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

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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. 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 conventional studies can produce misleading inference due to spurious changes in employment higher up in the wage distribution.

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1 Introduction

Minimum wage policies have featured prominently in recent policy debates in the United States at the federal, state and local levels. In the past year, two large states (California and New York) passed legislation to increase minimum wages to \$15/hour over the next 5 years. 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 effect, 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 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, the extent to which the 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.

What is the advantage of using the "bunching" method to estimate disemployment effects of the minimum wage? First, we show that there is clear link between bunching and disemployment of the directly affected workers in the standard frictionless model of labor demand. Moreover, the size of the bunching also identifies a crucial structural parameter of labor demand—the elasticity of substitution between various types of labor. Our approach, therefore, extends Saez (2010)—who identifies the compensated elasticity of labor supply in the standard model from bunching of taxable earnings in the neighborhood of tax kinks—and applies it to the context of labor demand.

Second, our approach transparently shows where any disemployment effect may be occurring by tracking employment changes throughout the wage distribution. A large class of theoretical models, including the neoclassical model, the monopsonistic competition model with heterogeneous labor (Butcher, Dickens and Manning, 2012), the Diamond-Mortensen-Pissarades model (Flinn 2011), and the Burdett-Mortensen model with heterogeneous labor (Engbom and Moser 2017, Van den Berg and Ridder 1998), predict that the changes in employment should be concentrated at the bottom of the wage distribution. At the same time, finding unusually large changes in the upper tail employment may reflect problems with the identification strategy and not the causal effect of the minimum wage.

We implement the bunching estimator proposed above by comparing the actual frequency distribution of wages observed in the data to our estimates for the counterfactual distribution. We deviate from the previous literature that constructed this counterfactual using either *ad hoc* functional forms (Meyer and Wise 1983) 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 also find an equally sized bunching between hourly wages of \$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. To overcome this challenge, we use hourly wage data from 1979-2016 Current Population Survey (CPS) to estimate the impact of state-level minimum wage increases.[2] 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%). Interpreting this lack of an employment response using the standard, frictionless labor demand model implies an elasticity of substitution between different types of labor that is close to zero.

Our estimates also allow us to calculate the impact of the policy on the average wages of affected workers, which rise by around 6.8% (s.e. 1.0%). The significant increase in average wages of affected workers implies an employment elasticity with respect to the wage (or the labor demand elasticity in a competitive model) of 0.41 (s.e. 0.43), and rules out elasticities more negative than -0.43 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. Both the missing jobs below the new minimum and the 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 (Sorkin 2015). Moreover, the excess number of jobs closely matches the evolution of missing number of jobs, which highlight that the employment effect of the minimum wage is similar in the short and in the longer run.

Our estimates are highly robust to a wide variety of approaches to controlling for time varying heterogeneity

[2]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 policy. Moreover, in Online Appendix D, 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.

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 also shows similar findings. We also show that our results are robust to focusing only on the events occurring in the states that do not allow tip credits; to dropping occupations that allows tipping; to using full-time equivalent job counts; or to 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 directly test for such labor-labor substitution, we partition workers into groups based on 4 education and 6 age categories. The comparison across age-by-education groups of excess jobs at and above the new minimum wage and missing jobs below it 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. 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 labor-labor substitution between these groups played a limited role and the benefit of minimum wage policies were equally shared.

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 large negative effect on employment in tradable sector, and manufacturing in particular—with an employment elasticity with respect to own-wage of -1.4—though the estimates are imprecise. At the same time, the effect of the minimum wage is close to zero in the non-tradable, restaurants, retail and other sectors—which together comprise the vast majority of minimum wage workers in the U.S. today. This evidence suggests that the industry composition of the local economy is likely to play an important role in determining the disemployment effect of the minimum wage (Harasztosi and Lindner (2016)).

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 spillover effects up to $4 dollar above the minimum wage, we do not find any evidence of spillovers for the new entrants. This asymmetry suggests that 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).

Finally, 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, excess jobs are also larger 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.

The 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 provide such an aggregate employment estimate in this paper, but our bunching 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 clarify the importance of workers far above the minimum wage, we also calculate the bin-by-bin employment effects using continuous minimum wage variation and an approach similar to Meer and West (2016). This exercise produces a striking finding: 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.[3] Our bunching approach also has advantages over methods that assess the effect of the minimum wage by pre-minimum wage hourly wage (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

[3] 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, similar to what we do in this paper. 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 0.46, as compared to 0.49 for the state of Washington in 2000 after the minimum wage increase we study. Instead, the discrepancy in findings highlights the importance of using many events for inference instead of relying on one particular minimum wage change.

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. 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 $4 above the minimum wage and represent around 40% of the overall wage increase from minimum wage changes. These estimates are similar to what Autor, Manning and Smith (2016) found in their recent analysis, and more limited than Lee (1999). Autor, Manning and Smith (2016) also demonstrate that spillover effects cannot be distinguished from wage misreporting in the 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 the in the short run, spillover effects are mainly driven by those who were employed before the minimum wage increase, while workers who moved from non-employment did not benefit from spillovers. [4]

Third, our paper also 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 limited 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 techniques to learn about behavioral responses to public polices (Kleven 2016). Our approach identifies the elasticity of substitution between different types of inputs in the frictionless model of labor demand. In this way, it complements Saez (2010) and Chetty, Friedman and Saez (2013) who use bunching to identify the labor supply elasticity in the frictionless model. However, while the standard bunching techniques estimate the counterfactual distribution from purely cross sectional variation, we are able to use information from states without minimum wage changes to construct the counterfactual wage distribution in absence of the minimum wage shock. In this way, we combine difference-in-difference and bunching approaches, and assess the robustness of the counterfactual using standard program evaluation methods.

The rest of the paper is structured as follows. Section 2 explains the bunching approach, and shows how it produces consistent estimates of the employment effect under the standard labor demand model. 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, heterogeneous effects by

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

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 compares our findings to those from a classic two-way fixed effects estimator using aggregate employment, and shows how that approach can sometimes reflect unrealistic movements in employment in the upper tail. Section 6 concludes.

2 Methodology

In this section, we first use a standard model of labor demand under perfect competition to derive employment and wage effects of a minimum wage, and relate them to our bunching estimator. We also discuss some implications for deviations from perfect competition. Subsequently, we describe the empirical implementation of our estimator using a difference in difference approach with state level variation.

Bunching in the standard labor demand model. We consider the standard model of labor demand with a continuous distribution of skill types to assess the employment effect of the minimum wage throughout the wage distribution. We abstract from changes in aggregate production and derive the effect of the minimum wage on the conditional labor demand function. This assumption simplifies the presentation with little cost, since the scale effects are negligible in the context of U.S. minimum wages. In Online Appendix B, we present results for the unconditional labor demand function that factors in such scale effects.

We assume that firms face a CES production function

$$Y = \left(\int_{\underline{w}}^{\overline{w}} \phi_j l_j^{\frac{\sigma-1}{\sigma}} dj \right)^{\frac{\sigma}{\sigma-1}}$$

where l_j is the quantity of type j workers used in production and ϕ_j is their productivity. We assume that at wage w_j, there is a perfectly elastic supply of type j labor, e.g., firms can increase or decrease labor demand without affecting wages. This simplifying assumption is analogous to the one made in Saez (2010), who studied behavioral responses to tax kinks in the frictionless labor supply model. Focusing on labor supply, Saez (2010) assumed that labor demand is perfectly elastic at a given wage. Since we focus here on understanding labor demand, we make an analogous simplifying assumption of perfectly elastic labor supply for each worker type. While this assumption produces a clear expression for the size of the bunch, it is not necessary for producing consistent estimates for the impact of minimum wages on low-wage employment. In Online Appendix B, we derive the expression for the general case with an inelastic labor supply.

Cost minimization subject to a budget constraint leads to the standard conditional labor demand function (see Online Appendix B):

$$l_i = Y c(w)^{\sigma} \left(\frac{\phi_i}{w_i} \right)^{\sigma} \tag{1}$$

where $c(w) = \left(\int_{\underline{w}}^{\overline{w}} \phi_j^{\sigma} w_j^{1-\sigma} dj \right)^{\frac{1}{1-\sigma}}$ is the unit cost of production. Therefore, a higher wage w_i reduces labor demand for type i workers, while a higher overall wage level (as measured by a higher $c(w)$) increases it.

As shown in Appendix Figure A.1, with the introduction of a minimum wage, MW, all types of workers with wages below MW who remain employed are pushed up to the new level, creating a spike at the new minimum wage. The increase in the wages of directly affected workers leads to a drop in conditional labor demand; the size of this drop is directly related to σ, the substitution elasticity across various types of labor. When σ is large, it is easy to substitute workers at the bottom of the wage distribution with workers at the top. This will lead to job losses among many low-wage workers, and produce a relatively small spike at the minimum wage. In contrast, when σ is small, such substitution is more difficult, allowing more low-wage workers to keep their jobs and creating a larger spike at the minimum wage. Therefore, the size of the spike relative to the workers directly affected by the minimum wage reflects the effect of the minimum wage on the targeted low-wage population.

Equation 1 also implies that demand for workers above the minimum wage shifts as a result of an increase in the unit product cost, $c(w)$. This is because the cost increase of low-wage labor pushes up the demand for high wage labor. The size of the shift is again related to the elasticity of substitution, σ. If substitution plays a limited role, then the spike at the minimum wage will be large, and we should expect no change at the upper tail at the minimum wage. In contrast, if the substitution elasticity is large, then we expect a small spike at the minimum wage and greater increase for higher wage workers. This implies that by focusing on the bottom of the wage distribution, we may *overestimate* the total job loss across all groups. As a result, if the goal is to estimate the impact of minimum wages on the total number jobs in the labor market, the size of the spike relative to the missing number of jobs will serve as a lower bound.[5] However, the extent to which we wish to estimate the employment change of lower wage workers who are the target of the policy, the comparison of employment at the spike relative to the missing number of jobs is precisely the estimate we want.

Our empirical approach identifies the employment effects by exploiting variation in the minimum wage. We calculate the sum of the change in the size of the spike (or the number of excess jobs) at the minimum wage, $\triangle a$, and the change in the number of missing jobs below the new minimum wage, $\triangle b$; this sum $(\Delta a + \Delta b)$ identifies the employment effect of the minimum wage on low-wage workers. In Online Appendix B,

[5] As a practical matter, however, by varying the upper wage limit for the excess mass, we empirically assess the scope for any such bias, and do not find any for our estimates.

we show that the employment elasticity with respect to the minimum wage is given by the following formula:

$$\frac{\%\triangle e}{\%\triangle MW} = \frac{(\triangle a + \triangle b)}{b} \frac{1}{\%\triangle MW} = -\sigma(1 - s_{MW}) \tag{2}$$

where $s_{MW} = \frac{\int_{\underline{w}}^{MW} a_j^{\sigma} MW^{1-\sigma} dj}{\int_{\underline{w}}^{MW} a_j^{\sigma} MW^{1-\sigma} dj + \int_{MW}^{\overline{w}} a_j^{\sigma} w_j^{1-\sigma} dj}$ is the cost share of minimum wage workers, and $\underline{w}$ is the lowest wage prevailing in the free market equilibrium.

The formula highlights that when $MW \approx \underline{w}$, then $s_{MW} \approx 0$ and the size of bunching is approximately equal to $-\sigma$. In that case, our estimator directly identifies the uncompensated elasticity of substitution across worker types. A large spike indicates that the employment change $\%\triangle e$ is small, and so is the substitution elasticity, σ. In contrast, if there is no bunching at the minimum wage, then $\%\triangle e$ is large and so is σ. Moreover, even if MW is above $\underline{w}$, the cost share of the minimum wage workers, s_{MW}, is very small in practice. In our sample around 8.6% (see Column 1 in Table 1) of the workers are directly affected by the minimum wage and the minimum-to-mean wage ratio is around 0.25, which indicates that $s_{MW} = 0.25 \times 0.086 = 0.02$. Therefore, the bunching estimates on employment will be closely related to σ.

When labor supply is not perfectly elastic, then employment changes for higher wage workers will be more muted, as some of the effects will materialize as higher wages instead of higher employment. This increase in wages will dampen the pressure on low-wage workers. As a result, the relationship between the size of the spike and the elasticity of substitution will be weaker. We derive the expression for the employment elasticity in the more general case in Online Appendix B, allowing for finite labor supply elasticities.[6] However, we show that when the share of minimum wage workers is low, like in the U.S., the effect of the minimum wage on the upper tail wages will be small, and independent of the labor supply elasticity. As a consequence, equation (2) remains a valid approximation for the general case.

Estimating employment effects from bunching. The standard labor demand model predicts that the employment effects of the minimum wage on low-wage workers must be negative (though they can be small in magnitude). Introducing frictions in the labor market can make the effect of the minimum wage on employment ambiguous (Flinn 2011; Manning 2003). Moreover, the presence of frictions can also generate some spillover effects on wages that are concentrated around the minimum wage. For instance, in Flinn (2011), a minimum wage induces some low-wage workers to participate in job search, and some of them may find a job above the minimum wage. Alternatively, in Manning (2003) or Van den Berg and Ridder (1998), firms paying above the minimum wage must raise wages under the new equilibrium. However, most of these

[6]We also show that when labor supply is elastic, then some of the workers initially earning below the minimum wage will see their wages pushed up somewhat above the minimum. This provides an additional reason for using changes in jobs paying slightly above the minimum wage when the number of excess jobs are considered, even if we wish to estimate the employment change of low-wage workers who were directly affected by the minimum wage.

effects are likely to fade out as 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).

To allow for spillover effects, we measure the extent of bunching at and slightly above the minimum wage. This situation is shown in Figure 1, where all spillover effects fade out by wage $\bar{W}$, and the number of excess jobs is the change in jobs between MW and $\bar{W}$, formally $\triangle a = Emp^1 \left[MW \leq w < \bar{W}\right] - Emp^0 \left[MW \leq w < \bar{W}\right]$.[7] Since we do not know $\bar{W}$ *a priori,* we will present robustness checks using various realistic values.

In practice, there may be some measurement error in reported wages. As a result, some of the jobs that are paying exactly the minimum wage will appear to be paying slightly above or below it Autor et al. (2016). This provides another reason why the excess mass estimate should include jobs somewhat above the new minimum wage, and not solely the jobs exactly at that level. Moreover, in the presence of reporting error in wages, not all workers below the minimum wage will disappear from the wage distribution. Therefore, the change in the number of directly affected jobs, $\triangle b = Emp^1 \left[w < MW\right] - Emp^0 \left[w < MW\right]$, may be smaller than the number of directly affected workers denoted by b in Figure 1.[8]

The measure of missing jobs below the new minimum wage (Δb) is arguably the most natural way to assess the bite of a minimum wage increase. In the absence of any missing jobs, it is difficult to infer that any observed employment changes are associated with minimum wages. However, this measure of missing jobs is rarely reported in the existing literature. The most common alternative measure is the change in average wage for particular groups, which can be contaminated by upward or downward shifts above the minimum wage.

We assess the employment effect of the minimum wage on low-wage workers by adding up the missing number of jobs and excess number of jobs $\triangle b + \triangle a$. It is easy to see that this sum is in fact equal to the employment change below $\bar{W}$: $\triangle b + \triangle a = Emp^1 \left[w < \bar{W}\right] - Emp^0 \left[w < \bar{W}\right]$. The key idea behind the bunching estimator, therefore, is to focus on employment changes at the bottom of the wage distribution, and employment shifts at the upper tail are not used for identification. However, it is important to emphasize that we not only estimate the sum of $\triangle b + \triangle a$, but we also separate the shifts in employment below and above the new minimum wage. As a result, we can detect the missing number of jobs and the excess number of jobs even if no employment change is found.

There are two key advantages of focusing on the bottom of the wage distribution. First, employment changes in the upper tail are unlikely to reflect changes in employment of the low-wage workers who are the primary target of minimum wage polices. This is particularly important in the standard labor demand model

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

[8] The presence of sub-minimum jobs can also come from imperfect coverage, where employers are allowed to pay below the minimum wage, or from imperfect compliance with the policy.

with labor-labor substitution presented above, as there the aggregate employment effects can be substantially smaller in magnitude than the actual effect on low-wage workers. Second, focusing on bunching alleviates the bias caused by confounding factors that may impact employment estimates at the upper tail of the wage distribution. For instance, state level trends in skill biased technical change or tax policy changes that disproportionately affect higher wage workers can be correlated with minimum wage changes. The potential bias from these confounding factors is especially large when only a small fraction of the workforce is directly affected by the minimum wage (as in the U.S.), since the contribution of these omitted variables would be sizable compared to the relatively small expected effect of the minimum wage on aggregate employment.

3 Washington State Case Study

To implement the bunching method proposed here, 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.[9] 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). Here we improve upon these research designs by implementing a difference-in-differences style estimator.

In particular, we discretize the wage distribution, and count employment for each dollar wage bin k. We normalize these counts by the pre-treatment aggregate employment in Washington, $e_{WA,k,Post} = \frac{E_{WA,k,Post}}{E_{WA,Pre}}$. We calculate the post treatment counterfactual wage distribution for each wage bin, $e^{CF}_{WA,k,Post}$, by adding the (population-weighted) average per capita employment change in the 39 states that did not experience a minimum wage increase during the 1998-2004 time period to the Washington state's pre-treatment per-capita wage distribution. After the appropriate normalization, this leads to the following expression:

[9] Washington State 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 of 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.

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

where $\frac{E_{skt}}{N_{s,t}}$ 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. 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, the pre-treatment Washington wage distribution, we use administrative data set on hourly wages. However, in the 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 highlights that in the absence of the minimum wage increase, there would have been more jobs between \$7 and \$9, but less jobs at and above \$9. 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.[10] Such a relationship between the actual and counterfactual distributions closely resembles the illustration of the bunching method shown in Figure 1.

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

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

$$e_{WA,k,Post} - e^{CF}_{WA,k,Post} = \underbrace{\frac{1}{\frac{E_{WA,Pre}}{N_{WA,Pre}}}}_{\text{normalization}} \times \left[\underbrace{\frac{E_{WA,k,Post}}{N_{WA,pre}} - \frac{E_{WA,k,Pre}}{N_{WA,pre}}}_{\text{Change in treatment}} - \underbrace{\sum_{s \in Control} \frac{1}{39} \left(\frac{E_{WA,k,Pre}}{N_{s,Pre}} - \frac{E_{WA,k,Post}}{N_{s,Post}} \right)}_{\text{Change in control}} \right] \quad (3)$$

which is a classic difference-in-difference 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-difference 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, $\sum_{k=\$5}^{\$9} e_{WA,k,Post} - e^{CF}_{WA,k,Post}$, between \$5 and \$9, 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%.[11]

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 rebounds close to 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 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.[12] Once the minimum wage is raised in two steps between

[11] We will discuss the details of how we exactly calculated the percentage change in employment and wages in the next section.
[12] 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

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.

