THE EFFECT OF MINIMUM WAGES ON LOW-WAGE JOBS*

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We estimate the effect of minimum wages on low-wage jobs using 138 prominent state-level minimum wage changes between 1979 and 2016 in the United States using a difference-in-differences approach. We first estimate the effect of the minimum wage increase on employment changes by wage bins throughout the hourly wage distribution. We then focus on the bottom part of the wage distribution and compare the number of excess jobs paying at or slightly above the new minimum wage to the missing jobs paying below it to infer the employment effect. We find that the overall number of low-wage jobs remained essentially unchanged over the five years following the increase. At the same time, the direct effect of the minimum wage on average earnings was amplified by modest wage spillovers at the bottom of the wage distribution. Our estimates by detailed demographic groups show that the lack of job loss is not explained by labor-labor substitution at the bottom of the wage distribution. We also find no evidence of disemployment when we consider higher levels of minimum wages. However, we do find some evidence of reduced employment in tradeable sectors. We also show how decomposing the overall employment effect by wage bins allows a transparent way of assessing the plausibility of estimates. *JEL* Codes: J23, J38, J88.

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I. Introduction

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

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

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

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

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

We use hourly wage data from the 1979–2016 Current Population Survey to estimate the effect of the minimum wage by wage bins. We find that an average minimum wage hike led to a large and significant decrease in the number of jobs below the new minimum wage in the five years after implementation. At the same time, there was clear evidence for the emergence of excess jobs at or slightly above the minimum wage. However, as expected, we find no indication of any employment changes in the upper part of the wage distribution—providing further validation to the empirical design. We estimate that the number of excess jobs closely matched the number of missing jobs: the employment for affected workers rose by a statistically insignificant 2.8% (std. err. 2.9%). Our estimates also allow us to calculate the impact of the policy on the average wages of affected workers, which rose by around 6.8% (std. err. 1.0%). The significant increase in average wages of

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affected workers implies an employment elasticity with respect to own wage (or the labor demand elasticity in a competitive model) of 0.41 (std. err. 0.43), which rules out elasticities more negative than −0.45 at the 95% confidence level.

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

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

To go beyond our overall assessment of the 138 case studies used for identification, we also produce event-by-event estimates of the minimum wage changes. Although we find substantial heterogeneity in the bite of the events, the distribution of employment effects are consistent with a sharp null of zero effect everywhere. For example, our event-by-event analysis finds that the estimated missing jobs rose substantially in magnitude with the

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minimum-to-median wage (Kaitz) index. At the same time, the number of excess jobs also rose for these events to a nearly identical extent. As a consequence, there is no relationship between the employment estimate and the Kaitz index up to around 59%, confirming that the minimum wage changes in the United States that we study have yet to reach a level above which significant disemployment effects emerge.

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

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

Our approach also allows us to provide a more comprehensive picture on responses across various sectors of the economy. We show that the minimum wage is likely to have a negative effect on employment in the tradeable sector, and in manufacturing in particular—with an employment elasticity with respect to own wage of around −1.4—although the estimates are imprecise. At the same time, the effect of the minimum wage is close to 0 in nontradeable sectors (such as restaurants or retail), which employ most minimum wage workers in the United States today. This evidence suggests that the industry composition of the local economy is likely to play an important role in determining the disemployment effect of the minimum wage (Harasztosi and Lindner forthcoming).

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This article makes several contributions to the existing literature on minimum wages. First, it relates to the large and controversial literature on the employment effects of the minimum wage. The debate has often been concentrated on the impact on teen employment (Card 1992; Neumark and Wascher 1992; Neumark, Salas, and Waschler 2014; Allegretto et al. 2017), workers in specific sectors (Lester 1964; Katz and Krueger 1992; Card and Krueger 1994; Dube, Lester, and Reich 2010), or workers earning low wages prior to the minimum wage increase (Currie and Fallick 1996; Abowd et al. 2000; Clemens and Wither 2019), while the evidence on the impact on overall employment is scant. By disaggregating the standard difference-in-differences estimates by wage bins, we can identify the effects of the minimum wage on overall employment and obtain meaningful first-stage wage effects at the same time.

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

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find that in the post-1992 period, there is little evidence of disemployment for teens across any of the standard specfications.

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

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

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

II. Methodology and Data

II.A. The Conceptual Framework

We infer the effect of the minimum wage from the employment changes at the bottom of the wage distribution. We illustrate our

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

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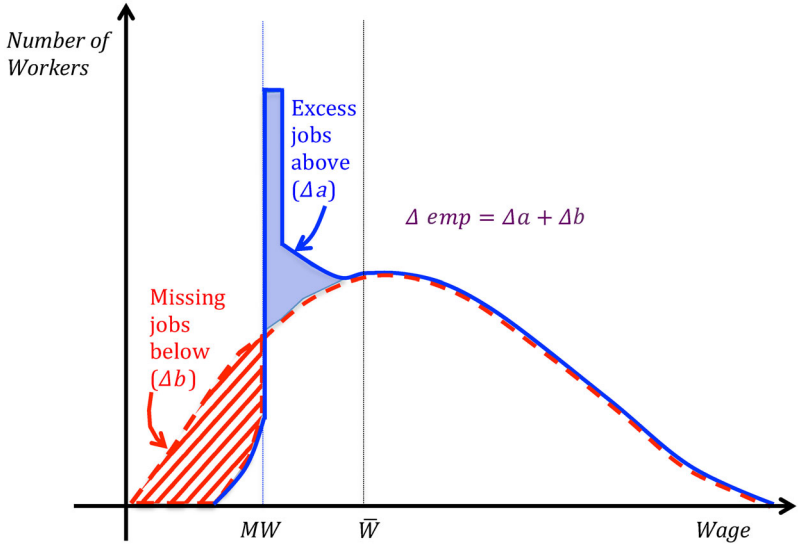

FIGURE I

The Impact of Minimum Wages on the Frequency Distribution of Wages

The figure shows the effect of the minimum wage on the frequency distribution of hourly wages. The red dashed line (color version available online) shows the wage distribution before the introduction of the minimum wage, and the blue solid line shows the distribution afterwards. Because compliance is less than perfect, some workers are paid below the minimum wage, and the post-treatment distribution starts below the minimum wage. For other workers, the introduction of the minimum wage produces "missing jobs" (Δb), as shown by the striped red shaded area (under the red dashed line) between the origin and MW. These missing jobs may either reflect workers getting a raise, or their jobs being destroyed. The former group creates the "excess jobs above" (Δa), as shown by the solid blue shaded area (under the blue solid line) between MW and $\overline{W}$, the upper limit for any effect of the minimum wage on the earnings distribution. The overall change in employment due to the minimum wage (Δe) is the sum of the two areas ($\Delta a + \Delta b$).

approach using Figure I, which summarizes the effect of the minimum wage on the wage distribution. The red dashed line (color version available online) shows a hypothetical (frequency) distribution of wages in the absence of the minimum wage. The blue solid line depicts the actual wage distribution with a minimum wage at MW.

In the presence of a binding minimum wage, there should be no jobs below MW. In practice, however, some jobs observed in the data will be sub–minimum wage because of imperfect coverage, imperfect compliance, or measurement error. Therefore, the

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number of missing jobs below MW, given by $\triangle b = Emp^1[w < MW] - Emp^0[w < MW]$, reflects the bite of the minimum wage.[2] Here $Emp^1[.]$ and $Emp^0[.]$ are the actual and counterfactual frequency distributions of wages, respectively.

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

The neoclassical model suggests that there may be some positive employment effects in the upper tail of the wage distribution caused by labor-labor substitution. However, as we discuss in Online Appendix B, because minimum wage workers' share in overall production is very small (around 2% in the United States), reasonable calibrations of a neoclassical model would suggest very small upper-tail effects. For example, if we consider an elasticity of substitution between high- and low-wage workers of around 1.4 based on Katz and Murphy (1992), and an output demand elasticity of around 1 based on Aaronson and French (2007), the implied upper-tail employment elasticity with respect to the minimum wage would be around 0.006. In Online Appendix Table B.1, we show that reasonable variations in the key parameters uniformly suggest that plausible estimates of minimum wage impact on upper-tail employment should be very small. Moreover, any

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

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theoretical upper-tail effects would be positive, so ignoring them will overstate the net job losses.

