# State minimum wages, employment, and wage spillovers: Evidence from administrative payroll data*

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### Abstract

We use administrative payroll data to estimate the effect of the minimum wage on employment and wages. We find that both effects are nuanced. While the overall number of low-wage workers in firms declines, incumbent workers are no less likely to remain employed. We find that firms reduce employment primarily through hiring, and there is significant heterogeneity across the non-tradable and tradable sectors. For wages, we find modest spillovers extending up to $2.50 above the minimum wage. Spillovers accrue to both incumbent workers and new hires, but only within firms that employ a significant fraction of low-wage workers.



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# 1 Introduction

The effect of the minimum wage on employment and wages is an important policy question. Despite a large volume of research (Card and Krueger [1995]; Neumark and Wascher [2007]), several aspects of this question remain under-examined. One reason for this is data availability. Most studies lack longitudinal data on employee wage rates. This makes it difficult to quantify important dimensions of the wage effect, such as spillovers (Autor et al. [2016]). Alternatively, to improve data quality, other studies confine their analysis to a single employer or industry. However, imposing such restrictions may mask important sources of heterogeneity (Harasztosi and Lindner [2019]).

In this paper, we use precise administrative payroll data to examine the effects of the minimum wage on employment and wages. We find that both effects are nuanced. While the overall number of low-wage workers declines following a minimum wage increase, incumbent workers are no less likely to remain employed. We find that firms reduce employment primarily through hiring rather than through other channels. Moreover, we find evidence of significant heterogeneity across the non-tradable and tradable sectors. For wages, we find modest spillovers extending up to \$2.50 above the minimum wage. We find that spillovers accrue to both incumbent workers and new hires, but only within firms that employ a significant fraction of low-wage workers.

Our empirical analysis leverages administrative payroll data from Equifax Inc., one of the three major credit bureaus. The data contains anonymized information on the monthly earnings, hours, and job tenures of millions of employees from over 2,000 firms in the United States between the years 2010 and 2015. The data distinguishes between hourly and salary employees, voluntary and involuntary turnover, and specifies exact hourly wage rates. We are unaware of any other research that uses administrative payroll data of this quality and breadth to study the minimum wage.

To identify the effects of the minimum wage, we use a difference-in-differences framework that exploits state-level variation in the minimum wage over time. We focus on six large, recent state-level minimum wage increases ($\geq$ \$0.75 per hour) with well-defined pre-and-post intervention windows.[1]

[1] These are the minimum wage changes for which "clean" variation exists during our sample period – i.e., those that are not immediately preceded or followed by another minimum wage increase (Dube and Zipperer [2015]).

These increases occurred in California, Massachusetts, Michigan, Nebraska, South Dakota, and West Virginia during the years 2014 and 2015. For each treated state, we select a set of geographically adjacent control states which did not implement a minimum wage change between the years 2012 and 2015. We then restrict our final sample to border counties in treated and control states (Dube et al. [2010]). Our identification assumption is that, in the absence of a minimum wage change, economic conditions in adjacent cross-border counties would have evolved similarly. In support of this assumption, we show that treated and control counties are observably similar and trend in tandem prior to a minimum wage increase.

We estimate our difference-in-differences model at both the firm-county and the individual level. The firms in our sample are spread across multiple counties; we refer to a firm-county combination as an establishment.[2] While our establishment-level analysis estimates the effect of the minimum wage on the stock, flow, and composition of low-wage employees, our individual-level analysis estimates the effect on the wages and employment of incumbent low-wage workers. In both analyses, we restrict the sample period to the 24 months surrounding a minimum wage change.[3] We also require establishments to employ low-wage labor.[4]

We begin our analysis by estimating the effect of the minimum wage on incumbent wages. For directly affected incumbent workers earning less than the new minimum wage, we confirm that the minimum wage raises hourly wages in the expected manner. We then repeat the estimation across the entire hourly wage distribution to test for wage spillover effects. We find evidence of wage spillovers extending up to $2.50 above the new minimum wage. Within the "spillover range", hourly wages increase by $0.05 per hour, on average, and spillovers represent around 20 percent of the total labor cost increase among incumbents. We find that the average spillover effect masks

[2]Our definition of an establishment does not correspond to the Bureau of Labor Statistics definition of an establishment (i.e., a worksite). Our definition is much coarser. In general, we do not observe precise employee worksite locations in our data. We can only reliably measure locations at the three-digit ZIP code level or higher. We cannot compare our establishment data to population statistics from the Bureau of Labor Statistics.

[3]Sorkin [2015] shows that the "saw-toothed" nature of variation in the minimum wage may prevent reduced-form models from detecting any difference between short-run and long-run elasticities, even if such differences exist. Therefore, we focus on estimating short-run elasticities.

[4]We define low-wage labor at the establishment-level to be workers earning less than or equal to $10.00 per hour. Later in the analysis, we relax this definition and estimate separate employment effects within each wage bin. We only impose the restriction that establishments employ low-wage labor in the establishment-level analysis.

considerable heterogeneity. Incumbent workers with longer firm-specific tenures receive larger hourly wage increases. Moreover, wage spillovers only occur within firms that employ a significant fraction of directly affected workers. We find no evidence of wage spillovers in the upper tail of the hourly wage distribution. This serves as a falsification test for our setting (Cengiz et al. [2019]).

We also test whether wage spillovers accrue to newly hired employees. To do this, we examine changes in the wage distribution of new hires in the same job (e.g., cashier) at the same firm (e.g., Burger Inc.) across treated and control counties. We again find evidence of modest wage spillovers extending up to $2.50 above the new minimum wage. We estimate that spillovers account for at least 40 percent of total labor cost increases for new hires. Combined with our prior findings for incumbents, these results suggest that wage spillovers could be driven by a combination of search frictions, bargaining power, and relative pay concerns (Flinn [2006]; Flinn [2011]; Dube et al. [2019]).

We then shift our focus towards the employment effect of the minimum wage. For directly affected incumbent workers, we find no evidence that the minimum wage generates disemployment effects. We also find no significant increases in voluntary and involuntary turnover, or any decreases in average hours worked per week. However, at the establishment level, we find that total low-wage employment declines. Our estimate of the elasticity of low-wage employment to the minimum wage is -0.43, which is below the "old" consensus range of -0.3 to -0.1 (Brown et al. [1982]).[5]

To reconcile our individual and establishment-level results, we show that establishments reduce low-wage employment primarily through hiring. We find no significant changes in low-wage turnover, hours, or the number of locations following a minimum wage increase. For all of our employment variables, we find no evidence of differential pre-trends across treated and control counties. In addition, there are no significant employment responses in the upper tail of the wage distribution. These findings lend further credibility to our setting.

Theory suggests that the employment response to the minimum wage may vary across industries (Manning [2016]; Harasztosi and Lindner [2019]). In particular, firms in the tradable sector may

[5] This is not exactly an "apples to apples" comparison. First, our low-wage employment variable captures a more directly affected subset of the population than the outcome variable used in most other studies. Second, our sample is only comprised of establishments that employ low-wage labor (and not all establishments). We present results for all establishments in the robustness sections.

be more responsive to the minimum wage than firms in the non-tradable sector due to higher output demand elasticities. Consistent with this hypothesis, we find that reductions in hiring are concentrated in the tradable sector. We find no evidence of reduced low-wage hiring in the non-tradable sector. However, we do find that establishments in both the tradable and non-tradable sectors engage in low-wage labor-labor substitution.[6] It is important to note that, because our difference-in-differences estimates capture relative changes in employment, we cannot rule-out that labor reallocation within the tradable sector drives our results. However, our formal tests do not show evidence for significant labor reallocation.

Our paper makes several contributions to the literature on the minimum wage. First, our paper contributes to the debate on the effect of the minimum wage on wage inequality (DiNardo et al. [1996]; Lee [1999]; Autor et al. [2016]). A key point of contention in this debate is the magnitude of wage spillovers. We provide the first large-scale estimates of wage spillovers based on administrative payroll data.[7] Consistent with existing research by Brochu et al. [2019] and Cengiz et al. [2019], we find that wage spillovers extend up to around $2.50 above the new minimum wage. However, we quantify the precise size of spillovers in each wage bin, and we also document heterogeneity in the size of wage spillovers across employers. In particular, we provide some of the first evidence that spillovers only occur within firms that employ a significant fraction of low-wage employees. Thus, our results are consistent with Dube et al. [2019], which documents sizeable wage spillovers within a large retail firm in response to the 1996 and 1997 federal minimum wage increases. We also provide some of the first evidence of wage spillovers among newly hired employees.

Second, our paper contributes to the debate on the employment effects of the minimum wage. Recent research on this topic includes Dube et al. [2010], Giuliano [2013], Neumark et al. [2014], Dube et al. [2016], Meer and West [2016], Jardim et al. [2018], Cengiz et al. [2019], Clemens and

[6]Specifically, we find that establishments substitute from low-wage, low-skilled (as proxied by age) hires to low-wage, higher-skilled hires following an increase in the minimum wage.

[7]Most studies identify wage spillovers from rightward shifts in the reported wage distribution. However, several economic forces besides wage spillovers could shift the wage distribution rightward following a minimum wage increase (Autor et al. [2016]). Our incumbent results are not subject to these concerns because they are estimated using variation within the same employees over time. For our results on newly hired employees, we alleviate these concerns by conditioning on job titles and employers.

Wither [2019], Harasztosi and Lindner [2019], Monras [2019],and Powell [2019].[8] We contribute to this debate by providing the first large-scale estimates of the employment effect based on administrative payroll data. We also document evidence of labor-labor substitution among low-wage hires by examining changes in employee ages within the same establishment over time. Finally, we contribute to a growing body of evidence that indicates a non-tradable versus tradable and a new hire versus incumbent distinction in the employment effect of the minimum wage (Brochu and Green [2012]; Dube et al. [2016]; Harasztosi and Lindner [2019]; Cengiz et al. [2019]).

The remainder of the paper is organized as follows: section 2 provides background on our sample of minimum wage changes, section 3 discusses our data, section 4 examines wages, and sections 5 and 6 examine employment. Section 7 concludes and states several caveats that should be considered when interpreting our employment estimates.

# 2 Institutional background

In this section, we discuss our sample of minimum wage changes and our experimental setting.

## 2.1 State minimum wage changes

We begin by providing background on state minimum wage changes between January 2010 and December 2015 – i.e., the period covered by our administrative payroll data. Following the federal minimum wage increase to $7.25 per hour in July 2009, few states enacted statutory minimum wage changes. Of the changes between 2010 and 2013, the vast majority were indexed to inflation. In contrast, beginning in 2014, several states enacted new one-time or multi-phase minimum wage increases. Many of these increases were for large amounts. In particular, 13 states enacted 16 minimum wage increases of at least $0.75 per hour in 2014 and 2015.

Overall, 29 states enacted 75 minimum wage changes between January 2010 and December 2015. There were no increases to the federal minimum wage. Table IA.1, located in the internet appendix,

[8] Earlier work is surveyed in Card and Krueger [1995] and Neumark and Wascher [2007]. Recent work is surveyed in Clemens [2019]. Belman and Wolfson [Forthcoming] provide a meta-analysis of studies between 2000 and 2015.

provides a list of minimum wage changes during this period.

## 2.2 Selection of treated and control geographies

We focus our analysis on large and isolated changes to the minimum wage. Specifically, we restrict our sample to state-level minimum wage increases that: (1) were for at least \$0.75 per hour and (2) were neither preceded nor followed by any other minimum wage increase during the 24 months prior to and the 12 months after the implementation date (hereafter the treatment date). Imposing these conditions helps facilitate our analysis by keeping the before and after treatment periods free of other minimum wage changes. It also ensures that our changes are not dissipated by inflation. Six states in the continental U.S. (hereafter the treated states) enacted minimum wage changes that satisfy the above conditions; these states are California, Massachusetts, Michigan, Nebraska, South Dakota, and West Virginia. Table 1 summarizes our sample of minimum wage changes. The sample consists of two increases of \$0.75 per hour, three increases of \$1 per hour, and one increase of \$1.25 per hour. The employment-weighted average increase in the minimum wage is \$0.935 (12.06 percent). All increases occurred during the years 2014 and 2015.

We match each treated state to a set of adjacent control states which did not increase their minimum wage between 2012 and 2015. Furthermore, we follow Dube et al. [2010] and limit our final sample to border counties in treated and control states.[9] Table 1 lists the eleven control states and the six treated states. The rightmost columns of table 1 list the 78 control counties and 85 treated counties. Figure IA.1 displays the geographic distribution of our sample.

We assign each border county to a "cross-border county pair" that is comprised of adjacent treated and control counties.[10] Cross-border county pairs attempt to proxy for areas over which economic conditions evolve smoothly but where the level of the minimum wage varies discontinuously. While this approach has intuitive appeal, Neumark et al. [2014] question whether border

[9]Recent papers to use this strategy include Dube et al. [2016], Aaronson et al. [2018], Jardim et al. [2018], and Zhang [2018]. Our results persist if we conduct our analysis at the state level.

[10]In most cases, cross-border county pairs are comprised of one treated and one control county. However, in some cases, our pairs have more than two members. The results are not sensitive to how the cross-border pairing is done – e.g., the results hold if we assign all counties along the same border to the same pair.

counties serve as valid counterfactuals. To help alleviate these concerns, table IA.2 compares the economic conditions in treated and control counties prior to a minimum wage change. Along most observable dimensions, we find that treated and control counties are statistically similar.[11] Figures IA.2 and IA.3 show that treated and control counties trend in tandem prior to the treatment date. We find similar results at the state level (table IA.4 and figures IA.4 and IA.5).

# 3 Data and sample selection

In this section, we discuss our data and how we construct our sample.

## 3.1 Data

Our empirical analysis uses administrative payroll data from Equifax Inc., one of the three major credit bureaus. The data contains anonymized information on the monthly earnings, hours, and job tenures of employees from over 2,000 firms in the United States between the years 2010 and 2015. The data distinguishes between hourly and salary employees, voluntary and involuntary turnover, and specifies exact hourly wage rates. For a subset of employers, the data contains information on employee job titles at a level of aggregation comparable to the Standard Occupational Classification System. The data does not report precise employee worksite locations for the vast majority of firms. In most cases, we can only reliably identify locations at the three-digit ZIP code level or higher.

The internet data appendix provides more details about the data. We note that the data is representative of the United States population along several dimensions, including median personal incomes, median employee tenures, and the distribution of employment across states. In addition, most industries are represented in the correct proportions. However, the share of employment in the retail trade industry is significantly higher than in the population.

We use this data to examine the effects of the minimum wage at both the firm-county (hereafter establishment) and the individual level. The establishment-level analysis examines the effects of

[11] Table IA.3 displays these comparisons by treated states. We find that 2 out of the 15 variables that we analyze are significantly different for each of the following treated states: Massachusetts, South Dakota, and West Virginia.

the minimum wage on the stock and flow of employment. In contrast, the individual-level analysis focuses on workers employed prior to a minimum wage change. For both analyses, we restrict the sample period to the 24 months surrounding a minimum wage change. Therefore, our estimates capture short-run effects.

In terms of sample construction, we allow for entry and exit in the establishment-level analysis. However, we restrict entry in the individual-level analysis to the period prior to treatment. Employees are dropped from the sample after they separate from their employer. For both analyses, we set the pre-treatment period of a control county to be the same its paired treated county.[12] We discuss our samples in more detail below.

## 3.2 Individual-level sample

Our individual-level sample consists of hourly wage employees in treated and control counties. We categorize employees as either *bound employees* or a *non-bound employees* based on their pre-treatment hourly wage.[13] Bound employees have pre-treatment hourly wages below their state's "new minimum wage" – i.e., the level of the minimum wage after the state enacts its scheduled increase.[14] Non-bound employees earn at least the new minimum wage during the pre-treatment period. Table A.1, in the appendix, records our definitions.

Table IA.5 provides descriptive statistics for our sample of 87,011 bound employees. The median bound employee is 26 years old and earns $8.25 per hour as of the date they enter the sample. Thirty-four percent of bound employees earn exactly the minimum wage. Consistent with Giuliano [2013], we find that bound employees have high rates of turnover; on average, 54% separate from their employer within twelve months of their hiring date. Along most observable dimensions, bound

[12] As an example, consider counties along the border of West Virginia and Kentucky. In January 2015, West Virginia increased its minimum wage by $0.75. Therefore, the pre-treatment period for both West Virginia and Kentucky counties is January 2014 to December 2014. Flow is allowed into the sample prior to December 31, 2014.

[13] An employee's pre-treatment hourly wage is their wage in the month closest to three months prior to the treatment date (month $\tau = -3$ in event time). This definition accounts for flow into and out of the sample.

[14] For control counties, we define the new minimum wage as the minimum wage it would have if it had implemented the same increase as its paired treated county. As an example, consider counties along the border of West Virginia and Kentucky. In January 2015, West Virginia increased its minimum wage by $0.75. Therefore, the new minimum wage in these Kentucky counties is their current minimum wage plus $0.75.

employees in treated counties are statistically similar to their peers in control counties.

### 3.3 Establishment-level sample

Our establishment-level sample consists of firm-county combinations that employ low-skilled labor. We define *low-wage employees* as the total number of employees at an establishment that earn less than or equal to $10.00 per hour (Jardim et al. [2018]). We define *low-wage hires* similarly. We use low-wage employees and low-wage hires to measure the impact of the minimum wage on the stock and flow of low-skilled labor. Later in the analysis, we relax these definitions and examine the effects of the minimum wage across wage bins (à la Cengiz et al. [2019]).

For an establishment to be included in the sample, we require that low-wage employees account for at least 5% of the total workforce as of their initial sample date. Therefore, our estimates capture the effects of the minimum wage on low-wage (and not all) employers.[15] Table IA.6 provides descriptive statistics for the 1,964 establishments in our sample. The average establishment has 138 employees – 88% of which are paid hourly – as of six months prior to the treatment date. On average, low-wage employees comprise 52% of establishment employment and 29% of payroll. Establishments in treated counties are observably similarly to establishments in control counties.

The establishments in our sample represent 168 distinct firms; the median firm has an establishment in 16 (8) border counties (states). Our sample is concentrated in the retail trade, leisure, and hospitality industries. However, a significant number of establishments are in the manufacturing, professional services, education and health, and finance industries.

## 4 Wages

In this section, we examine the effects of the minimum wage on wages.

[15]We do not impose this restriction for the individual-level analysis. For example, if an establishment employed one low-wage worker and 1,000 high-wage workers, then this single low-wage worker would be included in the individual-level analysis. However, this establishment would be excluded from the establishment-level analysis. We present results on the full sample of establishments in the robustness section.

## 4.1 Baseline results

We begin our analysis by estimating wage responses for bound employees. The model is given by:

$$\omega_{i,t} = \alpha + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \delta_i + \delta_{p,t} + \eta' X_{s,q-1} + [\delta_{f,t} + \delta_{C,t} + \delta_{T,t}] + \varepsilon_{i,t}, \tag{1}$$

where $\omega_{i,t}$ is the hourly wage of bound employee $i$ in month $t$, $\delta_i$ are employee fixed effects, $\delta_{p,t}$ are cross-border county pair × month effects[16], and $X_{s,q-1}$ is a vector of lagged quarterly realizations of state-level HPI and GDP PC growth (Clemens and Wither [2019]). The dummy variable $\text{Treated}_s$ is equal to one if state $s$ implements an increase to its minimum wage, and zero otherwise. The dummy variable $\text{Post}_{t,s}$ is equal to one for all months $t$ after the treatment date in state $s$, and zero otherwise. In alternative specifications of the model, we include firm × month effects ($\delta_{f,t}$), sample cohort × month effects ($\delta_{C,t}$), and employee tenure × month effects ($\delta_{T,t}$).[17] Standard errors are clustered at the county level.[18]

The coefficient of interest, $\Gamma$, measures the average change in wages of bound employees in treated counties relative to adjacent control counties. Table IA.7 reports the coefficient estimates. We find that hourly wages increase by $0.486, on average, in the period following a minimum wage change. This is slightly higher than the weighted average wage gap between the new minimum wage and the pre-treatment wages of bound employees ($0.45). Figure 1 displays the dynamics of the coefficient estimates. We find that hourly wages increase within one month of the treatment date. In addition, we find no economically significant evidence of pre-trends.[19] Both the timing and the

[16] These are separate month fixed effects for each cross-border county pair $p$. These fixed effects account for time-varying shocks common to cross-border county pairs.

[17] Firm × month effects restrict the identifying variation to within-firm comparisons across treated and control counties. This allows us to control for time-varying firm and industry-level confounders. These fixed effects are important in our tests of employment and wage spillovers. To see why, consider a shock that boosts low-wage warehouse and shipping employment nationwide. Suppose that some states have more warehouse and shipping establishments than others (e.g., because of right-to-work laws), and that these states are more likely to respond to the shock by increasing the minimum wage. Because the exposure to the shock jumps discontinuously at the border, cross-border county pair × month effects would not adequately control for the effects of the shock. Adding firm × month effects would help resolve this problem.

[18] We do not cluster at the state-level out of concerns for the relatively small number of state clusters (17). We note, however, that our results are not sensitive to the choice of clustering. See the robustness sections.

[19] Two pre-period coefficient estimates are statistically significant.

magnitude of the wage response support the validity of our data and setting. The magnitude of the wage response also suggests that some bound employees receive raises in excess of the amount required to comply with the new minimum wage.

## 4.2 Wage spillovers for incumbents

In models with labor market frictions, an increase in the minimum wage can generate wage spillovers (Engbom and Moser [2017]). For example, relative pay concerns may cause firms to increase wages above the new minimum in order to maintain previous wage hierarchies (Dube et al. [2019]). The minimum wage may also raise the bargaining power of existing workers (Flinn [2011]). Our data allows us to estimate wage responses across the entire distribution of hourly wages. Hence, we can examine the magnitude of wage spillovers above the new minimum wage.

To test for wage spillovers, we begin by assigning incumbent employees to wage bins based on their pre-treatment hourly wages. We define the wage bins as follows. Bin $b = -1$ corresponds to exactly the "old" minimum wage. Bin $b = 0$ corresponds to the wage interval between the old minimum wage and the new minimum wage – i.e., the interval $(\mathrm{MW}_s, \mathrm{MW}_s + \Delta_s)$, where $\Delta_s$ is the size of the minimum wage increase (or hypothetical increase) in state $s$. Finally, bin $b \geq 1$ corresponds to the wage interval that is between $b$ and $b+1$ increments of size $\Delta_s$ above the old minimum wage: $[\mathrm{MW}_s + b \cdot \Delta_s,\ \mathrm{MW}_s + (b+1) \cdot \Delta_s)$. Intuitively, bins $b = -1$ and $b = 0$ correspond to bound employees while bins $b \geq 1$ correspond to non-bound employees. We cap the wage bins above at $b = 19$; the corresponding wage interval is $[\mathrm{MW}_s + 19 \cdot \Delta_s,\ \infty)$.[20]

Given our assignment of employees to wage bins, we then estimate the following model:

$$\begin{aligned}\omega_{i,b,t} = \alpha &+ \sum_{b'=-1}^{19} \Gamma_{b'} \times \mathrm{Treated}_s \times \mathrm{Post}_{t,s} \times \mathrm{Bin}_{b'} \\ &+ \delta_i + \delta_{p,b,t} + \eta_b' X_{s,q-1} + [\delta_{f,b,t} + \delta_{C,b,t} + \delta_{T,b,t}] + \varepsilon_{i,t}\end{aligned} \tag{2}$$

[20]Essentially, we are partitioning the pre-treatment wage distribution by increments equal to the scheduled minimum wage increase. For example, an employee assigned to bin $b = 1$ was earning just above the new minimum wage prior to treatment. The results are similar if we use \$1.00 wage bins instead.

where the dummy variable $\text{Bin}_{b'}$ is equal to one if employee $i$ is assigned to wage bin $b = b'$, and zero otherwise. For each bin $b$, the model includes a separate set of fixed effects and a different control variable coefficient. The coefficients of interest, the $\Gamma_{b'}$'s, measure the average relative change in wages for employees in each wage bin.

Panel A in figure 2 presents coefficient estimates. As expected, we find that hourly wages increase for bound employees in bins $b = -1$ and $b = 0$. More importantly, we find evidence of positive wage spillovers extending up to three wage bins – or, around \$2.50 — above the new minimum wage.[21] For employees in this "spillover region", average hourly wages increase by \$0.046 per hour (s.e. = \$0.016; table IA.8). We estimate that wage spillovers account for 20.5 percent of the total labor cost increase among incumbent employees (table IA.9; internet calculation appendix IC.1).[22]

Panel B repeats the estimation in terms of hourly wage elasticities. Consistent with our prior results, we find evidence of modest wage spillovers extending up to three wage bins above the new minimum wage. Our estimated hourly wage to minimum wage elasticity is 0.03 (s.e. = 0.01; table IA.8). We find no indication of wage spillovers in the upper tail of the hourly wage distribution. This serves as a falsification test for our setting.

Panel C tests for heterogeneity in the spillover effect across the share of affected workers in each firm.[23] This test is motivated by the argument that internal considerations may play a role in generating wage spillovers (Dube et al. [2019]). Consistent with the importance of relative pay concerns, we find that spillovers only occur in firms that employ a significant fraction of minimum wage workers. Moreover, the size of the spillover effect is increasing in the fraction of minimum wage workers at the firm. We estimate that a 10 percentage point increase in the fraction of minimum wage workers results in a \$0.036 increase in wage spillovers (table IA.11). We also find that the

[21] The corresponding bin intervals are: $[\text{MW}_s + \Delta_s, \text{MW}_s + 2 \cdot \Delta_s)$, $[\text{MW}_s + 2 \cdot \Delta_s, \text{MW}_s + 3 \cdot \Delta_s)$, and $[\text{MW}_s + 3\Delta_s, \text{MW}_s + 4\Delta_s))$. To conserve space, the figure normalizes the $x$-axis by subtracting $\text{MW}_s$.

[22] This is despite the fact that the average hourly wage spillover is roughly one-fifteenth the size of the direct effect on minimum wage workers. In our sample, there are more employees that are affected by the spillover effect than the direct effect. These employees also work more hours per week, on average.

[23] In this test, we restrict the sample to incumbent employees with wages in the spillover region. We then re-estimate equation 1 across sub-samples split by the pre-treatment fraction of minimum wage workers at the firm. The results are similar if we instead re-estimate the equation across sub-samples of establishments split by the pre-treatment fraction of minimum wage workers at the establishment.

magnitude of the spillover effect is greater for employees with longer firm-specific tenure and in the non-tradable sector (table IA.11). The latter is consistent with Cengiz et al. [2019].

### 4.3 Wage spillovers for new hires

We also explore whether wage spillovers accrue to newly hired employees.[24] Empirically, this task is more challenging because, by construction, we cannot observe starting wages for the *same* hires before and after a minimum wage increase. To circumvent this challenge, we follow the existing literature and estimate the effect of the minimum wage on the distribution of new hire wages (DiNardo et al. [1996]; Lee [1999]). The baseline model is given by:

$$\begin{aligned} Y_{c,b,t} = \alpha + \sum_{b'=-1}^{19} \Gamma_{b'} \times \text{Treated}_s \times \text{Post}_{t,s} \times \text{Bin}_{b'} \\ + \delta_{c,b} + \delta_{p,b,t} + \eta_b' X_{s,q-1} + \varepsilon_{c,b,t} \end{aligned} \tag{3}$$

where $\delta_{c,b}$ are county $\times$ wage bin effects and $\delta_{p,b,t}$ are cross-border county pair $\times$ wage bin $\times$ month effects. The outcome variable, $Y_{c,b,t}$, is the density of new hires in wage bin $b$ in county $c$ in month $t$.[25] The wage bins are defined as in section 4.2.

By focusing on the density, total labor demand is effectively held fixed. Therefore, the $\Gamma_{b'}$ coefficients measure the relative change in the composition of new hires across wage bins. Panel A in figure 3 displays the coefficient estimates. If wage spillovers accrue to newly hired employees, then the wage distribution should shift to the right beyond the new minimum wage (Brochu et al. [2019]). This is exactly what we find. There is a significant increase in the density of new hires for each wage bin within the spillover region.[26] As expected, we also find that the density of new hires decreases (increases) in the wage bins directly below (at) the new minimum wage.

[24]Search frictions and bargaining power may generate wage spillovers for newly hired employees (Flinn [2006]; Brochu et al. [2019]). Internal considerations may also influence job-to-job transitions (Dube et al. [2019]).

[25]This is the number of hires in bin $b$ in county $c$ in month $t$ divided by the total hires in county $c$ in month $t$.

[26]Cengiz et al. [2019] find that wage spillovers accrue to previously employed individuals but not to new labor market entrants. We do not believe their result is inconsistent with our findings. Our sample of new hires is comprised of both new entrants and previously employed individuals that undergo job-to-job transitions. It is possible that a large fraction of our new hires sample involves individuals who will be classified as previously employed individuals as per Cengiz et al. [2019].