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-difference 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-difference approach as our Washington case study (e.g., equation 3). Like other difference-in-difference estimators, equation (3) 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 dataset; the details of how this is constructed is explained in Section 4.2. Using this data, we examine the effect of minimum wage events on 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 as k a \$1 interval relative to the new minimum wage, 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

controls states and hence restored the difference in excess and missing jobs prior to Washington's state minimum wage increase in 1999 and 2000.

$k \in \{0, 1, 2, 3, ..., 17\}$, where $k = 17$ includes jobs that pay \$17 above the new minimum wage or more.

Our event-study estimates of the effect of minimum wages are based on the following regression specification:

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

Here $I_{swt}^{\tau k}$ is an indicator variable that is equal to 1 if the minimum wage was raised τ years before 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, μ_{sw}, and period-by-wage bin effects, 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-difference estimate, as it nets out the change in the counterfactual distribution implicitly defined by the regression equation (4). Analogously, the change in the number of jobs (per capita) paying between the minimum and up to $\overline{W}$ dollars 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 the choice of this cutoff. We define the excess employment 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 employment below as $\Delta b_\tau = \frac{\sum_{k=0}^{4} \alpha_{\tau k} - \sum_{k=0}^{4} \alpha_{\tau k}}{\overline{EPOP}_{-1}}$. 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 $\Delta b = \frac{1}{5} \sum_{\tau=0}^{4} \Delta b_\tau$.

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, ϵ:

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

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,

$\overline{b}_{-1}$.[13]

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

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 $w = \left(\overline{wb}_{-1} + \Delta wb\right) / \left(\overline{b}_{-1} + \Delta e\right)$.[14] Therefore, the percentage change in the average wage is given by:

$$\%\Delta w = \frac{w}{\overline{w}_{-1}} - 1 = \frac{\frac{\overline{wb}_{-1} + \Delta wb}{\overline{b}_{-1} + \Delta e}}{\frac{\overline{wb}_{-1}}{\overline{b}_{-1}}} - 1 = \frac{\%\Delta wb - \%\Delta e}{1 + \%\Delta e} \tag{5}$$

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 change 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 (1) allow bin-by-division-by-period fixed effects that allow for regional time-varying heterogeneity by wage bin and (2) allow bin-by-state-specific linear and quadratic time trends that also allow for richer trends by wage bin. These allow for richer time-varying heterogeneity in the earnings distributions across states. We also

[13]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 below share is only known after the minimum wage increase, and so it cannot be used for a prospective analysis of the policy's impact.

[14]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 $[MW', MW'+1)$.

estimate a "triple-difference" specification which includes controls for state-by-period fixed effects, which nets out any aggregate state-specific employment shocks.[15] This is a rich specification, which also highlights the advantage of our approach that 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; 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.[16] 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, takes into account that employment changes at different parts of the wage distribution may be correlated within a state.[17]

4.2 Data and sample construction

To implement the bunching estimator, we rely mainly on two data sets. The first one is the 1990-2015 quarterly administrative employment data from Washington State. The state requires all employers to report data on hours and wages as part of the unemployment insurance (UI) payroll taxes. The data is micro-aggregated, and includes information on the total employment count and total hours worked at 5-cent hourly wage bins. Workers with hourly wages greater than $50 are censored for anonymity purposes. We use this data for assessing the Washington minimum wage changes in Section 3.

The second one is the individual-level NBER Merged Outgoing Rotation Group of the Current Population Survey for 1979-2016 (CPS). Using it, we calculate quarterly, state-level distribution 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.[18] There are no reliable imputation data for

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

[16]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 takes on 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.

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

[18]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

January 1994 through August 1995, so we exclude this entire period from our sample. Our available sample of employment counts therefore spans 1979q1 through 1993q4 and 1995q4 through 2016q4.[19] We use the CPS data to implement the pooled event study analysis.

We deflate wages in both of the data sets 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.[20] For each of these 117 wage bins we collapse the data into quarterly, state-level employment counts E_{swt} using the counts from the administrative data (for the Washington case study) or person-level ORG sampling weights (for the pooled event study analysis). To account for population changes, we calculate quarterly, state-level per-capita employment by dividing the employment counts by the weighted population count P_{st} of all ORG respondents (regardless of employment status). 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). Using the QCEW benchmark has little effect on our point estimates but largely increases their statistical precision.[21]

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).[22] Appendix Figure A.2 shows that during our CPS sample period (1979-2016) there are at most 516 minimum wage increases, where markers indicate all changes in the state or federal minimum wage, and grey, vertical lines illustrate the

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).

[19] In general, there has been in increase in the rate of imputation over time. However, event study estimates of imputation share as the outcome does not indicate a substantial or statistically significant change in the imputation rate in treated states following the treatment events. (See Online Appendix Table A.2 and Online Appendix Figure A.6.)

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

[21] 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 (1) 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.

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

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, only identified from time-series variation for these events as there are no "control states" with wage floor lower then the new minimum wage. We additionally 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, for our 138 events, 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 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, approximately 7 workers each quarter in each of the \$0.25 bins between \$5 and \$15/hour in our sample.[23] Since the coefficients for our event dummies are estimated at a \$1-bin-year-state level, on average, for each of these we use approximately 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 reported in columns (6) and (7) Appendix Table A.6. 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 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 above minimum wage will appear 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 effect of minimum wages on reported wages are below $\overline{W}$. The reason is straightforward. Assume that 1% of the workforce mistakenly report earning below the new minimum wage

[23] 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.

in the post-treatment period. This would lead our estimate of the missing jobs to be too small in magnitude: $\hat{\Delta b} = \Delta b + 0.01$. However, this misreporting would also lead to an equal reduction in the number of excess jobs above, producing the estimate $\hat{\Delta a} = \Delta a - 0.01$; this will be true as long as these misreported workers are coming from the range $[MW', \overline{W})$, which is likely to be satisfied for a wide variety of classical and non-classical measurement error processes where the support of the measurement error is contained in [MW'-$\overline{W}$,$\overline{W} - MW'$]. Therefore, the employment estimate $\hat{\Delta a} + \hat{\Delta b}$ is likely to be unaffected by measurement error in reported wages.

We also directly assess how misreporting of wages in the CPS may affect our results in Online Appendix C, where we compare the CPS hourly wage distribution to micro-aggregated administrative data on hourly wagea 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 matter 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 et al. (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 likely 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 wage rates. Figure 4 shows the results from our baseline specification with wage-bin-period and wage-bin-state fixed effects (see equation 4). We report employment changes averaged over the five year post-treatment period , $\frac{1}{5}\sum_{\tau=0}^{4}\alpha_{\tau k}$, for each dollar wage bin (k) relative to the minimum wage. Recall that all employment changes are 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. More than $\frac{3}{4}$ of this reduction occurs in the

$1 wage bin just under the new minimum.[24] 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, $2 and $4 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. 3%) of the total pre-treatment employment.[25] 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 the the red line representing the running sum of employment changes.

The bunching estimator proposed here counts the missing jobs below and excess jobs above the minimum wage, $\triangle a + \triangle b$. This is 0.28% (s.e. 0.29%), which is positive and statistically insignificant. Given that the average minimum wage change is 8.4% in our sample, this implies that the employment elasticity with respect to the minimum wage is 0.024 (s.e. 0.025). This estimates is statistically insignificant and we can rule out the aggregate employment elasticity of -0.074 calculated by Meer and West (2016) (see the baseline estimate in their Table 4). Second, using the formula in equation 5 we can also calculate the change in average wage and the employment elasticity with respect to own wage, which is the labor demand elasticity in the competitive model. We estimate that the effect of the minimum wage on average wages is 8.8% (s.e. 1.3%), which is highly significant. The estimate for the elasticity of employment with respect to own wage is 0.41 (s.e. 0.43). While the standard error of 0.43 makes this somewhat imprecise, it nonetheless rules out any own-wage elasticities more negative than 0.43 at the 95 percent confidence level.

Figure 4 shows visually 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.

[24] The discrepancy between the actual number of jobs below the new minimum, which is 8.5% 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 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 $b_{-1} = 0.10$ or around 10% 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) declines from 15% to 10% relative to Washington's pre-treatment total employment, which accounts for the gap between Δb and b_{-1}.

[25] In Appendix Table A.1 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.

There are four important findings that we would like to highlight. First, we find a very clear reduction in the jobs paying below the new minimum wage (shown in red) between the year just prior to treatment ($\tau = -1$) and the year of treatment ($\tau = 0$)—this shows that the minimum wage increases under study are measurably binding. Second, while there is some reduction in the magnitude of the missing jobs in the post-treatment window, it continues to be very substantial and statistically significant five years out, showing that the treatments are fairly durable over the medium run. 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 of affected worker 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 total employment of workers affected by the policy even as there is clear evidence that the policy has bite, and 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. This evidence is broadly consistent with the standard (frictionless) labor demand model presented in Section 2 with substitution elasticity σ close to zero (see equation 2). However, the standard labor demand fails to predict the limited spillover effects slightly above the minimum wage. This can reflect a distance-based substitutability of workers (Teulings 2000), presence of some frictions in the labor market, or relative-pay constraints within a firm. We discuss the size and nature of these spillovers in greater detail in Section 4.5.

Robustness Checks. In Table 1we 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.[26] 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.[27] 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, column (7) and (8) uses 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. The bite is modestly smaller ($\Delta b = -0.016$ or -0.015) when we consider only variation within Census divisions. 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 5.5% and 6.8% with common wage-bin-period effects, and between 4.3% and 5.0% with wage-bin-division-period controls. In contrast, the proportionate change in employment for affected workers is never statistically significant, and is numerically much 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 -0.385 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

[26]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 lossses 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.

[27]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.

to wage are -0.45 (s.e. 0.57) . 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. 0.46).

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.[28] 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, and may not be fully bound by the minimum wage changes. Although the average percentage increase in the minimum wage and the share of workforce earning below the new minimum wage are similar to those in the primary sample of events (0.093 instead of 0.101, respectively) 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 a larger number 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 -0.13 (s.e. 0.53).

Our analysis so far have 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 average bite (Δb) to be slightly larger 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 obtains even when we consider federal increases.

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

Effect by different workforce definitions. Thus 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 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 less hours. Consistent with this, the fall in jobs below the minimum wage is also lower 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 (9.4% versus 6.8% in Table 1) for this subset of workers than the overall sample. This suggests that measurement error in wages are less prominent for those who directly report their hourly wages. Nevertheless, the employment elasticities with respect to the minimum wage (0.029, s.e. 0.022) and with respect to the own wage (0.31 s.e. 0.39) 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 (tips, which may push these workers' hourly earnings above the minimum, are not captured in the hourly wage reported by hourly tipped workers). 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 diminishes the average bite, $\bar{b}_{-1}$= 0.061), while the missing jobs stays the same as in our benchmark estimate (1.6% vs. 1.8% in Table 1). Consequently, estimated wage effects are larger by around 20% (8.5% versus 6.8% in Table 1). However, excluding tipping workers has a negligible impact on the employment estimates: the employment elasticity with respect to the own wage is 0.33 (as opposed to 0.41 in Table 1).

Further robustness checks. In column (6), we present estimates using the raw CPS data, not the QCEW benchmarked CPS. The change in the number of jobs paying below the new minimum is 0.018, essentially the same as the baseline estimate. The wage and employment estimates (0.077 and 0.046) as well as the employment elasticities with respect to the minimum wage and own wage (0.039 and 0.590) are larger positive numbers. The benefit of the use of QCEW benchmarked CPS reveals when we examine the precision of the estimates. The confidence intervals of the former and the latter elasticities are 44% and 23% larger than those in column 1 of Table 1.

Finally, in column (7) we provide estimates without using population weights. These results are virtually identical to our benchmark estimates (Column 1 of 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 re-assuring, since a substantial difference between the two could reflect potential mis-specification (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 for the 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.[29] Such labor-labor substitution at the bottom of the wage distribution would make minimum wage polices 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 4). As expected, restricting the sample by education and age produces a larger bite. For example, for those without a high school degree, the jobs below the new minimum, Δb, changes by -6.5% relative to its pre-treatment employment, and for those with high school or less schooling

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

the change 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%), female (-2.3%), and black or Hispanic (-2.8%) workers all see significant and relatively larger changes in the missing jobs below the new minimum.

While there is large variation in the missing jobs across various demographic groups, the excess jobs above the new minimum wage closely match that, with an opposite sign. We highlight this in panel (a) 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 -0.099 and 0.595 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 0.493 (s.e. 0.29) 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 0.127. 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).

We also directly 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.[30] 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

[30] Education categories are, high school dropout, 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.

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 (such as high school dropouts between 30 and 40 years of age) are above the 45 degree line, others (such as high school dropouts 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, specific sectors like restaurants have been studied because the policy is much more binding in that industries than as a 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.[31]

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.[32] 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.[33] The minimum wage is less binding in that sector, which is reflected in both the number of jobs below the new minimum wage ($\bar{b}_{-1}$) and in the change of that ($\triangle b$). 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% (s.e. 13%), albeit not statistically significant. Our estimates on wages are also noisy (5.8%, s.e. 7.3%), and so the employment elasticity with respect to own wage is large in magnitude at -1.91 (s.e. 3.92) but very imprecisely estimated.

Column (3) highlights that minimum wage is more binding in the non-tradable sector, where the the

[31] 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.

[32] 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; the non-tradable sector is equal to restaurant and retail; construction consists of construction, real estate or land development-related industries; and the remaining industries fall into the "other" sector. 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 C.

[33] 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 C).

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. 0.59). 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, using revenue data, that the larger job losses in the tradable sector reflects more elastic consumer demand than in the non-tradable 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 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 slightly smaller than the estimates with all industries (-1.1% vs. -1.9% in column 1). Moreover, the missing jobs are fully offset by the excess jobs at and slightly above the minimum wage, and so the employment elasticity with respect to own wage is slightly positive at 0.16 (s.e. 0.76).

We additionally present separate results for the retail, restaurant, and manufacturing sectors. Column (6) shows that the the number of jobs below the new minimum wage in the restaurant sector is 10.1% (s.e. 0.3%) of pre-treatment (restaurant) employment, which is the largest missing jobs estimate among the sectors studied here, highlighting why restaurants are studied so frequently in the literature. Moreover, excess jobs ($\Delta a = -0.101$) are similar in size to the missing jobs and so there is little net change in restaurant employment. These small effects agree with other recent work that obtain 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 1.04 (s.e. 1.58). In contrast, in the manufacturing sector in column (8), the employment declines in response to the minimum wage in magnitude that is comparable to the tradable sector the point estimate suggest 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 elasticity is quite large at -1.38 (s.e. 2.95), though these. estimates are imprecise and statistically insignificant.

In summary, our point estimates are consistent with more negative employment effects in the tradable rather than 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 labor force status. We consider the effect of the minimum wage separately on workers who were employed prior to the minimum wage increase 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 to test whether minimum wage laws mainly affects job creation and not job destruction 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.[34] 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, and 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 α_{1k} for each dollar wage bin (k) relative to the minimum wage. 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 change in missing jobs is slightly larger for incumbents (1.2%, 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%) 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 effect 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 for the new entrants and for the 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 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

[34]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" or CPS), respondents are asked questions about wages. Appendix Online Appendix C explains how we match workers across rotation groups.

(Abowd et al. 2000; Currie and Fallick 1996; Clemens and Wither 2016).

4.5 Wage spillovers

One key advantage of estimating the impact of the 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 die 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 rapidly decline 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:

$$\%\Delta w_{\text{no spillover}} = \frac{\sum_{k=-4}^{-1} k\left(\alpha_k - \alpha_{-1k}\right)}{\overline{wb}_{-1}} \tag{6}$$

The total wage increase of affected workers $\%\Delta w$ in equation (5) 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 esetimates 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%). Column (2) shows that in the absence of spillovers, wages would increase by 4.1% (s.e. 0.9%). Column (3) shows that around 40% (11%) 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%), which is less than half of the 39.7 (s.e. 11%) spillover share for all workers–although the differences are not statistically significant. Notably, we find a substantially smaller change in wages due to spillovers for the tradable industry, where 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 are 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 mis-reporting by survey respondents.[35] Moreover, the stark differences in the size and scope of spillovers for the incumbent and for the new entrants is inconsistent with a simple measurement error process common to both groups, and suggest that at least some of the measured spillover increases are likely to be real.

Second, as we explain in Online Appendix B, the standard labor demand model with heterogenous workers can explain wage spillovers only through substantial substitution away from lower-paid towards higher-paid workers, which is inconsistent with a lack of employment effect at the bottom of the wage distribution, the lack of labor-labor substitution between lower-wage groups along observable dimension, and the lack of responses in the upper tail of the wage distribution. This suggests that spillovers are likely to reflect some

[35] In the Online Appendix C 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.

frictions in the labor market.

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 which may mitigate pay compression. This is consistent with findings in Dube, Giuliano and Leonard (2015), who study the wage adjustment using payroll data from 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 (4). 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:

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

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

Here $\overline{u}^a_j$ is the sum of the residuals in the five 1-dollar wage bins at or above the new minimum wage MW'_j, averaged over the post-treatment window, while $u^a_{-1,j}$ is the sum of the residuals in the same five 1-dollar wage bins during the 1 year prior to the minimum wage change. This mirrors the way the fitted values are used to construct the average response Δa. We construct the missing jobs estimates, Δb_j, in an analogous fashion.[36]

[36]Formally, we can write out the estimates as:

$$\Delta a_j = \Delta a + \frac{1}{EPOP_j}\left(\frac{1}{5}\sum_{\tau=0}^{4}\sum_{k=0}^{4}\left(I^{\tau jk}_{swt}\cdot u_{swt}\right) - \sum_{k=0}^{4}\left(I^{-1jk}_{swt}\cdot u_{swt}\right)\right)$$

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 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 by 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, $Kaitz_j = \frac{MW'_j}{\text{Median wage}_j}$.[37]

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. [38] 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.[39] 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). [40] 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

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

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 (4).

[37]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 finds that spillovers fade out at the 25th percentile in the U.S. context.

[38]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 to estimating event-specific weights, which are used the regression of Δb_j, Δa_j and Δe_j on $Kaitz_j$ and other covariates (Kinsler, 2016). 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 (4) and construct $\Delta a_{jm}, \Delta b_{jm}$ and Δe_{jm} for each replicate m and event j. We then take 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.5, 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.5, 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.5; these tend to suggest a somewhat more positive relationship between the $Kaitz$ and employment effect.

