State Minimum Wage Changes and Employment: Evidence from One Million Hourly Wage Workers*

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Abstract

We use detailed wage data on one million hourly wage employees from over 300 firms spread across 23 two-digit NAICS industries to estimate the effect of six state minimum wage changes on employment. We find that the effect of the minimum wage on employment is nuanced. While the overall amount of low wage employees within firms in states that increase the minimum wage declines, existing minimum wage employees are no less likely to remain employed. We find that firms are more likely to reduce hiring rather than increase turnover, reduce hours, or close locations in order to rebalance their workforce. We also document significant heterogeneity in the employment effect across industries. While firms in the non-tradable goods industries do not reduce employment or hours, firms in the tradable and other goods industries reduce employment and partially substitute lower wage employees with higher skilled labor.

Keywords: Minimum Wage, Labor Economics, Employment, Income

JEL Classification Numbers: J01, J23, J38, H11

1 Introduction

The effect of statutory minimum wages on employment is an important policy question. Despite a large volume of research (Neumark and Wascher [2007], Belman and Wolfson [2014]), consensus remains elusive. Alongside studies that document a decrease in employment following an increase in the minimum wage (e.g. Neumark and Wascher [2000], Meer and West [2016], Clemens and Wither [2016]) are others that show the opposite (e.g. Card and Krueger [1994], Addison et al. [2009], Dube et al. [2010], Cengiz et al. [2018]). One reason for the lack of consensus is data availability.[1] Most studies lack information on exact employee wage rates and hence use proxies such as average earnings or employee age to identify minimum wage employees. Alternatively, to improve data quality, some studies confine their analysis to a few employers, a single industry, or a certain geography. In this paper, we use precise administrative wage data on one million hourly wage employees from over 300 firms spread across 23 two-digit NAICS industries to estimate the effect of six large, isolated state minimum wage changes on employment. Our data allows us to precisely estimate the employment dynamics of workers directly affected by minimum wage increases. We find that the effect of the minimum wage on employment is nuanced. Not only is there a difference between the effect on existing employees and new hires, but there is also significant heterogeneity across industries.

Our empirical analysis leverages a novel dataset on individual employment from Equifax Inc., one of the three major credit bureaus. The data contains anonymized information on the wages, salaries, hours, and job tenures of millions of employees from over 2,000 businesses in the United States. Furthermore, 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 wage data on millions of individuals working in thousands of establishments spread across multiple industries to study the effect of the minimum wage on employment. For example, while the Seattle minimum wage study of Jardim et al. [2017] uses administrative payroll data, their study is limited to a single region and and their measure of hourly wages is imputed

[1] Another important reason for the lack of consensus is the choice of identification strategy. We discuss this in more detail in Section 2.

from total earnings and hours worked.

We identify the effect of the minimum wage on employment using a quasi-experimental difference-in-differences framework that exploits within-firm variation in the minimum wage across states over time. Specifically, we study the employment of firms in six states that implemented large (and isolated) increases to the minimum wage of at least 75 cents between the years 2010 and 2015: California, Massachusetts, Michigan, Nebraska, South Dakota, and West Virginia. These constitute our treated states, and all treatments occurred during the years 2014 and 2015.[2] For each treated state, we select a set of control states that are geographically close to the treated state, that have state minimum wage laws, and that did not implement a minimum wage increase during 2014-2015 or the 24 month period immediately preceding January 2014. Importantly, treated states are statistically indistinguishable from their respective control states in terms of their GDP per-capita, unemployment rate, racial make-up, House Price Index (HPI) growth rates, age demographics, pre-treatment levels of the minimum wage, democratic vote share, unionization rates, and industry compositions. In addition, the macroeconomic characteristics of the treated and control states evolve in a statistically indistinguishable manner prior to the year of treatment.

Using this framework, we estimate the employment effects of the minimum wage at both the firm-state and the individual level. The firms in our sample are spread across multiple states; we refer to a firm-state combination as an establishment. While our establishment-level analysis estimates the effect of the minimum wage on the total stock of low wage employees, our individual-level analysis pins down the effect on pre-treatment low wage employees.[3] This dual analysis allows us to understand the total effect of the minimum wage on employment and the channels through which the effect manifests (e.g. hiring, firings, reductions in hours, etc.).

We begin by estimating the employment effect at the individual level. In this analysis, we refer

[2]We focus on large increases in the minimum wage to increase the power of our tests. We also require that the minimum wage change is isolated to keep the pre- and post- treatment periods free from the effects of other minimum wage changes. The timing and size of our minimum wage changes ensures that the increase in real wages in not dissipated by inflation. See Sections 3 and 4 for more discussion of these issues. All of our results are robust to changing the identifying variation to cross-border counties.

[3]Each observation in our establishment-level (individual-level) model represents an establishment-month (individual-month) combination. In both analyses we focus on the twenty four month period (twelve months before, twelve months after) surrounding each increase in the minimum wage.

to employees whose wages are initially less than the new minimum wage – i.e. those directly affected by a minimum wage increase – as *Bound employees*, and we refer to employees making exactly the old minimum wage as *Minimum wage employees.* As a necessary first-step, we document how the hourly wages of *Minimum wage employees* and *Bound employees* evolve in the twelve month period following a minimum wage change. We find that an increase in the minimum wage generates a level increase in hourly wages. Moreover, the size of the wage increase is equal to the weighted average minimum wage change in our sample. Not only do these findings establish the quality of our wage data, but they also help ensure that the controls we employ in our baseline model will not attenuate our estimates of the employment effect (Neumark et al. [2014], Clemens and Strain [2017]).

We find that an increase in the minimum wage has a slightly positive, but statistically insignificant, effect on the employment of existing *Minimum wage* and *Bound employees.* We also find economically small and statistically insignificant effects when analyzing the rate of voluntary turnover, the rate of involuntary turnover, and the average number of hours worked. For each outcome variable, our dynamic difference-in-differences model rejects the existence of employment pre-trends and hence suggests that employers do not "pre-react" to changes in the minimum wage. We also find little-to-no heterogeneity in the employment effect across several individual and firm-level characteristics (e.g. tenure, state of employment, firm size, etc.). Overall, we find no significant evidence that increases in the minimum wage adversely affect existing low wage employees.

Despite its importance, the individual-level analysis can only document the effect of the minimum wage on existing low wage workers. Indeed, firms may adjust employment along other dimensions – such as through hiring or substituting low wage employees with high skilled workers – which would not be captured by the individual-level analysis.[4] Our establishment-level analysis allows us to evaluate the merits of such claims and understand the total effect of the minimum wage on employment. In our establishment-level analysis, we define low wage employees as those whose wages satisfy $\omega_{i,t} \leq \$10.00$.[5] We find that the fraction of low wage employees in establishments de-

[4]Oi [1962] and Hamermesh [1987] argue that the non-trivial fixed costs of hiring and firing new employees (e.g. training, interviewing, background checks, search costs) encourages reductions in hiring rather than increases in layoffs.

[5]Since we study the stock of low wage employees every period, we will not be able to use the employee categories,

clines by 1.0 percentage point in the twelve months following an increase in the minimum wage. In comparison, the average pre-treatment fraction of low-wage employees is 44 percentage points. Our estimates correspond to a negative 4% (2.5%) response of low wage employment (total employment) to a 10% increase in the minimum wage.[6] We find that the decline in low wage employment occurs within the first quarter after a minimum wage increase and exhibits no evidence of pre-trends that would invalidate the analysis.

We reconcile our establishment-level and individual-level results by documenting the channel through which establishments reduce employment. Consistent with the individual-level results, we find no evidence of a change in the rate of establishment-level turnover among either low wage or non-low wage employees. We also find no evidence that establishments close locations following an increase in the minimum wage. In contrast, we document large declines in establishment-level hiring. We find that establishments reduce their monthly fraction of low wage hires (relative to total employment) by 0.2 percentage points – a 6.7% reduction from the unconditional mean of 3.0 percentage points. We estimate an approximately -5% (-3%) response of low wage hiring (total hiring) to a 10% increase in the minimum wage.