As noted by Autor et al. [2016], several other economic forces besides wage spillovers may cause the wage distribution to shift to the right. This limits the conclusions that can be drawn from panel A. To isolate the effect of wage spillovers, we condition the wage distribution on the intersection of employers (e.g., Burger Inc.) and job titles (e.g., cashier). We then estimate the effect of the minimum wage on the conditional distributions of new hire wages. The model is given by:

$$\begin{aligned} Y_{c,f,j,b,t} = \alpha + \sum_{b'=-1}^{19} \Gamma_{b'} \times \text{Treated}_s \times \text{Post}_{t,s} \times \text{Bin}_{b'} \\ + \delta_{c,f,j,b} + \delta_{p,f,j,b,t} + \eta_b' X_{s,q-1} + \varepsilon_{c,b,t} \end{aligned} \tag{4}$$

where $f$ indexes firms and $j$ indexes job titles. The outcome variable, $Y_{c,f,j,b,t}$, is the density of new hires in job $j$ at firm $f$ in wage bin $b$ in county $c$ during month $t$.

By focusing on the conditional density, we implicitly control for labor substitution and disemployment effects. The $\Gamma_{b'}$ coefficients measure the relative change in the composition of new hires in the same job at the same firm across wage bins. Panel B displays the coefficient estimates. We continue to find a significant increase in the density of new hires throughout the spillover region. However, the coefficient estimates from the model are only marginally insignificant. We estimate that wage spillovers account at least 40.7 percent of the total labor cost increase among new hires (table IA.10; internet calculation appendix IC.2).[27] We find no significant changes in the density of new hires in the upper tail of the wage distribution. This helps support the validity of our setting.

Panel C tests for heterogeneity in the spillover effect across the share of affected workers in each firm. For this analysis, we restrict the the sample to new hires in wage bins $b = 2$ and $b = 3$.[28] We again find evidence that spillovers only occur within firms that employ a significant fraction of minimum wage workers.

[27] We combine our estimates for new hires and incumbents to measure the total importance of spillovers. We estimate that spillovers represent around 37.7 percent of labor cost increases due to the minimum wage during the first year (internet calculation appendix IC.3). For comparison, Cengiz et al. [2019] estimate that wage spillovers represent around 39.7 percent of total labor cost increases.

[28] Imposing this restriction prevents the effect from being mechanical. For firms with a higher share of affected workers, there will be a larger spike in the density of new hires in the bin right at the new minimum wage ($b = 1$) driven by compliance with the new law. Focusing on the wage bins above the new minimum wage more credibly isolates heterogeneity in the spillover effect.

# 5 Individual employment

In this section, we examine the effect of the minimum wage on incumbent employment.

## 5.1 Baseline results

We begin by estimating the effect of the minimum wage on bound employees. This group of workers is of direct policy interest (Neumark [2018]). The model is given by:

$$Y_{i,t} = \alpha + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \delta_i + \delta_{p,t} + \eta' X_{s,q-1} + [\delta_{f,t} + \delta_{C,t} + \delta_{T,t}] + \varepsilon_{i,t}, \tag{5}$$

where the outcome variable, $Y_{i,t}$, is either a dummy variable for the employment ($E_{i,t}$), voluntary turnover ($V_{i,t}$), involuntary turnover ($I_{i,t}$), or the natural logarithm of the average weekly hours worked ($H_{i,t}$) by employee $i$ in month $t$. The outcome variables are defined in the appendix (table A.2). Standard errors are clustered at the county level.

The coefficient of interest, $\Gamma$, measures the average change in the outcome variable for bound employees in treated counties relative to adjacent control counties. Table 2 reports the coefficient estimates. We find no significant changes in the likelihood of employment following a minimum wage increase. In fact, the coefficient estimate in column 2 suggests the likelihood of employment *increases* by 0.3 percent ($t = 1.45$). We find no indication of economically significant pre-trends across treated and control counties (figure IA.5). However, in the post-period, two of the coefficients are positive and significant at the 5 percent level.

In columns 3 and 4, we repeat the estimation with voluntary turnover as the outcome variable. We find no statistically or economically significant effects. Our results for involuntary turnover – reported in columns 5 and 6 – are sensitive to whether the bracketed fixed effects are included in the model. At worst though, we estimate that the likelihood of involuntary turnover decreases by 0.2 percent in response to the minimum wage ($t = -2.39$). We find that average weekly hours increase, but that the coefficient estimate is statistically insignificant ($\Gamma = 0.029$; $t = 1.62$). We find limited evidence of differential pre-trends for involuntary turnover, voluntary turnover, and average weekly hours (figure 4).[29]

To compare our results to the existing literature, we convert our coefficient estimates into elasticities (table IA.12). We estimate that the elasticity of incumbent employment to the minimum wage is 0.028 ($t = 1.40$). The own-wage elasticity of employment is 0.072 ($t$ =3.02). Closely related, we estimate that the own-wage elasticity of voluntary (involuntary) turnover is -0.059 (-0.005). Finally, the "implied labor demand" elasticity is 0.069. We emphasize that these estimates pertain to incumbent bound employees – i.e., those most directly affected by the minimum wage.

### 5.2 Stacked results

Our data also allows us to examine employment responses across the rest of the hourly wage distribution. To do this, we estimate the following stacked version of equation 5 across the wage bins defined in section 4:

$$\begin{aligned} Y_{i,b,t} = \alpha + \sum_{b'=-1}^{19} \Gamma_b \times \text{Treated}_s \times \text{Post}_{t,s} \times \text{Bin}_{b'} \\ \delta_i + \delta_{p,b,t} + \eta_b' X_{s,q-1} + [\delta_{f,b,t} + \delta_{C,b,t} + \delta_{T,b,t}] + \varepsilon_{i,t} \end{aligned} \tag{6}$$

where the dummy variable $\text{Bin}_b$ is equal to one if employee $i$'s pre-treatment wages fall within wage bin $b = b'$, and zero otherwise. Similar to before, the model includes a separate set of fixed effects and a different control variable coefficient for each wage bin. The coefficients of interest, the $\Gamma_{b'}$'s, measure the average relative change in the outcome variable for employees in each wage bin.

Panel A in figure 5 presents the coefficient estimates when the outcome variable is employment. As expected, we find no significant effect on the likelihood of employment in the wage bins pertaining to bound employees ($b = -1$ and $b = 0$). In addition, there are no employment responses in the spillover region or in the upper tails of the hourly wage distribution. The latter serves as a falsification test of our empirical setting (Cengiz et al. [2019]).

Panels B and C display the coefficient estimates when the outcome variable is turnover. We

[29]Approximately two pre-period coefficient estimates are statistically significant for each variable.

find a small, but statistically significant, decrease in voluntary (involuntary) turnover in wage bin $b = 0$ ($b = -1$). Across the rest of the hourly wage distribution, turnover responses are small and statistically insignificant.[30] Panel D repeats the estimation for average weekly hours. For employees earning exactly the minimum wage, we find a slight increase in average weekly hours. However, we find no significant effects on average weekly hours throughout the rest of the wage distribution.

### 5.3 Robustness

We conduct several robustness tests to supplement our individual-level employment analysis. A brief description of each test is provided below:

**Standard errors:** Table IA.13 reports standard errors using alternative clustering methods. We find the employment effect on bound employees remains statistically insignificant.

**Continuously measured treatment:** Table IA.14 redefines the treatment variable to be the difference between the new minimum wage and the pre-treatment hourly wage. For bound employees in control states, treatment is set equal equal to zero. After re-estimating the model, we continue to find no significant effects on employment among bound employees.

**Heterogeneity in treatment:** Table IA.15 examines heterogeneity across the non-tradable and tradable sectors. For bound employees in the non-tradable sector, we find a positive and statistically significant effect of the minimum wage on the likelihood of employment. In contrast, the effect is negative and statistically insignificant in the tradable sector. Table IA.16 (IA.17) examines heterogeneity across employee age (tenure) buckets. We find that teenage bound employees are slightly more likely to remain employed following a minimum wage increase. Higher tenured bound employees are slightly less likely to remain employed.

**Cross-county mobility:** Neumark [2018] notes that cross-border studies may be biased against finding disemployment effects because of spillovers from worker mobility. To examine whether a

[30] Bin $b = 17$ for voluntary turnover is an exception. Nevertheless, the coefficient estimate is economically small.

violation of the stable unit treatment value assumption is driving our results, we interact our difference-in-differences coefficient with the distance between adjacent county population centers. The idea is that workers are less likely to commute to a state with a higher minimum wage if the geographic distance to the state increases. Table IA.18 reports the results. We find that accounting for distance does not affect our conclusions.

**State-level results:** We repeat all of our tests at the state-level. Our results persist in this setting. The results are available from the authors upon request.

# 6 Establishment employment

In this section, we examine the effect of the minimum wage on establishments.

## 6.1 Baseline results

In response to a minimum wage increase, establishments may adjust employment through several channels. Some of these channels, such as hiring, would not be captured in our individual-level analysis. To examine the effect of the minimum wage on establishment employment, we begin by estimating the following model:

$$Y_{f,c,t} = \alpha + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \delta_{f,c} + \delta_{p,t} + \eta' X_{s,q-1} + [\delta_{f,t}] + \varepsilon_{f,c,t}, \tag{7}$$

where the pairs $f, c$ index establishments and $\delta_{f,c}$ are establishment fixed effects. The outcome variable, $Y_{f,c,t}$, is either the fraction of low-wage employees (scaled by lagged total employment), the natural logarithm of low-wage employees, or the natural logarithm of total employment. The outcome variables are defined in the appendix (table A.3). Standard errors are clustered at the county level.

The coefficient of interest, $\Gamma$, measures the average change in establishment outcomes in treated counties relative to adjacent control counties. If firm $\times$ month effects are included in the model, then the identifying variation is further restricted to within the same firm. Table 3 reports the coefficient estimates from the model. We find that the fraction of low-wage employees declines by 1.0 percentage points, on average, following a minimum wage increase ($t$=-2.74). Relative to the pre-treatment mean of 52 percentage points, this represents a 2.0 percent decline in the fraction of low-wage employees at establishments.

In columns 3 and 4, we repeat the estimation with the natural logarithm of low-wage employees as the outcome variable. We find that low-wage employees decline, on average, by 3.9% ($t = -2.09$). Our results for total employment – reported in columns 5 and 6 – are sensitive to whether firm × month effects are included in the model. We find that total employment declines by a statistically significant (insignificant) 1.0% (0.3%) when we include (exclude) firm × month effects.[31] For all the outcome variables, establishments in treated and control counties trend in a statistically similar manner prior to treatment (figure IA.6).

To compare our results to the existing literature, we convert our coefficient estimates into elasticities (table IA.19). The elasticity of low-wage establishment employment to the minimum wage is -0.43. This is below both the "old" consensus range of -0.3 to -0.1 (Brown et al. [1982]) and the "new" consensus range of -0.12 to -0.05 (Belman and Wolfson [Forthcoming]).[32] In models that include (exclude) firm × month effects, the elasticity of total establishment employment to the minimum wage is -0.09 (-0.03) and statistically insignificant. The implied low-wage labor demand elasticity is -1.04. Finally, the implied total labor demand elasticity is -0.38.[33]

Following Cengiz et al. [2019], we assess the plausibility of our estimates by disaggregating the total employment effect across wage bins. Specifically, we estimate the following stacked version of

[31]In general, our coefficient estimates are larger (in absolute terms) when we include firm × month effects in the model. This is consistent with minimum wage changes being correlated with positive industry or firm-specific shocks. Related to this observation, several papers argue that minimum wage increases tend to be enacted during "good times". This would exert a positive bias on the coefficient estimates and works against uncovering disemployment effects (Baskaya and Rubinstein [2012], Neumark et al. [2014], and Powell [2019]).

[32]Again, we note this is not exactly an apples-to-apples comparison (Neumark [2018]). See footnote 5.

[33]Our estimate of the elasticity of low-wage employment to the minimum wage is similar in magnitude to the estimate in Clemens and Wither [2019]. It is less negative than the estimate in Jardim et al. [2018]. Our estimate of the elasticity of total employment to the minimum wage is more negative than the estimate in Harasztosi and Lindner [2019]. However, it is well-above the critical -1 value (in absolute terms).

equation 7 across the wage bins defined in section 4.2:

$$
\begin{aligned}
Y_{f,c,b,t} = \alpha + \sum_{b=-1}^{19} \Gamma_b \times \text{Treated}_s \times \text{Post}_{t,s} \times \text{Bin}_b \\
+ \delta_{f,c,b} + \delta_{p,b,t} + \eta_b' X_{s,q-1} + [\delta_{f,b,t}] + \varepsilon_{f,c,b,t},
\end{aligned}
\tag{8}
$$

where the outcome variable, $Y_{f,c,b,t}$, is either the natural logarithm or the fraction of employees (scaled by initial employment) in wage bin $b$ at establishment $f, c$ in month $t$.[34]

Panels A and B in figure 6 present the coefficient estimates. As expected, we find that employment decreases (increases) in the wage bins directly below (at) the new minimum wage. We also find that employment increases by a modest amount in the first wage bin above the new minimum wage. However, the cumulative effect on employment remains negative; the cumulative effect is similar in magnitude to our prior regression estimates (panel B; blue line). In support of our setting, we find no evidence of employment effects in the upper tail of the wage distribution.

## 6.2 How do establishments reduce low-wage employment?

We now explore the channels through which establishments reduce employment. This analysis will help reconcile the decline in establishment employment with the null effects for incumbents. We focus on the channels of hiring, turnover, hours, and locations.

In columns 1 and 2 in table 4, we re-estimate equation 7 with the fraction of low-wage hires (scaled by lagged total employment) as the outcome variable. We find that the fraction of low-wage hires declines by 0.3 percentage points, on average, following a minimum wage increase ($t = -1.99$; 7.5 percent relative to the pre-treatment mean). Our estimate of the elasticity of low-wage hires to the minimum wage is -0.49 (table IA.19). We find no statistically significant evidence of differential pre-trends in low-wage hires at establishments (figure IA.6).

Panels C and D in figure 6 display coefficient estimates from the stacked model when the natural logarithm and fraction of low-wage hires are the outcome variables. We find that low-wage hiring

[34]We scale by initial total employment – and not current establishment employment – so that the coefficient estimates from our model do not mechanically sum to zero.

decreases (increases) in the wage bins directly below (at) the new minimum wage. The cumulative effect on low-wage hiring is negative (panel D; blue line). Again, we do not find any significant responses in the upper tail of the wage distribution.

In columns 3 through 8 of table 4, we re-estimate equation 7 with measures of low-wage turnover, average hours, and establishment locations as the outcome variables. We find no evidence that establishments reduce employment through any channels besides hiring. For all of the outcome variables, the coefficient estimates are economically and statistically insignificant.

Given the fact that terminating employees is costly, it is reasonable that establishments reduce employment strictly through hiring (Oi [1962]; Hamermesh [1987]). Slower rates of hiring can quickly reduce the stock of employment if employees have high voluntary turnover rates. In our setting, a back-of-the-envelope calculation suggests that establishments can reduce low-wage employment through hiring by around 5% within 12 months when turnover stays fixed.[35]

### 6.3 Which establishments reduce low-wage hiring?

Theory predicts that the effect of the minimum wage may differ across the non-tradable and tradable sectors.[36] To test this prediction, we classify firms into the non-tradable, tradable, and other goods sectors using the mapping in Mian and Sufi [2014]. We then estimate the following model:

$$\begin{aligned} Y_{f,c,t} = \alpha + \beta \times \text{Treated}_s \times \text{Post}_{t,s} \times \text{NT}_f + \Gamma{\times}\text{Treated}_s \times \text{Post}_{t,s} \\ + \delta_{f,c} + \delta_{p,t} + \delta_{f,t} + \eta' X_{s,q-1} + \varepsilon_{f,c,t} \end{aligned} \tag{9}$$

where the dummy variable $\text{NT}_f$ is equal to one if firm $f$ is in the non-tradable sector, and zero otherwise. The outcome variable, $Y_{f,c,t}$, is the fraction of low-wage hires in establishment $f, c$

[35]The back-of-the-envelope calculation is as follows. Low-wage employees account for 52% of the workforce of the average establishment in our sample. Holding control values constant, establishments reduce average low-wage hiring from 4% of total headcount per month to 3.7% per month after a minimum wage increase. Assuming voluntary turnover for low-wage employees (conditional) remains at around 7.5% per month, then low-wage employment declines by $(7.5\% \cdot 52\% - 3.7\%)/52\% = 0.38\%$ per month or 4.6% per year.

[36]In the non-tradable sector, competition is local. Any increase in the minimum wage affects all firms, and hence firms can raise prices and maintain output without suffering a competitive disadvantage. In contrast, competition is national (or global) in the tradable and manufacturing sectors. Affected firms cannot raise prices without suffering a competitive disadvantage and a fall in output. See Manning [2016] and Harasztosi and Lindner [2019].

in month $t$. The $\Gamma$ coefficient measures the average relative change in hiring for establishments in the tradable sector. The triple-differences coefficient, $\beta$, measures the differential impact on establishments in the non-tradable sector. Finally, $\beta + \Gamma$ measures the average relative change in hiring in the non-tradable sector.

Table 5 reports the coefficient estimates. We find that the fraction of low-wage hires declines by 0.6 percentage points, on average, for establishments in the tradable sector ($t = -2.79$). This is twice as large as the 0.03 percentage point decline in our baseline estimates. We find limited evidence that establishments in the non-tradable sector reduce low-wage hires. The triple-differences coefficient is positive ($\beta = -0.5$ percentage points) and the coefficient sum is statistically non-different from zero ($\beta + \Gamma = -0.1$ percentage points). Figure IA.7 displays the dynamics across sectors.

In columns 3 through 8, we re-estimate the model with low-wage turnover, average hours, and locations as the outcome variables. For establishments in both sectors, we find that the coefficient estimates are uniformly non-different from zero. The lack of a response for average hours alleviates the concern that non-tradable establishments reduce employment along the intensive margin.

Because our results are concentrated in the tradable sector, it is possible that our estimates simply reflect the reallocation of labor across state lines.[37] At some level, the null responses for low-wage turnover, average hours, and worksite locations cast doubt on this hypothesis (table 5). However, we also conduct two formal tests.

In our first test, we examine the hiring responses of establishments in border control counties as a function of their parent firm's exposure to the minimum wage increase in treated states (table IA.20).[38] For the full set of establishments in the tradable sector, we find a statistically insignificant positive association between hiring and firm-level exposure. In our second test, we examine the responses of establishments in border control counties relative to interior control counties (table

[37]Because of the nature of their businesses, firms in the tradable sector are more capable of reallocating labor to different locations in response to a local wage shock. Since our models identify the effects of the minimum wage by comparing responses of the same firm across different locations, we cannot separate relative declines (which could arise from reallocation) from absolute declines. Testing the reallocation hypothesis directly is notoriously difficult in this setting. At-best, we can only conduct indirect tests and falsification exercises.

[38]This test assumes the incentive to reallocate labor is stronger when firms face steeper wage-bill increases.

IA.21).[39] While the results indicate an increase in low-wage hiring in border counties, we find that the result is statistically insignificant in three-out-of-four models and there is no differential response in the tradable sector.[40] Overall, these results indicate that labor reallocation, if present, is unlikely to be significant.

### 6.4 Do establishments engage in low-wage labor-labor substitution?

The absence of a disemployment effect in the non-tradable sector may mask significant changes in the composition of low-wage workers (Giuliano [2013]). Specifically, non-tradable establishments may substitute from low-skilled, low-wage hires to higher-skilled, low-wage hires following an increase in the minimum wage. To test this prediction, we use age as a proxy for employee skill (Clemens et al. [2018]) and estimate the following stacked model:

$$\begin{aligned} Y_{f,c,a,t} = \alpha + \sum_{a' \in A} \Gamma_{a'} \times \text{Treated}_s \times \text{Post}_{t,s} \times \text{Age}_{a'} \\ + \delta_{f,c,a} + \delta_{p,a,t} + \eta_a' X_{s,q-1} + [\delta_{f,a,t}] + \varepsilon_{f,c,a,t}, \end{aligned} \tag{10}$$

where the outcome variable, $Y_{f,c,a,t}$, is the density of low-wage hires in age group $a$ (set to five year bin-widths) in establishment $f, c$ in month $t$.[41] The dummy variable $\text{Age}_{a'}$ is equal to one if age group $a$ is equal $a'$, and zero otherwise. The $\Gamma_{a'}$ coefficients measure the average relative changes in the composition of low-wage hires across age groups.

Panel A of figure 7 displays the coefficient estimates. There is a statistically significant decline in the share of teenage low-wage hires ($\Gamma_{[15,20)} = -4.15\%$; $t$=$-5.47$). Relative to the pre-treatment average share of teenage low-wage hires of 27 percent, this coefficient represents a 15 percent decline. We find that low-wage hiring shares increase along the rest of the age distribution. The greatest

[39] This test assumes that the cost of reallocating labor to an adjacent cross-border county is lower than reallocating further to the interior of the state.

[40] Note also there is a relative decline in the fraction of low-wage workers in the tradable sector in both tests.

[41] This is the number of low-wage hires in age group $a$ at establishment $f, c$ in month $t$ divided by the total number of low-wage hires at establishment $f, c$ in month $t$. We restrict the sample to establishment-month combinations with at least one hire.

increases occur in the young adult age groups ($[20, 25)$ and $[25, 30)$).[42]

Panels B and C re-estimate equation 10 across the non-tradable and tradable sectors. In both sectors, we find that the share of teenage low-wage hires declines. For the non-tradable sector, low-wage hiring shares increase the most most in the young adult age group and less-so in the older age groups. There is no discernible pattern to the increase in hiring shares in the tradable sector..[43]

## 6.5 Robustness

We conduct several robustness tests to supplement our establishment-level analysis. A brief description of each test is provided below:

**Standard errors:** Table IA.22 reports standard errors using a variety of alternative clustering methods. The decline in low-wage employment remains statistically significant in all cases.

**Full sample:** Table IA.23 eliminates the requirement that establishments must employ a significant fraction of low-wage employees. We find that low-wage employment and hiring continue to decline in the full sample. The effect of the minimum wage on total employment is negative but statistically insignificant.

**Continuously measured treatment:** Table IA.24 interacts the treatment variable with a measure of establishment exposure to the minimum wage. The exposure measure is equal to the fraction of employees earning less than the new (or hypothetical) minimum wage at the establishment as of the initial sample date. We find that the decline in low-wage employment is larger in establishments with greater pre-period exposure.

**State-level results:** We repeat all of our tests at the state-level. Our results persist in this setting. The results are available from the authors upon request.

[42]Only one of the coefficient estimates is statistically significant at the 95 percent level. However, a more coarse partitioning of the wage distribution (or a cumulative response) yields statistically significant results.

[43]Monras [2019] finds that the population of workers targeted by the minimum wage tends to leave or does not move to states that increase the minimum wage. This captures a feature of the extensive margin. In contrast, we focus on a feature of the intensive margin by estimating the effect on the distribution of new hire age (conditional on a hiring event).

# 7 Conclusion

In this paper, we use precise administrative payroll data to examine the effects of the minimum wage on employment and wages. We find that both effects are nuanced. While the overall number of low-wage workers declines following a minimum wage increase, incumbent workers are no less likely to remain employed. We find that firms reduce employment primarily through hiring rather than through other channels. Moreover, we find evidence of significant heterogeneity across the non-tradable and tradable sectors. For wages, we find modest spillovers extending up to $2.50 above the minimum wage. Spillovers accrue to both incumbent workers and new hires, but only within firms that employ a significant fraction of low-wage workers.

We note that our employment results should be interpreted with several caveats in mind. First, while our tests do not indicate significant evidence for labor reallocation, it could partly drive our establishment-level results within the tradable sector. Second, we only estimate the short-run effects of the minimum wage. Sorkin [2015] and Aaronson et al. [2018] show that long-run effects may be noticeably different than short-run effects if firms gradually adjusts towards capital and away from labor. Third, our employment results alone cannot speak to the total welfare effects of the minimum wage. For a comprehensive analysis of welfare, please see MaCurdy [2015].

Our administrative payroll data provides us with several advantages over other studies on the minimum wage. First, the data allows us to pinpoint the source of our effects within the wage distribution. As noted by Cengiz et al. [2019], this can be informative about both model validity and the structure of low-wage labor markets. Second, and perhaps more importantly, the data allows us to examine certain aspects of the minimum wage that have received less attention. Along with Dube et al. [2019] and Cengiz et al. [2019], we provide some of the first estimates of the magnitude of wage spillovers among incumbent and newly hired employees. Moreover, we can separate spillovers from other economic forces which may shift the wage distribution outward (Autor et al. [2016]). We also document significant heterogeneity in both the wage spillover and employment effect across employers and industries. Overall, our findings may be useful for distinguishing between theories of wage-setting and employment in low-wage labor markets.

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## Table 1: Descriptive statistics: state minimum wage changes

This table describes the state minimum wage changes studied in our analysis. There are a total of 6 treated states and 11 control states. The definition of treated and control states is provided in Section 2.2 of the text. The columns are defined as follows: MW $\Delta$ date refers to the year-month (YYYYMM) in which a treated state adjusts its minimum wage, BOP (EOP) MW refers to the state's minimum wage at the beginning (end) of the sample period, MW $\Delta$ amount is the size of the minimum wage change in the treated state, Control states refers to the control states for each treated state (* denotes states that are not used as a control state for the treated state in state-level robustness tests), # of border counties (T) refers to the number of counties in each treated state that border a county in a control state, and # of border counties (C) refers to the number of counties in the control states that border at least one county in their paired treated state. There are 163 total border counties in the analysis, 85 of which are from the treated states.

| **Treated state (1)** | **MW $\Delta$ date (2)** | **BOP MW (3)** | **EOP MW (4)** | **MW $\Delta$ amount (5)** | **Control states (6)** | **# of border counties (T) (7)** | **# of border counties (C) (8)** |
|---|---|---|---|---|---|---|---|
| CA | 201407 | 8.00 | 9.00 | 1.00 | (NV) | 10 | 8 |
| MA | 201501 | 8.00 | 9.00 | 1.00 | (NH) | 4 | 3 |
| MI | 201409 | 7.40 | 8.15 | 0.75 | (IN, WI) | 9 | 10 |
| NE | 201501 | 7.25 | 8.00 | 0.75 | (IA, KS, WY*) | 25 | 21 |
| SD | 201501 | 7.25 | 8.50 | 1.25 | (IA*, ND, WY) | 16 | 14 |
| WV | 201501 | 7.25 | 8.00 | 1.00 | (KY, PA, VA) | 21 | 22 |

## Table 2: Difference-in-differences regression: bound incumbent employment

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{i,t} = \alpha + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + [\delta_{f,t} + \delta_{C,t} + \delta_{T,t}] + \varepsilon_{i,t},$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are cross-border county pair × month fixed effects, $\delta_{f,t}$ are firm ×month fixed effects, $\delta_{C,t}$ are cohort × month fixed effects, $\delta_{T,t}$ are job tenure × month fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{i,t}$ , is either: (1) an indicator for employment ($E_{i,t}$), (2) an indicator for voluntary turnover ($V_{i,t}$), (3) an indicator for involuntary turnover ($I_{i,t}$), or (4) the natural logarithm of average hours per week ($H_{i,t}$) of individual $i$ in month $t$. All outcome variables are defined in the appendix. The sample is restricted to bound hourly wage employees. The variable Treated$_s$ is an indicator equal to one if state $s$ is treated, and Post$_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | $E_{i,t}$ **(1)** | $E_{i,t}$ **(2)** | $V_{i,t}$ **(3)** | $V_{i,t}$ **(4)** | $I_{i,t}$ **(5)** | $I_{i,t}$ **(6)** | $H_{i,t}$ **(7)** | $H_{i,t}$ **(8)** |
|---|---|---|---|---|---|---|---|---|
| Treated$_s$ × Post$_{t,s}$ | -0.003 | 0.003 | 0.002 | -0.001 | 0.000 | -0.002** | 0.019 | 0.029 |
| | (-0.60) | (1.45) | (0.76) | (-0.97) | (0.19) | (-2.39) | (1.53) | (1.62) |
| Individual FE | Y | Y | Y | Y | Y | Y | Y | Y |
| County pair × month FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Firm × month FE | | Y | | Y | | Y | | Y |
| Cohort × month FE | | Y | | Y | | Y | | Y |
| Tenure × month FE | | Y | | Y | | Y | | Y |
| Control variables | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 884,964 | 884,964 | 817,172 | 817,172 | 817,172 | 817,172 | 316,432 | 316,432 |
| $R^2$ | 0.32 | 0.40 | 0.31 | 0.37 | 0.33 | 0.37 | 0.89 | 0.91 |

