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 that significant heterogeneity exists 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.

Keywords: Minimum wage, labor economics, employment, wages

JEL Classification Numbers: J01, J23, J38, H11

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] and Neumark and Wascher [2007]), several aspects remain under-examined. One reason for this is data availability. Most studies lack longitudinal data on exact employee wage rates. This makes it difficult to quantify certain dimensions of the wage effect, such as spillovers (Autor et al. [2016]). Alternatively, to improve data quality, other studies confine the analysis to a single employer or industry. However, imposing such restrictions may mask potentially 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 our analysis on six large, recent state-level minimum wage increases ($\geq$ \$0.75 per hour) with well-defined pre-and-post intervention windows.[1] These increases occurred in California, Massachusetts, Michigan, Nebraska,

[1]These are the minimum wage changes for which "clean" variation exists during our sample period – i.e., those

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 empirical analysis by examining the effect of the minimum wage on incumbent wages. For directly affected incumbent employees, we confirm that the minimum wage raises hourly wages in the expected manner.[5] 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. However, this average effect masks considerable heterogeneity. We find that incumbent workers with longer firm-specific tenures receive larger hourly wage increases.

that are not immediately preceded or followed by another minimum wage increase.

[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.

[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 just focus on trying to credibly estimate short-run elasticities in all of our analyses.

[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.

[5]Directly affected incumbent workers are those who previously earn less than or equal to the new minimum wage. This test helps establish the quality of our data and the validity of our setting.

Moreover, wage spillovers only occur within establishments that employ a significant fraction of directly affected employees. 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., Big Box Co.) across treated and control counties.[6] We again find evidence of modest wage spillovers extending up to $2.50 above the new minimum wage. Combined with our prior findings for incumbents, this result suggests that wage spillovers could be driven by a combination of search frictions (e.g., Flinn [2006]) and relative pay concerns (e.g., 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 of a disemployment effect. 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 establishment low-wage employment with respect to the minimum wage is -0.43, which is below the "old" consensus range of -0.3 to -0.1 (Brown et al. [1982]).[7]

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 significant 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.

We conclude our analysis by documenting heterogeneity across the nontradable and tradable sectors. While establishments in the tradable sector reduce low-wage hiring following a minimum wage increase, low-wage hiring remains unchanged in the nontradable sector. We find that estab-

[6]By conditioning on these factors, we can alleviate the concern that disemployment effects or substitution towards different types of jobs are driving the observed changes in the wage distribution (Autor et al. [2016]).

[7]This is not exactly an "apples to apples" comparison (Neumark [2018]). First, our outcome variable focuses on a more directly affected subset of the population than most other studies. Second, the estimation sample is comprised of establishments that employ low-wage labor (and not all establishments). We present results for all establishments in the robustness sections.

lishments in both the nontradable and tradable sectors engage in low-wage labor-labor substitution. Specifically, we find that establishments substitute from low-wage, low-skilled (as proxied by age) workers to low-wage, higher-skilled workers following an increase in the minimum wage.

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], and Autor et al. [2016]). A key point of contention in this debate is the magnitude of wage spillovers. We provide the first estimates of wage spillovers based on administrative payroll data.[8] 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 document heterogeneity in the magnitude 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. We also provide some of the first evidence of wage spillovers for new hires.

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 Wither [2019], Harasztosi and Lindner [2019], Monras [2019],and Powell [2019].[9] We contribute to this debate by providing the first large-scale estimates of the employment effect based on administrative payroll data. We document evidence of labor-labor substitution among low-wage hires by examining changes employee ages within the same establishment over time. We also contribute to a growing body of evidence that indicates both a nontradable 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], and Cengiz et al. [2019]).

The remainder of the paper is organized as follows: section 2 provides background on our sample

[8]Most papers identify spillover effects from rightward shifts in the wage distribution. However, several economic forces besides wage spillovers could shift the wage distribution rightward following a minimum wage increase (Autor et al. [2016]). We bypass these concerns by tracking the precise evolution of employee wages over time.

[9]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.

of minimum wage changes, section 3 discusses our data, section 4 examines wages, and sections 5 and 6 examine employment. Section 7 concludes are provides important caveats for interpreting our employment estimates.

2 Institutional background

In this section, we discuss our sample of minimum wage changes and our experimental setting. Our discussion borrows from Clemens and Strain [2017].

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.

In total, 29 states enacted 75 distinct 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, 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.[10] Six states (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.[11] 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. 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.[12] 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 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.[13] 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 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.[14] Figures IA.1 and IA.2 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.3 and IA.4).

[10] States frequently adjust their minimum wage. This limits the number of instances where researchers can extract "clean" variation in the minimum wage – i.e., changes where the pre-and-post periods do not overlap (Dube and Zipperer [2015] and Meer and West [2016]).

[11] We limit our study to the continental United States. This eliminates Alaska from consideration.

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

[13] In most cases, cross-border county pairs are comprised of one treatment and one control county. However, in some cases, our pairs groups 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.

[14] 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.

3 Data and sample selection

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

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.[15]

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 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. Em-

[15]This definition of a worksite location is coarser than the one used by the Bureau of Labor Statistics (BLS). If there are multiple BLS worksites within a three-digit ZIP code, then our measure would only recognize this collection as one location. Therefore, we cannot compare our data to BLS populations statistics for establishments.

ployees are dropped from the individual-level analysis after they separate from their employer. For both analyses, we set the pre-treatment period for a control county to be the same as that of its paired treated county.[16] We discuss our samples in more detail below.

3.2 Individual-level sample

Our individual-level sample consists of all hourly wage employees in treated and control counties. We categorize each employee as either a *bound employee* or a *non-bound employee* based on their pre-treatment hourly wage.[17] 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.[18] Non-bound employees have pre-treatment hourly wages at-or-above the new minimum wage. 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 employees in treated counties are statistically similar to their peers in control counties.

[16]For example, consider West Virginia (a treated state) and Kentucky (an adjacent control state). On January 01, 2015, West Virginia implemented a minimum wage increase of \$0.75. Thus, the pre-treatment period for West Virginia is January 01, 2014 to December 31, 2014. The pre-treatment period for control counties in Kentucky along the West Virginia border is set to January 01, 2014 to December 31, 2014 as well. For the individual-level analysis, employees flow into the sample if they start employment before December 31, 2014.

[17]An employee's pre-treatment hourly wage is their wage in the month closest to three months prior to the treatment date (month -3 in event time). This definition accounts for flow into and out of the sample. For example, if an individual is employed from months -12 to -8, then her pre-treatment wage is her hourly wage rate in month -8.

[18]For control counties, we define the new minimum wage to be the hypothetical minimum wage it would have if it implemented the same increase as its paired treated county. For example, West Virginia enacted a \$0.75 increase to the minimum wage on January 01, 2015. Kentucky is one of the adjacent control states for West Virginia. The new minimum wage for control counties in Kentucky is its current minimum wage plus \$0.75.

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 the initial sample date. Therefore, our estimates capture the effects of the minimum wage on low-wage (and not all) employers.[19] 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 (employees earning less than or equal to $15.00 per hour) comprise 52% (79%) of establishment employment and 29% (55%) of payroll. Establishments in treated counties are observably similarly to establishments in control counties.

The establishments in our sample represent 168 distinct firms, and the median firm has an establishment in 16 (8) border counties (states). Our sample of establishments 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.

[19]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 Data validity

We begin our analysis by estimating wage responses for bound employees. This test serves two purposes: (1) it helps establish the quality of our administrative payroll data and (2) it helps validate our empirical setting. 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, δ_i are employee fixed effects, $\delta_{p,t}$ are cross-border county pair × month effects[20], 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 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 ($\delta_{T,t}$).[21] Standard errors are clustered at the county level.[22]

The coefficient of interest, Γ, measures the average change in wages for bound employees in treated counties relative to adjacent control counties. If our hourly wage data is accurate, then our estimate for Γ should be approximately equal to the weighted average gap between the new minimum wage and the pre-treatment wages of bound employees ($0.45 in our sample).[23] Table IA.7 reports the coefficient estimates. We find that hourly wages increase by $0.486, on average,

[20] These are separate time fixed effects for each cross-border county pair p. They account for time-varying shocks common to each cross-border county pair.

[21] Firm × month effects restrict the identifying variation to within-firm comparisons across treated and control counties. This allows us to flexibly control for time-varying firm and industry-level confounders. These fixed effects will be important in our tests for employment and wage spillovers. To see why, consider a shock that boosts low-wage warehouse and shipping employment nationwide. Suppose also 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 resolve this problem.

[22] 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.

[23] Our estimate may not be precisely equal to $0.45 because of several reasons, including the wage spillovers or non-compliance with the new minimum wage.

for bound employees in the period following a minimum wage change. Figure IA.5 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.[24] These results help validate our data and setting.

4.2 Wage spillovers for incumbents

Our data allows us to estimate wage responses across the entire distribution of hourly wages. Hence, we can examine the magnitude of wage spillovers associated with the minimum wage (Lee [1999]).

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 Δ_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 Δ_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 $[\mathrm{MW}_s + 19 \cdot \Delta_s,\ \infty)$.[25]

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

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

where the dummy variable $\mathrm{Bin}_{b'}$ 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

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

[25] 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.

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 interestingly, we find evidence of positive wage spillovers extending up to three wage bins – or, around \$2.50 — above the new minimum wage.[26] For employees in this "spillover region", average hourly wages increase by \$0.046 per hour with a standard error of \$0.016 (table IA.8).

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 with a standard error of 0.01 (table IA.8). 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]).

Panel C tests for heterogeneity in the spillover effect across the fraction of minimum wage workers in each establishment.[27] 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 intra-firm relative pay concerns, we find that spillovers only occur in establishments 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. 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.9). We also find that the magnitude of the spillover effect is greater for employees with longer firm-specific tenure and in the nontradable sector (table IA.9). 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.[28] Empirically, this task is more challenging because, by construction, we do not observe starting wages for the *same* hires

[26] 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 subtractingMW_s.

[27] 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 of establishments split by the fraction of minimum wage workers.

[28] Search frictions may play a role in generating wage spillovers for newly hired employees (Flinn [2006] and Brochu et al. [2019]).

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]). 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.[29] 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 spillover effects 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. As expected, we also find that the density of new hires decreases (increases) in the wage bins directly below (at) the new minimum wage.

As noted by Autor et al. [2016], several other economic forces besides wage spillovers may cause the wage distribution to shift to the right.[30] 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

[29]Stated differently, this is the number of new hires in wage bin b in county c in month t divided by the total number of new hires in county c in month t

[30]For example, neoclassical labor substitution from low-skilled jobs (e.g., widget maker) to medium-skilled jobs (e.g., widget machinist) would generate observably similar effects on the wage distribution. The same applies for disemployment effects in the lower end of the wage distribution.

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 conditional densities, we implicitly control for neoclassical labor substitution and low-wage 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. Again, we find that the density of new hires decreases (increases) in the wage bins directly below (at) the new minimum wage. We also continue to find a significant increase in the density of new hires for each wage bin throughout the spillover region. However, the coefficient estimates from the model are marginally insignificant.[31] We find no significant changes in the density of new hires in the upper tail of the wage distribution. This is further evidence in favor of our empirical setting (Cengiz et al. [2019]).

5 Individual employment

In this section, we examine the effect of the minimum wage on individual 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:

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

[31] This is likely due to the high-dimensional nature of the estimation.

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 ($H_{i,t}$) of employee i in month t. The outcome variables are defined in full the appendix (table A.2). Standard errors are clustered at the county level.

The coefficient of interest, Γ, 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. Odd (even) numbered columns correspond to models that exclude (include) the bracketed fixed effects. 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 for bound employees ($t = 1.45$). We do not find any economically significant evidence of differential pre-trends across treated and control counties (figure IA.4). However, we note that two of the post-period coefficients are positive and significant at the 95 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 increases, 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 IA.4).[32]

To compare our results to the existing literature, we convert our coefficient estimates into elasticities (table IA.10). We estimate that the elasticity of incumbent employment with respect to the minimum wage is 0.028 ($t = 1.40$). Our estimate of the own-wage elasticity of employment is 0.072 and is statistically significant (t =3.02). Closely related to this, we estimate that the own-wage elasticity of voluntary (involuntary) turnover is -0.059 (-0.005) with a standard error of 0.021 (0.002). Finally, the "implied labor demand" elasticity is 0.069. We emphasize that these estimates

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

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:

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

where the dummy variable 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 b. 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 4 presents the coefficient estimates for employment. As expected, we find no significant effect on the likelihood of employment for bound employees in bins $b = -1$ and $b = 0$. We also find no evidence of 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 for turnover. We find a small, but statistically significant, decrease in voluntary (involuntary) turnover for bound employees in wage bin $b = 0$ ($b = -1$). Across the rest of the hourly wage distribution, turnover responses are small and statistically insignificant.[33] 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.

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

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.11 reports standard errors using alternative clustering methods. We find the effect on bound employees remains statistically insignificant.

Continuous treatment: Table IA.12 re-estimates the model with a continuous measure of treatment for bound employees. We continue to find no significant effects on employment.

Heterogeneity across employees: Tables IA.13 examines heterogeneity in the employment effect across industries. IA.14 examines heterogeneity across employee age. IA.15 examines heterogeneity across employee tenure. We find that the likelihood of employment increases (decreases) in the nontradable (tradable), but that the result is marginally significant (insignificant). In addition, the likelihood of employment for teenagers (high tenure workers) increases (decreases) more than other age (tenure) groups.

