

The effect of minimum wages on the total number of jobs: Evidence from the United States using a bunching estimator*

April 30, 2017

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Abstract

We estimate the total impact of the minimum wage on affected employment by comparing the excess number of jobs just above the new minimum wage following an increase to the reduction in the number of jobs below the minimum. Using variation in state minimum wages in the United States between 1979 and 2016, we find that, on average, the number of missing jobs paying below the new minimum during the five years following implementation closely matches the excess number of jobs paying just above minimum. This leaves the overall number of low-wage jobs essentially unchanged, while raising average earnings of workers below those thresholds. The confidence intervals from our primary specification rule out minimum wage elasticities of total employment below -0.06 , which includes estimates from the existing literature. These bunching estimates are robust to a wide set of assumption about patterns of unobserved heterogeneity such as regional differences or state-specific trends, measurement error in reported wages, and the precise definition of the wage band used in the bunching approach. Our estimates for the subset of minimum wage changes that affect a large share of workers are similar to the main estimates. We also provide estimates for specific demographic groups that are policy-relevant or studied in the literature including: teens, women, workers without a college degree, women, and black/Hispanic workers. While the affected share of these groups vary considerably, the overall employment effect in each case is small and there is no evidence for substantial labor-labor substitution. We also do not find evidence for substitution away from routine-task intensive occupations. In contrast to the bunching-based estimates, we show that studies that estimate minimum wage effects on total employment can produce misleading inference due to spurious changes in employment higher up in the wage distribution.

*We thank David Card, Alan Manning, Suresh Naidu and participants at WEAI 2016 Annual Meetings, CREAM 2016 conference, Boston University Empirical Micro workshop, Colorado State University, and University of California Berkeley IRLE for very helpful comments. Dube acknowledges financial support from the Russell Sage Foundation. Dube and Lindner acknowledge financial support from the Arnold Foundation.

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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 wage during the past three years, typically substantially above state and federal standards. Underlying much of the policy debate is the central question: what is the overall effect of minimum wages on jobs?

The effect of minimum wage on employment is still a controversial topic among economists. One thread of the literature has continued to find large negative effect on teen employment (Neumark and Wascher 2008). On the other hand, some papers find statistically insignificant, small or positive effects on employment (Card and Krueger 1995; Dube et al. 2010). At the same time, most empirical papers to date have examined the effect of minimum wages changes on the employment of a particular group (teenage workers, restaurant workers) by estimating state- or county-level regressions. This shortcoming is particularly acute given the importance policymakers place on understanding the total employment effect. For example, in its attempt to arrive at such an effect, a 2014 Congressional Budget Office (CBO) noted the paucity of relevant research, and then used estimates for teen minimum wage elasticities to extrapolate the total impact on jobs.¹

In this paper, we provide new estimates on the effect of the minimum wage on the frequency distribution of earnings, and use these estimates to infer the number of jobs destroyed (or created) at the bottom of the wage distribution. We exploit state-level variation in minimum wage changes in the U.S. and estimate the effect of these policy changes on employment by detailed wage bins up to five years following the minimum wage increase. This approach allow us to understand the effect of the minimum wage on employment through-out the whole earnings distribution.

Then, using our new estimates on the earning distribution we assess the extent of bunching at the minimum wage. Building on Harasztosi and Lindner (2016) and Meyer and Wise (1983), we argue that the extent of bunching depends on the behavioral response to the minimum wage. If behavioral responses are limited, e.g., because the different type of labors complements each other, then all workers who earn below the new minimum wage bunch at the minimum wage. On the other hand, if workers are substituted by higher skilled workers, then no bunching will occur at the new minimum wage.² Therefore, we infer the total change in jobs due to the policy by comparing the number of missing jobs below the new minimum wage to the excess

¹Specifically, the report states: “[I]n part because they were the most commonly studied group, CBO arrived at a teen-employment elasticity...[and] then synthesized the teen elasticities with broader research to construct elasticities for adults.”

²A small employment response along with sizable bunching at the minimum wage can also be consistent with a model of monopsonistic competition.

number of jobs paying at (and above) the new minimum wage.³

We use data from the 1979-2016 Current Population Survey to estimate the impact of state-level minimum wage increases, excluding very small changes that were either less than \$0.25/hour, or where less than 2% of the workforce was directly affected. For these 137 minimum wage increases, which have a mean real increase of 10.2%, we find a very clear indication that there was a reduction in the number of workers reporting a wage below the new minimum. However, we also find a clear increase in the number of jobs paying at or above the new minimum, leaving total employment essentially unchanged. Our baseline specification shows that in the five years following the minimum wage increase, average wages of affected workers rose by 7.0%, while employment of affected workers rose by a statistically insignificant 3.0%. Our estimate for the employment elasticity with respect to the affected wage (or the labor demand elasticity in a competitive model) is 0.437, with a standard error of 0.426. This rules out that employment elasticities with respect to wages is more negative than -0.398 at the 95 percent confidence level. Our estimates are also quite robust to a wide variety of specifications for time-varying heterogeneity in employment along the wage distribution, such as wage-bin by state-specific linear or quadratic trends, or allowing the wage-bin by period effects to vary across the nine Census divisions. 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.

When we restrict our sample to those with minimum wages with a substantial bite, we find additional evidence that the total employment of affected workers remains the same. Focusing on 46 events with the largest bite, we estimate that average wages of the affected earners increase significantly by 10.8%. We also find employment is little changed with a statistically insignificant increase of 0.2%. Using these figures, we get 0.017 as the implied elasticity of employment with respect to wage with 0.307 as its standard error, which rules out elasticities smaller than -0.585 at 95 percent confidence level.

We also assess the heterogeneity in the treatment effect by worker demographics. In particular, we separately analyze high school dropouts, those with high school or less schooling, women, black or Hispanic individuals, and teens. Employment elasticities with respect to minimum wage of these groups range between -0.134 and -0.005; but none of them are statistically distinguishable from zero despite the considerably varying share of affected workers. Similarly, we cannot reject the null hypothesis of zero employment elasticity with respect to the wage at the 5% level for any of these groups, though for high school dropouts, the estimate (0.494) is positive and significant at the 10 percent level.

We further advance our analysis of the treatment effect on demographic groups with differing exposures by partitioning education levels into 4 and age into 6 categories. The group-level comparison of excess number of

³The underlying idea behind this estimation is similar to [Saez \(2010\)](#)'s bunching method that non-parametrically identifies behavioral responses by estimating the excess mass at kink-points in the tax schedule.

jobs at (or above) and missing number of jobs below new minimum wage across 23 usable education-by-age groups shows the absence of replacement of low-skilled with high-skilled workers after the minimum wage increase. On average, these groups' missing and excess number of jobs nearly exactly line up on the 45 degree line, and show no indication of systematic skill-based labor-labor substitution. We also assess heterogeneity in the employment response by routine task intensity of occupations, and find little indication of any loss in routine jobs.

We study state-level policies because the change in the number of jobs below the new minimum wage is not separately identified for federal increases using panel variation because there are no covered workers who are supposed to earn below that level in control states. However, the employment effect (sum of the change in the number of jobs below and jobs above the new minimum) is still identified. Our results using state and federal policies suggest very small employment changes for affected workers (-1.2%), along with substantial increases in the affected wage (8.8%).

There are several advantages of using our bunching method relative to standard estimation techniques. First, our approach identifies the employment effect of the minimum wage in a very transparent way. We show the evolution over time in the jobs paying below the (new) minimum wage in treatment and control states, allowing us to measure the bite of the policy. Tracking the causal effect on the number of jobs paying below the new minimum wage over time not only allows us to evaluate the presence of differential trends before the minimum wage hike, but also to assess whether real minimum wage changes caused by inflation or real wage growth contaminate the results ([Sorkin 2015](#)).

Second, by accounting for differences across states in the pre-treatment shares of workers at various parts of the earnings distribution, our method controls for a variety of confounders. For instance, in the presence of skill-biased technical change, employment of low wage workers would decline even in the absence of the minimum wage change. If states vary in their exposure to such a common technological change, not taking this into account may produce a biased estimate of employment effects ([Allegretto et al. 2017](#)).

Third, and relatedly, our method calculates the changes in employment at wages where employment is likely to be affected by the minimum wage, and it does not make use of employment changes at the upper tail of the wage distribution. The latter is unlikely to be a causal effect of changes in minimum wages, and so excluding them from the job count both improves the precision of the estimation and alleviates the influence of some confounding factors that influence the upper tail, such as demand shifts caused by skill-biased technological change or tax policies introduced by left leaning state governments. As a result, we are able to recover the total effect of minimum wages on employment, similar to [Meer and West \(2016\)](#) but unlike them, we do so without using aggregate employment as the outcome. Excluding workers earning far above the minimum wage is especially important in the U.S. context given that minimum wage only affects a small

fraction of the population directly (typically around 9% for the workforce in the events we study). Therefore, even small changes at the upper tail of distribution can have large consequences on the estimated change in employment. We show that in contrast to using the bunching method, standard regressions of aggregate employment on the minimum wage are very sensitive to alternative specifications and, because these methods do not illustrate where the employment effects occur in the wage distribution, they can spuriously attribute to the minimum wage large employment changes in the upper tail.

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 as well as under monopsonistic competition. This section also develops the empirical specification. Section 3 describes our data and sample construction. Section 4 presents our empirical findings including the main results, effect heterogeneity by type of worker characteristics as well as type of treatments, and additional robustness checks for sample and specification. We present in section 5 evidence that the bunching method guards against bias from employment movements in the upper tail. Section 6 concludes.

2 Methodology

Bunching Estimator. Similar to [Harasztosi and Lindner \(2016\)](#), we infer the employment consequences of the minimum wage from the changes in the earnings distribution. The underlying idea behind our identification approach is explained in [Figure 1](#). The figure shows the frequency distribution of (hourly) earnings in the absence of a minimum wage (red line), as well as the distribution with a binding minimum wage, MW (blue line).

When the minimum wage is introduced, covered workers with wages below the new minimum wage cannot be legally paid at their old wage. B denotes the number of jobs below the new minimum wage under the old earnings distribution, and ΔB denotes the change in those jobs after the minimum wage is introduced. Note that, in practice, some workers will earn below the new minimum wage even after its introduction so that ΔB is not necessarily equal to $-B$, as illustrated. The presence of sub-minimum jobs can come from imperfect coverage (e.g. employers are allowed to pay below the minimum wage in some cases) or from imperfect compliance with the policy. Jobs with sub minimum wages may also reflect the presence of measurement error in the reported wage.

The minimum wage might destroy some sub-minimum wage jobs and as a consequence these jobs disappear from the wage distribution. Other workers might keep their jobs and get the pay-rise to comply with the new minimum wage. The minimum wage might also attract some low-skilled workers to search for a job, which in fact can lead to job creation in non-competitive labor markets. Both the continuing and the newly created

jobs will appear somewhere in the new earnings distribution, and so the sum of the missing jobs below the minimum wage and the extra jobs created in the new earning distribution will provide an estimate on the employment loss (or gain).

Most (if not all) extra jobs in the earnings distribution will emerge at or slightly above the minimum wage. Many workers get a pay raise to the new minimum creating a spike in the wage distribution. Some workers may also experience a bigger than necessary pay increase and move above the new minimum wage; however it is unlikely that these workers will be paid substantially above the minimum wage. It is also possible that some workers above the minimum wage are pushed further up as a result of some spillover effects on them. However, these spillover effects are likely to fade-out at higher wages, so the jobs in the upper-tail of the wage distribution are unlikely to be affected.

Figure 1 illustrates a situation where the extra number of jobs in the new earning distribution concentrates between MW and \bar{W} . The spike and the bunching slightly above the MW and the convergence in the upper-tail of the distribution are usual features shown in many empirical studies. Moreover, such an effect on earning distribution naturally emerges from search and matching models (see Flinn (2011) or Manning (2003)) and from a neoclassical model where workers with similar wages are close substitutes (Teulings (2000)).⁴

Our identification strategy exploits that the excess jobs in the new earning distribution are concentrated between MW and \bar{W} . Instead of adding up all the job changes across the whole earning distribution, we only calculate the number of excess jobs at and slightly above it, which is denoted with ΔA in Figure 1. Therefore, our measure of the total employment effect of the minimum wage will calculate the sum of the missing number of jobs and the excess number of jobs between MW and \bar{W} , formally $\Delta E = \Delta A + \Delta B$.

Our bunching estimator does not make use of employment changes at the upper tail of the wage distribution. Such changes to upper tail employment are unlikely to reflect a causal effect of minimum wages. Therefore, excluding them from the job count improves the precision of the estimation, and alleviates the influence of some confounding factors that influence the upper tail, such as demand shifts caused by skill-biased technological change, or tax policies introduced by state governments.

To implement the employment estimate proposed above we need to calculate the number of missing jobs below the minimum wage, ΔB , and the number of excess jobs between MW and \bar{W} , ΔA . There are two main empirical challenges in estimating these objects. First, we do not know a priori the threshold above which the minimum wage has not affect on employment. To deal with that we try various reasonable values for \bar{W} .

Second, to calculate ΔA and ΔB we need to know the counterfactual wage distribution in the absence of

⁴While the Teulings model can explain an elevated earning distribution above the minimum, it fails to create a spike in the earnings distribution.

the minimum wage. While it is not possible to observe that directly, there are various way to approximate that counterfactual distribution.⁵ In this paper we exploit state-level variation in U.S. minimum wage and implement an event-study. We compare the earning distribution of states *without* a minimum wage to the earnings distribution of the states *with* a minimum wage change. Intuitively, we use the states not exposed to the minimum wage as a counterfactual of the earning distribution with lower minimum wages.