## Table 3: Difference-in-differences regression: establishment employment

This table contains the coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{f,c,t} = \alpha + \delta_{f,c} + \delta_{p,t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + [\delta_{f,t}] + \varepsilon_{f,c,t}$$

where $\delta_{f,c}$ are firm-county (*establishment*) fixed effects, $\delta_{p,t}$ are cross-border county pair $\times$ month fixed effects, $\delta_{f,t}$ are firm $\times$month fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{f,c,t}$, is either a: (1) the fraction of low-wage employment to lagged total employment (*LowWage / Total*), (2) the logarithm of low-wage employment (*log(LowWage)*), or (3) the logarithm of total employment (*log(Total)*) at establishment $f, c$ in month $t$. The outcome variables are defined in the appendix. The sample is restricted to establishments with at least 5% low-wage employees as of their initial date of entering the sample. The variable Treated$_s$ is an indicator equal to one if state $s$ is treated, and Post$_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory** | LowWage/Total | | log(LowWage) | | log(Total) | |
|---|---|---|---|---|---|---|
| **Variables** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| Treated$_s$ $\times$ Post$_{t,s}$ | -0.010*** | -0.012*** | -0.039** | -0.059*** | -0.003 | -0.010* |
| | (-2.74) | (-3.53) | (-2.09) | (-3.31) | (-0.52) | (-1.65) |
| Firm $\times$ county FE | Y | Y | Y | Y | Y | Y |
| County pair $\times$ month FE | Y | Y | Y | Y | Y | Y |
| Firm $\times$ month FE | | Y | | Y | | Y |
| Control Variables | Y | Y | Y | Y | Y | Y |
| N | 38,172 | 38,172 | 39,929 | 39,929 | 39,929 | 39,929 |
| $R^2$ | 0.91 | 0.94 | 0.91 | 0.96 | 0.98 | 0.99 |

## Table 4: Difference-in-differences regression: how do establishments reduce employment?

This table contains the coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{f,c,t} = \alpha + \delta_{f,c} + \delta_{p,t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + [\delta_{f,t}] + \varepsilon_{f,c,t}$$

where $\delta_{f,c}$ are firm-county (*establishment*) fixed effects, $\delta_{p,t}$ are cross-border county pair $\times$ month fixed effects, $\delta_{f,t}$ are firm $\times$month fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{f,c,t}$, is either a: (1) the fraction of low-wage hires to lagged total employment (*LowWageHires / Total*), (2) the fraction of low-wage turnover to lagged total employment (*LowWageTurn / Total*), (3) the logarithm of three-digit ZIP code worksites (*log(Locations)*), or (4) the logarithm of average hours worked (*log(AverageHours)*) at establishment $f, c$ in month $t$. The outcome variables are defined in the appendix. The sample is restricted to establishments with at least 5% low-wage employees as of their initial date of entering the sample. The variable Treated$_s$ is an indicator equal to one if state $s$ is treated, and Post$_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory** | LowWageHires/Total | | LowWageTurn/Total | | log(Locations) | | log(AverageHours) | |
|---|---|---|---|---|---|---|---|---|
| **Variables** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| Treated$_s$ $\times$ Post$_{t,s}$ | -0.003** | -0.003* | -0.001 | 0.000 | 0.002 | 0.000 | 0.001 | 0.000 |
| | (-1.99) | (-1.86) | (-1.11) | (-0.17) | (0.71) | (0.05) | (0.18) | (-0.04) |
| Firm $\times$ county FE | Y | Y | Y | Y | Y | Y | Y | Y |
| County pair $\times$ month FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Firm $\times$ month FE | | Y | | Y | | Y | | Y |
| Control Variables | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 38,172 | 38,172 | 38,172 | 38,172 | 39,929 | 39,929 | 13,935 | 13,935 |
| $R^2$ | 0.21 | 0.33 | 0.33 | 0.54 | 0.97 | 0.97 | 0.96 | 0.97 |

## Table 5: Difference-in-differences regression: heterogeneity across industries

This table contains the coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{f,c,t} = \alpha + \beta \times \text{Treated}_\text{s} \times \text{Post}_{t,s} \times \text{NT}_f + \Gamma \times \text{Treated}_\text{s} \times \text{Post}_{t,s} + \delta_{f,c} + \delta_{p,t} + \eta' X_{s,t-1} + \delta_{I(f),t} + [\delta_{f,t}] + \varepsilon_{f,c,t}$$

where $\delta_{f,c}$ are firm-county (*establishment*) fixed effects, $\delta_{p,t}$ are cross-border county pair $\times$ month fixed effects, $\delta_{I(f),t}$ are non-tradable sector $\times$ month fixed effects, $\delta_{f,t}$ are firm $\times$month fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The variable $\text{NT}_{I(f)}$ is an indicator equal to one if firm $f$ is in the non-tradable sector, and zero otherwise. The outcome variable, $Y_{f,c,t}$, is either a: (1) the fraction of low wage hires to lagged total employment (*LowWageHires / Total*), (2) the fraction of low wage turnover to lagged total employment (*LowWageTurn / Total*), (3) the logarithm of three-digit ZIP code locations (*log(Locations)*), or (4) the logarithm of average hours worked (*log(AverageHours)*) at establishment $f, c$ in month $t$. The outcome variables are defined in the appendix. The sample is restricted to establishments with at least 5% low-wage employees as of their initial date of entering the sample. The variable $\text{Treated}_s$ is an indicator equal to one if state $s$ is treated, and $\text{Post}_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. The

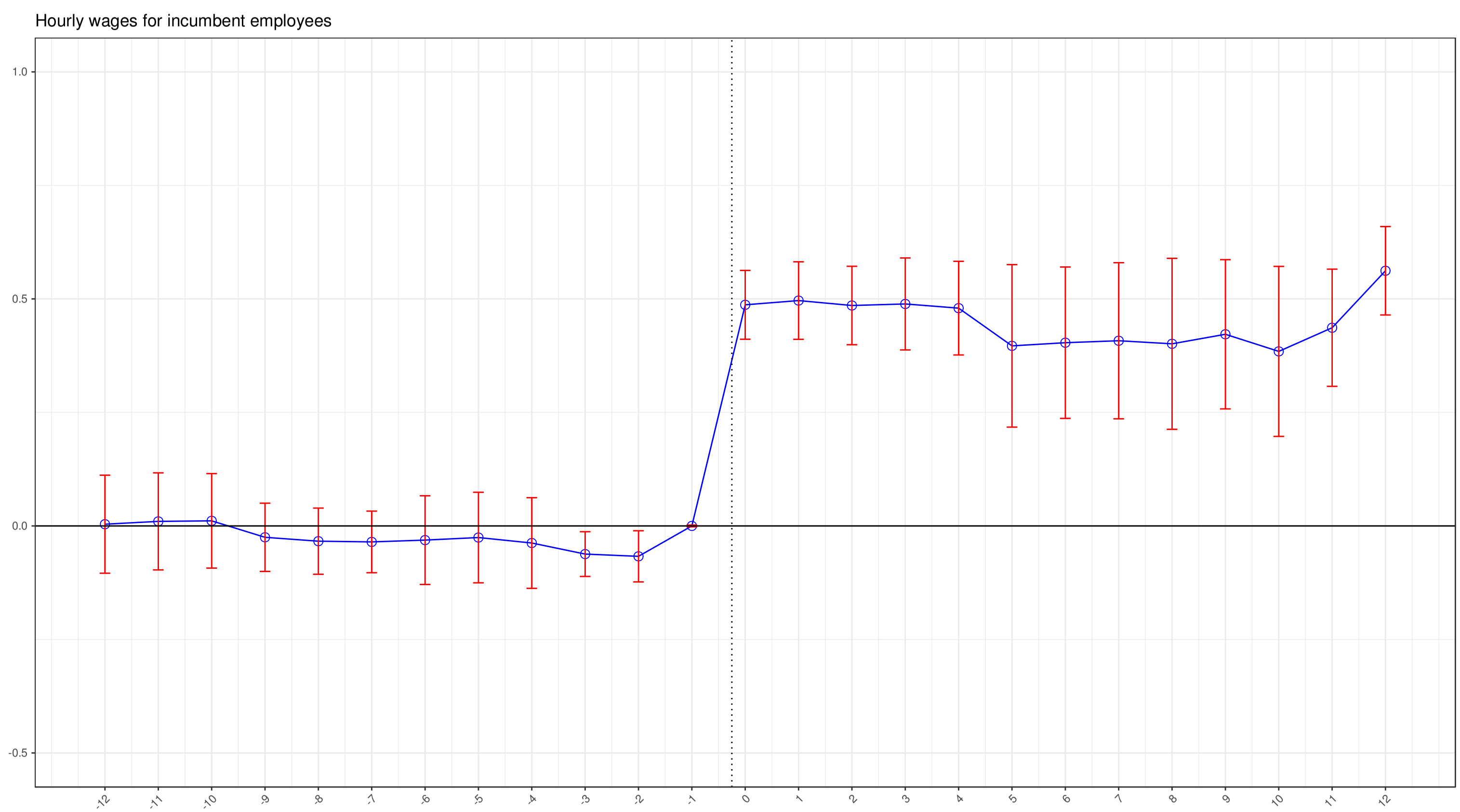
| **Explanatory Variables** | LowWageHires/Total **(1)** | **(2)** | LowWageTurn/Total **(3)** | **(4)** | log(Locations) **(5)** | **(6)** | log(AverageHours) **(7)** | **(8)** |
|---|---|---|---|---|---|---|---|---|
| $\text{Treated}_s \times \text{Post}_{t,s}$ | -0.006*** | -0.007*** | -0.001 | 0.000 | 0.000 | -0.002 | 0.001 | -0.001 |
| | (-2.79) | (-2.81) | (-0.41) | (-0.15) | (-0.04) | (-0.38) | (0.1) | (-0.19) |
| $\text{Treated}_s \times \text{Post}_{t,s}$ | 0.005 | 0.006 | 0.000 | 0.000 | 0.003 | 0.003 | 0.001 | 0.001 |
| $\times \text{NonTradable}_{I(f)}$ | (1.62) | (1.64) | (-0.15) | (0.08) | (0.58) | (0.51) | (0.1) | (0.17) |
| Firm $\times$ county FE | Y | Y | Y | Y | Y | Y | Y | Y |
| County pair $\times$ month FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Non-tradable $\times$ month FE | Y | | Y | | Y | | Y | |
| Firm $\times$ month FE | | Y | | Y | | Y | | Y |
| Control Variables | Y | Y | Y | Y | Y | Y | Y | Y |
| $F: \beta + \Gamma = 0$ | 0.46 | 0.56 | 0.31 | 0.93 | 0.38 | 0.77 | 0.79 | 0.99 |
| N | 38,148 | 38,148 | 38,148 | 38,148 | 39,903 | 39,903 | 13,934 | 13,934 |
| $R^2$ | 0.21 | 0.33 | 0.33 | 0.54 | 0.97 | 0.97 | 0.96 | 0.97 |

# Figure 1: Difference-in-differences regression: dynamics of wage responses

This figure plots coefficient estimates from the following dynamic difference-in-differences regression:

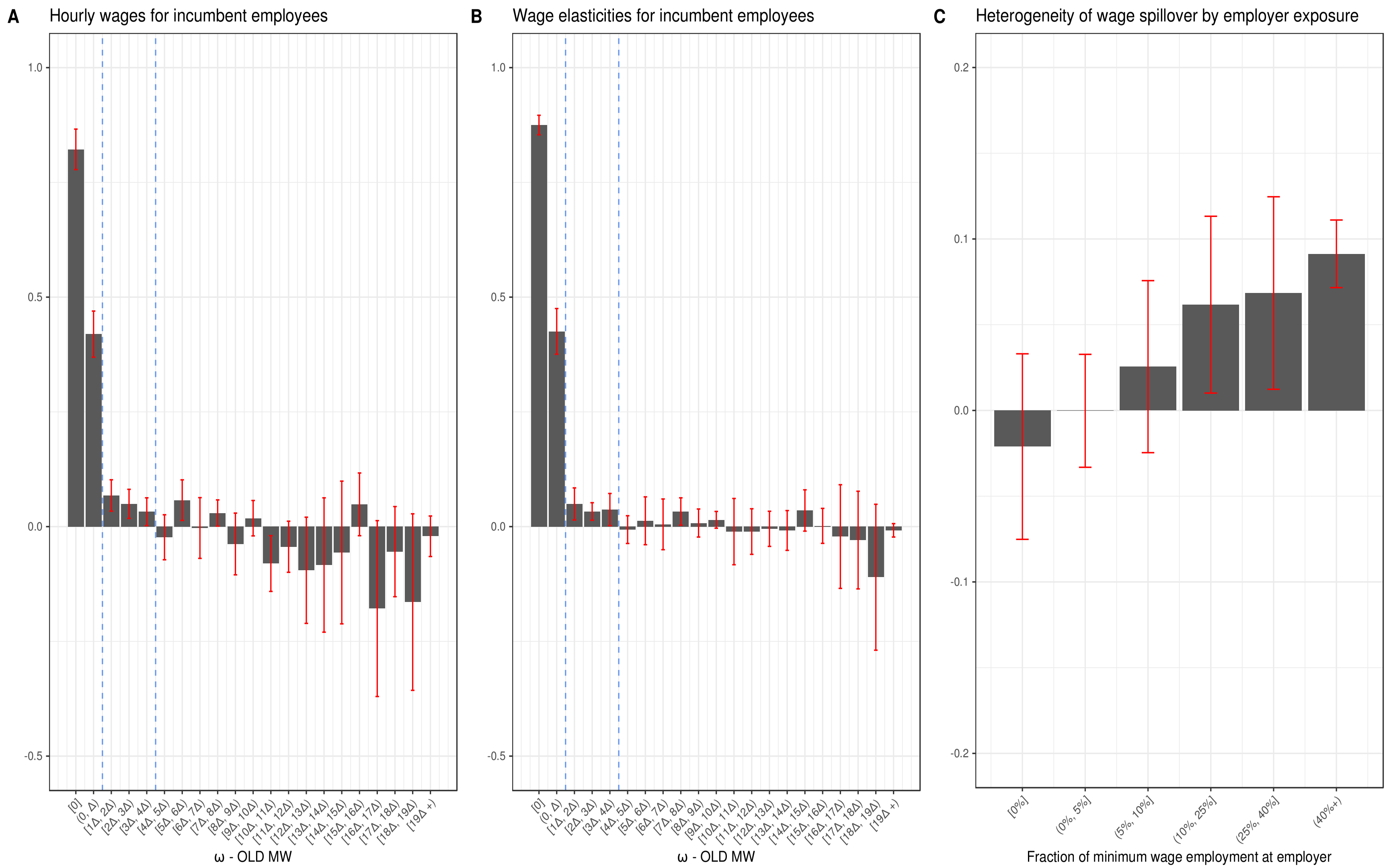
$$\omega_{i,t} = \alpha + \sum_{\tau=-12,\tau\neq-1}^{12} \Gamma_\tau \times \text{Treated}_s \times \text{D}(s,t,\tau) + \delta_i + \delta_{p,t} + \delta_{f,t} + \delta_{C,t} + \delta_{T,t} + \eta' X_{s,t-1} + \varepsilon_{i,t}$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair $\times$ month fixed effects, $\delta_{f,t}$ are firm $\times$ month fixed effects, $\delta_{C,t}$ are cohort $\times$ month fixed effects, $\delta_{T,t}$ are job tenure $\times$ month fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $\omega_{i,t}$, is the hourly wage of individual $i$ in month $t$. The sample is restricted to bound incumbent employees. The variable $\text{Treated}_s$ is an indicator equal to one if state $s$ is treated, and $D(s,t,\tau)$ is a dummy variable equal to one when month $t$ is $\tau$ months away a minimum wage increase in state $s$. The $x$-axis is the number of months ($\tau$) from a minimum wage increase. The month $\tau = -1$ is excluded as the reference level. The blue dots correspond to estimates of the $\Gamma_\tau$ coefficients. The vertical red bars correspond to 95% confidence intervals. Standard errors are clustered at the county level.



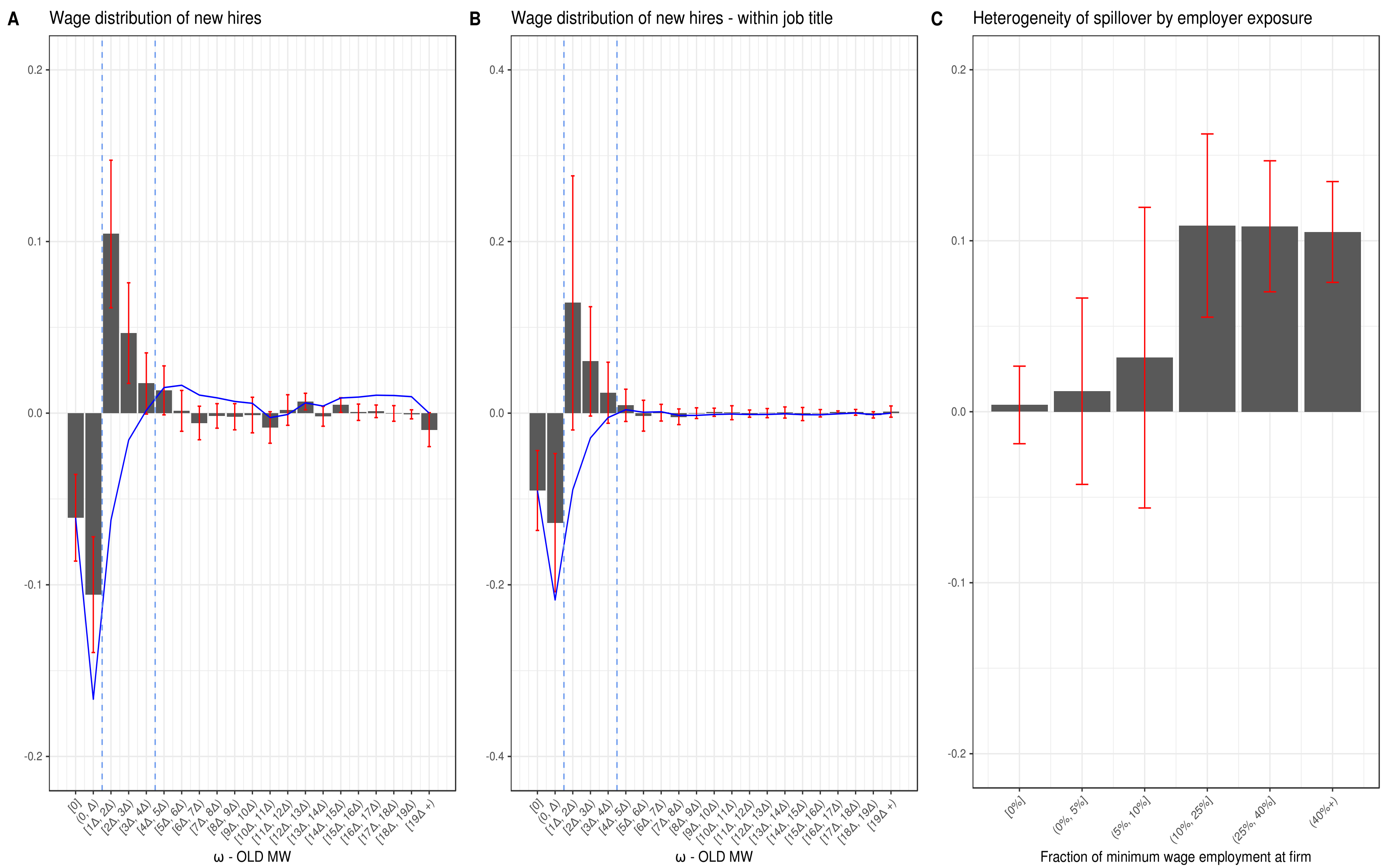
## Figure 2: Difference-in-differences regressions: incumbent wages

This figure plots coefficient estimates from variations of equation 2 in section 4. The gray bars correspond to coefficient estimates. The vertical red bars denote 95% confidence intervals, with standard errors clustered at the county level. In panels A and B, the $x$-axis corresponds to employee pre-treatment wage bins ($b = -1$ to $b = 19$). The left-most dashed blue vertical line corresponds to the new minimum wage. The interval between the first and second dashed blue vertical line corresponds to the “spillover region”. Panel C plots the heterogeneity in the difference-in-difference coefficient for employees in the “spillover region” across across firm exposure to a minimum wage increase.



# Figure 3: Difference-in-differences regressions: new hire wage densitities

This figure plots coefficient estimates from equations 3 and 4 in section 4. The gray bars correspond to coefficient estimates. The vertical red bars denote 95% confidence intervals, with standard errors clustered at the county level. The $x$-axis corresponds to new hire wage bins ($b = -1$ to $b = 19$). The left-most dashed blue vertical line corresponds to the new minimum wage. The interval between the first and second dashed blue vertical line corresponds to the "spillover region". The outcome variables in panel A, measured at the county $\times$ wage bin $\times$ month level, is hires scaled by total county hires. The outcome variable in panel B, measured at the establishment $\times$ job title $\times$ wage bin $\times$ month level, is hires scaled by total establishment-job title hires. The solid blue line corresponds to the cumulative sum of the coefficient estimates. Because the outcome variables are densities, this line should mechanically converge to zero. Panel C plots the heterogeneity of the difference-in-difference coefficient for employees in wage bins $b = 2$ and $b = 3$ across firm exposure to a minimum wage increase.

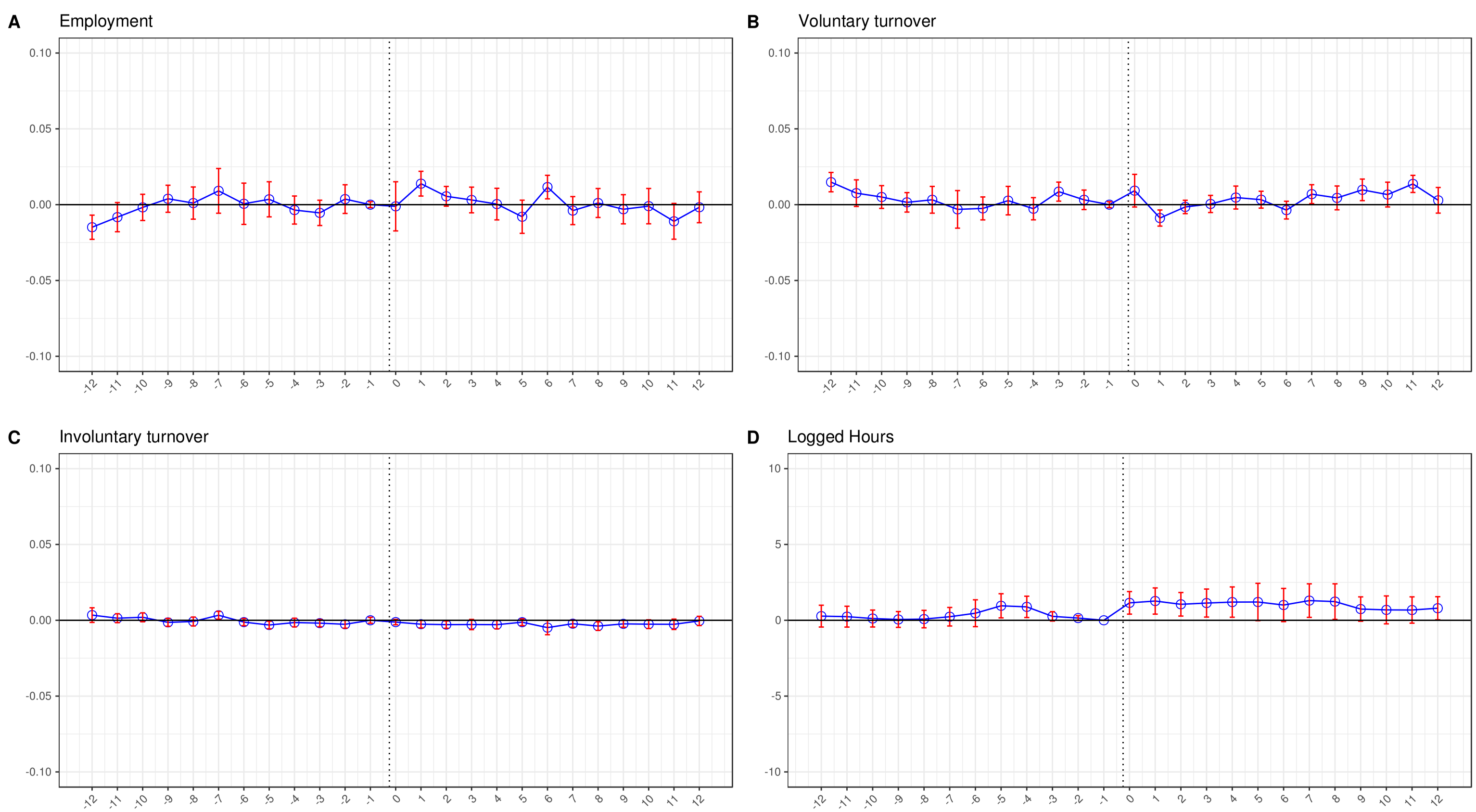


## Figure 4: Difference-in-differences regression: dynamics of bound employment

This figure plots the coefficient estimates from a dynamic difference-in-differences regression of the form:

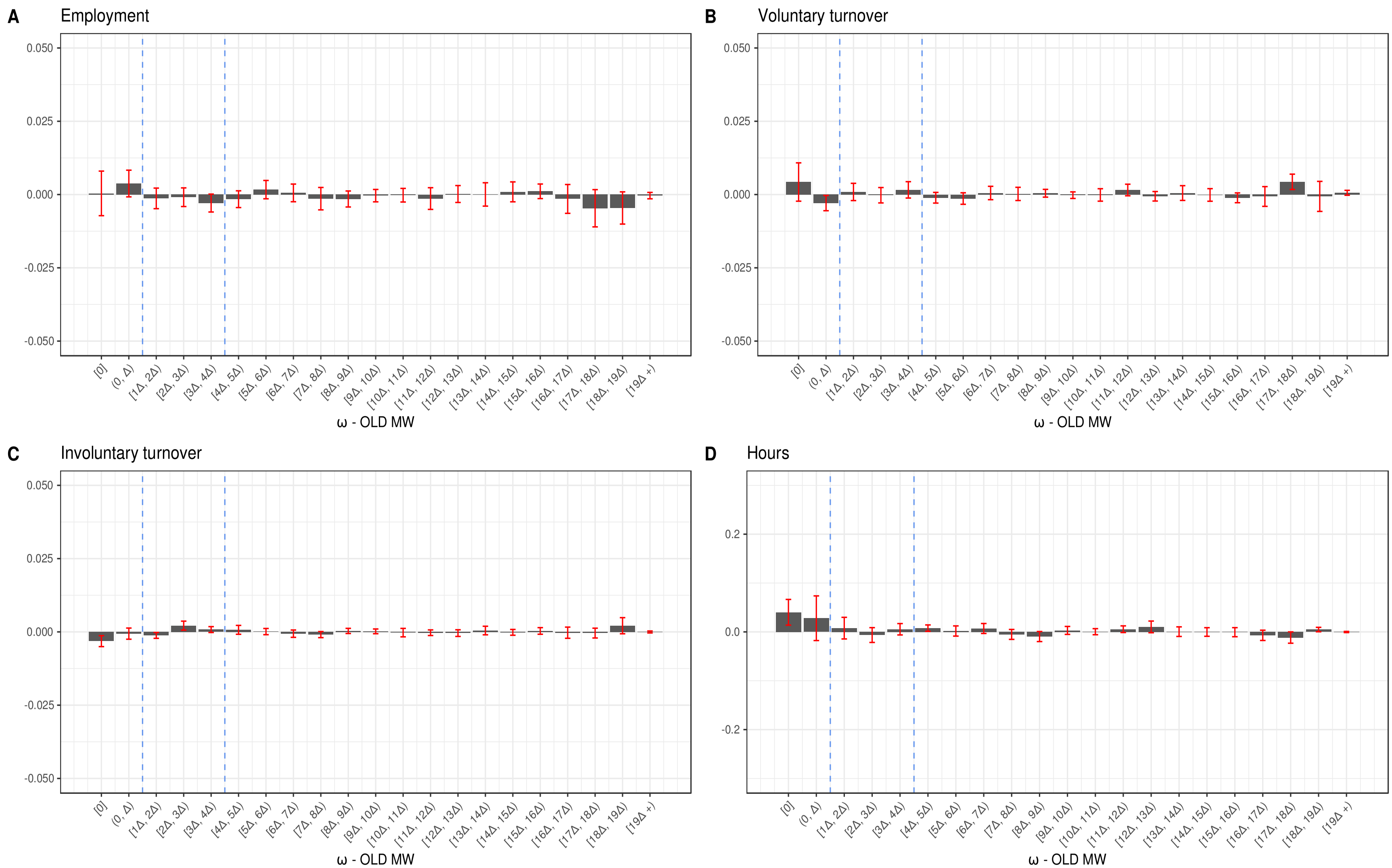
$$Y_{i,t} = \alpha + \sum_{\tau=-12,\tau\neq-1}^{12} \Gamma_\tau \times \text{Treated}_s \times \text{D}(s,t,\tau) + \delta_i + \delta_{p,t} + \delta_{f,t} + \delta_{C,t} + \delta_{T,t} + \eta' X_{s,t-1} + \varepsilon_{i,t}$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair $\times$ month fixed effects, $\delta_{f,t}$ are firm $\times$ month fixed effects, $\delta_{C,t}$ are cohort $\times$ month fixed effects, $\delta_{T,t}$ are job tenure $\times$ month fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{i,t}$, is either: (1) an indicator for employment ($E_{i,t}$), (2) an indicator for voluntary turnover ($V_{i,t}$), (3) an indicator for involuntary turnover ($I_{i,t}$), or (4) the natural logarithm of average hours per week ($H_{i,t}$) of individual $i$ in month $t$. The sample is restricted to bound incumbent employees. The variable $\text{Treated}_s$ is an indicator equal to one if state $s$ is treated, and $D(s,t,\tau)$ is a dummy variable equal to one when month $t$ is $\tau$ months away a minimum wage increase. The $x$-axis is the number of months ($\tau$) from a minimum wage increase. The month $\tau=-1$ is excluded as the reference level. The blue dots correspond to estimates of the $\Gamma_\tau$ coefficients. The vertical red bars correspond to 95% confidence intervals. Standard errors are clustered at the county level.