Cross-county spillovers: Neumark [2018] notes that cross-border studies may be biased against finding disemployment effects because of spillover effects from worker mobility. To examine whether a violation of the stable unit treatment value assumption is driving our results, we interact our difference-in-differences coefficient with the distance between border county population centers. The idea is that workers are less likely to commute to a state with a higher minimum wage distance if the distance to the state increases. Table IA.16 reports the results. We find that accounting for distance does not reverse 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 effects 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. Variables are defined in full in the appendix (table A.3). Standard errors are clustered at the county level.

The coefficient of interest, Γ, measures the average change in the outcome variable for establishments in treated counties relative to establishments in adjacent control counties. If firm $\times$ month effects are included in the model, then the identifying variation is further restricted to within-firm comparisons. 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.

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 $\times$ month effects are included in the model. We find that total employment declines by 1.0% (0.3%) and is marginally significant (insignificant) when we include (exclude) firm $\times$ month effects.[34] For

[34] In general, our coefficient estimates are larger (in absolute terms) when we include firm $\times$ 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", and therefore are likely to exert a positive bias that works against uncovering disemployment effects (Baskaya and Rubinstein [2012], Neumark et al. [2014], and Powell [2019]).

all of the outcome variables, establishments in treated and control counties trend in a statistically similar manner prior to treatment (figure IA.7). However, we find some evidence of an anticipation effect beginning two quarters prior to treatment.

To compare our results to the existing literature, we convert our coefficient estimates into elasticities (table IA.17). Our estimate of the elasticity of low-wage employees to the minimum wage is -0.43, which 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]).[35] We estimate that the elasticity of total employment with respect to the minimum wage is -0.09 (-0.03) with (without) firm $\times$ month effects and is statistically insignificant. The implied low-wage labor demand elasticity is -1.04, and the implied total labor demand elasticity is -0.38.[36]

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 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.[37]

Panels A and B in figure 5 present the coefficient estimates from the stacked model. 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 low-wage employment remains negative,

[35] This is not exactly an "apples-to-apples" comparison because the underlying populations are different (Neumark et al. [2014] and Neumark [2018]). In particular, we focus on a more directly targeted group of workers than conventional estimates.

[36] Our low-wage employee labor demand elasticity estimates are similar to the estimates in Clemens and Wither [2019] but above the estimates in Jardim et al. [2018]. Our total employment estimates are below Harasztosi and Lindner [2019] but well-above the critical -1 value.

[37] 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.

and it is similar in magnitude to our prior 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). The estimated elasticity of low-wage hires to the minimum wage is -0.49 (table IA.17). We find no statistically significant evidence of differential pre-trends in low-wage hires (figure IA.7).

Panels C and D in figure 5 display coefficient estimates from the stacked model where the natural logarithm and fraction of low-wage hires are the outcome variables. We find that low-wage hiring decreases (increases) in the wage bins directly below (at) the new minimum wage, and that 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 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] and Hamermesh [1987]). Reductions in 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 by around 5% within 12 months if turnover stays fixed. [38]

6.3 Which establishments reduce low-wage hiring?

Theory predicts that the effect of the minimum wage may differ across the nontradable and tradable sectors.[39] To test this prediction, we classify firms into the nontradable, 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 NT_f is equal to one if firm f is in the nontradable sector, and zero otherwise. In the model, the Γ coefficient measures the average relative change in hiring for establishments in the tradable sector. The triple-differences coefficient, β, measures the differential impact on establishments in the nontradable sector. Finally, $\beta + \Gamma$ measures the average relative change in hiring in the nontradable sector.

Table 5 reports coefficient estimates with low-wage hires as the outcome variable. 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 nontradable 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).[40]

In columns 3 through 8, we re-estimate the model with low-wage turnover, average hours, and

[38]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.

[39]In the nontradable 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].

[40]FigureIA.8 displays the dynamics across the nontradable and tradable sectors.

locations as the outcome variables. For establishments in both sectors, we find that the coefficient estimates are uniformly non-different from zero. These tests help us address two alternative explanations for our findings. First, the lack of a response for average hours alleviates the concern that nontradable establishments reduce employment along the intensive margin. Second, the null responses for low-wage turnover, average hours, and worksite locations casts some doubt on the hypothesis that tradable firms are simply reallocating labor across state lines. We note, however, that our empirical design prevents us from definitively ruling out this hypothesis.[41]

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

The absence of a disemployment effect in the nontradable sector may mask significant changes in the composition of low-wage workers (Flinn [2006] and 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 low-wage employee skill and estimate the following stacked model (Clemens et al. [2018]):

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

where the outcome variable, $Y_{f,c,a,t}$, is the density of low-wage hires for age group a (set to five year bin-widths) in establishment f, c in month t.[42] 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 6 displays the coefficient estimates from the model. We find a large, statistically

[41] 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. Moreover, because firms are not constrained by geographic distance (unlike individuals), conducting a "donut design" will not resolve this issue. At-best, we can only conduct falsification exercises, such as showing turnover and locations do not change.

[42] In other words, it is the number of low-wage hires in age group a 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.

significant decline in the share of teenage low-wage hires ($\Gamma_{[15,20)} = -4.15\%$; t=-5.47). Relative to the pre-treatment mean of 27 percent, this coefficient represents a 15 percent decline in the low-wage teenage hiring share. We find that low-wage hiring shares increase across the rest of the age distribution. The greatest increases occur in the young adult age groups ($[20, 25)$ and $[25, 30)$).[43]

Panels B and C re-estimate equation 10 across the nontradable and tradable sectors. In both cases, we find that the share of teenage low-wage hires declines following a minimum wage increase. For the nontradable sector, low-wage hiring shares increase across the rest of the age distribution. These increases are most (least) pronounced in the young adult (older adult) age groups. For the tradable sector, low-wage hiring shares increase in some parts of the age distribution without any discernible pattern.[44]

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.18 reports standard errors using a variety of alternative clustering methods. We find the decline in low-wage employment remains statistically significant in all cases.

Full sample: Table IA.19 relaxes the sample requirement that establishments must employ a significant fraction of low-wage employees. Low-wage employment and hiring still decline. The effect on total employment is negative but statistically insignificant.

Continuous treatment: Table IA.20 re-estimates the model with a continuous measure of treatment. The decline in low-wage employment is larger in establishments with greater 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.

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

[44] Monras [2019] finds thy the working age population for whom minimum wage policies are designed tend to leave or do not move towards states that increase the minimum wage. In contrast, we study the distribution of new hire age conditional on a hiring event.

7 Conclusion

We use 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.

Our administrative payroll data allows us to examine several under-examined aspects of the minimum wage. In particular, we provide some of the first estimates of the magnitude of wage spillovers for incumbent employees. We are also among the first to document how wage spillovers vary in the cross-section (see also Dube et al. [2019]). Importantly, the use of administrative payroll allows us to separate spillover effects from other economic forces that may shift the wage distribution upwards. This resolves several of the concerns noted in Autor et al. [2016] and Brochu et al. [2019].

Our administrative payroll data also allow us to examine the employment effects of the minimum wage in greater detail. In particular, we document evidence of labor-labor substitution among low-wage hires buy examining changes in the distribution of ages within the same establishment over time. We also contribute to a growing body of evidence that indicates both a nontradable versus tradable and a new hire versus incumbent distinction in the employment effect of the minimum wage (e.g., Brochu and Green [2012], Dube et al. [2016], Harasztosi and Lindner [2019], and Dube et al. [2019]).

We conclude by noting that our employment results should be interpreted with the following caveats in mind. First, the existence of a disemployment effect may depend on the initial level of the minimum wage (Jardim et al. [2018]), the size of the wage increase (Clemens and Strain [2017]), the prevailing economic conditions (Clemens and Wither [2019]), and the population of workers under consideration (Neumark [2018]). We estimate our model on a sample of low-wage establishments during a period when the labor market was relatively benign, and the level of the minimum wage was low. The average minimum wage increase in our sample was 12.5%, and we cannot definitively rule out that labor reallocation in the tradable sector drives a large portion of our results. Second, we 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, we cannot speak to the total welfare effects of the minimum wage. For a comprehensive analysis of welfare, please see MaCurdy [2015].

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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 Δ date refers to the year-month (YYYYMM) in which a treated state adjusts its minimum wage in our sample, BOP (EOP) MW refers to the state's minimum wage at the beginning (end) of the sample period, MW Δ 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 Δ date (2)**	**BOP MW (3)**	**EOP MW (4)**	**MW Δ 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 δ_i are individual fixed effects, $\delta_{p,t}$ are cross-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. 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 $\text{Post}_{t,s}$ 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)**	$V_{i,t}$ **(3)**	$V_{i,t}$ **(4)**	$I_{i,t}$ **(5)**	$I_{i,t}$ **(6)**	$H_{i,t}$ **(7)**	$H_{i,t}$ **(8)**
$\text{Treated}_s \times \text{Post}_{t,s}$	-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 $\times$ time FE	Y	Y	Y	Y	Y	Y	Y	Y
Firm $\times$ time FE		Y		Y		Y		Y
Cohort $\times$ time FE		Y		Y		Y		Y
Tenure $\times$ time 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 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 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	LowWage/Total		log(LowWage)		log(Total)	
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
$\text{Treated}_s \times \text{Post}_{t,s}$	-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 $\times$ time FE	Y	Y	Y	Y	Y	Y
Firm $\times$ time 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 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 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 $\times$ 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
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:

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

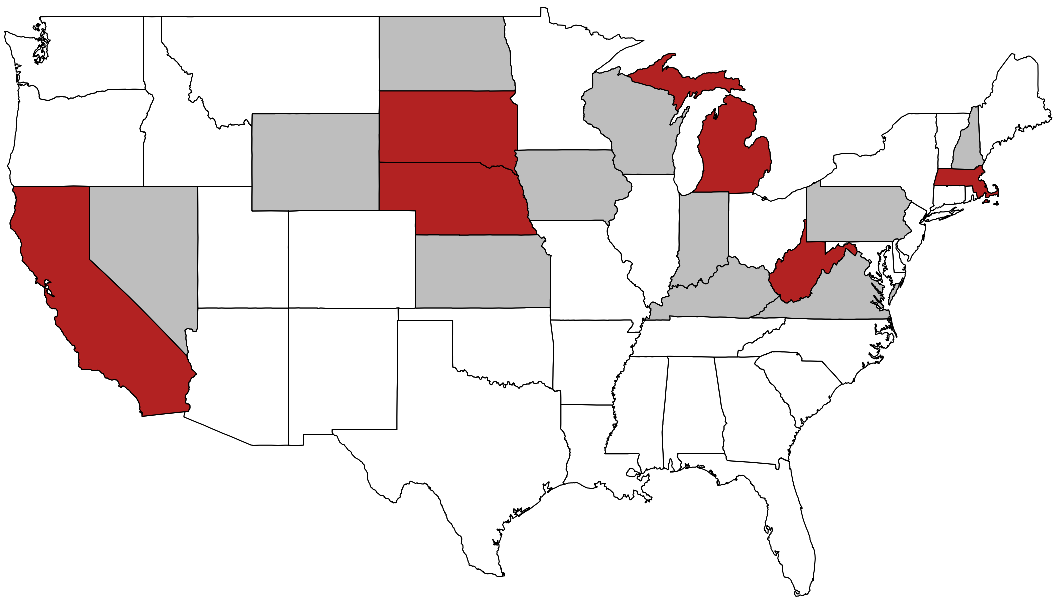
where $\delta_{f,c}$ are firm-county (*establishment*) fixed effects,$\delta_{p,t}$ are cross-border county pair $\times$ month fixed effects, $\delta_{I(f),t}$ are nontradable sector $\times$ month fixed effects, $\delta_{f,t}$ are firm $\times$month fixed effects, and $X_{s,t-1}$ is a vector of state-level control variables, including one quarter lagged realizations of quarterly HPI and GDP PC growth. The variable $\text{NonTradable}_{I(f)}$ is an indicator equal to one if firm f is in the nontradable 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 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 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 $\times$ time FE	Y	Y	Y	Y	Y	Y	Y	Y
Nontradable $\times$ time FE	Y		Y		Y		Y	
Firm $\times$ time 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: 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 border 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

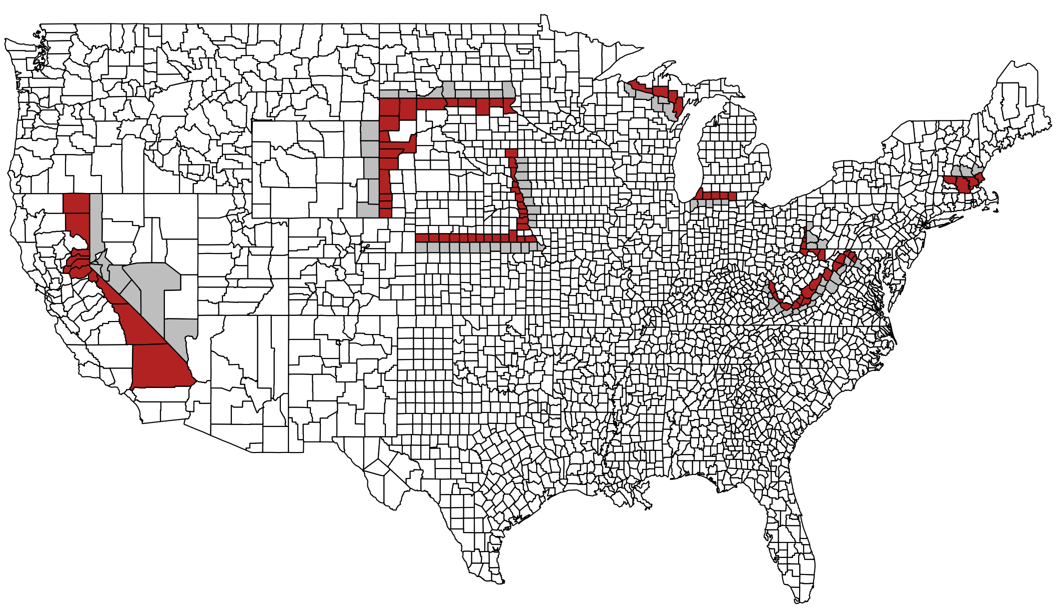


Figure 2: Difference-in-differences regressions: incumbent wages

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 prior tenure. Panel D plots the same heterogeneity across firm exposure to a minimum wage increase.