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

II.B. Empirical Implementation

The key empirical challenge is to estimate the counterfactual wage frequency distribution in the absence of a minimum wage increase. Instead of using either ad hoc functional forms (Meyer and Wise 1983; Dickens, Machin, and Manning 1998) or the distribution prior to the minimum wage increase (Harasztosi and Lindner forthcoming), we exploit state-level variation in the minimum wage and identify the counterfactual distribution using a difference-in-differences event study design. Our event-based approach uses a similar framework as Autor, Donohue, and Schwab (2006) and examines employment changes within an eight-year window around 138 prominent state-level minimum wage events, where states increased their minimum wage by at least \$0.25, and where at least 2% of the workers were directly affected by the increase.[3] By focusing on employment changes around the event window, we incompletely capture long-run effects of the minimum wage. Nevertheless, as we show below, we find no evidence of a change in employment up to five years after the minimum wage hike, and so it strikes us as unlikely that our empirical

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

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design misses important long-term employment changes. Online Appendix Table A.6 shows the robustness of estimates to alternative window lengths, including allowing for up to a seven-year post-treatment period.

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

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

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

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

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

In addition, since our approach locates the source of the employment effects within the wage distribution, we can use the upper-tail employment changes as an added falsification test.[5] Because large positive or negative changes in jobs paying above, say, $15 an hour are unlikely to reflect the causal effect of the minimum wage, reporting such employment changes in the upper tail can be highly informative about model validity. Moreover, the potential bias from the confounding factors affecting the upper tail can be especially large when only a small fraction of the workforce is directly affected by the minimum wage (as is true in the United States). The contribution of these omitted variables may be sizable compared with the relatively small expected effect of the minimum wage on aggregate employment. As a result, the bias arising from shocks to the upper tail can be particularly severe when we are interested in estimating the overall employment effect of the minimum wage.

There are numerous advantages of decomposing the aggregate employment changes by wage bins. First, such a decomposition allows us to focus on employment changes locally around the new minimum wage—the part of the wage distribution where we expect the policy to play a role. This variation is highly informative, yet rarely exploited. Second, and more importantly, our localized approach allows us to estimate the effects on overall employment as well as for subgroups where the standard approaches often fail to provide meaningful estimates on employment and wages. When only a small fraction of workers are directly affected by the minimum wage, the effect on the average wage of such subgroups will be very small. Without a clear wage effect, it is

5. This idea is similar to Autor, Manning, and Smith (2016) who use unrealistically large spillover effects to validate the empirical model in use.

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not clear how to interpret the size of any employment effect found for those groups.[6] Third, the localized focus around the minimum wage often improves the precision of estimates by filtering out random shocks to jobs in the upper part of the wage distribution.[7]

We use the estimated $\alpha_{\tau k}$ from equation (1) to calculate the change in employment throughout the wage distribution in response to the policy. The change in the number of jobs (per capita) paying below the new minimum wage between event date -1 and τ can be calculated as $\sum_{k=-4}^{-1} \alpha_{\tau k} - \sum_{k=-4}^{-1} \alpha_{-1k}$. To be clear, this is a difference-in-differences estimate, as it nets out the change in the counterfactual distribution implicitly defined by the regression equation (1). Analogously, the change in the number of jobs (per capita) paying between the minimum wage and $\overline{W}$ is $\sum_{k=0}^{\overline{W}-MW} \alpha_{\tau k} - \sum_{k=0}^{\overline{W}-MW} \alpha_{-1k}$. For our baseline estimates, we set $\overline{W} = MW + 4$.[8] We define the excess jobs at or above the minimum wage as $\Delta a_\tau = \frac{\sum_{k=0}^{4} \alpha_{\tau k} - \sum_{k=0}^{4} \alpha_{-1k}}{\overline{EPOP}_{-1}}$, and the missing jobs below as $\Delta b_\tau = \frac{\sum_{k=-4}^{-1} \alpha_{\tau k} - \sum_{k=-4}^{-1} \alpha_{-1k}}{\overline{EPOP}_{-1}}$. By dividing the employment changes by $\overline{EPOP}_{-1}$, the sample average employment-to-population ratio in treated states during the year (four quarters) prior to treatment, we normalize the excess and missing jobs by the pretreatment total employment. The Δa_τ and Δb_τ values plot out the evolution of excess and missing jobs over event time τ. We also report the excess and missing employment estimates averaged over the five years following the minimum wage increase, $\Delta b = \frac{1}{5} \sum_{\tau=0}^{4} \Delta b_\tau$ and $\Delta a = \frac{1}{5} \sum_{\tau=0}^{4} \Delta a_\tau$.

Given our normalization, $\Delta e = \Delta a + \Delta b$ represents the estimate for the percentage change in total employment due to the

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

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

8. Online Appendix Table A.5 shows that the results are robust to higher cutoffs.

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minimum wage increase. We refer to this estimate as "event-based bunching" or EB-bunching estimates to highlight that we are (i) using an event-based difference-in-differences design and (ii) estimating the excess and missing jobs locally around the bunching in the distribution at the minimum wage.

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

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

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

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

We also use the estimated coefficients to compute the percentage change in the average hourly wage for affected workers. We calculate the average wage by taking the ratio of the total wage bill collected by workers below the new minimum wage to the number of such workers. Prior to treatment, it is equal to $\overline{w}_{-1} = \frac{\overline{wb}_{-1}}{\overline{b}_{-1}}$. Here the wage bill, $\overline{wb}_{-1}$, and the number of workers earning below the new minimum wage just prior to the increase, $\overline{b}_{-1}$, are averages for the full sample of events. The minimum wage increase causes both the wage bill and employment to change. The new average wage in the post-treatment period is equal to $w = \frac{\overline{wb}_{-1}+\Delta wb}{\overline{b}_{-1}+\Delta e}$.[10] Therefore,

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

10. The change in wage bill can be written as a function of our regression coefficients as follows. Averaging the coefficients over the five-year post-treatment window, $\alpha_k = \frac{1}{5}\sum_{\tau=0}^{4}\alpha_{\tau,k}$, we can write $\Delta wb = \sum_{k=-3}^{4}\left(k + \overline{MW}\right)\cdot(\alpha_k - \alpha_{-1k})$, where

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the percentage change in the average wage of affected workers is given by:

$$(2) \qquad \%\Delta w = \frac{w}{\overline{w}_{-1}} - 1 = \frac{\dfrac{\overline{wb}_{-1} + \Delta wb}{\overline{b}_{-1} + \Delta e}}{\dfrac{\overline{wb}_{-1}}{\overline{b}_{-1}}} - 1 = \frac{\%\Delta wb - \%\Delta e}{1 + \%\Delta e}.$$

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

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

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

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

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

II.C. Data and Sample Construction

We use the individual-level NBER Merged Outgoing Rotation Group of the Current Population Survey for 1979–2016 (CPS) to

$\overline{MW}$ is (approximately) the sample average of the new minimum wage. We say approximately because k is based on \$1 increments, and so $\overline{MW}$ is calculated as the sample mean of $[MW, MW + 1)$.

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calculate quarterly, state-level distributions of hourly wages. For hourly workers, we use the reported hourly wage, and for other workers we define the hourly wage to be their usual weekly earnings divided by usual weekly hours. We do not use any observations with imputed wage data to minimize the role of measurement error.[11] There are no reliable imputation data for January 1994 through August 1995, so we exclude this entire period from our sample. Our available sample of employment counts therefore spans 1979q1 through 1993q4 and 1995q4 through 2016q4.[12]

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

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

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

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

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

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of Employment and Wages (QCEW), which is a near universe of quarterly employment (but lacks information on hourly wages). Online Appendix F explains the QCEW benchmarking in detail. As we discuss below, the QCEW benchmarking has little effect on our point estimates but substantially increases their statistical precision.

