provide a comprehensive assessment of the effect of minimum wages on employment 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, restaurant, retail and other sectors—which together comprise the vast majority of minimum wage workers in the U.S. 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).

We also explore whether minimum wages have a differential impact on workers who had a job before the minimum wage increase (incumbents) and new entrants to the labor market. We find no differences in terms of employment changes, but the pattern of wage increases is quite different: while incumbent workers experience significant wage spillovers up to $3 above the minimum wage, we do not find any evidence of spillovers for new entrants. This asymmetry suggests that it is unlikely that our estimates of spillovers reflect an increase in the value of outside options or reservation wages of non-employed workers (e.g. Flinn 2006).

This article makes several key contributions to the existing literature on minimum wages. First, our paper relates to a handful of papers that have tried to assess an overall employment effect of minimum wages. Meer and West (2016) examine the relationship between aggregate employment at the state-level and minimum wage changes. We also provide an aggregate employment estimate in this paper, but our approach refines this analysis by focusing only on the changes in employment at the bottom of the wage distribution where the employment effects are likely to be concentrated. In our event based analysis, both of these employment estimates are close to zero. To highlight the importance of assessing employment changes far above the minimum wage, we also calculate the bin-by-bin employment effects using variants of the classic two-way fixed effects regression on log minimum wage (e.g., Meer and West (2016)). This exercise produces a striking finding: the specifications that indicate a large negative effect on aggregate employment seem 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. These apparent job losses in the upper tail also happen in demographic groups with few minimum wage workers.

Our bunching approach also has advantages over methods that focus on the employment prospects of workers earning low wages prior to the minimum wage increase (Abowd et al. 2000; Currie and Fallick 1996; Clemens and Wither 2016). Restricting the sample to workers who had a job before the minimum wage does not account for impact on new entrants. In contrast, the bunching at the minimum wage reflects the effects on both the incumbents and on the new entrants—especially important given the high rate of job turnover at the bottom of the wage distribution.

Second, our paper contributes to the extensive literature on the effect of the minimum wage on overall wage inequality (DiNardo, Fortin and Lemieux 1996; Lee 1999; Autor, Manning and Smith 2016). These papers examine shifts in the wage density, and assume away any possible disemployment effect. The key novelty of our approach is that by focusing on the frequency distribution instead of the density, we can assess the effect on wage inequality and employment at the same time.[4] Namely, we show that the measured wage spillovers are not an artifact of disemployment which would truncate the wage distribution. We also produce new estimates of the spillover effect of the minimum wage, which has received particular attention in the literature: we find that such spillovers extend up to $3 above the minimum wage and represent around 40% of the overall wage increase from minimum wage changes. These estimates are similar to the findings of Autor, Manning and Smith (2016) and Brochu et al. (2017), and more limited than Lee (1999). Autor, Manning and Smith (2016) also demonstrate that spillover effects cannot be distinguished from wage misreporting in survey data. Here we show that spillovers are present in administrative data as well, which suggests that these spillovers are not only due to misreporting in survey data. Moreover, we extend the literature on wage inequality by showing that, in the short run, spillover effects are mainly driven by incumbent workers who were employed before the minimum wage increase, while new entrants who moved from non-employment did not benefit from spillovers.[5]

Third, our paper relates to the literature on labor-labor substitution in response to minimum wages. Our analysis goes well beyond the limited existing evidence on the question, which has typically focused on specific groups like teens (Giuliano 2013) or has used individual case studies (Fairris and Bujanda 2008).

Finally, our paper is also related to the growing literature that uses bunching to elicit behavioral responses to public policies (Kleven 2016). Minimum wages discontinuously increase the cost of hiring below the wage floor, while the extent of bunching provides prima facie evidence of a causal effect of the incentive in question. However, while the standard bunching analysis estimates the counterfactual distribution from purely cross sectional variation (Saez (2010); Chetty et al. (2013)), here we follow the most recent literature (e.g. Brown (2013); Best and Kleven (2013)) and use a difference-in-differences strategy to construct the counterfactual wage distribution.

The rest of the paper is structured as follows. Section 2 explains the bunching approach, and describes the key advantages. Section 3 uses administrative data from Washington state and a large, permanent minimum wage increase to illustrate our bunching approach. Section 4 develops our pooled event study implementation, describes the data and sample construction, and presents the empirical findings including the main results,

[4]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.

[5]The differential responses for incumbents and new entrants also suggest that the ripple effects are likely to be driven by economic factors and not by wage misreporting error, since the latter should be similar across these two groups.

heterogeneous effects by worker characteristics as well as types of treatments, additional robustness checks for sample and specification, and an event-specific analysis of effects when the bite is larger. Section 5 demonstrates the importance of assessing employment changes far above the minimum wage and highlight problems with theclassic two-way fixed effects estimation Section 6 concludes.

2 Methodology

We propose a novel method that identifies 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 shows 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 shows 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 in the data will be sub-minimum wage because of measurement error, imperfect coverage or imperfect compliance. Therefore, the missing jobs below the MW, $\triangle b = Emp^1 [w < MW] - Emp^0 [w < MW]$[6] reflect the size of the bite of the minimum wage.[7]

Not all missing jobs below the minimum wage are destroyed. Some (or all) jobs below the minimum wage will be kept and their hourly pay will be raised to the minimum wage, creating a spike at the MW. Some jobs will be pushed slightly above the minimum wage in order to keep within firm wage hierarchy or because the minimum wage raises the bargaining power of workers (e.g. Flinn, 2011). Moreover, a minimum wage might induce some low-wage workers to participate in job search, and some of them may find a job above the minimum wage. However, the ripple effects of the minimum wage likely to fade out at certain point, which we denote by $\overline{W}$ on Figure 1. In the neoclassical model there can be some positive employment effects in the upper tail of the wage distribution caused by labor-labor substitution. However, the magnitude of these effects are negligible in practice.[8] In models with labor market friction, wage spillovers also typically fade out,

[6] Here $Emp^1[.]$ and $Emp^0[.]$ are the actual and counterfactual frequency distributions of wages, respectively.

[7] We will discuss later how measurement error effect our estimates.

[8] To see this, consider a three-factor version of the Hicks-Marshall law of derived demand with: low-skilled, minimum wage labor (L); higher skilled, non-minimum wage labor (H); and capital. The effect of a change in low-skilled wage on higher skilled labor demand is given by the formula:

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

where s_L is the share of minimum-wage labor in total poduction, σ_{HL} is the elasticity of substitution between higher-skilled and low-skilled labor, and η is the ouput demand elasticity. The low-skilled share in production is $s_L \approx 2\%$, the Katz and Murphy (1993) estimates $\sigma_{HL} \approx 1.3$ and η is often assumed to be 1 (Aaronson and French, 2007). This implies a cross-wage elasticitiy of $\frac{\partial \ln L^H}{\partial \ln w^L} \approx 0.006$. On average, for a 10% increase in the minimum wage in our sample, hourly wages of affected workers increase by 8%. This implies a minimum wage elasticity for higher-skilled employment of $\frac{\partial \ln L^H}{\partial \ln MW} = \frac{\partial \ln L^H}{\partial \ln w^L} \times \frac{\partial \ln w^L}{\partial \ln MW} \approx 0.006 \times 0.8 = 0.0048$. Therefore, any plausible estimate of minimum wage impact on upper-tail employment should be very small. We also empirically show the absence of an effect on the upper-tail of the distribution using our event-based design in section 4.

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).

We assess the employment effect of the minimum wage on low-wage workers by summing the missing and excess jobs $\triangle b + \triangle a$. Such an estimator is analogous 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). Minimum wage creates a discontinuity (a notch) by making it very costly to hire a worker below a certain wage level and the changes in the frequency distribution of wages identifies behavioral responses to this policy. As usual for the bunching approach, the spike at the minimum wage and the elevated mass slightly above it provides prima facie evidence of a causal effect of the incentive in question.

It is also worth pointing out that that the sum of the missing and excess jobs is in fact equal to the employment change below $\overline{W}$: $\triangle b + \triangle a = Emp^1\left[w < \overline{W}\right] - Emp^0\left[w < \overline{W}\right]$. This highlights that the key idea behind the estimator is to focus on employment changes at the relevant part of the wage distribution, not the entire distribution.

There are four key advantages of focusing on the bottom of the wage distribution. First, the bunching approach proposed here identifies the employment effects using 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, and yet rarely exploited. Second, and more importantly, our localized approach allows us to estimate the effects on subgroups where the standard approaches 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. [9]

Third, our approach locates the source of the employment effects within the wage distribution, and so one can use the upper tail employment changes as a falsification test. Since large changes in jobs paying above $15 are unlikely to reflect the causal effect of the minimum wage, reporting such changes in employment at the upper tail can be highly informative about model validity. Moroever, 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 in the U.S.). The contribution of these omitted variables would be sizable compared to the relatively small expected effect of the minimum wage on aggregate employment. As a result, the bias in the upper tail can be particularly relevant when someone is interested in estimating the overall

[9] In the Online Appendix Table A.2 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 bunching based estimates always produce sizable and significant wage effects.

employment effect of the minimum wage. Fourth, even if changes in the upper tail do not bias the estimates, the bunching approach often improves the precision of estimates by filtering out random shocks to jobs in the upper part of the wage distribution.

3 Washington State Case Study

To implement our bunching method, we first study one of the largest state-level minimum wage changes instituted in the U.S. 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.[10] Using hourly wage data, we can easily calculate the actual post-reform wage distribution (blue line in Figure 1). However, the key challenge implementing the bunching method is that we do not directly observe the wage distribution in the absence of the minimum wage increase (red line in Figure 1). To overcome this challenge, the previous literature constructed the counterfactual by imposing strong parametric assumptions (Meyer and Wise 1983) or simply used the pre-reform wage distribution as a counterfactual (Harasztosi and Lindner 2016).[11] 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

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

[11] 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.

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:

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

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 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 4.2. For the second part of the expression, the pre-treatment Washington wage distribution, we use administrative data on hourly wages. However, in Appendix Figure A.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 2, 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.[12] Such a relationship between the actual and counterfactual distributions closely resembles the illustration of the bunching method shown in Figure 1.

The difference between the actual, $e_{WA,k,Post}$, and the counterfactual, $e^{CF}_{WA,k,Post}$, frequency distributions

[12] We will turn to discuss the extent and scope of spillovers further in Section 4.5.

of wages represents the causal effect of the minimum wage on the wage distribution. This difference can be expressed as:

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

which is the classic difference-in-differences estimator underlying the core estimates in the paper.

The blue bars in Panel (b) of Figure 2 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% 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%.[13]

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

[13] We will discuss the details of how we calculate the percentage change in employment and wages in the next section.

panel (a) in Online Appendix Figure A.3. The figure shows that excess and missing jobs are close to zero before 1999, and there are no systematic pre-existing trends.[14] 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 A.3).

4 Pooled Event Study Analysis

The Washington state case study provides key insights on how bunching at the minimum wage can be used to identify the employment effects of the minimum wage, and how a difference-in-differences strategy can be used to construct the counterfactual wage distribution. However, inference based on a single minimum wage change is inherently problematic. Therefore, we implement an event study analysis where we pool across various state-level minimum wage changes occurring between 1979 and 2016.

4.1 Event Study: Empirical Strategy

The empirical estimation of the pooled event study analysis follows the same difference-in-differences approach as our Washington case study (e.g., equation 1). Like other difference-in-differences estimators, equation 1 can be implemented using a regression—which is useful when aggregating across multiple events as we do in this section. In our empirical implementation, we begin by constructing a state-by-quarter-by-\$0.25 wage bin panel dataset spanning 1979q1 to 2016q4; the details of how this is constructed are explained in Section 4.2. Using this data, we examine the effect of minimum wage events on per-capita employment counts, $\frac{E_{swt}}{N_{st}}$, where E_{swt} is the employment in wage bin w, in state s and at time t, while N_{st} is the size of the population in state s and time t.

In our baseline specification, we use a 32 quarter treatment event window ranging between $[-3, 4]$ in annualized event time. Here $\tau = 0$ represents the first year following the minimum wage increase, i.e., the quarter of treatment and the subsequent three quarters. Similarly, $\tau = -1$ is the year (four quarters) prior to treatment, while $\tau = 4$ is the fifth year following treatment. Our treatment variables are not only a function of state and time, but also of the wage bins. We denote a \$1 interval relative to the new minimum wage

[14]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.

by k, so that $k = 0$ represents the four \$0.25 bins between MW and $MW + \$0.99$. The "below" bins are those with $k \in \{-4, -3, -2, -1\}$, i.e. with wages paying between $MW - \$0.01$ and $MW - \$4.00$. While our bunching approach focuses on wage bins within a few dollars of the new minimum wage, we estimate and report employment changes throughout the full distribution. Therefore, we allow "above" bins to include $k \in \{0, 1, 2, 3, ..., 17\}$, where $k = 17$ includes jobs that pay \$17 above the new minimum wage or more.

To assess the effect of the minimum wage on the wage distribution in an event study framework, we use the following regression specification estimated using the full panel:

$$\frac{E_{swt}}{N_{st}} = \sum_{\tau=-3}^{4} \sum_{k=-4}^{17} \alpha_{\tau k} I_{swt}^{\tau k} + \mu_{sw} + \rho_{wt} + u_{swt} \tag{2}$$

Here $I_{swt}^{\tau k}$ is an indicator variable that is equal to 1 if the minimum wage was raised τ years from date t and for the \$0.25 wage bins w that fall between k and $k + 1$ dollars of the new minimum wage. We examine the effects between three years before and five years after the minimum wage change. Our benchmark specification also controls for state-by-wage bin and period-by-wage bin effects, μ_{sw} and ρ_{wt}. This allows us to control for state specific factors in the earnings distribution and also the nation-wide evolution of wage inequality.

The estimated $\alpha_{\tau k}$ allow us 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 2. Analogously, the change in the number of jobs (per capita) paying 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 $\overline{W} = MW + 4$, but show robustness to differenct choices of this cutoff. 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 $\Delta b_\tau = \frac{\sum_{k=0}^{4} \alpha_{\tau k} - \sum_{k=0}^{4} \alpha_{-1k}}{\overline{EPOP}_{-1}}$. 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 bunching estimate for the percentage change in total employment due to the minimum wage increase. If we divide this 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, $\overline{b}_{-1}$:[15]

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

We can 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 $\overline{w}_{-1} = \overline{wb}_{-1}/\overline{b}_{-1}$. Here the wage bill, $\overline{wb}_{-1}$, and the number of workers earning below the new minimum wage just prior to the increase, $\overline{b}_{-1}$, are averages for the full sample of events. The minimum wage increase causes both the wage bill and employment to change. The new average wage in the post-treatment period is equal to $w = \left(\overline{wb}_{-1} + \Delta wb\right) / \left(\overline{b}_{-1} + \Delta e\right)$.[16] Therefore, the percentage change in the average wage is given by:

$$\%\Delta w = \frac{w}{\overline{w}_{-1}} - 1 = \frac{\frac{\overline{wb}_{-1} + \Delta wb}{\overline{b}_{-1} + \Delta e}}{\frac{\overline{wb}_{-1}}{\overline{b}_{-1}}} - 1 = \frac{\%\Delta wb - \%\Delta e}{1 + \%\Delta e} \tag{3}$$

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 can estimate the employment elasticity with respect to own-wage (or the "labor demand elasticity" in a competitive market):

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

Besides the baseline regression, we also estimate a variety of other more saturated specifications that

[15] 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.

[16] 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 $[MW, MW + 1)$.

(1) include bin-by-Census-division-by-period fixed effects that allow for regional time-varying heterogeneity by wage bin and (2) include bin-by-state-specific linear and quadratic time trends by wage bin. These allow for richer time-varying heterogeneity in the earnings distributions across states. We also estimate a "triple-difference" specification which includes controls for state-by-period fixed effects, which nets out any aggregate state-specific employment shocks.[17] This is a rich specification, which also highlights the advantage of our approach which can directly assess whether minimum wage estimates for total employment are contaminated by such aggregate shocks—something that is not possible when estimating a state panel regression with aggregate employment as the outcome (e.g., Meer and West 2016). At the same time, it is worth noting that if there is a positive employment effect on the upper tail (say from labor-labor substitution), the triple difference specification will tend to exaggerate any disemployment effect.

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.[18] We do not use federal minimum wages in our primary sample because in these cases there are no control locations with jobs below the new federal minimum wage—which means the excess and missing job counts are not well-identified separately. However, we show our results are robust to including federal minimum wage increases in our treatment definition. We cluster our standard errors by state, which is the level at which policy is assigned. Our standard errors, therefore, take into account that employment changes at different parts of the wage distribution may be correlated within a state.[19]

4.2 Data and sample construction

For the pooled event study, 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 use any observations with imputed wage data in order to minimize the role of measurement error.[20] There are no reliable imputation data for January 1994 through August 1995, so we exclude this entire period from our sample. Our available

[17]By an aggregate shock, we mean a change in state employment that preserves the shape of the wage distribution.

[18]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.

[19]When calculating the employment elasticity respect to own wage, we use the delta method (using STATA's `nlcom` command).

[20]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 1995-2015, 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).

sample of employment counts therefore spans 1979q1 through 1993q4 and 1995q4 through 2016q4.[21] We use the CPS data to implement the pooled event study analysis.

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.[22] 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 state-level population estimates, N_{st}, from the CPS, which 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). As we discuss below, the QCEW benchmarking has little effect on our point estimates, but substantially increases their statistical precision.[23]

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).[24] Appendix Figure A.1 shows that during our CPS sample period (1979-2016) there are 516 minimum wage increases, where markers indicate all changes in the state or federal minimum wage, and gray, vertical lines illustrate the timing of federal increases. Many increases are federal changes, in green, 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 a wage floor lower then the new minimum wage. We also

[21] In general, there has been an increase in the rate of imputation over time. However, in the Online Appendix, we show that minimum wage raisies 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. (See Online Appendix Table A.5 and Online Appendix Figure A.6.)

[22] 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 $(0.00, 1.25), [1.25, 1.50), \dots, [29.75, 30.00), [30, \infty)$.

[23] Our outcome, the per-capita count for wage bin w, $\frac{E_w}{N}$, can be rewritten as the product of the (discretized) wage density, $f_w = Prob(w \leq wage < w + 0.25)$, and the employment to population ratio, $\frac{E}{N}$, so $\frac{E_w}{N} = f_w \times \frac{E}{N}$ (here we omit the s and t subscripts for simplicity). The raw CPS-based estimate for per-capita count is $\widehat{\frac{E_w}{N}}^{CPS} = \widehat{f_w}^{CPS} \times \widehat{\frac{E}{N}}^{CPS}$. The QCEW benchmarked CPS uses the state-level employment counts from the QCEW which has no measurment error given that includes the near universe of workers; so formally, $\widehat{\frac{E_w}{N}}^{QCEW} = \widehat{f_w}^{CPS} \times \frac{E}{N}$. It is straightforward to show that the mean squared prediction error ($MSPE$) is lower for the QCEW benchmarked CPS than for the raw CPS, $MSPE\left(\widehat{\frac{E_w}{N}}^{QCEW}\right) < MSPE\left(\widehat{\frac{E_w}{N}}^{CPS}\right)$ if the measurment errors for $\widehat{f_w}^{CPS}$ are uncorrelated with $\widehat{\frac{E}{N}}^{CPS}$. The latter condition holds if the source of the error is sampling. We confirm this empirically in C.1 and show that benchmarking the CPS with QCEW increases the accuracy of predicting low-wage employment in the administrative data from Minnesota, Oregon, and Washington.

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

exclude small minimum wage increases, in orange, which we define as minimum wage changes less than \$0.25 (the size of our wage bins) or events where less than 2 percent of the workforce earn between the new and the old minimum wage. Excluding federal and small increases reduces our primary sample of minimum wage increases to 138 (blue) events. On average, 8.6% of workers are below the new minimum wage in the year before these 138 events and the mean real minimum wage increase is 10.1%.

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.[25] 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 \$3 below and \$5 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 that our approach is very similar to a simpler method of estimating a regression using state-by-quarter data, where the outcome is number of jobs paying under, say, \$15/hour divided by population. Our employment estimates and standard errors are very similar when using the simpler method, as we discuss below in Section 5 and report in columns (6) and (7) of Appendix Table A.9. However, we do not use this simpler method as our primary specification because it does not allow us to to separately track the missing and excess jobs, or to estimate the effect on wages.

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

[25] 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.

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 Online Appendix B, 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 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. In the same vein, as shown in Online Appendix Figure A.4, the Washington case study results using CPS data are similar to those in Figure 2, which uses administrative data from Washington state. Moreover, when we use $3 bins and 5 years averages, which is the aggregation level that matters for our main estimates, the cross-sectional distributions from the CPS and the administrative data are very similar to each other. Finally, we structurally estimate a model of measurement error in reported wages proposed by Autor, Manning and Smith (2016), and show that the likely contribution of such misreporting error to the overall variance in wages in the CPS and high quality administrative data are very similar. Overall, this confirms that the gains in the accuracy of our bunching estimate from using high quality administrative data through reduced measurement error are likely to be modest.

4.3 Empirical Findings Based on the Event Study Analysis

We begin our analysis by estimating the effect of the minimum wage on the frequency distribution of hourly wages. Figure 3 shows the results from our baseline specification with wage-bin-by-period and wage-bin-by-state fixed effects (see equation 2). We first 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 relative to pre-treatment total employment in the state. Figure 3 highlights that the estimated effects on the wage distribution uncovered from the event study analysis is very similar to the ones that estimated from the Washington case study (see Figure 2, Panel (b)).