Next we evaluate the theoretical prediction that firms in the tradable and non-tradable goods industries may differ in their response to the minimum wage. Manning [2016], among others, argues that low wage employment in the non-tradable goods industries should be less responsive to increases in the minimum wage. This is because non-tradable goods firms may find it easier to adjust along the price margin (Harasztosi and Lindner [2017]). We find evidence in support of this hypothesis in our data. While firms in the non-tradable goods industries neither reduce head-counts nor hours worked, firms in the tradable goods industries reduce employment across the board. We also find some evidence that tradable goods firms substitute lower wage employees with marginally higher-skilled labor.

such as *Minimum wage* employees and *Bound* employees, in our establishment-level analysis. See Section 4.2 for a detailed discussion of the issues involved and see Section 6 for robustness of the establishment-level results to alternative definitions of low-wage employees.

[6]This is slightly higher than the estimated response range of $1-3\%$ in Neumark and Wascher [2007]. However, relative to other studies in the literature, our data arguably better identifies the set of employees directly affected by the minimum wage. All else equal, this would reduce the scope of any attenuation bias.

Our paper makes several contributions to the vast minimum wage literature. First, we are unique in using administrative wage data spanning across a number of industries to evaluate the employment effect of the minimum wage. We can therefore speak to both the average effect of the minimum wage and how this effect varies across industries. Second, our data allows us to analyze the effect of the minimum wage on both existing employees and new hires. Third, we are able to evaluate the importance of the different channels through which firms can adjust employment in response to higher minimum wages – e.g. turnover, hiring, hours, or consolidating locations. Fourth, we are able to analyze how the minimum wage affects the composition of a firm's workforce and how this varies across subsamples of the population. Finally, we are able to control for a wide variety of confounding factors while still ensuring that sufficient residual variation remains to identify our effects of interest.

Our results should be interpreted with the following caveats in mind. The employment effect of a minimum wage hike may depend on the status of the labor market (Clemens and Wither [2016]), the size of the minimum wage increase (Jardim et al. [2017]) and may differ across firms of different sizes. We estimate the employment effect during 2013-15 when the labor market was relatively benign, the average size of the minimum wage increase in our sample is 10%, and our sample predominantly consists of large firms. We also cannot speak to the total welfare effects of the minimum wage (e.g. MaCurdy [2015], Flinn [2006], and Flinn [2002])– although we can document that existing minimum wage workers seem to be better off in terms of wages and no worse off in terms of employment likelihood.

The remainder of the paper is organized as follows: Section 2 outlines the relevant literature, Section 3 provides background on changes to state minimum wages during our sample period and describes how we select treated and control states, Section 4 describes our data, and Sections 5 and 6 present the effect of the minimum wage on individuals and establishments, respectively. Section 7 examines heterogeneity in the employment effect across industries, and Section 8 concludes.

2 Related Literature

In this section we outline the relevant literature. We draw the reader's attention to Neumark and Wascher [2007] and Belman and Wolfson [2014] for more comprehensive surveys.

2.1 Theory

Contrary to popular belief, the theoretical impact of a *small* increase in the minimum wage on low wage employment is ambiguous. Competitive labor market models predict that firms will reduce their demand for low wage labor in response to an increase in the price of labor above the competitive equilibrium level. Firms may also reduce output and increase the utilization of other factors of production, such as capital or higher skilled labor (MaCurdy [2015]). Alternate assumptions about the labor market, however, can generate starkly different predictions. For example, both monopsony models and bilteratal search models with heterogeneous workers predict that a minimum wage above the equilibrium wage may actually *increase* employment (Stigler [1946], Bhaskar and To [1999], Lang and Kahn [1998]). Efficiency wage models can generate similar employment predictions as monopsony models even when the number of employers is large (Rebitzer and Taylor [1995]). Using a continuous time search model with bargaining, Flinn [2006] finds that an increase in the minimum wage wage may or may not lead to an increase in unemployment. He also characterizes the conditions under which an increase in the minimum wage may be welfare enhancing on both the supply and demand sides of the labor market.

Several papers argue that the employment effect of the minimum wage may vary depending on industry characteristics. For example, Manning [2016] argues that the employment effects of the minimum wage may vary across the tradable and non-tradable goods sectors. To the extent the competition in the non-tradable goods sector is local, small increases to the minimum wage will be a shock to the industry cost structure. This may enable the firms to adjust their prices and mute the employment response. Furthermore, a higher minimum wage may have a positive spillover to local demand which may disproportionately benefit non-tradable goods firms (Mian and Sufi [2014]).

Although the theoretical impact of a *small* increase in the minimum wage is ambiguous, all of the above theories predict that there will be a point at which the minimum wage is so high that it reduces employment significantly. Thus, the existence of an employment effect may depend on both the size of the increase, the initial level of the minimum wage, and the time period being analyzed. Clemens and Strain [2017] present a model which is consistent with this argument. They show that the employment effect will be small (large) when minimum wage increases move through sparsely (densely) populated areas of the productivity distribution.

2.2 Recent Evidence and Contributions

Empirically, consensus on the employment effects of the minimum wage has remained elusive over the past decade.[7] While several recent papers have documented employment effects that are not statistically different from zero (Dube et al. [2010], Dube and Zipperer [2015], Giuliano [2013], Hirsch et al. [2015], Allegretto et al. [Forthcoming], Cengiz et al. [2018]) , several other papers have documented significantly negative employment effects (Clemens and Wither [2016], Clemens and Strain [2017], Jardim et al. [2017]) and employment effects that vary by industry (e.g. Harasztosi and Lindner [2017]).[8] As stated earlier, one important reason for the lack of consensus is data availability. Most studies use survey data and are unable to precisely identify low wage employees. This forces them to utilize proxies for low wage employment, such as teenage or restaurant industry

[7]The empirical literature on the minimum wage extends much beyond the past decade (e.g. Card and Krueger [1994], Neumark and Wascher [2000]). In this Section, we only aim to highlight the most recent evidence. An extensive discussion of earlier works can be found in Neumark and Wascher [2007].

[8]Clemens and Wither [2016] find that the increases to the federal minimum wage between 2007 and 2009 significantly reduced employment. Zipperer [2016] argues that the results in Clemens and Wither [2016] are biased because their treated and control states differ significantly in their composition of industries that were severely impacted by the Great Recession (e.g. the construction industry). Clemens [2017] refutes this argument by documenting evidence against Zipperer [2016]'s falsification tests. Jardim et al. [2017] study the effects of the 2015 and 2016 Seattle minimum wage increases and find an overall reduction in employment via hours worked at the region-level. Clemens and Strain [2017] examine recent minimum wage increases between 2013 and 2015 and find that employment among younger and less-educated adults expanded less quickly in states that enacted minimum wage increases than in those that enacted no minimum wage increases. Their specification of choice, however, is limited to only one observation in the post-treatment period. Harasztosi and Lindner [2017] find that a 60% real increase to the minimum wage in Hungary had only a limited effect on firm-level employment. Their estimates, however, are more pronounced in the tradable goods sector, while the non-tradable goods sector experiences an effect that is close to zero. Using a bunching estimator over 138 minimum wage changes between 1979 and 2016, Cengiz et al. [2018] find little-to-no effect of the minimum wage on the number of low-wage jobs. Similar to Harasztosi and Lindner [2017], Cengiz et al. [2018] find a larger disemployment effect in the tradable goods industries.

employment. The use of such proxies can potentially attenuate estimates of the employment effect towards zero (Belman and Wolfson [2014], Jardim et al. [2017]) or produce misleading inference due to spurious changes in employment in the higher parts of the wage distribution (Cengiz et al. [2018]).[9] Other studies utilize more granular, administrative wage data but are still confined to either a single employer (e.g. Giuliano [2013]), a single industry (e.g. Hirsch et al. [2015]), or a single location (e.g. Jardim et al. [2017]). Such restrictions can limit the external validity of the results, especially if there is heterogeneity in the employment effect across employers, industries, or locations. As described below, we are not limited in our ability to identify minimum wage employees across the United States, and thus we are not forced to analyze a single industry, demographic group, region, or location. We are also able to estimate the differential effect of the minimum wage across existing and new employees, and across total firm employment and hours.