[39]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.8.

[40]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.

relative to the median, the events have a bigger bite and a greater number of missing jobs below the new minimum, but also have a nearly equally sized number of excess jobs at or above the new minimum. As a consequence, the employment effect is virtually unchanged as we consider minimum wages that range approximately between 40 and 55 percent 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, and the decade of the minimum wage increase) do little to affect the relationships between the $Kaitz$ and excess jobs, missing jobs, or the change in employment.[41] Overall, these findings suggest that that the level of the minimum wage increases in the U.S. that we study—which range between 37% and 59% of the median wage—have yet to reach a point where the employment effects become sizable. At the same time, our sample includes 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 policy experiments for more elevated minimum wage standards, 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

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 standard model with a low substitution elasticity (see Section 2) and also with segmented labor markets in a search and matching model where the effect of the minimum wage fades out at higher wages.

In this section, we further explore the effect of the minimum wage on the wage distribution using alternative identification strategies. In the recent empirical literature using the classic two-way fixed effect specification with log minimum wage, large aggregate disemployment estimates are found in the U.S. context (see Meer and West 2016). To illustrate the advantage of examining the impact of the minimum wage on the wage

[41] 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.

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 and 3 annual lags and 1 annual lead 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).[42] The histogram bars show the sum of the contemporary and lagged minimum wage coefficients, divided by the sample average employment-to-population rate. The bars, therefore, represent the "long run" elasticity of employment in each wage bin with respect to the minimum wage. 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 a large impacts on the real dollar bins in the $6-$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-$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 shows that minimum wage changes were correlated to negative employment shocks in the upper part of the wage distribution and these confounding shocks were not absorbed by the simple two-way fixed effect specifications.[43] In fact, as we show in Panel b of Figure 9, the dramatic drop in the upper tail employment is not robust to alternative ways of controlling for confounding factors. When we estimate the model in first differences with year fixed effects, the bottom of the wage distribution is affected similarly to the fixed effect estimates: we see a drop in employment in the $6-$7/hour range and

[42] 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.9 and Online Appendix Table A.6, the unweighted estimates are similar.

[43] There are also other signs that the classic two-way fixed effect estimates are likely to be biased here. In the Online Appendix Figure A.10, we plot the estimated coefficients on the 1-year leading minimum wage. The graph highlights that large shifts in upper tail employment were present one year before the minimum wage increase, which suggests that the results in Figure 9 panel (a) are likely to driven by pre-existing trends.

an approximately equal sized increase in the \$8-\$11/hour range. However, now there are no employment reductions in the upper tail, and so changes in that part of the distribution do not influence the estimated impact of the minimum wage on the aggregate employment to population rate.[44]

The above example illustrates that showing the effect of the minimum wage throughout the wage distribution is an useful tool for model selection if one is willing to impose some restrictions on the potential impacts. For instance, a modest employment increase in the upper tail can be rationalized by labor-labor substitution in the standard model, or by an upward shift of the wage distribution in Burdett-Mortensen type models, and so such a changes should not be ruled out *a priori*. However, strong assumptions are needed to argue that a minimum wage change binding for a small number of workers at the bottom of the wage distribution has a large negative overall impact on employment above the median wage as the one seen in panel (a) in Figure 9.[45] These assumptions also have some testable implications beyond the effect of the minimum wage on the wage distribution, which should be directly assessed before we interpret those results as causal effects of the minimum wage.[46]

To summarize, estimating the effect of the minimum wage throughout that wage distribution can be used for illustrating the role of employment changes in the middle and upper part of the distribution which is unlikely to be substantially affected by minimum wage policy.[47] Moreover, a shift in the upper tail of the wage distribution is unlikely to reflect an impact on low-wage workers—the intended beneficiaries of minimum wage policies. Therefore, including such employment changes in the upper tail can understate (or overstate) the true employment effect on low-wage workers. As a consequence, empirical specifications that suggest large employment changes in the upper tail should be interpreted cautiously.

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

[44]As it is shown in Online Appendix Figure A.9 the unweighted first difference estimates used by Meer and West (2016) produces to somewhat larger disemployment estimates with an elasticity of -0.029 (s.e. 0.035), but still only 1/5 the size of the the fixed effects estimate of -0.164 (s.e. 0.065).

[45]The monopsony model presented in Chapter 12 of Manning (2003) can generate large disemployment effects in the upper tail under the assumption that there is only a single type of workers, who are paid differently solely because they are assigned to firms with different productivity. However, once workers with different productivities are introduced, the model predicts that employment of high productivity workers who are not directly exposed to the minimum wage should not be affected, and so the employment change in the upper tail should be limited.

[46]For instance, the monopsony model with a single type of labor mentioned in footnote 45 predicts that the sizable job losses in the upper tail coincide with large increases in the average and the median wage. However, Autor, Manning and Smith (2016) find no evidence that minimum wage has an effect on the median wages in the U.S. context.

[47]We also replicate our decomposition exercise for teenage workers in the Online Appendix Figure A.11. For teens, the changes in the upper tail of the wage distribution do not play any role in explaining the discrepancy across various empirical models. This is not surprising given that most teens are employed at very low wages, so shocks that might affect the upper tail of the wage distribution can only have a limited effect on the estimates.

impact of the minimum wage on low-wage workers, who are the primary target of minimum wage polices. 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 more than 100 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 $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 surprasses 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 presented in Section 2 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 of the minimum wage. While in principle these spillovers could reflect measurement error, our study highlights that this is unlikely to 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 to the labor force. 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 sector provide new empirical findings, which can be used to test and distinguish the importance of various frictions proposed in the literature.

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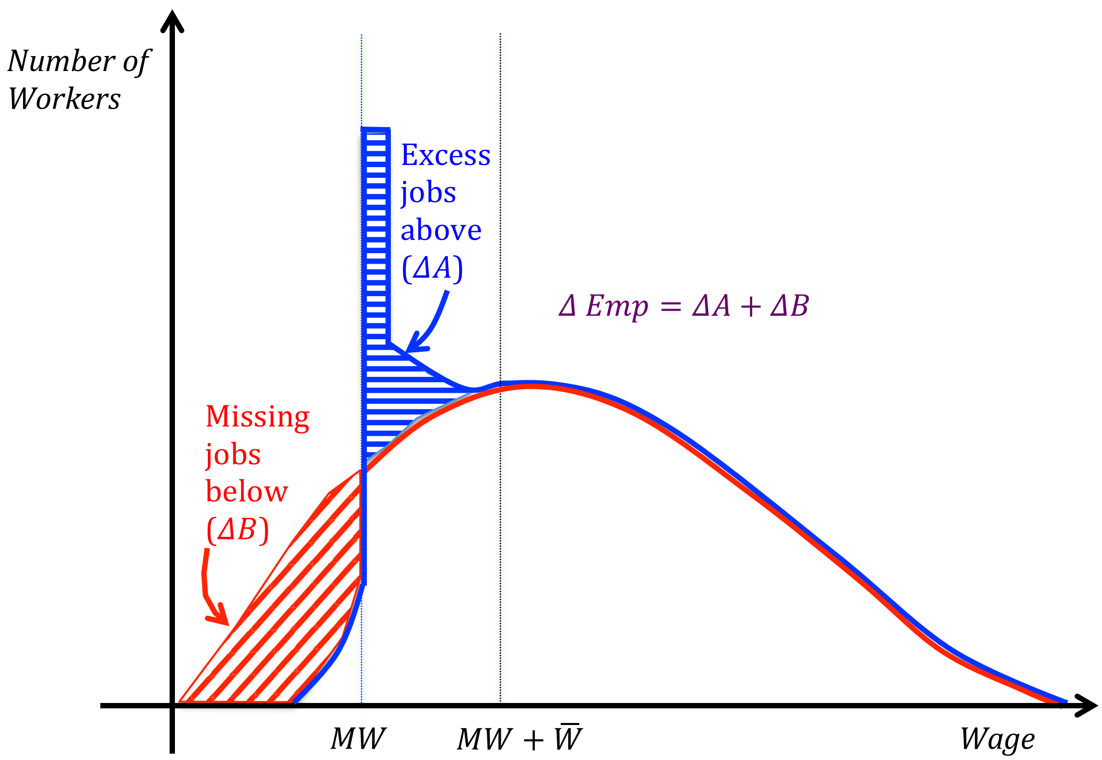
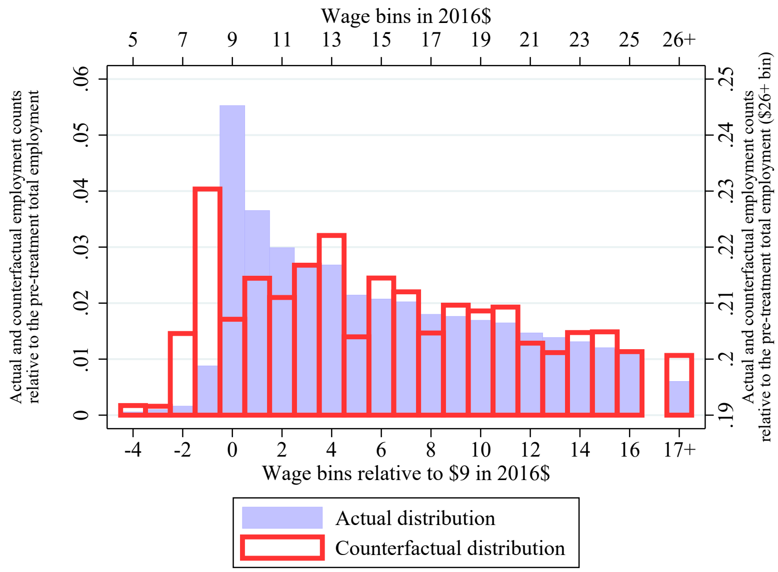
Figure 1: An Illustration of the Bunching Approach

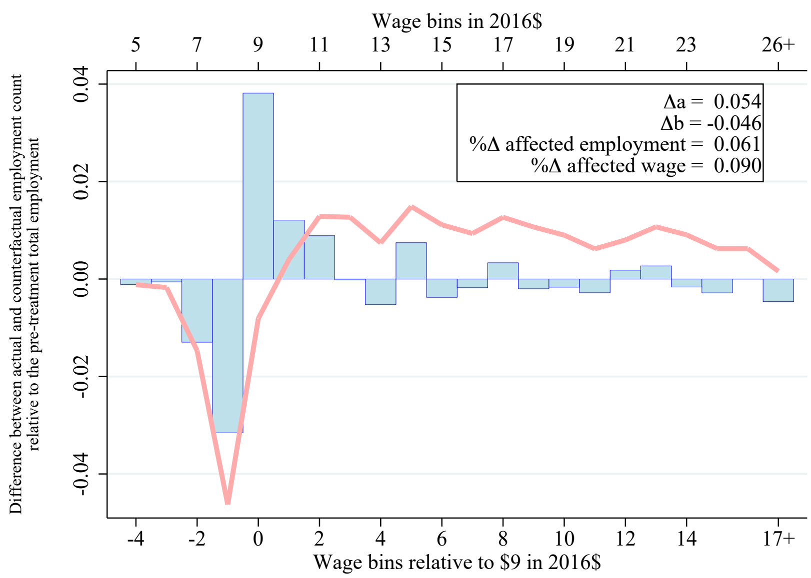


Notes: The figure shows the effect of the minimum wage on the frequency distribution of hourly wages. The red solid line shows the wage distribution before, and the blue solid line after the introduction of the minimum wage . Since compliance is less than perfect, some earners are uncovered and the post-event distribution starts before the minimum wage. For other workers, shown by the red dashed 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 $MW + \overline{W}$, 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



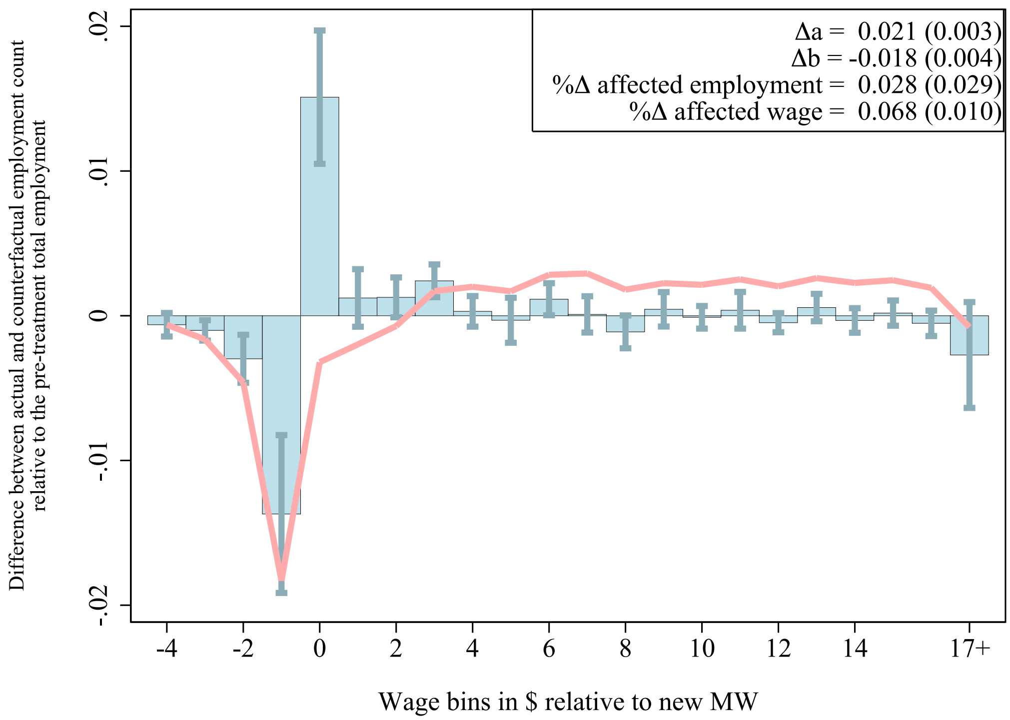
(a) The actual and counterfactual wage frequency distribution



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

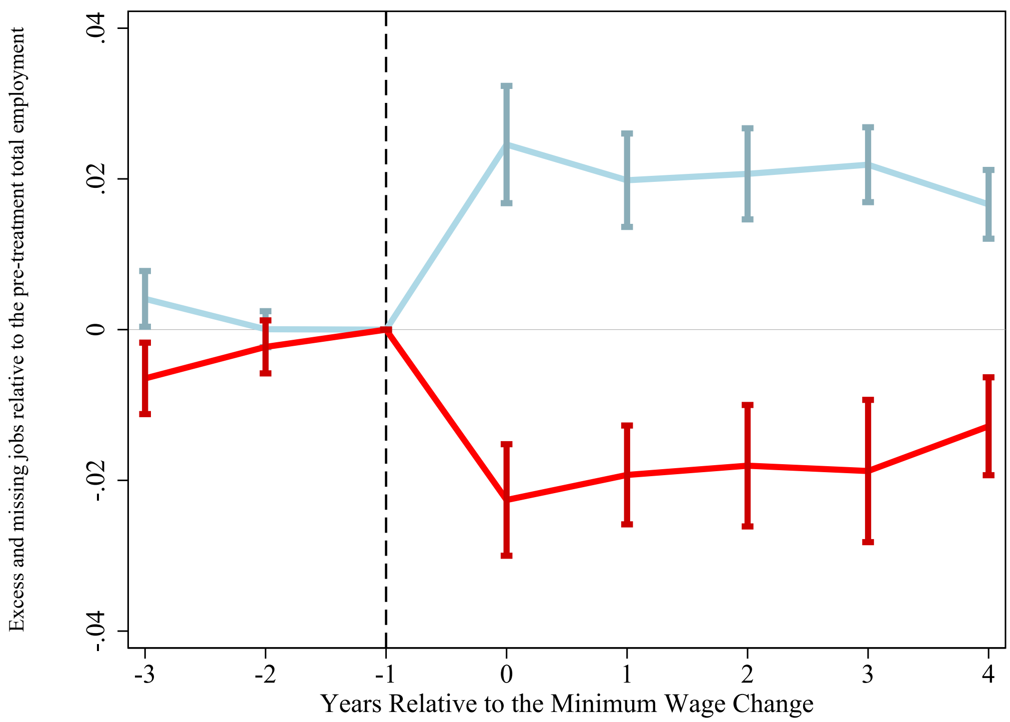
Notes: We examine the effect of the 1999-2000 minimum wage change in Washington on the frequency distribution of wages, normalized by the 1998 level of employment in WA. The minimum wage was raised from $7.51 to $9.18 (in 2016 values) and it was indexed by inflation afterwards. Panel (a) shows the actual (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 counterfectual 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 jobs bellow $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)



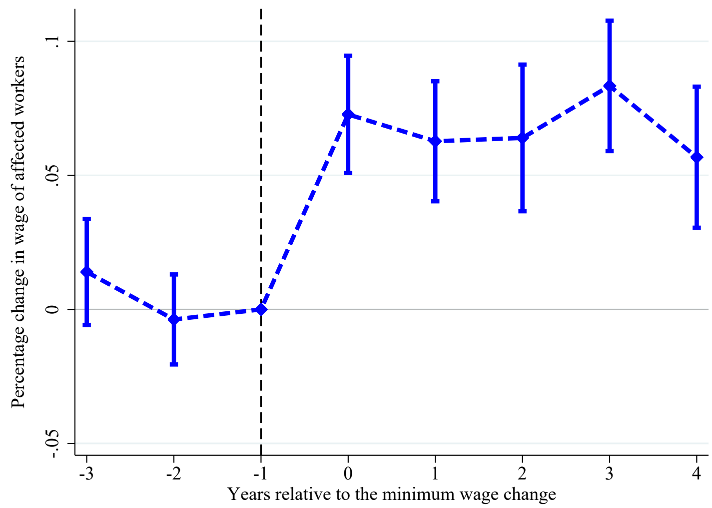
Notes: The figure shows the main results from our event study analysis (see equation 4) exploiting 138 state-level minimum wage changes between 1979-2016. The blue bars shows for each dollar bin (relative to the minimum wage) the estimated average employment changes in that bin during the 5-year post-treatment relative to the total employment in the state one year before the treatment. The error bars shows 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)