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.[26] More than $\frac{3}{4}$ of this reduction occurs in the $1

[26] 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 in Appendix Figure A.5 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. For example, in the Washington state

wage bin just under the new minimum. Second, there is 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) and Dube, Giuliano and Leonard (2015). The excess jobs between the new minimum and $4 above it represents 2.1% (s.e. 0.3%) of the total pre-treatment employment.[27] Finally, Figure 3 also displays the employment changes in the upper tail wage bins, from $5 above the minimum wage to $17 or more (the final bin). These changes 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.

The bunching estimate for employment change adds the missing jobs below and excess jobs above the minimum wage: $\triangle a + \triangle b$. We can divides this change by the jobs below the new minimum wage (b_{-1}) to obtain a change in the affected employentof 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 8.4% 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). Second, using the formula in equation 3 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.[28]

Figure 4 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 with wage-bin-period and wage-bin-state fixed effects. 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

case study, the missing jobs estimate is $\Delta b = -0.046$ or 4.6%, while the number of missing jobs below the new minimum is $b_{-1} = 0.107$ or around 10.7% of state's employment prior to the increase. The difference mostly stems from the rise in wages in the control states where there were no minimum wage changes. Between the pre- and post-treatment periods, the number of jobs in the control states paying below $9 (in 2016 values) decreased by 5.1%, which accounts for the gap between Δb and b_{-1}.

[27] In Appendix Table A.4 we explore using alternative wage windows to calculate the excess jobs. While the estimated excess jobs is slightly lower with using job changes $2 above the minimum wage, the excess jobs are very similar (and so the employment estimates) once we set the upper limit above $2.

[28] As a point of comparison, the Seattle case study by Jardim et al. (2017) find an own-wage employment elasticity of around -3, which is far outside our confidence bounds.

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. 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 the 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.

Figure 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 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 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.

To sum up, there is little indication of a reduction in employment of low-wage workers affected by the policy—even though there is clear evidence that the new minimum wage is binding, and that it raises wages for the affected workforce. Moreover, the impact of the minimum wage is concentrated at the bottom of the wage distribution, while there are no (positive or negative) changes in the upper tail of the wage distribution.

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 3 and 4. 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. Column (7) is a triple-difference specification that controls for state-period fixed effects, thereby taking out any aggregate employment shocks.[29] Column (8) includes interactions of wage bin-by-state fixed effects and state-level average wages of workers with hourly wage greater than \$15 to partial out any state-level wage shocks.[30] Therefore, columns (6) (7) and (8) are the most saturated specifications: whereas column (6) uses geographically proximate areas and time trends to construct finer grained controls, columns (7) and (8) use within-state higher wage groups to account for possible biases resulting from aggregate employment and wage shocks that are correlated with the treatment.

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 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, 7 and 8), 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. The employment elasticity with respect to the minimum wage ranges between -0.016 and 0.031, while the employment elasticity with respect to the wage ranges between -0.449 and 0.523.

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). Notably, the triple-difference specification (column 7) that uses higher wage workers as a control group produces similar estimates (of 0.523) as the baseline specification. In Appendix Table A.6 we further show that modifying this triple-difference specifications by dropping observations from the very top of the wage distributions (i.e., above \$15 or \$20) continues to produce similar estimates and shows no evidence of employment loss.

[29]Note that if the minimum wage increases employment in the upper tail through labor-labor substitution, the triple-difference specification estimate will exaggerate job losses at the bottom. Conversely, if there are employment reductions in the upper tail, this specification will under-estimate the job losses at the bottom. Therefore, finding a divergence between the baseline and the triple-difference specification indicates either the presence of some confounding employment shock, or a causal impact on the upper tail employment.

[30]A positive overall wage shock can reduce employment at the bottom of the distribution while increasing employment higher up in the distribution. However, the overall wage level is at least partly affected by the minimum wage; for this reason, we use the conditional mean wage above \$15, since that is unlikely to be affected by the policy.

Therefore, we find that the bunching estimates from the baseline specification with bin-period and bin-state fixed effects are broadly similar to those from more saturated models shown in Table 1. At the same time, the estimates from the baseline specification are often more precise (especially for the employment elasticity with respect to the wage), and so we will focus on the baseline specification in the sections below.

Effect by event type. In most states, tipped workers can legally receive sub-minimum hourly wages, which might further decrease the effective share of workers impacted by the minimum wage. In column (1) of Table 2, 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.[31] Minimum wage laws are more binding in these states than in others because a sizable portion of low-wage workers are employed as tipped employees, andthese workers are fully bound by the minimum wage changes in states without tip credit. Although the average percentage increase in the minimum wage (9.3%) and the share of workforce earning below the new minimum wage (9.9%) are similar to those in the primary sample of events, 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).

Our analysis so far has used all nontrivial state minimum wage changes, but has excluded federal increases. In the second column of Table 2, 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. As we discussed above, 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.

Effect by different workforce definitions. So far, we have used the employment status of an individual to obtain counts in each wage bin. However, this does not account for part-time versus full-time status, which could be affected by the policy. In column (3) of Table 2, we consider the number of hours employed

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

and estimate the effect of the minimum wage on full-time equivalent (FTE) workers. These estimates are not very different from Table 1. The actual number of FTE jobs below the minimum wage (relative to the pre-treatment employment) is lower ($\bar{b}_{-1}$ =6.7% as opposed to 8.6% in Table 1), indicating that low-wage workers work fewer hours. Consistent with this, missing jobs estimate is also smaller in magnitude when we use an FTE measure (-1.3% instead of -1.8%). The average wage change for affected workers accounting for hours is 7.3% (s.e 1.2%), while the employment change is 4.4% (s.e. 3.3%). After accounting for hours, the employment 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 2, 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 4.2, 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 the bunching estimate for the change in employment ($\Delta a + \Delta b$). Excluding tipped workers reduces the average bite, $\bar{b}_{-1} = 6.1\%$, while the estimate of missing jobs of -1.6% is close to our benchmark estimate of -1.8% in Table 1. Consequently, estimated wage effects are larger by around 20% (8.2% versus 6.8% in Table 1). However, excluding tipping workers has a negligible impact on the employment estimates: the own-wage employment elasticity is 0.337 as opposed to 0.41 in Table 1.

Further robustness checks. 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.[32]

Finally, 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).

4.4 Heterogenous Responses to the Minimum Wage

Besides estimating the overall employment effect for the low-wage workforce, our approach can also provide employment estimates for specific subgroups. In this section we report responses for various demographic groups, sectors, and by labor force status prior to the introduction of the minimum wage. The impact of the minimum wage on these sub-groups may be of direct interest to policy makers. Moreover, understanding heterogenous responses along various margins can provide new insights on how the low-wage labor market operates.

By demographic groups. As we showed in the previous section, we find no indication of substantial employment losses at the bottom of the wage distribution. However, a primary concern with our estimates is that the lack of an employment response could mask a shift in employment from low-skill to high-skill workers.[33] Such labor-labor substitution at the bottom of the wage distribution would make minimum wage policies less attractive even in the absence of an overall employment effect.

In Table 3 we consider the effect of the minimum wage on some low-wage subgroups whose employment prospects are often a primary concern for policy makers. We report estimates for workers without a high school degree, those with high school or less schooling, women, black or Hispanic individuals, and teens using our baseline specification (see equation 2). 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 construct a binary outcome variable that takes on 1 if a hourly worker has a wage less than 125% of the statutory minimum wage, and 0 otherwise. We exactly follow Card and Krueger (1995)'s model specification and employ the following demographic predictors: all three-way interactions of non-white, gender, and teen indicators, all three-way

[32] In Online Appendix C.1 we show that benchmarking the CPS data with the QCEW helps predict the low-wage employment counts in the administrative data from Minnesota, Oregon and Washington more accurately.

[33] For instance, the Teulings (2000) model predicts that the minimum wage induces substitution between various skill types at the bottom of the wage distribution.

interactions of non-white, gender, and age 20-25 indicators; less than high school dummy variable; continuous highest grade completed variable, up to third polynomial labor-market experience variables, Hispanic ethnicity indicator, interactions of the education and experience variables with gender. We fit this prediction model using a training sample that includes all state-quarter observations up to three years prior to the 138 events that also lie outside any of the 5-year post-treatment windows. The model predicts how likely an individual is to be a minimum wage worker. Since the predictors are composed only of demographic variables, we obtain predicted probabilities for all individuals in the sample regardless of their actual employment status. We then use the predicted probabilties to place individuals in three three groups: a "high probability" group that contains individuals in the top 10 percent of the predicted probabilty distribution; a "low probability" group that contains workers in the bottom 50 percent of the predicted probabilities; and a middle group containing the rest. Overall, around 34.8% of minimum wage workers outside of the training sample are in the high probability group, and an additional 49.1% are in the middle group.

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). Restricting by age, gender, and race or ethnicity also exhibits a larger bite than our estimates for the overall population. Teen (-11.4%), women (-2.3%), and black or Hispanic (-2.8%) workers see significant and relatively larger estimates of missing jobs as a share of their pre-treatment employment. Similarly, the high probability group show considerably larger bite (10.0%) than the middle group (2.0%), and the low probability group(0.4%).

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. This is shown in panel (a) of Figure 6 where we plot the relationship between missing jobs below (multiplied by -1) and the 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.

As a result, the employment elasticities with respect to own wage range between -0.086 and 0.570 for the five groups (see Table 3). 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 somewhat more positive than many estimates in the literature, though we note that it is not statistically significant given a standard error of 0.134. Moreover, it is similar to medium and longer term effects found in Allegretto et al. (2017) using a saturated model with controls for division-period effects and state-specific trends (which range between 0.061 and 0.255, as reported in Table 3 of their paper).

The employment elasticites are also very similar across the three Card and Krueger probability groups even as the associated missing jobs (and excess jobs) estimates differ greatly. The similarity of the employment estimates underscores that labor-labor substitution played a limited role at the bottom of the wage distribution. It is also worth mentioning that the most precise estimate for the parameter in this paper appears in column (6) of Table 3, 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. As shown in Online Appendix Figure A.7, the confidence region from this hybrid approach 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 bunching approach advanced in this paper. In contrast, the standard errors for the other Card and Krueger groups are substantially larger, since much fewer workers are located at the bottom of the wage distribution in those groups.[34]

We also assess labor-labor substitution by fully partitioning the population into age-by-education groups. We use 4 education categories and 6 age categories, yielding a total of 23 education-by-age groups.[35] 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. Panel (b) in Figure 6 shows the relationship between missing and excess jobs. 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 ($\Delta a = -\Delta b$).

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 6, the slope of the fitted line is very close to one, with $\hat{\mu}_1 = 1.070$ (s.e. 0.075). The binned scatter plot shows that there is little indication of a more negative slope at higher values of $-\Delta b$. While some specific groups (e.g., individuals with less than high school education between 30 and 40 years of

[34]As shown in Online Appendix Table A.3, when we estimate the effects of the minimum wage on aggregate wage and employment for each of the 3 probability groups, we can establish a causal argument for only the high probability group—since we cannot estimate a statistically significant wage effect for the middle and the low probability groups. This shows the limits of using the Card and Krueger probability groups by themselves, without combining the bunching approach, to estimate the effect on most of the low wage workers exposed to minimum wage changes. In contrast, the our bunching approach focuses on the changes in the frequency distribution for wages around the minimum wage, and is able to estimate wage and employment effects for all low wage workers affected by the policy.

[35]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.

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; the lack of a reduction in low-wage jobs does not appear to be driven by labor-labor substitution at the bottom of the wage distribution.

By industrial sectors. Our bunching method also allows us to provide a comprehensive assessment of the effect of the minimum wage across industries. In much of the literature, only specific sectors (like restaurants) have been studied because the policy is much more binding in these industries than in the whole economy, and it is therefore easier to detect a clear effect on the average wage. In contrast, the bunching approach, which tracks employment changes at the bottom of the wage distribution, can recover employment and wage responses in industries where only a small fraction of workers are directly affected by the minimum wage increase.[36]

In Table 4 we report estimates for tradable, non-tradable, construction, and other industries. We follow Mian and Sufi (2014) in classifying industries into these four categories.[37] 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 4. The estimated employment and wage effects on this restricted sample are similar to the full 1979-2016 sample.

Column (2) shows the effect of the minimum wage in the tradable sector.[38] The minimum wage is less binding in that sector, which is reflected in both the level ($\bar{b}_{-1}$) and the change in the number of jobs below the new minimum wage ($\triangle b$). The number of excess jobs at and 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. Our estimates on wages are 5.8%, (s.e. 7.3%), so the employment elasticity with respect to own wage is large in magnitude at -1.910 (s.e. 3.922) but imprecisely estimated.

Column (3) highlights that minimum wage is more binding in the non-tradable sector, where the missing jobs is -6.6% (s.e. 0.7%) and more than a quarter of the jobs (27%) are below the new minimum wage for an average event. Moreover, we find that the employment effects are positive, and the employment elasticity with respect to own wage is 0.387 (s.e. 0.597). This 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,

[36] Even a small fraction of workers can cover many workers if a sector is large. Therefore, having a small fraction of workers earning near the minimum wage does not necessarily mean that responses in those industries are not relevant for understanding the overall impact of minimum wage.

[37] 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-related industries; and the remaining industries fall into the "other" category. 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%, and 57% for tradable, non-tradable, construction, and other sectors, respectively. See more details in Online Appendix B.

[38] For the industry specific estimates in columns (2)-(8) we benchmark the CPS data with quarterly state-industry level employment from the QCEW (see Online Appendix B).

using revenue data, that the larger job losses for tradables reflect a more elastic consumer demand in that sector.

In column 4, we find no indication that minimum wage increases are binding in the construction sector: both the jobs below the new minimum wage ($\bar{b}_{-1}$) and the missing jobs ($\triangle b$) are close to zero. For the remaining industries in the "other" category in column (5), the bite of the minimum wage is statistically significant but is somewhat smaller than the estimates with all industries (Δb equal to -1.1% versus -1.9% in column 1). Moreover, the missing job count is fully offset by the excess job count—producing slightly positive (but statistically insignificant) own-wage employment elasticity of 0.166 (s.e. 0.763).

We also present separate results for the retail, restaurant, and manufacturing sectors. Column (6) shows that the missing jobs estimate, Δb, for the restaurant sector is -10.1% (s.e. 1.5.%)—the largest such estimate among the sectors studied here. The bite of the minimum wage in restaurants explains why this industry is studied so frequently in the literature. Moreover, excess jobs are similar in size to the missing jobs (Δa = 10.1%) and so there is little net change in restaurant employment. These small effects agree with other recent work that find little to no employment effects for restaurant workers overall (Neumark, Salas and Wascher 2014; Allegretto et al. 2017). However, different from most prior studies that look at overall restaurant employment, our estimates show that the effect specifically on low-wage restaurant employment is also small. For the retail sector, in column (7), we also find no indication of employment losses, with an employment elasticity with respect to own wage of 1.040 (s.e. 1.058). In contrast, in the manufacturing sector in column (8), the employment reduction in response to the minimum wage is similar in magnitude to the tradable sector: the point estimate 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 at -1.385 (s.e. 2.956), though these estimates are imprecise and statistically insignificant.

In summary, our point estimates are consistent with more negative employment effects in the tradable than in the non-tradable sector, although many of the tradable sector estimates are imprecise. While these results suggest some adverse consequences for tradable industries like manufacturing, they are of limited consequence for most workers earning near the minimum wage. Around 48 percent of the workers below the new minimum wage are employed in the non-tradable sector (which includes restaurant and retail industries); another 40 percent of the minimum wage workers are in "other" industries. In both of these categories (encompassing around 88 percent of minimum wage workers) we find clear evidence of hourly wage increases, but no evidence of negative employment effects.

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. This decomposition of total employment changes may of interest on it is own if policy makers value the employment prospects of the two groups differently. Moreover, this analysis allows us to directly test whether minimum wage laws affect employment primarily through reduced job creation as suggested by Meer and West (2016).

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.[39] We define incumbent workers to be those wage earners who were working one year prior the current period, and define new entrants to be wage earners who were were not employed one year ago. The partition limits our sample to the 1980-2016 time period, covering 137 eligible minimum wage-raising events. Because the CPS interviews are 12 months apart, we do not observe the pre-treatment employment status for workers when we consider periods more than a year after the minimum wage increase. Therefore, for these estimates we also restrict our time window to 1 year around the minimum wage increase, rather than the five years in our baseline sample.

Figure 7 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. We report the immediate effect of the minimum wage hike, α_{0k}, for each dollar wage bin k. The figure highlights that for both subgroups, new minimum wages clearly bind, with significantly fewer jobs just below and significant more at the new minimum. The missing jobs estimate is larger for incumbents (-1.3%, s.e. 0.2%) than for the previously non-employed (-0.5%, s.e. 0.1%). However, 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 employment in the upper tail. 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.

To sum up, we find no evidence that the employment responses differ substantially between new entrants and incumbents, at least in the short run. Nevertheless, since we detect clear changes in the missing and excess jobs for new entrants, studies that focus only on incumbent workers will at best provide a partial characterization of the full effects of the minimum wage increase. Our bunching approach therefore extends prior work that restricts its sample to workers earning positive wages prior to the minimum wage increase (Abowd et al. 2000; Currie and Fallick 1996; Clemens and Wither 2016).

[39]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 Online Appendix B explains how we match workers across rotation groups.

4.5 Wage spillovers

One key advantage of estimating the impact of minimum wages on the wage distribution is that we can directly assess the size and scope of wage spillovers (or ripple effects) of the minimum wage. These spillovers are important to understand the impact of the minimum wages on wage inequality and to learn about the economic mechanisms operating in low-wage labor markets.

As we pointed out earlier, the effect of the minimum wage on the wage distribution in Figure 2 for the Washington case study and in Figure 3 for the pooled event study analysis clearly indicates the presence of some wage spillovers. For instance, Figure 3 shows employment increases in wage bins that are \$1 to \$3 higher than the new minimum wage. These spillover effects fade out by \$3 above the new minimum wage, which on average is around the 23^{rd} percentile of the wage distribution. These results are very much in line with Autor, Manning and Smith (2016), who also find evidence of positive wage spillovers that decline rapidly and are effectively zero at around the 25^{th} percentile.

In this section, we quantify the size of the spillover effect by comparing the average wage increase to the increase that would occur in the absence of spillovers. We calculate the "no spillover" wage increase by moving each missing job under the new minimum wage exactly to the new minimum wage:

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

The total wage increase of affected workers, $\%\Delta w$, in equation 3 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.

We report our estimates of wage spillovers in Table 5, 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 $\frac{\%\Delta w - \%\Delta w_{\text{no spillover}}}{\%\Delta w_{\text{no spillover}}}$. 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.

These estimates meaningfully address some complications in the prior literature on wage spillovers. Earlier research (Card and Krueger 1995; DiNardo, Fortin and Lemieux 1996; Lee 1999; Autor, Manning and Smith 2016) documented the existence of spillovers by estimating changes in the density of wages. However, focusing on the density raises the possibility that some of the measured spillover is an artifact of disemployment truncating the wage distribution. In contrast, our approach does not suffer from this limitation since it focuses on the frequency distribution of wages—allowing us jointly estimate the effect of the minimum wage on employment and the distribution of wages.

In Table 5 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. We find a substantially smaller change in wages due to spillovers for the tradable sector, where the total affected worker wage increase (5.8%) is somewhat smaller due to the increase one would expect if all missing jobs moved up to the new minimum wage (6.5%).

We also find a stark difference in the spillover shares of wage increases for incumbents and 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 7.

These estimates provide some new insights into the economic mechanisms behind the wage spillovers. First, we can directly address whether spillovers are real or whether they only reflect measurement error in CPS-based wages, a possibility that is raised by Autor, Manning and Smith (2016). As we discussed, we find similar pattern of spillovers in Washington using administrative data from that state (see Figure 2), which suggests that the spillovers are not primarily caused by CPS-specific misreporting by survey respondents.[40] Moreover, 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, and suggest that at least some of the measured spillover increases are real.

Second, the standard labor demand model with heterogenous workers can explain wage spillovers only through substantial substitution away from lower-paid towards slightly higher-paid workers. Such a mechanism is inconsistent with our findings on: a lack of employment effect at the bottom of the wage distribution, a lack of labor-labor substitution between lower-wage groups along observable dimensions, and a lack of responses in the upper tail of the wage distribution. This suggests that spillovers are likely to reflect some frictions in the labor market.

[40] In the Online Appendix B we also assess the extent of misreporting error using the method developed by Autor, Manning and Smith (2016) and we show that misreporting is not substantially different in the CPS and administrative data.

What type of frictions are consistent with the observed spillovers? 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 relative-pay norms inside the firm. This is consistent with findings in Dube, Giuliano and Leonard (2015), who study the wage adjustment using payroll data from a major retailer following the 1996-1997 federal minimum wage increase and find that worker separations respond to relative pay differences.

4.6 Using event-specific estimates to assess heterogeneity of minimum wage effects

So far, most of our evidence has come from averaging the effects across all 138 events. However, one concern with minimum wage studies in the U.S. is that many increases are small, affecting only a small number of workers which might make it difficult to detect employment effects (e.g. see Sorkin (2015)). In this section, we estimate treatment effects for each of the events separately, and assess how this impact varies when we consider minimum wage increases that are more binding.