Standard labor demand model. Change in $\Delta A + \Delta B$ in response to the minimum wage is related to the substitution elasticity in the *standard labor demand* model. To show this, we assume that labor demand is determined by perfectly competitive firms maximize their profits, formally:

$$\max_{l_j} pY - \int_{\underline{w}}^{\bar{w}} l_j w_j dj$$

where each labor j represents the employment of the workers whose wage would be w_j in the competitive equilibrium. Suppose the production function has the following form $Y = \left(\int_{\underline{w}}^{\bar{w}} a_j l_j^{\frac{\sigma-1}{\sigma}} dj \right)^{\frac{\sigma}{\sigma-1}}$ and that the labors supply is perfectly elastic for each j at w_j .⁶ In Online Appendix A, we show that in that case the the effect of the minimum wage on employment will be the following

$$\frac{\% \Delta Emp}{\% \Delta MW} = \frac{\Delta A + \Delta B}{Emp} \times \frac{1}{\% \Delta MW} = -\eta \frac{\int_{\underline{w}}^{MW} MW \cdot l_j dj}{Y p^\sigma} - \sigma \left(1 - \frac{\int_{\underline{w}}^{MW} MW \cdot l_j dj}{Y p^\sigma} \right)$$

Notice that if the value of the minimum wage is relatively low, which is the case in the U.S. context, then $\frac{\int_{\underline{w}}^{MW} MW \cdot l_j dj}{Y p^\sigma}$ is small, then the formula above simplifies to

$$\frac{\% \Delta Emp}{\% \Delta MW} = \frac{\Delta A + \Delta B}{Emp} \times \frac{1}{\% \Delta MW} = -\sigma$$

Therefore the size of the bunching at the minimum wage, and the total employment effect, depends on the substitution elasticity between various labor inputs. This result is analogous to Saez (2010) that shows that bunching at kink points in marginal tax rate is related to the compensated elasticity of income in the standard labor supply model.

As we show in Appendix B, the bunching approach also consistently estimates the employment effect in a model of monopsonistic competition, where firms with heterogeneous productivities are labor supply constrained. In that model, while a minimum wage increase may produce considerable re-allocation of

⁵Harasztosi and Lindner (2016) uses the pre-minimum wage hike earning distribution to approximate the counterfactual earning distribution.

⁶This simplifying assumption ensures that when minimum wage is raised, employment is determined solely by the labor demand and labor supply does not play any role. In the literature on the effect of taxes on labor supply an analogous assumption is made implicitly about labor demand Saez (2010).

employment across firms, there is no effect on total employment in equilibrium. As long as we choose a sufficiently large \overline{W} , the bunching estimate of $\Delta A + \Delta B$ will consistently estimate the true (zero) employment effect. This means that a small or zero estimated disemployment effect would be consistent with either a monopsonistic competition model, or a standard competitive model of labor demand with a small elasticity of substitution between inputs.

Empirical implementation. We examine the employment effects on per-capita employment in wage bins relative to the minimum wage, $\frac{E_{swt}}{N_{st}}$, 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 define the wage bin relative to the (new) minimum wage and so $k \in \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$ represents the \$1 bins relative to the new minimum wage. The “above” bins are $k \in \{0, 1, 2, 3, 4\}$ while “below” bins are those with $k \in \{-4, -3, -2, -1\}$

We estimate the effect of minimum wage changes in the following event-study framework:

$$\frac{E_{swt}}{N_{st}} = \sum_{\tau=-3}^4 \sum_{k=-4}^4 \alpha_{\tau k} I_{swt}^{\tau k} + \mu_{sw} + \rho_{wt} + e_{swt} \quad (1)$$

where $I_{swt}^{\tau k}$ is a variable that is equal to 1 if minimum wage was raised τ years after date t , and for wage bins w that are within k dollars within the new minimum wage. We examine the effects three years before and four years after the minimum wage change. Our benchmark specification also controls for state-wage bin, μ_{sw} , and time-wage bin effects, ρ_{wt} . This allows us to control for state specific factors in the earning distribution and also the U.S. level evolution of wage inequality.

The estimates on $\alpha_{\tau k}$ allows us to directly assess the change in the jobs paying below and above the new minimum wage in response to the policy. The number of jobs that are directly affected by the minimum wage (relative to total population) is $\alpha_{\tau}^A = \sum_{k=-4}^{-1} \alpha_{\tau k}$, while the excess number of jobs four dollars above the minimum wage is $\alpha_{\tau}^B = \sum_{k=0}^4 \alpha_{\tau k}$.

Our approach shows the evolution over time in the excess number of jobs at or above the new minimum, and the missing jobs below the minimum. For convenience, we normalize these changes by the average number of jobs prior to treatment. The excess number of jobs (per capita) above the minimum wage, averaged over the 5 years following treatment is defined as: $\Delta a = \frac{1}{EPOP_{-1}} \times \frac{4}{5} \sum_{\tau=0}^4 (\alpha_{\tau}^A - \alpha_{-1}^A)$. For interpretational ease, the excess employment is normalized by $EPOP_{-1}$, the sample average EPOP in treated states during the

year (four quarters) prior to treatment. We calculate the estimated missing jobs below the minimum wage analogously: $\Delta b = \frac{1}{EPOP_{-1}} \frac{4}{5} \sum_{\tau=-4}^{-1} (\alpha_{\tau}^B - \alpha_{-1}^B)$. These shares can also be calculated by event time, τ ; for example $\Delta b_{\tau} = \frac{\alpha_{\tau}^B - \alpha_{-1}^B}{EPOP_{-1}}$ is the number of missing jobs per capita between dates -1 and τ , again normalized by average pre-treatment EPOP.

The bunching estimate for the percentage change in total employment due to the minimum wage increase is $\Delta a + \Delta b$. If we divide this by the average percentage change in the minimum wage, 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 share of the workforce earning below the new minimum wage, \bar{b}_{-1} .⁷

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

To contrast the percentage change in employment to the percentage change in wages we use our benchmark regression (equation 1) with an outcome variable on the average wage. Then we use the estimated coefficients to compute the percentage change in the average hourly wage for affected workers as follows:

$$\% \Delta W = \frac{\left[\frac{4}{5} \sum_{\tau=0}^4 \sum_{k=-3}^4 \left(k + \bar{W}_0^{MW} - \bar{W}_{-1}^B \right) \cdot \frac{(\alpha_{\tau k} - \alpha_{-1 k})}{\bar{b}_{-1} \times EPOP_{-1}} \right]}{\bar{W}_{-1}^B}$$

Here, \bar{W}_{-1}^B is the pre-treatment average hourly wages of workers below the new minimum wage; and \bar{W}_0^{MW} is the average wage in the \$1 bin containing the new minimum wage in the year of event. The numerator is the change in the wage due to a reallocation of workers across the wage bins. The ratio in the numerator is the change in employment by wage bin (relative to the minimum) averaged over 5 years following the treatment, normalized by the share of the workforce earning below the new minimum wage. This is multiplied by the distance of wage bin to \bar{W}_{-1}^B to obtain the change in average hourly wage. The denominator is the pre-treatment average wage of workers below the new minimum.

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⁷Notice 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 division is 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 hike, and so it cannot be used for a prospective analysis of the policy's impact.

⁸As an illustration, consider a simplified case where the minimum wage increases by \$1 and all employees are paid in whole dollar amounts. Assuming that the minimum wage affects only those earning less than the new minimum wage, if 80% of all

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), η :

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

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 dynamics in the earnings distributions across states over time. The most saturated specification with respect to these geographic and trend controls can be written as:

$$\frac{E_{swt}}{N_{st}} = \sum_{\tau=-3}^4 \sum_{k=-4}^4 \alpha_{\tau k} I_{swt}^{\tau k} + \mu_{0,sw} + \mu_{1,sw} \times t + \mu_{2,sw} \times t^2 + \rho_{dwt} + \epsilon_{swt} \quad (2)$$

We also estimate a “triple-difference” specification which includes controls for state-by-period fixed effects, which nets out any aggregate state-specific employment shocks. 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 not possible when estimating a state panel regression with EPOP as the outcome (e.g., [Meer and West 2016](#)).

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, since the wage effects (and below and above shares separately) are less well identified in absence of control location without a minimum wage change.⁹

Finally, it is worth discussing the effect of measurement error on our estimates. If 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 effect of minimum wages on reported wages are below \bar{W} . The reason is straightforward. Assume that due to measurement error, 1% of the workforce mistakenly report earning below the new minimum wage in the post-treatment period. This leads our estimate of the missing jobs to be too small in magnitude:

workers with the pre-treatment wages below the new minimum experiences an increase of \$1 and the remaining 20% an increase of \$2, then the changes in share for bins, where $k = -1$, $k = -0$ and $k = 1$, are $-\bar{b}_{-1}$, $0.8 \times \bar{b}_{-1}$ and $0.2 \times \bar{b}_{-1}$, respectively. Similarly, the distances of the wage bins to the \bar{W}_{-1}^B are 0, 1 and 2. Therefore, the increase in average hourly wage is \$1.2. The denominator divides by \bar{W}_{-1}^B to convert it into percentage change in hourly wages.

⁹In 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 that take 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.

$\hat{\Delta}b = \Delta b + 0.01$. But by definition, these workers show up as it also leads to an equal reduction in the number of excess jobs above, leading to $\hat{\Delta}a = \Delta a - 0.01$; 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 sum of $\hat{\Delta}a + \hat{\Delta}b$ is likely unaffected by measurement error in reported wages. This is an important advantage of our approach, especially given the likely measurement error in the CPS wage data.

3 Data and sample construction

To implement the bunching estimator described above, we calculate quarterly, state-level employment counts by hourly wage bins using the individual-level NBER Merged Outgoing Rotation Group of the Current Population Survey for 1979-2016 (CPS-ORG). 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.¹⁰ There are no reliable imputation data for January 1994 through August 1995, so we exclude this entire period from our sample. Our available sample of employment counts therefore spans 1979q1 through 1993q4 and 1995q4 through 2016q4.

We deflate wages to 2016 dollars using the CPI-U-RS and for a given real hourly wage assign its earner a \$0.25 wage bin w running from \$0.00 to \$30.00.¹¹ For each of these 117 wage bins we collapse the individual-level data into quarterly, state-level employment counts E_{swt} using the person-level ORG sampling weights. To account for population changes, we calculate quarterly, state-level per-capita employment by dividing the employment counts by the weighted count N_{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 demographic subgroups, where the bite of the policy varies.

The aggregate state-quarter-level employment counts from the CPS-ORG are subject to sampling error, which reduces the precision of our estimates. To account for this problem, we benchmark the CPS-ORG aggregate employment-to-population ratio to the implied employment-to-population ratio from the Quarterly

¹⁰The NBER CPS-ORG are available at <http://www.nber.org/morg/>. Wage imputation status markers in the CPS-ORG vary and are not comparable across time. In general we follow Hirsch and Schumacher (2004) to define wage imputations. During 1979-1988 and September 1995-2015, we define wage imputations as records with positive BLS allocation values for hourly wages (for hourly workers) and weekly earnings or hours (for other workers). For 1989-1993, we define imputations as observations with missing or zero “unedited” earnings but positive “edited” earnings (which we also do for hours worked and hourly wages).

¹¹We 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, 1.00), [1.00, 1.25), [1.25, 1.50), . . . , [29.75, 30.00), [30, ∞).

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.¹²

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).¹³ Figure A.1 shows that during our sample period 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 timing of federal increases. Many increases are federal changes, in green, which we also exclude from our primary sample of treatments because we cannot identify the change in the pre-treatment below share Δb for these events; in states not subject to the federal minimum wage increase, there are no covered workers below the new minimum. 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 earners are directly affected. Excluding federal and small increases reduces our primary sample of minimum wage increases to 137 (blue) events, for which we calculate the distance in dollars between a given state-time-specific wage bin and the new minimum wage $k(w) \in \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$. We find below that our results are robust to changes in these event and wage window definitions.

On average, 8.6% of workers are below the new minimum wage in the year before these 137 events and the mean real minimum wage increase is 10.2%. In addition to the full sample of 137 events, we also explore the heterogeneity of results across events in two dimensions. The true effects of minimum wage policy may vary for more binding statutory wage increases, so we present results using a subset of events where the below share is greater. We also consider the effects on demographic subgroups across which the below share substantially varies.

¹²Our outcome, per-capita count, $c(w) = \frac{C(w)}{P}$, can be rewritten as the product of the (binned) wage frequency distribution, $f(w)$, and the employment to population ratio, $\frac{E}{P}$, so $c(w) = f(w) \times \frac{E}{P} = \frac{C(w)}{P}$. We can estimate this using the CPS only, where $\hat{C}_1(w) = f(w)_{CPS} \times \frac{\hat{E}}{P}_{CPS}$, and we can alternatively, estimate it using the QCEW and Census population, where $\hat{C}_2(w) = f(w)_{CPS} \times \frac{\hat{E}}{P}_{QCEW}$. Assuming both are unbiased estimates and that the errors $f(\hat{w}) - f(w)$ and $\frac{\hat{E}}{P} - \frac{E}{P}$ are uncorrelated, as when the source of the error is sampling error in the CPS, it is straightforward to show that $MSPE(\hat{C}_2) < MSPE(\hat{C}_1)$ if $var\left(\frac{\hat{E}}{P}_{CPS}\right) > var\left(\frac{\hat{E}}{P}_{QCEW}\right)$.