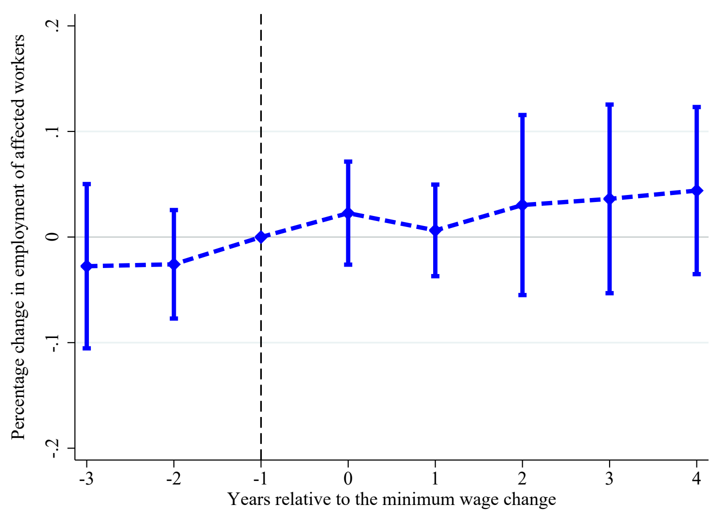


Notes: The figure shows the main results from our event study analysis (see equation 4) exploiting 138 state-level minimum wage changes between 1979-2016. The figure shows the effect of a minimum wage increase on the missing jobs below the new minimum wage (blue line) and on the excess jobs at and slightly 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 $5 above it / ($\Delta a$). 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)



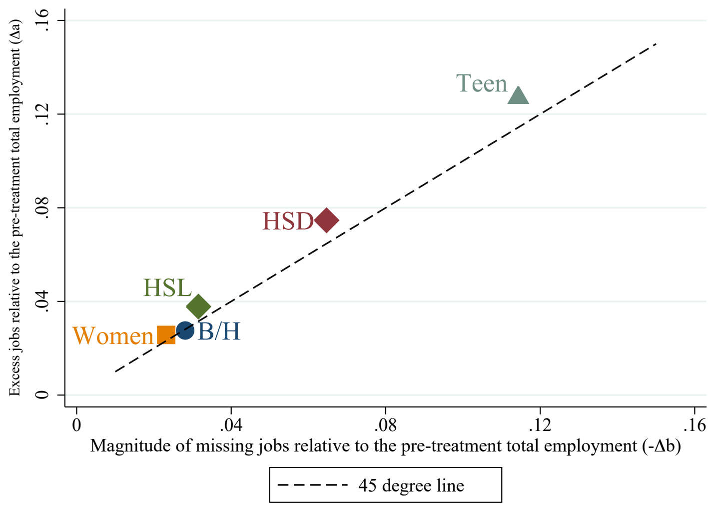
(a) Evolution of the average wage of the affected workers



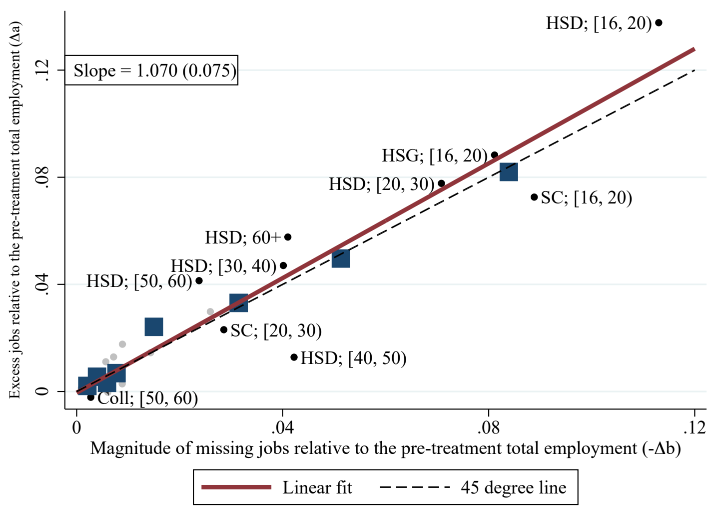
(b) Evolution of the employment of the affected workers

Notes: The figure shows the main results from our event study analysis (see equation 4) 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 5. 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 for significant disemployment effects.

Figure 6: Impact of the Minimum Wage by Demographic Groups



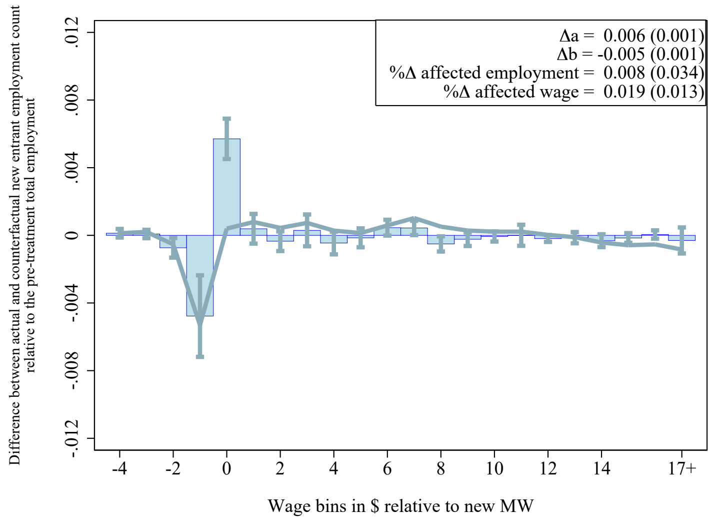
(a) Effect of the minimum wage by demographic groups



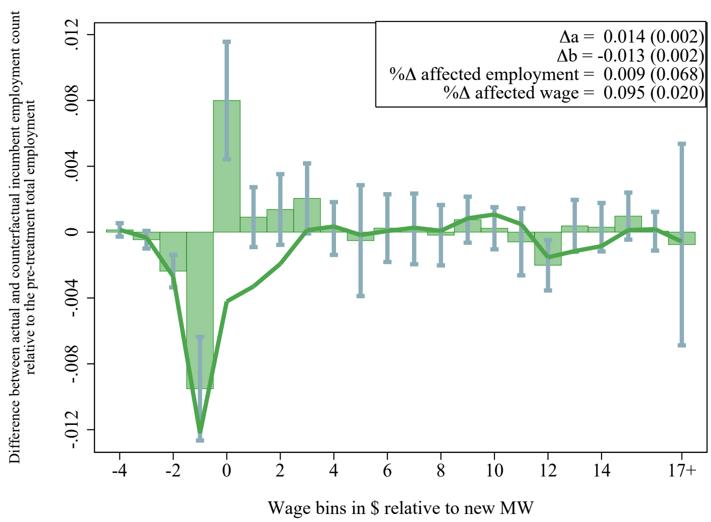
(b) Effect of the minimum wage by age-education groups

Notes: Both figures shows the excess jobs (relative to the pre-treatment total employment in that group) above the new minimum wage (Δa) and 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, below that line negative ones. Panel (a) shows the estimates for demographic groups in table 3: for high school dropout, for high school or less, for women, for teen, and for black or Hispanic workers. 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 New Entrants and Incumbents (Pooled Event Study Analysis)



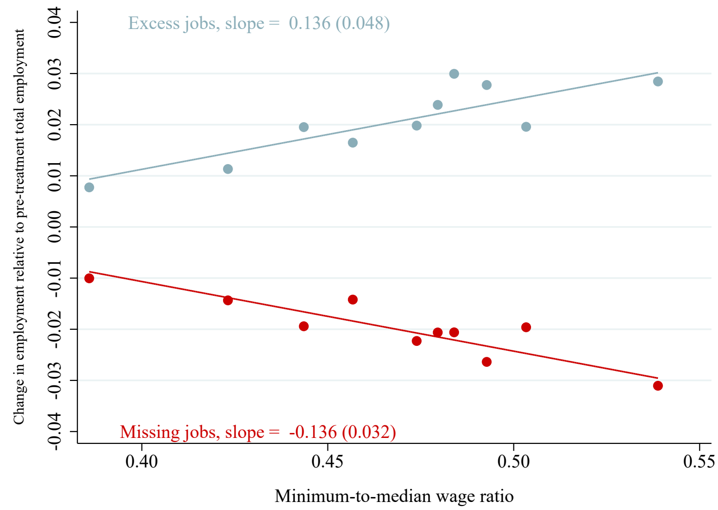
(a) New entrants



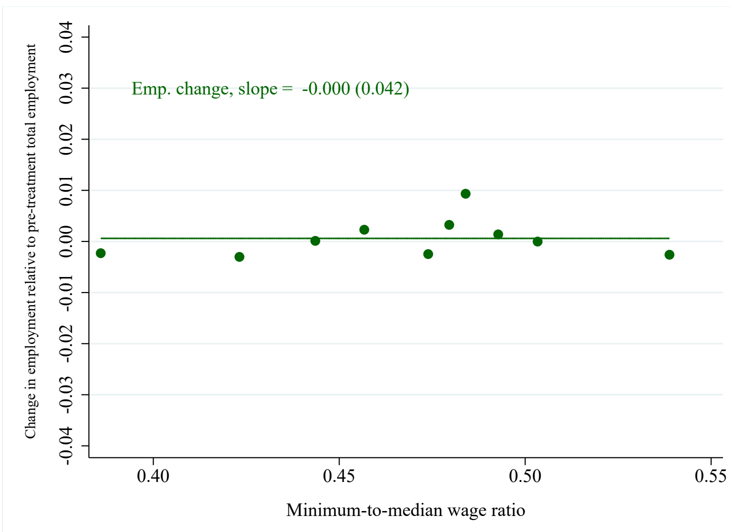
(a) Incumbents

Notes: The figure shows the main results by new entrants (panel a) and by incumbents (panel b) from our event study analysis (see equation 4) exploiting 138 state-level minimum wage changes between 1979-2016. The blue (green) bars shows for each dollar bin the estimated change in the number of new entrants (incumbents) in that bin 1-year post-treatment relative to the total employment of the new entrants (incumbents) 1 year before the treatment. Incumbent workers are employed; whereas new-entrants are not in the previous year. The error bars show the 95% confidence interval calculated using standard errors that are clustered at the state level. The green (blue) line shows the the running sum of employment changes for the new entrants (incumbents) up to the wage bin it corresponds to. The figures highlight that the ripple effect of the minimum wage is mainly comes from incumbent workers.

Figure 8: Relationship between Excess Jobs, Missing jobs, Employment Change and the Minimum-to-Median Wage Ratio Across Events



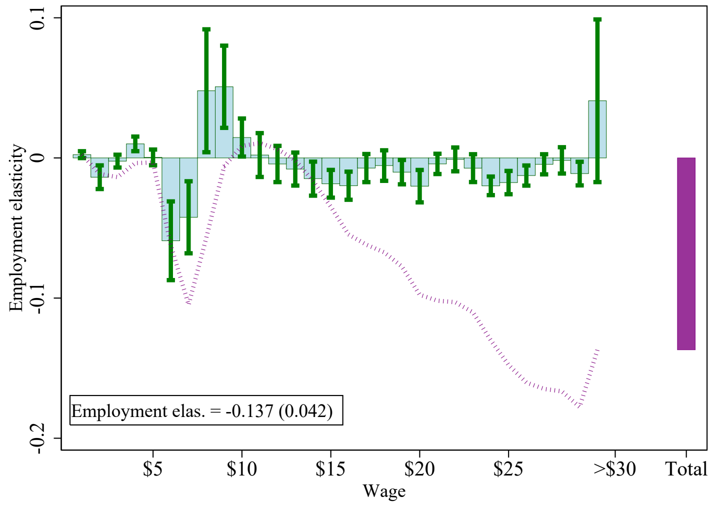
(a) Missing and excess jobs



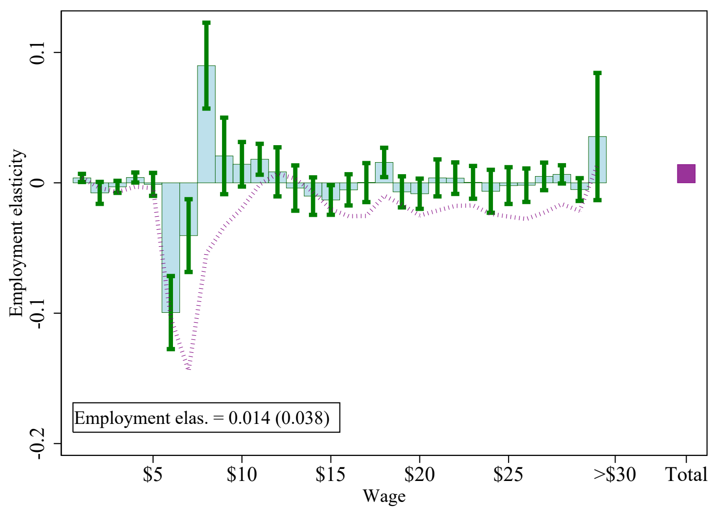
(b) Employment change

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 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 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 of Minimum Wages on the Wage Distribution in Fixed Effects and First Difference Specifications



(a) Fixed effect, weighted



(c) First differences, weighted

Notes: The figure shows the effect of the minimum wage on the wage distribution in fixed effects and first differences specifications. Panel (a) estimates two-way (state-bin and year) fixed effects regressions on contemporaneous as well as 3 annual lags and 1 annual lead of log minimum wage. In Panel (b) we employ first difference regression with 3 annual lags and 1 annual lead of the log change in the minimum wage. For each wage bin a separate regression is run, where outcome variables are number of jobs per capita in that state-wage bin. The green histogram bars show the sum of the contemporary and lagged minimum wage coefficients, divided by the sample average employment-to-population rate —which represents the "long run" elasticity of employment in each wage bin with respect to the minimum wage. 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 long run 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. Both regressions in panel (a) and (b) 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 1979-2016. 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 Column (2)-(6) explore robustness to bin-state time trends, bin-division-period fixed effects. Column (7) report triple difference specifications when we controll 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 minimum wage (Δa) relative to the pre-treatment total employment. Third row is the percentage change in average wages in the affected bins (%ΔW). The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by below share ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). 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	847314	847314	847314	847314	847314	847314	847314
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 1979-2016. 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 36 events 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 into changes in hours worked. Column 4 uses workers who directly reported as 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 the 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 minimum wage (Δa) relative to the pre-treatment total employment. Third row is the percentage change in average wages in the affected bins (%ΔW). The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by below share ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). 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)
Missing jobs below new MW (Δb)	-0.065***	-0.032***	-0.114***	-0.023***	-0.028***
	(0.010)	(0.007)	(0.010)	(0.005)	(0.008)
Excess jobs above new MW (Δa)	0.075***	0.038***	0.127***	0.026***	0.028***
	(0.011)	(0.006)	(0.020)	(0.004)	(0.006)
%Δ affected wages	0.077***	0.073***	0.081***	0.070***	0.045***
	(0.013)	(0.013)	(0.015)	(0.011)	(0.012)
%Δ affected employment	0.038	0.043	0.030	0.025	-0.004
	(0.024)	(0.030)	(0.032)	(0.027)	(0.044)
Employment elasticity w.r.t. MW	0.097	0.061	0.125	0.025	-0.005
	(0.061)	(0.042)	(0.134)	(0.027)	(0.058)
Emp. elasticity w.r.t. affected wage	0.493*	0.595	0.366	0.352	-0.086
	(0.289)	(0.419)	(0.338)	(0.380)	(0.998)
Jobs below new MW ($\bar{b}_{-1}$)	0.264	0.145	0.432	0.102	0.133
%Δ MW	0.103	0.103	0.102	0.101	0.100
Number of events	138	138	138	138	138
Number of observations	847,314	847,314	847,314	847,314	846,729
Number of workers in the sample	660,771	2,248,711	287,484	2,277,624	781,003
Sample	No high school degree	High school or less	Teen	Female	Black or Hispanic

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 1979-2016. 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) and for black or Hispanic workers (Column 5). 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 minimum wage (Δa) relative to the pre-treatment total employment. Third row is the percentage change in average wages in the affected bins (%ΔW). The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by below share ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). 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 1992-2016. 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 follow Mian and Sufi (2014) (see 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 minimum wage (Δa) relative to the pre-treatment total employment. Third row is the percentage change in average wages in the affected bins (%ΔW). The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by below share ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). 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
	$\%\Delta w$	$\%\Delta w_{\text{No spillover}}$	$\frac{\%\Delta w - \%\Delta w_{\text{No spillover}}}{\%\Delta w_{\text{No spillover}}}$
Overall	0.068***	0.041***	0.397***
	(0.010)	(0.009)	(0.119)
High school dropout	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)
Female	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 1979-2016. 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). We also report estimates for the upper-tercile events with large minimum wage increases, and estimates using full-time equivalent workers. The first column is the estimated change in the affected wages calculated according to the equation 4, and the second column assumes no spillover and calculates it accordingly (see equation 6 in Section 4.5). 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.

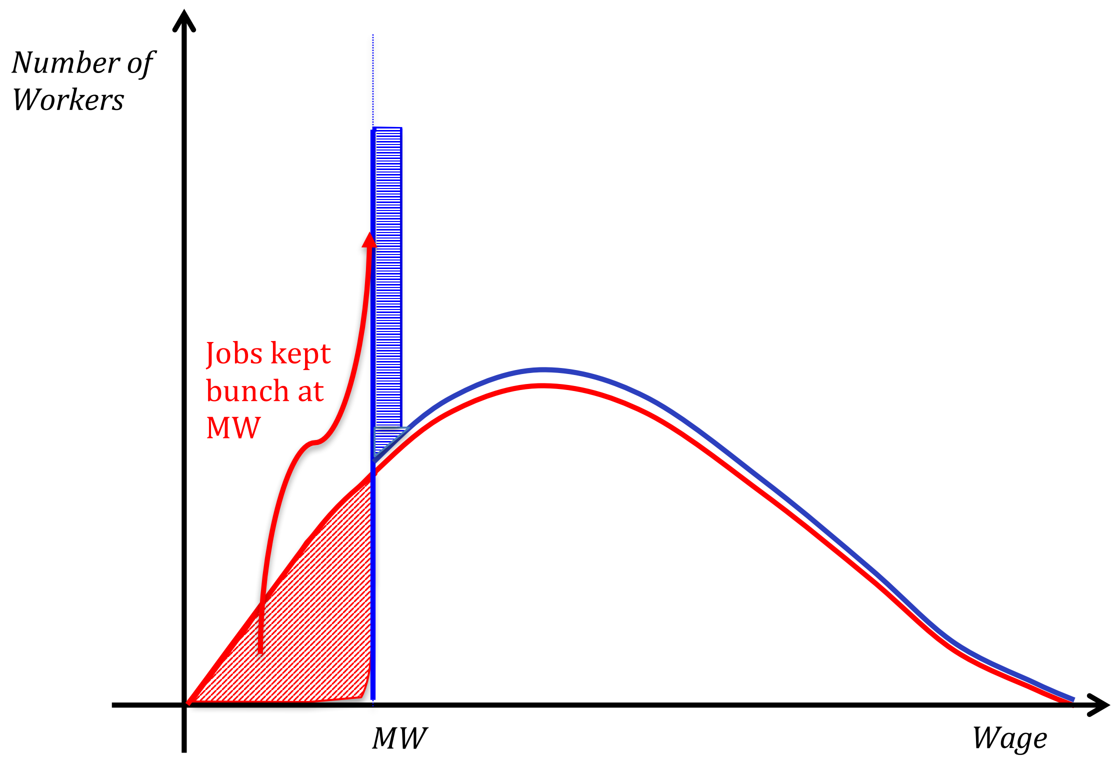
Table 6: Relationship Between Employment Changes and the Minimum-to-Median Wage Ratio (Kaitz Index) Across Events

	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, 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. The sample of 130 events excludes 8 minimum wage increases in the District of Columbia. Regressions are weighted by event-specific inverse-variances. Robust standard errors are in parentheses; significance levels are * 0.10, ** 0.05, *** 0.01.