Another key factor in the lack of recent consensus lies in the choice of empirical specification and identification strategy (Clemens and Strain [2017]). Papers which utilize variation in the minimum wage across states or smaller geographic regions tend to produce insignificant estimates of the employment effect (e.g. Dube et al. [2010]). Negative effects tend to be found in papers that exploit variation at the national level or that coming from the "bind" of federally induced changes (e.g. Clemens and Wither [2016]). This leaves open the question as to whether the former insignificant results are due to more precise estimation, a lack of power, or a form of selection bias (Gormley and Matsa [2014], Neumark et al. [2014]). Our paper focuses on constraining the variation to the same firm across neighboring treated and control states at the same point in time. We include separate fixed effects for each set of neighboring states at each point in time and each firm at each point in time to control for time-varying spatial and firm shocks to employment. Despite employing a strict empirical specification with a number of fixed effects, we are able to precisely pick up the increase in the minimum wage as a level shift in hourly wages of the affected employees. This confirms that there is sufficient residual variation in our sample to estimate the employment effect (Neumark et al.

[9] As shown by Manning [2016], teenagers only comprise one-ninth of the total minimum wage hours worked in the year 2014. In fact, individuals under 25 comprise only about one-third of all minimum wage hours worked. Slightly over half (under one-fifth) of all minimum wage hours worked are by individuals above the age of 30 (50).

[2014]). We also conduct a battery of robustness tests that exploit different sources of variation to help mitigate the concern that selection bias is driving our main results.

A final reason for the lack of consensus is the disagreement about whether one should focus on the stock or flow of employees. Several recent papers have argued that the employment effect should be more apparent in employment dynamics than stocks, highlighting the need for data on both existing and new low wage employees. For example, Meer and West [2016] find that the negative effects of the minimum wage manifest in employment growth, and Dube et al. [2010] find that minimum wages have a sizable negative effect on employment flows but not on levels. Both papers are consistent with theories of costly turnover (Oi [1962]). Our dual analysis at the individual and establishment level allows us to disentangle the effects of the minimum wage on existing and new employees, and thus examine both the stock and flow of employment.

3 Background and State Selection

In this section we provide background on changes to the minimum wage between the years 2010 and 2015 and we detail our procedure for selecting the treated and control states.

3.1 State Minimum Wage Changes Between 2010 and 2015

We begin by providing background on the frequency and size of state-level changes to the minimum wage between January 01, 2010 and December 31, 2015 (our sample period).[10] During this period, 29 states enacted 75 distinct increases to the minimum wage. The median state enacted 2 increases to the minimum wage, 12 out of the 29 states enacted exactly one increase, and 8 states increased their minimum wage annually as part of a cost-of-living adjustment program. Overall, changes to the minimum wage were quite common during our sample period.

Nearly half of all the minimum wage increases during our sample period were for economically small amounts of less than 25 cents. These mostly represent annual increases to the minimum wage

[10] We obtain information on state minimum wage changes from Meer and West [2016]'s online repository and the Bureau of Labor Statistics (BLS). There were no changes to the federal minimum wage during this time period.

arising from cost of living adjustments. There were also several large increases to the minimum wage during this period. Specifically, there were sixteen increases of 75 cents or more (enacted by 13 distinct states), and these increases were all enacted during the years 2014 and 2015. We use a subsample of these large increases in the minimum wage to conduct our analysis.

3.2 Selection of Treated and Control States

To increase the power of our tests, we focus on states that implemented large (and isolated) increases to the minimum wage. Specifically, we focus on states that (1) implement exactly one minimum wage increase of at least 75 cents between 2010-2015, and (2) did not implement any other minimum wage increase during the 24 months prior and 12 months after their 75+ cent minimum wage increase. Imposing these two conditions helps facilitate our difference-in-differences analysis by keeping the pre- and post- treatment periods free of other minimum wage changes, and ensures that our nominal changes to the minimum wage are not dissipated by inflation. A total of six states (hereafter the treated states) satisfy the required conditions: California, Massachusetts, Michigan, Nebraska, South Dakota, and West Virginia.[11] Table 1 summarizes the relevant minimum wage changes from the six treated states. There are two increases of 75 cents, three increases of $1, and one increase of $1.25. All of the increases occurred during the years 2014 and 2015.

For each treated state, we select a set of control states that are geographically close to the treated state, and hence are plausibly subject to similar economic conditions, but that did not implement an increase to the minimum wage during this period. Specifically, we require each of our control states to satisfy the following three conditions: (1) the state is geographically close (as measured by the same census region or within two states distance) to the treated state, (2)

[11] There are six other states in the continental U.S. that implemented minimum wage changes of at least 75 cents during the sample period. However, each of these states fails to satisfy the second required condition, and is therefore removed from the analysis. These states include Maryland, Minnesota, New Jersey, New York, and Washington D.C. – these states implemented a minimum wage increase within 12 months of their 75+ cent increase – and Rhode Island – this state implemented a minimum wage increase during the 24 months prior to its 75+ cent increase. In addition to these states, we also eliminate Alaska from the analysis because our identification strategy exploits geographic variation in the minimum wage over time. For reference, Table IA.1 in the Internet Appendix provides a year-by-year breakdown of minimum wage changes in the treated states, states with 75+ cent increases that are eliminated from consideration, and our control states (defined in the subsequent paragraph).

the state did not implement a minimum wage increase during 2014-2015 or during the twenty four months prior to January, 2014, and (3) the state enforces state-level minimum wage laws. Condition (1) helps alleviate the concern that control states face systematically different economic conditions than treated states (Allegretto et al. [Forthcoming], Dube et al. [2010, Forthcoming]). Condition (2) ensures that our estimates are not confounded by an increase in the minimum wage in control states. Condition (3) removes states that prior research has shown to be systematically different from states that have state-level minimum wage laws (Allegretto et al. [Forthcoming]).[12]

The last column of Table 1 lists the control states for each of our treated states, and Figure 1 displays the geographic distribution of treated and control states. In almost all cases, control states border treated states or are connected to a treated state through another bordering control state. The only exception to the criteria is Virginia which, along with Pennsylvania and New Hampshire, is chosen to serve as a control unit for Massachusetts.

Table 2 shows that the macroeconomic conditions in treated and control states are similar in the quarter before each treated state increases its minimum wage. We find that treated states are statistically indistinguishable from their respective control states in terms of average total population, GDP per-capita, unemployment rate, racial make-up, House Price Index (HPI) growth rates, age demographics, pre-treatment levels of the minimum wage, democratic vote share, and unionization rates. These similarities also hold when considering longer time horizons that look within pairings of treated and control states (Internet Appendix Table IA.2). In addition, treated states have a similar composition of industries as their paired control states (Internet Appendix Table IA.3) and implement minimum wage increases at similar points in time prior to the period of interest. The similar macroeconomic conditions in treated and control states helps alleviate concerns that other systematic policy trends may differ across these states (Allegretto et al. [Forthcoming]). In the next subsection we conduct a more formal comparison of the economic trends in the treated and control states.

Note that our selection procedure for treated and control states intentionally eliminates a large

[12]These states only adhere to the federal minimum wage. The states are Alabama, Louisiana, Mississippi, South Carolina, and Tennessee. Our results are insensitive to excluding any one treated state from the analysis.

number of minimum wage changes between 2010 and 2015. This is done for the sake of experimental validity. As is recognized in the literature, minimum wage changes tend to occur frequently across states (or federally) over a span of only a few years. This limits the number of instances in which clean variation in the minimum wage can be extracted (Meer and West [2016]). Not only must a minimum wage change be isolated in time to be included in our analysis, but it also cannot be eroded during the sample period by either inflation or sudden increases in its control observation's minimum wage. Imposing such restrictions, however, limits the geographic and temporal scope of our analysis and also potentially introduces a form of selection bias. We implement several methods to address bias stemming from states selecting into a higher minimum wage – including border county and within-state triple-difference analyses. We describe these in greater detail in later sections.