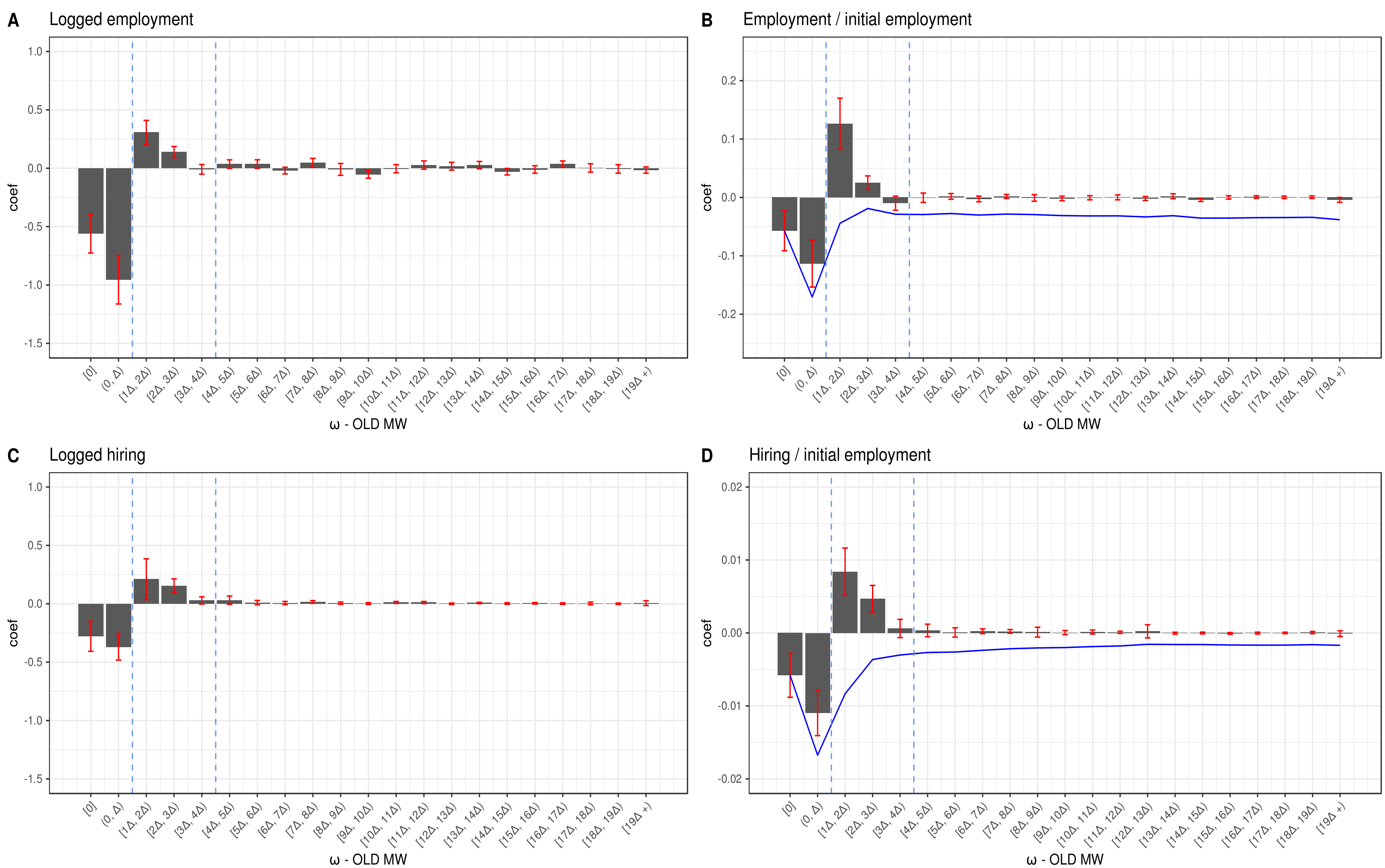
## Figure 5: Difference-in-differences regressions: incumbent employment

This figure plots coefficient estimates from variations of equation 6 in section 5. The gray bars correspond to coefficient estimates. The vertical red bars denote 95% confidence intervals, with standard errors clustered at the county level. The $x$-axis corresponds to employee pre-treatment wage bins ($b = -1$ to $b = 19$). The left-most dashed blue vertical line corresponds to the new minimum wage. The interval between the first and second dashed blue vertical line corresponds to the "spillover region". The outcome variables are employment($E_{i,t}$), voluntary turnover ($V_{i,t}$), involuntary turnover ($I_{i,t}$ ), and the natural logarithm of average hours worked ($H_{i,t}$) in Panels A, B, C, and D, respectively.



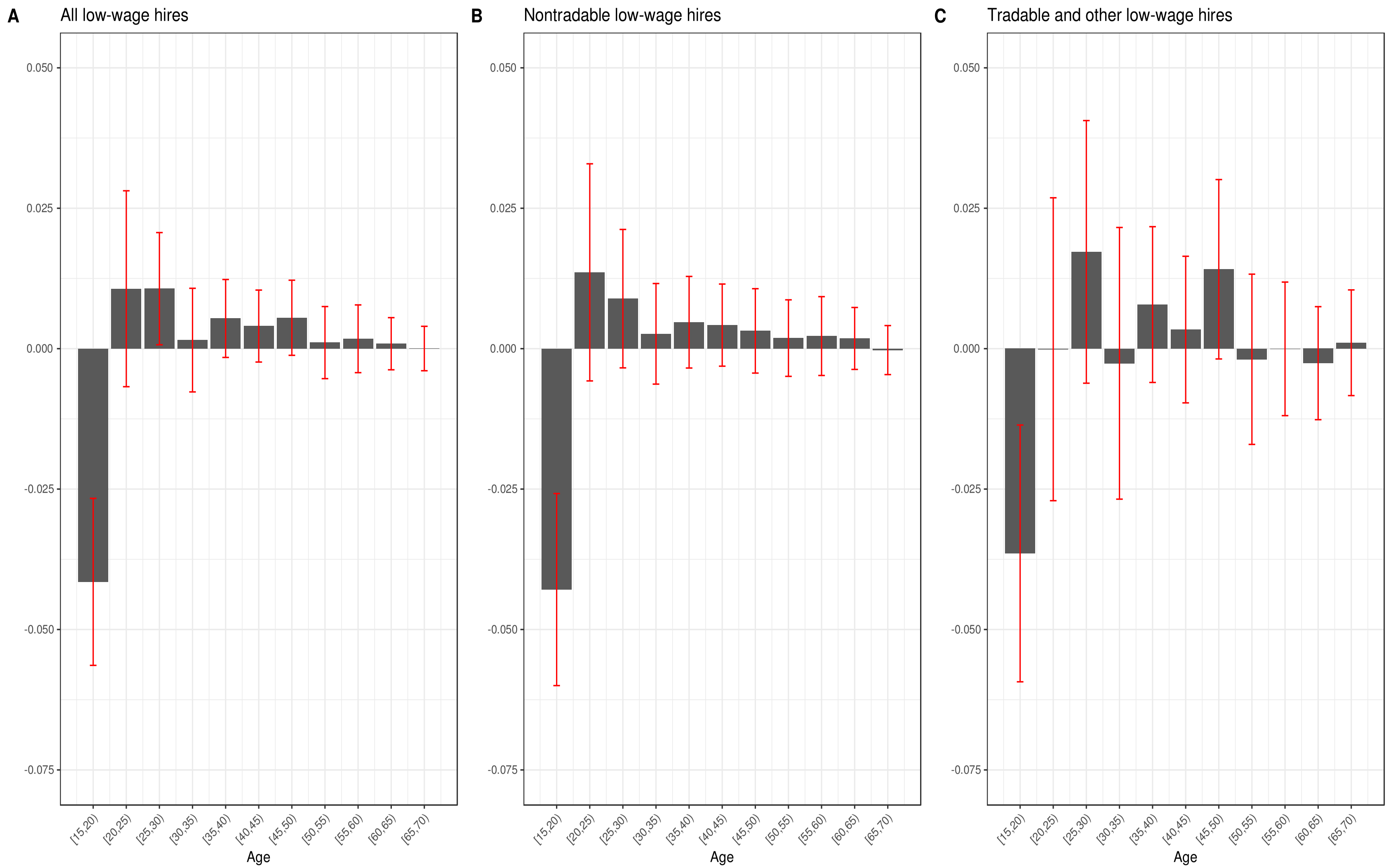
## Figure 6: Difference-in-differences regressions: establishment employment

This figure plots coefficient estimates from variations of equation 8 in section 6. The gray bars correspond to coefficient estimates. The vertical red bars denote 95% confidence intervals, with standard errors clustered at the county level. The $x$-axis corresponds to employee wage bins ($b = -1$ to $b = 19$). The left-most dashed blue vertical line corresponds to the new minimum wage. The interval between the first and second dashed blue vertical line corresponds to the “spillover region”. The outcome variables, measured at the establishment $\times$ wage bin $\times$ month level, are: logged employment (panel A), employment scaled by total establishment-level initial employment (panel B), logged hires (panel C), and hires scaled by total establishment-level initial employment. The sample is restricted to establishments with at least 5% low-wage employees as of their initial date of entering the sample. The solid blue line corresponds to the cumulative sum of the coefficient estimates.



## Figure 7: Difference-in-differences regressions: low-wage, new hire age densitities

This figure plots coefficient estimates from equation 10 in section 6. The gray bars correspond to coefficient estimates. The vertical red bars denote 95% confidence intervals, with standard errors clustered at the county level. The $x$- axis corresponds to employee age bins. The outcome variable, measured at the establishment $\times$ age bin $\times$ month level, is the low-wage hiring scaled by total establishment low-wage hiring in the month (i.e., the density). The sample is restricted to establishments with at least 5% low wage employees as of their initial date of entering the sample. Panel A estimates the model for all establishments in the sample. Panel B estimates the model for non-tradable establishments. Panel C estimates the model for tradable and other establishments.



# Appendix tables

List of appendix tables:

## Table A.1: Definition of employee subgroups

This table describes the employee subgroups used in our empirical analysis. The terms are defined as follows. $\omega_i$ is individual $i$'s hourly wage in the pre-treatment period, where pre-treatment period is defined as the time period immediately preceding a change in the minimum wage. For control border counties (states) which do not enact a minimum wage increase during the sample period, the pre-treatment period is equal to the pre-treatment period of their paired treated border county (state). The definition of treated and control states is given in Section 2.2. BOP $\text{MW}_s$ is the minimum wage of state $s$ in the pre-treatment period. NEW $\text{MW}_s$ is the new minimum wage after state $s$ enacts a minimum wage increase. For control states which do not enact minimum wage increases during the sample period, the term NEW $\text{MW}_s$ refers to the "counterfactual" minimum wage that state $s$ would have enacted if they adopted their paired treated state's minimum wage increase: $\text{NEW MW}_s = \text{BOP MW}_s + \Delta\text{MW}_s \;\; \forall s \in \text{Control states}$. $\omega_{i,t}$ is individual $i$'s hourly wage in month $t$. The column *Establishment or Individual level definition* indicates whether the definition applies for the individual or establishment level analyses.

| **Group name** | **Establishment or individual level definition** | **Description** | **Wage limits** |
|---|---|---|---|
| *Bound employees* | Individual | Employees making below the new minimum wage in the pre-treatment period. | $\omega_i < \text{NEW MW}_s$ |
| *Non-bound employees* | Individual | Employees making earning at least the new minimum wage in the pre-treatment period. | $\omega_i \geq \text{NEW MW}_s$ |
| *Spillover region* | Individual and establishment | The wage interval between the new minimum wage but less than the new minimum wage plus three times the actual or counterfactual minimum wage change. | $\omega_i \geq \text{NEW MW}_s$ and<br>$\omega_i \leq \text{NEW MW} + 3 \times \Delta\text{MW}_s$ |
| *Low wage employees* | Establishment | Employees making less than or equal to \$10 per hour (dynamic measure). Includes hourly and salary employees. The hourly wages of salaried employees is calculated by assuming a 40 hour work week. | $\omega_{i,t} \leq \$10$ |

## Table A.2: Variable definitions for individual-level analysis

This table provides definitions for the outcome variables in the individual-level analysis. These variables only pertain to hourly wage employees. Employees are removed from the sample the month after they separate from their job.

| **Outcome variable** | **Description** |
|---|---|
| $E_{i,t}$ | An indicator variable equal to one if employee $i$ remains employed in month $t$. |
| $V_{i,t}$ | An indicator variable equal to one if employee $i$ is voluntarily separated from their job in month $t$. If the separation cannot be mapped into a specific type of turnover (e.g., voluntary or involuntary), then this variable is left as null and the observation is excluded from the sample. Observations that remain employed until the end of the sample period are included in the sample. |
| $I_{i,t}$ | An indicator variable equal to one if employee $i$ is involuntarily separated from their job in month $t$. If the separation cannot be mapped into a specific type of turnover (e.g., voluntary or involuntary), then this variable is left as null and the observation is excluded from the sample. Observations that remain employed until the end of the sample period are included in the sample. |
| $H_{i,t}$ | The natural logarithm of average hours worked per week by employee $i$ in month $t$. If hours are not reported, then this variable is left as null and the observation is excluded from the sample. |
| $\omega_{i,t}$ | The hourly wage of employee $i$ in month $t$. |

## Table A.3: Variable definitions for establishment-level analysis

This table provides definitions for the outcome variables in the establishment-level analysis.

| **Outcome variable** | **Description** |
|---|---|
| $\text{LowWage}_{f,c,t}/\text{Total}_{f,c,t}$ | The total number of low-wage employees (earning $\leq$ \$10 per hour) divided by the lagged total headcount for establishment $f, c$ in month $t$. |
| $\log(\text{LowWage})_{f,c,t}$ | The natural logarithm of the number of low-wage employees (earning $\leq$ \$10 per hour) for establishment $f, c$ in month $t$. |
| $\log(\text{Total})_{f,c,t}$ | The natural logarithm of total headcount for establishment $f, c$ in month $t$. |
| $\text{LowWageHires}_{f,c,t}/\text{Total}_{f,c,t}$ | The total number of low-wage employees (earning $\leq$ \$10 per hour) hired at establishment $f, c$ in month $t$ divided by the lagged total headcount for establishment $f, c$ in month $t$. |
| $\text{LowWageTurn}_{f,c,t}/\text{Total}_{f,c,t}$ | The total number of low-wage employees (earning $\leq$ \$10 per hour) that separate from establishment $f, c$ in month $t$ divided by the lagged total headcount for establishment $f, c$ in month $t$. |
| $\log(\text{Locations})_{f,c,t}$ | The natural logarithm of distinct business locations for establishment $f, c$ in month $t$. Business locations are identified at the three-digit ZIP code level. The results are robust to the four-digit ZIP code level. |
| $\log(\text{AverageHours})_{f,c,t}$ | The natural logarithm of average weekly hours worked for employees at establishment $f, c$ in month $t$. If average weekly hours are not reported for an employee, then they are excluded from the calculation. |

# Internet Appendix - Not Intended for Publication

# Internet appendix tables

In this portion of the internet appendix, we provide supplemental tables to the main text.

## Table IA.1: Descriptive statistics: minimum wage changes by year

This table lists minimum wage changes between 2010 and 2017. Changes are aggregated to the yearly level. States without a minimum wage change are excluded from the table.

| State | 2010 (1) | 2011 (2) | 2012 (3) | 2013 (4) | 2014 (5) | 2015 (6) | 2016 (7) | 2017 (8) | BegMW (9) | EndMW (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| AK | 0.5 | 0 | 0 | 0 | 0 | 1 | 1 | 0.05 | 7.75 | 9.8 |
| AR | 0 | 0 | 0 | 0 | 0 | 0.25 | 0.5 | 0.5 | 7.25 | 8.5 |
| AZ | 0 | 0.1 | 0.3 | 0.15 | 0.1 | 0.15 | 0 | 1.95 | 7.25 | 10 |
| CA | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0.5 | 8 | 10.5 |
| CO | -0.03 | 0.11 | 0.28 | 0.14 | 0.22 | 0.23 | 0.08 | 0.99 | 7.25 | 9.3 |
| CT | 0.25 | 0 | 0 | 0 | 0.45 | 0.45 | 0.45 | 0 | 8.25 | 9.6 |
| DC | 0 | 0 | 0 | 0 | 1.25 | 1 | 1 | 0 | 8.25 | 11.5 |
| DE | 0 | 0 | 0 | 0 | 0.5 | 0.5 | 0 | 0 | 7.25 | 8.25 |
| FL | 0 | 0.06 | 0.36 | 0.12 | 0.14 | 0.12 | 0 | 0.05 | 7.25 | 8.1 |
| HI | 0 | 0 | 0 | 0 | 0 | 0.5 | 0.75 | 0.75 | 7.25 | 9.25 |
| IL | 0.25 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 8 | 8.25 |
| MA | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 1 | 8 | 11 |
| MD | 0 | 0 | 0 | 0 | 0 | 1 | 0.5 | 0.5 | 7.25 | 9.25 |
| ME | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1.5 | 7.5 | 9 |
| MI | 0 | 0 | 0 | 0 | 0.75 | 0 | 0.35 | 0.4 | 7.4 | 8.9 |
| MN | 0 | 0 | 0 | 0 | 0.75 | 1 | 0.5 | 0 | 7.25 | 9.5 |
| MO | 0 | 0 | 0 | 0.1 | 0.15 | 0.15 | 0 | 0.05 | 7.25 | 7.7 |
| MT | 0 | 0.1 | 0.3 | 0.15 | 0.1 | 0.15 | 0 | 0.1 | 7.25 | 8.15 |
| NE | 0 | 0 | 0 | 0 | 0 | 0.75 | 1 | 0 | 7.25 | 9 |
| NJ | 0 | 0 | 0 | 0 | 1 | 0.13 | 0 | 0.06 | 7.25 | 8.44 |
| NV | 0 | 0.7 | 0 | 0 | 0 | 0 | 0 | 0 | 7.55 | 8.25 |
| NY | 0 | 0 | 0 | 0 | 0.75 | 0.75 | 0.25 | 0.7 | 7.25 | 9.7 |
| OH | 0 | 0.1 | 0.3 | 0.15 | 0.1 | 0.15 | 0 | 0.05 | 7.3 | 8.15 |
| OR | 0 | 0.1 | 0.3 | 0.15 | 0.15 | 0.15 | 0.5 | 0.5 | 8.4 | 10.25 |
| RI | 0 | 0 | 0 | 0.35 | 0.25 | 1 | 0.6 | 0 | 7.4 | 9.6 |
| SD | 0 | 0 | 0 | 0 | 0 | 1.25 | 0.05 | 0.1 | 7.25 | 8.65 |
| VT | 0 | 0.09 | 0.31 | 0.14 | 0.13 | 0.42 | 0.45 | 0.4 | 8.06 | 10 |
| WA | 0 | 0.12 | 0.37 | 0.15 | 0.13 | 0.15 | 0 | 1.53 | 8.55 | 11 |
| WV | 0 | 0 | 0 | 0 | 0 | 0.75 | 0.75 | 0 | 7.25 | 8.75 |

## Table IA.2: Descriptive statistics: treated and control border counties

This table contains descriptive statistics on counties as of the quarter immediately preceding a minimum wage change. The sample is restricted to border counties in treated and control states. There are a total of 6 treated states and 11 control states. There are are a total of 163 border counties, 85 which are treated. The definition of treated and control states is provided in Section 2.2. The right-most columns are defined as follows: (1) T refers to the mean for treated counties, (2) C refers to the mean for control counties, and (3) $t$(DIFF) is the $t$ statistic for a test of difference in means between treated and control counties. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Variables** | **Mean (1)** | **SD (2)** | **P25 (3)** | **P50 (4)** | **P75 (5)** | **T (6)** | **C (7)** | **$t$(DIFF) (8)** |
|---|---|---|---|---|---|---|---|---|
| Population (1000's) | 88.4 | 266.7 | 5.7 | 16.3 | 48.27 | 98.4 | 77.5 | (0.50) |
| Unemployment rate | 5.03 | 2.32 | 3.10 | 4.40 | 6.75 | 5.10 | 4.94 | (0.46) |
| Employment | 32.8 | 104.1 | 1.4 | 4.1 | 17.1 | 36.0 | 29.4 | (0.41) |
| Number of QCEW establishments | 2.5 | 7.4 | 0.2 | 0.4 | 1.4 | 2.8 | 2.1 | (0.60) |
| Total hires | 7.8 | 22.0 | 0.5 | 1.2 | 3.8 | 8.4 | 7.2 | (0.31) |
| Total separations | 7.4 | 20.7 | 0.5 | 1.3 | 3.7 | 8.1 | 6.8 | (0.37) |
| Average weekly wage | 748 | 184 | 639 | 735 | 818 | 748 | 747 | (0.04) |
| % non-tradable | 0.31 | 0.18 | 0.21 | 0.29 | 0.39 | 0.29 | 0.32 | (-0.91) |
| % Tradable | 0.06 | 0.10 | 0.00 | 0.00 | 0.07 | 0.04 | 0.07 | (-1.50) |
| % Construction | 0.13 | 0.11 | 0.07 | 0.12 | 0.17 | 0.13 | 0.14 | (-0.22) |
| % Other | 0.47 | 0.20 | 0.38 | 0.50 | 0.58 | 0.48 | 0.46 | (0.62) |
| Age $\leq$ 35 employment fraction | 0.32 | 0.03 | 0.30 | 0.32 | 0.34 | 0.32 | 0.32 | (-0.19) |
| College educated employment fraction | 0.20 | 0.04 | 0.17 | 0.19 | 0.21 | 0.20 | 0.20 | (0.08) |
| $\leq$ \$10 employment / hourly employment | 0.34 | 0.16 | 0.25 | 0.33 | 0.44 | 0.34 | 0.35 | (-0.42) |
| $\leq$ \$20 employment / hourly employment | 0.78 | 0.17 | 0.73 | 0.82 | 0.88 | 0.78 | 0.78 | (0.09) |

## Table IA.3: Descriptive statistics: state-by-state county comparisons

This table contains descriptive statistics on counties as of the quarter immediately preceding a minimum wage change. The sample is restricted to border counties in treated and control states. There are a total of 6 treated states and 11 control states. There are are a total of 163 border counties, 85 which are treated. The definition of treated and control states is provided in Section 2.2. The columns correspond to treated states: CA, MA, MI, NE, SD, and WV. The interior cells correspond to $t-$statistics for tests of differences in means between border counties in the treated state and the adjacent cross-border control counties. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively..

| **Variables** | **CA (1)** | **MA (2)** | **MI (3)** | **NE (4)** | **SD (5)** | **WV (6)** |
|---|---|---|---|---|---|---|
| Population (1000's) | (-0.15) | (1.47) | (-0.86) | (0.81) | (1.11) | (-0.97) |
| Unemployment rate | (-0.01) | **(3.44)*** ** | (0.05) | (-0.93) | **(2.15)** ** | (1.16) |
| Employment | (-0.43) | (1.21) | (-1.36) | (0.87) | (1.09) | (-1.06) |
| Number of QCEW establishments | (-0.20) | (1.34) | (-1.05) | (0.81) | (1.12) | (-1.08) |
| Total hires | (-0.45) | (1.17) | (-1.47) | (0.79) | (1.29) | (-0.96) |
| Total separations | (-0.40) | (1.15) | (-1.59) | (0.80) | (1.32) | (-0.95) |
| Average weekly wage | (0.03) | (0.60) | (1.54) | (0.43) | (-0.04) | **(-2.49)** ** |
| % non-tradable | (-0.26) | (-1.54) | (-0.59) | (-1.25) | (-0.43) | (1.64) |
| % Tradable | (-1.41) | (0.01) | (0.17) | (-0.19) | (-0.93) | (-0.84) |
| % Construction | (-0.14) | (-1.47) | (-0.56) | (0.42) | (0.19) | (-1.61) |
| % Other | (0.75) | **(1.76)*** | (1.00) | (-0.83) | (-0.74) | (0.6) |
| Age $\leq$ 35 employment fraction | (1.09) | (-1.42) | (-0.82) | (1.08) | (0.96) | (-1.15) |
| College educated employment fraction | (-0.04) | (0.91) | (0.8) | (0.4) | (0.29) | **(-1.72)*** |
| $\leq$ \$10 employment / hourly employment | (0.11) | (0.90) | (0.00) | (-1.58) | **(2.53)** ** | (0.07) |
| $\leq$ \$20 employment / hourly employment | (-0.93) | (-0.41) | (-0.26) | (-0.74) | (1.40) | (0.59) |

## Table IA.4: Descriptive statistics: treated and control states

This table contains descriptive statistics on states as of the quarter immediately preceding a minimum wage change. There are a total of 6 treated states and 11 control states. The definition of treated and control states is provided in Section 2.2. The right-most columns are defined as follows: (1) T refers to the mean for treated states, (2) C refers to the mean for control states, and (3) $t$(DIFF) is the $t$ statistic for a test of difference in means between treated and control states. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| Variables | Mean (1) | SD (2) | P25 (3) | P50 (4) | P75 (5) | T (6) | C (7) | $t$(DIFF) (8) |
|---|---|---|---|---|---|---|---|---|
| Population (mm) | 6.43 | 9.03 | 1.85 | 3.10 | 6.76 | 9.99 | 4.49 | (1.22) |
| Population growth | 0.01 | 0.01 | 0.00 | 0.00 | 0.01 | 0.01 | 0.01 | (-0.36) |
| Unemployment rate | 5.10 | 1.59 | 3.90 | 5.20 | 6.00 | 5.48 | 4.89 | (0.72) |
| Average weekly earnings | 807 | 83 | 752 | 797 | 817 | 821 | 799 | (0.50) |
| Average hourly wages | 23.40 | 2.49 | 21.74 | 23.11 | 24.41 | 23.93 | 23.11 | (0.64) |
| GDP PC | 49.71 | 8.98 | 44.59 | 49.16 | 53.11 | 49.25 | 49.96 | (-0.15) |
| GDP PC growth | 0.01 | 0.01 | 0.01 | 0.01 | 0.02 | 0.01 | 0.01 | (0.2) |
| HPI growth | 0.05 | 0.03 | 0.03 | 0.04 | 0.04 | 0.06 | 0.05 | (0.65) |
| BOP MW | 7.41 | 0.33 | 7.25 | 7.25 | 7.25 | 7.53 | 7.34 | (1.11) |
| $\leq$ \$10 employment / hourly employment | 0.32 | 0.06 | 0.27 | 0.33 | 0.35 | 0.34 | 0.31 | (0.97) |
| $\leq$ \$20 employment / hourly employment | 0.77 | 0.06 | 0.75 | 0.77 | 0.80 | 0.78 | 0.77 | (0.44) |

## Table IA.5: Descriptive statistics: bound employees in border counties

This table contains descriptive statistics for our sample of *Bound employees* in treated or control border counties. The definition of treated and control states is provided in Section 2.2. There are 87,011 *Bound employees* (34% of which are *Minimum wage employees*). The definition of *Bound employees* is provided in Section 3. *Hourly wage, Weekly hours, Age,* and *Beginning tenure* are measured as of the date the employee enters the sample. *End tenure* and the turnover variables are measured as of the end of the sample period. The right-most columns are defined as follows: (1) T refers to the mean for treated counties, (2) C refers to the mean for control counties, and (3) $t$(DIFF) is the $t$ statistic for a test of difference in means between treated and control counties. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Variables** | **Mean (1)** | **SD (2)** | **P25 (3)** | **P50 (4)** | **P75 (5)** | **T (6)** | **C (7)** | **$t$(DIFF) (8)** |
|---|---|---|---|---|---|---|---|---|
| Hourly wage | 8.18 | 0.52 | 8.00 | 8.25 | 8.50 | 8.19 | 8.17 | (0.08) |
| Weekly hours | 27.61 | 10.72 | 19.00 | 26.00 | 40.00 | 26.34 | 29.11 | (-0.6) |
| Age | 31.45 | 13.55 | 21.00 | 26.00 | 39.00 | 29.84 | 33.11 | **(-3.47)** |
| Beginning tenure (months) | 11.22 | 15.36 | 1.00 | 5.00 | 14.00 | 9.87 | 12.75 | (-1.00) |
| End tenure (months) | 16.91 | 17.27 | 3.00 | 10.00 | 25.00 | 15.25 | 18.78 | (-0.96) |
| $1\{\text{Turnover} \leq 3 \text{ months?}\}$ | 0.26 | 0.44 | 0.00 | 0.00 | 1.00 | 0.27 | 0.25 | (0.39) |
| $1\{\text{Turnover} \leq 6 \text{ months?}\}$ | 0.39 | 0.49 | 0.00 | 0.00 | 1.00 | 0.41 | 0.38 | (0.43) |
| $1\{\text{Turnover} \leq 12 \text{ months?}\}$ | 0.54 | 0.50 | 0.00 | 1.00 | 1.00 | 0.56 | 0.52 | (0.54) |
| $1\{\text{Turnover by end of sample?}\}$ | 0.74 | 0.44 | 0.00 | 1.00 | 1.00 | 0.77 | 0.71 | (1.41) |
| $1\{\text{Voluntary} \mid \text{Turnover}\}$ | 0.82 | 0.39 | 1.00 | 1.00 | 1.00 | 0.82 | 0.81 | (0.67) |