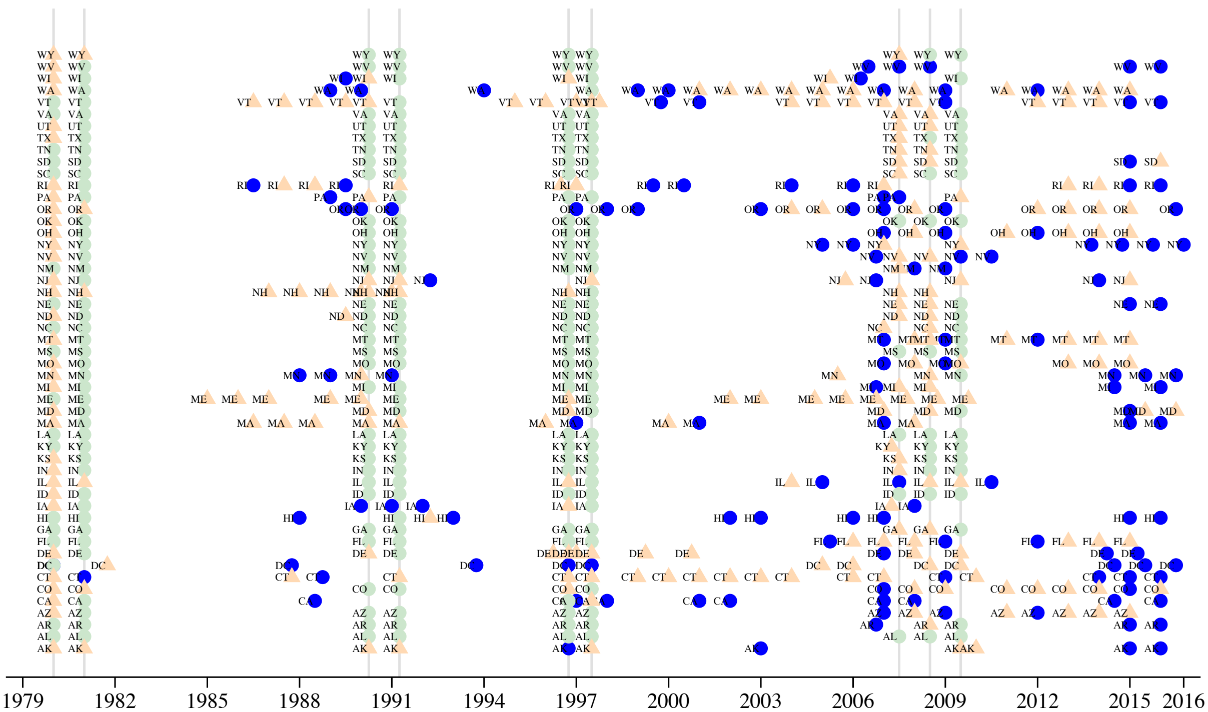
Online Appendix A Additional Figures and Tables

Figure A.1: Effect of the Minimum Wage on the Wage Distribution in the Standard Labor Demand Model



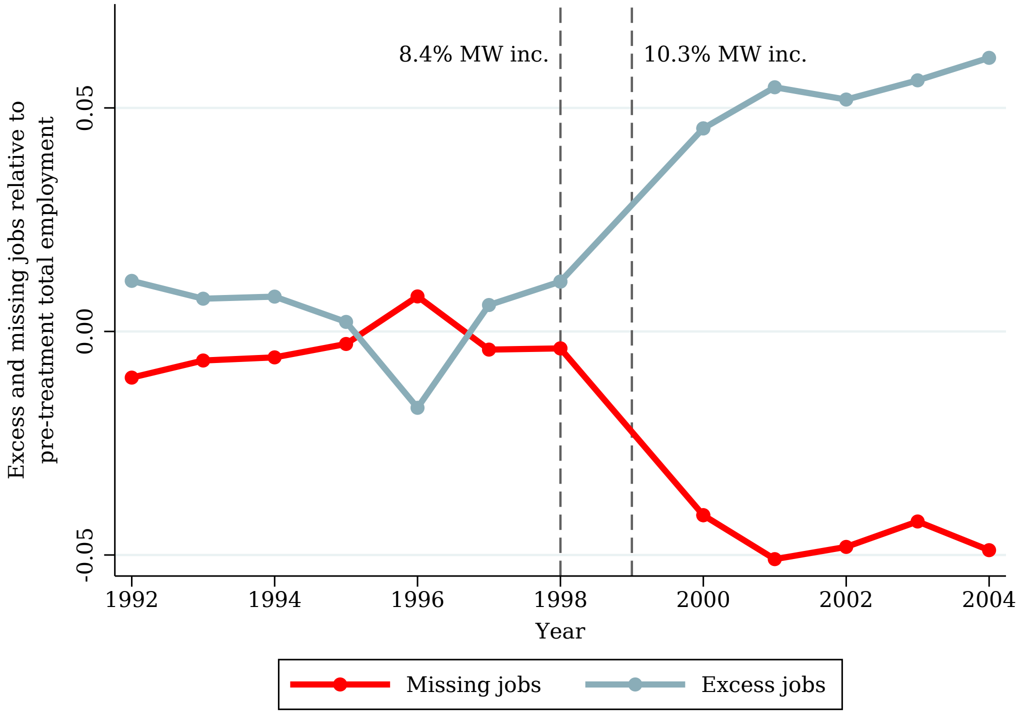
Notes: The figure shows the effect of the minimum wage on the frequency distribution of hourly wages in the frictionless standard labor demand model presented in Section 2 and in Appendix Section B. The red solid line shows the wage distribution in absence of the minimum wage. Once the minimum wage is introduced (blue solid line), all jobs below it will be destroyed or pushed up to the new minimum wage, thereby creating a spike. The difference between the missing jobs and number of jobs at the spike estimates the disemployment effect of the minimum wage for low-wage workers. Minimum wage also has a modest impact on the upper-tail of the wage distribution in this model, since the higher labor costs at the bottom of the wage distribution boost labor demand for workers at higher up the wage distributions. As a result, the post-treatment wage distribution (blue line) will be elevated at wage levels above the minimum wage.

Figure A.2: Minimum Wage Increases between 1979 and 2016



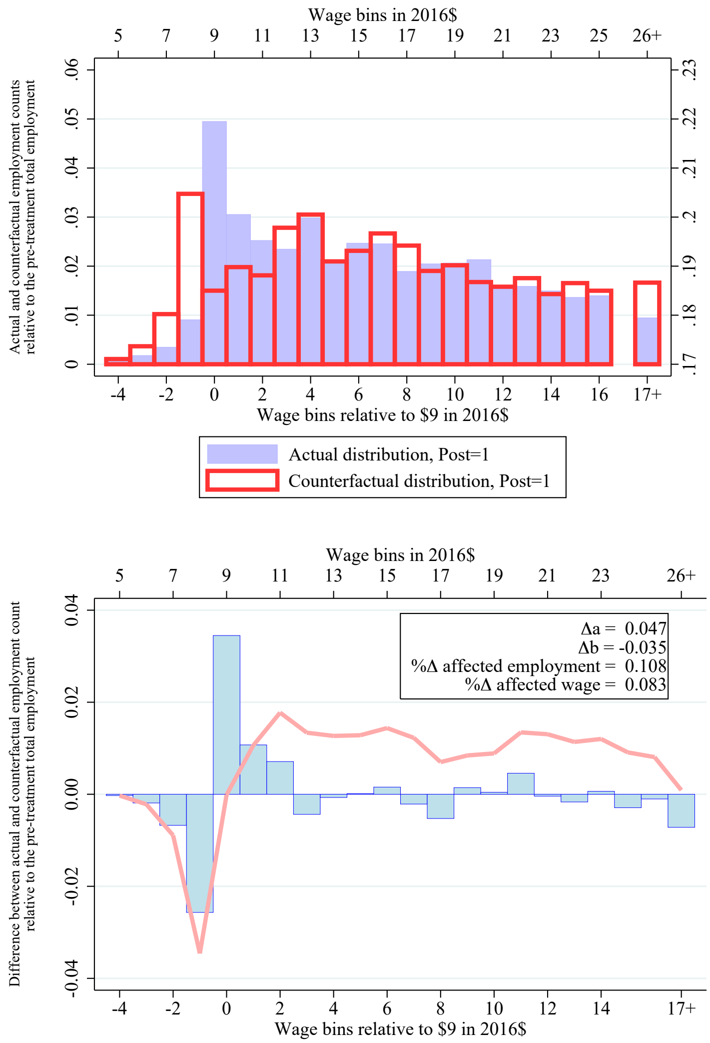
Notes: The figure shows all MW increases between 1979 and 2016. There are at total of 516 minimum wage increases. The blue circles show the primary minimum wage events used in estimating equation 4; the partially transparent orange triangles highlight small minimum wage changes where minimum wage increased less than \$0.25 (the size of our wage bins) or where less than 2 percent of the workforce earned between the new and the old minimum wage. The green circles indicate federal changes, which we exclude from our primary sample of treatments because the change in missing number of jobs, Δb, is only 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.3: Impact of Minimum wages on Missing and Excess jobs Over time in the Washington Case Study



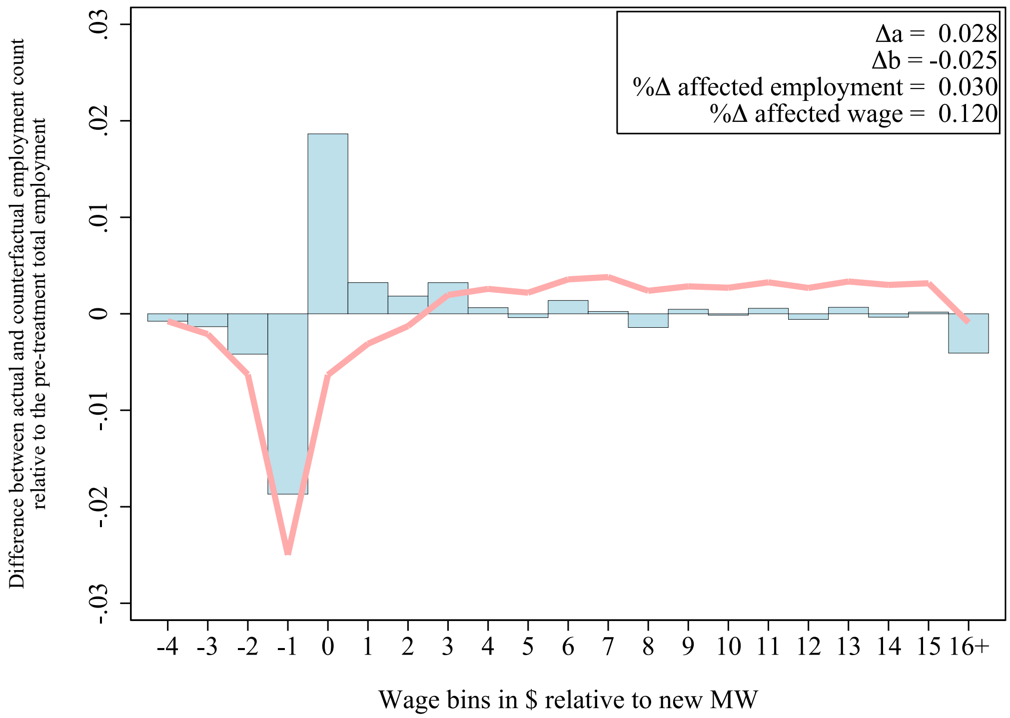
Notes: The figure shows the evolution of excess and missing jobs relative to the counterfactual frequency distribution of wages over time in Washington state. The red line represents the missing jobs— the difference between the actual and counterfactual wage distribution between \$5 and \$9. The blue line shows the excess jobs that is the difference between the actual and counterfactual frequency distributions for wages between \$9 and \$13. The counterfectual 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 show that the minimum wage was raised between 1999 and 2001 in two steps from from \$7.51 to \$9.18 (in 2016 values). The minimum wage was indexed to inflation after 2001.

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



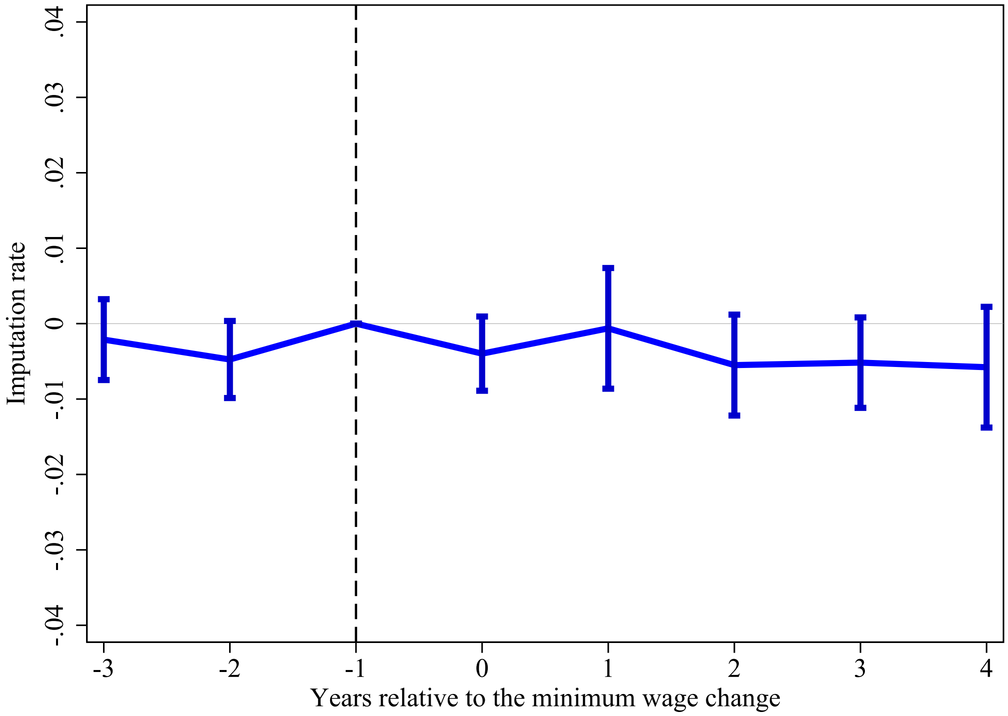
Notes: The figure replicates Figure 2 that examine the effect of the 1999-2000 minimum wage change in Washington on the (frequency) distribution of wages. The minimum wage was raised from $7.51 to $9.18 (in 2016 values) and it was indexed by inflation. Panel (a) shows the actual (purple solid bars) and 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. The counterfectual 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 $9, $\triangle b$; 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)



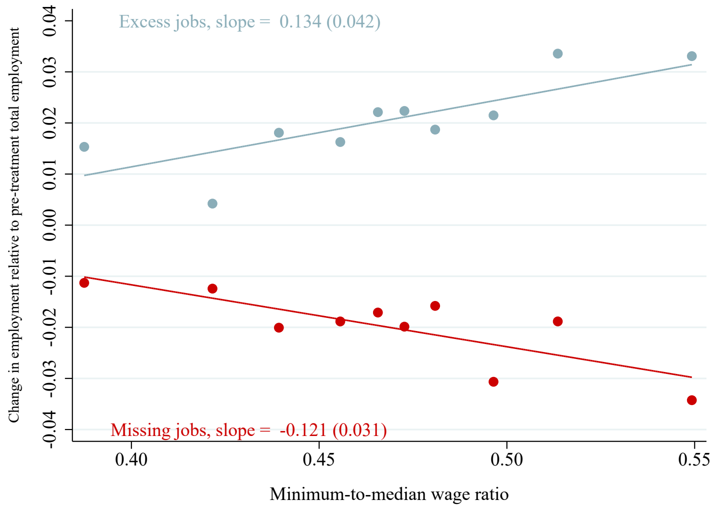
Notes: The figure shows the estimated difference in normalized employment counts in each of the $1 bins for the accumulated changes in the employment due to events occurring during the 5-year post-treatment relative to the 1-year pre-treatment period in the states compared to the controls for the 138 primary minimum wage events, using state-quarter-wage bin aggregated CPS data from 1979-2016. On average, 0.65 primary events take place in the first five years of the first event. 90.4% of them take effect in the same $1 wage bin as the first event; whereas 8,8% in the $1 bin above, and only 0.8% in the $1 bin below it. The red line is the running sum of the bin-specific impacts. The estimated changes in the missing jobs below and excess jobs above the minimum wage, percentage change in the affected employment and wages are reported on the top-right corner. These numbers should be compared to the benchmark analysis shown in Figure 3. The missing jobs estimate is larger in this graph (2.8%) than in Figure 3 (1.8%), which highlights that the cumulative effect of treatments following each other on the jobs below the new minimum wage is larger.