We begin by constructing event-specific estimates of excess (Δa_j) and missing (Δb_j) jobs for each event j using the pooled regression estimates and residuals from equation 2. We do so by adding the fitted value of the excess and missing jobs (Δa and Δb) to the bin-specific residuals averaged over the appropriate wage bins:

$$\Delta a_j = \Delta a + \frac{\overline{u}_j^a - u_{-1,j}^a}{EPOP_{-1,j}}$$

$$\Delta b_j = \Delta b + \frac{\overline{u}_j^b - u_{-1,j}^b}{EPOP_{-1,j}}$$

Here $\overline{u}_j^a$ is the sum of the residuals in the five 1-dollar wage bins between the new minimum wage MW_j and \$5 above it, averaged over the post-treatment window, while $u_{-1,j}^a$ is the sum of the residuals in the same five 1-dollar wage bins during the 1 year prior to the minimum wage change. The normalization by $EPOP_{-1}$ and the netting out of the 1-year lead follow the same procedure used to construct the average response Δa. We construct the missing jobs estimates, Δb_j, in an analogous fashion.[41]

Finally, we construct the event-specific employment change as the sum of the excess and missing jobs, i.e., $\Delta e_j = \Delta a_j + \Delta b_j$. We note that these event-specific estimates are numerically equivalent to estimating

[41] Formally, we can write the estimates as:

$$\Delta a_j = \Delta a + \frac{1}{EPOP_{-1,j}} \left(\frac{1}{5} \sum_{\tau=0}^{4} \sum_{k=0}^{4} \left(I_{swt}^{\tau jk} \cdot u_{swt} \right) - \sum_{k=0}^{4} \left(I_{swt}^{-1jk} \cdot u_{swt} \right) \right)$$

event-by-event regressions for each event j, where the effects for other events $-j$ are assumed to be the same as those from the pooled regression.

Armed with these event-specific estimates, we evaluate how they vary with the effective level of the minimum wage. A standard measure of this effective level is 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) We calculate the $Kaitz$ index for each event using the new minimum wage MW and the median wage at the time of the minimum wage increase, $Kaitz_j = \frac{MW_j}{\text{Median wage}_j}$.[42]

We regress the missing jobs Δb_j, excess jobs Δa_j, as well as employment change Δe_j on $Kaitz_j$, respectively, additionally controlling for several other possible sources of heterogeneity, including the 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.[43] The key findings are shown in Figure 8, which shows binned scatter as well as linear regression fits for the three outcomes as a function of the minimum-to-median wage ratio.[44] 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 with a higher minimum-to-median wage ratio had substantially more missing jobs — the coefficient on $Kaitz_j$ is sizable and statistically significant at -0.136 (s.e. 0.032).[45] At the same time, when we consider excess jobs, we find that the coefficient on $Kaitz_j$ has the same magnitude at 0.136 (s.e. 0.048). 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

$$\Delta b_j = \Delta b + \frac{1}{EPOP_{-1,j}} \left(\frac{1}{5} \sum_{\tau=0}^{4} \sum_{k=-4}^{-1} \left(I_{swt}^{\tau jk} \cdot u_{swt} \right) - \sum_{k=-4}^{-1} \left(I_{swt}^{-1jk} \cdot u_{swt} \right) \right)$$

for the set of s, t that comprise event j. Here $I_{swt}^{\tau jk}$ is the j^{th} event-specific set of treatment dummies, and u_{swt} are the regression residuals from equation 2.

[42] Using the Kaitz index is appropriate only if wage spillovers are modest and the median wage is not affected by the minimum wage. In Section 4.5 we show that this is indeed the case. Moreover, Autor, Manning and Smith 2016 also find that spillovers fade out by the 25th percentile in the U.S. context.

[43] Because individual events sometimes are based on a variable number of underlying worker-level observations, they are likely to have very different sampling variances and hence noise-to-signal ratios. To account for this, we use a bootstrap-based approach as in (Kinsler, 2016) to estimate event-specific weights, which are then used in the regression of Δb_j, Δa_j and Δe_j on $Kaitz_j$ and other covariates. In particular, we draw 250 bootstrap samples of worker level datasets stratified by state and quarter. We aggregate these into binned datasets as in our primary analysis, estimate the regression equation 2 and construct $\Delta a_{jm}, \Delta b_{jm}$ and Δe_{jm} for each replicate m and event j. We then calculate event-specific variances $\sigma^2_{j,\Delta e}$ for the employment change, and define event-specific weights as the inverse of this variance. In Appendix Table A.8, we also show the impact of using unweighted estimates as well as using population-based weights. These produce similar results, but tend to be somewhat less precise than using the inverse-variance weighting. Finally, Washington D.C. has a very small number of worker level observations and its estimates are extreme outliers. Inclusion of D.C. makes little difference when we pool estimates, but as shown in Appendix Table A.8, these outliers are influential for the relationship between the $Kaitz$ and the employment effect. For this reason, we exclude the events from D.C. in our main event-specific analysis. However, estimates including D.C. are reported in the Appendix Table A.8; these tend to suggest a somewhat more positive relationship between the $Kaitz$ and employment effect.

[44] An analogous figure without any controls is quite similar, as shown in Appendix Figure A.8. We also show the raw scatter plots in Appendix Figure A.9.

[45] This is also consistent with the fact that $Kaitz_j$ is highly correlated with the share of workers below the new minimum wage (b_{-1j}), as shown in Table 6, columns 1 and 2.

employment effect is virtually unchanged as we consider minimum wages that range between 37% and 59% of the median wage, as shown in the bottom panel of Figure 8.

These conclusions are reinforced by additional analysis presented in Table 6. Leaving out the controls (including the state-level unemployment rate at the time of the minimum wage increase, political orientation of the state, urban share of the state[46], and the decade of the minimum wage increase) does little to change the relationships between the *Kaitz* and excess jobs, missing jobs, or the change in employment.[47] 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. At the same time, our sample includes only the early phases of some minimum wage increases (like in California) which are likely to reach around 65% of the median wage over the next few years. Our approach offers a transparent way to track the missing and excess jobs from these policies for more elevated minimum wages, and can help us better understand how high the minimum wage can go without inducing substantial job losses.

5 Employment Changes along the Wage Distribution in the Classic Two-Way Fixed Effect Regression on log 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 we found no indication of considerable employment changes in the upper tail of the wage distribution (see Figure 3). The lack of responses $4 above the minimum wage or higher also implies that the effect of 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). To illustrate the advantage of examining the impact of the minimum wage on the

[46]The urban share is calculated using 2010 Census share of workers in urban areas for each state, while a state is defined as being Republican-leaning based on its 2-party vote share in the past 7 presidential elections.

[47]The estimates from the other control variables do not indicate substantial heterogeneities in the overall employment effect; there is a slight positive effect of unemployment, but this is quite small in magnitude. Urban share, whether the state is Republican, or the decade of the minimum wage change have no statistically significant impact on the changes in missing jobs, excess jobs, or employment.

wage distribution, we decompose the classic two-way fixed effects estimate of log minimum wage on the state level employment-to-population rate. In Figure 9 we divide 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 3 annual leads of log minimum wage, along with state and time fixed effects. This distributed lags specification is similar to those used in numerous papers (e.g., Meer and West 2016, Allegretto et al. 2017).[48] The histogram bars show the average post-treatment effect divided by the sample average employment-to-population rate.[49] The bars, therefore, represent the average response of employment over the post-treatment period in each wage bin with respect to the minimum wage, which added together produces the minimum wage elasticity of overall employment over the post-treatment period. The error bars show the confidence intervals where standard errors are clustered by state. To assess how wage-bin level employment changes add up to the total, the dashed purple line also 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 9 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 in panel a) accrues almost entirely in wage bins exceeding \$15/hour.

It strikes us as implausible that a minimum wage increase in the \$8 to \$9/hour range causally leads to losses mostly for jobs at or above the median wage, even though the minimum wage is binding far lower in the wage distribution. More plausibly, this suggests that minimum wage changes were correlated with negative employment shocks in the upper part of the wage distribution (possibly much earlier than the actual treatment dates), and these confounding shocks were not fully absorbed by the simple two-way fixed effect specifications estimated using a long panel. Online Appendix D provides additional evidence confirming the likely bias in the TWFE-logMW estimate. For instance, the negative employment effects shown in Figure 9 are mainly driven by pre-treatment shocks. Moreover, the negative employment changes arise only for the Card

[48] 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 it also closer to our event study estimates presented in Section 4. However, as we show in the Online Appendix Figure A.10 and Online Appendix Table A.9, the unweighted estimates are similar.

[49] 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.

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. In addition, including state-specific trends or controlling for historical industry and occupation shares produces estimates close to zero. Finally, the negative estimates arise only by including the sample prior to 1990, a period with very few minimum wage increases. These and other findings clarify that the negative estimate in the TWFE-logMW is driven by upper tail shocks that are predicted by a state's historic industry and occupational structure during the 1980s—substantially prior to most minimum wage increases we study. 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 when there is a long pre-treatment period through the biased estimation of the state fixed effects.

The above example illustrates how 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. Since it is unrealistic that minimum wages cause large negative disemployment effects in the upper tail of the wage distribution, the findings suggested that the classic two way fixed effect estimates were likely severely biased in this particular case—which we were able to confirm using additional tests. This type of model selection tool can be particularly helpful in the context of minimum wages, where the academic literature has oftengrappled with figuring out the "right" empirical model.

6 Discussion

We propose a novel approach that infers 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 implement the proposed method in two steps. First, combining the analysis based on a prominent minimum wage increase in the state of Washington with an event study analysis exploiting 138 minimum wage increases, we provide a robust and comprehensive assessment of how minimum wage increases 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. Overall, these findings suggest 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 surpasses the ones studied here remain an open question.

The key advantage of tracking the job changes throughout the wage distribution is that we can transparently show the source of 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. The standard frictionless model of labor demand can reconcile the bunching at the minimum wage if substitution across various types of labor is low, but has difficulties generating substantial ripple effects that are concentrated within a few dollars above the minimum wage. While in principle these spillovers could reflect measurement error, our findings suggest that this is unlikely to be the primary explanation, since similar spillovers are also found when we use administrative data, and since the spillovers seem to be present primarily for incumbent workers and not for new entrants. Therefore, our findings suggest that the presence of spillover effects are likely to reflect some frictions at the labor market. While understanding the nature of these frictions 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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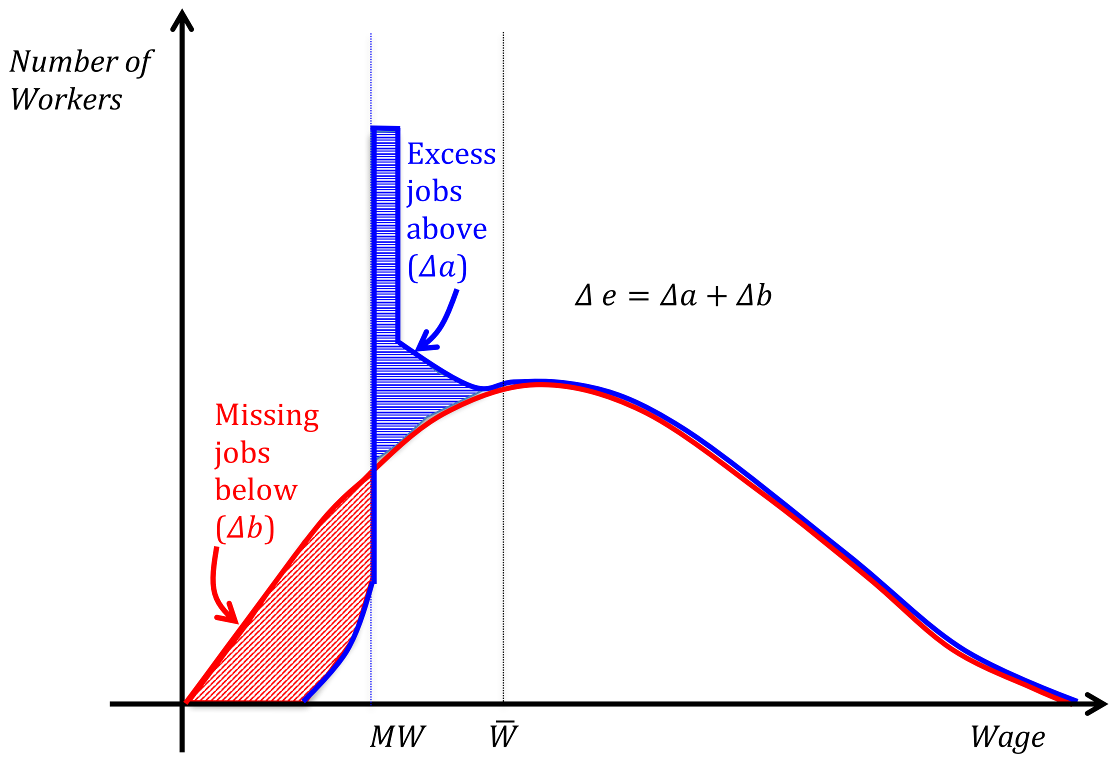
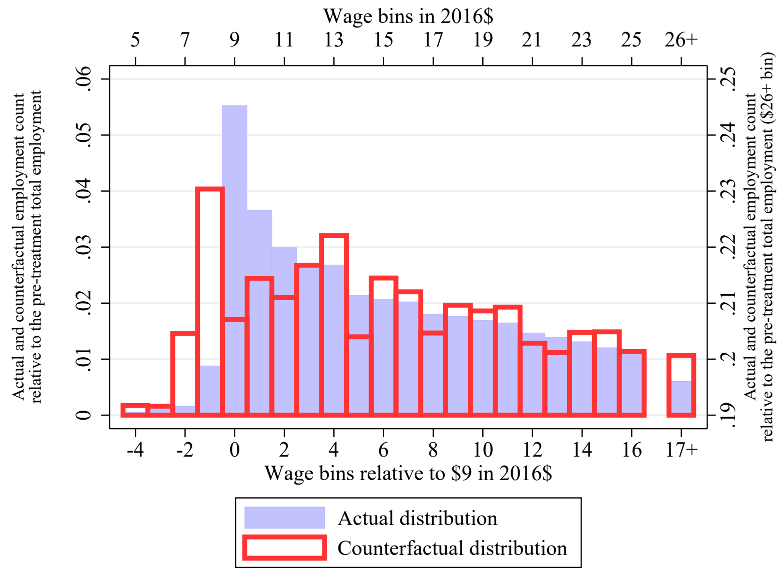
Figure 1: An Illustration of the Bunching Approach

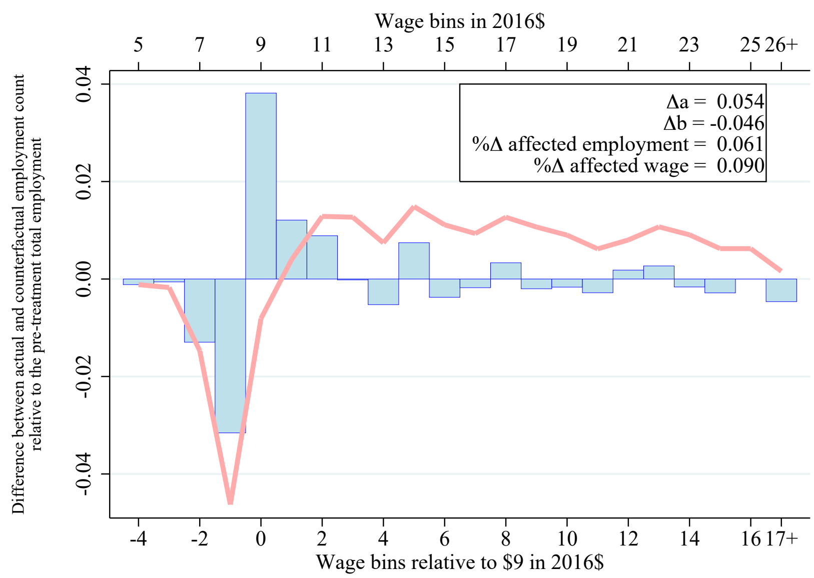


Notes: The figure shows the effect of the minimum wage on the frequency distribution of hourly wages. The red solid line shows the wage distribution before, and the blue solid line after the introduction of the minimum wage. Since compliance is less than perfect, some earners are uncovered and the post-event 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: Employment by Wage Bins in Washington between 2000-2004



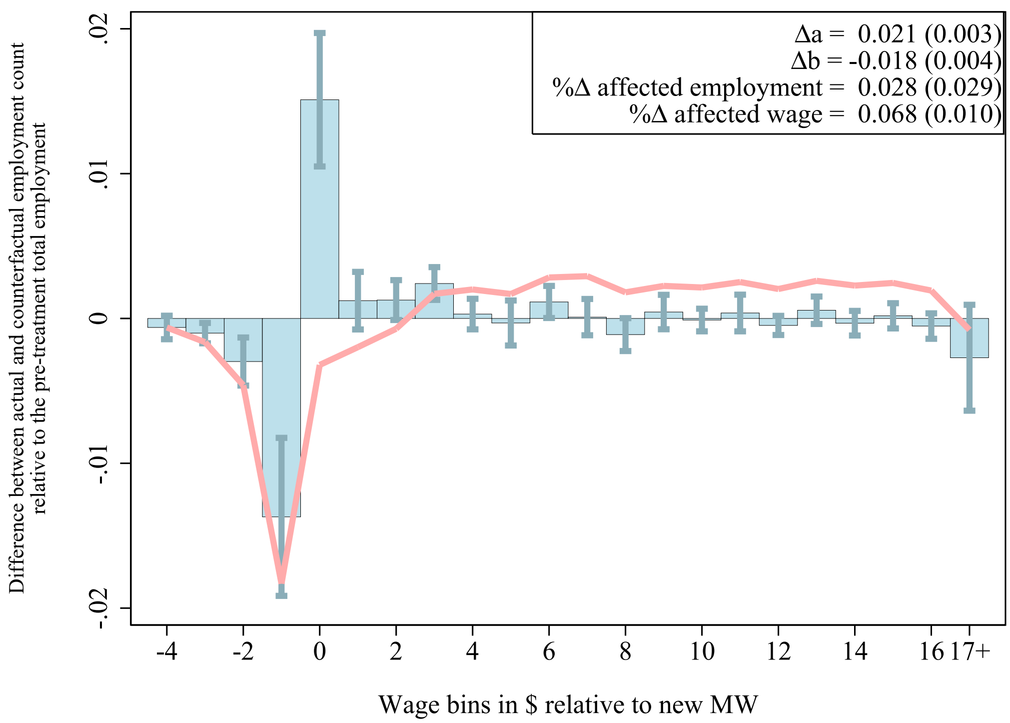
(a) The actual and counterfactual frequency distribution of wages



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

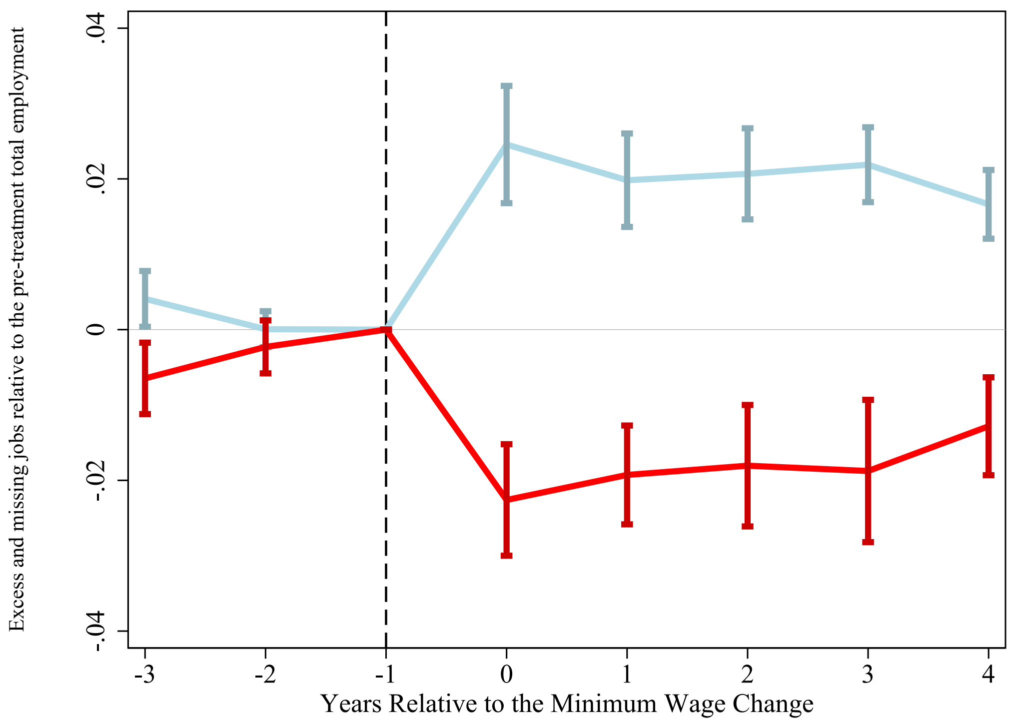
Notes: We examine the effect of the 1999-2000 minimum wage change in Washington state on the frequency distribution of wages (aggregated in $1 bins), normalized by the 1998 level of employment in Washington. The minimum wage was raised from $7.51 to $9.18 (in 2016 values) and it was indexed by inflation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

Figure 3: Impact of Minimum Wages on the the Wage Distribution (Pooled Event Study Analysis)