¹³The minimum wage series is available at <https://github.com/benzipperer/historicalminwage/releases>.

4 Empirical findings

4.1 Main results

We begin by establishing that the events under study clearly affect the number of workers earning below versus above the new minimum wage using our baseline specification, with wage-bin-by-state, and wage-bin-by-period fixed effects.

Figure 2 shows visually the change in the missing jobs (per capita) paying up to \$4 below the new minimum wage (Δb_τ), and the excess jobs (per capita) 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 $\tau = -1$, or the year just prior to treatment, the estimates for which are normalized to zero. There are four important findings that we would like to highlight. First, we find a very clear reduction in the below share (shown in red) between the four quarters just prior to treatment ($\tau = -1$) and the four quarters right afterwards ($\tau = 0$)—this shows that the minimum wage increases under study are measurably binding. Second, while there is some reduction in the below share in the post-treatment window, it continues to be very substantial and statistically significant five years out, showing that the treatments are fairly durable, at least over the medium run. Third, the response of the excess number of 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 the above share, a substantial portion of it persists and is statistically significant even five years out. Fourth, for both the changes in the excess jobs above, Δa , and the missing jobs below, Δb , 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 the 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 two shares 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.

While the previous exhibit shows the time pattern of the excess and missing jobs, Figure 3 breaks down the effects by \$1 wage bins between \$4 below to \$4 above the new minimum wage, while averaging across the five year post-treatment period. In other words, these estimates report the change in the number of workers in the \$1 wage bins between the year just prior to treatment ($\tau = -1$) and the five subsequent years ($\tau \in [0, 4]$). Much of the reduction (more than $\frac{3}{4}$) in the below share occurs in the \$1 wage bin just under the new minimum. The excess jobs are also disproportionately in the \$1 bin at or just above the new minimum wage; there is also a statistically significant increase in employment in the wage bin \$3 above the

new minimum, but modest increases in the \$1, \$2 and \$4 bins are statistically indistinguishable from zero. Overall, the pattern of employment changes is consistent with some, albeit limited wage spillovers resulting from the minimum wage increase, as suggested in [Autor et al. \(2016\)](#) and [Dube et al. \(2015\)](#).

Table 1 reports the five-year-averaged post-treatment estimates for alternative choices of \bar{W} . Column 3 employs the same specifications as Figures 2 and 3, hence $\bar{W} = 4$; whereas in columns 1 and 2, we consider narrower and in columns 4 and 5 broader wage windows. By construction, the change in the number of jobs paying below the new minimum is the same across alternative wage windows ($\Delta b = -0.018$). In contrast, the change in the number of jobs paying at or above the new minimum is slightly smaller in the first column ($\Delta a = 0.018$) than in the third column ($\Delta a = 0.021$), suggesting that spillover effects of the minimum wage event reaches to higher wage bins than $\bar{W} = 2$. As we move across columns to allow for larger wage spillovers, the excess number of jobs above the minimum increases and stabilizes around $\Delta a = 0.021$, when $\bar{W} = 4$, which motivates the use of this cutoff for our baseline specification. In the baseline specification (column 3), we find a clear increase in the wages of the affected workers ($\% \Delta W = 0.070$) in response to the minimum wage increase. In contrast, there is little indication of any employment effect: there is a statistically insignificant proportionate change ($\% \Delta E = 0.030$) in the employment of workers affected by the policy, calculated as $\frac{\Delta a + \Delta b}{b}$.

We also calculate two employment elasticities which are useful for comparison with the existing literature. For the baseline specification (column 3), the elasticity of total employment with respect to the minimum wage is 0.026, and not statistically significant. This suggests the total employment elasticity with respect to the minimum is no more negative than -0.025 at a 95 percent level of confidence, ruling out the elasticity of -0.074 calculated by [Meer and West \(2016\)](#) (see the baseline estimate in their Table 4). Second, we report the elasticity of employment with respect to own wage for affected workers. In a competitive labor market, this measures the labor demand elasticity. Our estimate for the elasticity of employment with respect to own wage is 0.437. While the standard error of 0.426 makes this somewhat imprecise, it nonetheless rules out any own-wage elasticities more negative than -0.398 at the 95 percent confidence level. Once we consider wage windows with $\bar{W} > 2$, we find very similar employment elasticities with respect to the minimum wage and own wage (ranges between 0.022 and 0.030 for the former, and between 0.380 and 0.479, for the latter).

As a check on our research design, we additionally examine employment responses higher in the upper tail. Figure 4 sets $\bar{W} = 17$ and shows that we do not spuriously estimate employment effects in the upper tail, which are all close to zero and statistically insignificant for all wage bins greater than \$3 above the new minimum through $\bar{W} = 17$. In addition to passing this falsification test, these results also show that there is little correlation between the employment responses near the minimum wage and well above it, as the pattern and size of the employment responses near the new minimum wage is similar in both Figures 3 and 4. The

time path of the wage and total employment change of affected workers shows a unambiguous wage increase with little employment response. 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, Figure 6 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.

In Table 2, we present results from estimating the specifications with additional controls for time-varying, unobserved heterogeneity. Column 1 reproduces our baseline results in column 3 of Table 1. Columns 2 through 6 in Table 2 report estimates from a wide variety of specifications with alternative assumptions about regional heterogeneity and trends that have been found to be important in the minimum wage literature (see Allegretto et al. (2017)). Columns 2 and 3 add wage-bin-by-state specific linear and quadratic time trends, respectively. Note that in 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. Therefore, columns 6 and 7 are the most saturated specifications: whereas column 6 uses geographically proximate areas and time trends to construct finer grained controls, column 7 uses within-state higher wage groups to do the same.

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 when considering only variation within Census divisions: $\Delta b = -0.15$ when the wage-bin-period effects vary by Census division (column 4) while $\Delta b = -0.18$ when they do not (column 1). 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 0.056 and 0.072 with common wage-bin-period effects, and between 0.043 and 0.050 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 -0.014 and 0.043 across the 7 specifications. The employment elasticity with respect to the minimum wage ranges between -0.012 and 0.036, while the employment elasticity with respect to the wage ranges between -0.323 and 0.595.

For most part, the point estimates are small or positive; the only exception is column (5) with state-specific linear trends and bin-division-specific period effects which is a little more negative, with an employment elasticity with respect to wage of -0.323. However, adding quadratic trends to this specification (column

6) reduces the magnitude of the employment elasticity with respect to the wage to 0.153. Our other most saturated specification, while less precise than the baseline specification, also indicates no evidence of job loss: the triple-difference specification (column 7) controlling for state-period FE obtains an elasticity with respect to the wage of 0.595.

Summarizing to this point, Tables 1 and 2 show that for our primary sample of 137 state minimum wage increases, there is little indication of change 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. Additionally, 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. At the same time, the estimates from the baseline specification are often more precise (especially for the employment elasticity with respect to the wage). In conjunction with the lack of a clear pre-existing trends in the baseline models (as shown in figures 3, 5 and 6), these considerations lead us to focus on the baseline specification in the sections below, where we consider both heterogeneity of treatment effects by groups, types of events, and assess additional robustness tests.

4.2 Heterogeneity of effect by size and jurisdiction of minimum wage increases

One concern with minimum wage studies in the United States is that many increases are small, affecting only a small number of workers which might make it difficult to detect employment effects. Another potential connection between size of the increase and long run effects. If the long run effects involve exit and entry, then the pace of such adjustment is likely to be faster when the increase is bigger.

While these concerns are potentially valid, we begin by re-iterating that in our primary sample of events, we find that even in the fifth year following a minimum wage increase, there is a clear “bite” from the policy as summarized by the Δb_t over time. Similarly, Figure 5 shows that the average wage of affected workers remains substantially higher five years out in treated states as compared to control ones. And yet, the employment in the fifth year out (while imprecise) does not indicate job losses (Figure 6).

Nonetheless, we additionally assess whether our estimates are different when we specifically focus on the events which entail larger minimum wage increases. While there are different ways of measuring the size of the increase, a natural way of doing so in our approach is by considering the share of workers between the old and the new minimum wage, which we call the “directly affected share.” We consider the set of 46 events in the upper tercile, where the directly affected share is at least 0.033, and 68 events in the upper half, where the directly affected share is at least 0.029. We control for the non upper-tercile (or upper half) events in the same way we control for small or federal events in the baseline case. Column 1 and 2 of Table 3, show that although the real increases in the minimum wage were also slightly larger for these subsets of the baseline

sample (ranging between 0.112 and 0.115 instead of 0.102), the larger affected share was driven more by the distribution of pre-treatment wages.

As expected, these events saw a larger reduction in the jobs paying below the new minimum—with Δb ranging between between -0.024 and -0.027 instead of -0.018 in the baseline specification. At the same time, the employment elasticity with respect to the affected wage is still indistinguishable from zero: 0.017 for the upper tercile events and 0.112 for the upper half events, as opposed to 0.437 for the primary sample. Even though the sample sizes for these groups are 1/3 to 1/2 of the primary sample size, owing to the larger bite in the policy standard errors for elasticity with respect to the wage are smaller: between 0.232 and 0.307 instead of 0.426 in Table 2, column 1. As a consequence, even though the point estimates are not as positive when using minimum wage increases with more bite, we can still rule out labor demand elasticities more negative than -0.585 or -0.343 at the 95% confidence level for the upper tercile and upper half events, respectively.

In column 4 of Table 3, we focus on the effect for the events that take place in the 7 states where the same minimum wage is applied to tipped and non-tipped employees.¹⁴ Minimum wage laws are more binding in these states than others because a sizable portion of low-wage workers are employed as tipped employees, and may not be directly affected by the minimum wage changes. Although the average percentage increase of the minimum wage or the share of workforce earning below the new minimum wage are similar to those of primary sample of events (0.099 instead of 0.102, respectively) the bite of the policy is larger, $\Delta b = -0.025$, as we would expect from lack of tip credits. However, the larger number of missing jobs is almost exactly compensated by a larger excess number of jobs above, with $\Delta a = 0.026$. The resulting employment elasticity with respect to affected wage is 0.095.

Our estimates so far have used primary sample of 137 state minimum wage increases that exclude both very small changes and federal increases. In the last column of Table 3, we expand the sample to (non-trivial) federal minimum wage increases, a total of 368 events. Here we find the average bite (Δb) to be slightly larger at -0.020. The wage effect for affected workers is 0.088 and statistically significant. The employment elasticities with respect to the minimum and the wage are both close to zero at -0.010 and -0.131, 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 below $(MW + \bar{W})$. Overall, we find it reassuring that the key finding of a small employment elasticity obtains even when we consider federal increases.

¹⁴These states are Alaska, California, Minnesota, Montana, Nevada, Oregon and Washington.

4.3 Heterogeneity of effects for different demographic groups

If a minimum wage increase leads firms to hire more higher skilled workers, the overall employment effect may understate the disemployment of lower skilled workers. Besides estimating the total employment effect for the workforce as a whole, our approach can also provide employment estimates for specific subgroups. In much of the literature, specific groups like teens have been studied because the policy is much more binding for them than the workforce as a whole and it is therefore easier to detect a clear effect on the average wage. In contrast, with the bunching approach we can study specific groups because they may be of interest to policymakers, and assess the total employment effect for these group regardless of whether the policy is more or less binding for that group. In this section, we provide estimates for a variety of groups by age, educational credentials, and gender. Specifically, we consider high school dropouts, those with high school or less schooling, women, black or Hispanic individuals, and teens.

Table 4 reports the estimates for these subgroups using our baseline specification. First, as expected, restricting the sample by education and age produces a larger bite. For example, for high school dropouts, the per-capita number of jobs below the new minimum, Δb , changes by -0.064, and for those with high school or less schooling the change is -0.031. These estimates for the missing jobs below the minimum are, respectively, 256% and 72% larger than the baseline estimate for the overall population (-0.018, from column 1 in Table 2). Restricting by age, gender, and race or ethnicity also exhibits a larger bite than our estimates for the overall population. Teen (-0.113), female (-0.023), and black or Hispanic (-0.027) workers all see significant and relatively larger changes in the per-capita number of jobs below the new minimum.

The employment elasticities with respect to the wage range between -0.099 and 0.494 for the five groups. In all cases but one, the elasticities are statistically indistinguishable from zero. The sole exception is high school dropouts, for whom the employment elasticity with respect to the wage is 0.494 and is marginally significant at the ten percent level. The minimum wage elasticity for teens is 0.134, which is somewhat more positive than 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).

As an alternative strategy for assessing whether there is heterogeneity of employment effects by skill groups—as would be the case with labor-labor substitution—we fully partition 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.¹⁵ For each of these 23 groups, and we separately estimate a regression using our baseline specification,

¹⁵Education 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.

and calculate changes in above and below shares ($\Delta a, \Delta b$) for each of them. A scatter plot (and binned scatter plot with 8 bins) of these shares are reported in Figure 7. Note that if there is no disemployment effect across any 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 ($-\Delta b$) is expected to be bigger—then we should expect the slope to be less than one, especially for larger values of $-\Delta b$. As shown in in Figure 7, the slope of the fitted line is very close to one, with $\hat{\mu}_1 = 0.99$. Indeed, the red solid fitted line is virtually indistinguishable from the dashed 45 degree line. 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 systematic evidence of heterogeneity of employment effect by skill level; the lack of an overall disemployment effect does not appear to be driven by labor-labor substitution.