3.3 Test for Pre-trends in Macroeconomic Characteristics:

A major concern for any study which focuses on minimum wage changes is the endogeneity of the changes themselves. That is, states that initiate minimum wage changes could be systematically different from the control states and such differences could affect employment dynamics. To alleviate such concerns, we compare the macroeconomic conditions of the treated and control states around the year of treatment. Specifically, we estimate variants of the following model:

$$y_{s,t} = \alpha + \sum_{\tau=2011}^{2015} \Gamma_\tau \times \mathrm{Treated_s} \times \mathrm{D}(\tau) + \delta_s + \delta_{t,tr(s)} + \varepsilon_{s,t}, \tag{1}$$

where the dependent variable $y_{s,t}$ is a state macroeconomic characteristic including both the logged levels and one-year growth of *Population*, *GDP, Unemployment Rate*, and *HPI*. The variable Treated$_s$ is a dummy variable that takes a value one if state s is a treated state, and $D(\tau)$ is a dummy variable equal to one in year τ. Standard errors are clustered at the state-level.

The sample for these tests include all the treated and control states for the years 2010-2015. Our coefficients of interest are the Γ_τs, and the omitted category in these regressions is the year 2010.

Thus, the coefficient estimates capture the extent to which the outcome variable is different across the treated and control states in the year τ relative to the year 2010. We include state (δ_s) fixed effects in the model to account for time-invariant state-level heterogeneity, and treatment specific time ($\delta_{t,tr(s)}$) effects to account for time-varying spatial heterogeneity common to the paired treated and control states.[13] All results are unchanged if we estimate the model for shorter horizons (e.g., 2012 to 2015).

Figure 2 plots the coefficient estimates from Equation 1 for the period 2010-2015. We find that the macroeconomic conditions in the treated and control states generally evolve in a statistically indistinguishable manner. Nevertheless, throughout our main empirical analysis we directly control for lagged realizations of quarterly GDP per-capita growth and house-price index growth to account for differential state macroeconomic trends.

4 Data Sources and Sample Selection

To conduct our analysis, we use anonymized payroll data on over 2,000 U.S. firms (22.5 million active employees per month) from Equifax Inc. Equifax Inc. is a global leader in information solutions, and is involved in the collection and transmission of data on credit histories and employment for individuals within the United States.[14] The data spans the years 2010 to 2015 and includes information on the location, wages, salary, bonus, job title, and job tenure of both current and former employees. The data distinguishes between hourly and salary employees, specifies exact hourly wage rates, and, in the case of employee turnover, identifies if turnover was voluntary or involuntary. The data is representative of the U.S. population in terms of median personal incomes, median employee tenures, per-capita personal incomes across states, and the distribution of employment across states.

[13]These are separate time fixed effects for each of the six treated-control groupings we analyze. The function $tr : \mathrm{S} \rightarrow \mathrm{T}$ is a mapping from the set of 18 treated and control states, S, to the set of 6 treated states, T. The notation $tr(s)$ is used to denote the matched set of treatment and control states to which state s belongs, and thus the fixed effect $\delta_{t,tr(s)}$ controls for time-varying spatial variation common to the matched sets. For example, $tr(\mathrm{KY}) = \mathrm{WV}$ and $tr(\mathrm{WV}) = \mathrm{WV}$.

[14]Over 5,000 firms across the country report employee-level information to Equifax Inc. on a payroll-to-payroll basis. We are only able to access data on (roughly) the largest 2,000 firms for research purposes. Business-wise, the data is primarily used for employment verification purposes.

However, the retail trade industry is over-represented in the data and the construction, wholesale trade, and other services industries are under-represented. All other industries are represented in the correct proportions. For more details on the data, please see Appendix B.

We use this data to examine the employment effects of the minimum wage at both the firm-state and the individual level at a monthly frequency. Our firm-state analysis employs a sample of firm-state combinations (hereafter called *establishments*) that are located in treated or control states. Our individual analysis employs a sample of employees that work at establishments that are located in treated or control states during the 12 month period prior to a change in the minimum wage. While the individual-level analysis examines the effect of the minimum wage on existing employees, the establishment-level analysis examines the effect on the total stock and flow of employment. In both analyses, we focus on the 24 month period surrounding each of our sample minimum wage increases (12 months before, 12 months after).

In terms of sample construction, we allow for employee entry and exit in our establishment-level analysis – as we study the stock of employees at any point in time. We also allow establishments to enter and exit the sample. However, we only allow employees to flow into the individual sample during the pre-treatment period (the period prior to a sample minimum wage increase) in order to estimate the effect of the minimum wage on existing employees. In both analyses, the pre-treatment period for a control state is set to be the same as that for its paired treated state.[15] Hence, we have staggered adoptions of treatment. We discuss our two samples in more detail below.

4.1 Individual-Level Analysis Sample

Our individual-level sample consists of one million hourly wage employees whose wages are in the neighborhood of the minimum wage. We separate these employees into three sub-groups: *Minimum wage employees, Bound employees,* and *Pseudo-low wage employees*. We define a *Minimum wage*

[15] For example, consider the case of West Virginia (a treated state) and Kentucky (West Virginia's paired control state -e.g. $tr(\text{Kentucky}) = \text{West Virginia}$). West Virginia enacted a minimum wage increase of 75 cents on January 01, 2015. Therefore, the pre-treatment period for West Virginia **and** Kentucky begins January 01, 2014 and ends December 31, 2014. Employees living in West Virginia and Kentucky are allowed to filter into the individual-level sample as long as they appear within the employment dataset before December 31, 2014. All states in our sample either enact strictly one or zero minimum wage increases.

employee as one whose wage in the month closest to three months prior to treatment satisfies $\omega_i = \text{OLD MW}_s$, where OLD MW_s is the initial minimum wage in state s before any increase (or no increase if the state is a control). For example, if individual i is employed from month -12 to month -8 and if her wage in month -8 is the minimum wage, then she is included in our sample as a *Minimum wage employee*. While increases to the minimum wage (in the treated states) undoubtedly affect the wages of *Minimum wage employees*, they also affect the wages of employees making slightly above the old minimum wage but below the new minimum wage. We refer to the union of this group of employees and *Minimum wage employees* as *Bound employees*. The pre-treatment wages of *Bound employees* satisfy the condition $\omega_i < \text{NEW MW}_s$, where NEW MW_s is the "new" minimum wage after any increase. For a control state, the NEW MW_s refers to the hypothetical minimum wage the state would have if it had implemented the same increase to its minimum wage as its paired treated state, i.e. $\text{NEW MW}_s = \text{OLD MW}_s + \Delta\text{MW}_{paired(s)} \;\; \forall s \in \text{ControlStates}$. For example, West Virginia enacted a 75 cent increase to its minimum wage on January 01, 2015. Kentucky is the paired control state for West Virginia. The NEW MW_s for Kentucky satisfies: $\text{NEW MW}_{Kentucky} = \text{OLD MW}_{Kentucky} + 0.75$. All of the *Bound employees* would experience (or would have experienced) a pay raise after the new minimum wage increase takes effect.

Our third subgroup of employees are those whose wages are not directly affected by changes to the minimum wage. We refer to them as *Pseudo-low wage employee*s: individuals whose pre-treatment hourly wage satisfies $\omega_i \in (\text{NEW MW}_s+\$1, \text{NEW MW}_s+\$3.50]$. As long as this subgroup of employees is also not indirectly affected by increases to the minimum wage, we can use these employees to conduct placebo tests and control for time-varying state-level shocks that may be correlated with minimum wage increases (see Clemens and Wither [2016]).

Note that for much of our individual-level analysis we exclude employees whose pre-treatment wages satisfy $\omega_i \in (\text{NEW MW}_s, \text{NEW MW}_s + \$1]$. We do this because the effect of the minimum wage on the employment of this group of employees can be ambiguous (Clemens and Wither [2016]). A summary of the definitions of *Minimum wage employees, Bound employees* and *Pseudo-low wage employees,* is provided in Table A.1 in the Appendix A.

We sample a total of 727,298 (253,580) *Bound employees* (*Minimum wage employees*) and 272,702 *Pseudo-low wage employees* for our individual-level analysis making an overall sample size is one million low wage employees. Our sample of *Bound employees* represents the entire universe of such employees in our data; these employees are the focus of our empirical tests. However, our sample of *Pseudo-low wage employees* only represents a randomly sampled subset.