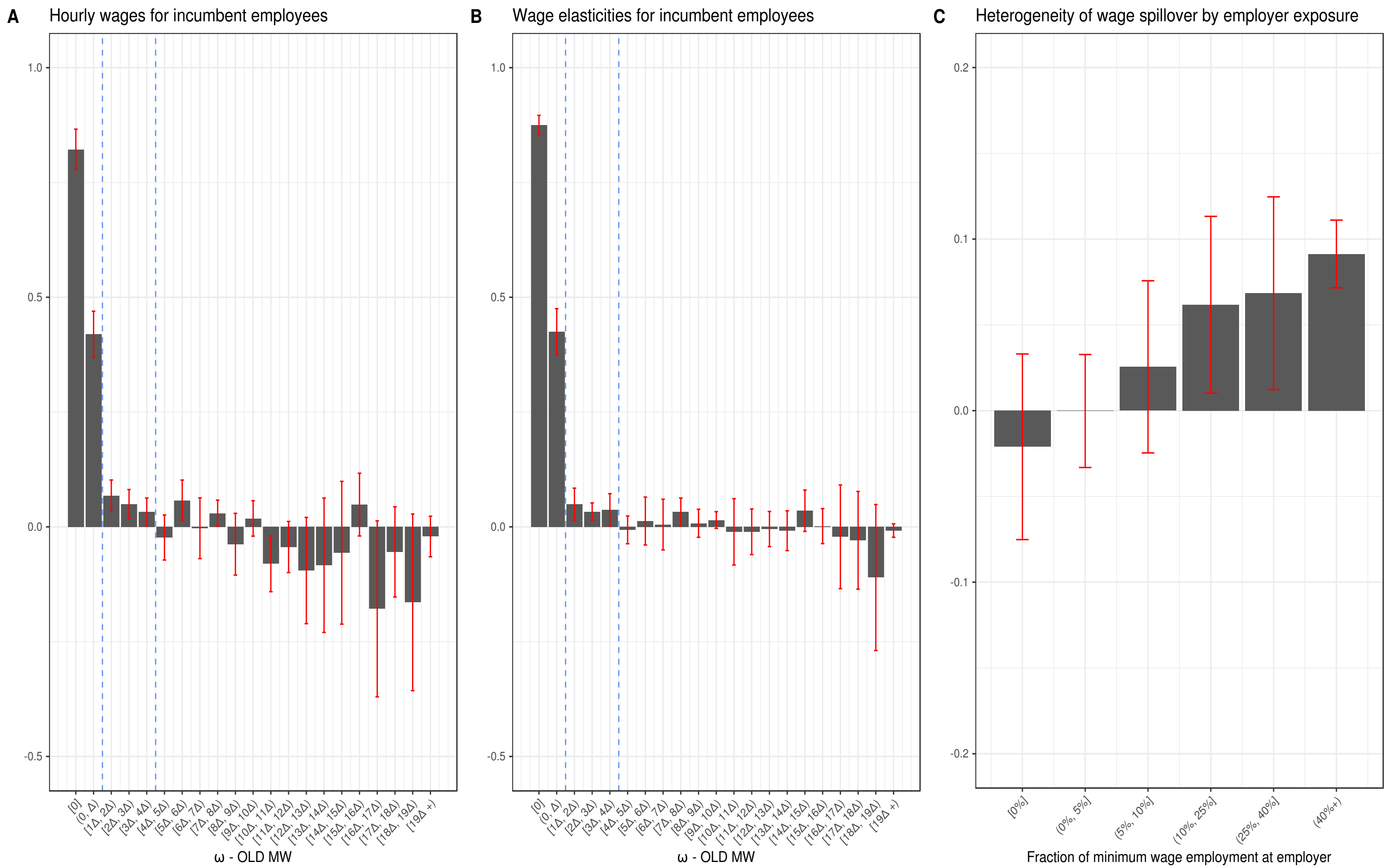


Figure 3: Difference-in-differences regressions: new hire wage densitities

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 panels A and B, measured at the county $\times$ wage bin $\times$ month level, are hires scaled by total county hires (panel A) or teenage hires scaled by total county teenage hires (panel B). The outcome variables in panels C and D, measured at the establishment $\times$ job title $\times$ wage bin $\times$ month level, are hires scaled by total establishment-job title hires (panel A) or teenage 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.

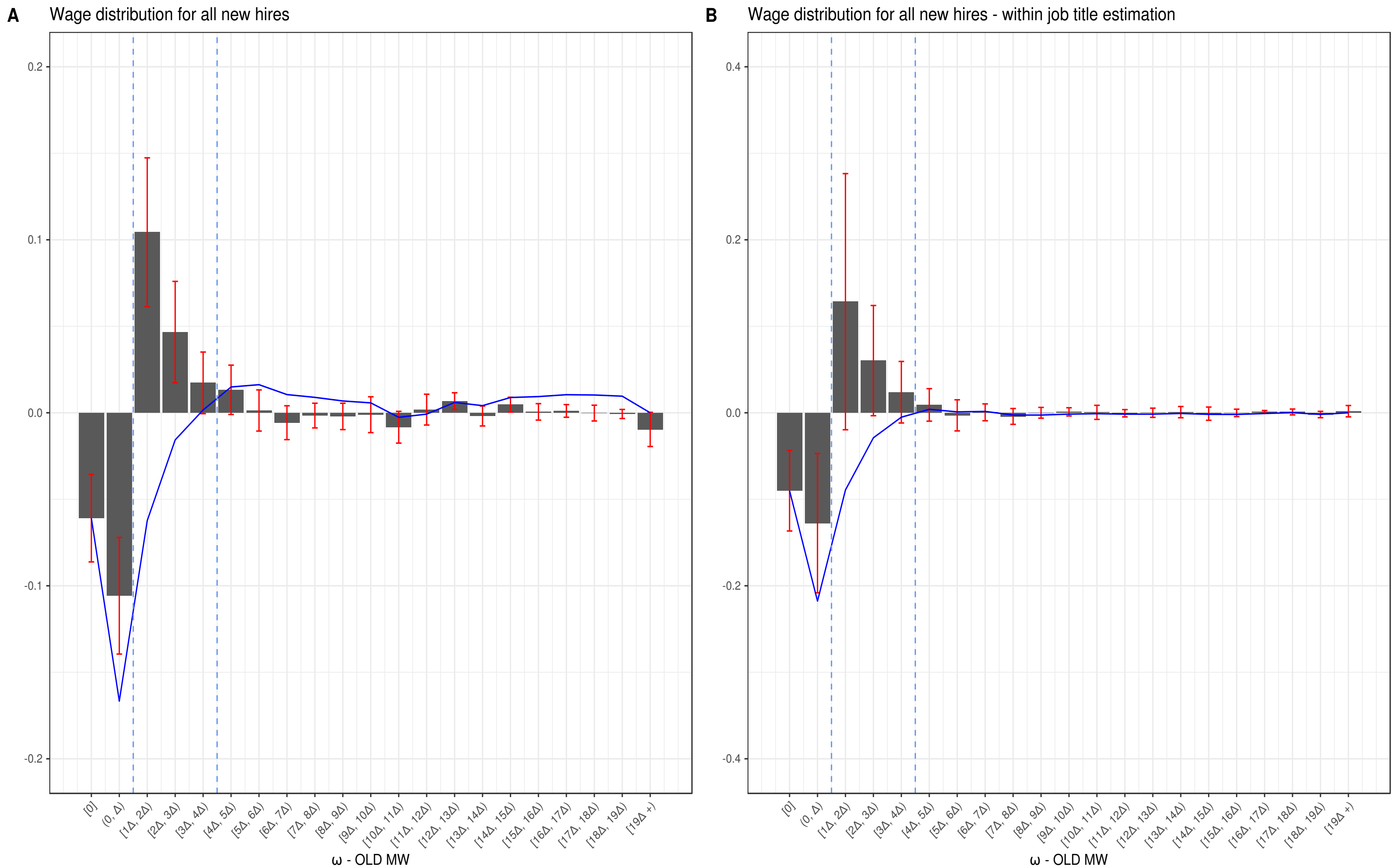


Figure 4: Difference-in-differences regressions: incumbent employment

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 $E_{i,t}$, $V_{i,t}$, $I_{i,t}$ and $H_{i,t}$ in Panels A, B, C, and D, respectively.

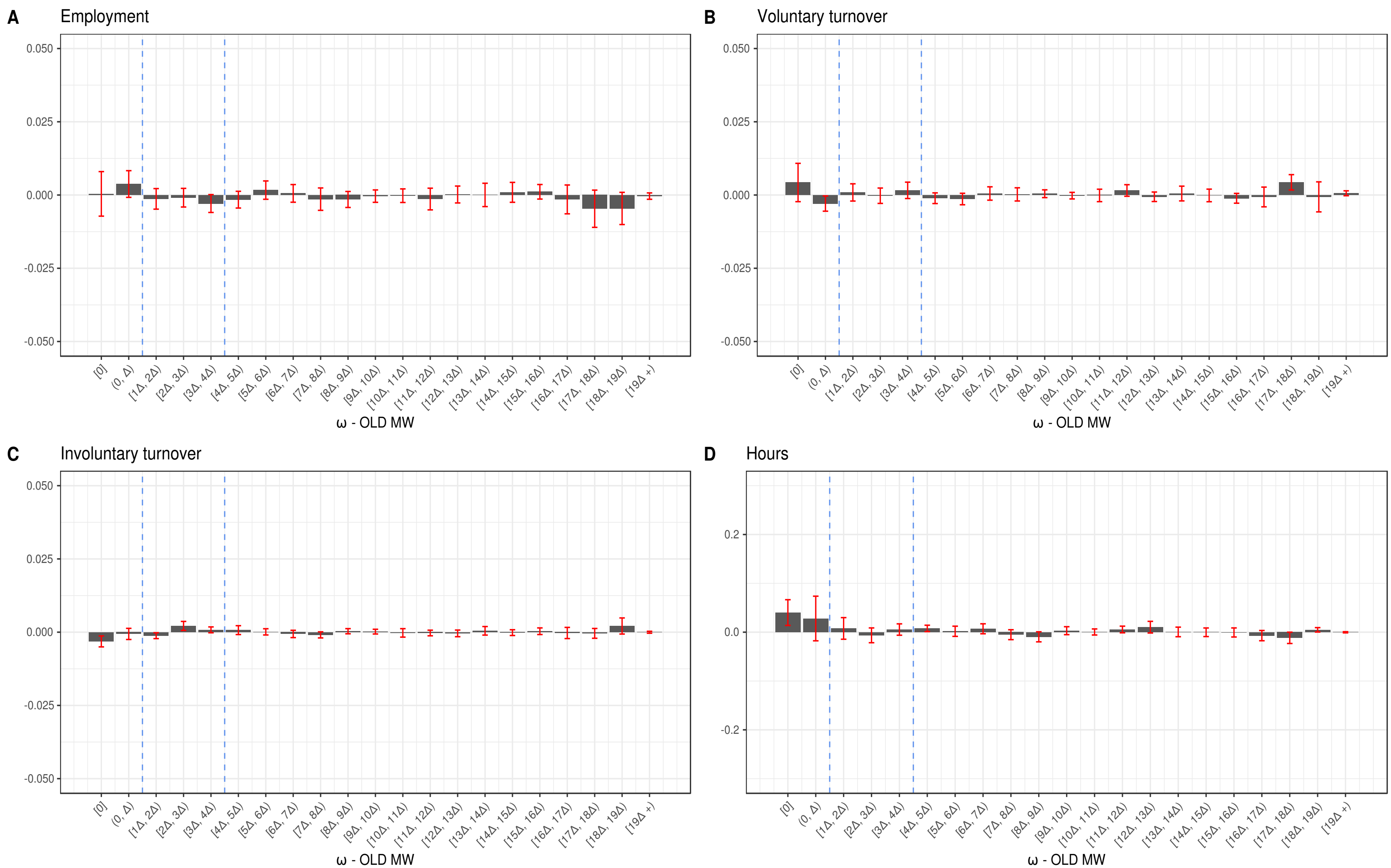


Figure 5: Difference-in-differences regressions: establishment employment

This figure plots coefficient estimates from variations ofequation 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.