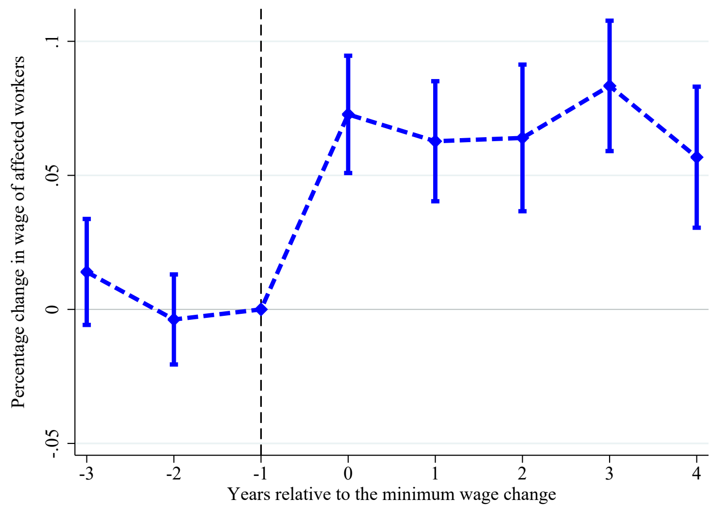
Notes: The figure shows the main results from our event study analysis (see equation 2) exploiting 138 state-level minimum wage changes between 1979-2016. The blue bars show for each dollar bin (relative to the minimum wage) the estimated average employment changes in that bin during the 5-year post-treatment 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 4: Impact of Minimum Wages on the Missing and Excess Jobs Over Time (Pooled Event Study Analysis)

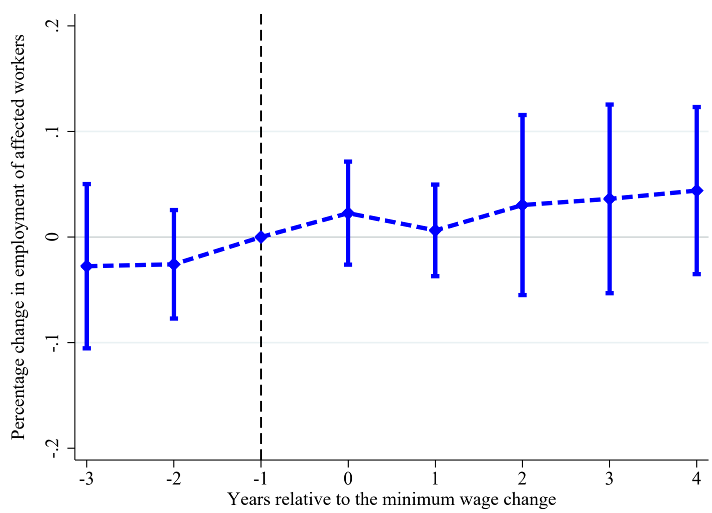


Notes: The figure shows the main results from our event study analysis (see equation 2) exploiting 138 state-level minimum wage changes between 1979-2016. The figure shows the effect of a minimum wage increase on the missing jobs below the new minimum wage (blue line) and on the excess jobs at and slightly 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 5: Impact of Minimum Wages on Average Wage and on Employment Over Time (Pooled Event Study Analysis)



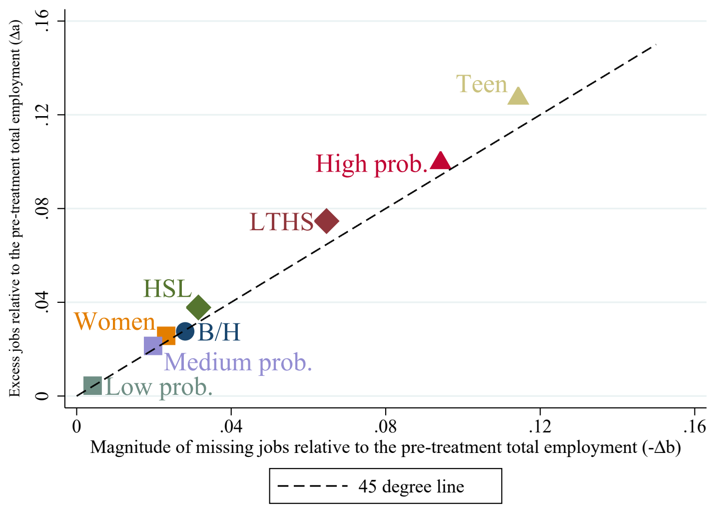
(a) Evolution of the average wage of the affected workers



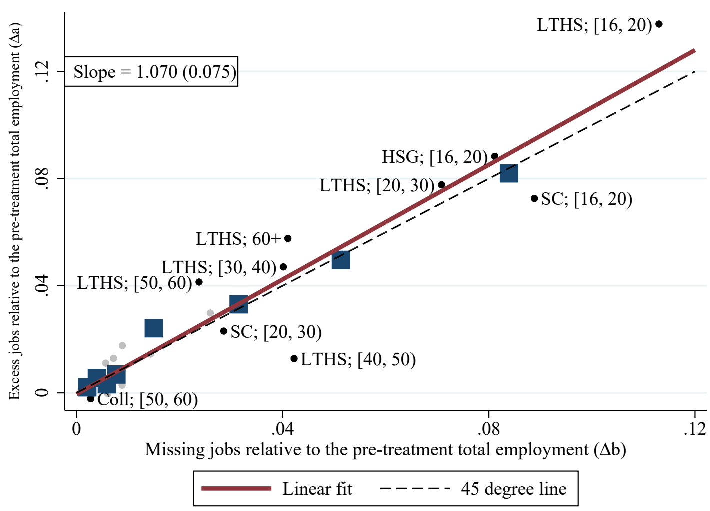
(b) Evolution of the employment of the affected workers

Notes: The figure shows the main results from our event study analysis (see equation 2) 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 3. 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 equals 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 signficant effect on the average wage of the affected population, but there is no sign of significant disemployment effects.

Figure 6: Impact of the Minimum Wage by Demographic Groups



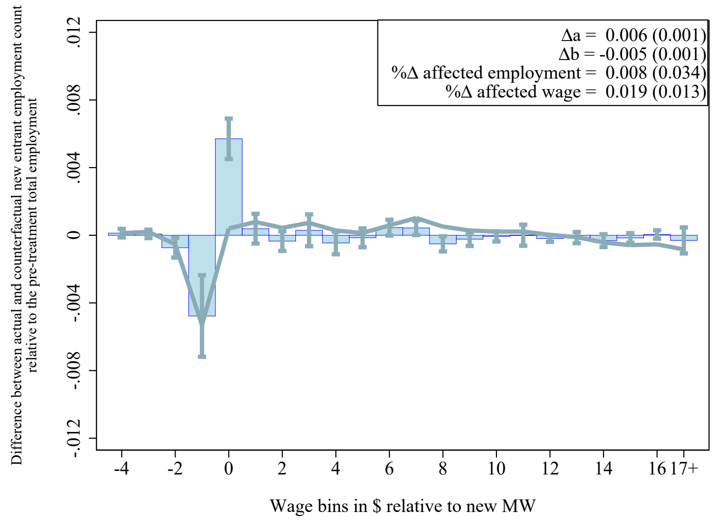
(a) Effect of the minimum wage by demographic groups



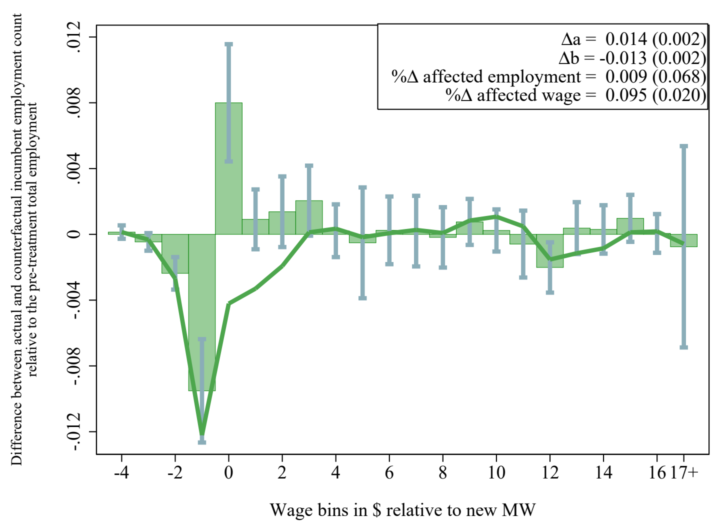
(b) Effect of the minimum wage by age-education groups

Notes: Both figures shows 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 3: 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 missing jobs (Δb). 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 7: Impact of Minimum Wages on the Wage Distribution by Pre-Treatment Employment Status: New Entrants and Incumbents (Pooled Event Study Analysis)



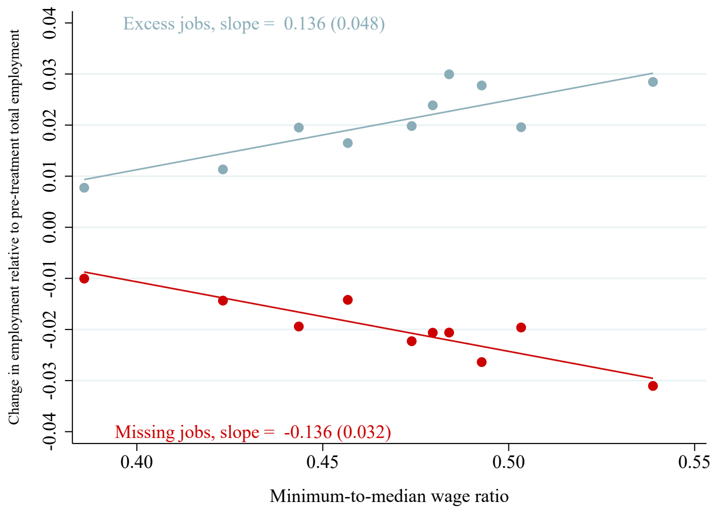
(a) New entrants



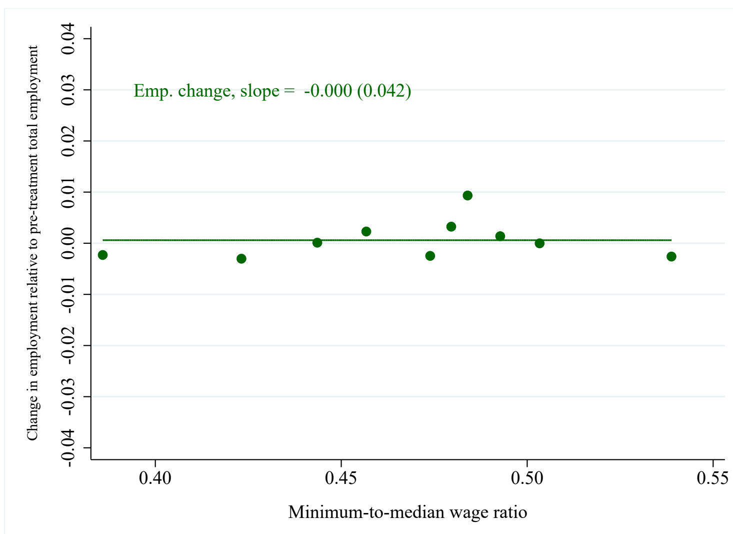
(a) Incumbents

Notes: The figure shows the main results for new entrants (panel a) and for incumbents (panel b) from our event study analysis (see equation 2) 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 8: Relationship between Excess Jobs, Missing Jobs, Employment Change and the Minimum-to-Median Wage Ratio Across Events



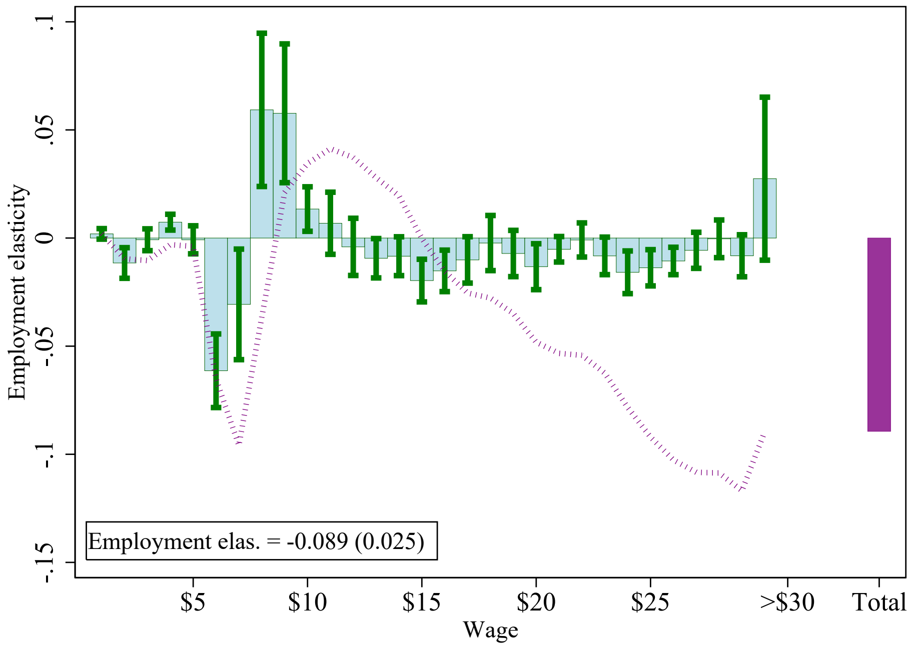
(a) Missing and excess jobs



(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. 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 binscatters 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 event-specific inverse variance of the employment change estimate using the bootstrap procedure described in the text. 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 9: 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 and first differences specifications. 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 a particular wage bin. The rightmost purple bar in each of the graphs is the elasticity of the overall state employment-to-population 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 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)	(8)
Missing jobs below new MW (Δb)	-0.018***	-0.018***	-0.018***	-0.016***	-0.016***	-0.015***	-0.018***	-0.018***
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)
Excess jobs above new MW (Δa)	0.021***	0.018***	0.020***	0.016***	0.014***	0.015***	0.021***	0.018***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
%Δ affected wages	0.068***	0.057***	0.068***	0.049***	0.043***	0.050***	0.069***	0.058***
	(0.010)	(0.010)	(0.012)	(0.010)	(0.010)	(0.011)	(0.009)	(0.010)
%Δ affected employment	0.028	0.000	0.022	-0.002	-0.019	-0.000	0.036	0.000
	(0.029)	(0.023)	(0.021)	(0.021)	(0.021)	(0.023)	(0.048)	(0.026)
Employment elasticity w.r.t. MW	0.024	0.000	0.019	-0.001	-0.016	-0.000	0.031	0.000
	(0.025)	(0.020)	(0.018)	(0.018)	(0.018)	(0.019)	(0.041)	(0.022)
Emp. elasticity w.r.t. affected wage	0.411	0.006	0.326	-0.032	-0.449	-0.003	0.523	0.008
	(0.430)	(0.402)	(0.313)	(0.439)	(0.574)	(0.455)	(0.676)	(0.446)
Jobs below new MW ($\bar{b}_{-1}$)	0.086	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	0.101
Number of events	138	138	138	138	138	138	138	138
Number of observations	847,314	847,314	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	4,694,104	4,694,104
Controls								
Bin-state FE	Y	Y	Y	Y	Y	Y	Y	Y
Bin-period FE	Y	Y	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-period FE							Y	
Bin-state upper tail wage controls								Y

Notes. The table reports the effects of a minimum wage increase based on the event study analysis (see equation 2) 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 triple difference specifications where we control for state-by-period fixed effects. Column (8) controls for state-level wage shocks by interacting wage-bin-by-state specific effects and state-level average wages of workers with hourly wages more than $15. 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 3 in Section 4.1. 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 qurater-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: Robustness of the Impact of Minimum Wages to Alternative Workforce, Treatment and Sample Definitions

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

Notes. The table reports robustness checks for the effects of a minimum wage increase based on the event study analysis (see equation 2) 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. 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 3 in Section 4.1. 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 qurater-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 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 2) 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 3 in Section 4.1. 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 qurater-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: 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 2) 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 Online Appendix part C 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 3 in Section 4.1. 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 qurater-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 5: 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 2) 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 3 in Section 4.1, and the second column assumes no spillovers (see equation 4 in Section 4.5). 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.

Table 6: 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)
Minimum-to-median ratio	0.302***	0.341***	–0.121***	–0.136***	0.134***	0.136***	0.013	–0.000
	(0.041)	(0.045)	(0.031)	(0.032)	(0.042)	(0.048)	(0.038)	(0.042)
Unemployment rate		–0.000		0.001		0.001		0.003**
		(0.001)		(0.001)		(0.001)		(0.001)
Urban share of population		0.050**		–0.018		0.015		–0.003
		(0.021)		(0.014)		(0.018)		(0.017)
Decade = 1990		0.003		–0.004		0.016**		0.012
		(0.009)		(0.006)		(0.008)		(0.009)
Decade = 2000		–0.003		–0.002		0.013**		0.011
		(0.006)		(0.006)		(0.006)		(0.008)
Decade = 2010		0.002		–0.004		0.013**		0.009
		(0.006)		(0.006)		(0.006)		(0.008)
Republican state		–0.004		–0.002		–0.004		–0.005
		(0.006)		(0.004)		(0.007)		(0.008)
Constant	–0.059***	–0.116***	0.037***	0.055**	–0.042**	–0.075***	–0.005	–0.020
	(0.019)	(0.031)	(0.014)	(0.022)	(0.020)	(0.029)	(0.018)	(0.025)
Number of observations	130	130	130	130	130	130	130	130

Notes. The table reports the effect of the minimum-to-median wage ratio and other covariates on four outcomes: jobs below the new minimum wage ($\bar{b}_{-1}$), missing jobs (Δb), excess jobs (Δa), and the total employment change (Δe). The minimum-to-median wage ratio is the new minimum wage divided by the state-level median wage. The sample of 130 events excludes 8 minimum wage increases in the District of Columbia, since individual treatment effects are very noisily estimated for those events. Regressions are weighted by event-specific inverse-variances. Robust standard errors are in parentheses; significance levels are * 0.10, ** 0.05, *** 0.01.

Online Appendix A Additional Figures and Tables

Figure A.1 shows all minimum wage increases between 1979 and 2016. We use the time series of state-level minimum wage changes from Vaghul and Zipperer (2016). Blue circles shows the minimum wage events that are used in the pooled 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 only the change in missing number of jobs, Δb, is identified 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 half of the minimum wage changes in our sample occurred after 2000.

Figure A.2 shows the evolution of the actual per capita job counts between \$5 and \$8 (solid red line) and \$9 and \$13 in Washington (solid blue line), using \$1 bins. We also plot the counterfactual job counts (dashed line) based on the average job counts among the 39 states that did no experience any minimum wage change during the 1998-2004 period. The figure highlights that the job counts in the counterfactual wage distribution closely follows the actual wage distribution before 1998. After 1998 there is a larger drop in the actual number of jobs paying below the \$9 minimum wage than in the counterfactual. The difference between these two lines constitutes the number of "missing jobs" between \$5 and the new minimum wage (\$9.18 in 2000). At the same time, the actual number of jobs between \$9 and \$13 is higher than at the counterfactual one. This difference is the number of "excess jobs." The figure also shows that the number of jobs between \$5 and \$13 in Washington fell sharply after 2000, which reflects the 2001 recession. As a result, a simple before-after comparison of the number jobs below and above the new minimum wage in Washington would lead to a misleading conclusion. On the other hand, the comparison to the counterfactual wage distribution takes into account the effect of the recession on job counts. This example demonstrates the main advantage of using a difference-in-difference style estimator when we identify the missing and excess jobs.

Figure A.3 panel (a) shows the impact of the minimum wage changes on excess jobs and missing jobs relative to the counterfactual wage distribution in Washington over time. The phased minimum wage increases in 1999 and 2000 created a large drop in the number of jobs paying just below the minimum wage (red line) and an equally sized increase in jobs paying between \$9 and \$13. The figure highlights that the changes in missing and excess jobs correspond closely to the timing of the minimum wage change. Panel (b) shows the change in employment below \$13, which is the sum of excess jobs and missing jobs shown in panel (a). There is no indication of a disemployment effect in Washington after 2000.

Figure A.4 panel (a) shows employment by wage bins for the actual (purple filled bars) and the counterfactual (red empty bars) distributions; panel (b) shows the difference between the two. This figure is constructed in the same way as Figure 2, except here the the actual wage distribution is calculated using benchmarked CPS data instead of administrative data. The pattern in Figure A.4 is very similar to our baseline results in Figure 2. Both the missing jobs and excess jobs are slightly lower when we use CPS data, but the affected employment estimates are similar (around 10% here versus 6% in Figure 2). This figure demonstrates that the results based on CPS data are similar to those obtained using high quality administrative data.

Figure A.5 shows the effect of the minimum wage on the wage distribution when we take into account that sometimes minimum wage increases are phased in over multiple events. In 65% of the cases we study, a primary minimum wage increase is followed by a secondary one within 5 years, on average at $0.56 above the minimum for the primary event. In contrast to the main results of the paper, where we show the partial effect of each event, here we show the cumulative effect of both primary and secondary events by taking into account the incidence and size of secondary increases averaged across our sample of events. The cumulative effect of primary and secondary events on missing jobs is 2.5%, which is larger than the partial effect of the primary events, which is 1.8% (see Figure 3). 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.

Some wages in the CPS are imputed. In most of our analysis we only use 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.6 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.

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 is 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 1 in Table 3. 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.

Figure A.8 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 8 where we do control for observable characteristics including as urban share, decade dummies and whether the state leans Republican.