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

One concern when using $0.25 bins and CPS data is that some of the bins may be sparse with very few or no workers. However, we stress that our employment estimate is based on the sum of employment changes in 36 cells covering a $9 range [$MW - \$4$, $MW + \$4$], summed over at least 4 quarters (typically 20 quarters). As a result, small or zero employment in particular cells is not a major concern. In each state, there are, on average, around seven workers each quarter in each of the $0.25 bins between $5 and $15/hour in our sample.[16] Since the coefficients for our event dummies are estimated at a $1-bin-year-state level, on average, for these we use around 112 individual-level observations per event. Moreover, when we assess the total employment effects, we calculate the sum of the $1-bin estimates between $4 below and $4 above the minimum wage, and we consider five-year averages. This implies that, on average, we use approximately 5,040 individual worker observations per event. This is a well-sized sample that allows a reliable estimate of the true counts of employment for each event. Consistent with this point, we note

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

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

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

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again that results from our approach are very similar to those from a simpler method that uses state-by-quarter data and where the outcomes are the (per capita) number of jobs or total wage bill under $15/hour.

Another potential concern with the data is that misreporting of wages in the CPS may bias our estimates. If reported wages contain some measurement error, some workers earning above the minimum wage will appear to earn below it, which could attenuate the estimate for Δb. However, this does not affect the consistency of the estimate for $\Delta a + \Delta b$ as long as the minimum wage only affects reported wages below $\overline{W}$. The reason is straightforward. Assume that 1% of the workforce mistakenly report earning below the new minimum wage in the post-treatment period. This would lead our estimate of the missing jobs to be too small in magnitude: $\hat{\Delta b} = \Delta b + 0.01$. However, this misreporting would also lead to an equal reduction in the number of excess jobs above, producing the estimate $\hat{\Delta a} = \Delta a - 0.01$; this will be true as long as these misreported workers are coming from the range $[MW, \overline{W})$, which is likely to be satisfied for a wide variety of classical and nonclassical measurement error processes where the support of the measurement error is contained in $[MW - \overline{W}, \overline{W} - MW]$. Therefore, the employment estimate $\hat{\Delta a} + \hat{\Delta b}$ is likely to be unaffected by measurement error in reported wages. We also directly assess how misreported wages in the CPS may affect our results in Online Appendix E, where we compare the CPS hourly wage distribution to microaggregated administrative data on hourly wages from three U.S. states that collect this information. Reassuringly, the evolution of the number of jobs paying below the minimum wage, and the number of jobs paying up to $5 above the minimum wage in the CPS data from these three states match quite well with their counterparts using administrative data.[17]

17. In Online Appendix F, we also structurally estimate a model of measurement error in reported wages proposed by Autor, Manning, and Smith (2016), and show that the contribution of misreporting error to the overall variance in wages in the CPS and in administrative data on hourly wages from three states are very similar. Furthermore, we semiparametrically deconvolve the CPS wage distribution using the estimated measurement error model and show that our estimates using this measurement error–corrected distribution are very similar to the baseline estimates (Online Appendix Table F.3). In Online Appendix C we implement our approach using administrative data from Washington and find estimates to be similar when using the CPS. Although each piece of evidence has limitations, together they suggest that our employment and wage results are not likely to be

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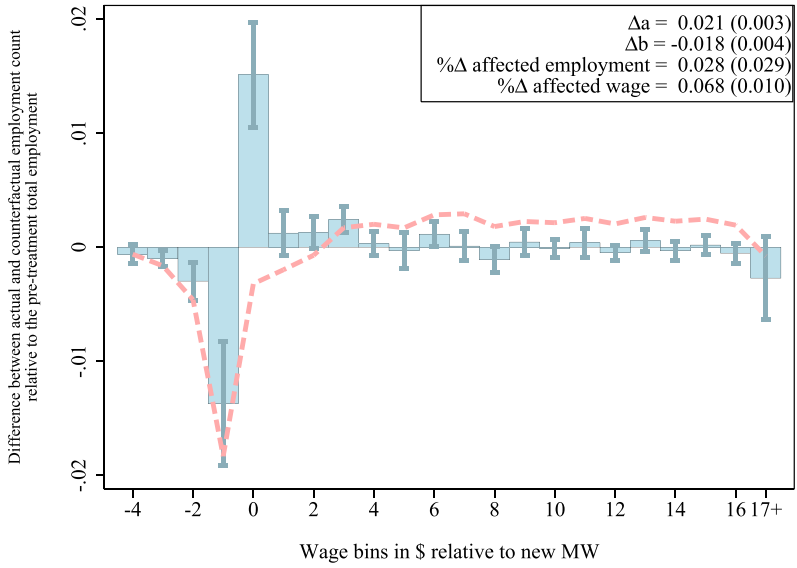

FIGURE II

Impact of Minimum Wages on the Wage Distribution

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

III. RESULTS

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

biased substantially due to measurement error. At the same time, more precise wage data could help better discern the exact nature of the wage effects including the extent of spillovers, the size of the spike, and the extent of noncompliance.

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changes are normalized to pretreatment total employment in the state. Several points should be noted.

First, there is a clear and significant drop in the number of jobs below the new minimum wage, amounting to 1.8% (std. err. 0.4%) of the total pretreatment employment.[18] Around $\frac{3}{4}$ of this reduction occurs in the $1 wage bin just under the new minimum. Second, there is also a clear and significant increase in jobs just at the new minimum wage (at the $0 wage bin). Third, there is also a statistically significant increase in employment in the wage bin $3 above the new minimum and modest, statistically insignificant increases in the $1 and $2 bins. This pattern of employment changes is consistent with limited wage spillovers resulting from the minimum wage increase, as suggested in Autor, Manning, and Smith (2016).[19] Fourth, the excess jobs between the new minimum and $4 above it represents 2.1% (std. err. 0.3%) of the total pretreatment employment. Fifth, the employment changes in the upper-tail wage bins, from $5 above the minimum wage to $17 or more (the final bin), are all small and statistically insignificant—individually and cumulatively as shown by the red line, which represents the running sum of employment changes. Finally, it is worth emphasizing that the drop in employment just below the new minimum, the equal-sized increase just above it, and the lack of employment change in the upper tail is exactly what we expect if employers are complying with the law and adjusting wages but not employment.

We estimate the employment change by adding the missing jobs below and excess jobs above the minimum wage: $\triangle a + \triangle b$. We divide this change by the jobs below the new minimum wage ($\bar{b}_{-1} = 8.6\%$) to obtain a change in the affected employment of

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

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

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2.8% (std. err. 2.9%), which is positive but statistically insignificant. We can also divide the employment change $\Delta a + \Delta b$ by the sample-averaged minimum wage increase of 10.1% to calculate the employment elasticity with respect to the minimum wage of 0.024 (std. err. 0.025). This estimate is statistically insignificant, and the 95% confidence interval rules out substantial reductions in the aggregate employment, including the baseline aggregate employment elasticity of −0.074 in Meer and West (2016) (see their Table 4). The most common minimum wage employment elasticities are from teens; for example, Neumark and Wascher (2008) argue that this falls between −0.1 and −0.3, while Allegretto et al. (2017) argue that it is closer to 0. However, the directly affected share of teens (43.2%) is much larger than the workforce overall (8.6%). Therefore, to make our estimates on overall employment comparable to the estimates for teens we can multiply our estimate and standard errors by the ratio of the shares $\frac{0.432}{0.086} = 5.02$. This leads to an affected-share-adjusted 95% confidence interval of [−0.13, 0.37], which rules out most of the −0.1 to −0.3 range.