4.4 Heterogeneity of effects by occupational tasks

Another potential source of heterogeneity in the effect of minimum wage arises from the differential susceptibility of occupations to technological substitution. A higher minimum wage increases the cost of low wage labor, and may decrease employment in occupations where capital can easily substitute with labor. In the face of rising labor costs, firms may switch to less labor-intensive technology and automatize some of the routine, codifiable tasks that were previously done by workers. The bunching approach enables us to assess such substitution effect by separately estimating the employment effect of minimum wage by occupation groups with different task contents.

In defining occupations that are susceptible to the technological substitution, we use the exact definition of routine task intensity (RTI) as proposed in Autor and Dorn (2013). They use job task information from *Dictionary of Occupational Titles* (DOT) to create a summary measure of RTI, by subtracting manual and cognitive task inputs from routine task input measures. Following Autor and Dorn (2013), we consider the top third of occupations of this measure to to “routine” occupations which are liable to be affected by technological substitution.

In addition, it may be useful to further divide these occupations into “routine manual” and “routine cognitive” categories. Here we take two approaches. First, following Acemoglu and Autor (2011), we construct six composite indices for each occupation: two of them measure routine manual (RMTI) and

routine cognitive intensities (RCTI) of the tasks¹⁶. Similar to the “routine” definition, we consider the top third of occupations in these two indices (RMTI and RCTI) to be “routine manual” and “routine cognitive” occupations, respectively.¹⁷

In their paper evaluating minimum wage effects by occupation, [Aaronson and Phelan \(2017\)](#) use a slightly different approach, and calculate routine cognitive and routine manual shares for each occupation. For comparability, we also construct routine cognitive and routine manual share intensities using this method. Following [Aaronson and Phelan \(2017\)](#), to transform [Acemoglu and Autor \(2011\)](#)’s routine cognitive and routine manual indices into shares, first, we subtract the minimum value of each index across all occupations from the original value. Then, the adjusted routine cognitive and routine manual indices are divided by the sum of all six adjusted indices. Finally, we pick the top third of the routine cognitive and manual task share intensive occupations as those vulnerable to the technological substitution.

The first column of [Table 5](#) reports the employment estimates for routine task intensive occupations. Compared to our baseline results, focusing on these routine occupations produces larger bite ($\Delta b = -0.028$) and a slightly greater wage effect for the affected employment ($\% \Delta W = 0.092$). The employment elasticity with respect to the minimum wage and with respect to the affected wage are, respectively, -0.002 and -0.019, less positive than the results from the overall sample but still statistically insignificant and close to zero. In other words, we do not see evidence of disemployment effects in routine-intensive occupations, similar to the overall sample.

Columns 2 and 3 reports the effects separately for routine cognitive and routine manual task intensive occupations. The average bite for the former is smaller ($\Delta b = -0.020$) than the latter ($\Delta b = -0.044$), indicating that wages in routine cognitive task intensive jobs tend to be higher with fewer jobs earning close to the minimum, relative to manual routine task occupations. The employment elasticity with respect to the wage for routine cognitive occupations is 0.267 but not precisely estimated enough to rule out 0 at conventional confidence levels. For routine manual occupations—where there is a bigger bite of the policy—the estimate is essentially zero ($\eta = -0.000$). Overall, there is little indication job loss in either sub-category of the routine task intensive occupations.

These results are qualitatively similar when we use the share based definition as proposed by [Aaronson and Phelan \(2017\)](#), and are reported in the last two columns of [Table 5](#). Using shares instead of untransformed index values marginally increases the average bite for both sub-groups: ($\Delta b = -0.025$ for routine cognitive, and $\Delta b = -0.049$ for routine manual. However, in both cases the employment elasticities are both positive

¹⁶Other four indices are as follows: cognitive analytical, cognitive interpersonal, manual interpersonal, and non-routine manual physical. These indices are based on Work Activities and Work Context Importance scales from O*NET data.

¹⁷Due to the change in occupation classification in 2011, the sample spans from 1983q4 to 2010q4. For the previous changes in occupation classification, we harmonize the occupation codes using crosswalks prepared by [Acemoglu and Autor \(2011\)](#).

and statistically indistinguishable from zero.

4.5 Additional robustness checks: workforce definition and shocks to low wage employment

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 the first column of Table 6, we calculate full-time equivalent employment in each wage bin. These estimates are not very different from Table 2. The fall in the below share is smaller in magnitude ($\Delta b = -0.013$, instead of -0.018), indicating lower-wage workers tend to have fewer weekly hours. The average wage change for affected workers accounting for hours is 0.076, while the employment change is 0.046. After accounting for hours, the employment elasticity with respect to the minimum wage and the own wage are 0.030 and 0.603, respectively. The analogous estimates for headcount employment in Table 2 were 0.026 and 0.437. In other words, the employment elasticities with respect to the minimum wage and own wage are not driven by changes in hours of work.

In column 2, we restrict the sample to hourly workers whom we anticipate report their hourly wage information more accurately than our calculation of hourly wages (as weekly earnings divided by usual hours) for workers that are not paid by the hour. Although the average bite is considerably larger ($\Delta b = -0.033$) and wage effect is more pronounced ($\% \Delta W = 0.096$) for this subset of workers than the overall sample, employment elasticities with respect to the minimum wage and own wage are not substantially different ($\epsilon = 0.032$ and $\eta = 0.333$).

In column 3, we drop from the sample workers in tipped occupations, as defined by Autor et al. (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 income above the minimum, are not captured in the hourly wage reported by hourly workers earned by tipped workers) Imperfect coverage does not cause a bias in the bunching estimate for the change in employment ($\Delta a + \Delta b$), though it does complicate the interpretation of the "affected employment." Excluding them diminishes the discrepancy between income and the hourly wage. However, note that many tipped workers are at the lower tail of the wage distribution. Excluding these workers yields a slightly smaller average bite ($\Delta b = -0.016$) and below share ($\bar{b}_{-1} = 0.061$), yet the wage effect (0.085) and employment elasticities ($\epsilon = 0.031$ and $\eta = 0.385$) are still similar to the baseline estimates.

Finally, we note that a potential drawback of the "triple-difference" specification of Table 2 is that within-state comparison group comprises of all other workers, including workers earning much higher than

the minimum wage. In columns 1 and 2 of Table 7, we exclude bins greater than \$15 or \$20, respectively. Limiting the sample to lower wage bins creates potentially more similar within-state comparison groups in these specifications. The estimated employment and wage effects, however, suggest that the triple difference specification of the bunching estimator is mostly unaffected by the composition of the internal control groups.

5 Comparing the bunching approach with using aggregate employment as the outcome

Estimating the effect of the minimum wage throughout that wage distribution can be very useful for illustrating the role of confounders affecting employment in the middle and upper part of the distribution, which is unlikely affected by minimum wage policy. As an illustration of the benefits of our bunching approach, in Figure 8 we provide a decomposition of the classic two-way fixed effects estimate of log minimum wage on state EPOP.

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 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)). The histogram bars show the sum of the contemporary and lagged minimum wage coefficients, divided by the sample average EPOP—which represents the “long run” elasticity of employment in each wage bin with respect to the minimum wage—along with confidence intervals where standard errors are clustered by state. The dashed purple line shows the running sum of the minimum wage effects up until that wage bin, with the purple bar showing the two-way fixed effects estimate of log minimum wage on EPOP. Since the per-capita employment in the wage bins add up to the total EPOP, the overall semi-elasticity of EPOP with respect to minimum wage (the purple bar) is exactly decomposed into its effects on employment in each of the wage bins.

Figure 8 shows that in the U.S. context, on average, minimum wage shocks are associated with a very big impact in the real dollar bins in the \$6-\$9/hr range. There is a sharp decrease in employment in the \$6/hr and \$7/hr bins, likely representing a reduction in jobs paying below new minimum wages; and a sharp rise in the number of jobs in the \$8/hr and \$9/hr 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 overall negative employment elasticity (-0.137) accrues almost entirely in wage bins exceeding \$15/hour. It strikes us as implausible that minimum wages in

the \$7-\$9 range causally lead to job losses mostly the earnings distribution at or above the median wage, when it is binding far below. More plausibly, this shows that minimum wage changes were confounded by shocks to the employment in the upper part of the wage distribution. This exercise highlights the perils of using aggregate employment as an outcome for estimating minimum wage effects, and motivates the bunching approach to estimating total employment effect. The figure also illustrates the need for us to use more precise location of where along the wage distribution the minimum wage is binding, which is why in this paper we use an event-based approach instead of using log of minimum wage as the treatment variable.

Table 8 reports the minimum wage elasticities for employment using (1) log minimum wage as treatment as well as (2) event based approach using state minimum wage increases. Columns 1 and 2 we show long run (3 year) elasticities based on two-way fixed effects regressions of state EPOP on contemporaneous and 3 annual lags of log minimum wages; column 1 shows the elasticity when the model is estimated in levels (same as the rightmost bar in Figure 8), while column 2 shows the elasticity when the model is estimated in first differences. Column 3 reports estimates using an event based approach using our 137 state events, but where we regress state EPOP on quarterly leads and lags on treatment spanning 3 years before to 5 years after the policy change. Finally, column 4 shows estimates from our bunching approach - i.e., the estimate from Table 2, column 1. In all cases we show estimates with and without population weighting.

When using log minimum wage as treatment (with up to three years of lags) and aggregate EPOP as the outcome produces estimates large, negative minimum wage elasticities of -0.137 (weighted by population) and -0.164 (unweighted). However, as we saw in Figure 8, all of the negative effects in the weighted regression accrued for wages higher than \$15/hour. Also, we find the estimates are quite small when we estimate the model in first differences, with estimates ranging between 0.011 and -0.033 depending on weights. When we consider event based estimates for the same set of state minimum wage changes we have focused on in this paper, we find estimates vary substantially depending on specification. For example, unweighted estimates are generally more negative: the baseline estimated elasticity is -0.114, while the weighted estimate is 0.010.

In contrast to the wide range of results when using aggregate employment as the outcome, our bunching estimates in column 4 are both more precise (smaller standard errors), do not differ visibly by use of weights, and are close to zero. In conjunction with the decomposition results which show how the This provides additional validation that when we focus our attention on employment in the part of wage distribution likely affected by minimum wages, we are able to obtain more reliable estimates.

6 Discussion

We propose and implement a novel approach to estimate the employment effects of the minimum wage that relies on the wage distribution. We estimate the number of missing jobs just below the minimum wage, and compare this with the excess number of jobs at or slightly above the minimum to infer the effect on employment. Our approach has several advantages. First, by focusing on employment changes at the bottom of the wage distribution we can estimate the overall employment effects of the minimum wage—even in the U.S. context where only small fraction of workers affected by the minimum wage. Second, we can jointly estimate the effect on employment and wages, including assessing the extent of wage spillovers. Third, our approach is much less subject to contamination from shocks affecting employment higher up in the wage distribution—which can bias estimates that use aggregate employment as the outcome. At the same time, we show the effect of the minimum wage throughout the earnings distribution—making our method transparent regarding the role of upper tail in influencing the estimates. Fourth, the bunching method allows us to evaluate the effect of the minimum wage for various demographic group, even for groups where minimum wage workers constitute a relatively small share, like prime-aged adults. Relatedly, our approach also allows us to estimate employment effect across different occupations, such as those that are more routine task intensive. The ability to estimate heterogeneous effects by skill and occupational tasks allows us to provide a comprehensive assessment of both labor-labor substitution, and the substitution of labor with capital.