Table 3 provides descriptive statistics for our sample of *Bound employees* and *Pseudo-low wage employees*. The median (mean) *Bound employee* is 25 (31) years old and earns $8.00 ($8.00) an hour as of the date they enter our sample. In contrast, the median (mean) *Pseudo-low wage employee* is 32 (36) years old and earns $10.73 ($10.72) an hour. The median *Bound employee* enters our sample with 1 month of tenure at their current job, while the median *Pseudo-low wage employee* enters with 7 months of tenure.

Bound employees are much more likely to leave their current job than *Pseudo-low wage employees*. Consistent with the findings in Giuliano [2013], we find that 77% of *Bound employees* leave their current job during our sample period. The median tenure of *Bound employees* as of the end of the sample period is only 9 months, and 29% (42%) of *Bound employees* leave their jobs within 3 (6) months of the date of hire. *Pseudo-low wage* employees, in contrast, have a 59% turnover rate, a median tenure of 26 months, and a 3 (6) month turnover rate of 14% (22%). The short job tenure of *Bound employees* is similar to the findings in Dube et al. [2011].

4.2 Establishment-Level Analysis Sample

Our establishment-level sample consists of 2,470 firm-state combinations that employ a material fraction of low wage employees. To measure low wage employment at the establishment-level, we define *Low wage employees* as the total number of employees at an establishment whose wages satisfy

$\omega_{i,t} \leq \$10.00$- i.e. *Bound employees* with an additional $\$1.00 - \$2.00$ buffer.[16,17] The stock and flow of *Low wage employees* is of primary interest in our analysis, as it measures the effect on lower skilled laborers whose wages are in the neighborhood of the minimum wage. We also measure marginally higher-skilled employment at the establishment-level by adjusting the definition of *Pseudo-low wage employees* to the total number of employees with wages satisfying $\omega_{i,t} \in (\$10.00,\ \$15.00]$. A summary of our definitions of employees at the establishment-level is provided in Table A.1 in Appendix A.

In terms of sample construction, we require that establishments employ a material fraction of *Low wage employees* (5% of their workforce) as of the date they enter the sample. This helps alleviate the concern that employment effects are "hidden" due to the inclusion of non-low wage firms (Sabia et al. [2012], Belman and Wolfson [2014], Jardim et al. [2017]). Our final sample consists of $2,470$ establishments from 339 distinct firms, with the median firm having 8 establishments in the treated or control states.[18] As shown in Figure IA.1 in the Internet Appendix, our establishments are concentrated in the retail trade, leisure and hospitality industries. There is, however, a significant number of establishments that belong to the manufacturing, professional and business services, education, health, and finance industries. In addition, our sample establishments' employment is distributed similarly across states as the overall U.S. population (Internet Appendix Figure IA.2).

Table 4 provides descriptive statistics for our sample of 2,470 establishments as of six months prior to treatment. The average establishment in our sample employs 1,784 employees, $1,526$ of

[16]Since we study the stock of low wage employees every period, we will not be able to use the same employee categories as defined in the individual-level analysis. For example, if we focused on the proportion of *Minimum wage* employees before and after treatment, then this proportion may mechanically increase in the treated states in the post-treatment period if there is an equalization of wages for all pre-treatment *Bound employees* at the new minimum wage. Moreover, focusing on *Bound employees* in the pre-treatment period and *Minimum wage* employees in the post-treatment period would also be problematic if some of the *Bound employees* receive wage increases in response to increases in the minimum wage.

[17]We add the buffer both to take into account any wage spillovers to pre-treatment *Bound* employees. Our results are robust to numerous alternative definitions of *Low wage employees*, including definitions of $\omega_{i,t} \leq \$12.50$ and $\omega_{i,t} \leq \$15.00$. Sample results for $\omega_{i,t} \leq \$15.00$ are discussed in Section 6. Note that $\omega_{i,t} \leq \$10.00$ estimates will not bias us if individuals with wages near $10.00 do not experience wage increases in response to increases in the minimum wage (i.e. we can partition the wage distribution into an affected and unaffected component). This is later confirmed in our analysis of the wages of *Pseudo-low wage employees* at the individual-level in the next Subsection. Similar hard cutoffs are used in Jardim et al. [2017] and Cengiz et al. [2018].

[18]Approximately 20% of the firms in our sample have only one establishment in the treated or control states.

which are hourly (non-salary) employees. The average firm (i.e. a collection of establishments) in our sample employs over $20,000$ hourly wage employees across its establishments in the U.S. Therefore, our sample is comprised of relatively large firms in terms of employees, and these firms have a large fraction of their workforce in establishments in the treated and control states.

By construction, *Low wage employees* have a significant presence in the establishments in our sample. The average establishment has 735 *Low wage employees*, and this number is significantly right skewed -e.g. the establishment in the 99th percentile has 30,090 *Low wage employees.* In other words, approximately 25 establishments (one percent of 2,470) in our sample have more than 30,000 *Low wage employees.* These employees make up 43% of the lagged total workforce for the median establishment in our sample and nearly 100% of the lagged workforce for establishments in the 99th percentile. Wages paid to *Low wage employees* account for 21% (96%) of total payroll at the median (99th percentile) establishment.

To summarize, our sample primarily consists of large firms which are present in many states across the U.S. The overlap between the establishments in our individual-level sample and our establishment-level sample is approximately 75%.[19]

5 Individual Wages, Employment, and Turnover

In this section we document the effect of the minimum wage on the employment of existing low wage employees. We begin by analyzing the effect of an increase in the minimum wage on the level and growth of employee wages. We then analyze the effect on individual employment and turnover.

5.1 Individual-level Wage Regressions and Specification Validity

Before we proceed with our analysis of employment, we first document the effect of an increase in the minimum wage on the wages of *Minimum wage employees* and *Bound employees.* This exercise

[19] The overlap is imperfect because we do not require establishments in the individual-level analysis to employ at least 5% of their workforce in *Low wage employees.* All results are robust to restricting the individual-level analysis to the set of establishments in the establishment-level analysis.

serves three purposes: (1) it helps establish the quality of our wage data, (2) it evaluates the extent to which the control variables in our regressions are correlated with minimum wage increases and hence possibly attenuate our employment results, and (3) it documents the effect of minimum wage increases on short-term income trajectories. We start by estimating the following model on our sample of *Minimum wage employees* for the twenty-four month period surrounding the month of treatment[20]:

$$\omega_{i,s,t} = \alpha + \sum_{\tau=-12,\tau\neq-9}^{12} \Gamma_\tau \times \mathrm{Treated_s} \times \mathrm{D}(s,\tau) + \delta_s + \varepsilon_{i,s,t}. \tag{2}$$

The variable $\omega_{i,s,t}$ denotes the hourly wage of a *Minimum wage employee* i in state s in month t. δ_s denotes state fixed effects. The variable Treated_s is a dummy variable that takes a value one if state s implements an increase to its minimum wage, and $D(s,t,\tau)$ is a dummy variable that turns on for all individuals in state s, τ months relative to the treatment month. The excluded category is 9 months before treatment.

The coefficients of interest are the Γ_τs. If our hourly wage data is accurate and timely, then we expect our estimate of Γ_τs in the immediate post-treatment period to reflect the weighted average increase in minimum wage ($\Delta\mathrm{MW_s}$) in our sample, with the weights equal to the number of *Minimum wage employees* in the different treated states. The sample only includes individuals that remain employed at each point in time. Once an individual leaves her current job, she is dropped from the sample for all remaining time periods.

The top panel of Figure 3 displays the results. In the figure, the x axis is the number of months relative to the month of the minimum wage increase. The blue dots correspond to the estimates of the $\{\Gamma_\tau\}_{\tau\neq-9}$ coefficients, and the vertical red bars denote 95% confidence intervals. We find that changes to the minimum wage are reflected in our data within the first month. Our estimate of $\Gamma_{\tau=0}$ almost exactly matches the weighted average minimum wage change of 95.7 cents. We also find evidence of wage growth among *Minimum wage employees* in treated states in the post-treatment period – note though that these estimates are not net of control states' wage growth because the

[20]Again, the pre-treatment period for a control state is the same as that for its paired treated state.

model is estimated without time fixed effects. The upward trend is consistent with prior research that shows a positive association between tenure and wages among low-wage employees (Brown [1989], Meer and West [2016]).