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

Figure III shows the changes in the missing jobs paying below the new minimum wage (Δb_τ), and the excess jobs paying up to \$4 above the minimum wage (Δa_τ) over annualized event time using our baseline specification. All the estimates are expressed as changes from event date $\tau = -1$, or the year just prior to treatment, the estimates for which are normalized to 0. There are four important findings that we would like to highlight. First, we find a very clear reduction in the jobs paying below the new minimum wage (shown in red dashed line) between the year just prior to treatment ($\tau = -1$) and the year of treatment ($\tau = 0$)—this

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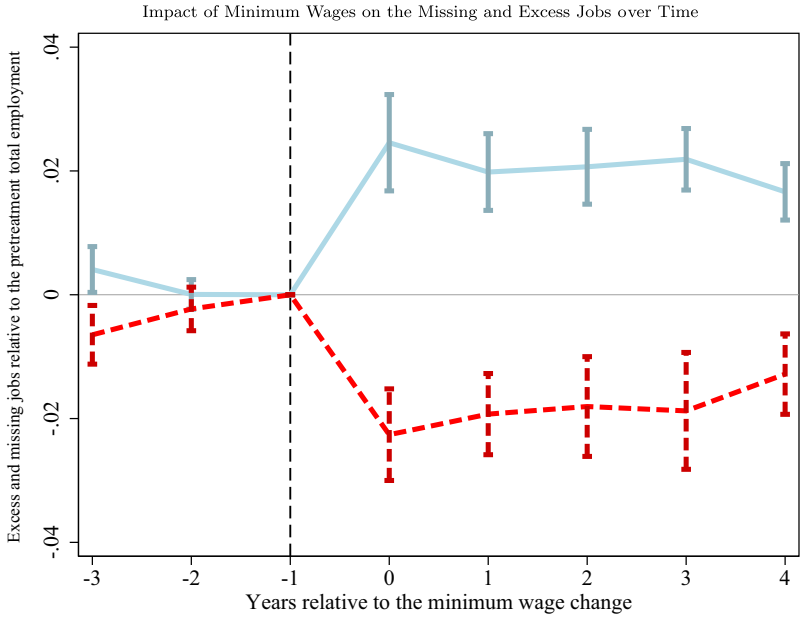

FIGURE III

Impact of Minimum Wages on the Missing and Excess Jobs over Time

The figure shows the main results from our event study analysis (see equation (1)) exploiting 138 state-level minimum wage changes between 1979 and 2016. The figure shows the effect of a minimum wage increase on the missing jobs below the new minimum wage (red dashed line; color version available online) and on the excess jobs at or slightly above it (blue solid line) over time. The red dashed line shows the evolution of the number of jobs (relative to the total employment one year before the treatment) between \$4 below the new minimum wage and the new minimum wage (Δb); the blue solid line shows the number of jobs between the new minimum wage and \$5 above it (Δa). We also show the 95% confidence interval based on standard errors that are clustered at the state level.

shows that the minimum wage increases under study are measurably binding. Second, although there is some reduction in the magnitude of the missing jobs in the post-treatment window, it continues to be very substantial and statistically significant five years out, showing that the treatments are fairly durable over the medium run.[20] Third, the response of the excess jobs at or above the new minimum (Δa) exhibits a very similar pattern in magnitudes, with the opposite sign. There is an unmistakable

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

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jump in excess employment at $\tau = 0$, and a substantial portion of it persists and is statistically significant even five years out. Fourth, for both the changes in the excess and missing jobs there is only a slight indication of a preexisting trend prior to treatment. The $\tau = -2$ leads are statistically indistinguishable from 0 and although there is some evidence of changes three years prior to treatment, the leading effects are very small relative to the post-treatment effect estimates. Moreover, the slight downward trend in excess jobs, and the slight upward trend in missing jobs is consistent with a falling value of the real minimum wage prior to treatment. The sharp jump in both the excess and missing jobs at $\tau = 0$, the lack of substantial pretreatment trends, and the persistent post-treatment gap between the two shares all provide strong validation of the research design. Online Appendix Figure A.5 shows analogous time paths for wages and employment showing a sharp and persistent wage effect at $\tau = 0$ coupled with little change in employment over the event window—either before or after treatment.

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

Overall, the estimates from the additional specifications are fairly similar to the baseline estimate. In all cases, there is a clear bite of the policy as measured by the reduction in jobs paying below the minimum, Δb. Consistent with the presence of a substantial bite, there is a statistically significant increase in real wages of affected workers in all specifications: these range

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TABLE I

IMPACT OF MINIMUM WAGES ON EMPLOYMENT AND WAGES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Missing jobs below new MW (Δb)	-0.018^{***}	-0.018^{***}	-0.018^{***}	-0.016^{***}	-0.016^{***}	-0.015^{***}	
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	
Excess jobs above new MW (Δa)	0.021^{***}	0.018^{***}	0.020^{***}	0.016^{***}	0.014^{***}	0.015^{***}	
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	
%Δ affected wages	0.068^{***}	0.057^{***}	0.068^{***}	0.049^{***}	0.043^{***}	0.050^{***}	0.065^{***}
	(0.010)	(0.010)	(0.012)	(0.010)	(0.010)	(0.011)	(0.010)
%Δ affected employment	0.028	0.000	0.022	-0.002	-0.019	-0.000	0.027
	(0.029)	(0.023)	(0.021)	(0.021)	(0.021)	(0.023)	(0.028)
Employment elasticity w.r.t. MW	0.024	0.000	0.019	-0.001	-0.016	-0.000	0.023
	(0.025)	(0.020)	(0.018)	(0.018)	(0.018)	(0.019)	(0.024)
Emp. elasticity w.r.t. affected wage	0.411	0.006	0.326	-0.032	-0.449	-0.003	0.410
	(0.430)	(0.402)	(0.313)	(0.439)	(0.574)	(0.455)	(0.421)
Jobs below new MW ($\bar{b}_{-1}$)	0.086	0.086	0.086	0.086	0.086	0.086	0.086
%Δ MW	0.101	0.101	0.101	0.101	0.101	0.101	0.101
Number of events	138	138	138	138	138	138	138
Number of observations	847,314	847,314	847,314	847,314	847,314	847,314	14,484
Number of workers in the sample	4,694,104	4,694,104	4,694,104	4,694,104	4,694,104	4,694,104	4,694,104

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TABLE I

CONTINUED

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Controls							
Bin-state FE	Y	Y	Y	Y	Y	Y	
Bin-period FE	Y	Y	Y	Y	Y	Y	
Bin-state linear trends		Y	Y		Y	Y	
Bin-state quadratic trends			Y			Y	
Bin-division-period FE				Y	Y	Y	
State FE							Y
Year FE							Y

Notes. The table reports the effects of a minimum wage increase based on the event study analysis (see equation (1)) exploiting 138 state-level minimum wage changes between 1979 and 2016. The table reports five-year averaged post-treatment estimates on missing jobs up to $4 below the new minimum wage, excess jobs at and up to $5 above it, employment, and wages. Column (1) shows the benchmark specification while columns (2)–(6) explore robustness to bin-state time trends and bin-division-period fixed effects. Column (7) reports the simpler methodology estimates where we calculate changes in affected wage and employment by using state-by-quarter data, where the outcomes are the number of jobs or the total wage bill under $15 per hour. Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state. Significance levels are *0.10, **0.05, ***0.01.

The first two rows report the change in number of missing jobs below the new minimum wage (Δb), and excess jobs above the new minimum wage (Δa) relative to the pretreatment total employment. The third row, the percentage change in average wages in the affected bins, ($\%\Delta$W), is calculated using equation (2). The fourth row, percentage change in employment in the affected bins, is calculated by dividing change in employment by jobs below the new minimum wage ($\frac{\Delta a+\Delta b}{b_{-1}}$). 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 $\frac{1}{\%\Delta W}\frac{\Delta a+\Delta b}{b_{-1}}$. The line on the number of observations shows the number of quarter-bin cells used for estimation, while the number of workers refers to the underlying CPS sample used to calculate job counts in these cells.

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between 5.7% and 6.9% with common wage-bin-period effects (columns (1)–(3)), and between 4.3% and 5.0% with division-specific wage-bin-period effects (columns (4)–(6)). In contrast, the proportionate change in employment for affected workers is never statistically significant, and is numerically smaller than the wage change, ranging between −1.9% and 3.6% across the eight specifications. For the most part, the employment estimates are small or positive; the only exception is column (5) with state-specific linear trends and bin-division-specific period effects. The employment elasticity with respect to the wage is −0.449 (std. err. 0.574). However, adding quadratic trends to the former specification (column (6)) substantially reduces the magnitude of the employment elasticity with respect to the wage to −0.003 (std. err. 0.455).