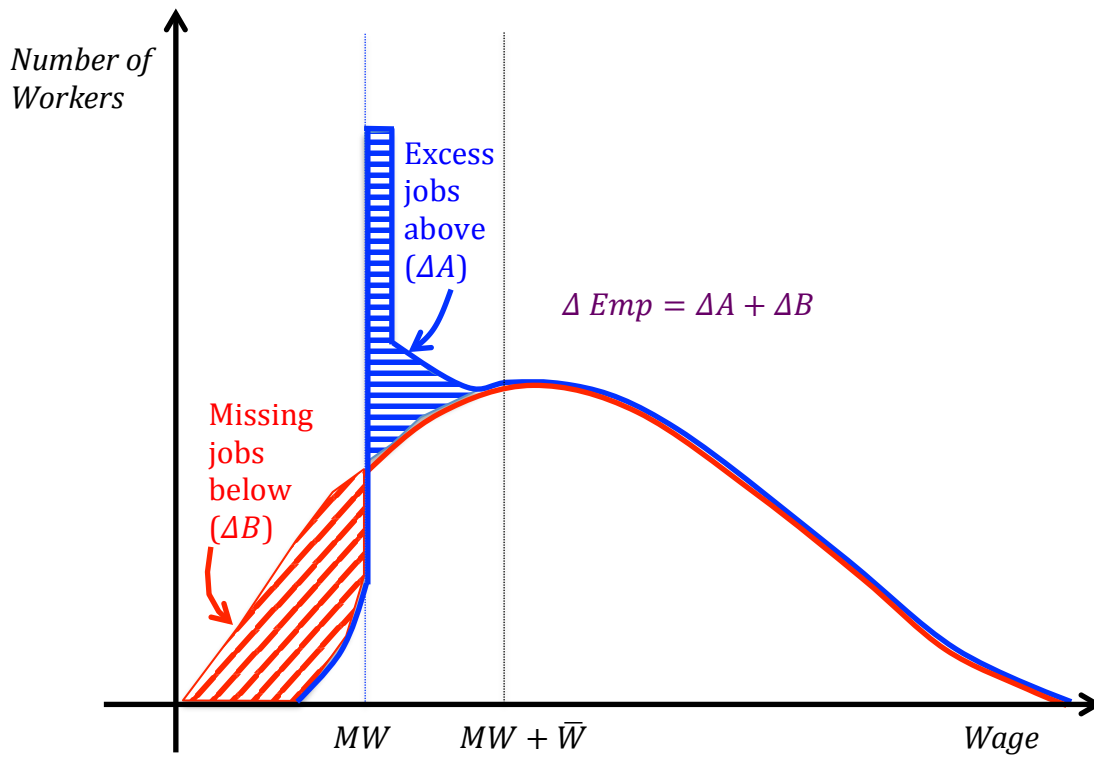
The bunching method provides several new insights about the employment consequences of the minimum wage in the U.S. Using state level minimum wage changes, we find that the overall employment effects of the minimum wage is likely to be close to zero, and that previous findings on disemployment effects were likely driven by changes in the upper tail of the wage distribution. We also provided new estimates on the employment effects accross various demographic groups, and find no indication of substantial employment effect in groups that would indicate labor labor substitution. These findings are found to be robust to a variety of assumptions about unobserved heterogeneity in low-wage employment shocks Finally,our strategy also provides evidence on the effect of the minimum wage on the shape of the wage distribution. Similar to estimates in the literature such as [Autor et al. \(2016\)](#), we find evidence of wage spillovers above the minimum wage. Going beyond the literature, however, we can rule out that these measured spillovers are due to disemployment—another virtue of jointly estimating the effect of the policy on earnings and employment.

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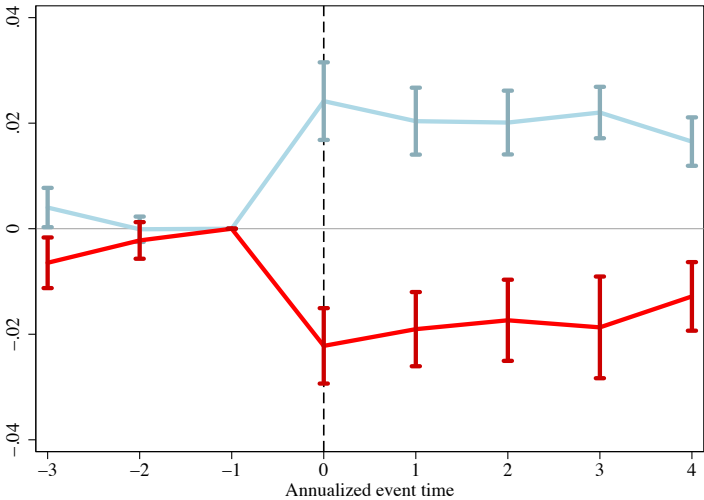
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Figure 1: An Illustration of the bunching approach: Effect of a minimum wage on the number of jobs



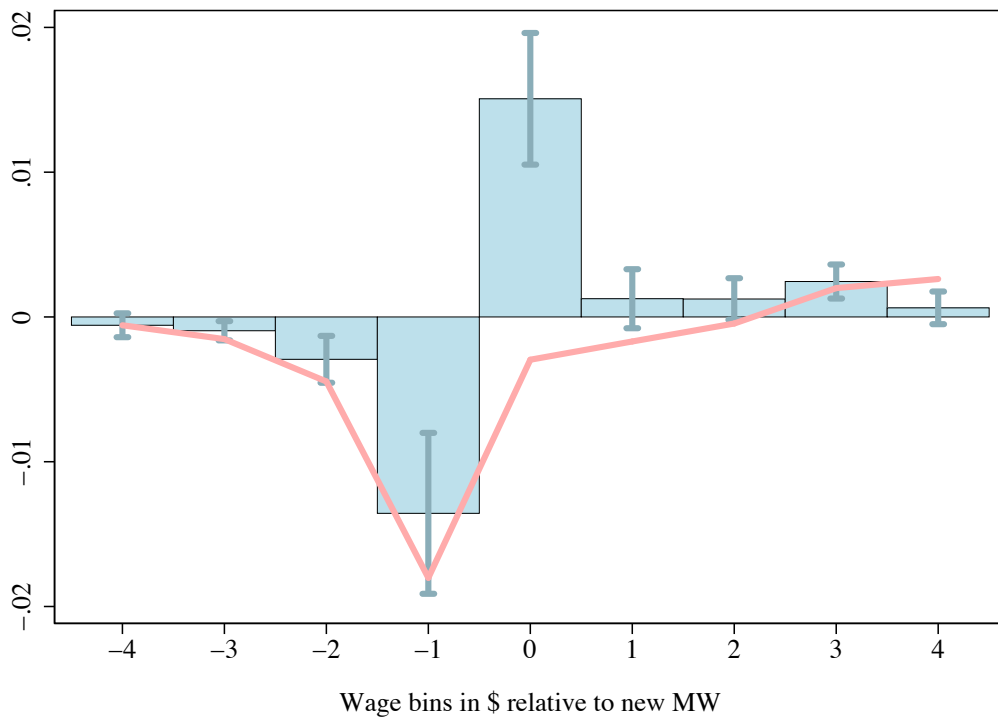
Notes: The figure shows the effect of the minimum wage on the frequency distribution of hourly wages. The red solid line shows the wage distribution before, and the blue solid line after the minimum wage event. Since compliance is less than perfect, some earners are uncovered and the post-event distribution 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 $MW + \bar{W}$, 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: Evolution of excess jobs above, and missing jobs below the new minimum wage over event time



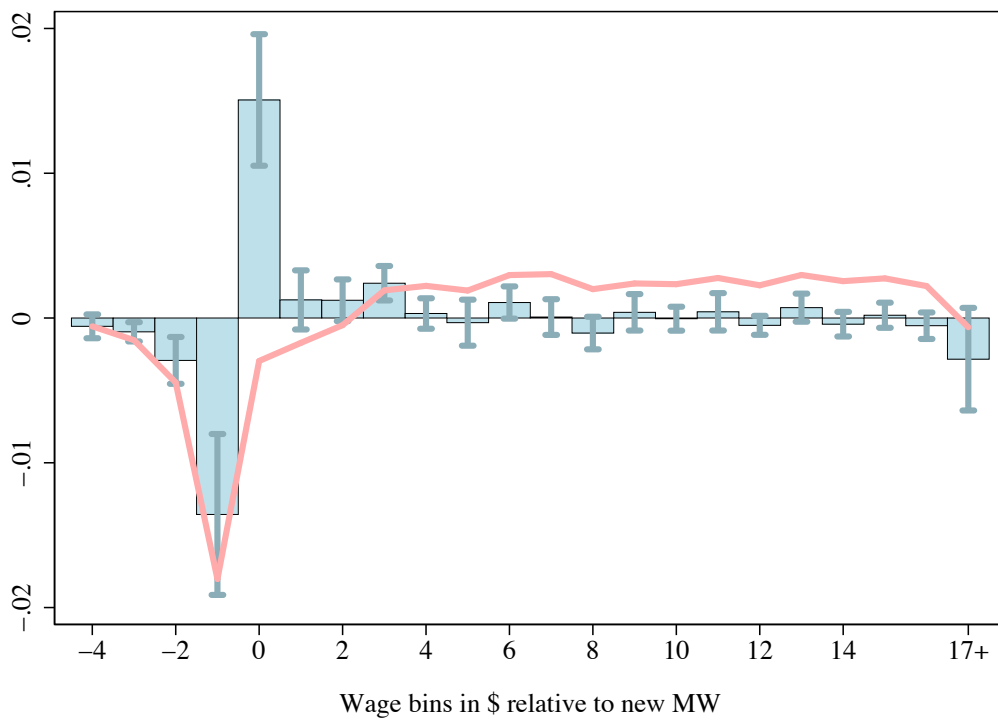
Notes: The figure shows the time path of the impact of the minimum wage event on number of jobs in affected wage bins, using the baseline specification. Blue and red lines show the per-capita excess jobs above (Δa), and missing jobs below the new minimum wage MW' (Δb), respectively. Jobs below the new minimum are those with wages in the range $[W, MW')$, and jobs above are those in the range $[MW', \bar{W})$. 95% confidence intervals are calculated by adding up the impact of the average event by wage bins estimated from the regression using the baseline specification, where outcome is per-capita employment by wage bins.

Figure 3: Change in employment by wage bins relative to the new minimum wage between pre-treatment and post-treatment periods



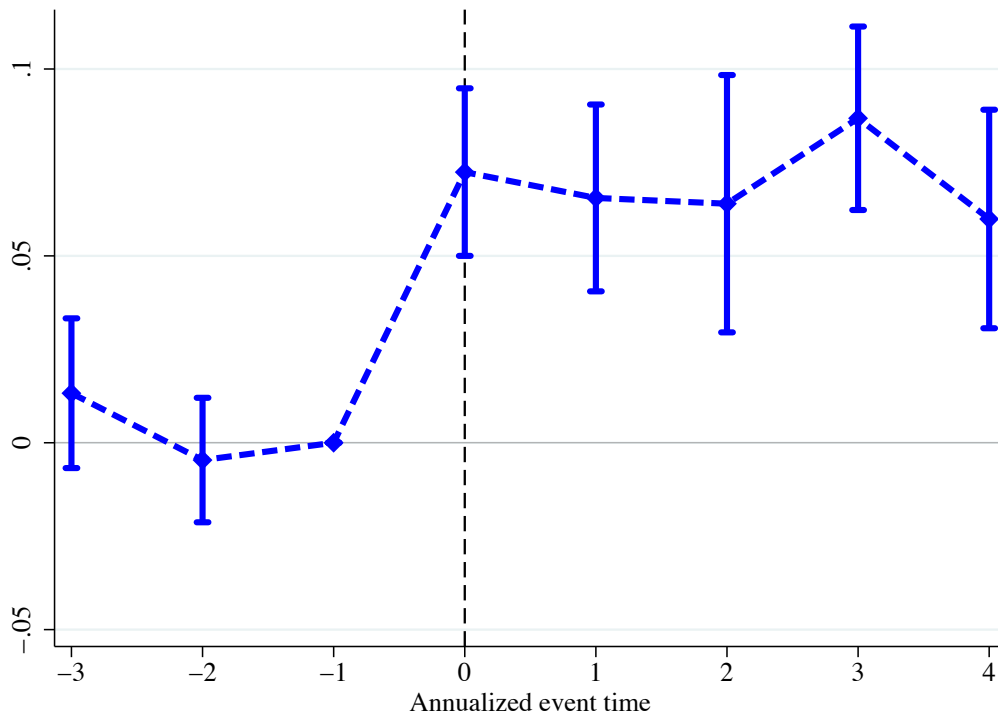
Notes: The figure shows the change in the average employment between the 5-year post-treatment and 1-year pre-treatment periods, by \$1 wage bins relative to new minimum wage. 95% confidence intervals are calculated by averaging estimates from the baseline specification across the five-year post-treatment period. The red line is the running sum of the bin-specific impacts.

Figure 4: Change in employment by wage bins in the upper part of the wage distribution



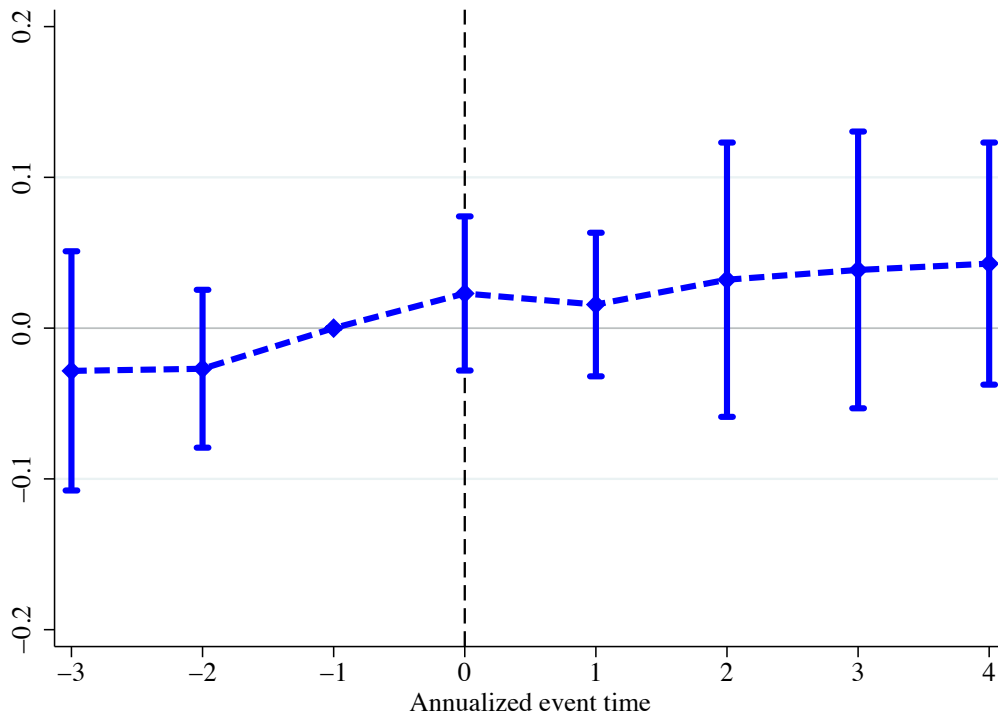
Notes: The figure shows the change in the average employment between the 5-year post-treatment and 1-year pre-treatment periods, by \$1 wage bins relative to new minimum wage. 95% confidence intervals are calculated by averaging estimates from the baseline specification across the five-year post-treatment period. The red line is the running sum of the bin-specific impacts.