In our main empirical specification we add several additional high dimensional fixed effects and control variables to Equation 2. We evaluate the extent to which these fixed effects control for counterfactual wage growth and absorb the variation coming from the minimum wage increase (Neumark et al. [2014]) by progressively augmenting Equation 2 as follows:

$$\omega_{i,s,t} = \alpha + \sum_{\tau=-12,\tau\neq-9}^{12} \Gamma_\tau \times \text{Treated}_\text{s} \times \text{D}(s,t,\tau) + \delta_i + \left[\delta_{tr(s),f(i),t}\right] + \left[\delta_{tr(s),C(i),t}\right] + \left\{\eta' X_{i,t}\right\} + \varepsilon_{i,s,t}, \quad (3)$$

where δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treatment specific firm-time fixed effects[21], $\delta_{tr(s),C(i),t}$ are treatment specific cohort-time fixed effects[22], and $X_{i,t}$ is a vector of control variables including a quadratic in employee tenure and lagged realizations of GDP PC and HPI growth. The fixed effects in the square brackets account for counterfactual wage growth among *Minimum wage employees* in paired control states and across firms and employment cohorts. The control variables in the curly brackets account for additional heterogeneity stemming from individuals' job tenure or state economic conditions. If our hourly wage data is accurate and if our specification adequately controls for the other differences between the treated and control states, then we expect the Γ_τs in the post-treatment period to reflect the weighted average difference between employee wages in month $\tau = -9$ and the new minimum wage (NEW MW$_s$), net of control employees' average wage growth, of 86.4 cents in our sample.[23]

The middle panel of Figure 3 plots the coefficient estimates after including all fixed effects. We

[21]These are separate time fixed effects for each firm within each treatment-control state pairing, and hence they act as controls for regional firm-level shocks to low wage employees (e.g. regional mass layoffs at Company A). Note that the inclusion of these fixed effects constrains the sample to firms that are present in both treated and control states.

[22]These are separate time fixed effects for when employees enter the sample (e.g. some join in January, 2014, while others join in June, 2014) for each treatment-control state pairing.

[23]This number may not exactly equal the weighted average minimum wage change because employees in both treated and control states may have wage growth between the month they are identified as *Minimum wage employees* and the date in which the minimum wage change is enacted.

find that the “upward drift” in wages found in the top panel of Figure 3 disappears. The coefficient estimates throughout the entirety of the post-treatment period are nearly identical to the weighted average difference between employee wages in month $\tau = -9$ and the new minimum wage. This suggests that, after controlling for counterfactual wage growth, increases in the minimum wage manifest almost entirely as level shifts in hourly wages. Moreover, as displayed in the bottom panel of Figure 3, the inclusion of control variables does not materially affect the results. Combined, these results suggest that our specification is well suited to estimate the effect of minimum wage increases on employment.

In Figure 4, we examine the effects of a higher minimum wage on *Bound employees* and *Pseudo-low wage employees*. The top panel of the Figure merely repeats the results from the bottom panel of Figure 3 for *Minimum wage employees*. The middle panel estimates the same empirical specification on the subsample of *Bound employees*. For the *Bound employees*, we find that the coefficient estimates are almost exactly equal to the pre-treatment difference in average wages and the NEW MW_s for the employees in this sample. That is, we find no evidence of wage spillovers to employees that were previously making above the OLD MW_s. Instead, our results suggest that the wages of *Bound employees* on average just moves up to the NEW MW_s.

In the bottom panel we report the estimates from the subsample of *Pseudo-low wage employees*. We find that minimum wage increases do not affect the wages of these employees. The estimated coefficients are statistically insignificant and close to zero. Thus, consistent with the findings in-Clemens and Wither [2016], *Pseudo-low wage employees* do not appear to be materially affected by increases in the minimum wage. This result also suggests that our empirical specification adequately controls for time-varying economic conditions and lends credibility to our choice of a hard wage cut-off of $10.00 for the establishment-level analysis.

To summarize, even with our most stringent empirical specification, we are able to pick up the exact size of the minimum wage increases. This helps mitigate the common (and valid) concern that such a high-dimensional fixed effects specification “over-controls” and attenuates the results (Neumark et al. [2014], Clemens and Strain [2017]). Instead, our empirical specification demonstrates the ability to trace out counterfactual wage growth, leaving only variation related to the minimum wage increase to be exploited.

5.2 Baseline Results - Employment and Turnover

We now document the effect of the minimum wage on the employment and turnover of existing employees. To do this, we begin by estimating a static version of our baseline model (Equation 3):

$$Y_{i,s,t} = \alpha + \Gamma \times \text{Treated}_{\text{s}} \times \text{Post}_{t,\tau(s)} + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + [\eta' X_{i,t}] + \varepsilon_{i,s,t}, \tag{4}$$

where the outcome variable is an indicator for the *Employment* ($E_{i,s,t}$), *Voluntary Turnover* ($V_{i,s,t}$), or *Involuntary Turnover* ($I_{i,s,t}$) of individual i in state s in month t (defined in Appendix A). Our coefficient of interest is Γ, which compares the relative pre-post difference in the outcome variable between treated and control individuals. Standard errors are clustered two-dimensionally at the state and month level.

The key assumption needed to consistently estimate the parameter Γ is the existence of parallel-trends. That is, in the absence of a minimum wage increase, the change in the conditional average outcomes of the individuals in the treated states is equal to the change in the conditional average outcomes of the individuals in the control states. Critically, the specification in Equation 4 controls for time-varying regional firm shocks ($\delta_{tr(s),f(i),t}$), time-varying regional employment cohort shocks ($\delta_{tr(s),C(i),t}$), time-varying state and individual-level characteristics ($X_{i,t}$), and time invariant differences between individuals (δ_i).

Panel A of Table 5 presents the results from estimating the model on our sample of *Minimum wage employees* for the twenty-four month period surrounding the date of treatment. Odd (even) numbered columns present estimates including (excluding) the bracketed control variables $X_{i,t}$ to assess the extent to which the time varying state and individual-level controls affect our results (Oster [2016]). The coefficient estimates in Columns (1) and (2) suggest that increases in the minimum wage have a *positive* but statistically insignificant effect on employment of existing *Minimum* *wage employees* ($\approx 0.4\%$ effect for a $\approx 10\%$ increase in the minimum wage, t-statistic $= 0.91$). In Columns (3) through (6) we examine voluntary and involuntary turnover. We find that an increase in the minimum wage has an economically insignificant effect on voluntary turnover ($\approx -0.2\%$, t-statistic $= -0.63$) and involuntary turnover ($\approx -0.2\%$, t-statistic $= -1.14$). For all the outcome variables, the coefficient estimates are near-identical with and without the bracketed controls.

Panel B of Table 5 expands the sample to include the full set of *Bound employees*. The results are similar to those presented in Panel A. The coefficient estimates in Columns (1) through (6) suggest that there is a statistically zero effect of a minimum wage increase on the employment and turnover of *Bound employees*. Moreover, the coefficients are similar in magnitude to those in Panel A. This suggests little heterogeneity across *Minimum wage* and *Bound employees* in terms of the response to a higher minimum wage.

While the results from Table 5 suggest null effects of the minimum wage on employment and turnover, subtle intricacies may be hidden by the static DID model. For example, if employers "pre-react" to changes in the minimum wage by increasing layoffs, then the static model might estimate a null employment effect even in the presence of a truly negative employment effect. A dynamic analysis will let us examine if and exactly when the effects manifest.

The top panel of Figure 5 plots the results from estimating the dynamic version of Equation 4 on the subsample of *Minimum wage employees*, where the outcome variable is an indicator for *Employment*. We find that increases in the minimum wage have an economically negligible and statistically insignificant effect on the employment of *Minimum wage employees*. Our estimates of Γ_τs hover around zero throughout the entirety of the post-period and the pre-period. The latter observation suggests a lack of pre-trends and supports our parallel trends assumption.