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

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

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

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III.A. Heterogenous Responses to the Minimum Wage

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

1. By Demographic Groups. We assess the presence of labor-labor substitution at the bottom of the wage distribution by examining employment responses across various demographic groups.[22] In Table II we report estimates for workers without a high school degree, those with high school or less schooling, women, black or Hispanic individuals, and teens using our baseline specification (see equation (1)).

As expected, restricting the sample by education and age produces a larger bite. For example, for those without a high school degree, the missing jobs estimate, Δb, is -6.5% while for those with high school or less schooling it is -3.2%. These estimates for the missing jobs are, respectively, 261% and 78% larger than the baseline estimate for the overall population (-1.8%, from Table I, column (1)). Nevertheless, the large variation in the missing jobs across various demographic groups is matched closely by excess jobs above the new minimum wage.[23] In all cases, except for the black or Hispanic group, the excess jobs are larger than the missing jobs, indicating a positive albeit statistically insignificant employment effect. For black or Hispanic individuals, the difference between excess and missing jobs is negligible. As a result, the employment elasticities with respect to own wage range between -0.086 and 0.570 for the first five demographic groups of the table. In all cases but one, the elasticities are statistically indistinguishable from 0. The sole exception is those without a high school degree, for whom the employment elasticity with respect to the wage is 0.475 (std. err. 0.268) which is marginally significant at the ten percent level. The minimum wage elasticity for teens is 0.125, which is more positive than some of the estimates in the

22. Existing evidence on labor-labor substitution has typically focused on specific groups like teens (Giuliano 2013), individual case studies (Fairris and Bujanda 2008), or specific segments like online labor platforms (Horton 2018).

23. In Online Appendix Figure A.8 we also show that the close match between excess jobs and missing jobs holds also if we fully partition the workforce into 23 age-education cells.

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TABLE II

IMPACT OF MINIMUM WAGES ON EMPLOYMENT AND WAGES BY DEMOGRAPHIC GROUPS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Missing jobs below new MW (Δb)	-0.065^{***}	-0.032^{***}	-0.114^{***}	-0.023^{***}	-0.028^{***}	-0.094^{***}	-0.020^{***}	-0.004^{***}
	(0.010)	(0.007)	(0.010)	(0.005)	(0.008)	(0.010)	(0.005)	(0.001)
Excess jobs above new MW (Δa)	0.075^{***}	0.038^{***}	0.127^{***}	0.026^{***}	0.028^{***}	0.100^{***}	0.021^{***}	0.004^{***}
	(0.011)	(0.006)	(0.020)	(0.004)	(0.006)	(0.012)	(0.003)	(0.001)
%Δ affected wages	0.080^{***}	0.076^{***}	0.083^{***}	0.072^{***}	0.044^{***}	0.073^{***}	0.051^{***}	0.060^{*}
	(0.014)	(0.014)	(0.018)	(0.011)	(0.012)	(0.011)	(0.013)	(0.032)
%Δ affected employment	0.038	0.043	0.030	0.025	-0.004	0.015	0.015	0.011
	(0.024)	(0.030)	(0.032)	(0.027)	(0.044)	(0.018)	(0.048)	(0.055)
Employment elasticity w.r.t. MW	0.097	0.061	0.125	0.025	-0.005	0.052	0.016	0.003
	(0.061)	(0.042)	(0.134)	(0.027)	(0.058)	(0.062)	(0.049)	(0.014)
Emp. elasticity w.r.t. affected wage	0.475^{*}	0.570	0.356	0.343	-0.086	0.206	0.304	0.184
	(0.268)	(0.386)	(0.317)	(0.362)	(1.005)	(0.233)	(0.904)	(0.841)
Jobs below new MW ($\bar{b}_{-1}$)	0.264	0.145	0.432	0.102	0.133	0.358	0.104	0.027
%Δ MW	0.103	0.103	0.102	0.101	0.100	0.103	0.103	0.103

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TABLE II

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of events	138	138	138	138	138	138	138	138
Number of observations	847,314	847,314	847,314	847,314	846,729	847,314	847,314	847,314
Number of workers in the sample	660,771	2,248,711	287,484	2,277,624	781,003	469,226	1,830,393	2,349,485
Sample	Less than high school	High school or less	Teen	Women	Black or Hispanic	High probability	Medium probability	Low probability

Notes. The table reports effects of a minimum wage increase by demographic groups based on the event study analysis (see equation (1)) exploiting 138 state-level minimum wage changes between 1979 and 2016. The table reports five-year averaged post-treatment estimates on missing jobs up to \$4 below the new minimum wage, excess jobs at and up to \$5 above it, employment, and wages for individuals without a high school degree (column (1)), for individuals with high school degree or less schooling (column (2)), for teens (column (3)), for women (column (4)), for black or Hispanic workers (column (5)). Columns (6)–(8) report the results for groups of workers with differential probability of being exposed to the minimum wage changes. We use the Card and Krueger (1995) demographic predictors to estimate the probability of being exposed (see the text for details). Column 6 shows the results for the workers who have a high probability of being exposed to the minimum wage increase, column (7) for the middle-probability group, and column (8) for the low-probability group. All specifications include wage bin-by-state and wage bin-by-period fixed effects. Regressions are weighted by state-quarter aggregated population of the demographic groups. Robust standard errors in parentheses are clustered by state; significance levels are $^{*}0.10$, $^{**}0.5$, $^{***}0.01$.

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

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literature, though we note that it is not statistically significant given a standard error of 0.134.[24]

In addition, we examine the effects on groups of workers with differential probability of being exposed to the minimum wage changes. To determine the likelihood of exposure, we construct a prediction model analogous to Card and Krueger (1995). We use observations from three years prior to the 138 events that also lie outside any of the five-year post-treatment windows and estimate a linear probability model of having a wage less than 125% of the statutory minimum wage on a rich set of demographic predictors.[25] We use the estimated model to obtain predicted probabilities of being exposed to minimum wage increases for all individuals in the sample regardless of their actual employment status. We then use the predicted probabilities to sort individuals into three groups: a "high-probability" group that contains individuals in the top 10% of the predicted probability distribution; a "low-probability" group that contains workers in the bottom 50% of the predicted probabilities; and a middle group containing the rest.

As expected, the high-probability group shows a considerably larger bite ($\Delta b = -9.4\%$) than the middle group ($\Delta b = -2.0\%$) and the low-probability group ($\Delta b = -0.4\%$). At the same time, the employment elasticities are very similar across the

24. We note that the teen estimates are unrelated to a focus on low-wage jobs, since the benefits of focusing on employment changes around the minimum wage is small for groups where most workers are low-wage ones. In Online Appendix Table A.10 we show that our event study estimates are close to 0 for teens even if we use overall teen employment. At the same time, the classic two-way fixed effect specification with log minimum wage (TWFE-logMW) generates a sizable negative estimate for teens and for overall employment as well. In Section IV we discuss this discrepancy and argue that the difference between our approach and TWFE-logMW are driven by how the two empirical models are affected by employment shocks in the 1980s and early 1990s. Online Appendix Table G.7 shows that in the post-1992 period, there is little divergence in teen minimum wage elasticities across standard specifications (including the TWFE-logMW); none of the specifications suggest noticeable losses to teen employment, and the elasticities are no more negative than −0.03.

25. We use the same predictors as in Card and Krueger (1995): all three-way interactions of nonwhite, gender, and teen indicators; all three-way interactions of nonwhite, gender, and age 20–25 indicators; an indicator for having less than high school education; continuous highest grade completed variable; a third-order polynomial in labor market experience; Hispanic ethnicity indicator; interactions of the education and experience variables with gender. Cengiz (2018) shows the predictions using this Card and Krueger model compare favorably with those from more sophisticated machine learning–based methods.