Figure 5: Evolution of the average wages of affected workers over event time



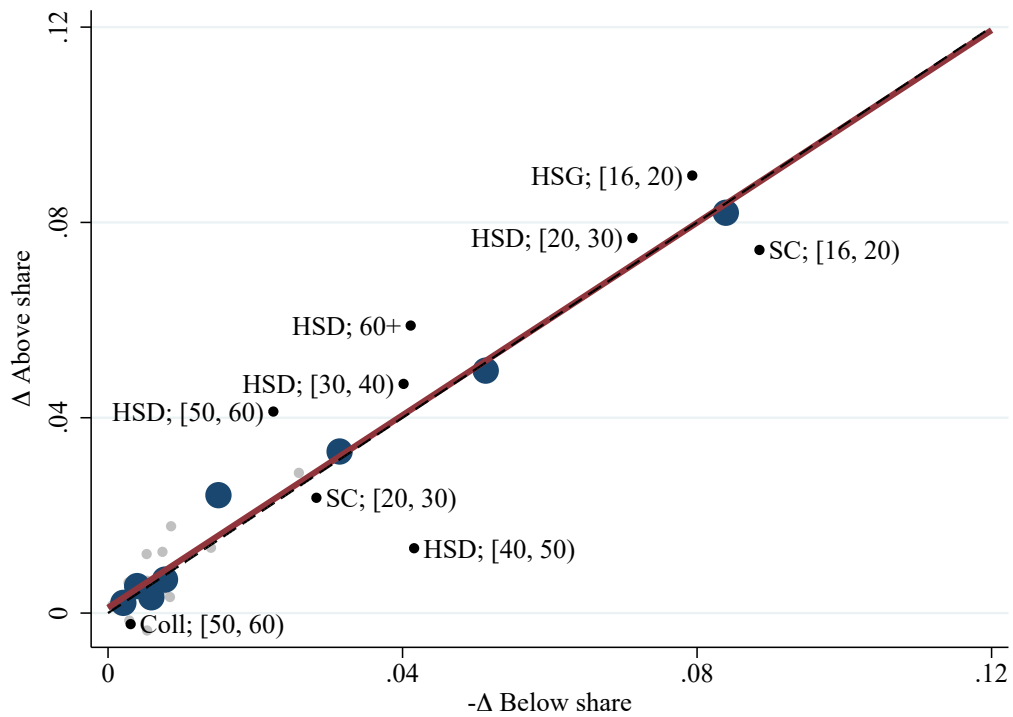
Notes: The figure shows the time path of average real wages of affected workers ($\% \Delta W$) following a primary minimum wage event, relative to the year prior to the treatment. 95% confidence intervals are calculated by estimating $\% \Delta W$ at each four quarters interval relative to the minimum wage event using baseline specification, where outcome is per-capita employment by wage bins.

Figure 6: Evolution of the employment of affected workers over event time



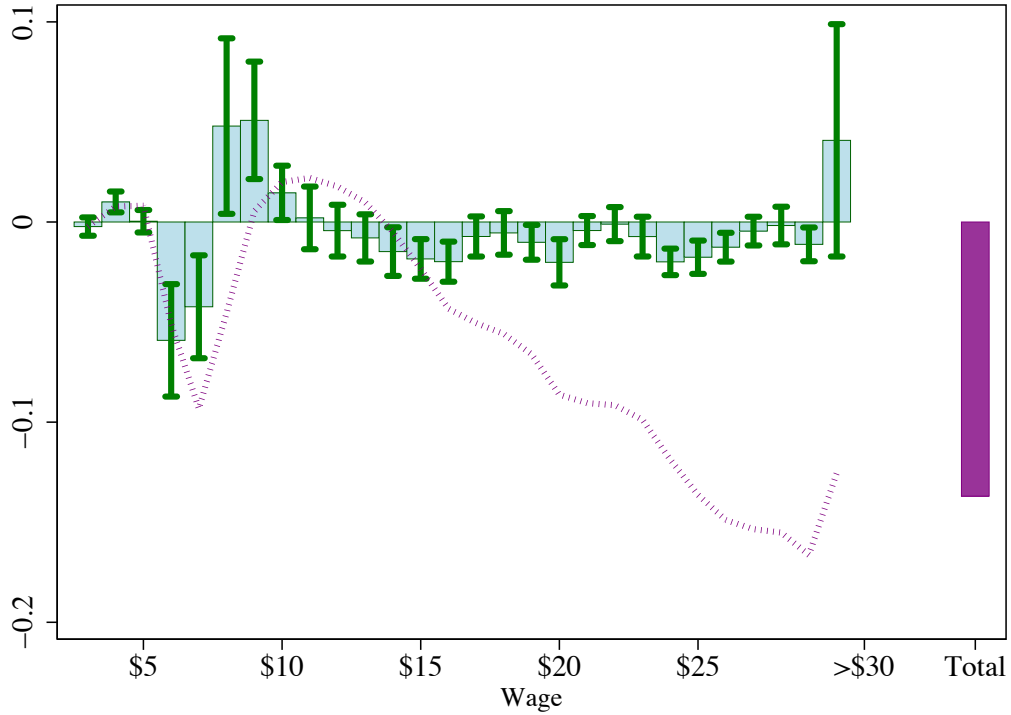
Notes: The figure shows the time path of employment of affected workers ($\% \Delta Emp$) after a primary minimum wage event, relative to the year prior to the treatment. 95% confidence intervals are calculated by estimating $\% \Delta Emp$ at each year relative to the year before the minimum wage event using the baseline specification, where outcome is per-capita employment by wage bins.

Figure 7: Raw and binned scatterplot of changes in employment above versus below the new minimum wage by age-education groups



Notes: The figure shows excess jobs above the minimum (Δa) and missing jobs below ($-\Delta b$) by 23 education-by-age groups. The red line is the fitted line and the black dashed line is the 45 degree line. The small light gray and black points report the raw changes in the above and below share estimates for each regression, while the large blue dots are the 8 binned averages.

Figure 8: Decomposing the minimum wage elasticity for EPOP by wage bins



Notes: The estimates in the figure are based on two-way (state and year) fixed effects regressions of per-capita employment in particular \$1 bins on contemporaneous as well as 3 annual lags of log minimum wage. The figure shows the (long run) cumulative response elasticities of per-capita employment in that wage bin with respect to the minimum wage that are obtained by summing up the contemporaneous and lagged minimum wage coefficients and dividing them by the sample average EPOP. The 95% confidence intervals are from 1\$ bin-specific regressions that control for wage bin-by-state and wage bin-by-period effects, where outcome variables are number of jobs per capita. The purple dotted line is the running sum of the semi-elasticities. The rightmost purple bar is the long run elasticity of the overall state EPOP with respect to minimum wage, obtained from a two way fixed effects regression of employment per capita on contemporaneous and lags of log minimum wage; the sum of minimum wage coefficients are divided by the sample average EPOP. Regressions are weighted by state population. Standard errors are clustered by state.

Table 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 MW (Δb)	-0.018*** (0.004)	-0.018*** (0.004)	-0.018*** (0.004)	-0.018*** (0.004)	-0.018*** (0.004)
Excess jobs above MW (Δa)	0.018*** (0.003)	0.021*** (0.002)	0.021*** (0.003)	0.020*** (0.003)	0.021*** (0.002)
% Δ affected wages	0.045*** (0.009)	0.066*** (0.009)	0.070*** (0.011)	0.069*** (0.015)	0.083*** (0.015)
% Δ affected employment	0.000 (0.026)	0.032 (0.032)	0.030 (0.031)	0.026 (0.033)	0.035 (0.036)
Employment elasticity w.r.t. MW	0.000 (0.022)	0.027 (0.027)	0.026 (0.026)	0.022 (0.028)	0.030 (0.030)
Emp. elasticity w.r.t. affected wage	0.007 (0.567)	0.479 (0.471)	0.437 (0.426)	0.380 (0.441)	0.421 (0.384)
Below Share (\bar{b}_{-1})	0.086	0.086	0.086	0.086	0.086
% Δ MW	0.102	0.102	0.102	0.102	0.102
# Event	137	137	137	137	137
Observations	841,347	841,347	841,347	841,347	841,347
Specification:	$\bar{W} = 2$	$\bar{W} = 3$	$\bar{W} = 4$	$\bar{W} = 5$	$\bar{W} = 6$

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-ORG data from 1979-2016. The first column limits the range of the wage window by setting the upper limit to $\bar{W} = \$2$, and the last column expands it until $\bar{W} = \$6$. The dependent variable is the per capita employment in wage bins. Specifications include wag -bin-by-state and wage bin-by period fixed effects. The first two rows report the change in per-capita number of missing jobs below the new minimum wage (Δb), and excess jobs above the minimum wage (Δa), each normalized by the sample averaged pre-treatment EPOP. 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 $\frac{1}{\% \Delta W} \frac{\Delta a + \Delta b}{\bar{b}_{-1}}$. Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table 2: Impact of minimum wage increase: robustness to controls for unobserved heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Missing jobs below MW (Δb)	-0.018*** (0.004)	-0.018*** (0.004)	-0.017*** (0.004)	-0.015*** (0.003)	-0.016*** (0.003)	-0.015*** (0.003)	-0.018*** (0.004)
Excess jobs above MW (Δa)	0.021*** (0.003)	0.018*** (0.003)	0.020*** (0.003)	0.016*** (0.002)	0.014*** (0.003)	0.015*** (0.003)	0.021*** (0.002)
% Δ affected wages	0.070*** (0.011)	0.056*** (0.011)	0.072*** (0.013)	0.050*** (0.010)	0.043*** (0.011)	0.052*** (0.012)	0.072*** (0.011)
% Δ affected employment	0.030 (0.031)	0.002 (0.024)	0.031 (0.023)	0.004 (0.022)	-0.014 (0.022)	0.008 (0.023)	0.043 (0.050)
Employment elasticity w.r.t. MW	0.026 (0.026)	0.002 (0.020)	0.026 (0.019)	0.003 (0.018)	-0.012 (0.019)	0.007 (0.020)	0.036 (0.043)
Emp. elasticity w.r.t. affected wage	0.437 (0.426)	0.037 (0.430)	0.433 (0.312)	0.070 (0.422)	-0.323 (0.570)	0.153 (0.424)	0.595 (0.650)
Below Share (\bar{b}_{-1})	0.086	0.086	0.086	0.086	0.086	0.086	0.086
% Δ MW	0.102	0.102	0.102	0.102	0.102	0.102	0.102
# Events	137	137	137	137	137	137	137
Observations	841,347	841,347	841,347	841,347	841,347	841,347	841,347
<i>Controls</i>							
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

Notes. The table reports five year averaged post-treatment estimates of employment and wages of the affected bins, using state-quarterly wage bin aggregated CPS-ORG data from 1979-2016. Directly affected share (share of workforce between old and new minimum wage in the year prior to the event) is 0.024. The dependent variable is the per capita employment in wage bins. Specifications include wage bin-by-state and wage bin-by-period fixed effects. Column 6 is the most saturated specification with respect to geographic and trend controls in the sense that it includes both bin-by-division-by period fixed effects as well as up to quadratic bin-by-state specific trends. Last column reports the results of "triple-difference" specification, where state-by-period fixed effects are also accounted for. The first two rows report the change in per-capita number of missing jobs below the new minimum wage (Δb), and excess jobs above the minimum wage (Δa), each normalized by the sample averaged pre-treatment EPOP. 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 $\frac{1}{\% \Delta W} \frac{\Delta a + \Delta b}{\bar{b}_{-1}}$. Regressions are weighted by state-quarterly aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table 3: Impact of minimum wage increase: Heterogeneity by size and jurisdiction of treatments

	(1)	(2)	(3)	(4)
Missing jobs below MW (Δb)	-0.027*** (0.003)	-0.024*** (0.003)	-0.025*** (0.003)	-0.020*** (0.003)
Excess jobs above MW (Δa)	0.027*** (0.002)	0.026*** (0.002)	0.026*** (0.002)	0.019*** (0.003)
% Δ affected wages	0.108*** (0.030)	0.126*** (0.028)	0.102*** (0.032)	0.088*** (0.023)
% Δ affected employment	0.002 (0.034)	0.014 (0.032)	0.010 (0.040)	-0.012 (0.022)
Employment elasticity w.r.t. MW	0.002 (0.031)	0.012 (0.028)	0.010 (0.043)	-0.010 (0.019)
Emp. elasticity w.r.t. affected wage	0.017 (0.307)	0.112 (0.232)	0.095 (0.366)	-0.131 (0.278)
Below Share (\bar{b}_1)	0.104	0.098	0.099	0.083
% Δ MW	0.115	0.112	0.093	0.096
# Events	46	68	36	368
Observations	841,347	841,347	841,347	841,347
<i>Events:</i>	Upper tercile	Upper half	No tip credit states	State & Federal

Notes. The table reports five year averaged post-treatment estimates of employment and wages of the affected bins for 46 and 68 of the primary events with largest shares of directly affected earners (share of workforce between old and new minimum wage in the year prior to the event), for 36 events occurring in states that do not allow tip credit, and for 368 state or federal minimum wage increases, using state-quarter-wage bin aggregated CPS-ORG data from 1979-2016. Directly affected shares of upper tercile and upper half events are 0.032 and 0.030, respectively; whereas it is 0.029 for events in no tip credit states and 0.020 when federal events are also considered. The dependent variable is the per capita employment in wage bins. Specifications include wage bin-by-state and wage bin-by period fixed effects. The first two rows report the change in per-capita number of missing jobs below the new minimum wage (Δb), and excess jobs above the minimum wage (Δa), each normalized by the sample averaged pre-treatment EPOP. 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 ($\frac{\Delta a + \Delta b}{b_{-1}}$).