Figure A.9 show 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 pooled 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 8 and in Figure A.8. However we keep those events in the rest of the paper where we report the pooled event study estimates.

Figure A.10 shows the impact of minimum wages on the wage distribution in weighted and unweighted fixed effects and first difference specifications. Panel (a) reports Figure 9 from the main text estimated using (level) fixed effects. Panel (b) reports the unweighted version of Figure 9, also using fixed effects. Panels (c) and (d) report weighted and unweighted estimates, but now estimated using the first difference instead of the fixed effects estimator. Contrary to the fixed effects estimates, the first difference specifications shows more muted employment responses in the upper tail of the wage distribution, and there is no indication for significant disemployment effects overall. At the same time, the use of weights has a modest impact on the results.

Figure A.11 shows the impact of the minimum wage increase on the pre-reform wage distribution in the fixed effects and first difference specifications. We do not expect the minimum wage changes to have an effect on the wage distribution one year before the minimum wage was increased. This is more or less the case in the first difference specifications; however, in the fixed effect specifications there are significant changes in the wage distribution even before the minimum wage was raised. This suggests that the fixed effect results in panel (a) in A.10 and in panel (a) in 9 are likely to reflect pre-existing trends and not the only the causal effect of the minimum wage.

Figure A.12 shows the impact of the minimum wage on teens in the fixed effect and in the first difference regressions. The figure highlights that the effect of the minimum wage is sensitive to the chosen specification. Moreover, the key differences come from the differential impact of the minimum wage at the bottom of the wage distribution, while the upper tail is stable in all specification. This is not surprising since very few teenagers earn more than $15 an hour, and so biases caused by employment changes in the upper tail must be limited. This example highlights that understanding the source of disemployment effects are less important for teens than for the general population, where the estimates are sensitive to employment changes in the upper tail.

Table A.1 compares the point estimates and standard errors of the bunching estimator and an“aggregate event-based (EB) estimator for calculating the elasticity of employment with respect to the minimum wage. The bunching estimator uses wage-bin by state by quarter data, wage-bin specific employment per capita as the outcome, and wage-bin-specific treatment indicators. The employment elasticity with respect to the minimum wage for the bunching estimator is calculated by focusing on employment changes within \$4 of the new minimum wage. In contrast, the aggregate EB estimator uses state by quarter data, the overall employment per capita as the outcome, and simply uses overall treatment indicators that only vary by state and time (and not by wage bin), thus estimating the employment elasticity of all workers rather than just low-wage workers. For almost all subgroups, the bunching estimator is at least as precise as the aggregate estimator, sometimes substantially more so in the case of smaller demographic groups. Row 1 shows that, for all workers, the point estimates of both approaches are rather similar when estimating the policy's employment elasticity, with the standard error of the bunching approach modestly smaller, at 88% of the aggregate EB estimator. In the cases of workers with lower education, the bunching estimator's employment elasticity standard errors are between 65% and 76% of those from the aggregate EB estimator. The last three rows of the Table examine the the high probability, middle, and low probability groups described in section 4. Only for the middle group does the aggregate EB estimator largely outperform the bunching estimator's precision. (As we discuss in the paragraph below, however, for this middle group there is no significant wage effect detectable using the aggregate approach, which makes the precision meaningless.)

The precision gains from the bunching estimator are clearest in the case of estimating wage effects of the minimum wage. Table A.2 compares the t-statistics obtained from estimates of wage effects using the preferred bunching estimator and the aggregate EB estimator. Here, the underlying estimated wage effects for the bunching estimator are the estimated percent wage increases of affected workers; and the underlying estimated wage effects for the aggregated estimator are the wage elasticity with respect to the minimum, calculated using the logarithm of the average quarterly state wage for that demographic group as the outcome. Both sets of estimates use the paper's same underlying 138 events for the minimum wage increases. In nearly every demographic group, the bunching estimator's wage effects are much more precisely estimated and the aggregated estimator's wage effects are often not distinguishable from zero at conventional levels of statistical significance. For all workers, the t-statistic for the bunching estimator is 12 times as large as the t-statistic from the aggregated estimator. Only in the smaller subgroup of teens does the aggregated estimator's precision modestly outperform that of the bunching estimator. In almost all cases, the bunching estimator is able to estimate a wage effect statistically different from zero at the 1 percent level of significance. The only exception is for the low probability group, in which the bunching estimator estimates a positive wage effect statistically distinguishable from zero at the 5 percent level, and where the aggregated estimator obtains a negative and highly imprecise wage effect estimate.

Table A.3 reports estimated wage and employment effects of the aggregate event-based (panel A), and the bunching (panel B) estimators for the Card and Krueger predicted probability groups. In column 1, both approaches estimate a sizable and statistically significant wage effects with no indication of disemployment. The wage and employment elasticities with respect to the minimum wage are 0.187 (s.e. 0.062) and 0.081 (s.e. 0.084) in panel A, respectively, using the aggregate approach;, these are consistent with the findings in panel B using the bunching estimator. However, the former approach fails to detect a statistically significant wage effect of the policy for the middle and the low probability groups in columns 2 and 3. The wage elasticity estimates in columns 2 and 3 are 0.065 (s.e. 0.057) and -0.005 (s.e. 0.038). This limits the ability of using the CK probabilty group approach by itself to examine the employment effects of the minimum wage. Since the program effect is missing for the latter two groups, it is difficult to assess the size of the estimated employment effects (0.057 (s.e. 0.047) and 0.001 (s.e. 0.023) for the middle and low probability groups, respectively). On the other hand, the bunching estimator captures a sizable and statistically significant wage effect for all of the groups (0.051 (s.e. 0.013) and 0.060 (s.e. 0.032) for the middle, and low probability groups). By examining changes in the frequency distribution for wages around the minimum wage, the bunching estimator enables us to establish a causal relationship between the policy and the employment effects for each of the groups.

Table A.4 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.

As a further check on the correlation between minimum wages and the imputation rate of wages, Table A.5 shows the effect of the minimum wage on imputation rate using various alterative specifications. All specifications confirm that minimum wages have no impact on the imputation rate.

The triple-difference specifications include state-period fixed effects, and so they identify the effect of the minimum wage from the employment changes at the bottom of the wage distribution relative to the employment changes at the upper tail. However, the employment changes at the very top of the wage distribution might not be a good control group for low wage workers. Table A.6 explores the effect of the minimum wage in triple-difference specifications when observations from the very top of the wage distributions (i.e., above \$15 or \$20) are dropped. The results in these triple-difference specifications are very close to the baseline results in column (1) in Table 1 and the triple-difference specifications in column (7) in Table 1.

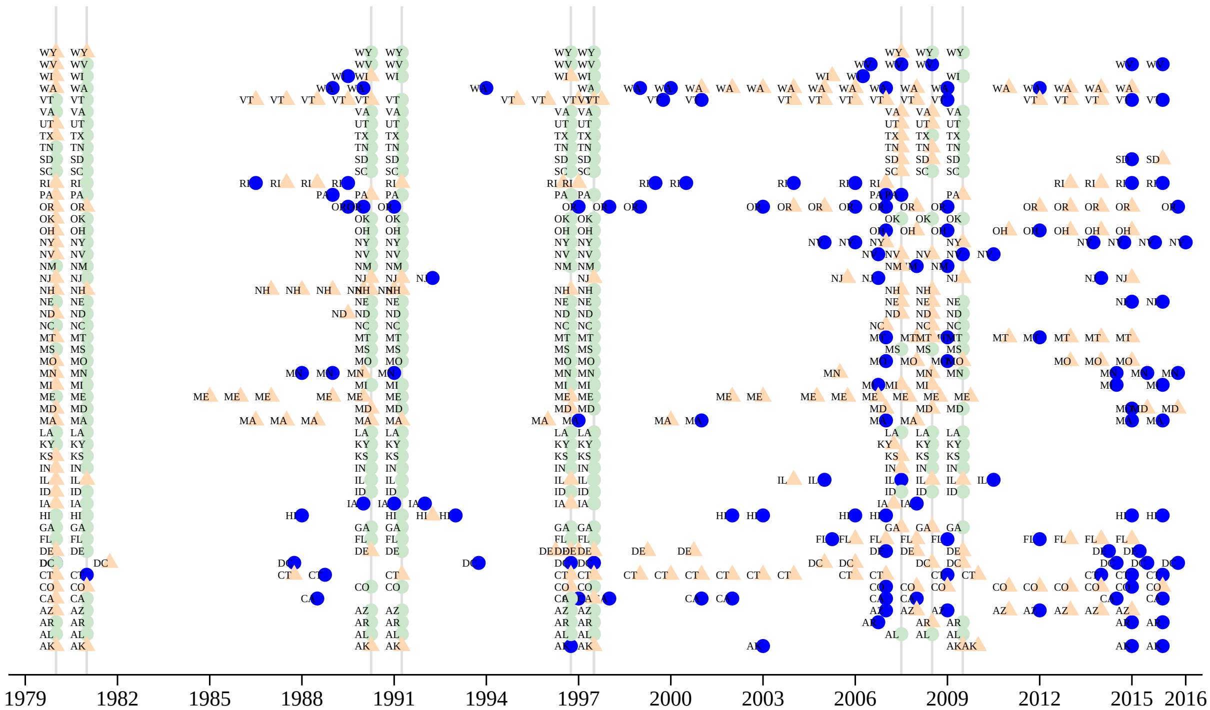
Table A.7 shows the impact of the minimum wage for incumbents and for new entrants to the labor force. Since CPS interviews individuals twice (one year apart), we can only assess short term impact of the minimum wage for these two subgroups. However, columns (1) and (2) highlight that the short term and the long term impact of the minimum wage is very similar for the overall sample. By matching the CPS over time, we lose observations either because matching is not possible, or because there are "bad" matches (see Online Appendix B 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 7).

Table A.8 shows estimates for the event-by-event analysis presented in Table 6 using alternative specifications. The estimated effect of the minimum to median wage on the jobs below, on the missing jobs, on the excess jobs, and on the employment change are very similar across various specifications, which underlines the robustness of the results presented in Table 6.

Table A.9 shows the estimated employment elasticities using fixed effects, first difference, event-based regressions. We report employment estimates on aggregate employment (columns 1, 2 and 5) and employment under $15 (columns 3, 4 and 6). There is a wide range of estimates for aggregate employment, as we pointed out in Figure 9. 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. 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 4.2. 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.

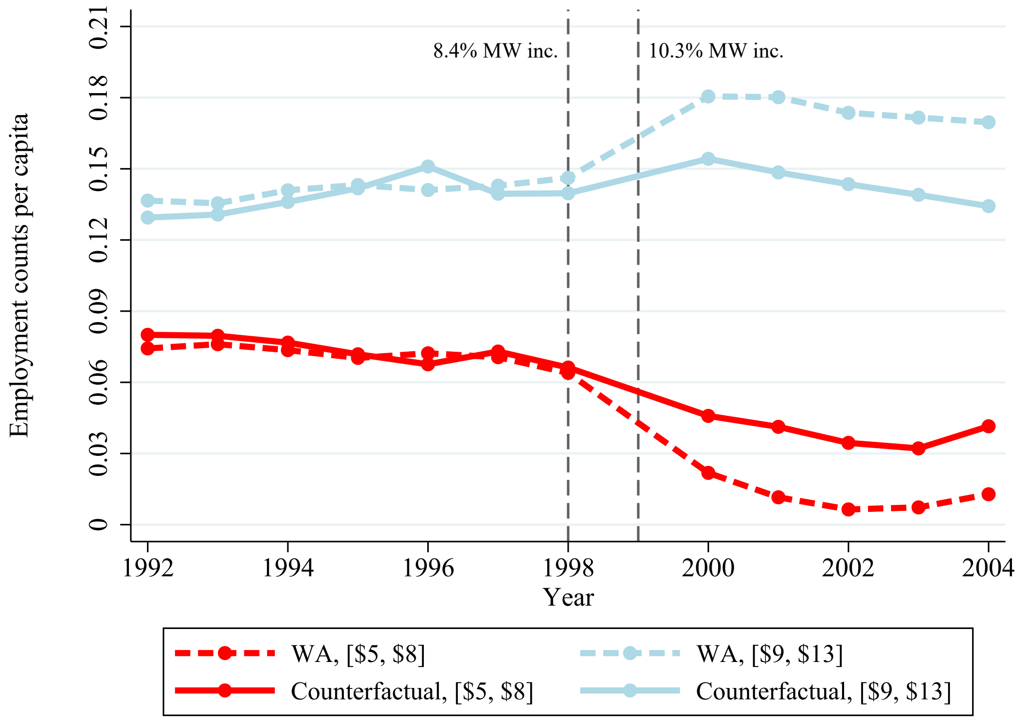
Table A.10 shows the same results as in Table A.9, but now only for teens. The variability in the estimates for teens is not driven by changes in employment in the upper tail. This is not surprising, since most teens earn below $15, and so variation in the upper tail can only have limited impact on the 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



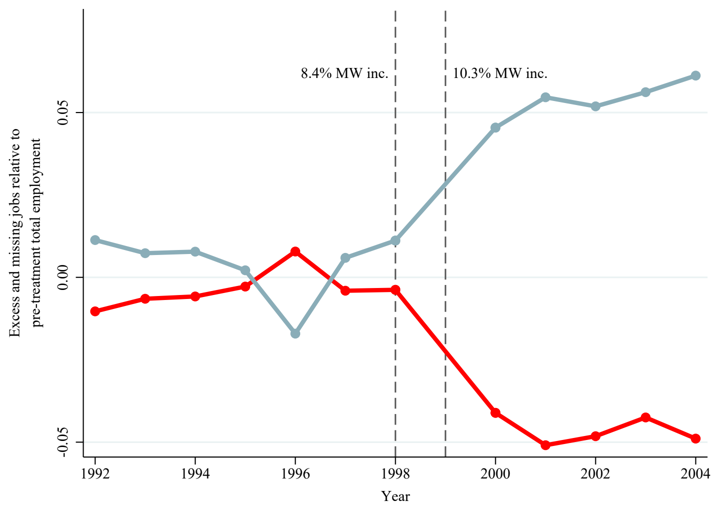
Notes: The figure shows all minimum wage increases between 1979 and 2016. There are a total of 516 minimum wage increases. The blue circles show the primary minimum wage events used in estimating equation 2; the light orange triangles highlight small minimum wage changes where minimum wage increased 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 only the change in missing number of jobs, Δb, is identified 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: Comparison of Per-capita Employment Counts of Washington and the Counterfactual

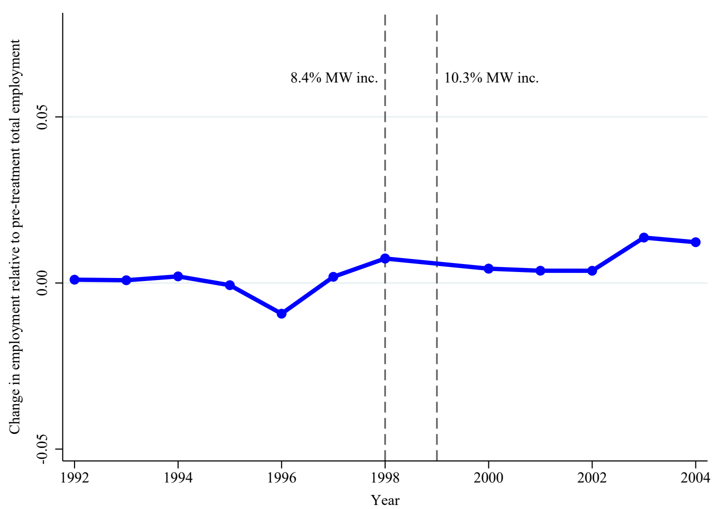


Notes: The figure shows the evolution of the number of jobs per capita with hourly wages between \$5 and \$8, and \$9 and \$13 in Washington and in the counterfactual, with data aggregated in \$1 bins. The counterfactual jobs are calculated using states without any minimum wage change during the 1998-2004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



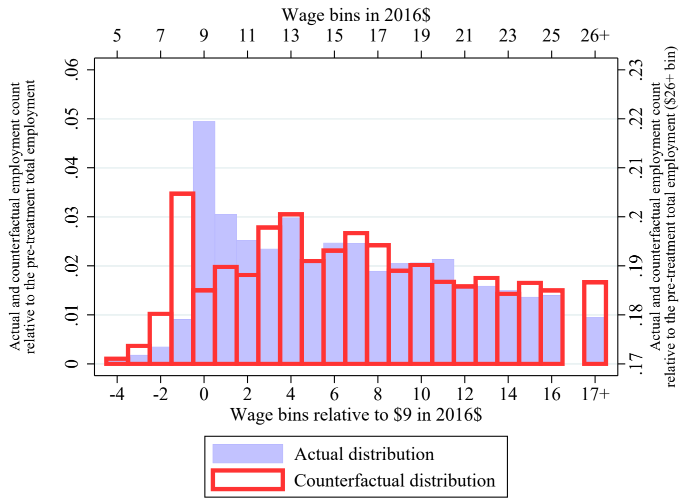
(a) Missing and excess jobs over time

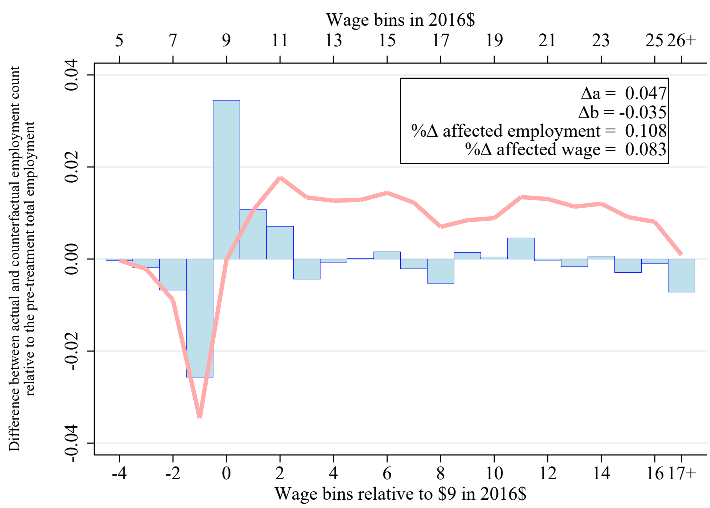


(b) Employment change over time

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.

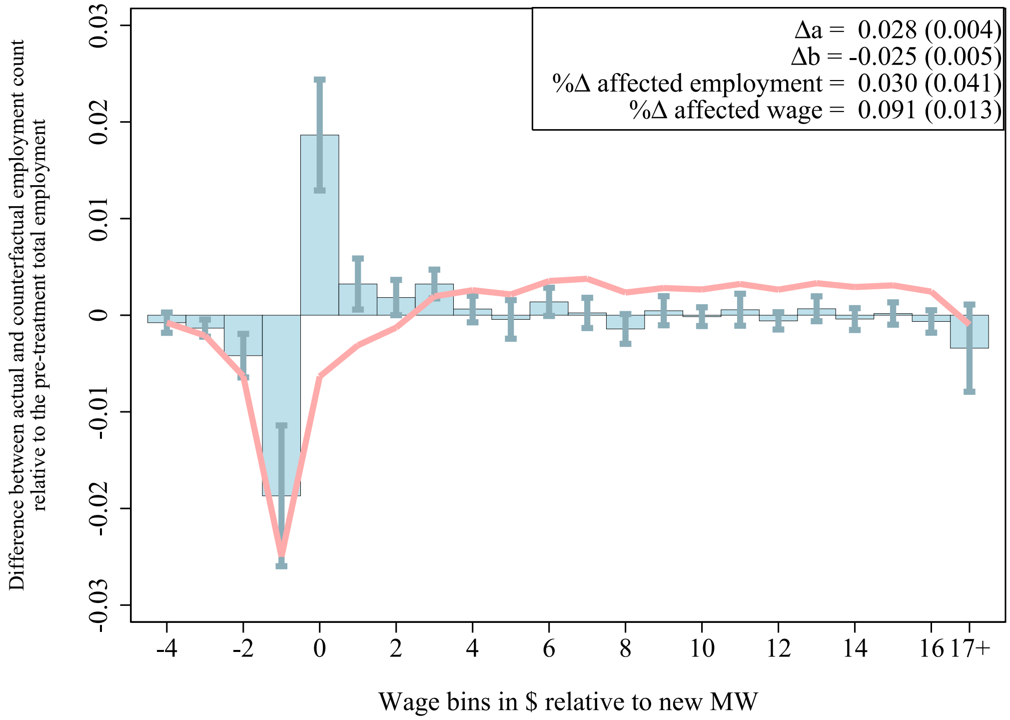
Figure A.4: Employment by Wage Bins in Washington between 2010-2004 (Replication of Figure 2 using CPS data)





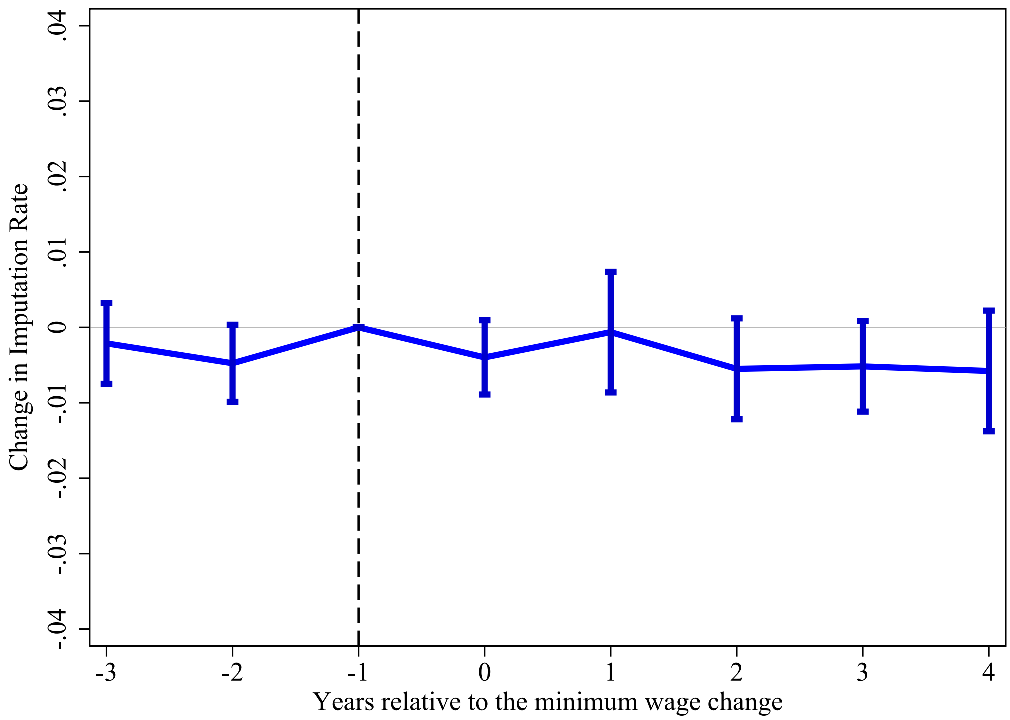
Notes: The figure replicates Figure 2 that examine the effect of the 1999-2000 minimum wage change in Washington on the frequency distribution of wages (aggregated in $1 bins), normalized by the 1998 level of employment in Washington. The minimum wage was raised from $7.51 to $9.18 (in 2016 values) and 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 2. 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 righ y-axis. Panel (b) depicts the difference between the actual and the countefactual wage distribution. The blue bars shows the change in employment at each wage bin (relative to the 1998 total employment in Washington). The red line shows the overall employment changes up to that wage bin. The upper left panel shows the estimates on missing number of jobs between $5 and $8, $\triangle b$; on the excess number of jobs between $9 and $13, $\triangle a$, and on the estimated employment and wage effects.