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

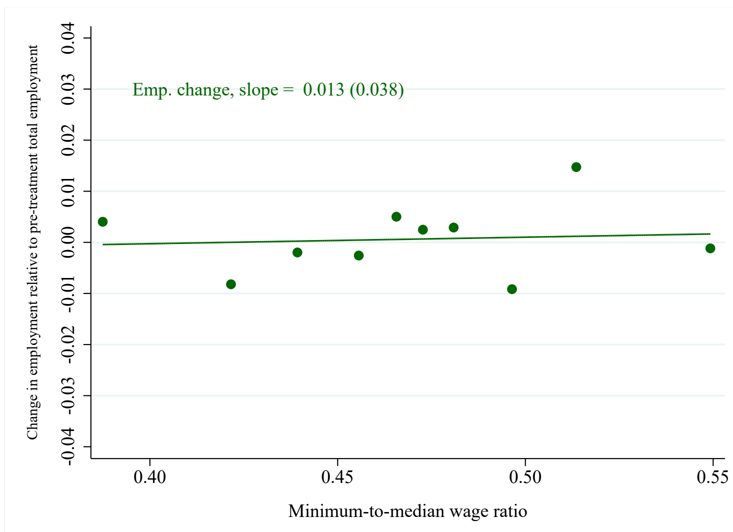


Notes: The figure shows the effect of the minimum wage on the imputation rate. In our pooled event study analysis we only use non-imputed hourly wages. To alleviate the concern that imputation has an effect on our estimtes, we implement an event study regression where the outcome variable is state-level 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.2 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. Standard errors are clustered at the state level.

Figure A.7: 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)



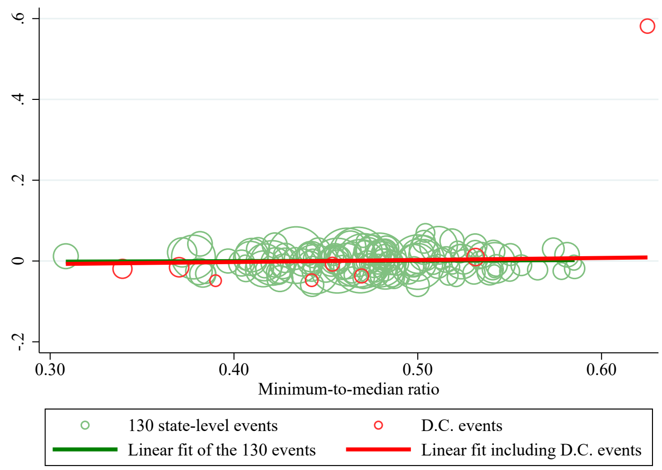
(a) Missing and excess jobs



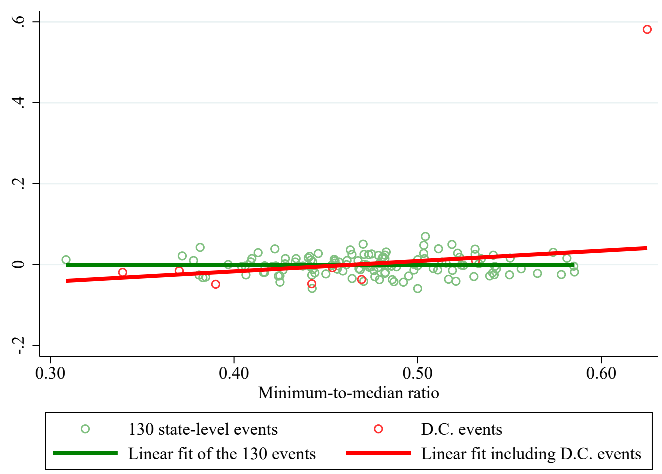
(b) Employment change

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 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. No controls included in the regression.

Figure A.8: Relationship between Employment Change and the Minimum-to-Median Wage Ratio Across Events, Scatterplot



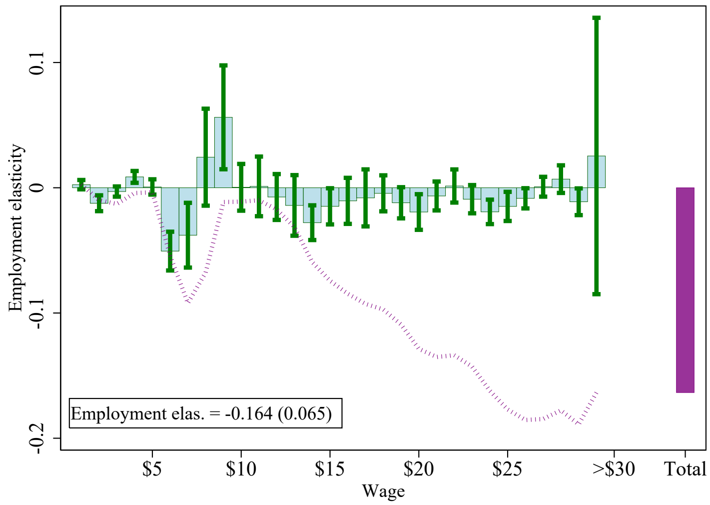
(a) Inverse-variance weighted



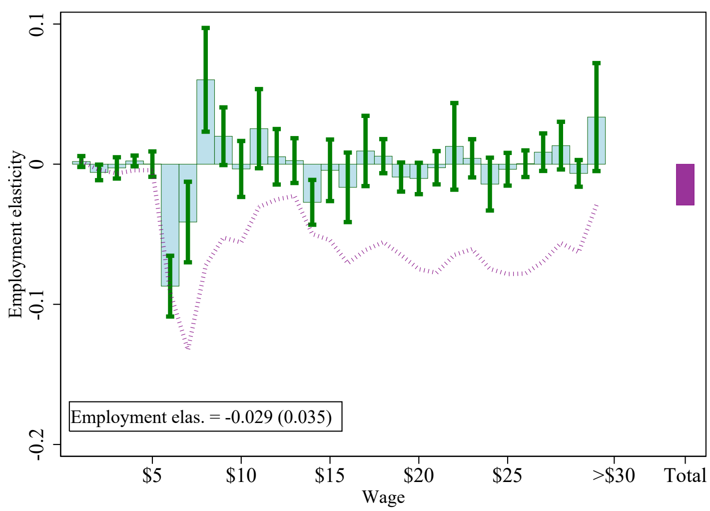
(a) Unweighted

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.9: Impact of Minimum Wages on the Wage Distribution in Fixed Effects and First Difference Specifications (Unweighted Version of Figure 9 in the Main Text)



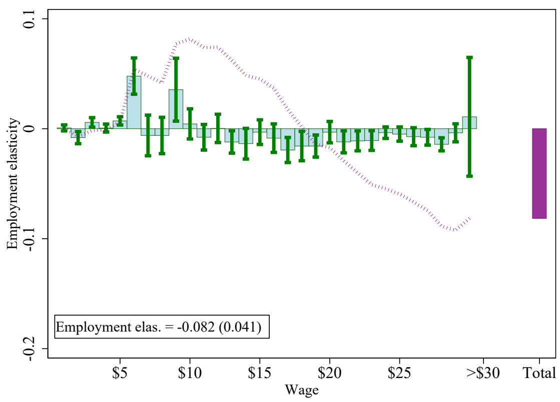
(a) Fixed effect, unweighted



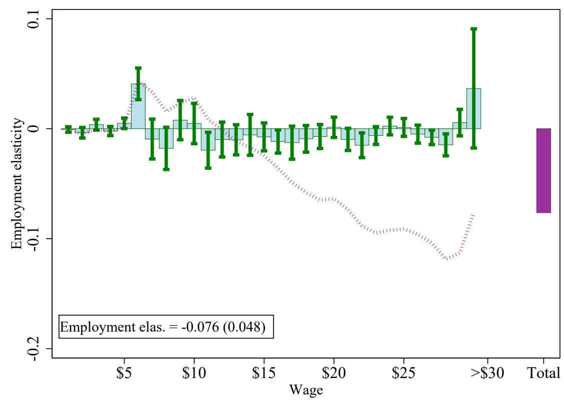
(b) First differences, unweighted

Notes: The figure shows the unweighted version of Figure 9 in the main text: the effect of the minimum wage on the wage distribution in fixed effects and first differences specifications. Panel (a) estimates two-way (state-bin and year) fixed effects regressions on contemporaneous as well as 3 annual lags of log minimum wage. In Panel (b) we employ first difference regression with 3 annual lags of the log change in the minimum wage. For each wage bin a separate regression is run, where outcome variables are number of jobs per capita in that state-wage bin. The green histogram bars show the sum of the contemporary and lagged minimum wage coefficients, divided by the sample average employment-to-population rate —which represents the "long run" elasticity of employment in each wage bin with respect to the minimum wage. 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 long run 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. Both panel (a) and (b) are *unweighted*.

Figure A.10: Estimated Leading Effects of the Impact of Minimum Wages on the Wage Distribution for Fixed-effects and First-difference specifications



(a) Fixed effect, weighted



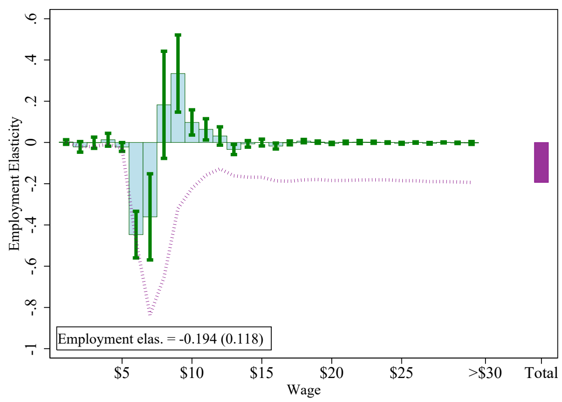
(b) Fixed effect, unweighted

(c) First differences, weighted

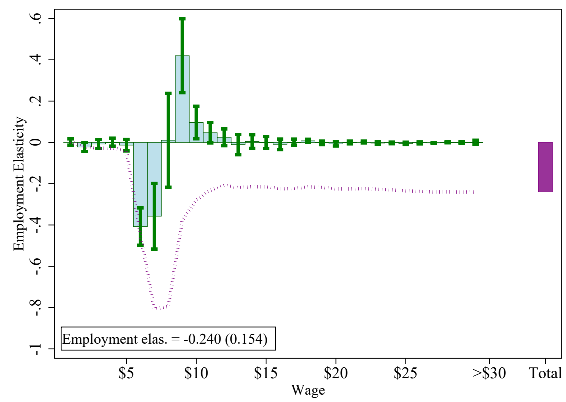
(d) First differences, unweighted

Notes: The figure shows the 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 3 annual lags and 1 annual lead of log minimum wage. For each wage bin a separate regression is run, where outcome variables are number of jobs per capita in that state-wage bin. The green histogram bars show the one year leading minimum wage coefficients, divided by the sample averaged employment-to-population rate. 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 long run 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.

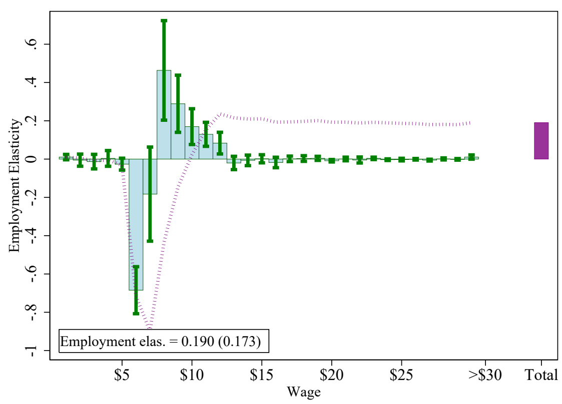
Figure A.11: Impact of Minimum Wages on the Wage Distribution in Fixed Effects and First Difference Specifications (Reporting results in Figure A.9 and 9 for teens)



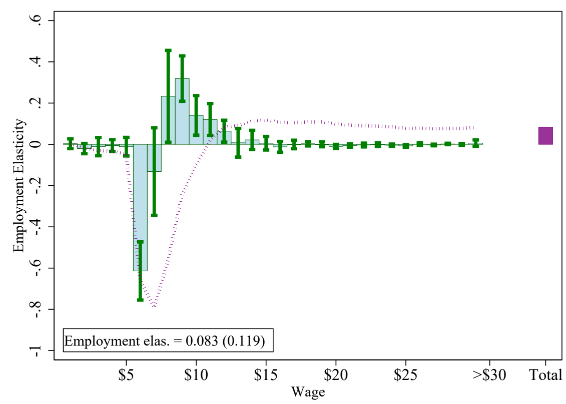
(a) Fixed effect, weighted



(b) Fixed effect, unweighted



(c) First differences, weighted



(d) First differences, unweighted

Notes: The figure report the results in Figure A.9 and 9, but instead of using the whole population here we only focus on teenagers. The figure shows the effect of the minimum wage on the teenage workers' wage distribution in fixed effects and first differences specifications. Panel (a) and (b) estimate two-way (state-bin and year) fixed effects regressions on contemporaneous as well as 3 annual lags 1 annual lead of log minimum wage. In Panel (c) and (d) we employ first difference regression with year fixed effects with 3 annual lags 1 annual lead. For each wage bin a separate regression is run, where outcome variables are number of teenage jobs per capita in that state-wage bin. The green histogram bars show the sum of the contemporary and lagged minimum wage coefficients, divided by the sample average teenage employment-to-population rate —which represents the "long run" elasticity of employment in each wage bin with respect to the minimum wage. The 95% confidence intervals around the point estimates are calculated using clustered standard errors at the state level. The dashed purple line shows the 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 long run elasticity of the overall state teenage employment-to-population with respect to minimum wage, obtained from regressions where outcome variables are the state level teenage 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 panel (a) and (c) are weighted by state teenage population; whereas the ones in panel (b) and (d) on the right-hand side are not weighted. The figure highlights that large aggregate disemployment effects are often driven by shifts in employment at the upper tail of the wage distribution.

Table A.1: Impact of minimum wage increase on the average wage and employment of affected workers

	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 five year averaged post-treatment estimates of employment and wages of the affected bins by alternative wage windows, using state-quarter-wage bin aggregated CPS data from 1979-2016. The first column limits the range of the wage window by setting the upper limit to $\overline{W} = \$2$, and the last column expands it until $\overline{W} = \$6$. The dependent variable is the per capita employment in wage bins. Specifications include wag -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 minimum wage (Δa) relative to the pre-treatment total employment. Third row is the percentage change in average wages in the affected bins (%ΔW). The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by below share ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). 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.2: 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 total 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.3: 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 five year averaged post-treatment estimates of employment and wages of the affected bins using triple-difference specification, using state-quarter-wage bin aggregated CPS data from 1979-2016. In column (1) observations with wages greater than $15, and in column (2) $20, are dropped.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 minimum wage (Δa) relative to the pre-treatment total employment. Third row is the percentage change in average wages in the affected bins (%ΔW). The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by below share ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). 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.4: Impact of Minimum Wage Increase by Labor Force 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, incumbent and new-entrant workers 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 minimum wage (Δa) relative to the pre-treatment total employment. Third row is the percentage change in average wages in the affected bins (%ΔW). The fourth row, percentage change in employment in the affected bins is calculated by dividing change in employment by below share ($\frac{\Delta a+\Delta b}{\bar{b}_{-1}}$). 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: 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.6: Employment Elasticities of Minimum Wage from Alternative Approaches

	Continuous treatment - ln(MW)				Event based		
	Levels	First Difference	Levels	First Difference			
Weighted	-0.137***	0.014	-0.016	-0.007	0.016	0.027	0.024
	(0.042)	(0.038)	(0.036)	(0.020)	(0.029)	(0.022)	(0.025)
Unweighted	-0.164**	-0.029	-0.059	-0.050*	-0.089	0.023	0.028
	(0.065)	(0.035)	(0.061)	(0.028)	(0.060)	(0.026)	(0.030)
Aggregate	Y	Y			Y		
Under $15			Y	Y		Y	
$[MW\text{–}\$4, MW + \$5)$							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 4.1., 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.7: Teen employment elasticities of minimum wage from alternative approaches

	Continuous treatment - ln(MW)				Event based		
	Levels	First Difference	Levels	First Difference			
Weighted	-0.194	0.190	-0.168	0.209	0.163	0.152	0.125
	(0.118)	(0.173)	(0.112)	(0.171)	(0.115)	(0.107)	(0.134)
Unweighted	-0.240	0.083	-0.215	0.113	-0.004	-0.017	-0.043
	(0.154)	(0.119)	(0.149)	(0.117)	(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 Bunching in a Competitive Model

In this section, we relate the size of the bunching at the minimum wage to the key parameters of the standard labor demand model with heterogeneous workers. First, we solve the cost minimization problem under the assumption that labor supply is perfectly elastic and highlight that the size of bunching is directly related to the elasticity of substitution across various worker types. Then we turn to behavioral responses under profit maximization, which additionally allows for scale effects. In this case, the size of the bunching will depend not just on the substitution elasticity but also on the effect of the minimum wage on aggregate production. However, the latter will be negligible when minimum wage workers represent only a small fraction of aggregate labor cost—as is true in the U.S. Finally, we relax the assumption that labor supply is perfectly elastic, and we derive a more general formula for the size of the bunching. The more general formula depends not just on the elasticity of substitution, but also on the labor supply elasticity. Here, too, we show that when minimum wage workers account for a small share of aggregate labor cost, the labor supply elasticity only plays a minor role in determining the size of behavioral responses to the minimum wage.

Cost minimization. We begin by solving the cost minimization problem, which abstracts away from the changes in the aggregate production level induced by the minimum wage. This assumption simplifies the presentation with little cost as these scale effects are likely to be very small in context of the minimum wage changes we study.

Firms solve the following problem:

$$\min_{l_j} \int_{\underline{w}}^{\overline{w}} l_j w_j dj$$

$$s.t. \quad Y = \left(\int_{\underline{w}}^{\overline{w}} \phi_j l_j^{\frac{\sigma-1}{\sigma}} dj \right)^{\frac{\sigma}{\sigma-1}}$$

where l_j is the amount of labor used from type j, a_j is the productivity of type j, and w_j is the wage cost of type j labor. We assume that at wage w_j, wage firms can hire as many type j workers as they want, which is equivalent to assuming that the supply of type j labor is perfectly elastic. This assumption is analogous to the implicit assumptions made in Saez (2010) who studies behavioral responses to tax kinks in a frictionless model of labor supply. In his benchmark model workers are free to set their labor supply without affecting wages, and so it is implicitly assumed that labor demand is perfectly elastic at a given wage. Since we focus here on labor demand, we make an analogous simplifying assumption by imposing a perfectly elastic labor supply for each type of worker. Later we relax this assumption, and consider the case with inelastic labor supply.