## Table IA.6: Descriptive statistics: exposed establishments in border counties

This table contains descriptive statistics for our sample of *establishments* (firm-county combinations) in treated or control border counties with at least 5% low wage employment as of the beginning of the sample. The definition of treated and control states is provided in Section 2.2. There are 1,964 establishments from 168 firms and 21 two-digit NAICS industries in our sample. The 25th, 50th, and 75th percentile firm has an establishment in 2 (2), 7 (4), and 16 (8) of the 163 (17) border counties (states) in our sample, respectively. The right-most columns are defined as follows: (1) $T$ refers to the mean for treated counties, (2) $C$ refers to the mean for control counties, and (3) $t(DIFF)$ is the $t$ statistic for a test of difference in means between treated and control counties. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| | Mean | SD | P25 | P50 | P75 | T | C | $t$(DIFF) |
|---|---|---|---|---|---|---|---|---|
| **Variables** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| Total employees | 138 | 1053 | 15 | 30 | 74 | 104 | 174 | (-0.64) |
| % Hourly wage employees | 0.88 | 0.16 | 0.85 | 0.92 | 1.00 | 0.88 | 0.88 | (-0.05) |
| % Low wage employees | 0.52 | 0.39 | 0.28 | 0.55 | 0.73 | 0.52 | 0.52 | (-0.1) |
| % Employees earning $\leq$ \$15 / hour | 0.79 | 0.40 | 0.67 | 0.81 | 0.91 | 0.79 | 0.78 | (0.34) |
| Total new hires | 4 | 17 | 0 | 1 | 3 | 4 | 5 | (-0.43) |
| % Low wage new hires | 0.04 | 0.13 | 0.00 | 0.01 | 0.06 | 0.05 | 0.04 | (0.50) |
| Employment growth | 0.01 | 0.41 | -0.06 | 0.00 | 0.03 | 0.03 | 0.00 | (1.39) |
| Average annual income (all employees) | 25,144 | 14,316 | 16,232 | 21,430 | 30,599 | 25,557 | 24,695 | (0.65) |
| % Payroll from low wage employees | 0.29 | 0.22 | 0.10 | 0.26 | 0.44 | 0.28 | 0.30 | (-0.52) |
| % Payroll from $\leq$ \$15 / hour employees | 0.55 | 0.24 | 0.37 | 0.55 | 0.72 | 0.54 | 0.55 | (-0.38) |

## Table IA.7: Difference-in-differences regression: Bound incumbent wages

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$\omega_{i,t} = \alpha + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + [\delta_{f,t} + \delta_{C,t} + \delta_{T,t}] + \varepsilon_{i,t},$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair $\times$ time fixed effects, $\delta_{f,t}$ are firm $\times$time fixed effects, $\delta_{C,t}$ are cohort $\times$ time fixed effects, $\delta_{T,t}$ are job tenure $\times$ time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $\omega_{i,t}$ , is the hourly wage of individual $i$ in month $t$. All outcome variables are defined in detail in the appendix. The sample is restricted to *Bound* hourly wage employees. The variable Treated$_s$ is an indicator equal to one if state $s$ is treated, and Post$_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | $\omega_{i,t}$ **(1)** | **(2)** | **(3)** | **(4)** |
|---|---|---|---|---|
| Treated$_s$ $\times$ Post$_{t,s}$ | 0.486*** | 0.493*** | 0.777*** | 0.825*** |
| | (9.68) | (12.73) | (15.79) | (36.97) |
| Individual FE | Y | Y | Y | Y |
| County pair $\times$ time FE | Y | Y | Y | Y |
| Firm $\times$ time FE | | Y | | Y |
| Cohort $\times$ time FE | | Y | | Y |
| Tenure $\times$ time FE | | Y | | Y |
| Control variables | Y | Y | Y | Y |
| Sample | Bound | Bound | MW | MW |
| Baseline difference | 0.45 | 0.45 | 0.85 | 0.85 |
| N | 866,679 | 866,679 | 269,454 | 269,454 |
| $R^2$ | 0.72 | 0.80 | 0.71 | 0.81 |

## Table IA.8: Difference-in-differences regression: Spillover incumbent wages

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$\omega_{i,t} = \alpha + \Gamma \times Z_{s,t} + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + [\delta_{f,t} + \delta_{C,t} + \delta_{T,t}] + \varepsilon_{i,t},$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair $\times$ time fixed effects, $\delta_{f,t}$ are firm $\times$time fixed effects, $\delta_{C,t}$ are cohort $\times$ time fixed effects, $\delta_{T,t}$ are job tenure $\times$ time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable is either: (1) the hourly wage ($\omega_{i,t}$) or (2) the natural logarithm of the hourly wage ($\log(\omega_{i,t})$ ) of individual $i$ in month $t$. All outcome variables are defined in detail in the appendix. The sample is restricted to *Spillover* hourly wage employees. The independent variable of interest, $Z_{s,t}$, is either: (1)$\text{Treated}_s \times \text{Post}_{t,s}$ or (2) $\log(\text{MW})_{s,t}$ . $\text{Treated}_s$ is an indicator equal to one if state $s$ is treated, and $\text{Post}_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | $\omega_{i,t}$ **(1)** | **(2)** | $\log(\omega_{i,t})$ **(3)** | **(4)** |
|---|---|---|---|---|
| $\text{Treated}_s \times \text{Post}_{t,s}$ | 0.046*** | 0.041*** | | |
| | (2.73) | (4.47) | | |
| $\log(\text{MW})_{s,t}$ | | | 0.030*** | 0.033*** |
| | | | (2.90) | (4.23) |
| Individual FE | Y | Y | Y | Y |
| County pair $\times$ time FE | Y | Y | Y | Y |
| Firm $\times$ time FE | | Y | | Y |
| Cohort $\times$ time FE | | Y | | Y |
| Tenure $\times$ time FE | | Y | | Y |
| Control variables | Y | Y | Y | Y |
| Sample | Spillover | Spillover | Spillover | Spillover |
| N | 1,419,498 | 1,419,498 | 1,419,498 | 1,419,498 |
| $R^2$ | 0.80 | 0.85 | 0.86 | 0.89 |

## Table IA.9: Magnitude of spillover effect: incumbent

This table presents estimates of the magnitude of wage spillovers for incumbents. We describe our calculation methods in section IC.1 of the Internet Calculation Appendix. The columns are defined as follows: (1) $b$ are wage bins relative to the new minimum wage, (2) spillover region is whether wage bin $b$ is equal to $1, 2$, or 3, (3) $N_b$ is the percentage of incumbent employees in treated states in each wage bin, (4) $\bar{H}_b$ is the average pre-treatment hours worked per-week in bin $b$ in treated states, (5) $\Gamma_b$ is the coefficient estimate of the total effect of the minimum wage on wages in wage bin $b$, (6) direct effect is the gap between the average new minimum wage and the average wage in bin $b$ one month prior to treatment, (7) and indirect effect is the residual of the difference between the total effect $\Gamma_b$ and the direct effect $G_b$. The bottom two rows report the share of total wage effect due to the direct effect ($\iota_D$) and the share of total wage effect due to the indirect effect ($\iota_I$). The calculation methods are described in section IC.1 of the Internet Calculation Appendix.

| Wage bin ($b$) | Spillover region? | %Obs. ($N_b$) | Hours ($\bar{H}_b$) | Total ($\Gamma_b$) | Direct effect ($G_b$) | Indirect effect ($\Gamma_b - G_b$) |
|---|---|---|---|---|---|---|
| -1 | No | 12.3 | 26.68 | 0.82 | 0.84 | -0.02 |
| 0 | No | 21.5 | 27.10 | 0.42 | 0.37 | 0.05 |
| 1: | Yes | 25.9 | 28.96 | 0.07 | 0.00 | 0.07 |
| 2 | Yes | 20.9 | 32.28 | 0.05 | 0.00 | 0.05 |
| 3 | Yes | 19.5 | 34.03 | 0.03 | 0.00 | 0.03 |
| Share of total wage effect due to the direct effect ($\iota_D$): | | | | | | 79.5% |
| Share of total wage effect due to the indirect effect ($\iota_I$): | | | | | | 20.5% |

## Table IA.10: Magnitude of spillover effect: new hires

This table presents estimates of the magnitude of wage spillovers for new hires, conditional upon hiring. We describe our calculation methods in section IC.2 of the Internet Calculation Appendix. In panel A, the columns are defined as follows: (1) $b$ are wage bins relative to the new minimum wage, (2) $\Gamma_b$ is the coefficient estimate of the effect on the density of new hires in wage bin $b$, (3) $\omega_b$ is the average lowest pre-treatment wage in bin $b$ in treated states, (4) Spillover is whether bin $b$ is in the spillover region $b = 1, 2$, or 3, (5) gap is the distance between the average new minimum wage and the average lowest wage in bin $b$ prior to treatment. In panel B, the columns are defined as follows: (1) $\bar{\omega} \downarrow$ is the average wage of new jobs that previously would have paid below the minimum wage, (2) $\bar{\omega} \uparrow$ is the average wage of new jobs created above the new minimum wage in the spillover region, (3) $\%\bar{\omega}$ is the percent change in average wages for new hires due to the minimum wage increase, (4) $\%\bar{\omega}_D$ is the percent gain due the direct effect, and (5) $\%\bar{\omega}_D$ is the percent gain due to the indirect effect. The share of the percent gain due to the direct effect is given by $\zeta_D$ at the bottom. The share of the percent gain due to the indirect effect is given by $\zeta_I$ at the bottom. The calculation methods are described in section IC.1 of the Internet Calculation Appendix.

**Panel A: Inputs**

| Wage bin ($b$) | Density coefficient ($\Gamma_b$) | Bottom pre-wage ($\omega_b$) | Spillover region? | Gap ($G_b$) |
|---|---|---|---|---|
| -1 | -0.061 | 7.79 | Yes | 0.82 |
| 0 | -0.106 | 7.80 | Yes | 0.81 |
| 1: | 0.104 | 8.73 | No | 0.00 |
| 2 | 0.047 | 9.66 | No | 0.00 |
| 3 | 0.017 | 10.60 | No | 0.00 |

**Panel B: Outputs**

| $\bar{\omega} \downarrow$ | $\bar{\omega} \uparrow$ | $\%\bar{\omega}$ | $\%\bar{\omega}_D$ | $\%\bar{\omega}_I$ |
|---|---|---|---|---|
| 7.79 | 9.18 | 0.178 | 0.106 | 0.072 |

Share of percent change $\%\bar{\omega}$ due to the direct effect ($\zeta_D$): 59.3%

Share of percent change $\%\bar{\omega}$ due to the indirect effect ($\zeta_I$): 40.7%

## Table IA.11: Difference-in-differences regression: heterogeneity of spillover effect

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$\omega_{i,t} = \alpha + \beta \times \text{Treated}_s \times \text{Post}_{t,s} \times \text{Z}_i + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + \delta_{f,t} + \delta_{C,t} + \delta_{T,t} + \varepsilon_{i,t},$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair $\times$ time fixed effects, $\delta_{f,t}$ are firm $\times$time fixed effects, $\delta_{C,t}$ are cohort $\times$ time fixed effects, $\delta_{T,t}$ are job tenure $\times$ time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $\omega_{i,t}$ , is the hourly wage of individual $i$ in month $t$. All outcome variables are defined in detail in the appendix. The sample is restricted to hourly wage employees in the spillover region. The variable $\text{Treated}_s$ is an indicator equal to one if state $s$ is treated, and $\text{Post}_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. The variable $\text{Z}_i$ is a cross-sectional cut for either: (1) employee tenure (measured in years), (2) the fraction of minimum wage workers in employee $i$'s establishment, or (3) whether employee $i$ works in a non-tradable establishment. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| Explanatory Variables | $\omega_{i,t}$ (1) | $\omega_{i,t}$ (2) | $\omega_{i,t}$ (3) |
|---|---|---|---|
| $\text{Treated}_s \times \text{Post}_{t,s}$ | -0.1297 | -0.004 | 0.016 |
| | (-1.31) | (-0.08) | (0.28) |
| $\text{Treated}_s \times \text{Post}_{t,s}$ $\times\text{TENURE}_i$ | 0.0668*** | | |
| | (3.21) | | |
| $\text{Treated}_s \times \text{Post}_{t,s}$ $\times\text{EXPOSURE}_i$ | | 0.357*** | |
| | | (3.36) | |
| $\text{Treated}_s \times \text{Post}_{t,s}$ $\times\text{NONTRADABLE}_f$ | | | 0.0992 |
| | | | (1.54) |
| Individual FE | Y | Y | Y |
| County pair $\times$ time FE | Y | Y | Y |
| Firm $\times$ time FE | Y | Y | Y |
| Cohort $\times$ time FE | Y | Y | Y |
| Tenure $\times$ time FE | Y | Y | Y |
| Control variables | Y | Y | Y |
| N | 1,419,004 | 1,419,468 | 1,418,022 |
| $R^2$ | 0.541 | 0.541 | 0.541 |

# Table IA.12: Difference-in-differences regression: bound employment elasticities

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{i,t} = \alpha + \Gamma \times Z_{s,t} + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + [\delta_{f,t} + \delta_{C,t} + \delta_{T,t}] + \varepsilon_{i,t},$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair × time fixed effects, $\delta_{f,t}$ are firm ×time fixed effects, $\delta_{C,t}$ are cohort × time fixed effects, $\delta_{T,t}$ are job tenure × time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{i,t}$ , is either: (1) an indicator for employment ($E_{i,t}$), (2) an indicator for voluntary turnover ($V_{i,t}$), (3) an indicator for involuntary turnover ($I_{i,t}$), or (4) the natural logarithm of average hours per week ($H_{i,t}$) of individual $i$ in month $t$. All outcome variables are defined in detail in the appendix. The sample is restricted to *Bound* hourly wage employees. The independent variable of interest, $Z_{s,t}$, is either: (1)log(MW)$_{s,t}$ or (2) log($\omega_{i,t}$) . Treated$_s$ is an indicator equal to one if state $s$ is treated, and Post$_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | $E_{i,t}$ **(1)** | $V_{i,t}$ **(2)** | $I_{i,t}$ **(3)** | $H_{i,t}$ **(4)** | $E_{i,t}$ **(5)** | $V_{i,t}$ **(6)** | $I_{i,t}$ **(7)** | $H_{i,t}$ **(8)** |
|---|---|---|---|---|---|---|---|---|
| $\log(\text{MW})_{s,t}$ | 0.028 | -0.029 | -0.013** | 0.286* | | | | |
| | (1.40) | (-1.09) | (-2.24) | (1.82) | | | | |
| $\log(\omega_{i,t})$ | | | | | 0.072*** | -0.059*** | -0.005** | 0.256** |
| | | | | | (3.02) | (-2.87) | (-2.28) | (2.23) |
| Individual FE | Y | Y | Y | Y | Y | Y | Y | Y |
| County pair × time FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Firm × time FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Cohort × time FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Tenure × time FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Control variables | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 884,964 | 817,172 | 817,172 | 316,432 | 884,964 | 817,172 | 817,172 | 316,432 |

## Table IA.13: Difference-in-differences regression: Bound employment with alternative clustering

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$E_{i,t} = \alpha + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + [\delta_{f,t} + \delta_{C,t} + \delta_{T,t}] + \varepsilon_{i,t},$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair × time fixed effects, $\delta_{f,t}$ are firm ×time fixed effects, $\delta_{C,t}$ are cohort × time fixed effects, $\delta_{T,t}$ are job tenure × time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $E_{i,t}$ , is an indicator for employment of individual $i$ in month $t$. All outcome variables are defined in detail in the appendix. The sample is restricted to *Bound* hourly wage employees. The variable Treated$_s$ is an indicator equal to one if state $s$ is treated, and Post$_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the level indicated in the table, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | $E_{i,t}$ **(1)** | $E_{i,t}$ **(2)** | $E_{i,t}$ **(3)** | $E_{i,t}$ **(4)** | $E_{i,t}$ **(5)** | $E_{i,t}$ **(6)** | $E_{i,t}$ **(7)** | $E_{i,t}$ **(8)** |
|---|---|---|---|---|---|---|---|---|
| Treated$_s$ × Post$_{t,s}$ | -0.003 | -0.003 | -0.003 | -0.003 | 0.003 | 0.003 | 0.003 | 0.003 |
| | (-0.60) | (-0.84) | (-0.56) | (-0.42) | (1.45) | (1.01) | (1.16) | (1.10) |
| Individual FE | Y | Y | Y | Y | Y | Y | Y | Y |
| County pair × time FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Firm × time FE | | | | | Y | Y | Y | Y |
| Cohort × time FE | | | | | Y | Y | Y | Y |
| Tenure × time FE | | | | | Y | Y | Y | Y |
| Control variables | Y | Y | Y | Y | Y | Y | Y | Y |
| Clustering | County | $i$ & $t$ | Company | State | County | $i$ & $t$ | Company | State |
| N | 884,964 | 884,964 | 884,964 | 884,964 | 884,964 | 884,964 | 884,964 | 884,964 |
| $R^2$ | 0.32 | 0.32 | 0.32 | 0.32 | 0.40 | 0.40 | 0.40 | 0.40 |

## Table IA.14: Difference-in-differences regression: Bound employment with continuous treatment

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{i,t} = \alpha + \Gamma \times \text{CTreated}_{i,s} \times \text{Post}_{t,s} + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + \delta_{f,t} + \delta_{C,t} + \delta_{T,t} + \varepsilon_{i,t},$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair $\times$ time fixed effects, $\delta_{f,t}$ are firm $\times$time fixed effects, $\delta_{C,t}$ are cohort $\times$ time fixed effects, $\delta_{T,t}$ are job tenure $\times$ time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{i,t}$ , is either: (1) an indicator for employment ($E_{i,t}$), (2) an indicator for voluntary turnover ($V_{i,t}$), (3) an indicator for involuntary turnover ($I_{i,t}$), or (4) the natural logarithm of average hours per week ($H_{i,t}$) of individual $i$ in month $t$. All outcome variables are defined in detail in the appendix. The sample is restricted to *Bound* hourly wage employees. The variable $\text{CTreated}_{i,s}$ is a continuous measure of treatment equal to the difference between the new minimum wage and the employee's pre-treatment wage, $\text{NEW MW}_s - \omega_i$, in treated states and zero otherwise, and $\text{Post}_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | $E_{i,t}$ **(1)** | $V_{i,t}$ **(2)** | $I_{i,t}$ **(3)** |
|---|---|---|---|
| $\text{CTreated}_{i,s} \times \text{Post}_{t,s}$ | -0.002 | 0.001 | 0.000 |
| | (-0.49) | (0.29) | (-0.18) |
| Individual FE | Y | Y | Y |
| County pair × time FE | Y | Y | Y |
| Firm × time FE | Y | Y | Y |
| Cohort × time FE | Y | Y | Y |
| Tenure × time FE | Y | Y | Y |
| Control variables | Y | Y | Y |
| N | 884,964 | 817,172 | 817,172 |
| $R^2$ | 0.38 | 0.35 | 0.35 |

# Table IA.15: Difference-in-differences regression: Bound employment across industry groups

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$E_{i,t} = \alpha + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + \delta_{f,t} + \delta_{C,t} + \delta_{T,t} + \varepsilon_{i,t},$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair $\times$ time fixed effects, $\delta_{f,t}$ are firm $\times$time fixed effects, $\delta_{C,t}$ are cohort $\times$ time fixed effects, $\delta_{T,t}$ are job tenure $\times$ time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $E_{i,t}$ , is an indicator for employment of individual $i$ in month $t$. All outcome variables are defined in detail in the appendix. The sample is restricted to *Bound* hourly wage employees and the model is estimated across sub-samples split by firm industry. The variable Treated$_s$ is an indicator equal to one if state $s$ is treated, and Post$_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | $E_{i,t}$ **(1)** | $E_{i,t}$ **(2)** |
|---|---|---|
| Treated$_s$ $\times$ Post$_{t,s}$ | 0.004* | -0.010 |
| | (1.83) | (-1.39) |
| Individual FE | Y | Y |
| County pair $\times$ time FE | Y | Y |
| Firm $\times$ time FE | Y | Y |
| Cohort $\times$ time FE | Y | Y |
| Tenure $\times$ time FE | Y | Y |
| Control variables | Y | Y |
| Industry | Non-tradable | Tradable |
| N | 707,561 | 177,369 |
| $R^2$ | 0.38 | 0.38 |

## Table IA.16: Difference-in-differences regression: Bound employment across age groups

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$E_{i,t} = \alpha + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + \delta_{f,t} + \delta_{C,t} + \delta_{T,t} + \varepsilon_{i,t},$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair × time fixed effects, $\delta_{f,t}$ are firm ×time fixed effects, $\delta_{C,t}$ are cohort × time fixed effects, $\delta_{T,t}$ are job tenure × time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $E_{i,t}$ , is an indicator for employment of individual $i$ in month $t$. All outcome variables are defined in detail in the appendix. The sample is restricted to *Bound* hourly wage employees and the model is estimated across subsamples split by individual age. The variable Treated$_s$ is an indicator equal to one if state $s$ is treated, and Post$_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | $E_{i,t}$ **(1)** | $E_{i,t}$ **(2)** | $E_{i,t}$ **(3)** | $E_{i,t}$ **(4)** | $E_{i,t}$ **(5)** | $E_{i,t}$ **(6)** |
|---|---|---|---|---|---|---|
| Treated$_s$ × Post$_{t,s}$ | 0.013*** | 0.002 | 0.006 | 0.010 | -0.015** | 0.002 |
| | (2.70) | (0.62) | (1.22) | (1.03) | (-2.27) | (0.49) |
| Individual FE | Y | Y | Y | Y | Y | Y |
| County pair × time FE | Y | Y | Y | Y | Y | Y |
| Firm × time FE | Y | Y | Y | Y | Y | Y |
| Cohort × time FE | Y | Y | Y | Y | Y | Y |
| Tenure × time FE | Y | Y | Y | Y | Y | Y |
| Control variables | Y | Y | Y | Y | Y | Y |
| Age group | Teens | 20-24 | 25-29 | 30-34 | 35-39 | 40+ |
| N | 65,412 | 203,536 | 90,308 | 58,650 | 45,998 | 223,492 |
| $R^2$ | 0.40 | 0.40 | 0.44 | 0.48 | 0.49 | 0.40 |

## Table IA.17: Difference-in-differences regression: Bound employment across tenure groups

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$E_{i,t} = \alpha + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + \delta_{f,t} + \delta_{C,t} + \delta_{T,t} + \varepsilon_{i,t},$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair $\times$ time fixed effects, $\delta_{f,t}$ are firm $\times$time fixed effects, $\delta_{C,t}$ are cohort $\times$ time fixed effects, $\delta_{T,t}$ are job tenure $\times$ time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $E_{i,t}$ , is an indicator for employment of individual $i$ in month $t$. All outcome variables are defined in detail in the appendix. The sample is restricted to *Bound* hourly wage employees and the model is estimated across subsamples split by individual tenure (measured during the pre-treatment period). The variable $\text{Treated}_s$ is an indicator equal to one if state $s$ is treated, and $\text{Post}_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | $E_{i,t}$ **(1)** | $E_{i,t}$ **(2)** | $E_{i,t}$ **(3)** | $E_{i,t}$ **(4)** | $E_{i,t}$ **(5)** | $E_{i,t}$ **(6)** |
|---|---|---|---|---|---|---|
| $\text{Treated}_s \times \text{Post}_{t,s}$ | 0.007 | 0.011 | 0.006 | 0.008 | -0.002 | -0.005*** |
| | (0.97) | (1.39) | (1.22) | (1.54) | (-1.00) | (-2.10) |
| Individual FE | Y | Y | Y | Y | Y | Y |
| County pair × time FE | Y | Y | Y | Y | Y | Y |
| Firm × time FE | Y | Y | Y | Y | Y | Y |
| Cohort × time FE | Y | Y | Y | Y | Y | Y |
| Tenure × time FE | Y | Y | Y | Y | Y | Y |
| Control variables | Y | Y | Y | Y | Y | Y |
| Tenure group (months) | [0,3] | [4,6] | [7,9] | [10,12] | [13,36] | [37+] |
| N | 131,413 | 94,939 | 87,936 | 76,435 | 324,944 | 169,276 |
| $R^2$ | 0.58 | 0.36 | 0.33 | 0.33 | 0.24 | 0.28 |

## Table IA.18: Difference-in-differences regression: SUTVA for bound employment

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$\begin{aligned} Y_{i,t} &= \alpha + \beta \times \text{Treated}_s \times \text{Post}_{t,s} \times \text{Distance}_i + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} \\ &\quad + \eta' X_{s,t-1} + \delta_i + \delta_{p,t} + \delta_{f,t} + \delta_{C,t} + \delta_{T,t} + \varepsilon_{i,t}, \end{aligned}$$

where $\delta_i$ are individual fixed effects, $\delta_{p,t}$ are border county pair × time fixed effects, $\delta_{f,t}$ are firm ×time fixed effects, $\delta_{C,t}$ are cohort × time fixed effects, $\delta_{T,t}$ are job tenure × time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{i,t}$ , is either: (1) an indicator for employment ($E_{i,t}$), (2) an indicator for voluntary turnover ($V_{i,t}$), (3) an indicator for involuntary turnover ($I_{i,t}$), or (4) the natural logarithm of average hours per week ($H_{i,t}$) of individual $i$ in month $t$. All outcome variables are defined in detail in the appendix. The sample is restricted to *Bound* hourly wage employees. The variable Treated$_s$ is an indicator equal to one if state $s$ is treated, and Post$_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The variable Distance$_i$ is the distance of individual $i$ from the nearest state border. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | $E_{i,t}$ **(1)** | $V_{i,t}$ **(2)** | $I_{i,t}$ **(3)** |
|---|---|---|---|
| Treated$_s$ × Post$_{t,s}$ | 0.006** | -0.002 | -0.001 |
| | (2.28) | (-0.71) | (-1.00) |
| Treated$_s$ × Post$_{t,s}$ | 0.000 | 0.000 | 0.000 |
| ×Distance$_i$ | (-1.48) | (0.16) | (-0.94) |
| Individual FE | Y | Y | Y |
| County pair × time FE | Y | Y | Y |
| Firm × time FE | Y | Y | Y |
| Cohort × time FE | Y | Y | Y |
| Tenure × time FE | Y | Y | Y |
| Control variables | Y | Y | Y |
| N | 884,964 | 817,172 | 817,172 |
| $R^2$ | 0.38 | 0.35 | 0.35 |

# Table IA.19: Panel regression: establishment elasticities

This table contains the coefficient estimates from static panel regressions of the form:

$$Y_{f,c,t} = \alpha + \delta_{f,c} + \delta_{p,t} + \Gamma \times \log(\mathrm{MW}_s) + \eta' X_{s,t-1} + [\delta_{f,t}] + \varepsilon_{f,c,t}$$

where $\delta_{f,c}$ are firm-county (*establishment*) fixed effects,$\delta_{p,t}$ are border county pair $\times$ time fixed effects, $\delta_{f,t}$ are firm $\times$time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{f,c,t}$, is either: (1) the natural logarithm of low-wage employment (*log(LowWage)*), (2) the natural logarithm of total employment (*log(Total))*, (3) the natural logarithm of low wage hires (*log(LowWageHires)*), or (4) the natural logarithm of total hires (*log(Hires)*) at establishment $f, c$ in month $t$. The sample is restricted to establishments with at least 5% low wage employees as of their initial date of entering the sample. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory** | log (LowWage) | | log (Total) | | log (LowWageHires) | | log (Hires) | |
|---|---|---|---|---|---|---|---|---|
| **Variables** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| $\log(\mathrm{MW}_s)$ | -0.427*** | -0.589*** | -0.031 | -0.086 | -0.488*** | -0.557*** | -0.233* | -0.275* |
| | (-2.59) | (-3.51) | (-0.06) | (-1.49) | (-3.37) | (-3.48) | (-1.84) | (-1.90) |
| Firm $\times$ county FE | Y | Y | Y | Y | Y | Y | Y | Y |
| County pair $\times$ time FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Firm $\times$ time FE | | Y | | Y | | Y | | Y |
| Control Variables | Y | Y | Y | Y | Y | Y | Y | Y |
| Wage response: | 0.41 | 0.41 | 0.08 | 0.08 | 0.41 | 0.41 | 0.08 | 0.08 |
| N | 39,929 | 39,929 | 39,929 | 39,929 | 39,929 | 39,929 | 39,929 | 39,929 |