Figure A.5: Change in Employment by Wage Bins after Aggregating Multiple Treatment Events (Pooled Event Study Analysis)



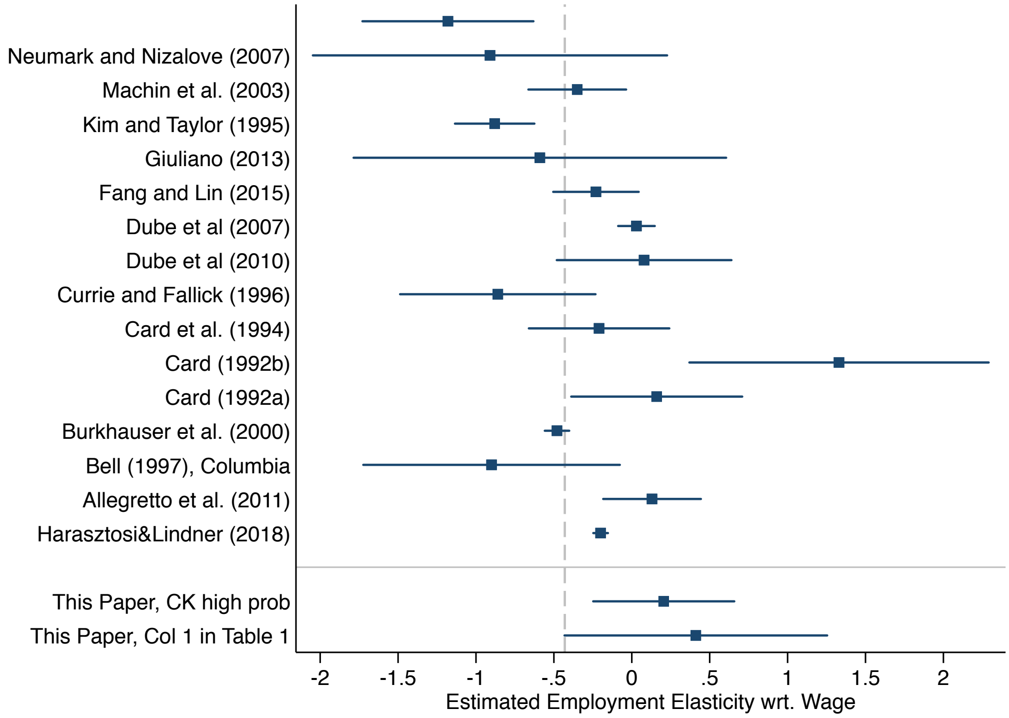
Notes: This figure replicates Figure 3 in the main text, but calculates a cumulative effect when there are multiple events in the 5-year post-treatment window. Overall, 65% of the time, a primary minimum wage increase is followed by a secondary one within 5 years, on average at $0.56 above the minimum for the primary event. Figure 3 shows the partial effect of each event. Here we show the cumulative effect of all events within a 5-year post-treatment window by taking into account the incidence and size of secondary increases averaged across our sample of events. The blue bars show for each dollar bin (relative to the minimum wage) the estimated average employment changes in that bin during the 5-year post-treatment relative to the total employment in the state one year before the treatment. The red line is the running sum of the bin-specific impacts. Adjusting for multiple events increases the estimate for missing jobs below the new minimum from 1.8% to 2.5%. Therefore, some of the difference between jobs below the new minimum wage, which is around 8.6%, and the missing jobs below the new minimum wage can be explained by multiple events following each other.

Figure A.6: Impact of Minimum Wages on the Imputation Rate (Pooled Event Study Analysis)



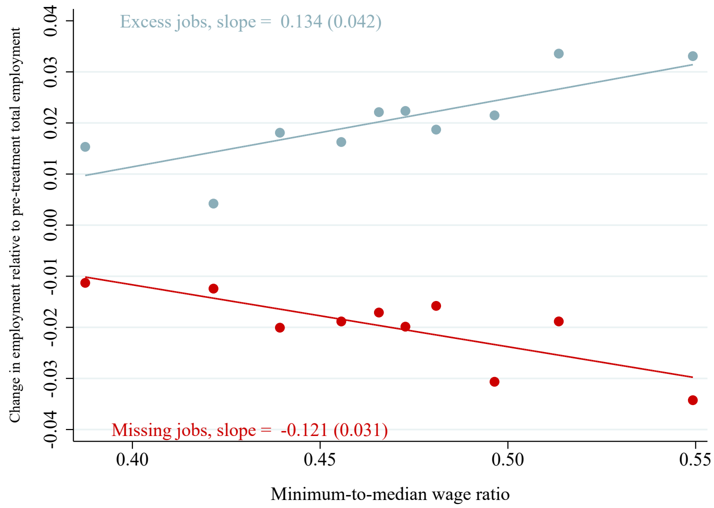
Notes: The figure shows the effect of the minimum wage on the imputation rate. In our pooled event study analysis we only use non-imputed hourly wages. To alleviate the concern that imputation has an effect on our estimates, we implement an event study regression where the outcome variable is state-level 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 specificaion we include state and time fixed effects in the regression. In the Online Appendix Table A.5 we report results with other specifications. The blue line shows the evolution of the state 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.7: Employment Elasticity with Respect to Own Wage in the Literature and in this Paper

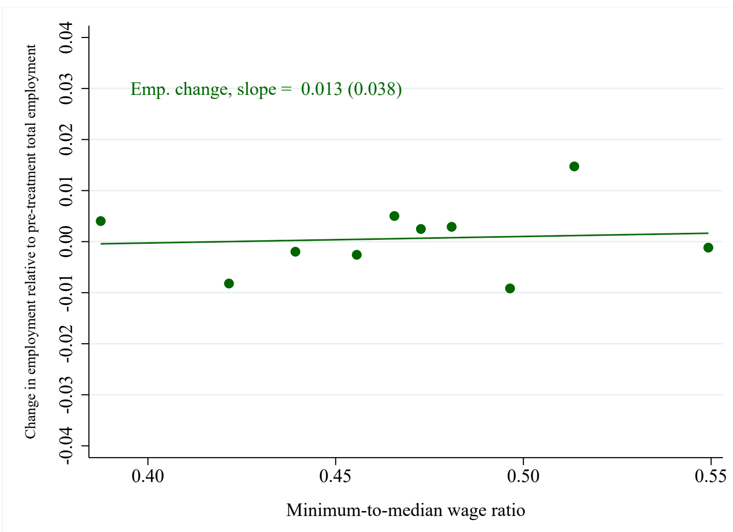


Notes: This figure summarizes the estimated employment elasticity with respect to wage and compares it to the previous estimates in the literature. The estimates in the literature is collected by Harasztosi and Lindner (2016). The two estimates from our paper is the benchmark estimate on overall employment (Column 1 in Table 1) and the estimates for the Card and Krueger high probability group Column 6 in Table 3. The dashed vertical line shows the lower bound of our benchmark estimates. The benchmark estimates can rule out 7 out of the 11 neagtive estimates provided in the previous literature.

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



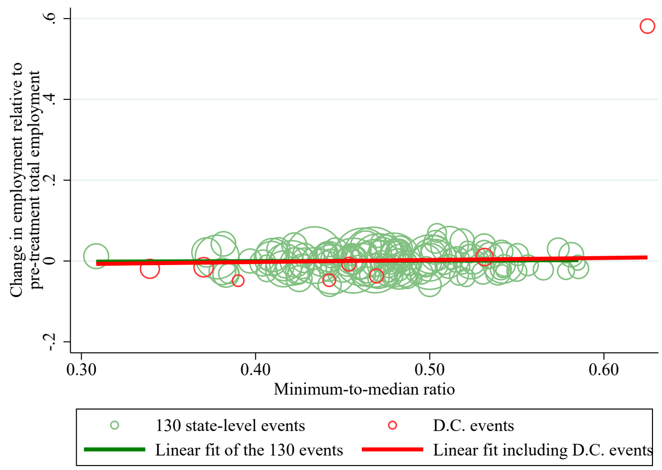
(a) Missing and excess jobs



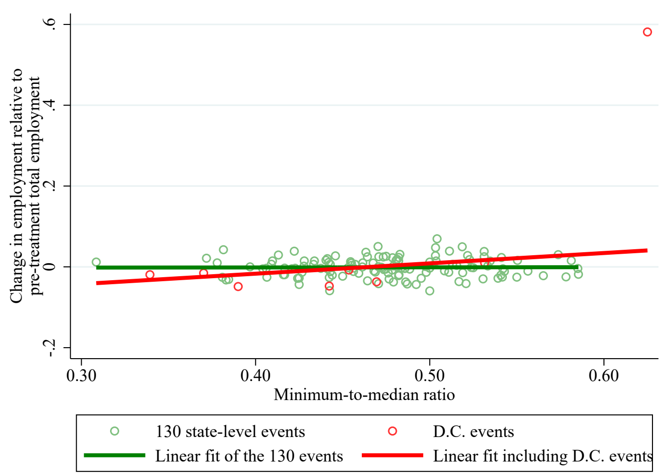
(b) Employment change

Notes: This figure replicates Figure 8 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 event-specific inverse variance of the employment change estimate that was calculated using the bootstrap procedure described in the text. The slope (and robust standard error in parentheses) is from the weighted linear fit of the outcome on the minimum-to-median wage ratio.

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



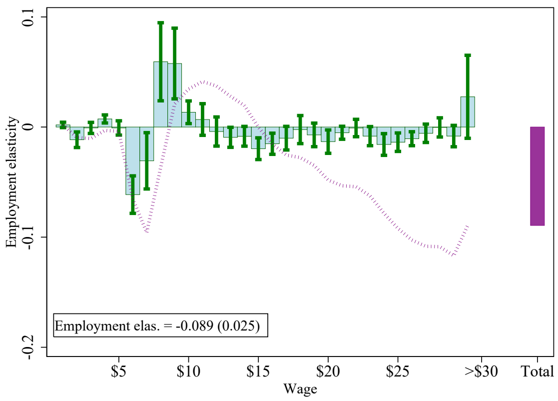
(a) Inverse-variance weighted



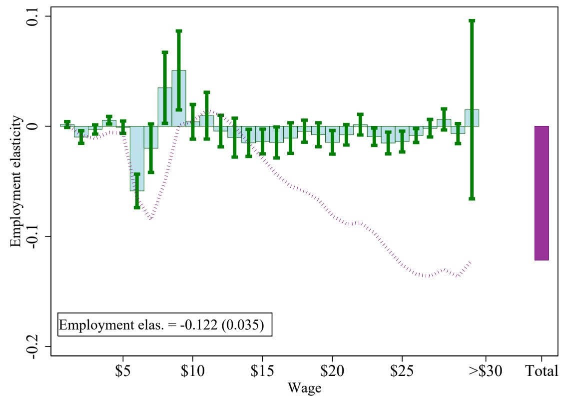
(a) Unweighted

Notes: The figure shows the inverse-variance 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 by adding the baseline regression residuals of the relevant bins to the missing and excess jobs estimates, as explained in Section 4.6. The red circles indicate D.C. events, and the green circles the remaining 130 events. The lines are linear fits. The green line employs the 130 events; while the red one all events.

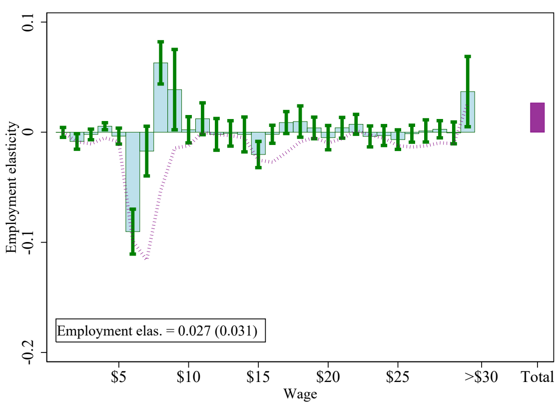
Figure A.10: Impact of Minimum Wages on the Wage Distribution in Fixed Effects and First Difference Specifications



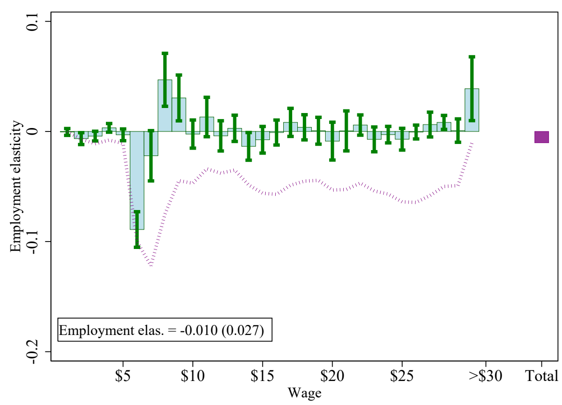
(a) Fixed effect, weighted



(b) Fixed effect, unweighted



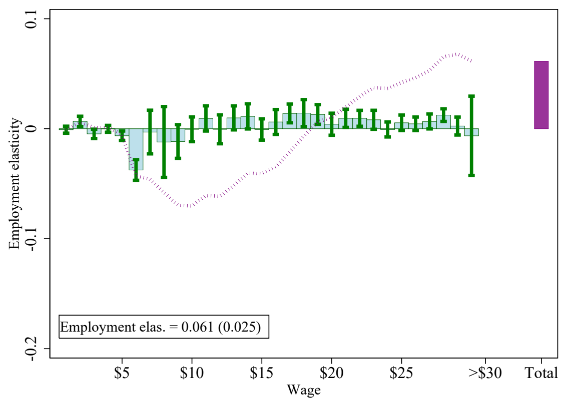
(c) First differences, weighted



(d) First differences, unweighted

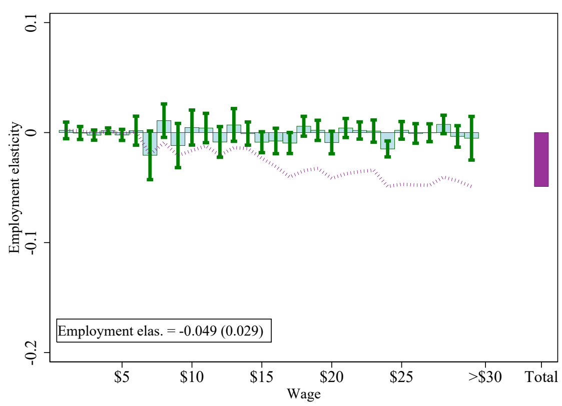
Notes: The figure shows the effect of the minimum wage on the wage distribution in population fixed effects and first difference specifications, with and without population weights.. Panel (a) and (b) estimates two-way (state-bin and year) fixed effects regressions on contemporaneous as well as 2 annual leads, contemporaneous, and 4 annual lags of log minimum wage (panel (a) is the same as Figure 9 in the main text. In Panel (c) and (d) we employ first difference regression with 2 annual leads, contemporaneous, and 4 annual lags of the log change in the minimum wage. For each wage bin we run a separate regression, where the outcome is the number of jobs per capita in that state-wage bin. The 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 a 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) and (c) are weighted by state population; whereas the ones in panels (b) and (d) on the right-hand side are not weighted.

Figure A.11: Estimated Three Year Leading Effects of the Impact of Minimum Wages on the Wage Distribution for Fixed-effects and First-difference specifications



(a) Fixed effect, weighted

(b) Fixed effect, unweighted

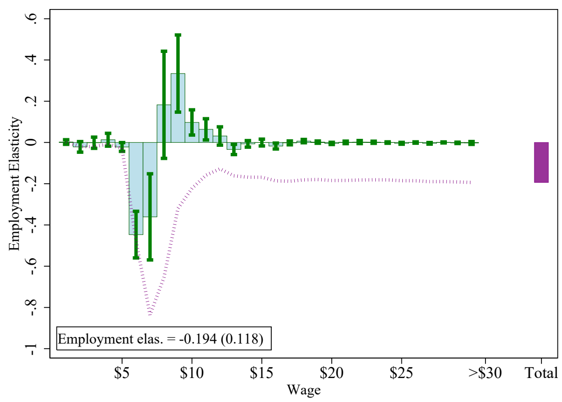


(c) First differences, weighted

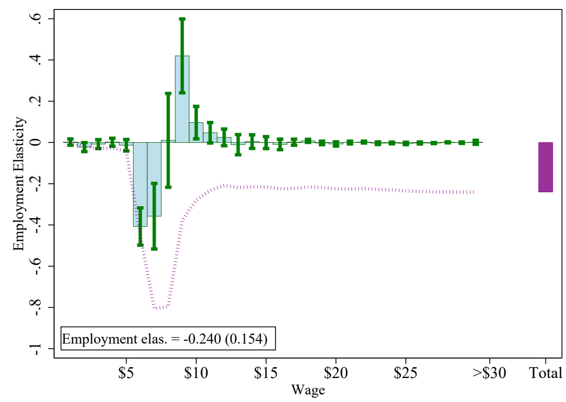
(d) First differences, unweighted

Notes: The figure shows the three year leading effect of the minimum wage on the wage distribution in population weighted and unweighted fixed effects and first difference specifications. All panels estimate two-way (state-bin and year) fixed effects or first difference regressions on contemporaneous as well as 4 annual lags and 2 annual leads of log minimum wage. For each wage bin, we run a separate regression, where the outcome is the number of jobs per capita in that state-wage bin. The three year leading effect is formed by adding the first and the second leading coefficients and multiplying the sum by -1: when the cumulative response at $\tau = -1$ is normalized to 0, this represents the three year leading effect relative to the date -1. For the fixed effects specifications, the three year leading effect estimate represents three or more years prior to treatment; for the first difference specifications, this estimate represnts exactly three years prior to treatment. The green histogram bars show the cumulatative response divided by the sample average employment-to-population rate —and represent the average three year leading 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 a particular wage bin. The rightmost purple bar in each of the graphs is the three years leading 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 for this elasticity with standard errors that are clustered at the state level. Regressions in panels (a) and (c) are weighted by state population; whereas the ones in panels (b) and (d) on the right-hand side are not weighted.