The Lagrangian of this problem is as follows:

$$\mathcal{L} = \int_{\underline{w}}^{\overline{w}} l_j w_j dj - \lambda \left[Y - \left(\int_{\underline{w}}^{\overline{w}} \phi_j l_j^{\frac{\sigma-1}{\sigma}} dj \right)^{\frac{\sigma}{\sigma-1}} \right]$$

and the first order condition (FOC) for type j labor is as follows:

$$w_j = \lambda \left(\int_{\underline{w}}^{\overline{w}} \phi_j l_j^{\frac{\sigma-1}{\sigma}} dj \right)^{\frac{\sigma}{\sigma-1}-1} \phi_j l_j^{\frac{\sigma-1}{\sigma}-1}$$

Taking the ratio of the FOC of worker types j and i leads to the following expression:

$$\phi_j l_j^{\frac{\sigma-1}{\sigma}} = l_j w_j \frac{\phi_i}{w_i} l_i^{\frac{\sigma-1}{\sigma}-1}$$

We integrate this expression between $\underline{w}$ and $\overline{w}$:

$$\int_{\underline{w}}^{\overline{w}} \phi_j l_j^{\frac{\sigma-1}{\sigma}} dj = \frac{\phi_i}{w_i} l_i^{\frac{\sigma-1}{\sigma}-1} \int_{\underline{w}}^{\overline{w}} l_j w_j dj$$

which can be rewritten as:

$$Y^{\frac{\sigma-1}{\sigma}} = \frac{\phi_i}{w_i} l_i^{\frac{\sigma-1}{\sigma}-1} C(Y,w)$$

where $C(Y,w) = \int_{\underline{w}}^{\overline{w}} l_j^* w_j dj$ is the cost function. The labor demand for each worker type as a function of total labor cost can be expressed as:

$$l_i = Y^{1-\sigma} C(Y,w)^{\sigma} \left(\frac{\phi_i}{w_i} \right)^{\sigma} \tag{7}$$

Multiplying both sides by w_i and integrating it between $\underline{w}$ and $\overline{w}$ leads to the following expression:

$$\int_{\underline{w}}^{\overline{w}} w_i l_i di = Y^{1-\sigma} C(Y,w)^{\sigma} \int_{\underline{w}}^{\overline{w}} \phi_i^{\sigma} w_i^{1-\sigma} di$$

This can be used to derive the cost function:

$$C(Y,w) = Y \left(\int_{\underline{w}}^{\overline{w}} \phi_i^{\sigma} w_i^{1-\sigma} di \right)^{\frac{1}{1-\sigma}} \tag{8}$$

Plugging the cost function into equation 7 leads to the conditional labor demand function:

$$l_i = Yc(w)^{\sigma}\left(\frac{\phi_i}{w_i}\right)^{\sigma}$$

where $c(w) = \left(\int_{\underline{w}}^{\overline{w}} \phi_j^{\sigma} w_j^{1-\sigma} dj\right)^{\frac{1}{1-\sigma}}$ is the unit cost of production.

Next, we introduce a minimum wage $MW > \underline{w}$. The new labor demand for each type of worker can be written as:

$$l_i = \begin{cases} Y\left(\frac{\phi_i}{MW}\right)^{\sigma} c(MW, w)^{\sigma} & if \quad w_i \leq MW \\ Y\left(\frac{\phi_i}{w_i}\right)^{\sigma} c(MW, w)^{\sigma} & if \quad w_i > MW \end{cases}$$

where $c(MW, w) = \left(\int_{\underline{w}}^{MW} \phi_j^{\sigma} MW^{1-\sigma} dj + \int_{MW}^{\overline{w}} \phi_j^{\sigma} w_j^{1-\sigma} dj\right)^{\frac{1}{1-\sigma}}$ is the unit cost of production given a minimum wage MW.

The effect of the minimum wage on the wage distribution is highlighted in Figure A.1. Under perfect compliance, all worker types with wages below MW are pushed up to the new level, creating a spike at the new minimum wage. The size of the spike is given by the following formula:

$$a = \int\limits_{\underline{w}}^{MW} Y\left(\frac{\phi_j}{MW}\right)^{\sigma} c(MW, w)^{\sigma} dj$$

When starting from an economy without a minimum wage, the number of workers who would earn below the minimum wage, $b_{\text{no MW}}$, can be expressed as

$$b_{\text{no}MW} = \int\limits_{\underline{w}}^{MW} Y\left(\frac{\phi_j}{w_j}\right)^{\sigma} c(MW, w)^{\sigma} dj$$

and the change in employment for all workers who would earn below $w_i \leq MW$ without a wage floor is as follows:

$$\triangle e = a - b_{\text{noMW}} = \int\limits_{\underline{w}}^{MW} \left[Y\left(\frac{\phi_j}{MW}\right)^{\sigma} c(MW, w)^{\sigma} - Y\left(\frac{\phi_j}{w_j}\right)^{\sigma} c(MW, w)^{\sigma}\right] dj$$

The formula above highlights that the bunching estimator captures the employment change for the targeted group ("low-wage" workers). At the same time, the increase in the unit cost of production, $c(w)^{\sigma}$, indicates that labor demand for workers earning above the minimum wage increases. This is the classic labor-labor substitution effect: the cost increase of low-wage labor pushes up the demand for high wage labor. The bunching estimator proposed here, therefore, will provide an estimate on the overall effect of the minimum wage on the targeted low-wage population without taking into consideration the job gains at the upper tail of the wage distribution. Therefore, if someone is interested in employment changes throughout the whole wage distribution, then the estimates provided here will overestimate the disemployment effect of the minimum wage.

It is also worth pointing out that the standard labor demand model can explain the spike at the minimum wage, but it fails to predict ripple effects that are concentrated slightly above the minimum wage (e.g. see the illustration in Figure 1). To capture that property of the spillovers, we need to introduce measurement error in wages (Autor, Manning and Smith (2016)), distance based substitution across various labor types (Teulings (2000)) or some frictions (e.g. Flinn (2011)).[48]

Our empirical approach identifies the employment effects by exploiting variation in the minimum wage. The effect of the minimum wage change on the excess number of jobs is given by the following formula:

$$\frac{\partial a}{\partial MW} = Y\left(\frac{\phi_i}{MW}\right)^{\sigma} c(w)^{\sigma} - \sigma\frac{1}{MW}\left(1 - s_{MW}\right)\left(\int_{\underline{w}}^{MW} Y\left(\frac{\phi_j}{MW}\right)^{\sigma} c(w)^{\sigma} dj\right) \tag{9}$$

where $s_{MW} = \frac{\int_{\underline{w}}^{MW} \phi_j^{\sigma} MW^{1-\sigma} dj}{\int_{\underline{w}}^{MW} \phi_j^{\sigma} MW^{1-\sigma} dj + \int_{MW}^{\overline{w}} \phi_j^{\sigma} w_j^{1-\sigma} di}$ is the cost share of workers who earn the minimum wage. The change in the number of jobs below the minimum wage is:

$$\frac{\partial b}{\partial MW} = -Y\left(\frac{\phi_i}{MW}\right)^{\sigma} c(w)^{\sigma}$$

The number of workers who are directly affected by the minimum wage change is given by $b = \int_{\underline{w}}^{MW} Y\left(\frac{\phi_j}{MW}\right)^{\sigma} c(w)^{\sigma} dj$. Notice that the empirical measure of exposure to the minimum wage, b, is not the same as the total number of workers who would earn below the minimum wage in its absence, b_{noMW}, if the level of the initial minimum wage is $MW > \underline{w}$. This is because the measure of directly affected workers does not include those who had already lost their jobs due to the presence of a minimum wage, even prior to the minimum wage increase being considered.

As a result the percentage change in employment is given by the following formula:

$$\frac{\%\triangle e}{\%\triangle MW} = \frac{\partial a}{\partial MW}\frac{MW}{b} + \frac{\partial b}{\partial MW}\frac{MW}{b} = -\sigma(1 - s_{MW})$$

Profit Maximization. To solve the profit maximization problem, we use the cost function derived

[48]If labor supply is inelastic, there will be spillover effect on wages in the model presented here. However, such a spillover effects will not be concentrated at the bottom of the wage distribution.

above. We assume that perfectly competitive firms maximizes their profits

$$\max_{Y} PY - C(Y, w)$$

where $C(Y, w)$ is given by equation A.1. To close the model, we assume that the aggregate output is negatively related to the price of the output good, P.

The FOC from the firm's optimization problem is the following:

$$P = \frac{\partial C(Y, w)}{\partial Y} = c(w) = \left(\int_{\underline{w}}^{\overline{w}} \phi_i^{\sigma} w_i^{1-\sigma} di \right)^{\frac{1}{1-\sigma}}$$

and in the presence of a binding minimum wage, the preceding formula can be written as follows:

$$P = c(MW, w) = \left(\int_{\underline{w}}^{MW} \phi_j^{\sigma} MW^{1-\sigma} dj + \int_{MW}^{\overline{w}} \phi_j^{\sigma} w_j^{1-\sigma} di \right)^{\frac{1}{1-\sigma}}$$

The price increase in response to the minimum wage is equal to the share of the expenses on minimum wage workers in total production:

$$\frac{\partial P}{\partial MW} = \frac{\int_{\underline{w}}^{MW} \phi_j^{\sigma} MW^{1-\sigma} dj}{\int_{\underline{w}}^{MW} \phi_j^{\sigma} MW^{1-\sigma} dj + \int_{MW}^{\overline{w}} \phi_j^{\sigma} w_j^{1-\sigma} di} = s_{MW}$$

The change in the output price, P, will affect the output level; therefore, we need an additional term in the formula for the number of excess jobs derived above (see equation 9):

$$\frac{\partial a}{\partial MW} = \underbrace{Y \left(\frac{\phi_i}{MW} \right)^{\sigma} c(w)^{\sigma} - \sigma \frac{1}{MW} (1 - s_{MW})\, b}_{\text{substitution effect}} \quad \underbrace{+ \frac{1}{Y} \frac{\partial Y}{\partial P} \frac{\partial P}{\partial MW} b}_{\text{scale effect}}$$

The first part of the formula above is the same as the one derived in the cost minimization problem, and solely reflects the substitution effect. The second part reflects the change in the scale of production. Assuming a constant product demand elasticity, $\frac{\Delta \log Y}{\Delta \log p} = -\eta$, we get:

$$\frac{\%\triangle e}{\%\triangle MW} = \underbrace{-\sigma(1-s_{MW})}_{\text{substitution effect}} \qquad \underbrace{-\eta s_{MW}}_{\text{scale effect}}$$

We want to make two important points about the preceding formula. First, note that when the minimum wage is set at a low enough level so that $MW \approx \underline{w}$, then

$$s_{MW} = \frac{\int_{\underline{w}}^{MW} \phi_j^{\sigma} MW^{1-\sigma} dj}{\int_{\underline{w}}^{MW} \phi_j^{\sigma} MW^{1-\sigma} dj + \int_{MW}^{\overline{w}} \phi_j^{\sigma} w_j^{1-\sigma} di} \approx 0$$

and the size of the bunching is equal to $-\sigma$. In that case, our estimator directly identifies the uncompensated labor-labor substitution parameter across various worker types. A large spike indicates that the $\%\triangle e$ is small, and so is the substitution elasticity, σ; in contrast, if there is no bunching at the minimum wage, then $\%\triangle e$ is large and so is σ. This result is analogous to Saez (2010), who identifies the uncompensated labor supply demand elasticity in the frictionless model from the bunching at tax kink points.

Second, in the U.S. context the cost share of the minimum wage workers, s_{MW} will be very small in practice. In our sample around 8.6% (see Column 1 in Table 1) of the workers are directly affected by the minimum wage and the minimum to average wage is around 0.25, which indicates that $s_{MW} = 0.25 * 0.086 = 0.02$. Therefore, our bunching estimates on employment will be closely related to σ.

Inelastic Labor Supply. So far, we have assumed that firms can hire as many workers as they want at a given wage. We now allow for an inelastic labor supply. In this case, the wage level is no longer exogenous, and so there is not a one-to-one relationship between workers' types and their wage. Therefore, we use the following production function:

$$Y = \left(\int_{j \in \Omega} \phi_j l_j^{\frac{\sigma-1}{\sigma}} dj \right)^{\frac{\sigma}{\sigma-1}}$$

where Ω is the set of worker types. For simplicity, we assume that the labor supply elasticity, λ, is constant across labor types:

$$w_j = \kappa_j l_j^{\frac{1}{\lambda}} \tag{10}$$

Firms minimize their cost given wages, which leads to the conditional labor demand

$$l_i = Yc(w)^{\sigma}\left(\frac{\phi_i}{w_i}\right)^{\sigma} = Yc(w)^{\sigma}\left(\frac{\phi_i}{\kappa_j l_j^{\frac{1}{\lambda}}}\right)^{\sigma}$$

where $c(w) = \left(\int_{j\in\Omega}\phi_j^{\sigma}w_j^{1-\sigma}dj\right)^{\frac{1}{1-\sigma}}$ is the unit product cost defined above. Wage determination follows from the equation of supply and demand for each type of labor:

$$l_i = Yc(w)^{\sigma}\left(\frac{\phi_i}{w_i}\right)^{\sigma} = Yc(w)^{\sigma}\left(\frac{\phi_i}{\kappa_j l_j^{\frac{1}{\lambda}}}\right)^{\sigma}$$

This can be rearranged to express labor demand and equilibrium wages:

$$l_i = Y^{\frac{\lambda}{\lambda+\sigma}}c(w)^{\frac{\lambda\sigma}{\lambda+\sigma}}\left(\frac{\phi_i}{\kappa_i}\right)^{\frac{\lambda\sigma}{\lambda+\sigma}}$$

$$w_i = \kappa_i l_i^{\frac{1}{\lambda}} = Y^{\frac{1}{\lambda+\sigma}}c(w)^{\frac{\sigma}{\lambda+\sigma}}a_i^{\frac{\sigma}{\lambda+\sigma}}\kappa_i^{\frac{\lambda}{\lambda+\sigma}}$$

Given that $c(w)$ is a function of individual wages, the preceding equation is not an equilibrium solution. However, for our purpose, these equations will suffice. The above equations also highlights that wages are increasing in both a_i and κ_i, while the employment depends on the ratio $\frac{a_i}{\kappa_i}$. Without loss of generality, assume that a_i is increasing in i. Moreover, assume that κ_i is (weakly) increasing in i, and so equilibrium wages, w_i, will increase in a_i. Note that this assumptions does not restrict the shape of wage distribution, since the level of employment at a given wage depends on $\frac{a_i}{\kappa_i}$, which can be increasing or decreasing even if κ_i increasing in a_i.

Now introduce a binding minimum wage, and so for some workers the minimum wage is larger then their equilibrium wage,. Under a binding minimum wage workers can be at three qualitatively distinct regimes. The first group of workers are those whose equilibrium wages are substantially below the minimum wage. Employers are now forced to pay these workers MW even if at that wage level labor supply exceeds demand. All of these workers end up at heaping at the minimum wage, creating a spike. Whether a worker belongs to this category or not depends on the unit cost of production and the size of $\phi_i^{\frac{\sigma}{e+\sigma}}\kappa_i^{\frac{e}{e+\sigma}}$. Since w_i is increasing a_i, there will be threshold $\phi(MW)$ below which all workers will belong to that category, and so employment is determined by labor demand. Therefore, the number of workers at the minimum wage spike will be given by

$$a = \int_{\underline{\phi}}^{\phi(MW)} Y\left(\frac{\alpha_j}{MW}\right)^{\sigma}c(w, MW)^{\sigma}dj$$

where

$$c(w,MW)=\left(\int\limits_{j\in\Omega}\phi_j^{\sigma}w_j^{1-\sigma}dj\right)^{\frac{1}{1-\sigma}}=\left(\int\limits_{\underline{\phi}}^{\phi(MW)}\phi_j^{\sigma}MW^{1-\sigma}dj+Y^{\frac{1-\sigma}{\lambda+\sigma}}c(w)^{\frac{(1-\sigma)\sigma}{\lambda+\sigma}}\int\limits_{\phi(MW)}^{\overline{\phi}}\phi_j^{\sigma+\frac{(1-\sigma)\sigma}{\lambda+\sigma}}\kappa_j^{\frac{(1-\sigma)\lambda}{\lambda+\sigma}}dj\right)^{\frac{1}{1-\sigma}} \tag{11}$$

.

The second group of workers are those who earn slightly below or at the minimum wage. As a result of the minimum wage shock, the unit cost labor $c(w, MW)$ increases, which pushes up labor demand for this group of workers. When labor supply is inelastic, this lead to an increase in wages: as a result, some workers whose equilibrium wage was close to the minimum wage will now be pushed slightly above it creating a spillover effect. These workers' employment increases and their wages will be pushed slightly above the minimum wage. Therefore, in the model with inelastic labor supply, setting $\overline{W}$ slightly above the minimum wage is desirable if we aim to estimate the effect of the minimum wage on those directly affected by the minimum wage.

The third group of workers' initial equilibrium wage is above the minimum wage. The increase in the unit labor cost $c(w, MW)$ makes these workers relatively cheaper and so the demand for labor increase. This leads to higher wages when labor supply is inelastic, and so the whole wage distribution is shifted slightly to the right. To sum up, labor demand is now given by the following equation:

$$l_i=\begin{cases}Y\left(\frac{\phi_i}{MW}\right)^{\sigma}c(MW,w)^{\sigma} & if \quad \phi_i<\phi(MW)\\ Y\left(\frac{\phi_i}{w_i}\right)^{\sigma}c(MW,w)^{\sigma} & if \quad \phi_i\geq\phi(MW)\end{cases}$$

Since our empirical approach identifies the employment effects by exploiting variation in the minimum wage, we now turn to understanding the effect of changing the minimum wage on the number of excess and missing jobs. The effect of the minimum wage change on the number of excess jobs is given by the following formula:

$$\frac{\partial a}{\partial MW}=Y\left(\frac{\phi_i}{MW}\right)^{\sigma}c(w)^{\sigma}\frac{\partial\phi(MW)}{\partial MW}-\sigma\left(\frac{1}{MW}-\frac{1}{c(MW,w)}\frac{\partial c(MW,w)}{\partial MW}\right)\left(\int\limits_{\underline{\phi}}^{\phi(MW)}Y\left(\frac{\phi_i}{MW}\right)^{\sigma}c(w)^{\sigma}di\right) \tag{12}$$

Note that differentiating equation 11 with respect to the minimum wage will lead the following expression (using the Leibniz integral rule):

$$\frac{\partial c(w,MW)}{\partial MW} = s_{MW}\frac{c(w,MW)}{MW} + \frac{\partial c(w,MW)}{\partial MW}(1-s_{MW})\frac{\sigma}{\lambda+\sigma}$$

where $s_{MW} = \frac{\int_{\underline{\phi}}^{\phi(MW)} \phi_j^{\sigma} MW^{1-\sigma} dj}{\int_{\underline{\phi}}^{\phi(MW)} \phi_j^{\sigma} MW^{1-\sigma} dj + \int_{\phi(MW)}^{\overline{\phi}} \phi_j^{\sigma} w_j^{1-\sigma} di}$. By rearranging this equation, one can express the derivative of the unit cost function with respect to the minimum wage:

$$\frac{\partial c(w,MW)}{\partial MW} = s_{MW}\frac{c(w,MW)}{MW}\frac{1}{1-(1-s_{MW})\frac{\sigma}{\lambda+\sigma}}$$

Plugging this back to equation 12 leads to the following expression:

$$\frac{\partial a}{\partial MW} = Y\left(\frac{\phi_i}{MW}\right)^{\sigma} c(w)^{\sigma} - \sigma\frac{1}{MW}\left(1 - s_{MW}\frac{1}{1-(1-s_{MW})\frac{\sigma}{\lambda+\sigma}}\right)\left(\int_{\underline{\phi}}^{\phi(MW)} Y\left(\frac{\phi_i}{MW}\right)^{\sigma} c(w)^{\sigma} di\right)$$

The change in the number of jobs below the minimum wage is:

$$\frac{\partial b}{\partial MW} = -Y\left(\frac{\phi_i}{MW}\right)^{\sigma} c(w)^{\sigma}\frac{\partial a(MW)}{\partial MW}$$

The number of workers who are directly affected by the minimum wage change is given by $b = \int_{\underline{w}}^{MW} Y\left(\frac{a_j}{MW}\right)^{\sigma} c(MW,w)^{\sigma} dj$. As a result, the percentage change in employment is given by the following formula:

$$\frac{\%\triangle e}{\%\triangle MW} = \frac{\partial a}{\partial MW}\frac{MW}{b} + \frac{\partial b}{\partial MW}\frac{MW}{b} = -\sigma\left(1 - s_{MW}\frac{1}{1-(1-s_{MW})\frac{\sigma}{\lambda+\sigma}}\right) = -\sigma\left(\frac{\lambda - s_{MW}\lambda}{\lambda + s_{MW}\sigma}\right)$$

When s_{MW} is close to zero, which is a good approximation in the U.S. context, the minimum wage bunching estimate is again very close to σ, the conditional substitution elasticity across various types of labor.