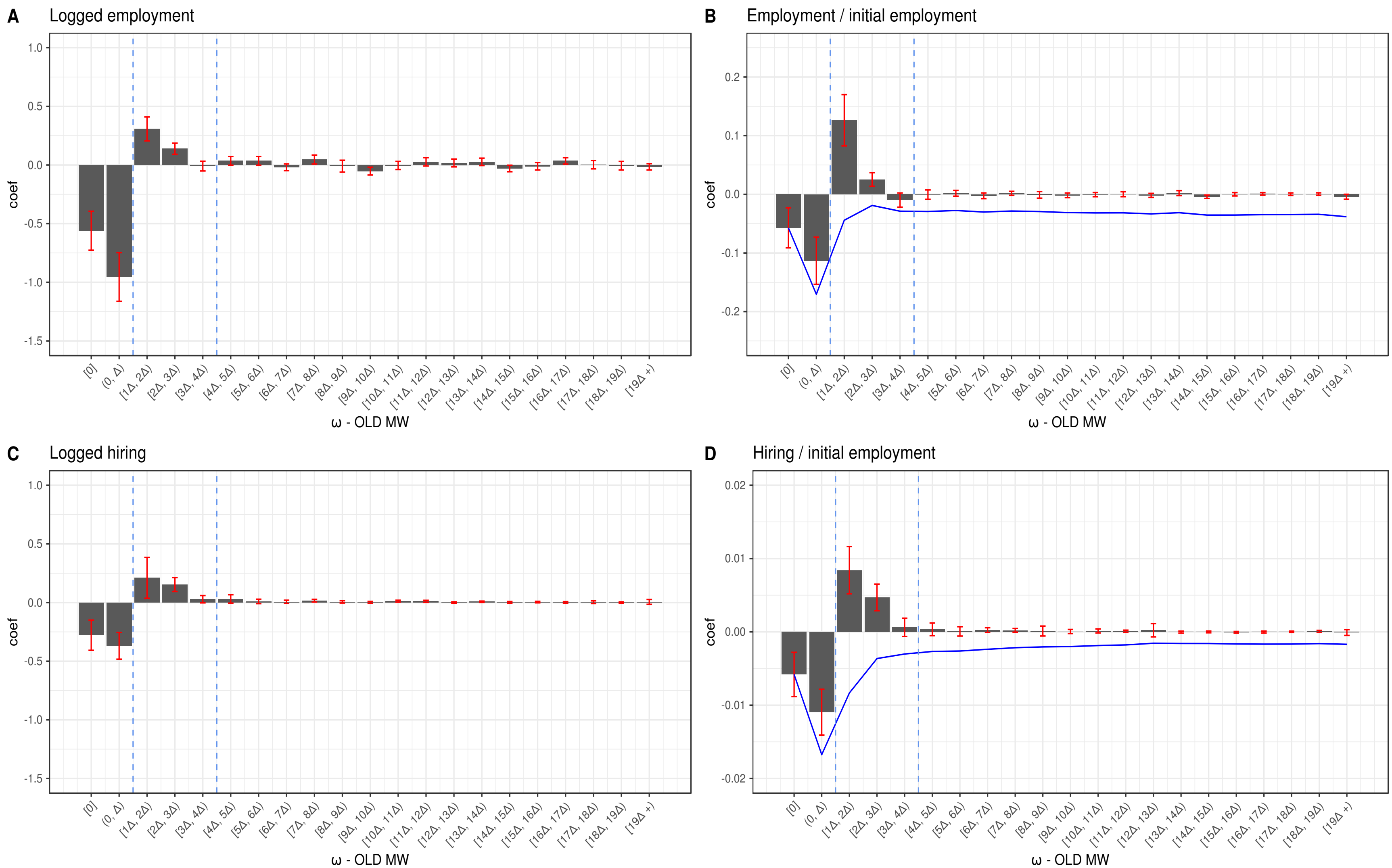
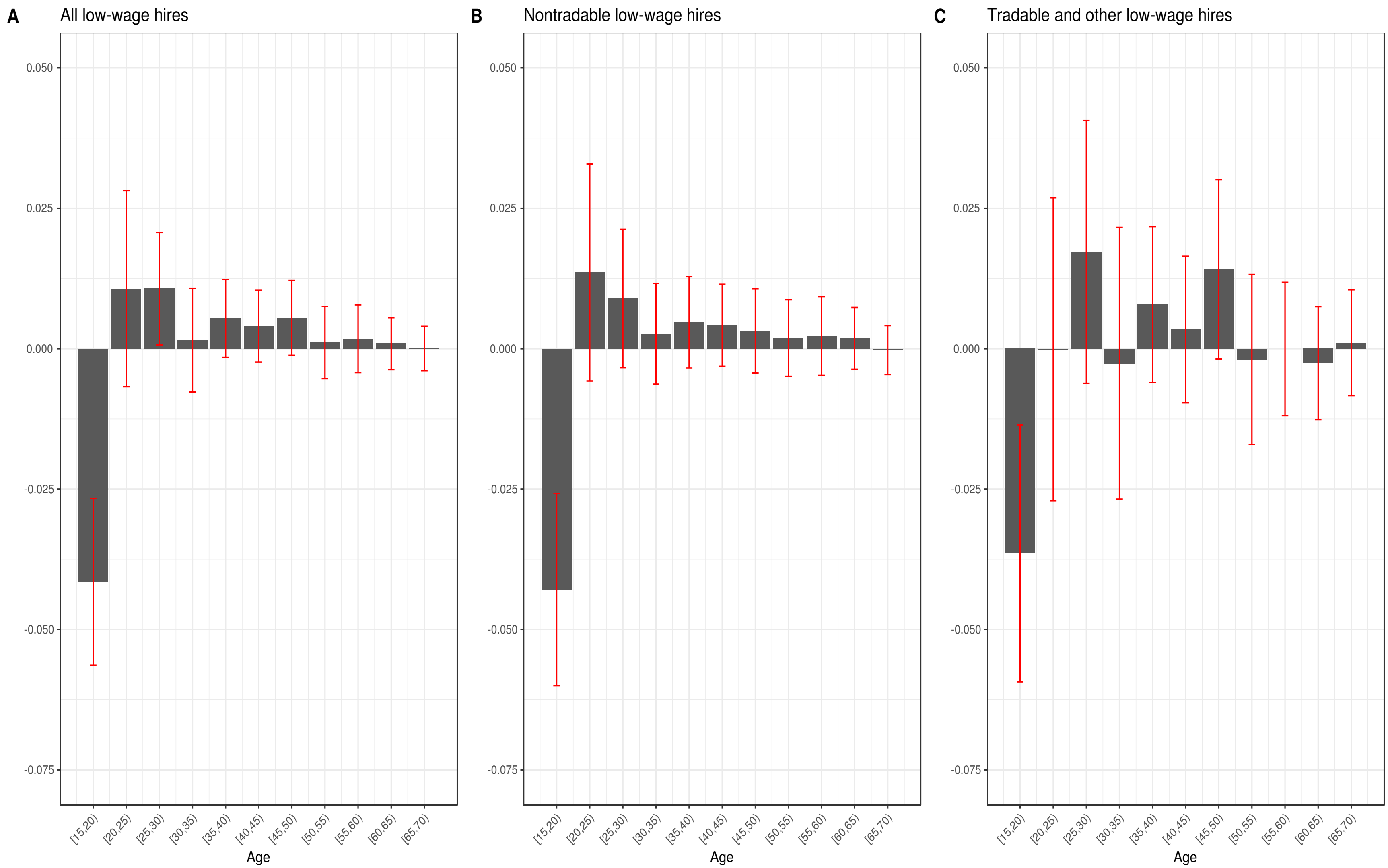


Figure 6: Difference-in-differences regressions: low-wage, new hire age densitities

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 nontradable 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. ω_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 MW_s is the minimum wage of state s in the pre-treatment period. NEW 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 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: $\text{NEW MW}_s = \text{BOP MW}_s + \Delta\text{MW}_s \ \ \forall s \in$ 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 $\omega_i \leq \text{NEW MW} + 3 \times \Delta\text{MW}_{tr(s)}$
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. Hourly wage 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 all 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 at the county level as of the quarter immediately preceding a minimum wage change or a counterfactual 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)
% Nontradable	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 at the county level as of the quarter immediately preceding a minimum wage change or a counterfactual 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)****
% Nontradable	(-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 at the state level as of the quarter immediately preceding a minimum wage change or a counterfactual 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)
$1\{$Turnover $\leq$ 3 months?$\}$	0.26	0.44	0.00	0.00	1.00	0.27	0.25	(0.39)
$1\{$Turnover $\leq$ 6 months?$\}$	0.39	0.49	0.00	0.00	1.00	0.41	0.38	(0.43)
$1\{$Turnover $\leq$ 12 months?$\}$	0.54	0.50	0.00	1.00	1.00	0.56	0.52	(0.54)
$1\{$Turnover by end of sample?$\}$	0.74	0.44	0.00	1.00	1.00	0.77	0.71	(1.41)
$1\{$Voluntary $\mid$ 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.

Variables	**Mean (1)**	**SD (2)**	**P25 (3)**	**P50 (4)**	**P75 (5)**	**T (6)**	**C (7)**	***t*(DIFF) (8)**
Total employees	138	1053	15	30	74	104	174	(-0.64)
% Hourly wageemployees	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:

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

whereδ_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:

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

whereδ_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) $\log(\text{MW})_{s,t}$. 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	$\omega_{i,t}$		$\log(\omega_{i,t})$	
Variables	**(1)**	**(2)**	**(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: Difference-in-differences regression: heterogeneity of spillover effect

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

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

whereδ_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 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 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 nontradable 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.10: Difference-in-differences regression: bound employment elasticities

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

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

whereδ_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 *Spillover* 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 (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 $\times$ time FE	Y	Y	Y	Y	Y	Y	Y	Y
Firm $\times$ time FE	Y	Y	Y	Y	Y	Y	Y	Y
Cohort $\times$ time FE	Y	Y	Y	Y	Y	Y	Y	Y
Tenure $\times$ 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.11: Difference-in-differences regression: Bound employment with alternative clustering

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

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

whereδ_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. 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$_s$ $\times$ 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 $\times$ time FE	Y	Y	Y	Y	Y	Y	Y	Y
Firm $\times$ time FE					Y	Y	Y	Y
Cohort $\times$ time FE					Y	Y	Y	Y
Tenure $\times$ 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.12: Difference-in-differences regression: Bound employment with continuous treatment

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

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

whereδ_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, NEW $\text{MW}_s - \omega_i$, 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 $\times$ 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	884,964	817,172	817,172
R^2	0.38	0.35	0.35

Table IA.13: Difference-in-differences regression: Bound employment across industry groups

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

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

whereδ_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 $\text{Post}_{t,s}$ 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)**
$\text{Treated}_s \times \text{Post}_{t,s}$	0.004*	-0.010
	(1.83)	(-1.39)
Individual FE	Y	Y
County pair × time FE	Y	Y
Firm × time FE	Y	Y
Cohort × time FE	Y	Y
Tenure × time FE	Y	Y
Control variables	Y	Y
Industry	Nontradable	Tradable
N	707,561	177,369
R^2	0.38	0.38

Table IA.14: Difference-in-differences regression: Bound employment across age groups

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

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

whereδ_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 age. The variable 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)**
$\text{Treated}_s \times \text{Post}_{t,s}$	0.013***	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 $\times$ time FE	Y	Y	Y	Y	Y	Y
Firm $\times$ time FE	Y	Y	Y	Y	Y	Y
Cohort $\times$ time FE	Y	Y	Y	Y	Y	Y
Tenure $\times$ 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.15: Difference-in-differences regression: Bound employment across tenure groups

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

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

whereδ_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 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 $\times$ time FE	Y	Y	Y	Y	Y	Y
Firm $\times$ time FE	Y	Y	Y	Y	Y	Y
Cohort $\times$ time FE	Y	Y	Y	Y	Y	Y
Tenure $\times$ 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.16: Difference-in-differences regression: SUTVA for bound employment

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

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

whereδ_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 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$_s$ $\times$ Post$_{t,s}$	0.006**	-0.002	-0.001
	(2.28)	(-0.71)	(-1.00)
Treated$_s$ $\times$ Post$_{t,s}$ $\times$Distance$_i$	0.000	0.000	0.000
	(-1.48)	(0.16)	(-0.94)
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	884,964	817,172	817,172
R^2	0.38	0.35	0.35

Table IA.17: Panel regression: establishment elasticities

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

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

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)**
$\log(\text{MW}_s)$	-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.18: 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	log (LowWage)					
Variables	**(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.19: 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 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.20: 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 $\times$ 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 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 $\text{Post}_{t,s}$ 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 $\times$ 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	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: 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 $y_{c,t}$ is either the natural logarithm of *Hires*, *Separation, Average weekly wages*, *Average employment*, *Average establishments*, or the *Unemployment rate* in county c in quarter t, δ_c are county fixed effects, $\delta_{p,t}$ are border county pair $\times$ quarter 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 for in quarter $t = \tau$. The regressions are estimated for the period 2010-2015, 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.