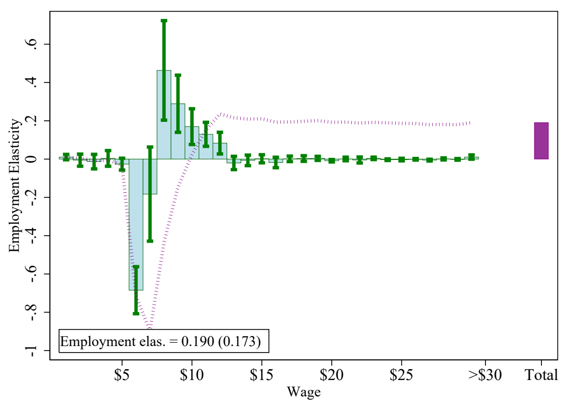
Figure A.12: Impact of Minimum Wages on the Wage Distribution in Fixed Effects and First Difference Specifications For Teens (Reporting analogous results as in Figure A.10 and 9, but for teens)



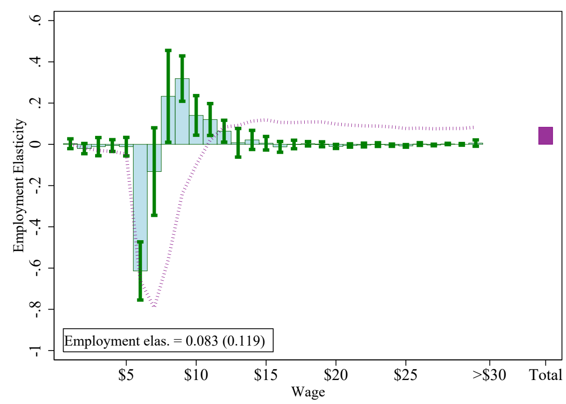
(a) Fixed effect, weighted



(b) Fixed effect, unweighted



(c) First differences, weighted



(d) First differences, unweighted

Notes: The figure reports the results from Figures A.10 and 9, but instead of using the whole population here we focus only on teens. The figure shows the effect of the minimum wage on the teenage workers' wage distribution in fixed effects and first differences specifications. Panels (a) and (b) estimate two-way (state-bin and year) fixed effects regressions on contemporaneous as well as 4 annual lags 2 annual leads of log minimum wage. In Panels (c) and (d) we employ a first difference regression with contemporaneous as well as 4 annual lags 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 teen employment-to-population rate —and represents the average elasticity of teen 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 a 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) and (c) are weighted by state population; whereas the ones in panels (b) and (d) on the right-hand side are not weighted. The figure highlights that for teens, discrepancies across specifications is not driven by employment at the upper tail of the wage distribution, which is as expected given the small number of teens earning high wages.

Table A.1: Precision of the Employment Elasticities with Respect to the Minimum Wage - Bunching and Aggregate Approaches

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

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

Table A.2: T-statistics for the Wage Effects of the Minimum Wage - Bunching and Aggregate Approaches

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

Notes. Each cell reports the t-statistic from the estimated wage effect with respect to the minimum wage for various demographic groups. The bunching approach is the preferred specification in this paper, estimating the wage effect from bin-specific employment changes near the relevant minimum wage. The aggregated approach uses as the outcome overall aggregate employment. For the bunching case, the wage effect is the estimated percentage change of affected workers. For the aggreaged 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. Robust standard errors in parentheses are clustered by state.

Table A.3: Impact of Minimum Wages on Employment and Wages for Card and Krueger Probability Groups - Bunching and Aggregate approaches

	(1)	(2)	(3)
Panel A: Aggregate			
Wage elas. wrt MW	0.187***	0.065	-0.005
	(0.062)	(0.057)	(0.038)
Emp. elas. wrt MW	0.081	0.057	0.001
	(0.084)	(0.047)	(0.023)
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)
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	Low prob.

Notes. The table reports the wage and employment elasticities with respect to the minimum wage for the high probability, 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 5 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.4: 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 2) 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 3 in Section 4.1. 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 qurater-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 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.6: Impact of Minimum Wages in Various Triple-Difference Specifications

	(1)	(2)
Missing jobs below new MW (Δb)	-0.018***	-0.019***
	(0.004)	(0.004)
Excess jobs above new MW (Δa)	0.021***	0.020***
	(0.003)	(0.003)
%Δ affected wages	0.067***	0.064***
	(0.011)	(0.010)
%Δ affected employment	0.025	0.008
	(0.036)	(0.038)
Employment elasticity w.r.t. MW	0.021	0.007
	(0.031)	(0.032)
Emp. elasticity w.r.t. affected wage	0.376	0.130
	(0.530)	(0.587)
Jobs below new MW ($\bar{b}_{-1}$)	0.086	0.086
%Δ MW	0.101	0.101
Number of events	138	138
Number of observations	412,794	557,634
Number of workers in the sample	2,146,370	2,955,355
Excluding wages above	\$15	\$20

Notes. The table reports the effects of a minimum wage increase based on the event study analysis (see equation 2) 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. All specifications report triple-difference specifications where we control for wage bin-by-state, wage bin-by period, and state-by-period fixed effects. In column (1) observations with wages greater than \$15, and in column (2) observations with wages greater than \$20, are dropped. The results should be compared to column 1 in Table 1 (baseline) and column 7 in Table 1 (triple-difference without dropping any observations from the upper tail). 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 3 in Section 4.1. 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 qurater-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 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 3 in Section 4.1. 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 qurater-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.8: 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	0.302***	0.341***	-0.121***	-0.136***	0.134***	0.136***	0.013	-0.000
	(0.041)	(0.045)	(0.031)	(0.032)	(0.042)	(0.048)	(0.038)	(0.042)
Panel B: With D.C.								
Minimum-to-median ratio	0.298***	0.336***	-0.099***	-0.109***	0.150***	0.156***	0.050	0.048
	(0.040)	(0.044)	(0.036)	(0.039)	(0.044)	(0.050)	(0.051)	(0.060)
Panel C: Population weighted								
Minimum-to-median ratio	0.314***	0.361***	-0.130***	-0.153***	0.155***	0.160***	0.026	0.007
	(0.068)	(0.059)	(0.045)	(0.042)	(0.047)	(0.053)	(0.041)	(0.045)
Panel D: Unweighted								
Minimum-to-median ratio	0.275***	0.286***	-0.116***	-0.119***	0.119***	0.122***	0.003	0.002
	(0.035)	(0.035)	(0.025)	(0.028)	(0.039)	(0.045)	(0.039)	(0.045)
Number of observations								
Panels A, C, D	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 7. Regressions are weighted by event-specific inverse-variances (see the text for details). Robust standard errors are in parentheses; significance levels are * 0.10, ** 0.05, *** 0.01.

Table A.9: 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			
Weighted	-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)
Unweighted	-0.122***	-0.010	-0.015	-0.049***	-0.089	0.023	0.028
	(0.035)	(0.027)	(0.047)	(0.020)	(0.060)	(0.026)	(0.030)
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 employment elasticities of minimum wage from alternative approaches. Columns (1)-(4) show long run (3 year) elasticities calculated from regressions of state-level employment to population rate on contemporaneous and 3 annual lags of log minimum wages. Weuse state-by-year aggregated CPS data from 1979-2016. In columns (1) and (3) estimates 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. Column (5) we use state by quarter aggregated CPS data. In column (6) we directly estimates effect of the minimum wage on jobs below $15. We refer to this specification as simpler method in Section 4.2., since it direcetly estimate the sum of missing and excess jobs. Finally, column (7) shows estimates from the bunching approach (same as in Table 1, column 1). 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.

Table A.10: Teen 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			
Weighted	-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)
Unweighted	-0.187	0.133	-0.167	0.131	-0.004	-0.017	-0.043
	(0.153)	(0.143)	(0.152)	(0.150)	(0.100)	(0.101)	(0.124)
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 teen employment elasticities of minimum wage from alternative approaches. Columns 1-4 show long run (3 year) elasticities based on two-way (state and year) fixed effects regressions of state EPOP on contemporaneous and 3 annual lags of log minimum wages, using state-by-year aggregated CPS data from 1979-2016. In columns 1 and 3, the model is estimated in levels, while in columns 2 and 4 the model is estimated in first differences. Columns 5 and 6 report estimates using quarterly data and an event based approach using 138 state events, where we regress state EPOP on quarterly leads and lags on treatment spanning 12 quarters before and 19 quarters after the policy change. Columns 3, 4 and 6 exclude workers with hourly wages greater than \$15. Finally, column 7 shows estimates from our bunching approach, same as in Table 1, column 1. In all cases we show estimates with and without population weighting. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Online Appendix B Data Appendix

The primary data set we use in the pooled 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 couns 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 2. This data was provided to us as counts of workers in (nominal) \$0.05 bins between 1992 and 2016 by the state's Employment Security Department. We convert this data into \$0.25 (real 2016 USD) hourly wage bins for our analysis using the CPI-U-RS. We also use similar micro-aggregated administrative data from Minnesota and Oregon for conducting comparison of data quality and measurement error in Online Appendix C.

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 porposed by Madrian and Lefgren (2000), and use household id (HHID), household number (HHNUM), person line number in household (LINENO), month in sample (MINSAMP), and month and state variables to match observations in two consecutive CPS files. We confirm the validity of matches by evaluating reported sex, race, and age in the two surveys. If sex or race do not match, or if individual's age decreases by more than 1 or increases by more than 2, we declare them as "bad matches" and exclude from the matched sample. Additionally, since matching is not possible from July to December in 1984 and 1985, from January to September in 1985 and 1986, from June to December in 1994 and 1995, or from January to August in 1995 and 1996, we exclude these periods. On average, 72% of the observations in the CPS are matched: around 25% of the individuals in are absent in the 8th sample month, while an additional 3% are dropped because they are bad matches. We determine the incumbency of individual from employment status information in the 4th sample month. Similar to our primary CPS sample, we drop observations with imputed wages in the 8th sample month. Overall, the number of worker-level observations is smaller in the matched sample because we only use the 8th sample month in the matched sample, as opposed to both 4th and 8th sample months in the baseline sample.

Industry classifications

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

Online Appendix C Comparison of Administrative Data to CPS

In our pooled 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 C.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-bencharked 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 C.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 C.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.

C.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 will have 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".

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

In figure C.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 C.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.

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

Figure C.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.

C.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.[50] 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 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

[50] 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.

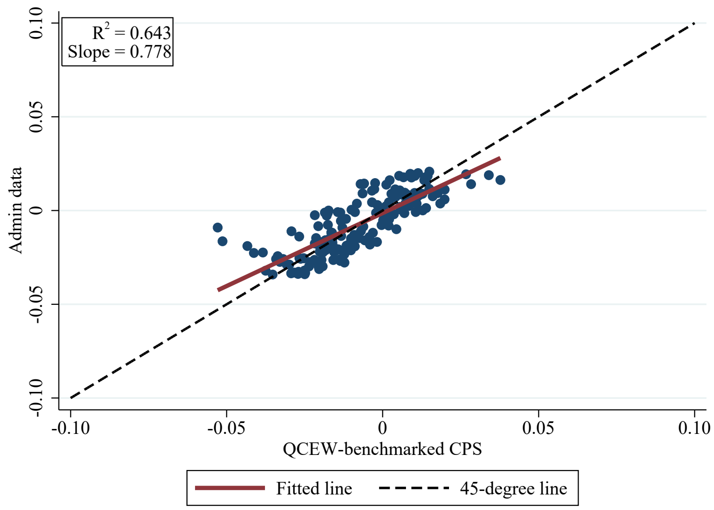
workers in our analysis.

Table C.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).[51] 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).

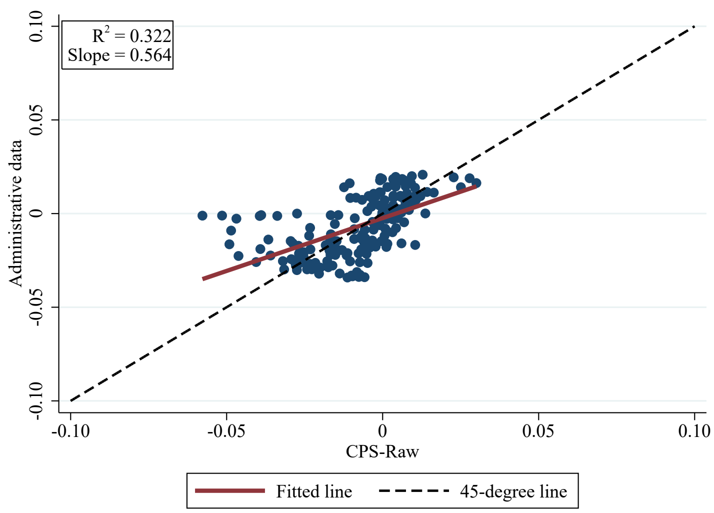
Overall, the structural estimation results suggest that the extent to which there is misreporting of wages, they are of similar magnitude in the CPS and in high quality administrative wage data. This provides additional support for the validity of our bunching estimates using CPS data.

[51] 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.

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

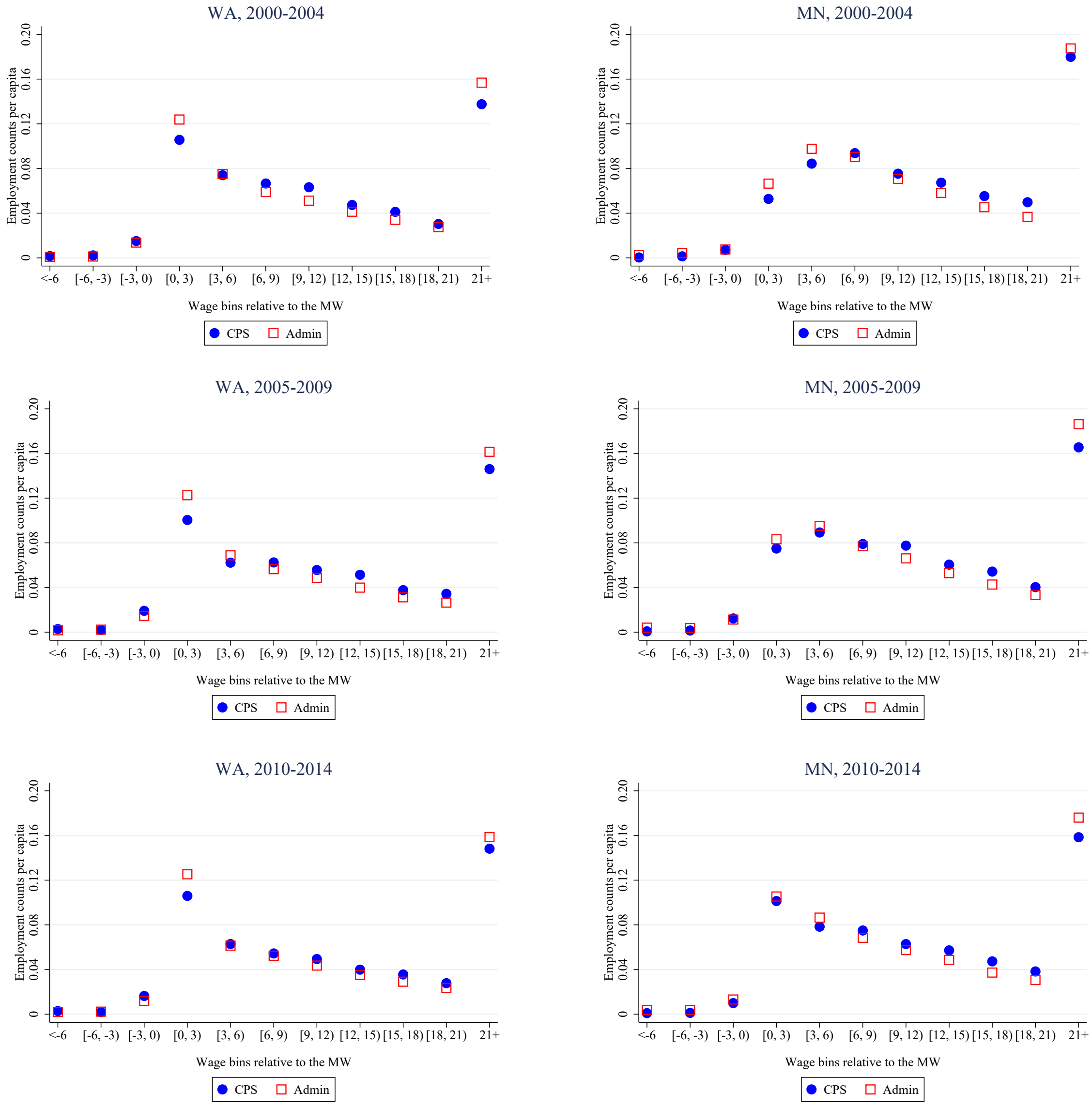
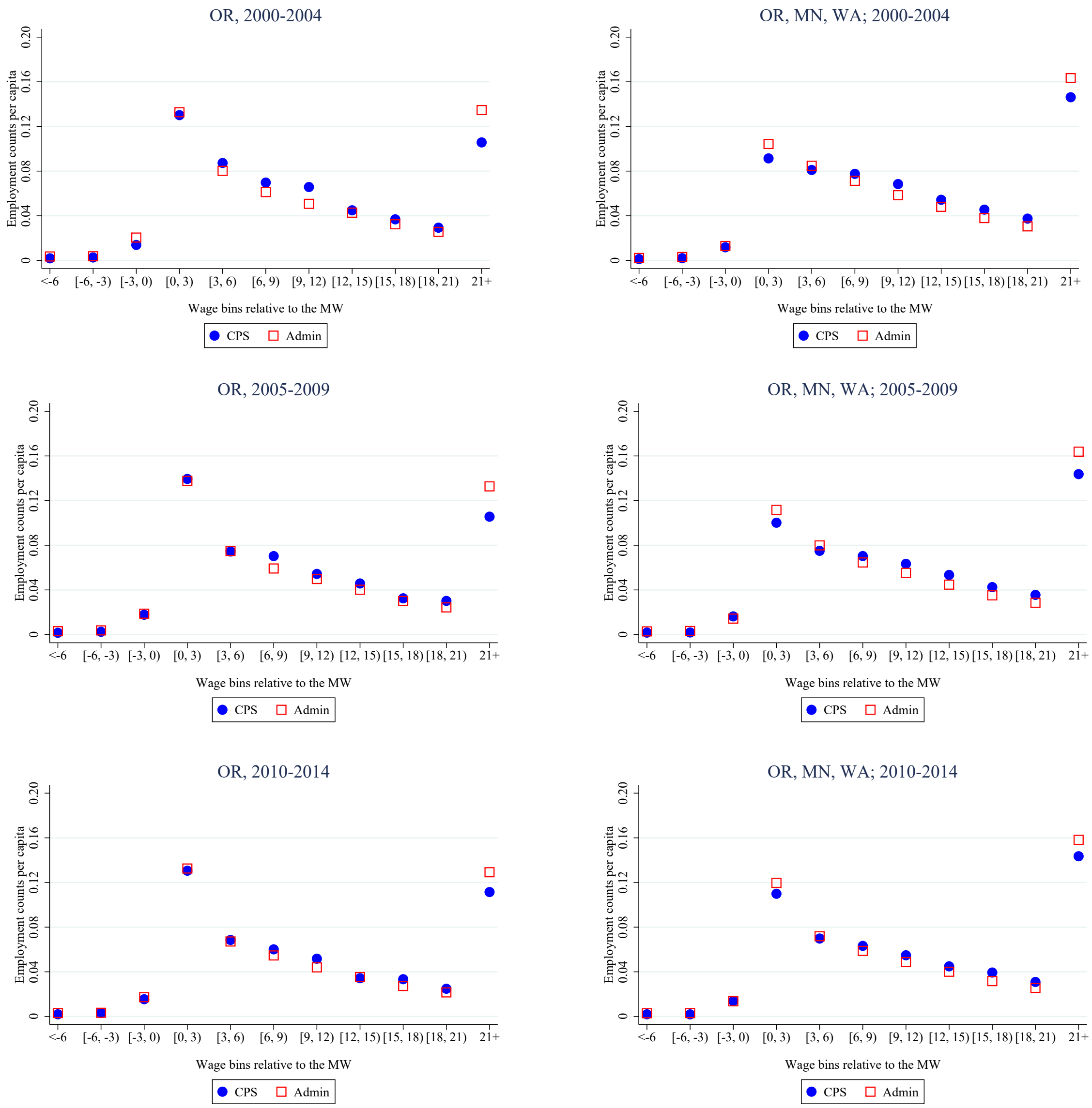
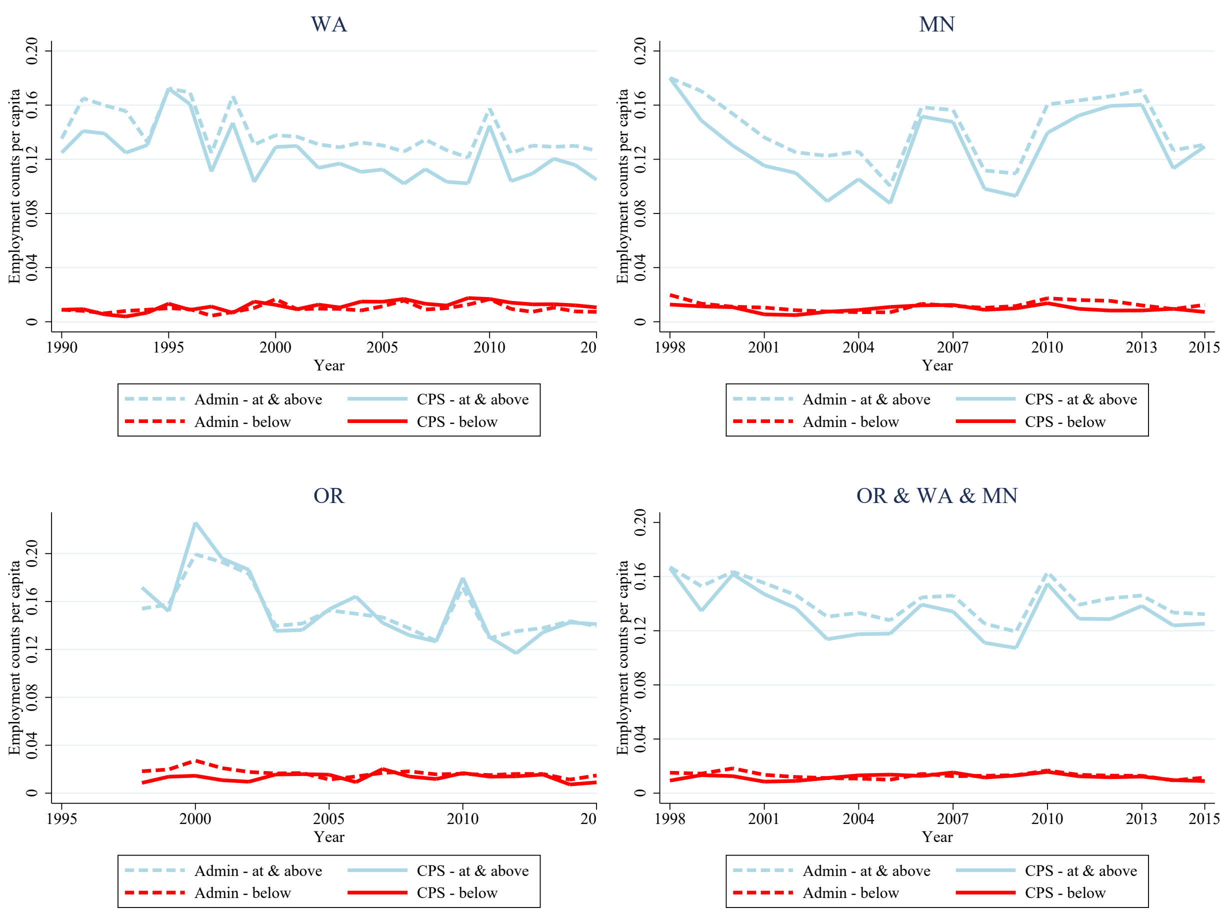


Figure cont'd: Frequency Distributions in the Administrative and CPS data



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 C.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.