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probability groups. It is worth mentioning that the most precise estimate of the own-wage employment elasticity reported in this article appears in Table II, column (6), where we look only locally around the minimum wage and also focus on the high-probability group: the confidence interval rejects any value smaller than −0.251 and larger than 0.663. Such a confidence interval is quite narrow and rejects many estimates in the literature—highlighting the gains from combining the demographic profiling approach of Card and Krueger with the approach based on low-wage jobs advanced herein. The employment elasticities for the other groups are similar in magnitude, though less precise.[26]

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

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

In Table III we report estimates for various sectors in the economy. We assign workers to tradeable and nontradeable sectors following Mian and Sufi (2014).[27] The table shows that the bite of

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

27. Mian and Sufi (2014) define "tradeable" industries as having either the sum of imports and exports exceeding $10,000 per worker or $500 million total; their "nontradeable" sector consists of a subset of restaurant and retail industries; "construction" consists of construction, real estate, or land development–related industries. We use the list in Mian and Sufi (2014) of four-digit NAICS industries and census industry crosswalks to categorize all the industries in the CPS for 1992–2016. In our sample the shares of employment are 13%, 14%, 10%, for tradeable, nontradeable, and construction, respectively. See more details in Online Appendix E. Since consistent industrial classifications limit our sample to

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TABLE III

IMPACT OF MINIMUM WAGES ON EMPLOYMENT AND WAGES BY SECTORS (1992–2016)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Missing jobs below new MW (Δb)	-0.019^{***}	-0.016^{*}	-0.066^{***}	-0.003	-0.011^{***}	-0.101^{***}	-0.033^{***}	-0.017^{**}
	(0.004)	(0.008)	(0.007)	(0.002)	(0.003)	(0.015)	(0.003)	(0.008)
Excess jobs above new MW (Δa)	0.020^{***}	0.011	0.072^{***}	0.005	0.011^{***}	0.101^{***}	0.041^{***}	0.011
	(0.003)	(0.008)	(0.011)	(0.006)	(0.002)	(0.015)	(0.010)	(0.009)
%Δ affected wages	0.058^{***}	0.058	0.056^{***}	0.097	0.056^{***}	0.049^{***}	0.060^{***}	0.073
	(0.011)	(0.073)	(0.014)	(0.086)	(0.013)	(0.012)	(0.021)	(0.078)
%Δ affected employment	0.008	-0.111	0.022	0.051	0.009	-0.001	0.062	-0.101
	(0.031)	(0.136)	(0.037)	(0.163)	(0.044)	(0.026)	(0.080)	(0.145)
Employment elasticity w.r.t. MW	0.007	-0.056	0.060	0.019	0.005	-0.002	0.086	-0.052
	(0.027)	(0.069)	(0.103)	(0.059)	(0.026)	(0.117)	(0.111)	(0.074)
Emp. elasticity w.r.t. affected wage	0.140	-1.910	0.387	0.530	0.166	-0.011	1.040	-1.385
	(0.523)	(3.922)	(0.597)	(1.311)	(0.763)	(0.542)	(1.058)	(2.956)
Jobs below new MW ($\bar{b}_{-1}$)	0.087	0.050	0.270	0.036	0.057	0.434	0.136	0.050
%Δ MW	0.098	0.098	0.098	0.098	0.098	0.098	0.098	0.098

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TABLE III

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of events	118	118	118	118	118	118	118	118
Number of observations	554,931	554,931	554,931	554,931	554,931	554,931	554,931	554,931
Number of workers in the sample	2,652,792	358,086	384,498	274,812	1,504,643	156,634	315,397	349,749
Sector	Overall	Tradeable	Nontradeable	Construction	Other	Restaurants	Retail	Manufacturing

Notes. The table reports the effects of a minimum wage increase by industries based on the event study analysis (see equation (1)) exploiting 138 state-level minimum wage changes between 1992 and 2016. The table reports five-year averaged post-treatment estimates on missing jobs up to \$4 below the new minimum wage, excess jobs at and up to \$5 above it, employment, and wages for all sectors (column (1)), tradable sectors (column (2)), nontradeable sectors (column (3)), construction (column (4)), other sectors (column (5)), restaurants (column (6)), retail (column (7)), and manufacturing industries (column (8)). Our classification of tradable, nontradeable, construction, and other sectors follows Mian and Sufi (2014) (see Online Appendix D for the details). Regressions are weighted by state-quarter aggregated population. Robust standard errors in parentheses are clustered by state; significance levels are $^{*}0.10$, $^{**}0.05$, $^{***}0.01$.

The first two rows report the change in number of missing jobs below the new minimum wage (Δb), and excess jobs above the new minimum wage (Δa) relative to the pretreatment total employment. The third row, the percentage change in average wages in the affected bins, ($\%\Delta$W), is calculated using equation (2). The fourth row, percentage change in employment in the affected bins, is calculated by dividing change in employment by jobs below the new minimum wage ($\frac{\Delta a+\Delta b}{b_{-1}}$). 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 $\frac{1}{\%\Delta W}\frac{\Delta a+\Delta b}{b_{-1}}$. The line on the number of observations shows the number of quarter-bin cells used for estimation, while the number of workers refers to the underlying CPS sample used to calculate job counts in these cells.

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

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

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

3. By Pretreatment Employment Status. We consider the effect of the minimum wage separately on workers who were employed prior to the minimum wage increase (incumbent workers) and for new entrants into the labor market. We partition our sample of wage earners into incumbent workers and new entrants by exploiting the fact that the CPS interviews each respondent twice, exactly one year apart.[28] The partition limits our sample to

the 1992–2016 period, we first replicate our benchmark analysis using all industries for this restricted sample in Table III, column (1). The estimated employment and wage effects on this restricted sample are similar to the full 1979–2016 sample.

28. All CPS respondents are interviewed for four months in the first interview period, then rotated out of the survey for eight months, and then rotated back into the survey for a final four months of interviews. In the fourth month of each

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the 1980–2016 time period covering 137 eligible minimum wage-raising events and also restricts our time window to one year around the minimum wage increase rather than the five years in our baseline sample.

Figure IV shows the event study estimates for new entrants (Panel A) and incumbents (Panel B) for each k-dollar wage bin relative to the new minimum wage. For both subgroups, new minimum wages clearly bind, with significantly fewer jobs just below and significant more at the new minimum. This highlights that studies that restricts their sample to incumbent workers (e.g., Currie and Fallick 1996; Abowd et al. 2000; Clemens and Wither 2019) can only provide a partial characterization of the full effects of the minimum wage increase, since new entrants are also affected by the policy.

For both groups the excess jobs closely match the missing jobs (for incumbents $\Delta a = 1.3\%$ and $\Delta b = -1.2\%$ and for new entrants $\Delta a = 0.6\%$ and $\Delta b = -0.5\%$), so the net employment changes are approximately 0. The green and blue dashed lines (color version available online) show the running sums of employment changes up to the corresponding wage bin for each group. The lines show that in both cases there is little change in upper-tail employment. We note that if employers are replacing lower-skilled workers with higher-skilled ones, we should expect to see some reduction in jobs for previously employed workers, perhaps offset by high-skilled entrants; the lack of job loss for incumbents provides additional evidence against such labor-labor substitution. The affected wage increase for incumbents (9.5%, std. err. 2.0%) is significantly larger than it is for new entrants (1.9%, std. err. 1.3%) and some of these differences can be explained by the lack of spillover effects for the new entrants. In the next section we return to this issue.

III.B. Wage Spillovers

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

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interview period (the "outgoing rotation group"), respondents are asked questions about wages. Online Appendix E explains how we match workers across rotation groups.