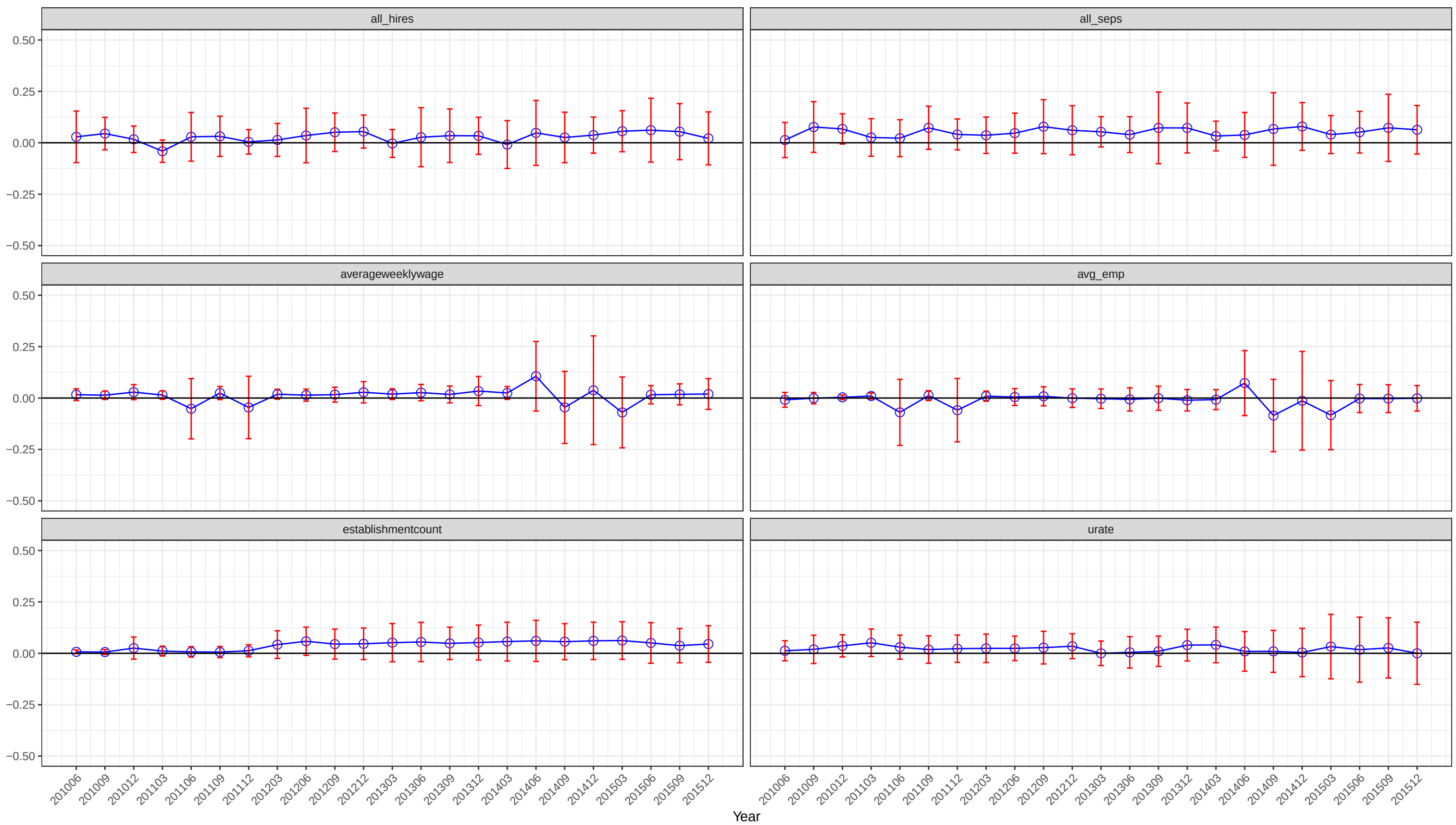


Figure IA.2: Hourly wage trends in treated and control states

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, nontradable industry, and tradable goods industries), *Employment earning less than or equal to $10 or $20 per hour*, and *Minimum wage employment* for county c in year t, δ_c are county fixed effects, $\delta_{p,t}$ are border county pair $\times$ quarter 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 τ 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.

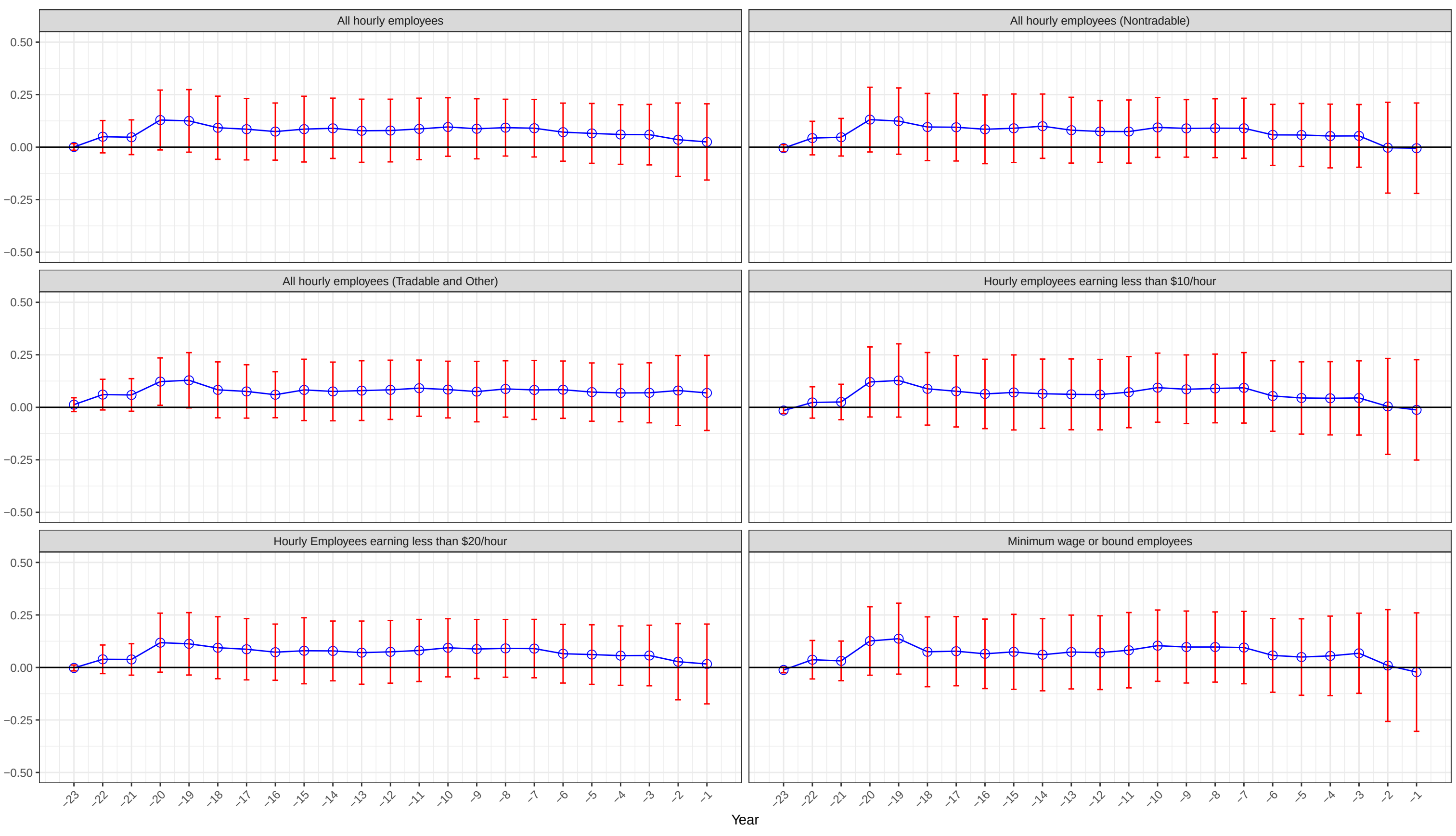


Figure IA.3: Macroeconomic trends in treated and control states

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, δ_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 for in year $t = \tau$. The regressions are estimated for the period 2010-2015, 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.

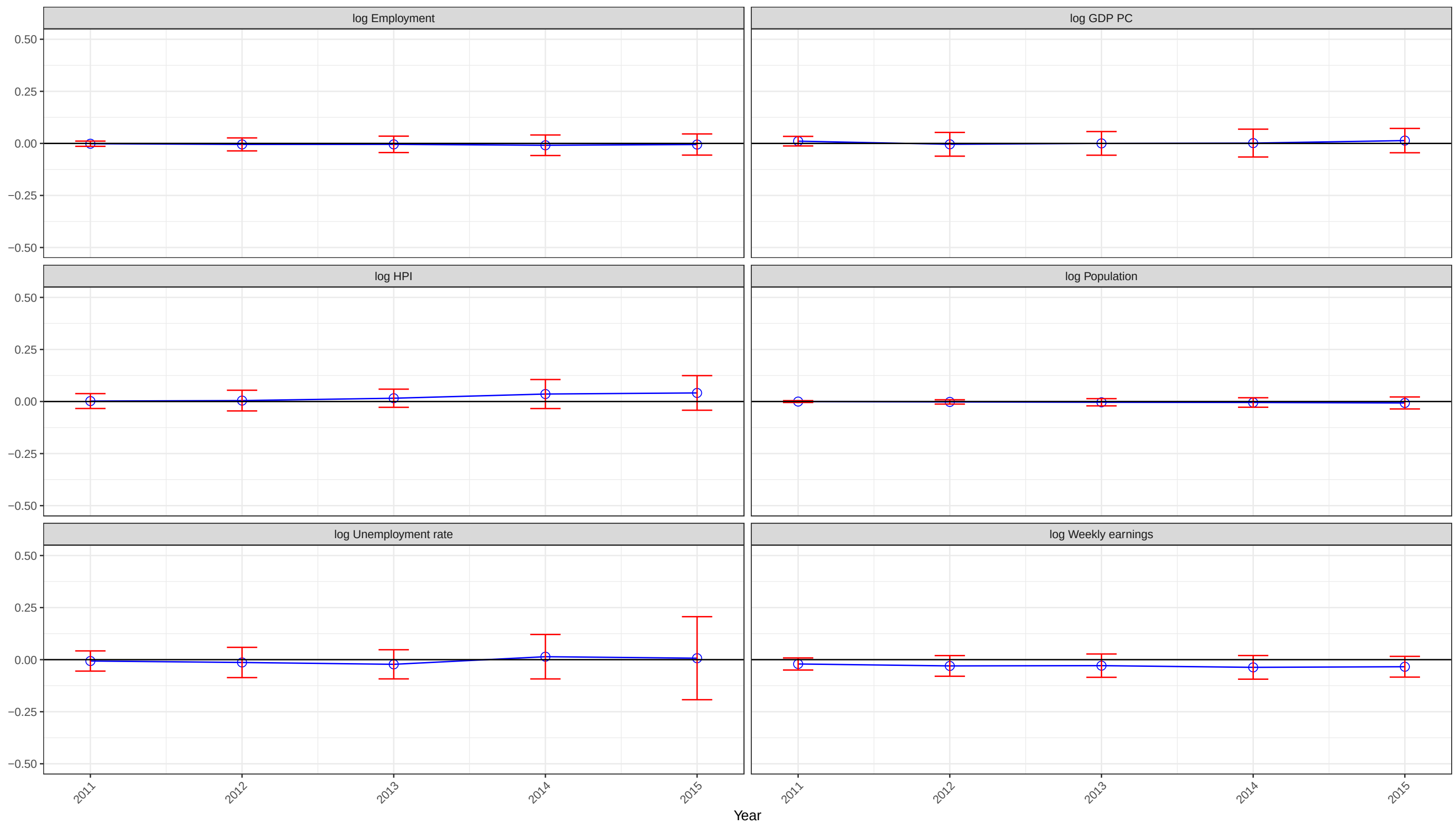


Figure IA.4: Hourly wage trends in treated and control states

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, nontradable 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, δ_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 τ 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.