Table C.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 C.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-γ	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.

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

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

We begin our analysis by assessing the contribution of various factors that drive the differences in estimates between our benchmark EB and the TWFE-logMW specifications. Table D.1 compares the employment elasticities with respect to the minimum wage of the benchmark EB (see Table 1 and Figure 3) and the TWFE-logMW estimator (Figure 9), as well as several intermediate forms that bridge the two specifications. Column 1 reproduces our baseline estimate of the policy elasticity which considers employment changes locally within a \$9 window around the new minimum wage using the regression specification in equation 4. In Column 2 we use the event study design, but estimate the effect on overall, below \$15, and above \$15 employment counts per capita using aggregated state-level treatment variation. The resulting employment elasticity of 0.027 for the below \$15 group is very similar to our baseline employment elasticity. Reassuringly, we see little employment change for higher wage workers earning above \$15. The resulting overall employment elasticity with respect to the minimum wage, 0.016, is very close to our benchmark estimate.

The EB estimator uses a discrete 0-1 indicator for minimum wage changes, while the TWFE-logMW uses continuous treatment. In Column 3 and 4 we provide intermediate results that continue to use the 8-year event windows, but use a continuous treatment meaure. In particular, we multiply the wage-bin-state-specific treatment indicators in EB by the change in log minimum wage; column 3 uses the \$9 window around the minimum wage, while column 4 simply considers employment under \$15, employment above \$15, and overall employment. This latter EB-logMW specification can be written as follows:

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

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

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

$$\frac{E_{st}}{N_{st}} = \sum_{\tau=-3}^{4} \alpha_\tau \log MW_{s,t-\tau} + \mu_s + \rho_t + u_{st} \quad \text{(D.2)}$$

The cumulative responses can be calculated as $\beta_\tau = \sum_{j=-3}^{\tau}\alpha_j$ by successively summing the coefficients. Again, we report the 5 year averages in employment change from the year prior to treatment: $\frac{1}{5}\sum_{\tau=0}^{4}\beta_\tau - \beta_{-1}$. The TWFE-logMW estimate is the same as what is reported in Figure 9. The overall employment elasticity with respect to the minimum wage is large, negative, and statistically distinguishable from zero (-0.089., s.e. 0.025).

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

$$\triangle\frac{E_{st}}{N_{st}} = \sum_{\tau=-3}^{4} \alpha_\tau \triangle \log MW_{s,t-\tau}^{\tau} + \mu_s + \rho_t + u_{st} \quad \text{(D.3)}$$

The cumulative responses can be calculated as $\beta_\tau = \sum_{j=-3}^{\tau}\alpha_j$ by successively summing the coefficients. Again, we report the 5 year averages in employment change from the year prior to treatment: $\frac{1}{5}\sum_{\tau=0}^{4}\beta_\tau - \beta_{-1}$. This latter specification is consistent under the same strict exogeniety assumptions as the TWFE estimated in levels; however, the small sample properties differ substantially when the number of units (N) is small (Wooldridge, 2001). Our sample has a relatively small N with 51 states and DC, and we have an outcome variable that is highly persistent. This opens up the possibility that small sample biases may be quite different for the TWFE-logMW model estimated in levels than the FD specification.

Table D.2 provides further evidence that the negative employment estimate in the TWFE-logMW specification happens for the "wrong workers" and that this estimate likely reflects confounding shocks to employment in states raising minimum wages. We begin by documenting that the event-based and first differenced approach produce sensible results for groups that are unlikely affected by the policy. The first column of Table D.2 use EB-logMW specification and report the estimates on overall employment, as well as employment below and above $15/hour, and also for the 3 Card and Krueger (CK) probability groups using demographic predictors (see Section 4.4 for the details). The employment estimates are typically small in magnitude; and for either the high wage (above $15/hour) group or the middle and low probability groups the effects are all statistically indistinguishable from zero. Column 2 reports the estimates using the first-differenced version (FD) specifcation, which finds small estimates for aggregate employment and for employent by wage cagegories and the CK probability groups. In contrast, Column 3 shows the TWFE-logMW estimates are large, and are driven by high wage employment. Moreover, the putative disemployment in the TWFE-logMW specification is entirely driven by the low-probability group that is not supposed to be affcted by the minimum wage. Figure D.3 shows graphically the employment changes by wage bin for each of the three CK groups. For the high and middle probability CK groups that likely includes the vast majority of minimum wage workers, we find an indication for missing jobs at low wage bins, and excess jobs at and slightly above those bins, and small effects in the upper tail—leading to little overall job losses. In contrast, for the low probability group (panel C), there are no missing jobs or excess jobs at the bottom of the distribution, and yet there is a large negative disemployment estimate at the top of the wage distribution. This provides another piece of evidence that the source of disemployment effect of the TWFE-logMW model comes from workers whose employment should not be affected by the minimum wage. It is also worth noting that for the high and middle probability groups capturing most minimum wage workers, none of the specifications suggest a sizable disemployment effect anywhere in the distribution.

Additionally, the TWFE-logMW estimates are not robust to controls for time-varying heterogeneity. In Column we 5 add state level linear trends to the TWFE-logMW secification, while in columns 6 we add industry and occupation shares in the 1979-1980 (interacted with time periods) to the TWFE-logMW specificaion. The latter accounts for shocks to upper tail employment that are predicted by historical industrial/occupational patterns in the state. The employment estimates in columns 5 and 6 are close to zero and not statisically significant: allowing for richer controls for time-varying heterogeneity produce results that are very similar to the benchmark EB estimates. Overall, Table D.1 highlights the fragility of the two-way fixed estimate suggesting a large overall employment effect.

Figure D.1 panel (a) shows another problem with the TWFE-logMW specification. This figure plots the time path of employment elasticities with respect to the minimum wage for the TWFE-logMW. Since increases in nominal minimum wages, $log(MW)$, are always permanent, the last lag in the distributed lag model reflects the "long term effect" - the weighted average of effect at or after 4 years following a minimum wage increase. Moreover, since we normalize the estimates relative to the one year before the minimum wage, $-\alpha_{-1}$ measures the average employment occurring at 3 (or more) years prior to the minimum wage increase.

The time path of the estimates shows that the TWFE-logMW estimator obtains a spurious, positive leading effect three (or more) years prior to the minimum wage increase. This shows that there were large employment reductions substantially prior to minimum wage increases, which can impart a bias on the treatment effect estimated using the TWFE-logMW model; moreover, because we are "binning up" the leads and lags at -3 and +4, respectively, biases associated with these binned estimates can impart a bias on the estimated leads and lags, producing a spurious dynamic pattern even within the event window. These sizable and statistically significant pre-treatment and post-treatment effects disappear once we add state-specific linear trends (see panel b).

Figure D.2 plots the time path of the employment elasticities separately for workers earning below $15 per hour (panel a) and above $15 per hour (panel b). The pattern of positive leads and the overall downward trend in employment preceeding the minimum wage change comes from workers earning above $15 per hour. Lower wage workers, earning under $15/ hour, see little change in employment before or after the minimum wage increase.

Finally, and importantly, Table D.3 provides a placebo exercise that demonstrates that the disemployment detected by the TWFE-logMW estimator is not caused by the minimum wage changes, but rather is driven entirely by how employment shocks much earlier than the minimum wage changes affect the estimation of the state fixed effects. Column 1 uses the entire 1979-2016 period and shows the overall employment elasticities with respect to the minimum wage from the classic first difference estimator (panel A), first-differenced version (panel B), and event-based continuous variation (panel C). As before, the TWFE-logMW estimator stands out in estimating a large, statistically significant overall employment decline due to minimum wage changes, in contrast to the first-difference and event-based specifications. However, when we restrict the sample to the 1990-2016 period, the TWFE-logMW estimates become small and stastitically indistinguishable from zero, and are quite similar to the first-difference and event-based estimates. This is noteworthy because there were few state minimum wage changes prior to 1990. As an intermediate step before conducting the placebo exercise, column 3 shows results using the full 1979-2016 sample of data, but excluding the ten states that experienced any minimum wage increases prior to 1990. The pattern of results remains the same in column 3, with the TWFE-logMW specification estimating large employment declines due to the minimum wage.

The placebo exercise uses actual employment data until 1990 for these forty states without any minimum wage changes in the 1980s, but then sets employment outcomes after 1990 to exactly 0 for all states and time periods. Because there were no minimum wage increases prior to 1990 in this sample, and because the employment outcomes are exactly constant after 1990, 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. Column 5 shows that these results are not due to anticipation effects, by excluding the only two additional states with minimum wage increases prior to 1996. This placebo exercise establishes that the estimated disemployment in the TWFE-logMW specification is entirely due to upper-tail 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 or the FD 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.

Overall, these findings clarify that the large, negative TWFE-logMW estimate is driven by upper tail shocks in the 1980s—substantially prior to most minimum wage increases we study. Moreover, these shocks are predicted by a state's historical industrial/occupational structure. Importantly, these shocks died out subtantially 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 when there is a long pre-treatment period through the biased estimation of the state fixed effects.

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

	(1)	(2)	(3)	(4)	(5)
Bunching	0.024		0.024		
	(0.025)		(0.020)		
Below $15		0.027		0.020	0.020
		(0.022)		(0.016)	(0.028)
Over $15		–0.010		–0.012	–0.109***
		(0.042)		(0.033)	(0.030)
Overall		0.016		0.008	–0.089***
		(0.029)		(0.025)	(0.025)
Event-based	Y	Y	Y	Y	
Bin-state-specific treatment	Y		Y		
State-specific treatment		Y		Y	Y
Discrete treatment	Y	Y			
Continuous treatment			Y	Y	Y
Standard TWFE					Y

Notes. The table reports estimated employment elasticities of minimum wage from alternative approaches. Column 1 reports our baseline estimates (Column 1 in Table 1) that is derived by using local employment changes within a $9 window around the new minimum wage. Column 2 use the same event study design as in Column 1 (see equation 2), but estimate the effect on below $15 employment counts, above $15 employment counts, and overall employment counts using aggregated state-level treatment variation. 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 D.1). For comparison, Column (5) report the results using two way fixed effects estimator with log minimum wage (equation D.2) shown in Figure 9. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

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

	(1)	(2)	(3)	(4)	(5)
All workers below \$15	0.020	–0.005	0.020	0.003	0.014
	(0.016)	(0.020)	(0.028)	(0.022)	(0.030)
All workers over \$15	–0.012	0.035	–0.109***	0.007	–0.039
	(0.033)	(0.023)	(0.030)	(0.033)	(0.033)
All workers	0.008	0.031	–0.089***	0.010	–0.025
	(0.025)	(0.031)	(0.025)	(0.036)	(0.029)
High probability CK group	0.022*	0.015	–0.014	0.006	–0.010
	(0.013)	(0.010)	(0.009)	(0.012)	(0.010)
Middle probability CK group	0.001	0.019	0.027	0.010	0.026
	(0.014)	(0.024)	(0.035)	(0.020)	(0.023)
Low probability CK group	–0.015	–0.004	–0.103***	–0.006	–0.041
	(0.022)	(0.013)	(0.038)	(0.019)	(0.030)
Event-based	Y				
First-differenced		Y			
Standard TWFE			Y	Y	Y
State-specific treatment	Y	Y	Y	Y	Y
Discrete treatment					
Continuous treatment	Y	Y	Y	Y	Y
State fixed effects	Y		Y	Y	Y
State-specific linear trends				Y	
Base industry/occupation shares					Y

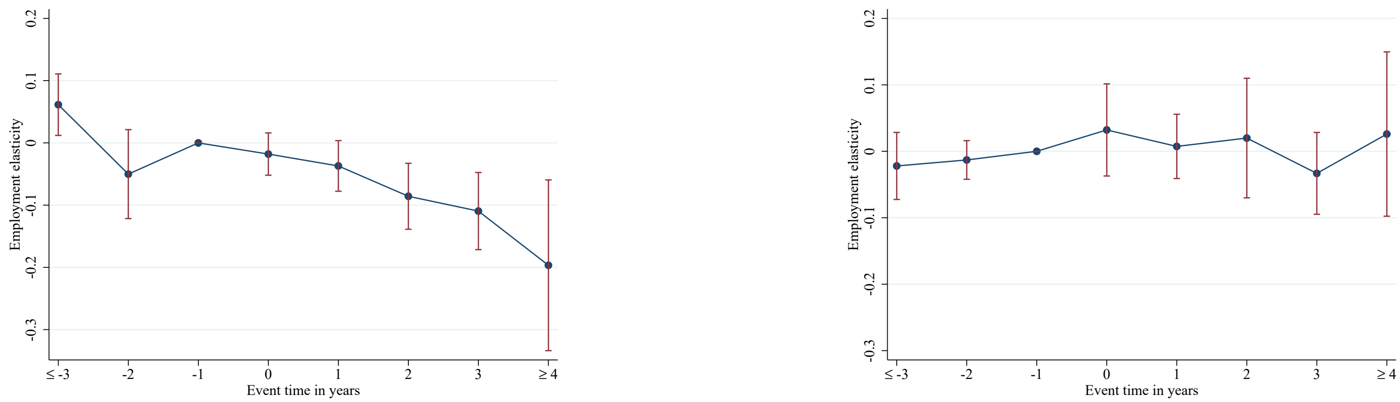
Notes. The table reports estimated employment elasticities of minimum wage from alternative approaches and outcome groups. Each column and row is a separately estimated model specification and outcome group, respectively. Columns 1 shows the results using EB-logMW specification (see equation D.1), Column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

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

	Actual	Post 1990 sample	Excl. states with pre-1990 events		Excl. states with pre-1996 events
	(1)	(2)	(3)	(4)	(5)
Panel A: FE					
Emp. elas. wrt MW	-0.089***	-0.012	-0.107***	-0.073**	-0.073*
	(0.025)	(0.035)	(0.036)	(0.034)	(0.038)
Panel B: FD					
Emp. elas. wrt MW	0.031	0.025	-0.030	0.005	0.012
	(0.031)	(0.033)	(0.021)	(0.014)	(0.010)
Panel C: EB-log(MW)					
Emp. elas. wrt MW	-0.002	0.006	-0.031	0.009	0.009
	(0.021)	(0.031)	(0.021)	(0.006)	(0.007)
Observations	1479	1173	1189	1189	1131
Number of states	51	51	41	41	39
Period estimated	1979-2016	1990-2016	1979-2016	1979-2016	1979-2016
Outcome variable	Actual epop	Actual epop	Actual epop	Simulated epop	Simulated epop

Notes. The table reports the effect of a minimum wage increase on the actual and simulated employment using the fixed effects (FE), first-differences (FD), and the event-based continuous specifications (EB-log(MW)). The simulated outcome variable uses the actual data until 1990, and replaces it with 0 from 1990 onwards. 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-1990 years. The third column employs the entire time span of the data, yet leaves out the states that have a minimum wage event before 1990. The fourth column replaces the actual outcome variable in the previous column with the simulated employment data. The fifth column also uses the simulated outcome variable, 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 averaged population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

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

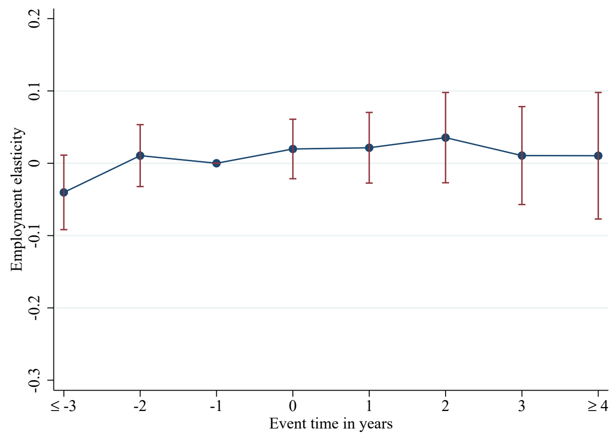


(a) Fixed effects

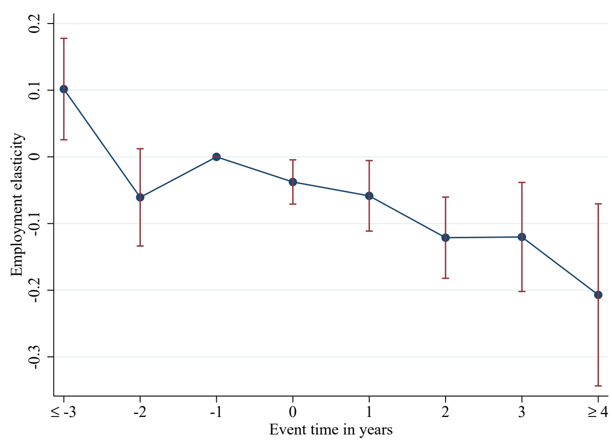
(b) Fixed effects with Linear trends

Notes: The figure shows the effect of the minimum wage on employment over time in the fixed effects (panel (a)), the fixed effects augmented with state-specific linear trends (panel (b)). All panels estimate regressions of state-level employment rate on the state-level contemporaneous log minimum wage, as well as on 4 annual lags and 2 annual leads. The blue markers show cumulative employment elasticities by event date. These cumulative effects are calculated by successively summing the coefficients on leads and lags of log minimum wage, and thendividing them by the sample average employment-to-population rate. Furthermore, the cumulative elasticity at event date -1 is normalized to 0; the elasticity for event dates $\tau \leq -3$ is, therefore, equal to the negative of the (non-normalized) cumulative elasticity at $\tau = -1$. 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 D.2: Impact of Minimum Wages on Lower- and Upper-tail Employment Over Time for Fixed Effects Specification



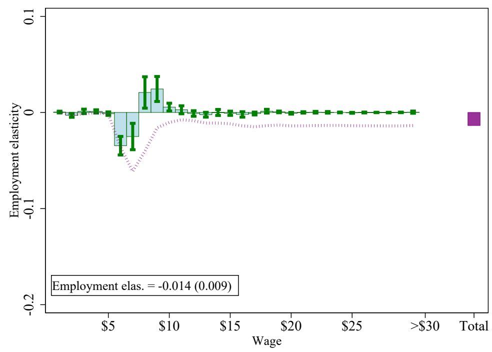
(a) Fixed Effects; Below $15



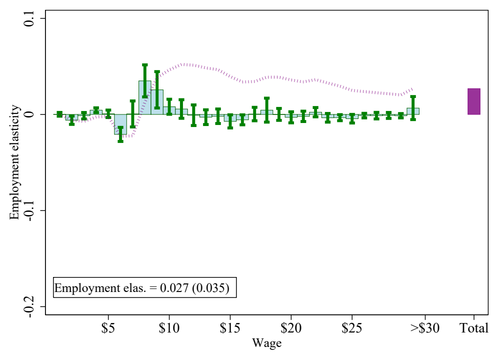
(b) Fixed Effects; At or above $15

Notes: The figure shows the effect of the minimum wage on the number of jobs below (panel (a)), and at or above $15 (panel (b)) over time in the fixed effects specification. Panels (a) and (b) estimate regressions of state-level total number of jobs below, and at or above $15 over state populationon 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 thendividing them by the sample average employment-to-population rate. Furthermore, the cumulative elasticity at event date -1 is normalized to 0; the elasticity for event dates $\tau \leq -3$ is, therefore, equal to the negative of the (non-normalized) cumulative elasticity at $\tau = -1$. 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.

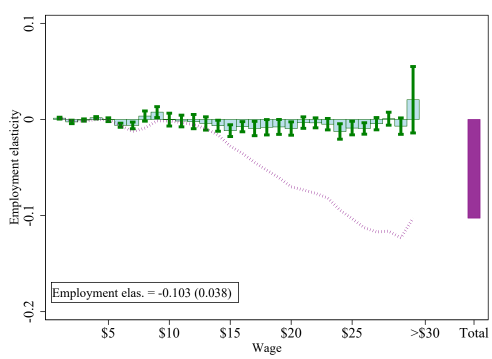
Figure D.3: Impact of Minimum Wages on the Wage Distribution by Predicted Probability Groups for Fixed Effects Specification



(a) High probability group (top 10%)



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



(c) Low probabilty group (bottom 50%)

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