## Table IA.20: Alternative explanations: reallocation of labor I

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{f,c,t} = \alpha + \Gamma \times \mathrm{E}_f \times \mathrm{Post}_{t,s} + \delta_{f,c} + \delta_{s,t} + [\delta_{f,t}] + \varepsilon_{f,c,t}$$

where $\delta_{f,c}$ are establishment fixed effects, $\delta_{s,t}$ are state$\times$ month fixed effects, and $\delta_{f,t}$ are firm $\times$month fixed effects. The outcome variable, $Y_{i,t}$ , is either: (1) the fraction of low-wage hires to lagged total employment (*LowWageHires / Total*) or (2) the fraction of low-wage employees to lagged total employment (*LowWage/ Total*) at establishment $f, c$ in month $t$. The outcome variables are defined in the appendix. The sample is restricted to establishments in control states in border counties. The variable $\mathrm{E}_f$ is the fraction of firm $f$'s employees in treated states that will receive an increase in wages due to the new minimum wage. This variable varies within states and counties. The definition of treated and control states is provided in Section 2.2. The row Sample corresponds to whether the model is estimated on all establishments or establishments in the tradable sector. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory** | LowWageHires/Total | | | | LowWage/Total | | | |
|---|---|---|---|---|---|---|---|---|
| **Variables** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| Treated$_s$ $\times$ Post$_{t,s}$ | -0.008 | 0.002 | 0.028 | 0.034 | 0.018 | 0.026 | -0.024 | -0.001 |
| | (-1.09) | (0.09) | (1.10) | (0.64) | (1.13) | (1.26) | (-0.49) | (-0.02) |
| Firm $\times$ county FE | Y | Y | Y | Y | Y | Y | Y | Y |
| State $\times$ month FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Firm $\times$ month FE | | Y | | Y | | Y | | Y |
| Sample | All | All | Tradable | Tradable | All | All | Tradable | Tradable |
| N | 18,856 | 18,856 | 5,149 | 5,149 | 18,856 | 18,856 | 5,149 | 5,149 |
| $R^2$ | 0.22 | 0.26 | 0.38 | 0.46 | 0.90 | 0.93 | 0.89 | 0.93 |

## Table IA.21: Alternative explanations: reallocation of labor II

This table contains coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{f,r,s,,t} = \alpha + \Gamma \times \text{Border}_r \times \text{Post}_{t,s} + \delta_{f,r,s} + \delta_{s,t} + [\delta_{f,t}] + \varepsilon_{f,r,s,t},$$

where $s$ indexes states, $f$ indexes firms, $t$ indexes months, and $r$ corresponds to either the border ($r = b$) or the interior ($r = i$) of state $s$. We aggregate establishment observations to the firm-region level defined by the triple $f, r, s$, $\delta_{f,r,s}$ are firm-region fixed effects, , $\delta_{s,t}$ are state× month fixed effects, and $\delta_{f,t}$ are firm ×month fixed effects.. The outcome variable, $Y_{i,t}$ , is either: (1) the fraction of low-wage hires to lagged total employment (*LowWageHires / Total*) or (2) the fraction of low-wage employees to lagged total employment (*LowWage/ Total*) at firm-region $f, r, s$ in month $t$. The outcome variables are defined in the appendix. The sample is restricted to establishments in control states. The variable $\text{Border}_r$ is a dummy variable equal to one if region $r$ is the border, and zero otherwise. The definition of treated and control states is provided in Section 2.2. The row Sample corresponds to whether the model is estimated on all establishments or establishments in the tradable sector. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory** | LowWageHires/Total | | | | LowWage/Total | | | |
|---|---|---|---|---|---|---|---|---|
| **Variables** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| $\text{Treated}_s \times \text{Post}_{t,s}$ | 0.042 | 0.053* | 0.040 | 0.069 | -0.0029 | -0.0054 | -0.022* | -0.025 |
| | (1.15) | (1.90) | (0.41) | (0.90) | (-0.51) | (-1.10) | (-1.74) | (-2.23) |
| Firm-region FE | Y | Y | Y | Y | Y | Y | Y | Y |
| State × month FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Firm × month FE | | Y | | Y | | Y | | Y |
| Sample | All | All | Tradable | Tradable | All | All | Tradable | Tradable |
| N | 18,169 | 18,169 | 5,270 | 5,270 | 18,169 | 18,169 | 18,169 | 18,169 |
| $R^2$ | 0.83 | 0.89 | 0.76 | 0.86 | 0.89 | 0.92 | 0.83 | 0.89 |

## Table IA.22: Difference-in-differences regression: establishment clustering

This table contains the coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{f,c,t} = \alpha + \delta_{f,c} + \delta_{p,t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + [\delta_{f,t}] + \varepsilon_{f,c,t}$$

where $\delta_{f,c}$ are firm-county (*establishment*) fixed effects,$\delta_{p,t}$ are border county pair $\times$ time fixed effects, $\delta_{f,t}$ are firm $\times$time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{f,c,t}$, is the natural logarithm of the number of low wage employees (*log(LowWage)*) at establishment $f,c$ in month $t$. The sample is restricted to establishments with at least 5% low wage employees as of their initial date of entering the sample. The variable Treated$_s$ is an indicator equal to one if state $s$ is treated, and Post$_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the level indicated in the table, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | log (LowWage) **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
|---|---|---|---|---|---|---|
| Treated$_s$ $\times$ Post$_{t,s}$ | -0.059*** | -0.059*** | -0.059** | -0.059** | -0.059** | -0.059** |
| | (-2.72) | (-2.74) | (-2.21) | (-2.27) | (-2.22) | (-2.26) |
| Firm $\times$ county FE | Y | Y | Y | Y | Y | Y |
| County pair $\times$ time FE | Y | Y | Y | Y | Y | Y |
| Firm $\times$ time FE | Y | Y | Y | Y | Y | Y |
| Control Variables | Y | Y | Y | Y | Y | Y |
| Clustering | $s$ | $s$ & $t$ | $f$ | $f$ & $t$ | $f$ & $s$ | $f$ & $s$ $t$ |
| N | 39,929 | 39,929 | 39,929 | 39,929 | 39,929 | 39,929 |
| $R^2$ | 0.963 | 0.963 | 0.963 | 0.963 | 0.963 | 0.954 |

## Table IA.23: Difference-in-differences regression: establishment results on full sample

This table contains the coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{f,c,t} = \alpha + \delta_{f,c} + \delta_{p,t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \eta' X_{s,t-1} + \delta_{f,t} + \varepsilon_{f,c,t}$$

where $\delta_{f,c}$ are firm-county (*establishment*) fixed effects,$\delta_{p,t}$ are border county pair $\times$ time fixed effects, $\delta_{f,t}$ are firm $\times$time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{f,c,t}$, is either a: (1) the fraction of low wage employment over lagged total employment (*LowWage/Total)*, (2), the natural logarithm of low wage employment (*log(LowWage)*), (3) the natural logarithm of total employment (*log(Total)*), (4) the fraction of low wage hires to lagged total employment (*LowWageHires / Total*), (5) the natural logarithm of low wage hires (*log(LowWageHires)*), or (6) the natural logarithm of total hires (*log(Hires)*) at establishment $f, c$ in month $t$. The outcome variables are defined in full in the appendix. The sample is not restricted to establishments with at least 5% low wage employees as of their initial date of entering the sample. The variable $\text{Treated}_s$ is an indicator equal to one if state $s$ is treated, and $\text{Post}_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | LowWage/Total **(1)** | log (LowWage) **(2)** | log (Total) **(3)** | LowWageHires/Total **(4)** | log (LowWageHires) **(5)** | log (Hires) **(6)** |
|---|---|---|---|---|---|---|
| $\text{Treated}_s \times \text{Post}_{t,s}$ | -0.007*** | -0.033*** | -0.005 | -0.002** | -0.034*** | -0.138 |
| | (-2.89) | (-2.61) | (-1.05) | (-1.96) | (-3.08) | (-1.26) |
| Firm $\times$ county FE | Y | Y | Y | Y | Y | Y |
| County pair $\times$ time FE | Y | Y | Y | Y | Y | Y |
| Firm $\times$ time FE | Y | Y | Y | Y | Y | Y |
| Control Variables | Y | Y | Y | Y | Y | Y |
| N | 63,679 | 66,575 | 66,575 | 63,679 | 66,575 | 66,575 |
| $R^2$ | 0.97 | 0.98 | 0.99 | 0.39 | 0.82 | 0.80 |

## Table IA.24: Difference-in-differences regression: establishment continuous treatment

This table contains the coefficient estimates from static difference-in-differences regressions of the form:

$$Y_{f,c,t} = \alpha + \beta \times \text{Treated}_s \times \text{Post}_{t,s} \times \text{EXP}_{f,c} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,s} + \delta_{f,c} + \delta_{p,t} + +\eta' X_{s,t-1} + \delta_{f,t} + \varepsilon_{f,c,t}$$

$$=$$

where $\delta_{f,c}$ are firm-county (*establishment*) fixed effects,$\delta_{p,t}$ are border county pair × time fixed effects, $\delta_{f,t}$ are firm ×time fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{f,c,t}$, is either a: (1) the fraction of low wage employment over lagged total employment (*LowWage/Total)*, (2), the natural logarithm of low wage employment (*log(LowWage)*), (3) the natural logarithm of total employment (*log(Total)*), (4) the fraction of low wage hires to lagged total employment (*LowWageHires / Total*), (5) the natural logarithm of low wage hires (*log(LowWageHires)*), or (6) the natural logarithm of total hires (*log(Hires)*) at establishment $f, c$ in month $t$. The outcome variables are defined in full in the appendix. The sample is restricted to establishments with at least 5% low wage employees as of their initial date of entering the sample. The variable $\text{Treated}_s$ is an indicator equal to one if state $s$ is treated, and $\text{Post}_{t,s}$ is an indicator equal to one if for all months $t$ after the month of treatment for state $s$, and zero otherwise. The variable $\text{EXP}_{f,c}$ is an interaction term that measures the fraction of employees subject to receiving wage increases from the minimum wage increase as of the initial sample date. The definition of treated and control states is provided in Section 2.2. Standard errors are calculated by clustering at the county level, and $t$-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| **Explanatory Variables** | LowWage/Total **(1)** | log (LowWage) **(2)** | log (Total) **(3)** | LowWageHires/Total **(4)** | log (LowWageHires) **(5)** | log (Hires) **(6)** |
|---|---|---|---|---|---|---|
| $\text{Treated}_s \times \text{Post}_{t,s}$ | -0.002 | 0.000 | 0.045*** | -0.001 | 0.020 | 0.052 |
| | (-0.4) | (-0.02) | (5.00) | (-0.34) | (0.55) | (1.5) |
| $\text{Treated}_s \times \text{Post}_{t,s}$ | -0.041*** | -0.223*** | -0.208*** | -0.051*** | -0.259* | -0.305** |
| $\times \text{EXP}_{f,c}$ | (-5.31) | (-5.73) | (-10.05) | (-2.74) | (-1.80) | (-2.02) |
| Firm × county FE | Y | Y | Y | Y | Y | Y |
| County pair × time FE | Y | Y | Y | Y | Y | Y |
| Firm × time FE | Y | Y | Y | Y | Y | Y |
| Control Variables | Y | Y | Y | Y | Y | Y |
| N | 38,172 | 39,929 | 39,929 | 10,714 | 11,212 | 11,212 |
| $R^2$ | 0.94 | 0.96 | 0.99 | 0.63 | 0.82 | 0.85 |

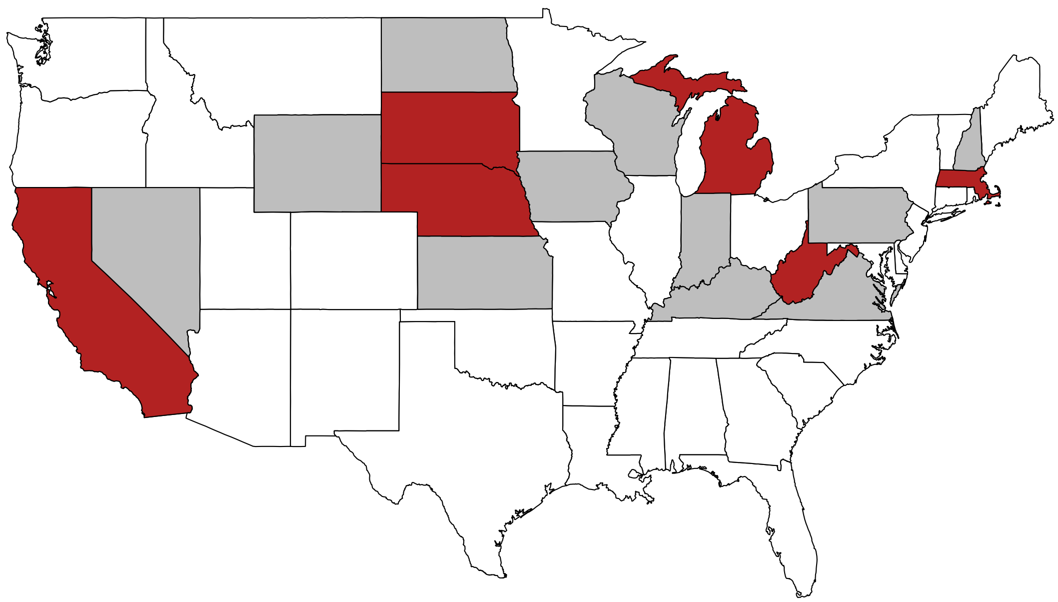
# Internet appendix figures

In this portion of the internet appendix, we provide supplemental figures to the main text.

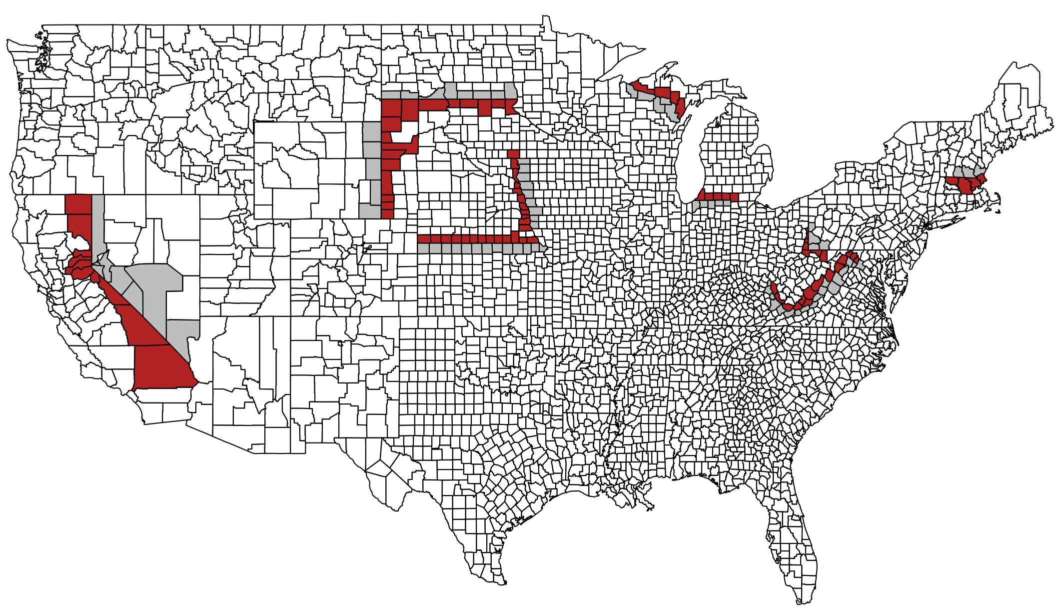
## Figure IA.1: Map of treated and control geographies

This figure plots the treated and control geographies. Panel A contains the treated and control states. The states with the dark-red shading are treated states, and the states with the gray shading are the control states. The states with the white shading are excluded from the analysis. Panel B contains the treated and control counties. The counties with the dark-red shading are treated border counties, and the counties with the gray shading are the control border counties.

**Panel A: Map of treated and control states**



**Panel B: Map of treated and control border counties**

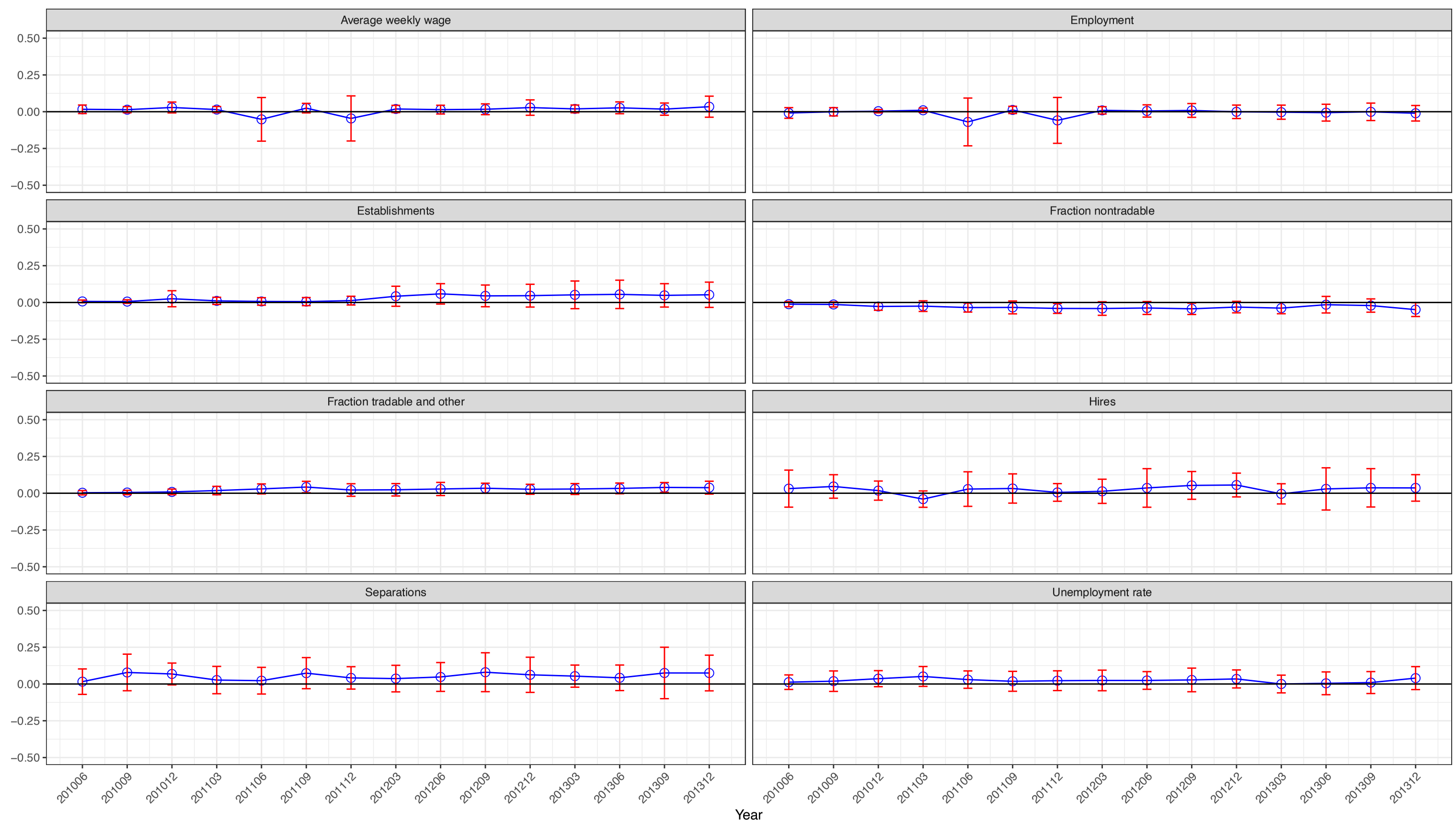


## Figure IA.2: Macroeconomic trends in border counties

This figure plots coefficient estimates from dynamic difference-in-difference regressions of the form:

$$y_{c,t} = \alpha + \sum_{\tau \neq 2010-03} \Gamma_\tau \text{Treated}_s \times \text{D}(t,\tau) + \delta_c + \delta_{p,t} + \epsilon_{c,t},$$

where the outcome variable, $y_{c,t}$, is either the natural logarithm of *Average weekly wages, Employment, Establishments, the Fraction of Non-Tradable Employment, the Fraction of Tradable and Other Employment, Hires*, *Separations,* or the *Unemployment rate* in county $c$ in quarter $t$, $\delta_c$ are county fixed effects, $\delta_{p,t}$ are border county pair $\times$ quarter fixed effects, $\text{Treated}_s$ is a dummy variable that takes a value one if state $s$ is a treated state, and $D(t,\tau)$ is a dummy variable equal to one for in quarter $t = \tau$. The regressions are estimated for the period 2010-2013, with the reference quarter being Q1 2010. The definition of treatment and control states is provided in Section 2.2 of the text. In the figure, the blue dots indicate coefficient estimates for the $\{\Gamma_\tau\}_\tau$'s and the vertical red bars denote confidence 95% confidence intervals. Standard errors are clustered at the county level.

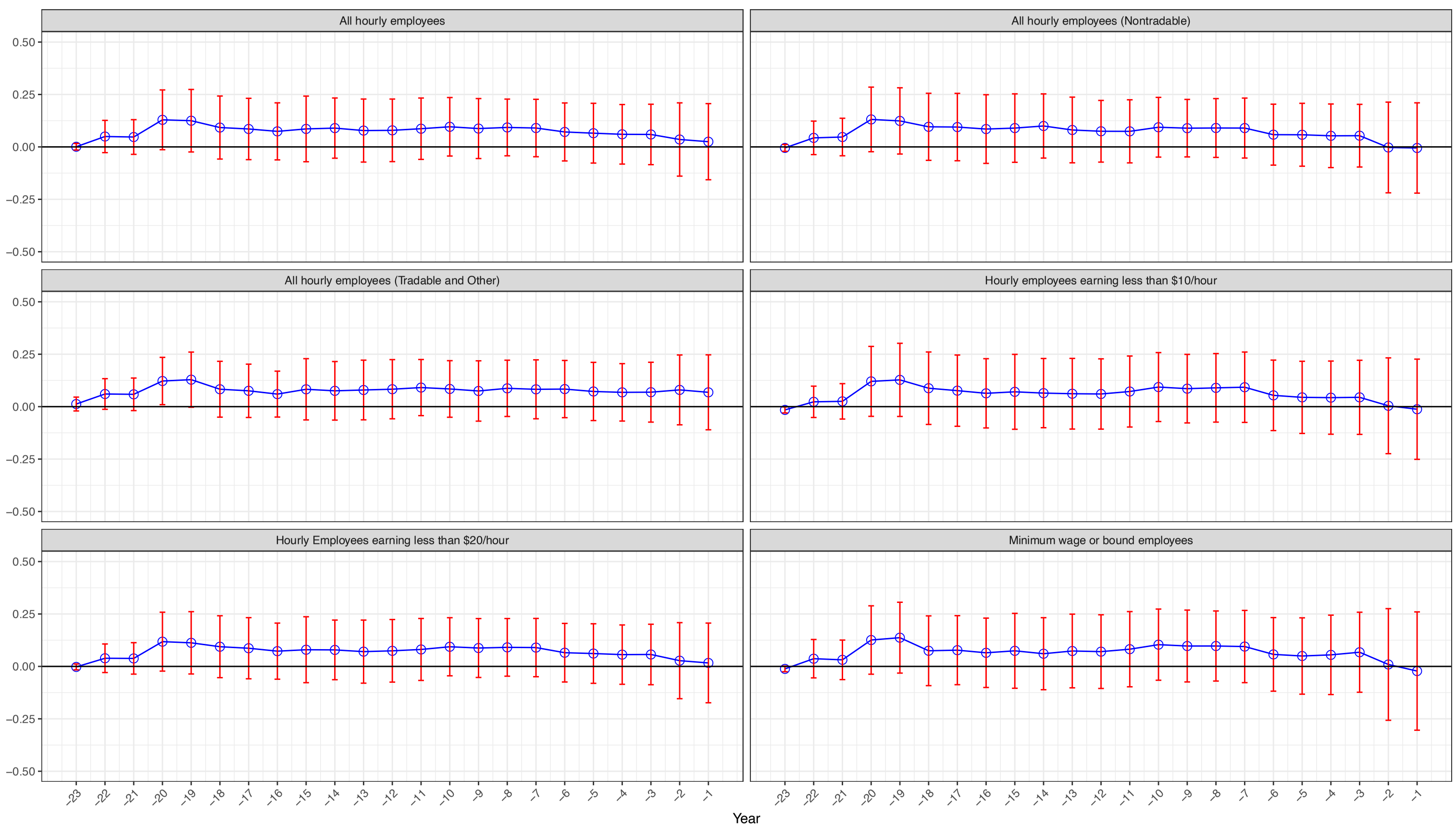


# Figure IA.3: Hourly wage trends in treated and control states

This figure plots coefficient estimates from dynamic difference-in-difference regressions of the form:

$$y_{c,t} = \alpha + \sum_{\tau=-23}^{-1} \Gamma_\tau \text{Treated}_s \times \text{D}(t,\tau) + \delta_c + \delta_{p,t} + \epsilon_{c,t},$$

where the $y_{c,t}$ is either the natural logarithm of *Hourly wage employment* (total, non-tradable sector, and tradable sector), *Employment earning less than or equal to \$10 or \$20 per hour*, and *Minimum wage employment* for county $c$ in year $t$, $\delta_c$ are county fixed effects, $\delta_{p,t}$ are border county pair $\times$ quarter fixed effects, $\text{Treated}_s$ is a dummy variable that takes a value one if state $s$ is a treated state, and $D(t,\tau)$ is a dummy variable equal to one when month $t$ is $\tau$ months from a minimum wage change. The regressions are estimated for the twenty four month period period prior to a minimum wage increase, with the reference period being the first month. The definition of treatment and control states is provided in Section 2.2 of the text. In the figure, the blue dots indicate coefficient estimates for the $\{\Gamma_\tau\}_\tau$'s and the vertical red bars denote confidence 95% confidence intervals. Standard errors are clustered at the state level.

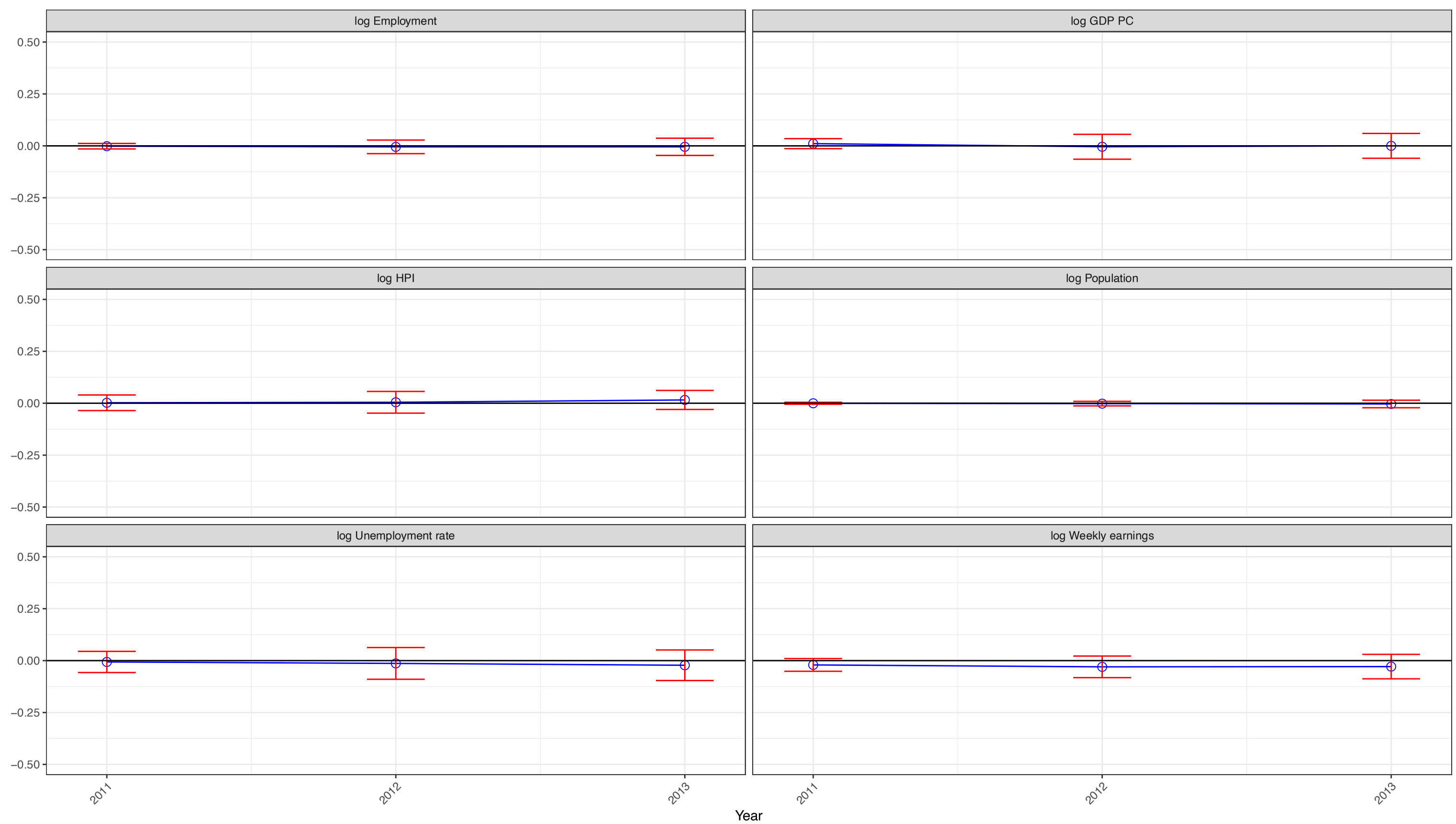


# Figure IA.4: Macroeconomic trends in treated and control states

This figure plots coefficient estimates from dynamic difference-in-difference regressions of the form:

$$y_{s,t} = \alpha + \sum_{\tau \neq 2010} \Gamma_\tau \text{Treated}_s \times \text{D}(t,\tau) + \delta_s + \delta_{tr(s),t} + \epsilon_{s,t},$$

where the $y_{s,t}$ is either the natural logarithm of *Employment*, *GDP PC*, *HPI*, *Population*, *Unemployment rate*, or *Average weekly earnings* for state $s$ in year $t$, $\delta_s$ are state fixed effects, $\delta_{tr(s),t}$ are treated $\times$ year fixed effects, $\text{Treated}_s$ is a dummy variable that takes a value one if state $s$ is a treated state, and $D(t,\tau)$ is a dummy variable equal to one for in year $t = \tau$. The regressions are estimated for the period 2010-2013, with the reference year being 2010. The definition of treatment and control states is provided in Section 2.2 of the text. In the figure, the blue dots indicate coefficient estimates for the $\{\Gamma_\tau\}_\tau$'s and the vertical red bars denote confidence 95% confidence intervals. Standard errors are clustered at the state level.