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 $\frac{1}{\% \Delta W} \frac{\Delta a + \Delta b}{b_{-1}}$. Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table 4: Impact of minimum wage increase: Heterogeneity by demographic groups

	(1)	(2)	(3)	(4)	(5)
Missing jobs below MW (Δb)	-0.064*** (0.010)	-0.031*** (0.007)	-0.113*** (0.010)	-0.023*** (0.005)	-0.027*** (0.008)
Excess jobs above MW (Δa)	0.074*** (0.010)	0.037*** (0.006)	0.127*** (0.019)	0.026*** (0.004)	0.027*** (0.006)
% Δ affected wages	0.079*** (0.014)	0.074*** (0.014)	0.083*** (0.018)	0.073*** (0.011)	0.041*** (0.012)
% Δ affected employment	0.039 (0.024)	0.042 (0.030)	0.032 (0.030)	0.030 (0.030)	-0.004 (0.045)
Employment elasticity w.r.t. MW	0.099 (0.061)	0.058 (0.042)	0.134 (0.127)	0.030 (0.030)	-0.005 (0.059)
Emp. elasticity w.r.t. affected wage	0.494* (0.276)	0.568 (0.401)	0.383 (0.296)	0.408 (0.393)	-0.099 (1.121)
Below Share (\bar{b}_{-1})	0.264	0.144	0.430	0.102	0.133
% Δ MW	0.104	0.104	0.103	0.102	0.101
# Event	137	137	137	137	137
Observations	841,347	841,347	841,347	841,347	840,762
Groups:	High school dropouts	High school or less	Teen	Female	Black or hispanic

Notes. The table reports five year averaged post-treatment estimates of employment and wages of the affected bins for high school dropouts (HSD), individuals with high school diploma or less (HSL), teens, women and black or Hispanic workers, using state-quarter-wage bin aggregated CPS-ORG data from 1979-2016. Directly affected shares (shares of workforce between old and new minimum wage in the year prior to the event) of HSD, HSL, teens, women and black or Hispanic workers are 0.079, 0.042, 0.147, 0.28, and 0.038, respectively. The dependent variable is the per capita employment of the demographic group in wage bins. Specifications include wage bin-by-state and wage bin-by period fixed effects. The first two rows report the change in per-capita number of missing jobs below the new minimum wage (Δb), and excess jobs above the minimum wage (Δa), each normalized by the sample averaged pre-treatment EPOP. 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 $\frac{1}{\% \Delta W} \frac{\Delta a + \Delta b}{\bar{b}_{-1}}$. 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.

Table 5: Impact of minimum wage increase: Heterogeneity by occupational tasks

	(1)	(2)	(3)	(4)	(5)
Missing jobs below MW (Δb)	-0.028*** (0.004)	-0.020*** (0.003)	-0.044*** (0.009)	-0.025*** (0.006)	-0.049*** (0.009)
Excess jobs above MW (Δa)	0.027*** (0.004)	0.022*** (0.005)	0.044*** (0.008)	0.027*** (0.006)	0.054*** (0.009)
% Δ affected wages	0.092*** (0.019)	0.096*** (0.030)	0.112*** (0.023)	0.097*** (0.026)	0.108*** (0.021)
% Δ affected employment	-0.002 (0.049)	0.026 (0.070)	0.000 (0.044)	0.022 (0.045)	0.026 (0.037)
Employment elasticity w.r.t. MW	-0.002 (0.045)	0.017 (0.046)	0.000 (0.057)	0.020 (0.041)	0.040 (0.057)
Emp. elasticity w.r.t. affected wage	-0.019 (0.535)	0.267 (0.660)	0.000 (0.388)	0.228 (0.447)	0.239 (0.324)
Below Share (\bar{b}_{-1})	0.100	0.073	0.143	0.100	0.171
% Δ MW	0.110	0.110	0.110	0.110	0.110
# Events	93	93	93	93	93
Observations	704,106	704,106	704,106	704,106	704,106
<i>Task Intensive:</i>	Routine	Routine cognitive	Routine manual	Routine cognitive share	Routine manual share

Notes. The table reports five year averaged post-treatment estimates of employment and wages of the affected bins for routine task intensive occupations, using state-quarter-wage bin aggregated CPS-ORG data from 1979-2010. Column 1 shows estimates for all occupations categorized as routine, i.e., in the top third of the Routine Task Intensity scale. Columns 2 and 3 show estimates for occupations in the top third of Routine Cognitive Task Intensity, and Routine Manual Task Intensity indices, respectively. Columns 4 and 5 show estimates using occupations whose Routine Cognitive or or Routine Manual Task Intensity as a share of all 6 task indices are in the top third. Directly affected shares (shares of workforce between old and new minimum wage in the year prior to the event) of routine intensive, routine cognitive intensive, routine manual intensive, routine cognitive share intensive, and routine manual share intensive workers are 0.037, 0.028, 0.052, 0.032, and 0.060, respectively. The dependent variable is the per capita employment in wage bins. Specifications include wage bin-by-state and wage bin-by period fixed effects. The first two rows report the change in per-capita number of missing jobs below the new minimum wage (Δb), and excess jobs above the minimum wage (Δa), each normalized by the sample averaged pre-treatment EPOP. 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 $\frac{1}{\% \Delta W} \frac{\Delta a + \Delta b}{\bar{b}_{-1}}$. Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table 6: Impact of minimum wage increase: Robustness to workforce definitions

	(1)	(2)	(3)
Missing jobs below MW (Δb)	-0.013*** (0.003)	-0.033*** (0.008)	-0.016*** (0.004)
Excess jobs above MW (Δa)	0.016*** (0.002)	0.036*** (0.007)	0.017*** (0.003)
% Δ affected wages	0.076*** (0.013)	0.096*** (0.020)	0.085*** (0.014)
% Δ affected employment	0.046 (0.035)	0.032 (0.036)	0.033 (0.042)
Employment elasticity w.r.t. MW	0.030 (0.023)	0.032 (0.037)	0.019 (0.025)
Emp. elasticity w.r.t. affected wage	0.603 (0.425)	0.333 (0.391)	0.385 (0.491)
Below Share (\bar{b}_1)	0.067	0.103	0.061
% Δ MW	0.102	0.102	0.102
# Event	137	137	137
Observations	841,347	841,347	841,347
Specification:	FTE employment	Only hourly workers	Non-tipped occupations

Notes. The table reports five year averaged post-treatment estimates of employment and wages of the affected bins by alternative workforce definitions, using state-quarter-wage bin aggregated CPS-ORG data from 1979-2016. In the first column, we calculate full-time equivalent employment by wage bins, and in the second and third columns, earners are limited to hourly workers and workers in non-tipped occupations. Tipped occupations are identified in the same way as Autor et al. (2016). Directly affected shares (shares of workforce between old and new minimum wage in the year prior to the event) of full-time equivalent workers, hourly workers and workers in non-tipped occupations are 0.018, 0.043, and 0.023, respectively. Specifications include wage bin-by-state and wage bin-by-period fixed effects. The first two rows report the change in per-capita number of missing jobs below the new minimum wage (Δb), and excess jobs above the minimum wage (Δa), each normalized by the sample averaged pre-treatment EPOP. 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 $\frac{1}{\% \Delta W} \frac{\Delta a + \Delta b}{\bar{b}_1}$. Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table 7: Impact of minimum wage increase: Triple-Difference with upper limit

	(1)	(2)
Missing jobs below MW (Δb)	-0.018*** (0.004)	-0.019*** (0.004)
Excess jobs above MW (Δa)	0.020*** (0.003)	0.020*** (0.003)
% Δ affected wages	0.069*** (0.012)	0.065*** (0.011)
% Δ affected employment	0.029 (0.038)	0.014 (0.039)
Employment elasticity w.r.t. MW	0.024 (0.032)	0.012 (0.033)
Emp. elasticity w.r.t. affected wage	0.421 (0.517)	0.212 (0.584)
Below Share (\bar{b}_{-1})	0.086	0.086
% Δ MW	0.102	0.102
# Event	137	137
Observations	409,887	553,707
<i>Added controls</i>		
State-period FE	Y	Y
Excluding wages above	\$15	\$20

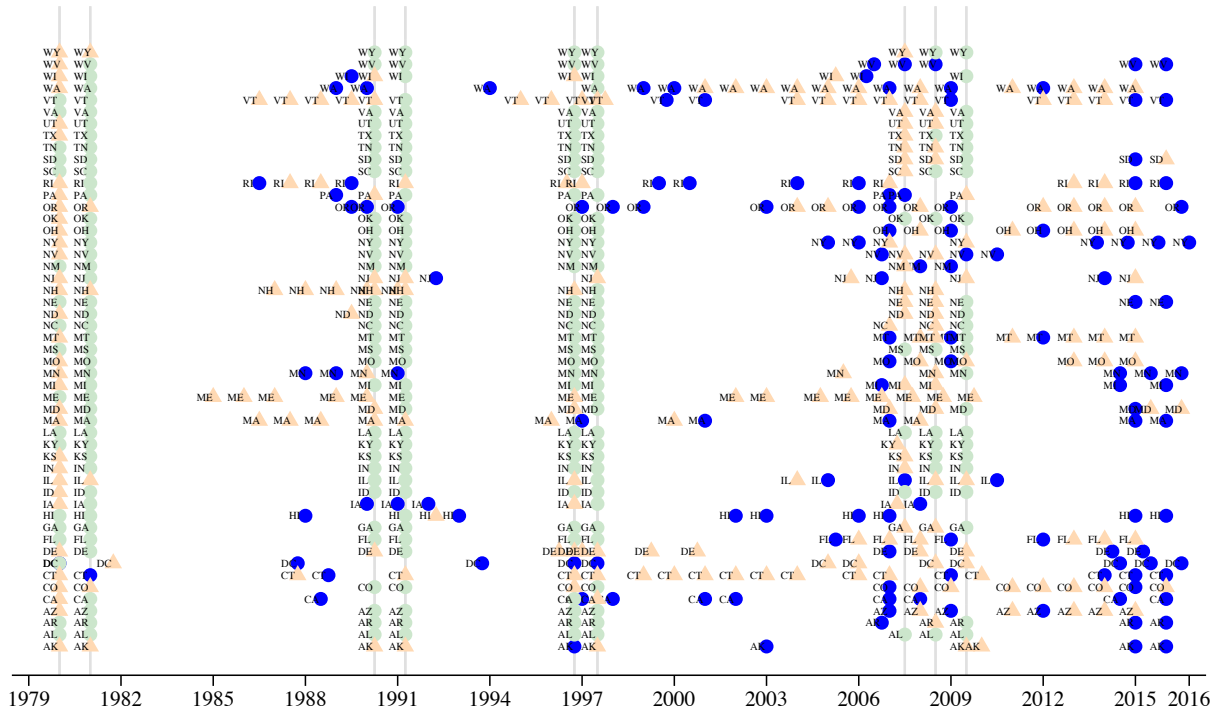
Notes. The table reports five year averaged post-treatment estimates of employment and wages of the affected bins using triple-difference specification, using state-quarter-wage bin aggregated CPS-ORG data from 1979-2016. In column (1) observations with wages greater than \$15, and in column (2) \$20, are dropped. Specifications include wage bin-by-state, wage bin-by period, and state-by-period fixed effects. The first two rows report the change in per-capita number of missing jobs below the new minimum wage (Δb), and excess jobs above the minimum wage (Δa), each normalized by the sample averaged pre-treatment EPOP. 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 $\frac{1}{\% \Delta W} \frac{\Delta a + \Delta b}{\bar{b}_{-1}}$. Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are * 0.10, ** 0.05, *** 0.01.

Table 8: Employment elasticities of minimum wage from alternative approaches

	Continuous treatment - ln(MW)		Event based	
	Levels	First Difference		
Weighted	-0.137*** (0.042)	0.011 (0.036)	0.010 (0.033)	0.026 (0.026)
Unweighted	-0.164** (0.065)	-0.033 (0.033)	-0.114 (0.085)	0.024 (0.025)
Aggregate	Y	Y	Y	
Bunching				Y

Notes. Columns 1 and 2 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. In Column 1, the model is estimated in levels, while in column 2 the model is estimated in first differences, and in both columns the data in annual. Column 3 reports estimates using quarterly data and an event based approach using 137 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. Finally, column 4 shows estimates from our bunching approach, same as in Table 2, 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.