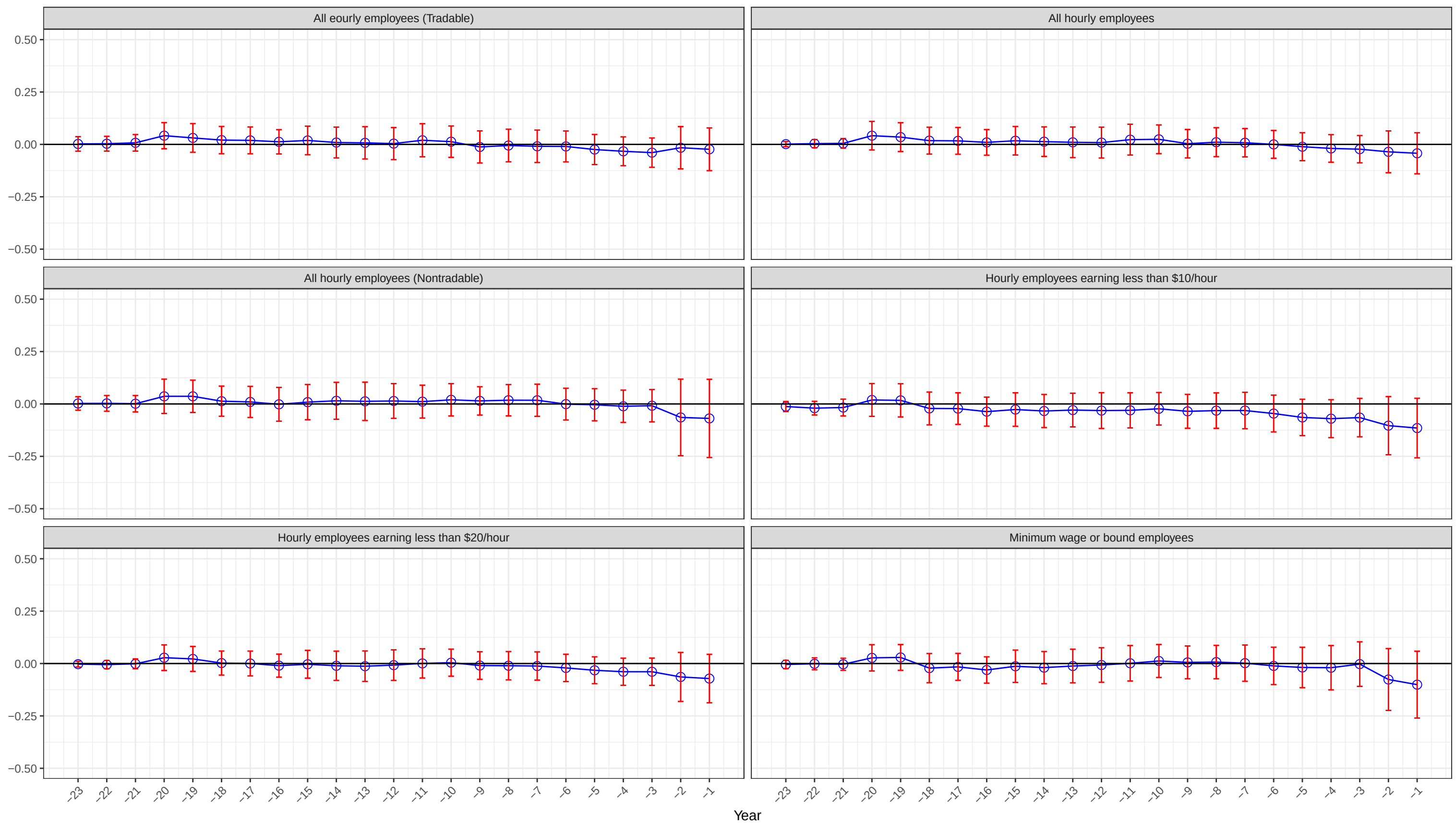


Figure IA.5: Difference-in-differences regression: dynamics of wage responses

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

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

whereδ_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 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 *Minimum wage, Bound, Spillover,* and employees earning above the spillover region in Panels A, B, C, and D respectively. The variable 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, τ months relative to the treated month. The definition of treated and control states is provided in Section 2.2. In the figure, the x-axis indicates the number of months (τ) from a minimum wage increase in event time. The blue dots in the figure correspond to the estimates of the Γ_τ coefficients, where the month corresponding to $\tau = -1$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the county level.

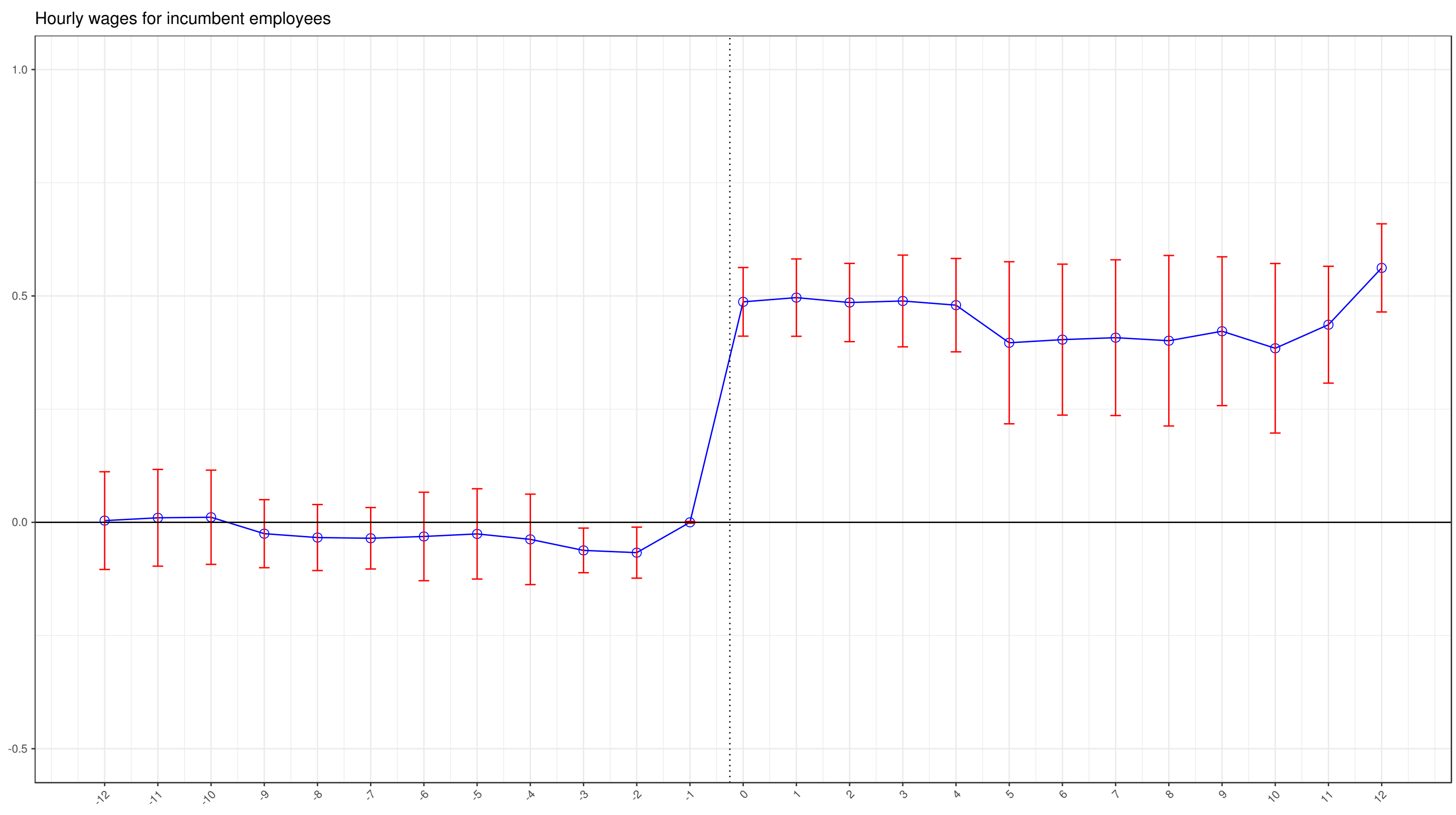


Figure IA.6: Difference-in-differences regression: dynamics of bound employment

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

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

whereδ_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 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 $D(s,t,\tau)$ is a dummy variable equal to one for all individuals in state s, τ months relative to the treated month. The definition of treated and control states is provided in Section 2.2. In the figure, the x-axis indicates the number of months (τ) from a minimum wage increase in event time. The blue dots in the figure correspond to the estimates of the Γ_τ coefficients, where the month corresponding to $\tau = -1$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the county level.

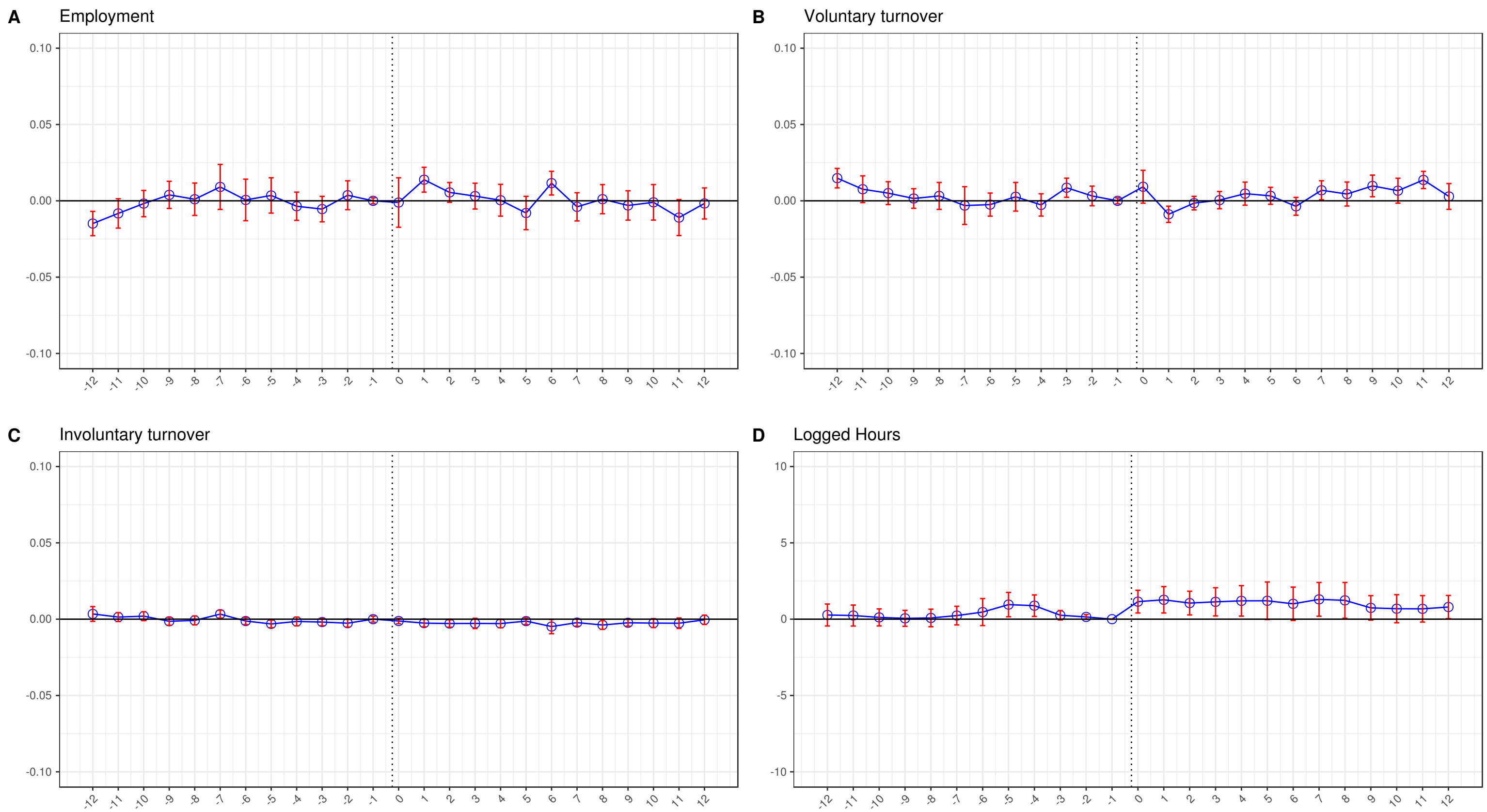


Figure IA.7: Evolution of Establishment Employment

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 $\times$ 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 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 logarithm of low wage hires(log(LowWageHires)) at establishment f, c in month t. The variable 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, τ quarters relative to the treated quarter. In the figure, the x-axis indicates the number of quarters (τ) from a minimum wage increase in event time. The blue dots in the figure correspond to the estimates of the Γ coefficients, where the quarters corresponding to $\tau = -1$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the county level.