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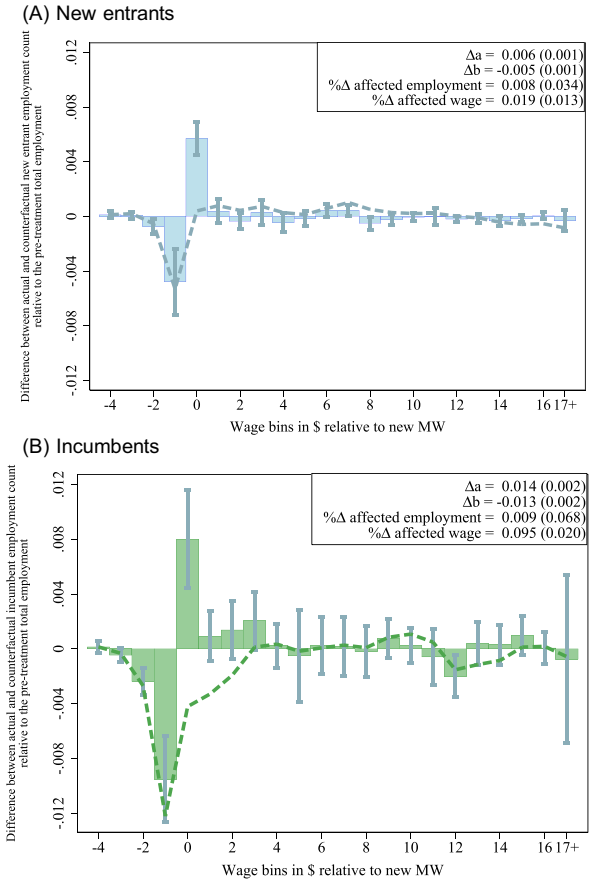

FIGURE IV

Impact of Minimum Wages on the Wage Distribution by Pretreatment Employment Status: New Entrants and Incumbents

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

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

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

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

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

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

In Table IV we also report estimates for several subgroups. The share of spillovers in the total wage increase is relatively similar for several key demographic groups, such as those without a high school degree (37.0%), teens (34.7%), those without a college degree (40.2%), and women (35.9%). In most cases, the spillover share is statistically significantly different from 0 at the 5% level. One exception is black or Hispanic individuals, for whom the estimated share of wage spillover is much smaller at 17.9% (std. err. 26.5%), which is less than half of the 39.7% (std. err. 11.9%) spillover share for all workers. Although the difference is not statistically significant, this finding nonetheless suggests that the

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TABLE IV
THE SIZE OF THE WAGE SPILLOVERS

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

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

wage gains at the bottom may be more muted for some disadvantaged groups.[29]

We also find a substantially smaller change in wages due to spillovers in the tradeable sector, though the estimates here are a bit imprecise. This highlights that wage effects are small

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

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in the tradeable sector. The combination of this evidence and the disemployment effects suggest that there may be more unintended consequences of minimum wages when the tradeable sector constitutes a more sizable share of the affected workforce.

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

First, the stark differences in the size and scope of spillovers for the incumbent and for the new entrants are inconsistent with a simple measurement error process common to both groups. This suggests that spillover effects found are likely to reflect real responses and not measurement error in CPS-based wages, a possibility that is raised by Autor, Manning, and Smith (2016).[30]

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

III.C. Event-Specific Estimates

So far, most of our evidence has come from averaging the effects across all 138 events. In this section, we estimate treatment

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

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effects for each of the events separately and assess how these impacts vary when we consider minimum wage increases that are more binding.

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

$$Y_{sjth} = \sum_{\tau=-3}^{4} \sum_{k=-4}^{4} \alpha_{\tau kh} I_{sjth}^{\tau k} + \mu_{sjh} + \rho_{jth} + \Omega_{sjth} + u_{sjth} \tag{4}$$

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

Figure V, Panel (A) shows the nonparametric bin-scattered relationship between the event-by-event estimates on missing jobs

31. Online Appendix Figure D.1 reports event-specific estimates for excess and missing jobs, and employment effects, along with (Ferman and Pinto, forthcoming) confidence intervals that are appropriate for a single treated unit and heteroskedasticity. Although there is considerable heterogeneity in the bite of the policy, the distribution of employment estimates is consistent with the sharp null of zero effect everywhere: only 5.3% of estimates are statistically significant at the 5% level. In addition, the stacked event-by-event estimates can be also used to estimate the average effect of the minimum wage across events. In Online Appendix Table D.1 we report estimates using that approach and show that estimates are very similar to our panel regression-based event study. This shows that issues about negative weighting using staggered treatments (e.g., Abraham and Sun 2018) are unlikely to be driving our results. Finally, the event-by-event estimates in Online Appendix Figure D.2 confirm that the lack of leading effects and upper-tail employment changes hold event by event, and not just on average: only 5.4% of the events experience statistically significant upper-tail effects at the 5% level, while 7.7% of the events experience statistically significant leading effects. For additional details, see Online Appendix D.

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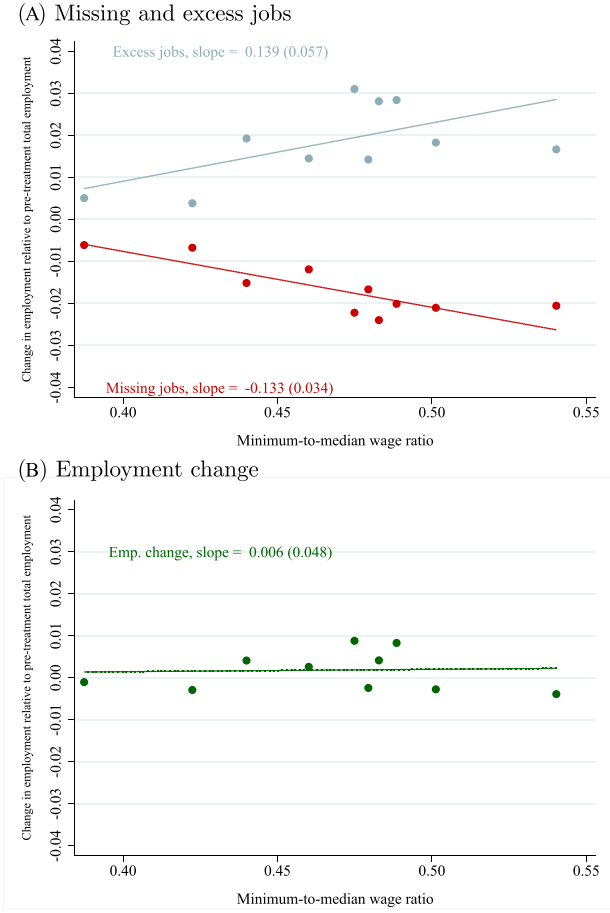

FIGURE V

Relationship between Excess Jobs, Missing Jobs, Employment Change, and the Minimum-to-Median Wage Ratio across Events

The figure shows the binned scatter plots for missing jobs, excess jobs, and total employment changes by the value of the minimum-to-median wage ratio (Kaitz index) for the 130 event-specific estimates. The 130 events exclude 8 minimum-wage-raising events in the District of Columbia, since individual treatment effects are very noisily estimated for those events. (See Online Appendix Figure A.10 for a raw scatterplot including the events in DC.) The minimum-to-median wage ratio is the new minimum wage *MW* divided by the median wage at the time of the minimum wage increase (Kaitz index). The bin-scatters and linear fits control for decade dummies, the state-specific unemployment rate at the time of the minimum wage increase, the urban share of the state's population, and an indicator for being a Republican-leaning state. Estimates are weighted by the state populations. The slope (and robust standard error in parentheses) is from the weighted linear fit of the outcome on the minimum-to-median wage ratio.

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

IV. Employment Changes along the Wage Distribution in the Classic Two-Way Fixed Effect Regression on Log Minimum Wage

In the previous section, we estimated the impact of minimum wages on the wage distribution using our event study specification. We found that the effect of the minimum wage was concentrated at the bottom of the wage distribution, and reassuringly we found no indication of considerable employment changes in the upper tail of the wage distribution (see Figure II). The lack of responses $4 above the minimum wage or higher also implies that the effect of the minimum wage on aggregate employment is close to the estimated employment effect at the bottom of the wage distribution. Such stability of upper-tail employment is consistent with the observation that the share of workers affected by the

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

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minimum wage changes we study is too small to affect upper-tail employment to a noticeable degree.