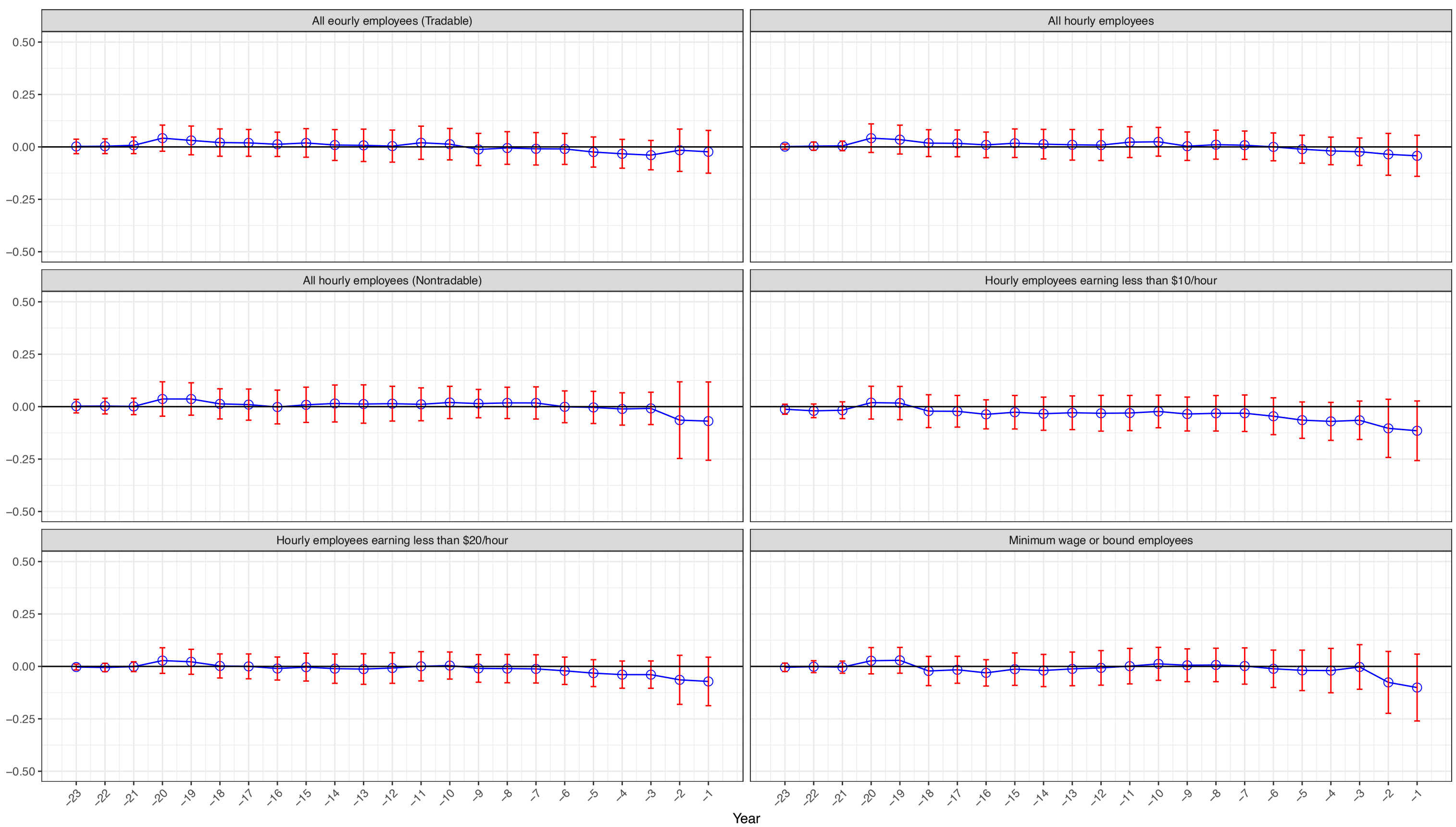


# Figure IA.5: Hourly wage trends in treated and control states

This figure plots coefficient estimates from dynamic difference-in-difference regressions of the form

$$y_{s,t} = \alpha + \sum_{\tau=-23}^{-1} \Gamma_\tau \text{Treated}_s \times \text{D}(t,\tau) + \delta_s + \delta_{tr(s),t} + \epsilon_{s,t},$$

where the $y_{s,t}$ is either the natural logarithm of *Hourly wage employment* (total, non-tradable industry, and tradable goods industries), *Employment earning less than or equal to $10 or $20 per hour*, and *Minimum wage employment*.for state $s$ in year $t$, $\delta_s$ are state fixed effects, $\delta_{tr(s),t}$ are treated $\times$ year fixed effects, Treated$_s$ is a dummy variable that takes a value one if state $s$ is a treated state, and $D(t,\tau)$ is a dummy variable equal to one when month $t$ is $\tau$ months from a minimum wage change. The regressions are estimated for the twenty four month period period prior to a minimum wage increase, with the reference period being the first month. The definition of treatment and control states is provided in Section 2.2 of the text. In the figure, the blue dots indicate coefficient estimates for the $\{\Gamma_\tau\}_\tau$'s and the vertical red bars denote confidence 95% confidence intervals. Standard errors are clustered at the state level.

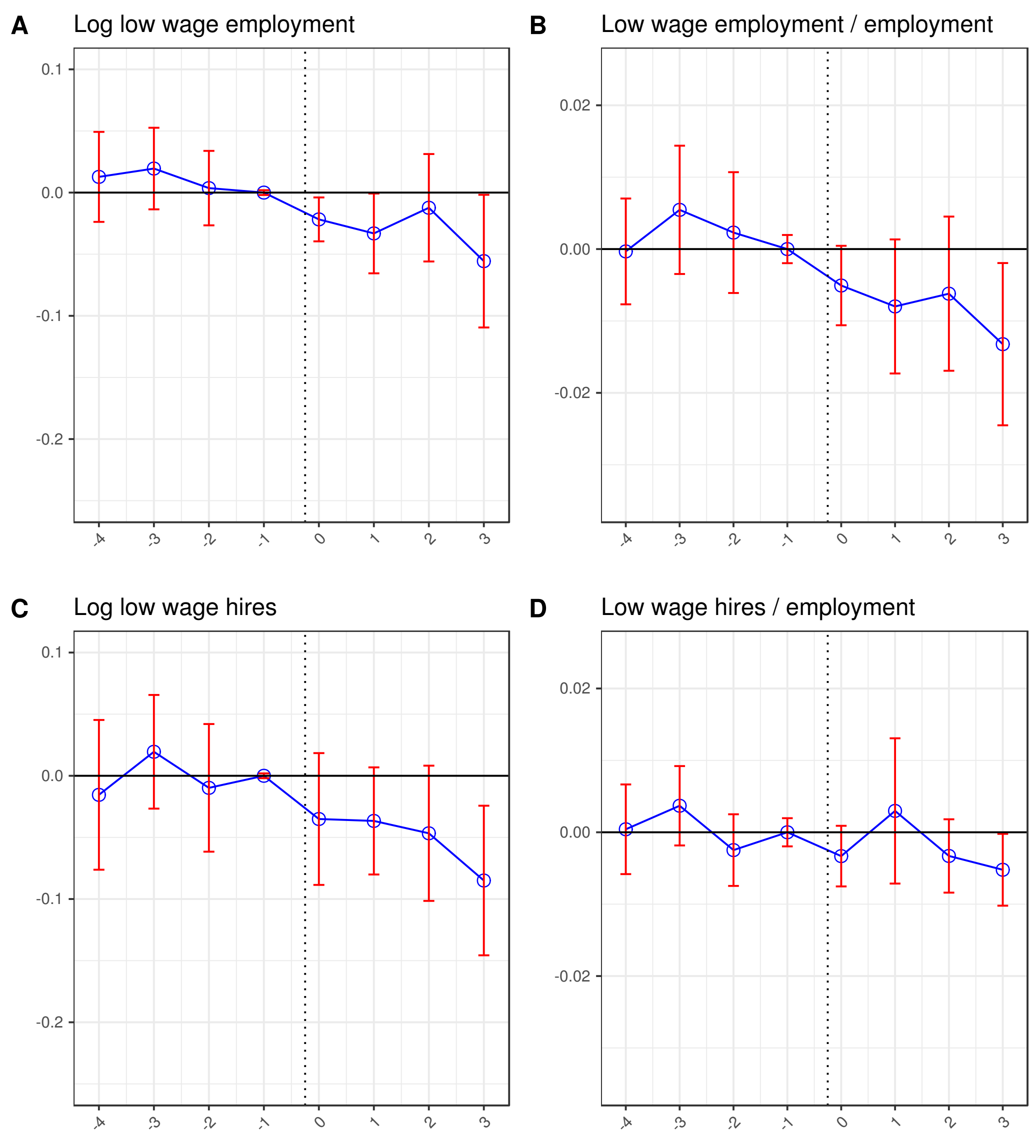


## Figure IA.6: Evolution of establishment employment

This figure plots the coefficient estimates from a dynamic difference-in-differences regression of the form:

$$Y_{f,c,t} = \alpha + \delta_{f,c} + \delta_{p,t} + \delta_{f,t} + \sum_{\tau=-4,\tau\neq-1}^{3} \Gamma_\tau \text{Treated}_s \times \text{D}(s,t,\tau) + \eta' X_{s,t-1} + \varepsilon_{f,c,t}$$

where $\delta_{f,c}$ are firm-county (establishment) fixed effects, $\delta_{p,t}$ are border county pair $\times$ month fixed effects, $\delta_{f,t}$ are firm $\times$month fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{f,c,t}$, is either a: (1) the fraction of low-wage employment to lagged total employment (LowWage / Total), (2) the logarithm of low-wage employment (log(LowWage)), (3) the fraction of low-wage hires to lagged total employment (LowWageHires/Total), or (4) the natural logarithm of low-wage hires(log(LowWageHires)) at establishment $f, c$ in month $t$. The variable $\text{Treated}_s$ is an indicator equal to one if state $s$ is treated, and $D(s, t, \tau)$ is a dummy variable equal to one for all individuals in state s, $\tau$ quarters relative to the treated quarter. In the figure, the $x$-axis indicates the number of quarters ($\tau$) from a minimum wage increase in event time. The blue dots in the figure correspond to the estimates of the $\Gamma$ coefficients, where the quarter corresponding to $\tau = -1$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals Standard errors are clustered at the county level.

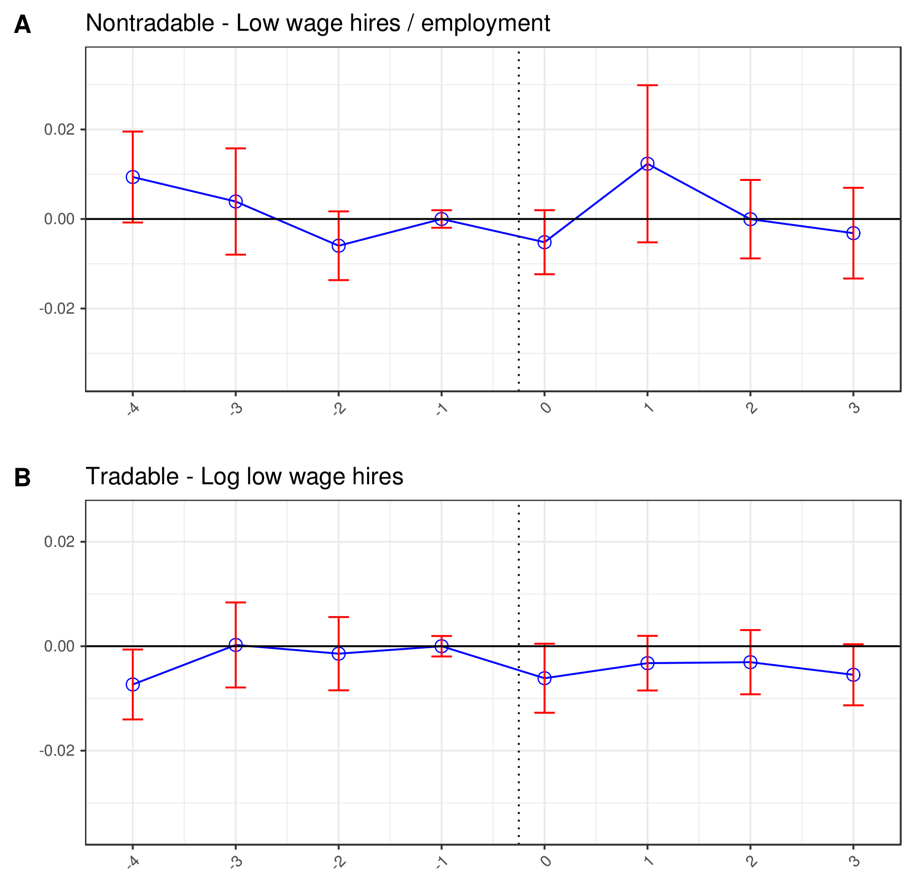


# Figure IA.7: Evolution of establishment employment: non-tradable vs. tradable

This figure plots the coefficient estimates from a dynamic difference-in-differences regression of the form:

$$Y_{f,c,t} = \alpha + \delta_{f,c} + \delta_{p,t} + \delta_{f,t} + \sum_{\tau=-4,\tau\neq-3}^{3} \Gamma_\tau \text{Treated}_s \times \text{D}(s,t,\tau) + \eta' X_{s,t-1} + \varepsilon_{f,c,t}$$

where $\delta_{f,c}$ are firm-county (establishment) fixed effects, $\delta_{p,t}$ are border county pair $\times$ month fixed effects, $\delta_{f,t}$ are firm $\times$month fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{f,c,t}$, is the fraction of low-wage hires (scaled by lagged total employment) at establishment $f, c$ in month $t$. The variable $\text{Treated}_s$ is an indicator equal to one if state $s$ is treated, and $D(s,t,\tau)$ is a dummy variable equal to one for all individuals in state s, $\tau$ quarters relative to the treated quarter. In the figure, the $x$-axis indicates the number of quarters ($\tau$) from a minimum wage increase in event time. The blue dots in the figure correspond to the estimates of the $\Gamma$ coefficients, where the quarters corresponding to $\tau = -1$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals. Standard errors are clustered at the county level.



# Internet Calculation Appendix - Not Intended for Publication

## IC.1 Incumbent spillover effect

This section describes how we calculate the magnitude of wage spillovers for incumbent employees. We use the following notation:

1. Wage bins $b \in \{-1, 0, ...., 19\}$.
2. Bins below new minimum wage: $b \in \{-1, 0\}$.
3. Spillover region: $b \in \{1, 2, 3\}$.
4. Coefficients on hourly wages in each bin from equation 2: $\Gamma_b$.
5. Number treated individuals in each bin: $N_b$.
6. Average wage in bin $b$ one month prior to treatment: $\bar{\omega}_b$.
7. Average pre-treatment hours in bin $b$: $\bar{H}_b$.
8. Average new minimum wage in treated states (weighted by individuals): $\bar{m}$.
9. Gap between average new minimum wage and average pre-treatment wage (direct effect):

$$G_b = \max\left(\bar{m} - \bar{\omega}_b, 0\right).$$

Given the above the total effect on wages is:

$$\sum_{b=-1}^{3} \Gamma_b \cdot N_b \cdot \bar{H}_b.$$

The total direct effect on wages is:

$$\sum_{b=-1}^{3} G_b \cdot N_b \cdot \bar{H}_b = G_{-1} \cdot N_{-1} \cdot \bar{H}_{-1} + G_0 \cdot N_0 \cdot \bar{H}_0.$$

The total indirect effect on wages is:

$$\sum_{b=-1}^{3} (\Gamma_b - G_b) \cdot N_b \cdot \bar{H}_b.$$

The share of the total wage effect due to the direct effect is:

$$\iota_D = \frac{G_{-1} \cdot N_{-1} \cdot \bar{H}_{-1} + G_0 \cdot N_0 \cdot \bar{H}_0}{\sum_{b=-1}^{3} \Gamma_b \cdot N_b \cdot \bar{H}_b}.$$

The share of total income effect due to the indirect effect is:

$$\iota_I = \frac{\sum_{b=-1}^{3} (\Gamma_b - G_b) \cdot N_b \cdot \bar{H}_b}{\sum_{b=-1}^{3} \Gamma_b \cdot N_b \cdot \bar{H}_b}.$$

Our estimates suggest $\iota_D = 0.795$ and $\iota_I = 0.205$ (table IA.9). The average hourly wage amounts are \$0.16 for the direct effect and \$0.04 for the indirect effect.

## IC.2 Conditional new hire spillover effect

This section describes how we calculate a lower bound on the magnitude of wage spillovers for newly hired employees, conditional upon hiring. We use the following notation:

1. Wage bins $b \in \{-1, 0, ...., 19\}$.
2. Bins below new minimum wage: $b \in \{-1, 0\}$.
3. Spillover region: $b \in \{1, 2, 3\}$.
4. Coefficients on density of hires in each bin from equation 3: $\Gamma_b$.
5. Average bottom wage in bin $b$ one month prior to treatment: $\omega_b$.
6. Average pre-treatment hours in bin $b$: $\bar{H}_b$.
7. Average new minimum wage in treated states (weighted by individuals): $\bar{m}$.

8. Gap between average new minimum wage and lowest pre-treatment wage in bin $b$:

$$G_b = \max\left(\bar{m} - \omega_b, 0\right).$$

Given the above, we first calculate the average wage of new jobs that previously would have paid below the new minimum wage. We assume that all new hires in each bin would be hired at the lowest wage in the bin. The average amount is given by:

$$\bar{\omega}_{\downarrow} = \frac{\sum_{b=-1}^{0} \Gamma_b \cdot \omega_b}{\sum_{b=-1}^{0} \Gamma_b}.$$

Next, we calculate the average wage of new jobs created above the new minimum wage (in the spillover region). This amount is given by:

$$\bar{\omega}_{\uparrow} = \frac{\sum_{b=1}^{3} \Gamma_b \cdot \omega_b}{\sum_{b=1}^{3} \Gamma_b}.$$

New jobs with an average wage of $\bar{\omega}_{\downarrow}$ are never created due to the new minimum wage. These jobs are replaced with new jobs with an average wage of $\bar{\omega}_{\uparrow}$. Jobs beyond the spillover region are unaffected. Thus, the percent change in average wages for new hires due to the minimum wage is:

$$\%\bar{\omega} = \frac{\bar{\omega}_{\uparrow}}{\bar{\omega}_{\downarrow}} - 1.$$

Our estimates suggest $\bar{\omega}_{\downarrow} = 7.80$, $\bar{\omega}_{\uparrow} = 9.18$, and $\%\bar{\omega} = 0.178$ (table IA.10). We next calculate the portion of $\%\bar{\omega}$ due to the direct effect and the spillover effect of the minimum wage. The direct effect is calculated by moving new jobs lost below the new minimum wage up to the new minimum wage, and then calculating the average wage gain (Cengiz et al. [2019]):

$$\Delta\bar{\omega}_D = \frac{\sum_{b=-1}^{0} \Gamma_b \cdot G_b}{\sum_{b=-1}^{0} \Gamma_b}.$$

Dividing $\Delta\bar{\omega}_D$ by $\bar{\omega}_{\downarrow}$ gives the percent change due to the direct effect:

$$\%\bar{\omega}_D = \frac{\Delta\bar{\omega}_D}{\bar{\omega}_{\downarrow}}.$$

Our estimates suggest $\%\bar{\omega_D} = 0.106$ (table IA.10). The percent change due to the indirect effect is:

$$\%\bar{\omega}_I = \%\bar{\omega} - \%\bar{\omega}_D.$$

Our estimates suggest $\%\bar{\omega}_I = 0.072$ (table IA.10). Finally, the share of the percent change due to the direct effect is:

$$\zeta_D = \frac{\%\bar{\omega}_D}{\%\bar{\omega}}.$$

The share of the percent change due to the indirect effect is:

$$\zeta_I = \frac{\%\bar{\omega}_I}{\%\bar{\omega}}.$$

Our estimates suggest $\zeta_D = 0.593$ and $\zeta_I = 0.407$ (table IA.10). Note that since the model is estimated on wage densities, these estimates do not take into account changes in labor demand. If the minimum wage reduces hiring, then the actual effect on wages ($\%\bar{\omega}$) will be smaller.

## IC.3 Total spillover effect

We combine our estimates on incumbents and new hires to estimate the importance of the spillover effect over one year. Our calculation is as follows. First, we multiply the pre-treatment fraction of low-wage employees (52%) by the average incumbent indirect wage increase. Second, we multiply the pre-treatment fraction of low-wage hires that would occur in a year (48%) by the average new hire indirect wage increase. Third, we adjust the hiring wage gains for the reduction in low-wage hires after the minimum wage increase (-4%). Fourth, we divide the sum for incumbent and new hire wages by the average number of low-wage employees if hiring was not affected. Finally, we divide this amount by the same calculation applied to the total wage effect. Our estimate of the share of total labor costs attributable to the indirect effect is thus:

$$\Omega = \left[\frac{(0.52 \cdot 0.04) + (0.48 - 0.04) \cdot 0.56}{0.52 + 0.44}\right] \cdot \left[\frac{(0.52 \cdot 0.20) + (0.48 - 0.04) \cdot 1.39}{0.52 + 0.44}\right]^{-1}.$$

$$= 0.377$$

Our estimate of 37.7 percent is similar to the 39% estimated by Cengiz et al. [2019].

# Internet Data Appendix - Not Intended for Publication

# Data appendix

## General information

Ours is one of the first papers to use Equifax Inc.'s detailed employment data. Hence, in this part of the data appendix, we discuss the source of this data, who uses it and how it gets reported.

Equifax Inc provides employment and income verification services where it acts as an information intermediary between employers and users of the data. Employers are firms that subscribe to these services and outsource employment and income verification of their employees to Equifax. They provide their entire payroll data to Equifax on a payroll-to-payroll basis. Users of the data on the other hand purchase this service to verify employment and income details for individuals for different purposes. For example, lenders are the most common users of this service who use this information to judge the loan applicant's ability to repay debt over and beyond what is reflected by their credit score.

As discussed in the paper, there are over 5,000 employers that subscribe to these services and provide their payroll data to Equifax. These employers in total employ over 30 million employees across the U.S. These firms provide detailed granular information including employee's wages, bonus, commissions, job tenure, and firm level details.

Using this data, Equifax Inc offers two separate products for employment and income verification services - verification of employment (VOE) and verification of employment and income.[44] As part of VOE, the company provides information including employer name and address, headquarters location, job title (when available), employment status, most recent hire date, and length of time with the employer. While with verification of employment and income services, the company in addition to the above listed information, also provides detailed compensation information such as wages, bonuses, commissions and overtime. The customers also have the option to get information on historical pay data, and dates and amounts of the applicant's most recent and projected pay increases.

[44]Description of these services can be found here: https://www.theworknumber.com/verifiers/products/income-and-employment-verification/employment?pageid=Income

Such detailed data helps the lenders to access an applicant's ability to repay debt and allows them to make more informed decisions on loan applications. For instance, particularly for low income individuals, the lenders may benefit from getting more information about the type of job to access the income and employment risks of the applicant over and above the employment status and level of income itself.

In addition to these services, Equifax Inc also provides unemployment insurance claims management services. Specifically, around 25% of all unemployment insurance claims in the U.S. are outsourced to Equifax by large employers. This service ensures that unemployed workers do not receive more benefits than they are entitled. For example, Equifax verifies prior wages and income to ensure that claimants are not overcharging their employer's unemployment insurance account.

The turnover data, particularly the indicators of voluntary and involuntary turnover, that we use in our analysis come from a data set linked to these unemployment insurance management services. The employers that subscribe to these services provide information on all turnover including the terms of separation like date of turnover, whether the turnover was voluntary or involuntary, the reason for turnover if involuntary etc. Equifax uses this data to verify whether a former employee is eligible for unemployment insurance based on the type of separation among other things. For instance, if an employee which voluntarily separated submits an unemployment insurance claim, Equifax will protest the claim with the state agency.

We note that well over 90% of the employers in our sample who subscribe for employment and income verification services, also subscribe to unemployment insurance services and provide separations data. However, in the case that a separation cannot be mapped into a specific type of turnover, then the voluntary and involuntary turnover variables are left as null and the observation is excluded from the sample for the part of the analysis that utilizes types of turnover.

## Comparison to population

In this part of the appendix we compare the employment data we use throughout the analysis to data on the U.S. population as of March 2015. As stated above, our employment data comes from Equifax Inc. The particular database we use is called TheWorkNumber. TheWorkNumber contains information on over 5,000 firms at a monthly frequency. However, we are only authorized to access information on approximately 2,000 of the larger firms for research purposes. In this Appendix, we compare this research sample of data to the U.S. population. Our non-seasonally adjusted employment data on the U.S. population comes from the Bureau of Labor Statistics (BLS) Current Employment Situation (CES) report, and our income and tenure information on the U.S. population comes from the St. Louis Fed's FRED database.

As of March 2015, there were 22.5 million active employee records in our Equifax data sample.[45] This accounts for roughly 20% of the U.S. private non-farm payroll. The employment coverage rate (sample employment/population employment) varies significantly by industry.[46] Figure ID.1 plots the employment coverage rate of our sample across the major industries in the BLS CES report. Our data contains nearly half of all the employees working in the retail trade sector in the United States (48%). Other industries with high coverage rates include utilities (31%) and manufacturing (24%). The median coverage rate across industries is 14%, and industries with coverage rates around the median include transportation and warehousing (21%), finance (20%), education and health (18%), information (14%), leisure and hospitality (14%), professional and business services (14%), and mining and logging (12%). Our data has poor coverage for the wholesale trade (3%), construction (2%), and other services (1%) industries.

Figure ID.2 compares the distribution of employment in our sample to the U.S. non-farm private population. Similar to before, our data is over-weights the retail trade industry and under-weights the wholesale trade, construction, and other services industries. All other industries are represented in a similar proportion to their population weights.[47]. As shown in Figure ID.3, our data is

[45]To be included in our sample, we require that an employee record satisfies a variety of data-quality checks. More information is provided in our replication documents. In addition to active employee records, we also observe hundreds of millions of employment records for separated (inactive) employees. Employees that are separated prior to our sample period are not studied in our analysis.

[46]We use the same level of industry aggregation as the BLS CES report: `https://www.bls.gov/bls/naics_aggregation.htm`.