The middle panel of Figure 5 estimates the dynamic model on the full sample of *Bound employees*. Again, we find that increases in the minimum wage have no discernible effect on employment. In both the pre- and the post-period the coefficient estimates are not different from zero.

In the bottom panel of Figure 5 we examine the effects of the minimum wage on the sample of *Pseudo-low wage employees*. Our tests in Section 5.1 show that these employees do not experience an increase in their hourly wages following the increase in the minimum wage. The dynamics of their employment will hence help us detect the presence of contemporaneous shocks to the local economy in the treated states (and thus the presence of selection bias) and whether firms adjust employment along any additional margins. Here again, we find an insignificant effect on the employment of existing *Pseudo-low wage employees* following the increase in the minimum wage.

Figure 6 plots the coefficient estimates from re-estimating Equation 4 where the outcome variable is an indicator for voluntary turnover. Overall, an increase in the minimum wage does not seem to affect the probability of voluntary turnover of *Minimum wage, Bound,* and *Pseudo-low wage employees*. The coefficient estimates are statistically insignificant and hover around zero in both the pre- and post-period. Figure 7 repeats the analysis with an indicator for involuntary turnover (e.g. firing) as the outcome variable. Again, we find that increases to the minimum wage do not seem to affect the likelihood of involuntary turnover of *Minimum wage*, *Bound*, and *Pseudo-low wage employees*.

Note that even though we do not find an effect of the minimum wage on the level of employment of existing employees, firms may respond to the minimum wage increase by reducing the number of hours. To test the validity of this hypothesis, we re-estimate the baseline model with measures of average employee hours as the outcome variable. While our data does report average hours worked for an employee over recent pay-periods, the coverage is not one hundred percent and requires significant cleaning.[24] Notwithstanding this, in Figure IA.3 in the Internet Appendix we plot the evolution of employee hours in response to increases in the minimum wage. We find no discernible effect of a minimum wage increase on existing employees' hours. This helps rule out the hypothesis that firms are adjusting the hours of the existing employees in response to a minimum wage increase.

Summarizing, the individual-level estimates indicate that an increase in the minimum wage has no significant effect on the employment or rate of turnover of existing low wage employees. These results imply that if firms do adjust employment in response to the minimum wage, then it must be along a different dimension.

[24]We elaborate more on our measures of hours in Section 7.1.

5.3 Robustness

We conduct a variety of robustness tests to support the conclusions from our individual-level analysis. A brief description of each is provided below:

Triple-Differences Analysis: The foremost concern in our analysis is selection bias and the existence of time-varying state-specific correlated omitted variables. To help assuage this concern, we follow Clemens and Wither [2016] and estimate a triple-difference model that exploits variation in the effect of the minimum wage across *Bound* and *Pseudo-low wage employees* residing in the same state. In this test, we include both *Pseudo-low wage employees* and *Bound employees* employees in the sample and use the former as within-state counterfactuals for *Bound employees* by estimating the following regression:

$$\begin{aligned} Y_{i,s,t} = \alpha + \delta_i + \delta_{s,t} + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \delta_{Bound,tr(s),t} \\ + \sum_{\tau=-12,\tau\neq-9}^{12} \Gamma_\tau \times \text{Treated}_s \times \text{D}(s,t,\tau) \times \text{Bound}_i + \eta' X_{i,t} + \varepsilon_{i,s,t} \end{aligned} \tag{5}$$

where $\delta_{s,t}$ are within-state time effects and $\delta_{Bound,tr(s),t}$ are *Bound employee* treated time effects.

The results are shown in Figure IA.4 in the Internet Appendix. We continue to find an insignificant effect of the minimum wage on the employment and turnover of *Bound employees*. In both the pre- and the post-period the coefficient estimates are mostly insignificant and close to zero. Note that these results are not surprising given that our earlier tests indicate no significant changes in the employment dynamics of both *Pseudo-low* wage and *Bound* employees in response to a minimum wage increase. However, the results add support to the hypothesis that selection bias and time-varying state-level confounders are not biasing our coefficient estimates.

Bordering Counties: In another attempt to control for selection bias, we re-estimate our model on the subsample of employees that reside in counties along U.S. state borders. This approach utilizes a more focused (and arguably less objective) type of geographic variation to estimate the employment effects of the minimum wage. Moreover, bordering counties should have similar economic conditions and could serve as better counterfactuals. Figure IA.5 displays the results from the estimation after replacing all of our treatment-specific fixed effects with treatment-border-specific fixed effects. We continue to find no significant effects of the minimum wage on the employment of *Minimum wage*, *Bound*, and *Pseudo-low wage employees*. We find similar null effects when estimating the model with voluntary and involuntary turnover as the outcome variable.

Heterogeneity Across States: Another concern may be that our results are entirely driven by just a subset of the larger treated states. To address this concern, we re-estimate our baseline model after allowing the difference-in-difference coefficient to vary by state. Specifically, we estimate the following model on our sample of *Bound employees*:

$$\begin{aligned} Y_{i,s,t} = \alpha + \sum_{S' \in \text{TreatedStates}} & \Gamma_{S'} \times \text{Treated}_{\text{s}} \times \text{Post}_{t,\tau(s)} \times 1\{s = S'\} \\ & + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \eta' X_{i,t} + \varepsilon_{i,s,t}, \end{aligned} \tag{6}$$

where TreatedStates = {CA, MA, MI, NE, SD, WV} is the set of treated states and $Y_{i,s,t}$ is either employment, voluntary turnover, or involuntary turnover. The coefficients of interest are the $\Gamma_{S'}$s. These coefficients capture the impact of the minimum wage for each treated state. All results are robust to estimating separate panel regressions for each treatment and control state pairing. Table IA.4 in the Internet Appendix presents the results. We find little-to-no heterogeneity in the the $\Gamma_{S'}$ estimates across treated states. For each of the treated states, we find an economically small and statistically insignificant effect of the minimum wage on employment, voluntary turnover, and involuntary turnover.

Heterogeneity Across Observables: In additional tests reported in the Internet Appendix (see Tables IA.5, IA.6 and IA.7), we examine whether our baseline results mask any heterogeneity in the employment effect across individual and firm-level characteristics. We find no meaningful evidence of heterogeneity across the following dimensions: (1) low versus high tenure *Bound employees*[25], (2) low versus high wage *Bound employees*[26], and (3) establishments with a high versus

[25]The hypothesis is that employees with greater tenure may have more firm-specific knowledge and hence be more valuable to the firm (Becker [1962]). In response to an increase in the minimum wage, firms may be more willing to retain such employees and selectively let go employees with low tenure.

[26]The hypothesis is that better-paid *Bound employees* may respond negatively to the wage compression induced

low fraction of low wage employees.

Less and More Saturated Models, and Alternative Clustering Schemes: Our main results are robust to using less saturated fixed effects models for the estimation. For example, we find no material change in our results after replacing $\delta_{tr(s),f(i),t}$ and $\delta_{tr(s),C(i),t}$ with their less saturated counterparts $\delta_{tr(s),t}$, $\delta_{f(i),t}$ and $\delta_{C(i),t}$. In addition, we find similar null results for more saturated models, including models with: (1) time fixed effects for employee job titles (e.g. "cashier-time" effects), (2) treatment specific time fixed effects for employee job titles, and (3) state specific linear time trends. Our dynamic results are also robust to alternative methods for calculating the standard errors, including two dimensional clustering at the company and time level, individual and time level, and one dimensional clustering at the state level.

6 Establishment Employment, Turnover, and Hiring

The results in the previous section suggest that existing minimum wage workers are no less likely to remain employed following an increase in the minimum wage. In this section, we examine the employment impact of the minimum wage at the establishment-level (recall: firm-state combination) to better understand if firms adjust employment along any other margins, such as hiring or layoffs. Our employment data allows us to deconstruct employment changes into distinct components and thus provide a precise description of the effects of the minimum wage on establishment-level employment.