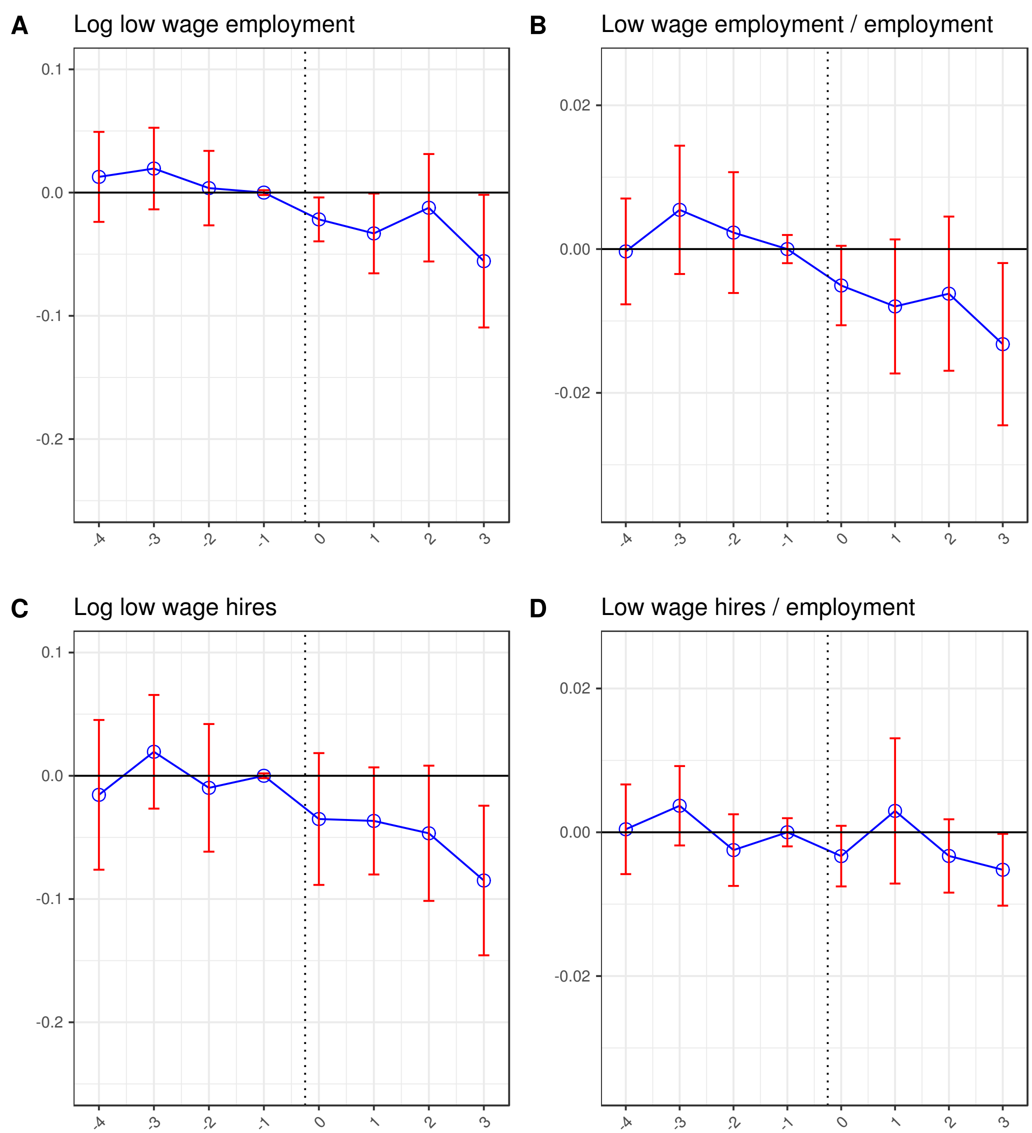
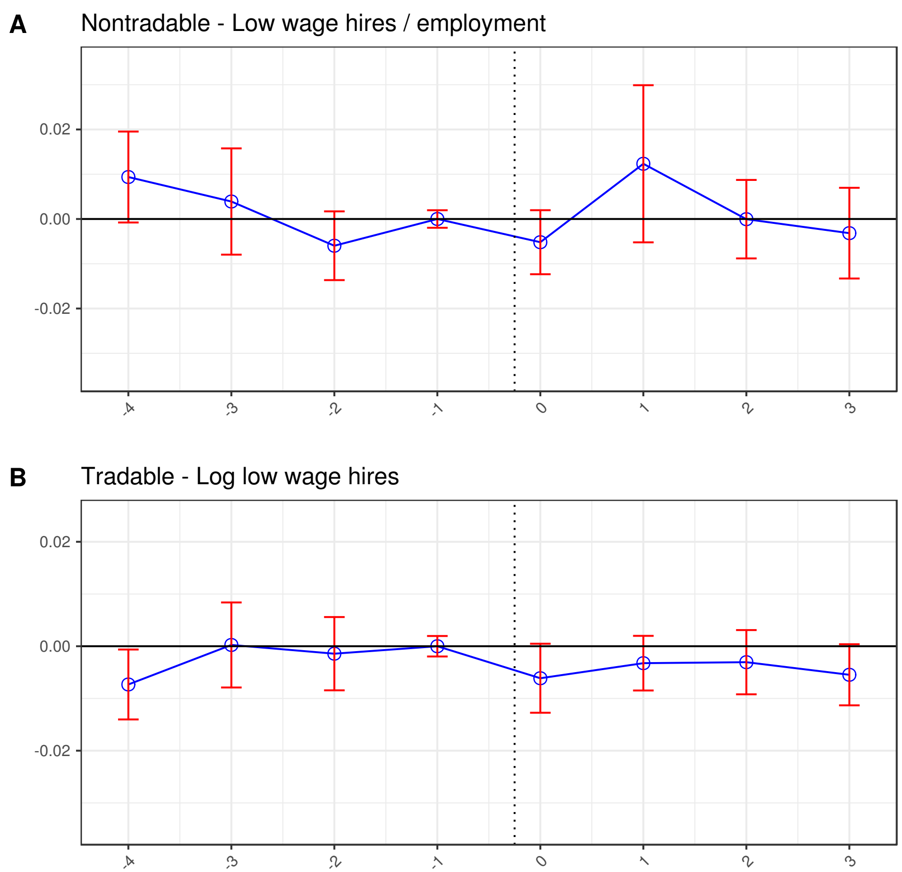


Figure IA.8: Evolution of Establishment Employment - Nontradable vs. Tradable

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 $\times$ 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 fraction of low-wage hires (scaled by lagged total employment) at establishment f, c in month t. The variable 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, τ quarters relative to the treated quarter. In the figure, the x-axis indicates the number of quarters (τ) from a minimum wage increase in event time. The blue dots in the figure correspond to the estimates of the Γ coefficients, where the quarters corresponding to $\tau = -3$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the county level.



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.[45] 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.

[45]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 dataset 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.[46] 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.[47] 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.[48]. As shown in Figure ID.3, our data is

[46]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.

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

[48]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".

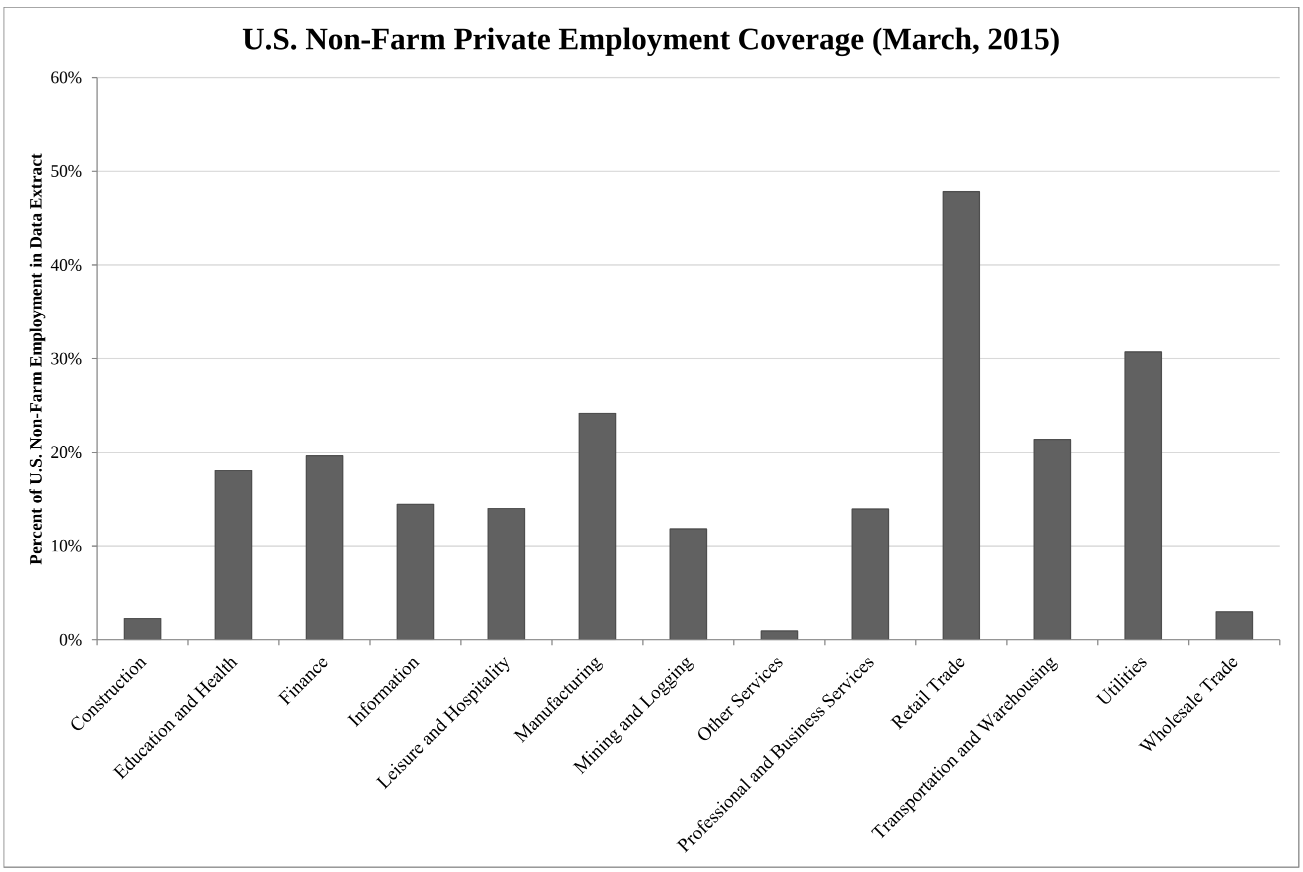


Figure ID.2: Distribution of Employment Data

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".

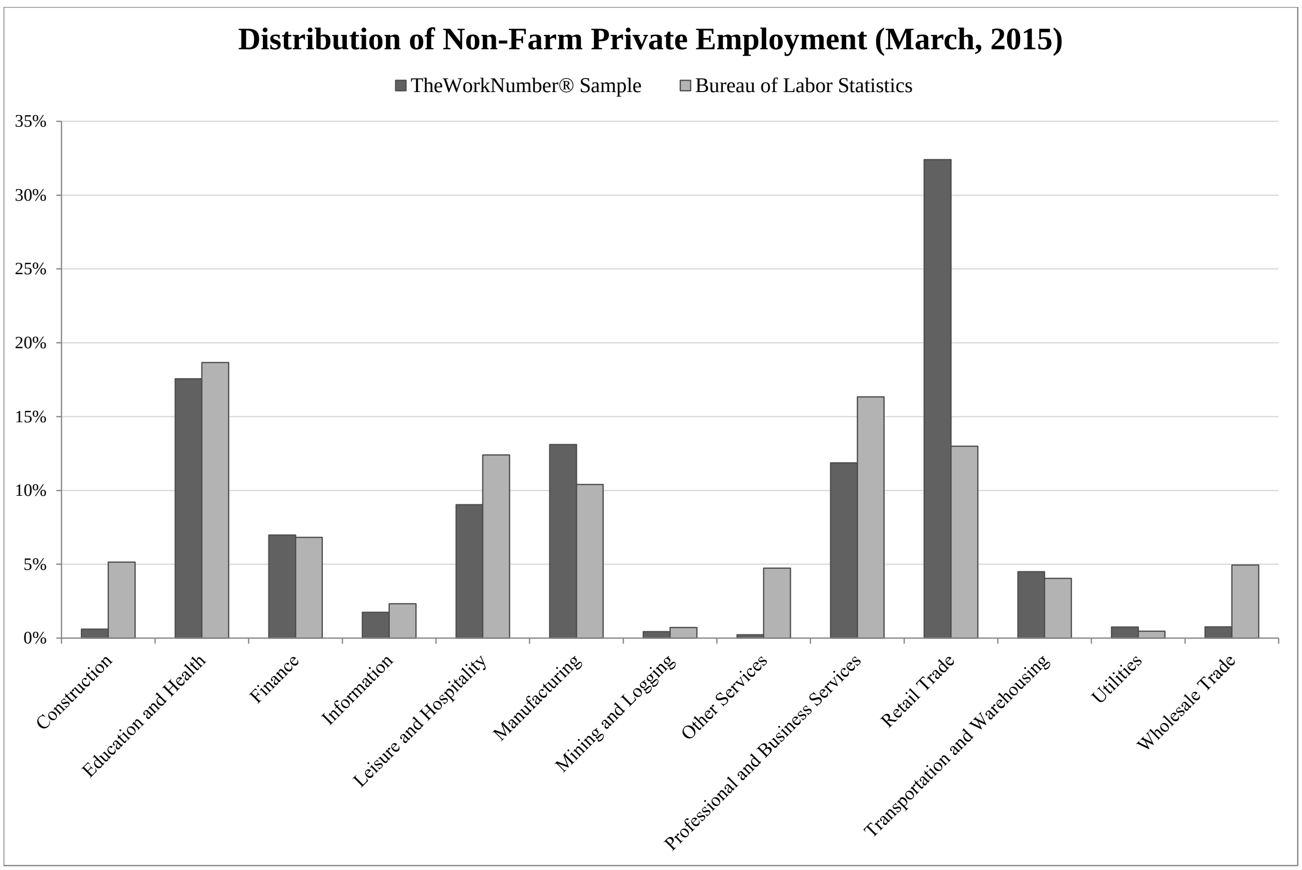


Figure ID.3: State Distribution of Employment Data

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.