Figure A.1: Minimum wage increases: 1979-2015



Notes: The figure shows all MW increases between 1979 and 2016. The blue circles show the primary minimum wage events; whereas the partially transparent orange triangles and green circles indicate small and federal minimum wage changes, respectively.

Online Appendix A: Estimating employment effect in a competitive model

Suppose that firms in perfectly competitive factor and product markets maximizes their profits,

$$\max_{l_j} pY - \int_{\underline{w}}^{\bar{w}} l_j w_j dj$$

where $Y = \left(\int_{\underline{w}}^{\bar{w}} a_j l_j^{\frac{\sigma-1}{\sigma}} dj \right)^{\frac{\sigma}{\sigma-1}}$ is a CES production function. Here l_j represent the employment of the workers whose wage would be w_j in the competitive equilibrium. We also assume that at this particular wage there is perfectly elastic labor supply, e.g. firms can increase or decrease labor demand without affecting wages.

The FOC from the firm's optimization problem is as follows:

$$p \frac{\partial Y}{\partial l_j} = w_j$$

Using that the production function has a CES structure implies that

$$a_j p \left(\int_{\underline{w}}^{\bar{w}} l_j^{\frac{\sigma-1}{\sigma}} dj \right)^{\frac{1}{\sigma-1}} l_j^{-\frac{1}{\sigma}} = w_j$$

which leads to the following solution:

$$l_j = Y \left(\frac{a_j p}{w_j} \right)^{\sigma}$$

In equilibrium, we also have a zero profit condition, which implies

$$pY = \int_{\underline{w}}^{\bar{w}} l_j w_j dj$$

Plugging in l_j into this equation leads to

$$p = \int_{\underline{w}}^{\bar{w}} \left(\frac{a_j p}{w_j} \right)^{\sigma} w_j dj$$

We express p from the equation above, which will be a function of wages and the productivity parameter

a_j .

$$p = \left(\int_{\underline{w}}^{\bar{w}} a_j^\sigma w_j^{1-\sigma} dj \right)^{\frac{1}{1-\sigma}}$$

Now suppose that the government introduces a binding minimum wage $MW > \underline{w}$. The effect of that change of w_j on prices is the following:

$$\frac{\partial \log p}{\partial w_j} \Delta w_j = \frac{1}{p^{1-\sigma}} a_j^\sigma w_j^{-\sigma} (MW - w_j)$$

where $\Delta w_j = (MW - w_j)$. The above expression can be simplified to

$$\frac{\partial \log p}{\partial w_j} \Delta w_j = \frac{l_j (MW - w_j)}{pY}$$

Therefore, the price change caused by the minimum wage is given by the following formula:

$$\Delta \log p = \int_{\underline{w}}^{MW} \frac{\partial \log p}{\partial w_j} \Delta w_j dj = \frac{\int_{\underline{w}}^{MW} l_j (MW - w_j) dj}{\int_{\underline{w}}^{\bar{w}} l_j w_j dj}$$

The formula above highlights that the price change will depend on the “wage gap,” namely the average increase in wages needed to bring workers beneath the mandated minimum up to the minimum.

Next, we calculate the change in employment. Assuming that $\frac{\Delta \log Y}{\Delta \log p} = -\eta$, the change in employment has the following form:

$$\frac{\partial \log l_j}{\partial w_j} \Delta w_j = (-\eta + \sigma) \Delta \log p - \sigma \frac{1}{w_j} (mw - w_j)$$

Based on this, the aggregate employment change between \underline{w} and the MW can be written as:

$$\Delta Emp = \int_{\underline{w}}^{MW} \frac{\partial l_j}{\partial w_j} \Delta w_j dj = (-\eta + \sigma) \Delta \log p \int_{\underline{w}}^{MW} l_j dj - \sigma \int_{\underline{w}}^{MW} \frac{l_j (MW - w_j)}{w_j} dj$$

$$\% \Delta Emp = \frac{\int_{\underline{w}}^{MW} \frac{\partial l_j}{\partial w_j} \Delta w_j dj}{\int_{\underline{w}}^{MW} l_j dj} = -\eta \Delta \log p - \sigma \left(\frac{\int_{\underline{w}}^{MW} l_j \frac{MW - w_j}{w_j} dj}{\int_{\underline{w}}^{MW} l_j dj} - \frac{\int_{\underline{w}}^{MW} l_j w_j \frac{mw - w_j}{w_j} dj}{\int_{\underline{w}}^{\bar{w}} l_j w_j dj} \right)$$

Notice that when $MW \approx \underline{w}$, (i.e., we are considering a small minimum wage), then $\Delta \log p \approx 0$ and so

this simplifies to

$$\% \Delta Emp = -\sigma$$

Therefore, the size of the bunching will depend on the substitution elasticity between different type of labor. A large spike indicates that the $\% \Delta Emp$ is small, and so is σ . On the other hand, if there is no bunching at the minimum wage, $\% \Delta Emp$ is large and so σ is also large.

What about raising the minimum wage from MW_1 to MW_2 ? With a binding minimum wage MW the employment at bunching is the following:

$$EM = \int_{\underline{w}}^{MW} Y \left(\frac{a_j p}{MW} \right)^\sigma dj = Y p^\sigma MW^{-\sigma} \int_{\underline{w}}^{MW} a_j^\sigma dj$$

and the price is the following

$$p = \left(\int_{\underline{w}}^{MW} a_j^\sigma MW^{1-\sigma} dj + \int_{MW}^{\bar{w}} a_j^\sigma w_j^{1-\sigma} dj \right)^{\frac{1}{1-\sigma}}$$

The change in bunching will be the following:

$$\frac{\partial \log EM}{\partial MW} = \frac{\partial \log Y}{\partial MW} + \sigma \frac{\partial \log p}{\partial MW} - \sigma \frac{1}{MW} + \frac{a_{MW}^\sigma}{\int_{\underline{w}}^{MW} a_j^\sigma dj}$$

(also notice that however $\frac{Y p^\sigma MW^{-\sigma} a_{MW}^\sigma}{\int_{\underline{w}}^{MW} Y p^\sigma MW^{-\sigma} a_j^\sigma dj} = \frac{l_{MW}}{\int_{\underline{w}}^{MW} l_j dj}$), while the effect on prices is the following:

$$\frac{\partial \log p}{\partial MW} = \int_{\underline{w}}^{MW} a_j^\sigma MW^{-\sigma} dj = \frac{1}{Y p^\sigma} \int_{\underline{w}}^{MW} Y p^\sigma a_j^\sigma MW^{-\sigma} dj = \frac{1}{Y p^\sigma} \int_{\underline{w}}^{MW} l_j dj$$

We can write the change in the missing mass (MM) of jobs below the minimum wage as:

$$\frac{\partial \log MM}{\partial MM} = \frac{a_{mw}^\sigma}{\int_{\underline{w}}^{mw} a_j^\sigma dj}$$

The difference between the excess jobs at the minimum wage (bunching) and the missing jobs below the minimum wage measures the effect of the minimum wage on employment:

$$\% \Delta Emp = \frac{\partial \log EM}{\partial MW} - \frac{\partial \log MM}{\partial MW} = \frac{\partial \log Y}{\partial MW} + \sigma \frac{\partial \log p}{\partial MW} - \sigma \frac{1}{MW}$$

and the employment elasticity:

$$\frac{\% \Delta Emp}{\% \Delta MW} = -\eta \frac{\int_w^{MW} MW \cdot l_j dj}{Y p^\sigma} - \sigma \left(1 - \frac{\int_w^{MW} MW \cdot l_j dj}{Y p^\sigma} \right)$$

Online Appendix B: Estimating employment effect of minimum wages in a model of monopsonistic competition

In this appendix, we consider a classic monopsonistic competition model (e.g., [Butcher et al. \(2012\)](#)) to analyze employment effects of a minimum wage in the low-wage labor market. The labor market for lower-skilled workers affected by minimum wage is assumed to be a separate segment of the full labor market. In this market, there is a large number of employers, who differ in productivity $A \sim G(A)$ distributed over the interval $[0, \bar{A}]$. Employers compete over a supply of workers, L . The share of labor supply going to a firm rising in wage w_i :

$$n_i = w_i^\epsilon \frac{L}{\int_0^{\bar{A}} W(A)^\epsilon dG(A)} = w_i^\epsilon \Theta$$

This form of constant elasticity labor supply function facing the firm comes out of a framework where heterogeneous worker preferences over amenities at the workplace follow a type-1 Extreme Value distribution ([Card et al. \(2016\)](#)). Since labor is assumed to be inelastically supplied to the market, and all employers are labor supply constrained, there is full employment in the equilibrium:

$$\int_0^{\bar{A}} n_i di = \int_0^{\bar{A}} \frac{w_i^\epsilon L}{\int_0^{\bar{A}} w(A)^\epsilon dG(A)} \cdot di = L$$

Importantly, from the firm's perspective, Θ is a fixed parameter that they do not control. Firms maximize

$$\max \Pi_i = (A_i - w_i)n_i$$

$$FOC : w_i = \frac{\epsilon}{1+\epsilon} A_i$$

$$n_i = \left(\frac{\epsilon}{1+\epsilon} A_i \right)^\epsilon \Theta$$

Wages w and employment n rise with firm-level productivity, A . Wages follow a standard mark-down rule which depends inversely on the labor supply elasticity ϵ .

What happens with imposition of a minimum wage MW ? In equilibrium, since L is fixed, the aggregate employment is unaffected by a minimum wage increase. However, the lack of overall employment effect does not mean there are not important reallocation effects across firms. First, all employers with $A < MW$ go out of business. However, as some firms go out of business, the recruitment rate rises in incumbent firms to exactly offset the job loss: Θ rises to fully reallocate all workers in equilibrium to keep overall employment constant.

We can decompose the overall employment change into several components relevant for understanding the

bunching estimate. First, there will be no jobs paying below the new minimum. The number of “missing mass” of jobs below the new minimum wage can be written as:

$$MM = \Theta \left(\int_{MW}^{MW \frac{1+\epsilon}{\epsilon}} \left(\frac{\epsilon}{1+\epsilon} A_i \right)^\epsilon dG(A) + \int_0^{MW} \left(\frac{\epsilon}{1+\epsilon} A_i \right)^\epsilon dG(A) \right)$$

This missing jobs include previous employment at surviving firms are forced to raise their wage up to \underline{w} , i.e., $\int_{MW}^{MW \frac{1+\epsilon}{\epsilon}} \Theta \left(\frac{\epsilon}{1+\epsilon} A_i \right)^\epsilon dG(A)$. Additionally, it includes jobs that disappear because the productivity is lower than the minimum wage \underline{w} : $\Theta \int_0^{MW} \left(\frac{\epsilon}{1+\epsilon} A_i \right)^\epsilon dG(A)$.

Denoting as Θ_{MW} the new equilibrium value accounting for wage changes, the excess mass of jobs exactly at the spike is:

$$EM = \Theta_{MW} \int_{MW}^{MW \frac{1+\epsilon}{\epsilon}} MW^\epsilon dG(A) = \Theta_{MW} \left[G \left(MW \frac{1+\epsilon}{\epsilon} \right) - G(MW) \right] MW^\epsilon$$

This is employment at surviving firms now forced to pay \underline{w} . For surviving firms, for a given value of Θ , employment will be strictly greater than before, as they are raising the wages attracting more workers. The spike at the minimum (EM) is larger when, ceteris paribus, (1) the mass of surviving firms paying below MW is large and (2) when the labor supply elasticity facing the firm is large, allowing a relatively larger number of workers to be recruited when firms are forced to pay MW .

Finally, there is a market level reallocation that affects recruitment efficiency that affects all surviving firms via a change in Θ :

$$R = (\Theta_{MW} - \Theta) \int_{MW \frac{1+\epsilon}{\epsilon}}^{\infty} \left(\frac{\epsilon}{1+\epsilon} A_i \right)^\epsilon dG(A)$$

Reallocation leads to a proportionate change in employment in firms paying above MW . The sign of reallocation effect ($\Theta_{MW} - \Theta$) depends on model parameters. As polar cases, consider when all of the firms initially paying below MW go out of business; in this case, the reallocation effect is likely to be positive as increased employment at firms paying above MW compensates for the job loss. In contrast, if no employer goes out of business, the surviving firms who raise wages see increased employment—which has to come from reduced employment from firms paying above the minimum wage.

We can write the total change in employment as the sum of the excess mass in jobs at the spike less than missing jobs below the new minimum, along with a market-level reallocation effect. However, since we know $\int_0^{\bar{A}} n_i di = L$ with or without the minimum wage, the overall employment effect is zero.

$$\Delta Emp = [EM - MM + R] = 0$$

If the bunching estimator uses a $\bar{W} \geq \frac{\epsilon}{1+\epsilon} \bar{A}$, it will consistently estimate the true zero effect: $\Delta \hat{N}_{Bunching} = EM - MM + R = 0$. If we pick a sufficiently high upper limit \bar{W} to calculate the excess number jobs above MW , we will capture all the relevant reallocation effect, and will correctly find a zero disemployment effect implied by this data generating process. Note that this is a much weaker requirement than choosing the maximum wage in the overall labor market (i.e., using aggregate employment), because the relevant range of wages for the market for low-skilled workers is likely much smaller than the full range of wages in the labor market. Moreover, if there is a reallocation effect, we will likely be able to detect it empirically by estimating the change in employment counts for wages above MW . If the changes in employment count are close to zero for wages around \bar{W} , this suggests all reallocation effects have likely been captured.