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

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

Figure VI shows that on average, minimum wage shocks are associated with large employment changes in the real dollar bins in the $6 to $9/hour range. There is a sharp decrease in

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

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

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

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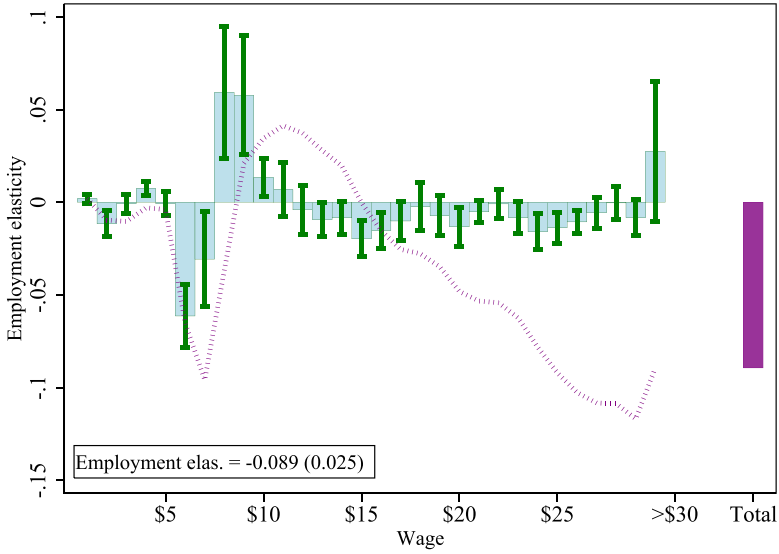

FIGURE VI

Impact on Employment throughout the Wage Distribution in the Two-Way Fixed Effects Model on Log Minimum Wages

The figure shows the effect of the minimum wage on the wage distribution in fixed effects (TWFE-logMW) specification. We estimate two-way (state and year) fixed effects regressions on the contemporaneous log minimum wage, as well as on four annual lags and two annual leads. For each wage bin we run a separate regression, where the outcome is the number of jobs per capita in that state-wage bin. The cumulative response for each event date 0, 1, ..., 4 is formed by successively adding the coefficients for the contemporaneous and lagged log minimum wages. The green histogram bars (color version available online) show the mean of these cumulative responses for event dates 0, 1, ..., 4, divided by the sample average employment-to-population rate—and represent the average elasticity of employment in each wage bin with respect to the minimum wage in the post-treatment period. The 95% confidence intervals around the point estimates are calculated using clustered standard errors at the state level. The dashed purple line plots the running sum of the employment effects of the minimum wage up until the particular wage bin. The rightmost solid purple bar is the elasticity of the overall state employment-to-population with respect to the minimum wage, obtained from regressions where the outcome variable is the state-level employment-to-population rate. In the bottom left corner we also report the point estimate on this elasticity with standard errors that are clustered at the state level. Regressions are weighted by state population. The figure highlights that large aggregate disemployment effects are often driven by shifts in employment at the upper tail of the wage distribution.

employment in the $6/hour and $7/hour bins, likely representing a reduction in jobs paying below new minimum wages, and a sharp rise in the number of jobs in the $8/hour and $9/hour wage bins, likely representing jobs paying above the new minimum. At

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the same time, the figure shows consistent, negative employment effects of the minimum wage for levels far above the minimum wage: indeed, the aggregate negative employment elasticity (e.g., −0.137) accrues almost entirely in wage bins exceeding $15/hour.

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

How is it possible that our benchmark specification that focuses on employment changes around the event window leads to such different results compared to the TWFE-logMW specification? The differences in the estimates suggest that the negative employment changes in the upper part of the wage distribution in TWFE-logMW must come from outside of the event window. Indeed, we find that the employment losses are driven by the 1980s expansion and the 1990–1991 recession, even though most of the minimum wage changes in our sample occurred after 2000. When we restrict the regression to the 1993–2016 period—the period where 86% ($\frac{118}{138}$) of all our events occurred—we indeed find very similar estimates across the two specifications: the TWFE-logMW specification in this sample suggests small employment effects and little upper-tail employment changes (Online Appendix Figure G.5). Moreover, even if we limit our sample to the 39 states with no state minimum wage increases until the late 1990s, the negative disemployment effects are driven entirely by the inclusion of a period (1980s) long before any cross-state variation in treatment occurred in these states (see Online Appendix Table G.5).

We also show in Online Appendix Figure G.4 that the overall employment-to-population rate evolved very similarly between the early 1990s and 2016 in high minimum wage states (those that instituted a minimum exceeding the federal standard after

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the early 1990s) and low minimum wage states (where the federal standard was always binding). This is noteworthy because the period between the early 1990s and 2016 is when much of the cross-sectional variation in minimum wages emerged. Importantly, however, the employment-to-population rate had diverged between these two groups of states during the 1980s, at least a decade before most high minimum wage states started to raise their minimum wage. This creates a spurious correlation between employment changes in the 1980s and minimum wage changes in the early 2000s that confounds the TWFE-logMW specification in the full sample, which is sensitive to shocks occurring long before the event window. However, this does not affect our event study estimates, since these only consider employment changes within the event window. This also explains why we do not find any pre-existing trends in our event-based analysis, while the TWFE-logMW in the full sample exhibits sizable and statistically significant leads. At the same time, TWFE-logMW estimates restricted to the post-1992 sample produce neither sizable leads nor sizable employment effects (see further details in Online Appendix G).[36]

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

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

37. Online Appendix G also uses these findings to provide a reconciliation of other longstanding discrepancies in the minimum wage literature. We show in Online Appendix Table G.7 that the expansion in the 1980s and the downturn dur-

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V. Discussion

In this article we infer the employment effects of the minimum wage from the change in the frequency distribution of wages. The key advantage of this approach is that it allows us to assess the overall impact of the minimum wage on low-wage workers, who are the primary target of minimum wage policies. We use an event study analysis exploiting 138 prominent minimum wage increases and provide a robust and comprehensive assessment of how minimum wages affect the frequency distribution of wages. Second, we calculate the number of missing jobs just below the minimum wage, the number of excess jobs at or slightly above the minimum wage, and the job changes in the upper tail of the wage distribution. Our main estimates show that the number of excess jobs at and slightly above the minimum wage closely matches the number of missing jobs just below the minimum wage, and we find no evidence for employment changes at or more than $4 above the minimum wage. A similar pattern obtains for low-skilled workers, suggesting labor-labor substitution is unlikely to be a factor in our setting. Moreover, we find that the level of the minimum wages that we study—which range between 37% and 59% of the median wage—have yet to reach a point where the job losses become sizable. However, the employment consequences of a minimum wage that surpass the ones studied here remain an open question. Furthermore, if minimum wage increases affect tradeable sectors more, our findings suggest employment effects may be more pronounced.

A key advantage of tracking job changes throughout the wage distribution is that we can transparently show the source of any disemployment effects. As a result, we can detect when an empirical specification suggests an unrealistic impact on the shape of the wage distribution. More important, the relationship between minimum wages and the wage distribution can also be used to infer the structure of low-wage labor markets. While providing a unified theoretical framework is beyond the scope of this article,

ing 1990–1991 is also responsible for the sensitivity of teen employment estimates to specification that has plagued the literature, such as controls for trends (e.g., Neumark, Salas, and Wascher 2014; Allegretto et al. 2017). In the post-1992 period, there is little divergence in teen elasticity across standard specifications (including the TWFE-logMW): they all suggest any losses to teen employment are small, with elasticities no more negative than −0.03. Use of trend controls also matters little in the post-1992 sample which is where most minimum wage variation is.

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our empirical results on the wage distribution together with the estimates on labor-labor substitution across demographic groups and the heterogeneous responses across sectors provide new empirical findings which can be used to test and distinguish various theories of the low-wage labor market.

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Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics*. Data and code replicating tables and figures in this article can be found in Cengiz et al. (2019), in the Harvard Dataverse, doi:10.7910/DVN/TJCTC7.

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