[47]Ideally, we would also like to compare the number of business establishments in our data to the distribution of business establishments in the quarterly census of employment and wages (QCEW). We are unable to do so, however, because our data does not provide granular enough information on locations. Our most reliable identifiers for a business establishment are at the firm-3 digit ZIP level or higher. In contrast, the QCEW identifies establishments

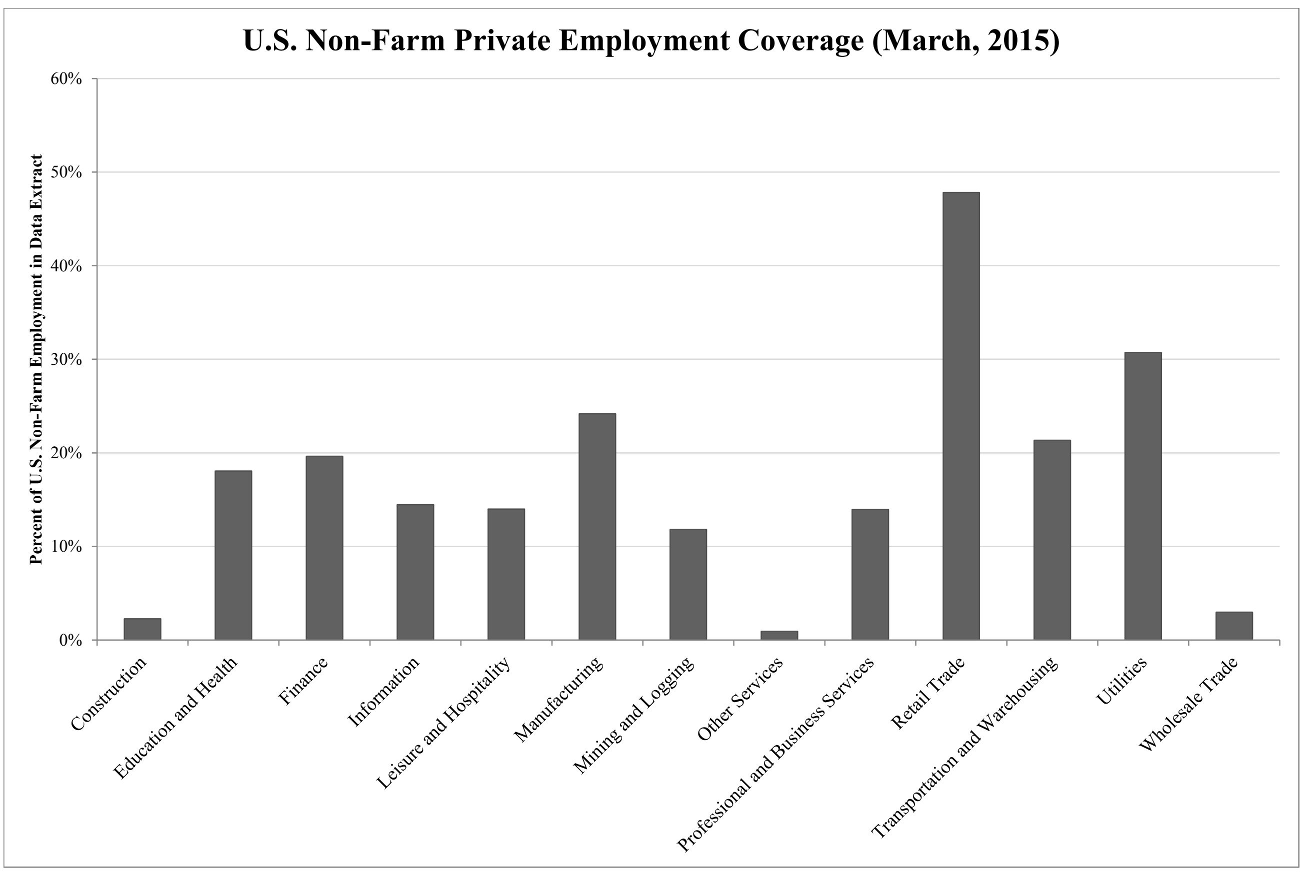
geographically representative of the distribution of employment across U.S. states.

Figures ID.4 and ID.5 compares our data to the U.S. population in terms of income and tenure. The median personal income of employees in our sample is $34,970. This is noticeably larger than the U.S. median personal income of $30,622 in the year 2015. In contrast, the median tenure of the employees in our sample is 3.5 years, slightly lower than the median of 4.2 years for the U.S. population. Finally, with the exception of the District of Columbia, our data matches state-level per-capita personal incomes well (Figure ID.6).

at the traditional level of a single business entity (e.g., two of the same gas station one mile apart are two different establishments in the QCEW).

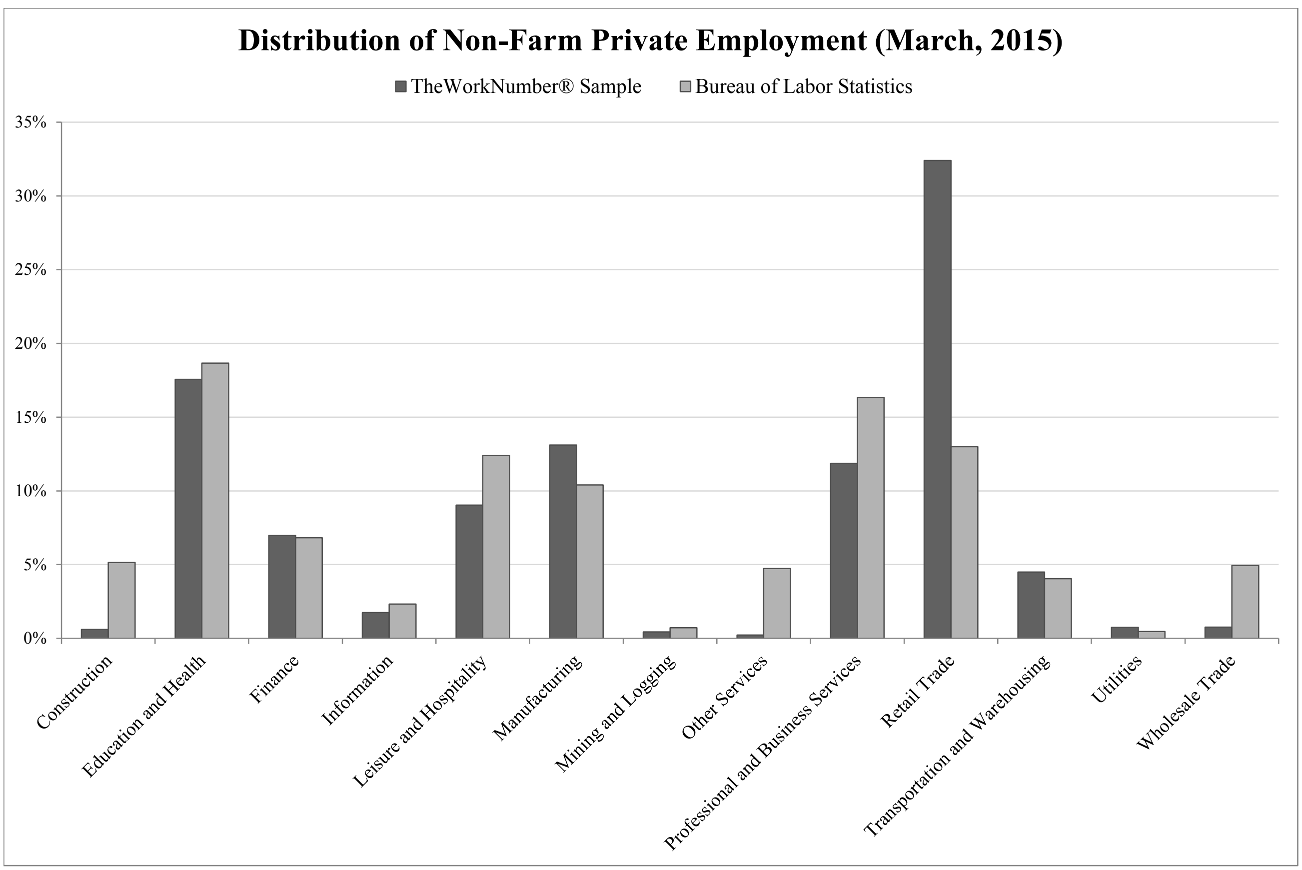
## Figure ID.1: Employment Coverage Across Industries

This figure plots the percent of aggregate employment covered by TheWorkNumber sample. The sample is taken as of March, 2015. Employment coverage is calculated as the fraction of employees in TheWorkNumber sample relative to the aggregate U.S. data, and the overall coverage rate for aggregate non-seasonally adjusted U.S. non-farm private payroll is 19.2%. In the figure, the $x$-axis corresponds to industries. The $y$-axis corresponds to the percent of U.S. non-farm private payroll covered by TheWorkNumber for each industry. Industries, excluding farming and government, are defined using two and three digit NAICS codes as follows: Construction (11), Education and Health (61,62), Finance (52,23), Information (51), Leisure and Hospitality (71,72), Manufacturing (31,32,33), Mining and Logging (11,21), Other Services (81), Professional and Business Services (54,55,56), Retail Trade (44,45), Transportation and Warehousing (48,49), Utilities (22), and Wholesale Trade (42). Data on non-seasonally adjusted U.S. non-farm private payroll is sourced from the Bureau of Labor Statistics "The Employment Situation Report".



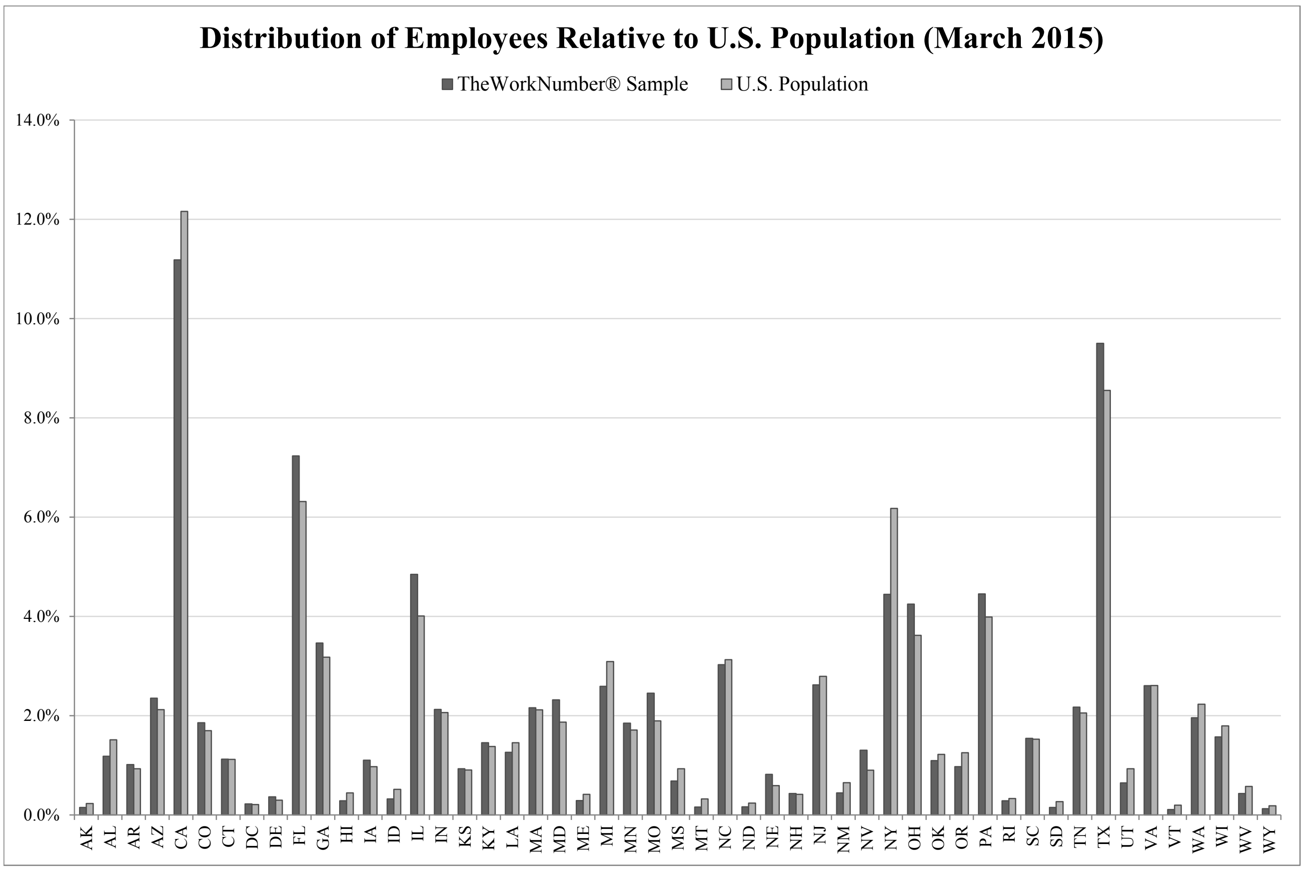
# Figure ID.2: Distribution of Employment Data

This figure compares the distribution of employment across industries in TheWorkNumber sample to the aggregate U.S. non-farm private payroll employment distribution. The data is taken as of March, 2015. The $x$-axis corresponds to industries. The $y$-axis corresponds to the percent of employment in each industry. The distribution is displayed for both TheWorkNumber sample (dark gray bars) and the aggregate U.S. non-farm private payroll (light gray bars). Industries, excluding farming and government, are defined using two and three digit NAICS codes as follows: Construction (11), Education and Health (61,62), Finance (52,23), Information (51), Leisure and Hospitality (71,72), Manufacturing (31,32,33), Mining and Logging (11,21), Other Services (81), Professional and Business Services (54,55,56), Retail Trade (44,45), Transportation and Warehousing (48,49), Utilities (22), and Wholesale Trade (42). Data on non-seasonally adjusted U.S. non-farm private payroll is sourced from the Bureau of Labor Statistics "The Employment Situation Report".



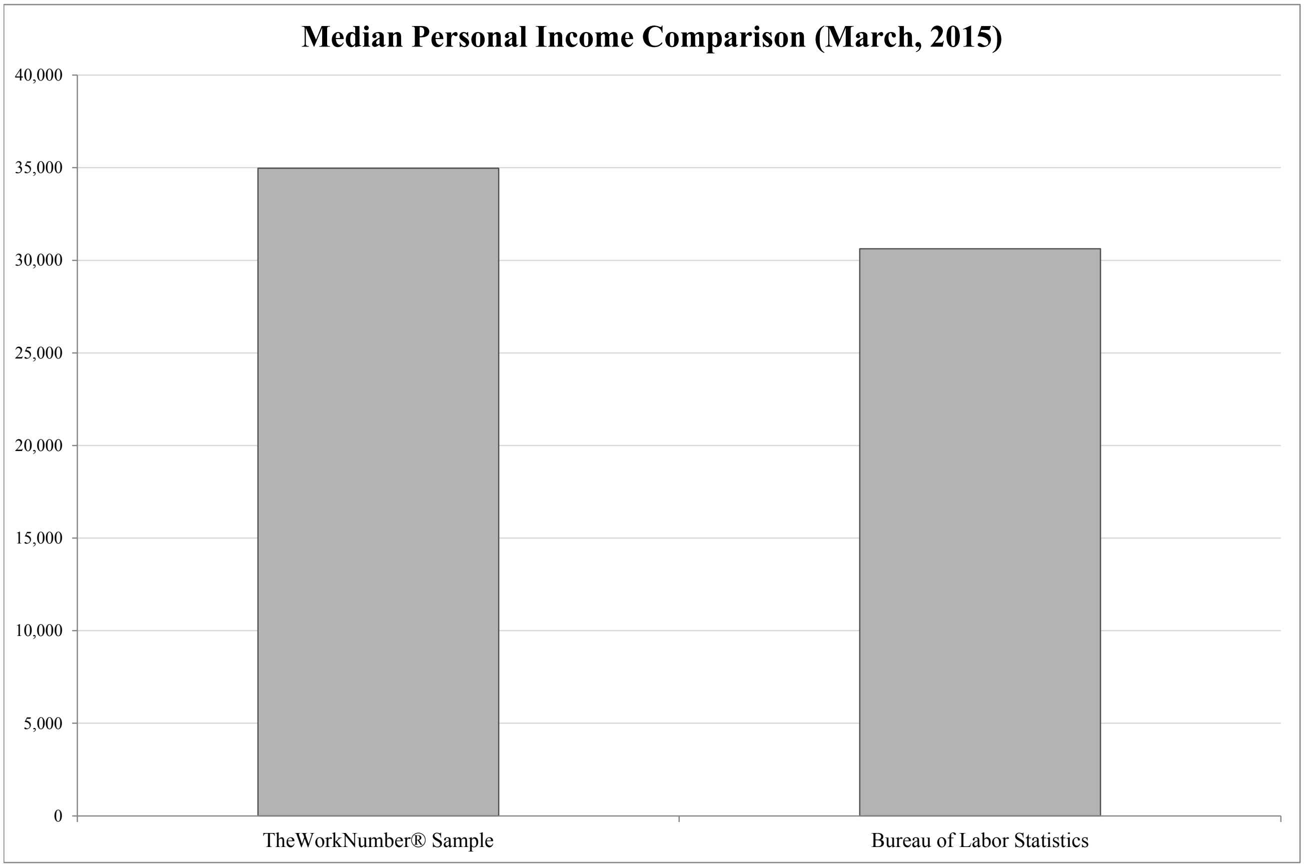
## Figure ID.3: State Distribution of Employment Data

This figure compares the distribution of employment across across in TheWorkNumber sample to the aggregate U.S. population. The data is taken as of March, 2015. The $x$-axis corresponds to states. The $y$-axis corresponds to the percent of employment (or population) in each state. The distribution is displayed for both TheWorkNumber sample (dark gray bars) and the U.S. population (light gray bars). Data on population is sourced from the U.S. Census Bureau.



# Figure ID.4: Median Incomes of Employment Data

This figure compares the median personal income of employees in TheWorkNumber sample to the U.S. population. The sample is taken as of March, 2015 and dollars are in 2015 equivalents. Data on U.S. median personal income is acquired from the St. Louis Federal Reserve database for the year 2015.



## Figure ID.5: Median Tenure of Employment Data

This figure compares the median job tenure of employees in TheWorkNumber sample to the U.S. population. The sample is taken as of March, 2015 and dollars are in 2015 equivalents. Data on U.S. median employee job tenure is acquired from the Bureau of Labor Statistics for the year 2016 (data is only published bi-annually).

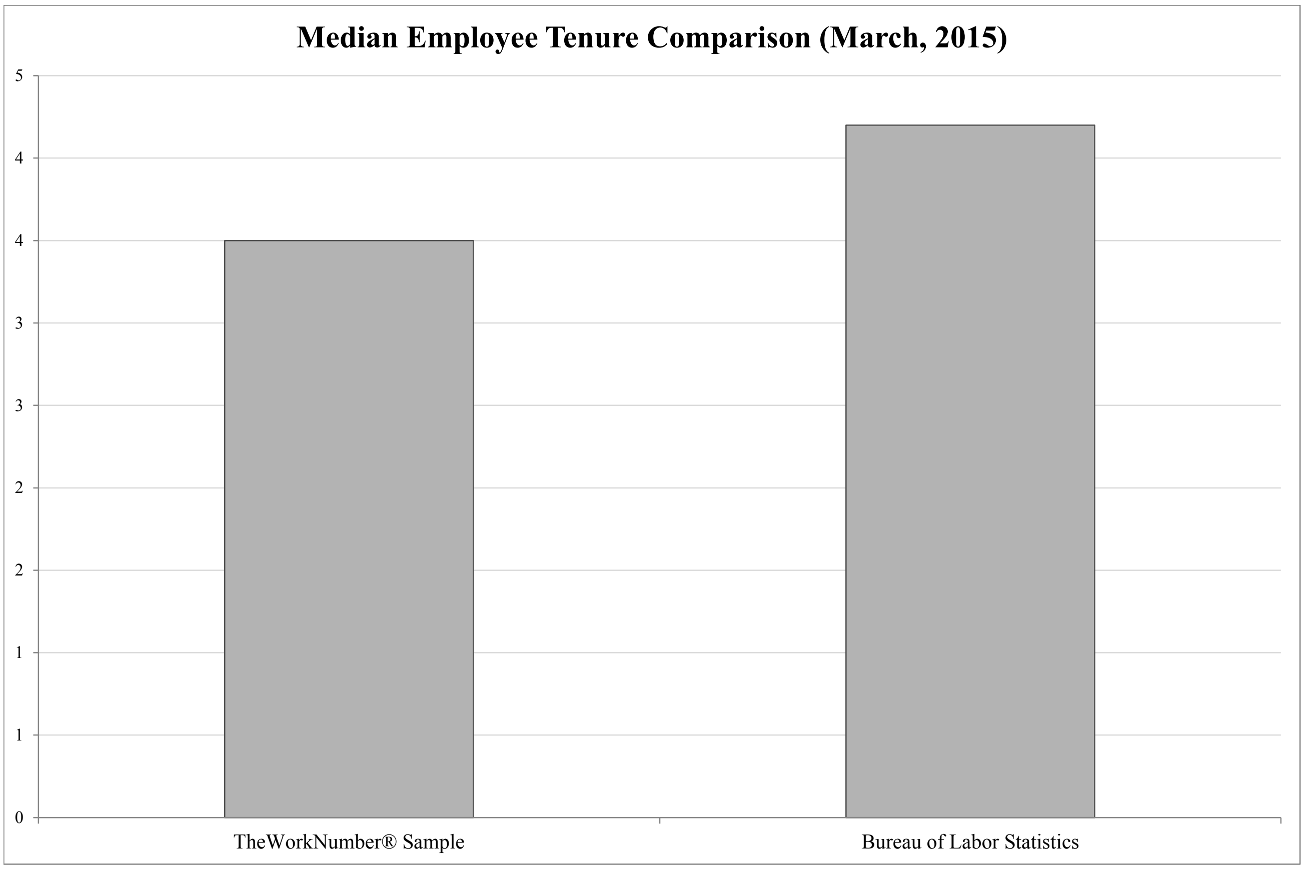
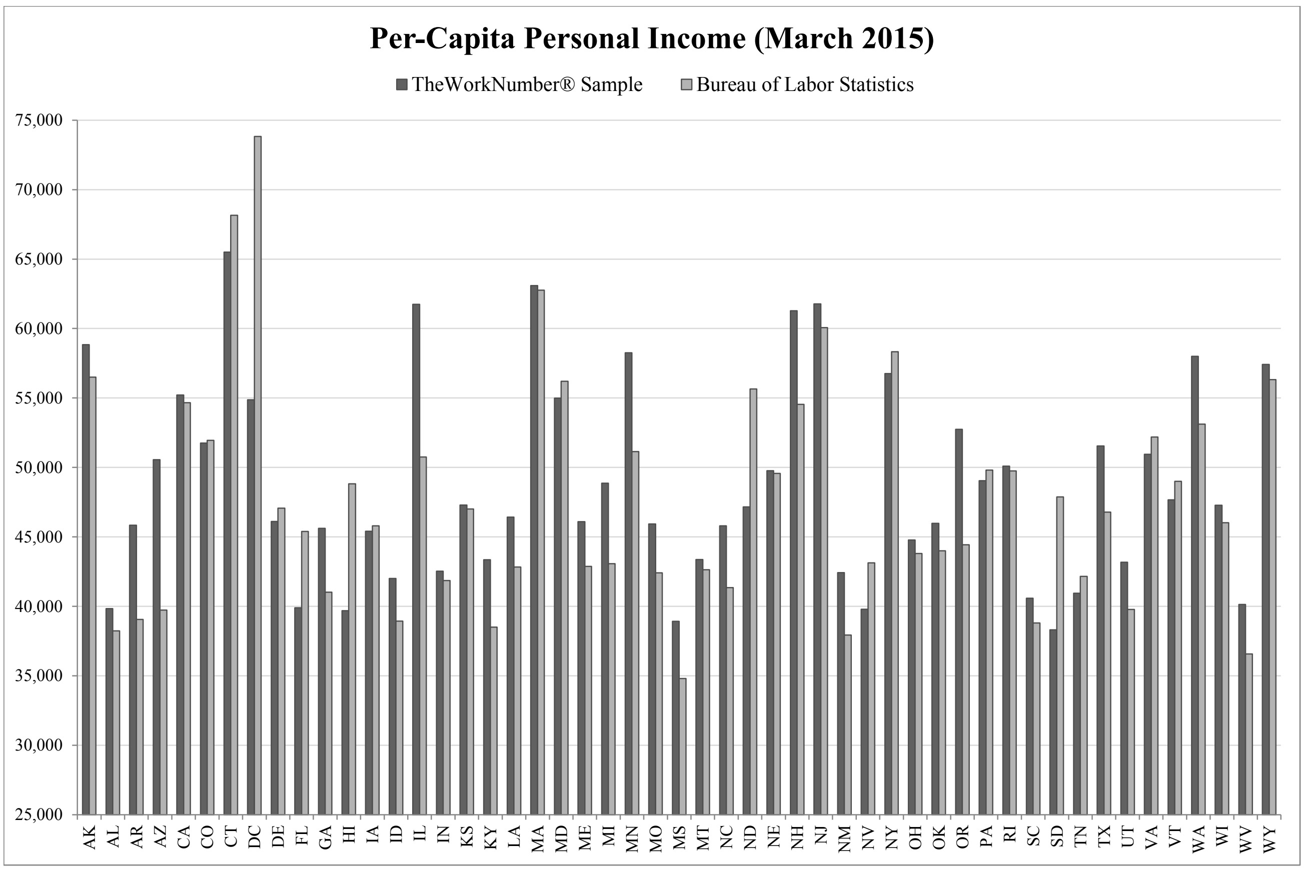


**Figure ID.6: State Per Capita Personal Income of Employment Data**

This figure compares the per-capita personal income of employees across states in TheWorkNumber sample to the aggregate U.S. population. The data is taken as of March, 2015. The $x$-axis corresponds to states. The $y$-axis corresponds to the per-capita personal income in each states. For TheWorkNumber, this figure is calculated as the average annual income of employees in the state. The distribution is displayed for both TheWorkNumber sample (dark gray bars) and the aggregate U.S. non-farm private payroll (light gray bars). Data on state per-capita personal incomes is sourced from the St. Louis Federal Reserve database. Note that per-capita personal income differs from median personal incomes.



## Table ID.1: Definition of Non-Tradable and Tradable Goods Industries

This table provides a mapping between three-digit NAICS codes and types of goods industries (non-tradable and tradable). The mapping is adopted from Mian and Sufi [2014].

| Three-Digit NAICS | Industry Name | Classification |
|---|---|---|
| 441 | Motor Vehicle and Parts Dealers | Non Tradable |
| 442 | Furniture and Home Furnishings Stores | Non Tradable |
| 443 | Electronics and Appliance Stores | Non Tradable |
| 445 | Food and Beverage Stores | Non Tradable |
| 446 | Health and Personal Care Stores | Non Tradable |
| 447 | Gasoline Stations | Non Tradable |
| 448 | Clothing and Clothing Accessories Stores | Non Tradable |
| 451 | Sport. Goods, Hobby, Mus. Instr., & Book Stores | Non Tradable |
| 452 | General Merchandise Stores | Non Tradable |
| 453 | Miscellaneous Store Retailers | Non Tradable |
| 722 | Food Services and Drinking Places | Non Tradable |
| 211 | Oil and Gas Extraction | Tradable |
| 311 | Food Manufacturing | Tradable |
| 312 | Beverage and Tobacco Product Manufacturing | Tradable |
| 315 | Apparel Manufacturing | Tradable |
| 322 | Paper Manufacturing | Tradable |
| 323 | Printing and Related Support Activities | Tradable |
| 324 | Petroleum and Coal Products Manufacturing | Tradable |
| 325 | Chemical Manufacturing | Tradable |
| 326 | Plastics and Rubber Products Manufacturing | Tradable |
| 333 | Machinery Manufacturing | Tradable |
| 334 | Computer and Electronic Product Manufacturing | Tradable |
| 335 | Elec. Equip., Appliance, and Component Manuf. | Tradable |
| 336 | Transportation Equipment Manufacturing | Tradable |
| 339 | Miscellaneous Manufacturing | Tradable |

## Table ID.2: Definition of Other Goods and Construction Industries

This table provides a mapping between three-digit NAICS codes and types of goods industries (other and construction). The mapping is adopted from Mian and Sufi [2014].

| Three-Digit NAICS | Industry Name | Classification |
|---|---|---|
| 236 | Construction of Buildings | Construction |
| 321 | Wood Product Manufacturing | Construction |
| 444 | Building Mat., Garden Equip., + Supplies Dealers | Construction |
| 531 | Real Estate | Construction |
| 424 | Merchant Wholesalers, Nondurable Goods | Other |
| 454 | Nonstore Retailers | Other |
| 481 | Air Transportation | Other |
| 484 | Truck Transportation | Other |
| 485 | Transit and Ground Passenger Transportation | Other |
| 486 | Pipeline Transportation | Other |
| 488 | Support Activities for Transportation | Other |
| 492 | Couriers and Messengers | Other |
| 512 | Motion Picture and Sound Recording Industries | Other |
| 515 | Broadcasting (except Internet) | Other |
| 517 | Telecommunications | Other |
| 518 | Data Processing, Hosting, and Related Services | Other |
| 522 | Credit Intermediation and Related Activities | Other |
| 523 | Securities, Commodity Contracts, and Other Inv. | Other |
| 524 | Insurance Carriers and Related Activities | Other |
| 532 | Rental and Leasing Services | Other |
| 551 | Management of Companies and Enterprises | Other |
| 561 | Administrative and Support Services | Other |
| 562 | Waste Management and Remediation Services | Other |
| 611 | Educational Services | Other |
| 621 | Ambulatory Health Care Services | Other |
| 622 | Hospitals | Other |
| 623 | Nursing and Residential Care Facilities | Other |
| 624 | Social Assistance | Other |
| 713 | Amusement, Gambling, and Recreation Indus | Other |
| 721 | Accommodation | Other |
| 812 | Personal and Laundry Services | Other |
| 813 | Religious, Grantmaking, Civic, Etc. | Other |