Online Appendix C Data Appendix

The primary data set employed is the individual-level NBER Merged Outgoing Rotation Group of the Current Population Survey for 1979-2016 (CPS). In the latter data, we employ variables EARNHRE (hourly wage), EARNWKE (weekly earnings), and UHOURSE (usual hours) to construct hourly wage variable. For the period after 1995q4, we exclude observations whose hourly wages are imputed (I25a>0) among those with positive EARNHRE values, and exclude observations whose usual weekly earnings or hours information are imputed (I25a>0 or I25d>0) among those with positive EARNWKE values. Between 1994q1 and 1995q3, there is no information on the imputation, 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 substantial sampling error in the dataset. In addition, in most of our analysis, we are not using observations with imputed hourly wages in our data sets, which means the employment counts of the raw CPS data are biased downwards. To reduce the sampling error and also address the undercount due to dropping imputed observations, our primary sample combines the CPS wage densities with the true state-level employment counts from the QCEW (E) and state-level population estimates from the Census (N). Specifically, in the QCEW benchmarked CPS, employment counts for a wage bin w is calculated as $\widehat{\frac{E_w}{N}}^{QCEW} = \widehat{f_w}^{CPS} \times \frac{E}{N}$, where $\widehat{f_w}^{CPS}$ is the (discretized) wage density estimated using the CPS: $\hat{f_w}^{CPS} = Prob(w \leq wage < w + 0.25)$. We also do a similar benchmarking by NAICS-based industry-and-state-specific QCEW employment (between 1990-2016) 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\$) 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 D.

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 in separately estimating the impacts of the minimum wage on new entrant and incumbent workers. This requires matching two CPS files. We 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 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 between July to December 1984 and 1985, or between January to September 1985 and 1986, or between June to December 1994 and 1995,or between January to August 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 deemed 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 $500M. 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 IND80 and IND02 variables in the CPS.

Online Appendix D Comparison of Administrative Data to CPS

In our pooled event study analysis, we use the Current Population Survey (CPS), which provide information on wages for a large sample of individuals, after benchmarking to aggregate state-level employment counts in the QCEW. As a results, when we estimates job counts at each wage bins there is some error caused by observing a sample instead of the whole population. In this section we assess the accuracy of CPS based jobs counts by contrasting administrative data job counts from three states with reliable information on hourly wages (Minnesota, Oregon, and Washington).

In section D.1 we show that the QCEW-benchmarked CPS closely matches the administrative data counts at the aggregation-level matters for our analysis. In particular, we show that the number of workers reporting earning 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 mis-reporting in the CPS. In section D.2 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 shows that the implied misreporting is of similar magnitudes in the two data sources.

D.1 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 D.1 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 shows 5 years employment changes in \$3 to \$5 bins relative to the minimum wage. The red squares shows the distribution in the administrative data while the blue dots depict the distribution calculated usingQCEW 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 examined here (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 sub-minimum jobs may reflect misreporting. If this is the case, then Figure D.1 suggests that the extent of misreporting is quite similar in the CPS and in the administrative data. We formally test this latter in the next section. At the same time, we should point out that some of the sub-minimum wage jobs 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—contain 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 D.2; as a consequence, our difference-in-difference estimates are not likely to be substantially affected by this gap between the two counts. Finally, the CPS tends to place slightly more workers at the middle-income ($[MW + \$6, MW + \$21)$), and fewer workers at the high-income bins ($[MW + \$21, \infty)$).

Figure D.2 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 line 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-differences estimator implemented in this paper is unlikely to be affected by the small discrepancy between the administrative and the CPS data.

D.2 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.[49] A portion (γ) of the workers report their wages correctly, while others report it with some error. The observed latent (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. D is a binary variable that is equal to 1 when the wage is mis-reported, and 0 otherwise. Therefore, $P(D = 0) = \gamma$ measures the probability of accurate reporting of wages. When the wage is mis-reported, the distribution of the (logged) error is

[49] The latent wage distribution refers to the distribution that would prevail in the absence of a minimum wage.

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 mis-reporting distorts the observed wage distribution. Here $1 - \gamma$ measures the rate of mis-reporting, while $\frac{1-\rho^2}{\rho^2}$ measures the variance of the error conditional on mis-reporting.

We can summarize the overall importance of mis-reporting 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, mis-reporting 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 adminstrative data (at least in terms of estimating $1 - \gamma$). For this reason, we present two sets of estimates. Besides keeping the data as is, 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 workers in our analysis.

Table D.1 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). 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, now the estimated misreporting rate (1-γ) increases, while the variance of the error conditional on misreporting $\left(\frac{1-\rho^2}{\rho^2}\right)$ declines.

Overall, the ratio of the true and observed standard deviations for administrative data in panel B (.90) remains very similar to those reported in panel A (.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.

Figure D.1: Frequency Distributions in the Administrative and CPS data

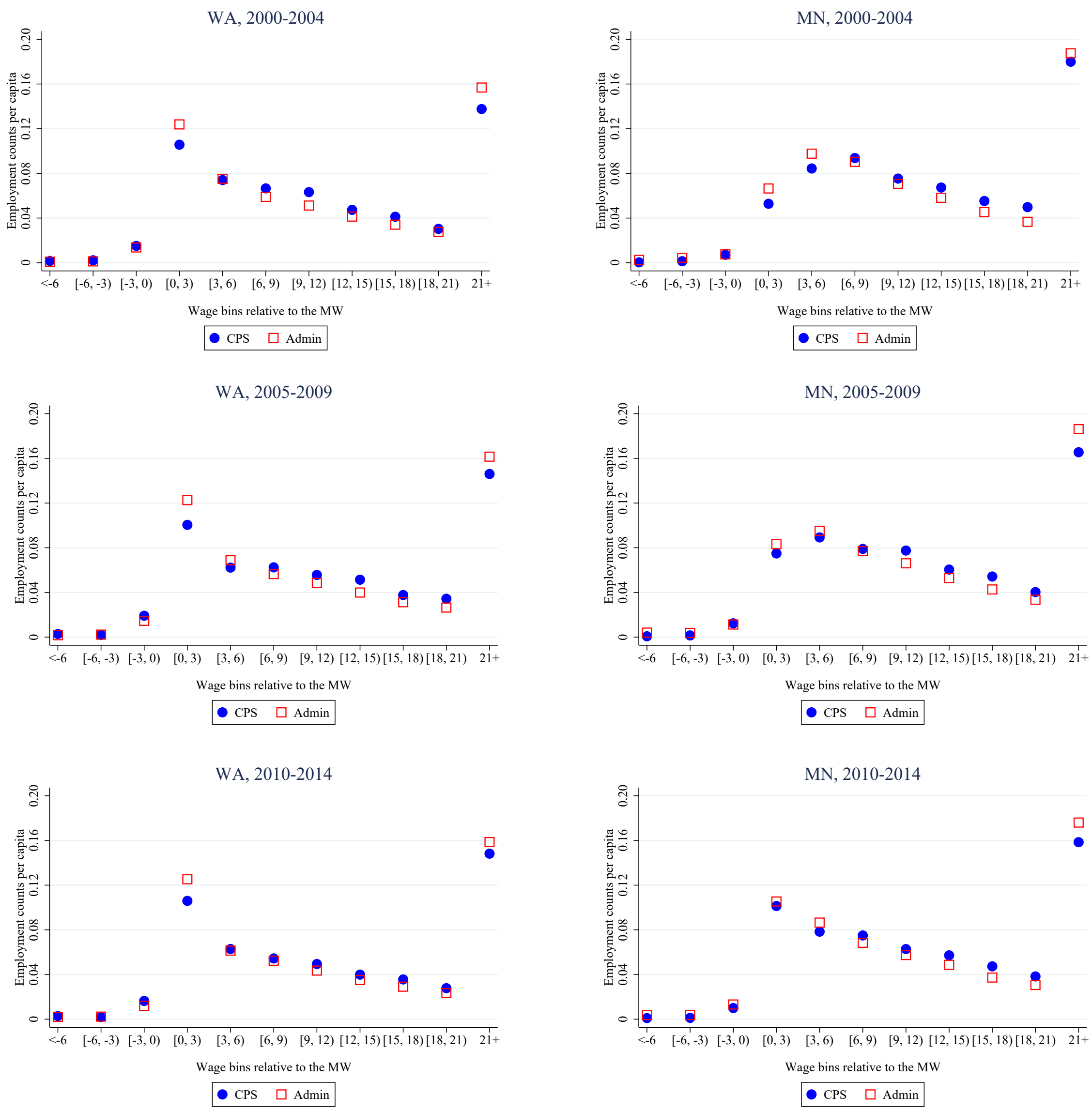


Figure D.1 cont'd: Frequency Distributions in the Administrative and CPS data

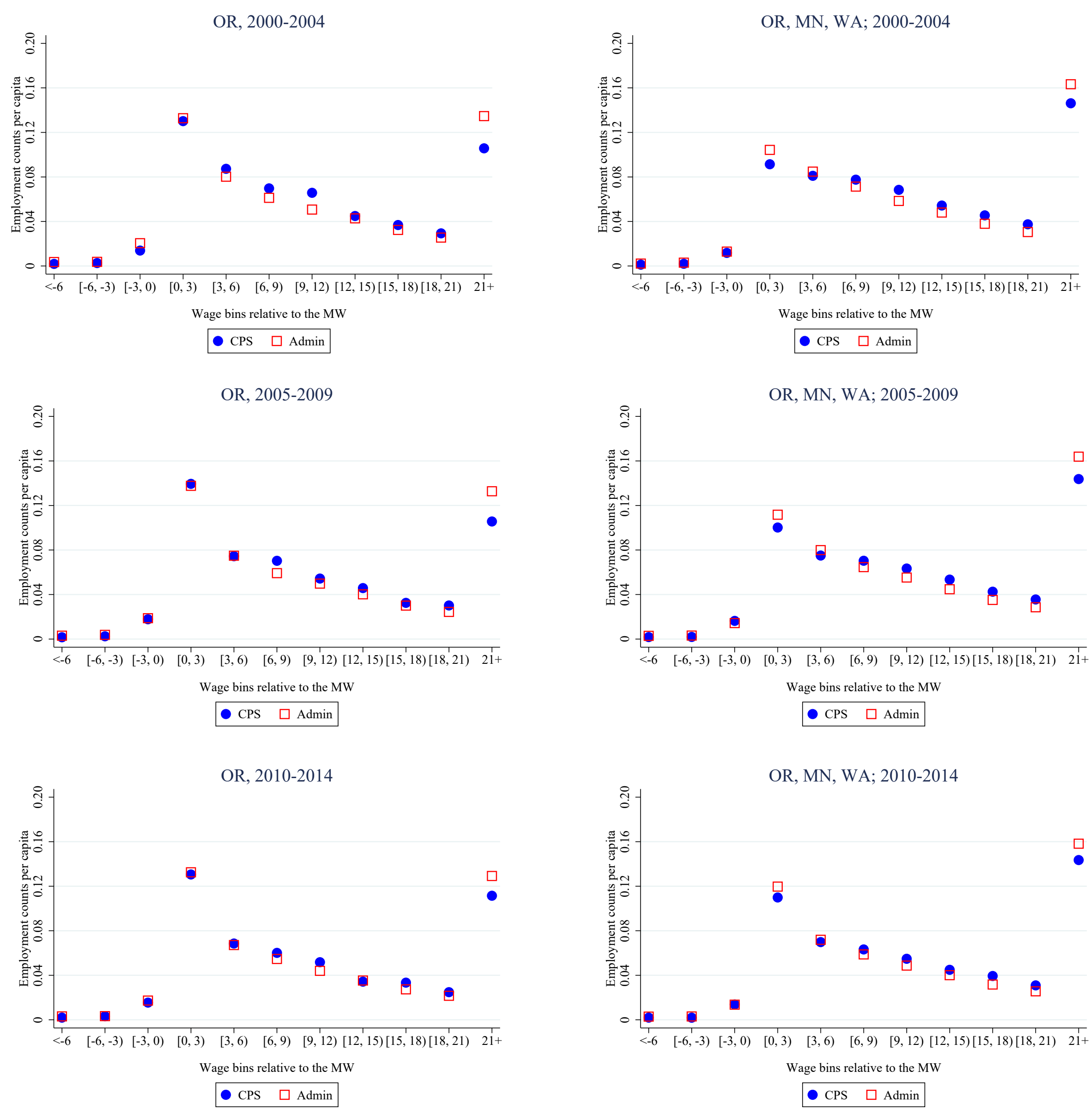
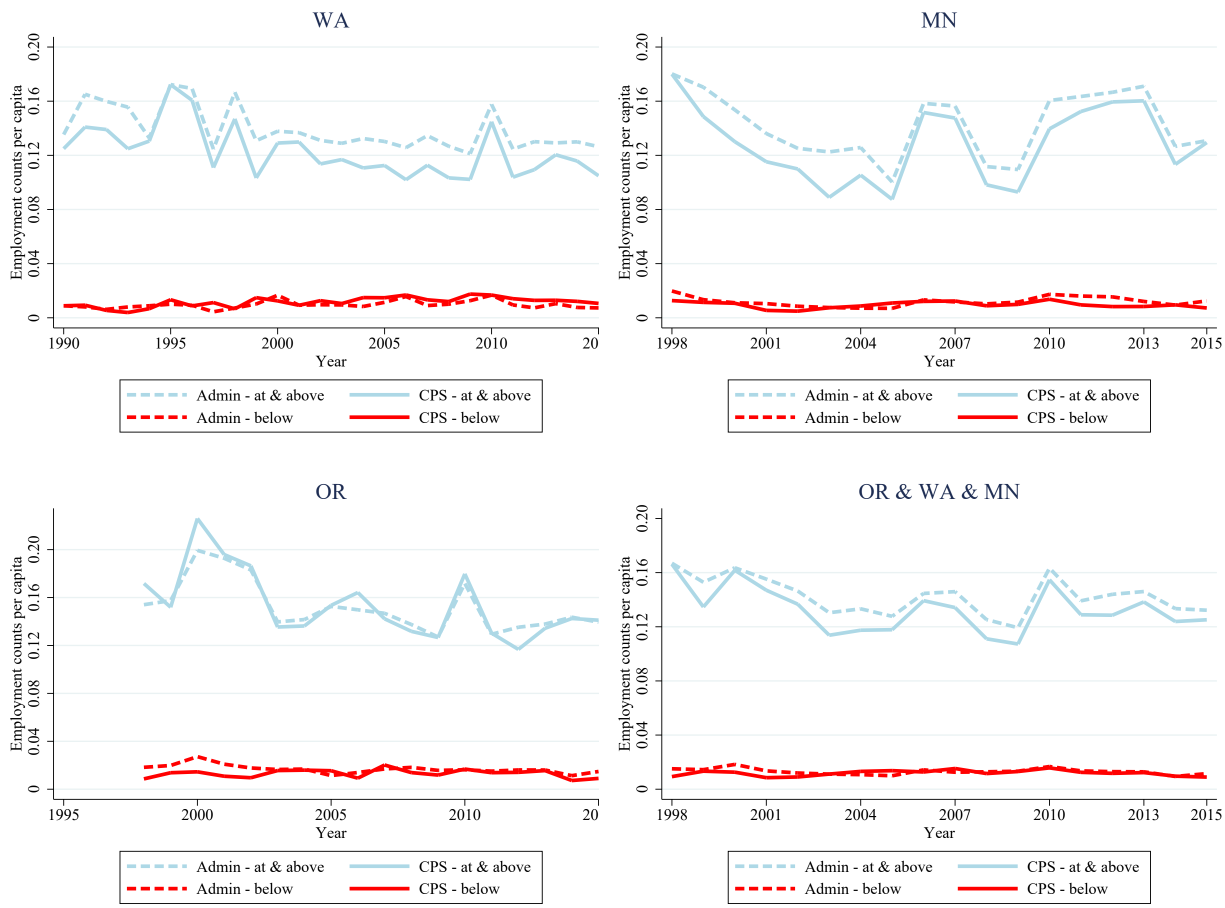


Figure D.2: Comparing Administrative and CPS data; Time path



Notes: TBA

Table D.1: Structural Estimation of the Autor, Manning and Smith (2016) Model of Measurement Error in Wages: Evidence from CPS and Administrative Data

Dataset	1-γ	$\frac{1-\rho^2}{\rho^2}$	$\frac{\sigma_w}{\sigma}$
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 mis-reporting rate, in Column 1; $(1-\rho^2)/\rho^2$, the variance of the error conditional on mis-reporting in Column 2; and the ratio of the standard deviation of the true latent distribution (w) and the observed latent distribution in Column 3.