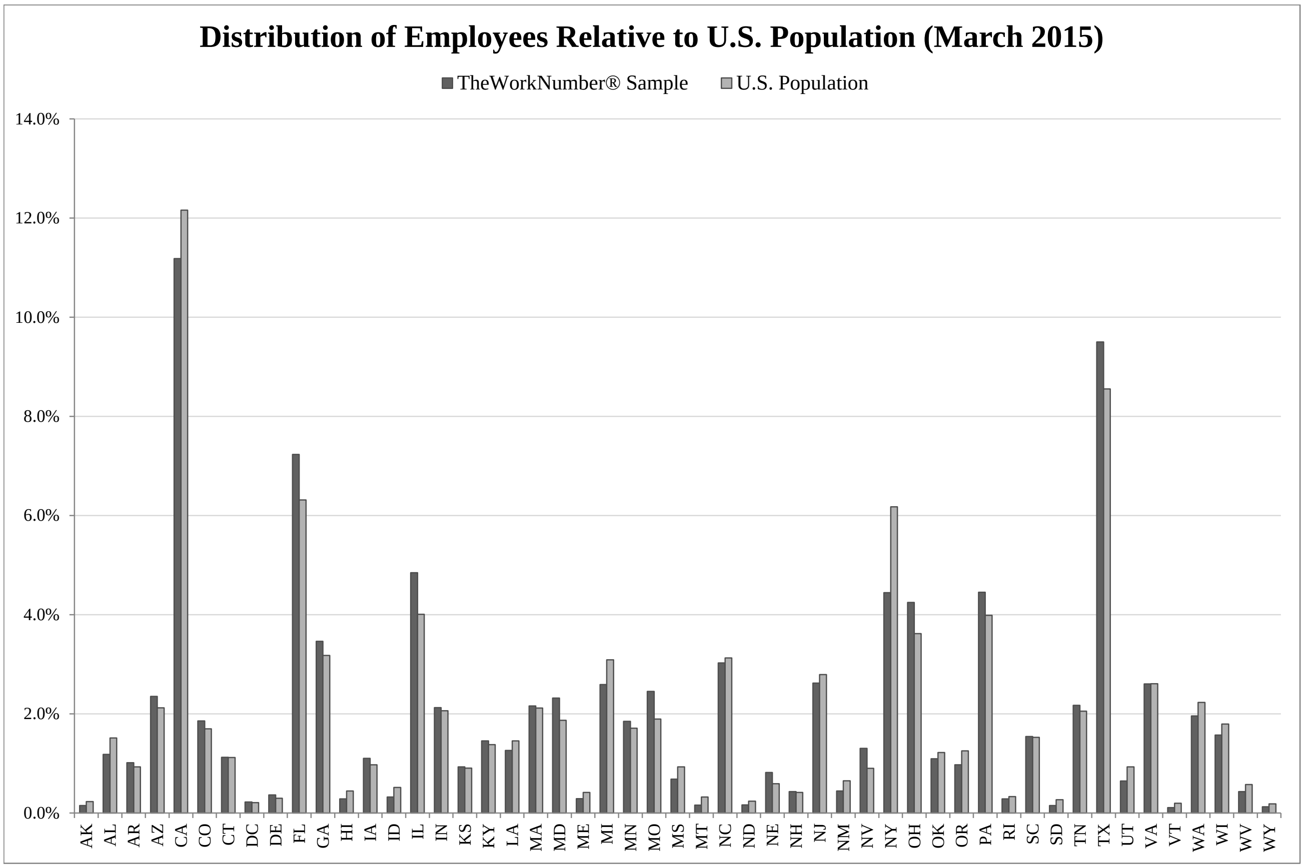


Figure ID.4: Median Incomes of Employment Data

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.

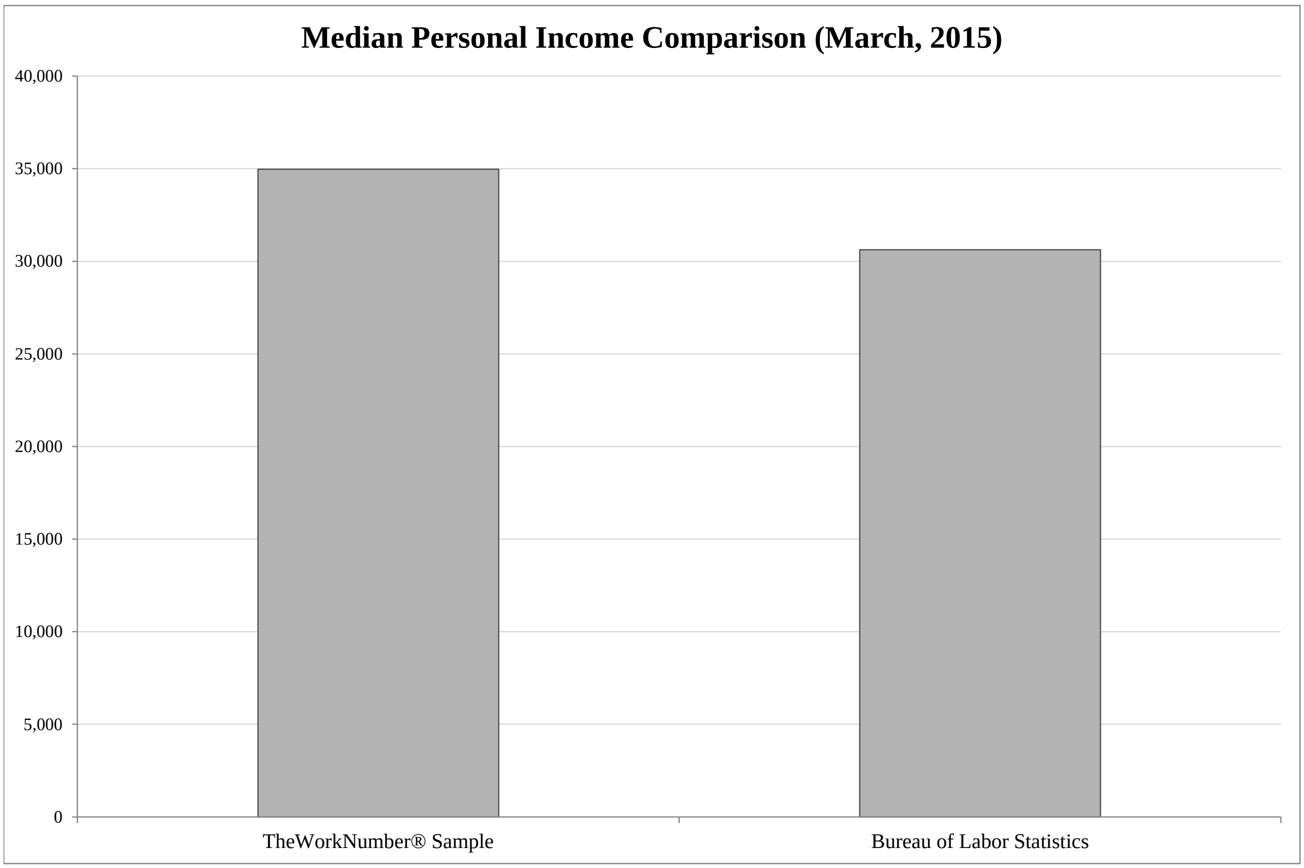


Figure ID.5: Median Tenure of Employment Data

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).

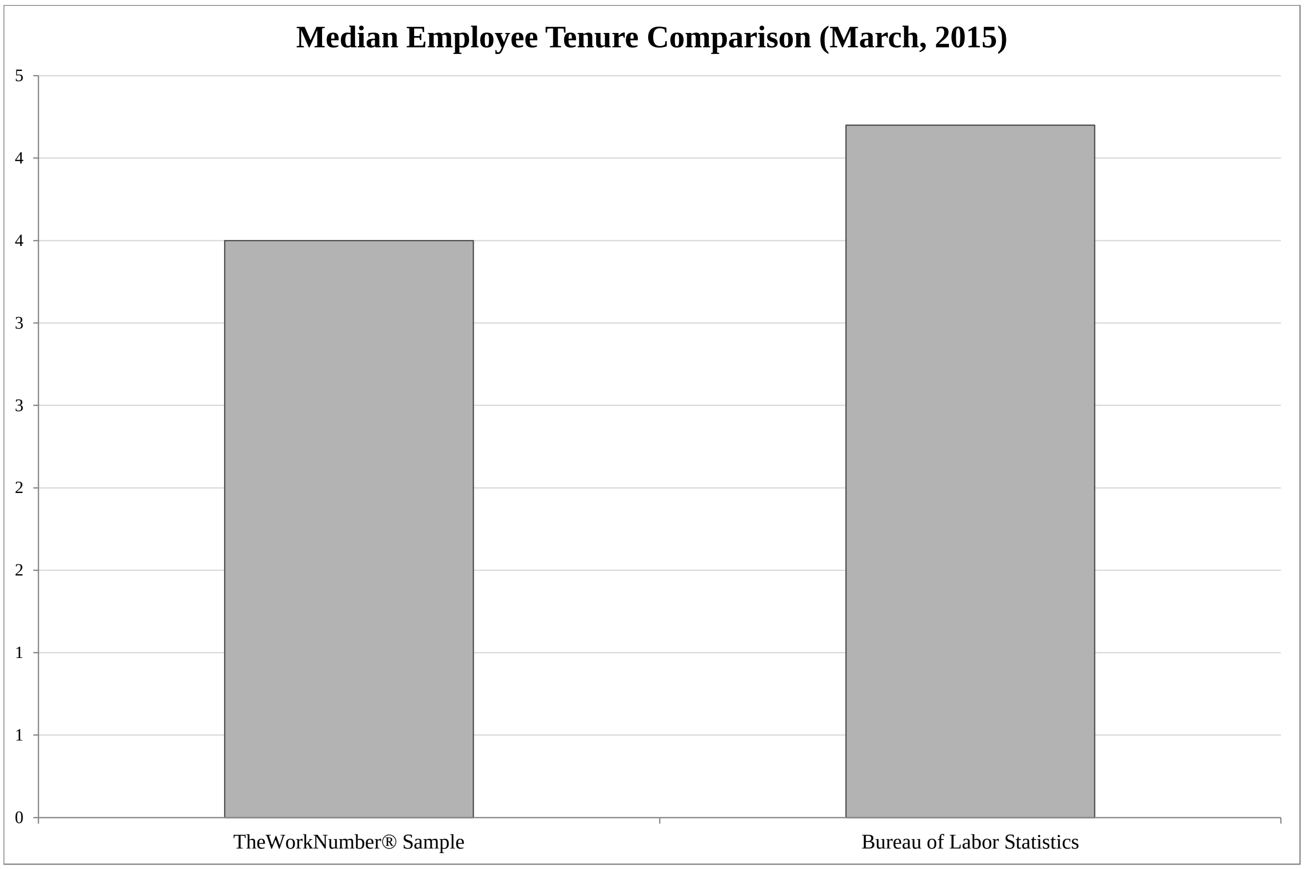


Figure ID.6: State Per Capita Personal Income of Employment Data

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.

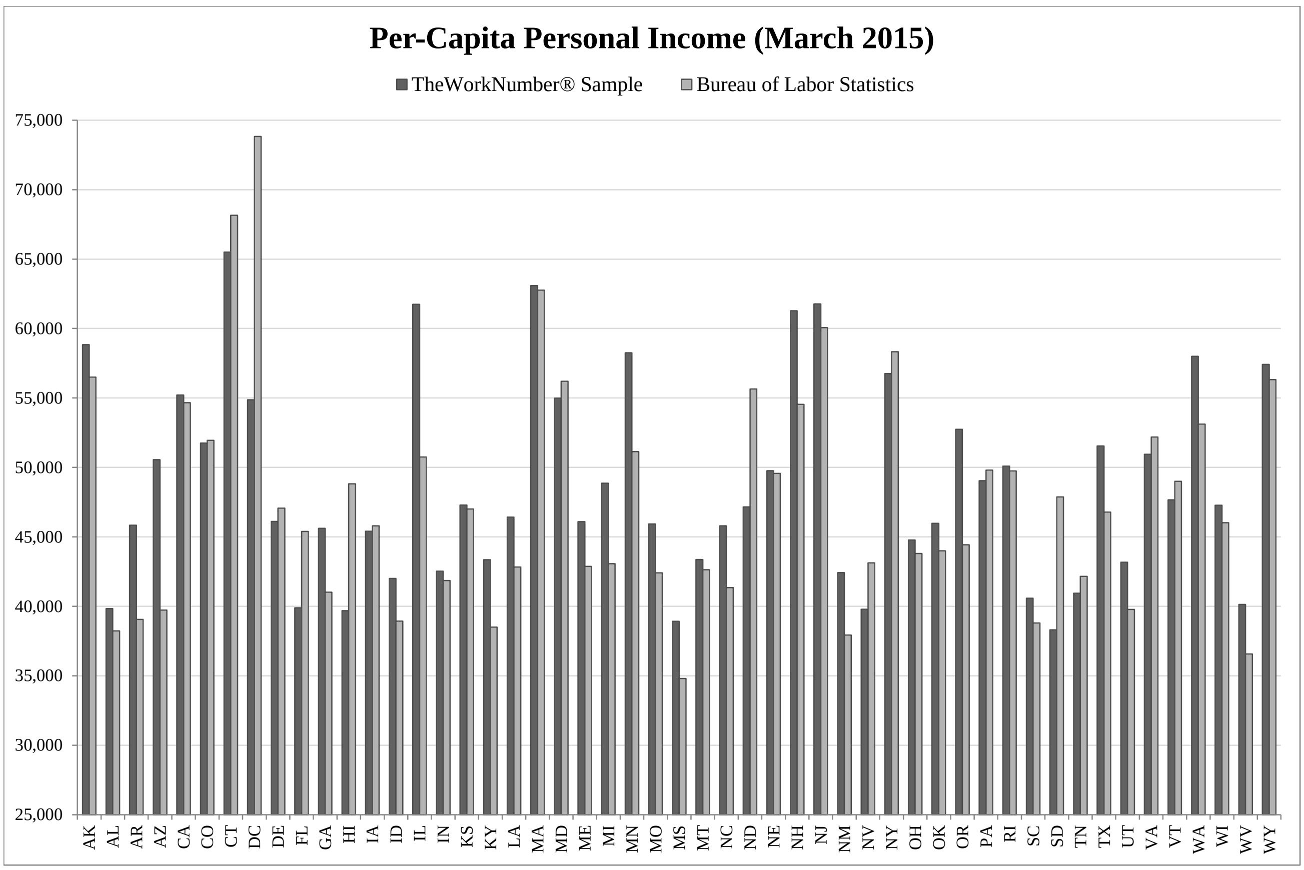


Table ID.1: Definition of Non-Tradable and Tradable Goods Industries

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