6.1 Establishment-level regressions

We employ the difference-in-differences methodology of Section 5.2 to estimate the employment effects of the minimum wage at the establishment-level. Specifically, for our sample of establishments we estimate static and dynamic variants of the following model:

by the increase in the minimum wage (Akerlof and Yellen [1990]). This could result in higher voluntary turnover for *Bound* employees with wages close to the new minimum wage.

$$Y_{f,s,t} = \alpha + \Gamma \times \text{Treated}_{\text{s}} \times \text{Post}_{t,\tau(s)} + \delta_{f,s} + \delta_{tr(s),t} + \delta_{f,t} + \eta' X_{s,t-1} + \varepsilon_{f,s,t}, \tag{7}$$

where time t is measured in months, the subscript f denotes firms, and the firm-state pair f, s refers to an establishment. The sample period for these tests is the twenty four month window (twelve months before, twelve months after) surrounding an increase in the minimum wage. The outcome variable is generally a measure of the stock or flow of *Low wage employees* (recall: employees with $\omega_{i,t} \leq \$10.00$), although we also examine establishment openings and closures, and employee hours in the following subsections. Our coefficient of interest is Γ, which captures the extent to which the outcome variable is different for establishments from the same firm across treated and control states in the post-treatment period relative to the pre-treatment period.

Similar to before, we include a robust set of controls to ensure we estimate Γ using only within-firm variation across pairs of treated and control states over time. In addition to treatment specific time effects ($\delta_{tr(s),t}$), we include establishment fixed effects ($\delta_{f,s}$) to control for establishment-level time invariant characteristics. We also include firm specific time fixed effects ($\delta_{f,t}$) to control for time-varying heterogeneity at the firm level (e.g. mass layoffs across stores, seasonal employment adjustments by retail firms). We also expand the model to include firm-treatment specific time effects ($\delta_{f,tr(s),t}$) in some specifications to isolate variation coming only from the same firm across treated and control states in the same region at the same point in time. Finally, we control for time-varying economic conditions at the state level by including lagged realizations of GDP per-capita and HPI growth ($X_{s,t-1}$). Standard errors are clustered two-dimensionally at both the state and the month level.[27]

6.2 Baseline Results - Establishment Employment

Panel A of Table 6 presents the results from estimating Equation 7 where the outcome variable is either the fraction of *Low wage employees* (relative to lagged total employment), the natural log-

[27]Our results are robust to additional clustering methods, including one-dimensional state clustering (commonly used in the literature) and three-dimensional clustering at the state, month, and firm level.

arithm of *Low wage employees*, or the natural logarithm of total establishment employment. The coefficient estimate in Column (1) indicates that the fraction of *Low wage employees* in establishments in states that increase the minimum wage declines by approximately 1.0 percentage point in the post-treatment period, relative to the control establishments. In terms of economic magnitudes, a 1.0 percent point decline represents a roughly 2.5% decline from the unconditional mean of 44 percentage points. Column (2) re-estimates the model after including our time-varying state-level control variables. The results are almost identical to those in Column (1). Furthermore, Column (3) presents results after replacing the firm specific time ($\delta_{f,t}$) and treatment specific time effects ($\delta_{tr(s),t}$) with firm-treatment specific time effects ($\delta_{f,tr(s),t}$) . The results are again near-identical to those in Columns (1) and (2), and suggest that region-specific time-varying correlated omitted variables at the firm-level do not materially affect our results.

In Columns (4) through (6) we replace the fraction of *Low wage employees* by its natural logarithm. Conducting such a test helps us understand whether changes in the numerator (i.e. a reduction from the counterfactual amount of *Low wage employees*) or the denominator (i.e. the total size of the workforce) drives our prior findings. The coefficient estimates suggest that there is a statistically significant reduction in the number of *Low wage employees* following a minimum wage increase. The coefficient estimate in Column (5) translates into an approximately nevative 4.5% reponse of low wage employment to a 10% increase in the minimum wage. This is slightly higher than the documented range of $1 - 3\%$ in Neumark and Wascher [2007]. However, relative to other studies in the literature, our data arguably better identifies the set of employees directly affected by the minimum wage and hence limits the scope of any attenuation problems stemming from the inclusion of non-low wage employees. In Columns (7) through (9) we re-estimate the model with the natural logarithm of total establishment employment as the outcome variable. The response of total firm employment to a 10% increase in the minimum wage is approximately -2.5% and falls within the ranges described in Neumark and Wascher [2007]. Similar to the estimates in Columns (1) through (3), our estimates in Columns (4) through (9) are resilient to the inclusion of time-varying state-level controls and firm-treatment specific time effects.

As a robustness test, we repeat the previous analysis using alternative definitions of *Low wage employees* at the establishment-level. Namely we define low wage employees as those earning less than \$15 an hour. We continue to find a reduction in both the fraction and the number of low wage employees. These results are reported in Columns (1-3) of Table IA.8 in the Internet Appendix.

We also estimate a dynamic version of Equation 7 by dividing the sample period into eight quarters, four for the pre-treatment period and four for the post-treatment period, and replacing the static difference-in-difference variable ($\text{Treated}_\text{s} \times \text{Post}_{t(s)}$) with treatment $\times$ quarter interactions. The omitted category in these tests is the third quarter prior to treatment.[28] Coefficient estimates from the dynamic model are plotted in Figure 8. As displayed in the top-left panel, we find that an increase in the minimum wage is associated with a statistically significant decline in the fraction of *Low wage employees* in an establishment. This decline begins in the quarter immediately following the increase in the minimum wage (although not statistically significant), and continues for the three quarters following the increase. The top-right panel of Figure 8 plots the coefficient estimates when the natural logarithm of *Low wage employees* is the outcome variable. We find a decline in the number of such employees following the increase in minimum wage. The dynamics are robust to alternative definitions for *Low wage employees,* as evidenced in the bottom-left panel. In the bottom-right panel of Figure 8 we focus on the natural logarithm of total establishment employment. Again, the results suggest that establishments reduce employment following an increase in the minimum wage. We find no significant evidence of pre-trends across all the models with the coefficient estimates being uniformly insignificant in the pre-treatment period.

Finally, to better gauge the overall economic impact of minimum wage changes on employment, we take the substantial heterogeneity in the size of the firms in our sample into account and perform a weighted least squares estimation.[29] The results are reported in Panel B of Table 6. We find that the magnitude and statistical significance of the coefficient estimates are similar to our OLS estimates. The only exceptions are Columns (1) and (3). In these Columns, the weighted least

[28]We collapse the time dimension to quarters to reduce the noise in our estimates stemming from the smaller establishment-level sample. Our results are robust (albeit noisier) to a month-by-month estimation.

[29]The weights are proportional to the logged size of each establishment. See also Harasztosi and Lindner [2017].

squares estimates are marginally insignificant. Overall, the results in Table 6 and Figure 8 suggest that firms reduce their demand for *Low wage employees* following an increase in the minimum wage.

6.3 How do Establishments Reduce Employment?

In this subsection we examine the mechanisms through which establishments reduce employment and reconcile our establishment results with our individual employment. We examine three possible mechanisms: turnover, hiring, and the opening and closing of locations.

Panel A of Table 7 examines how increases in the minimum wage affect the number of locations within a state and total establishment turnover. We define the *Number of Locations* of establishment f, s as the number of distinct three-digit ZIP-codes in which establishment f, s has employees in state s.[30] We also define the *Change in Number of Locations* as the change in establishment locations in state s from month $t-1$ to month t. As shown in Columns (1) through (6) of Panel A, we find that increases in the minimum wage have no effect on both the *Number of Locations* and the *Change in Number of Locations.*

In Columns (7) through (9) we examine the effect of the minimum wage on establishment-level turnover. We define *Turnover* as the number of employees that either voluntarily or involuntarily leave establishment f, s in month t. We earlier found that there was no change in the turnover of pre-treatment *Bound* and *Pseudo-low wage* employees following a minimum wage increase. The establishment-level variable *Turnover,* captures overall establishment-level turnover which – in addition to pre-treatment *Bound* and *Pseudo-low wage employee* turnover - includes turnover of both higher wage employees and of employees that are hired post-treatment. Analyzing this variable will therefore allow us to see if establishments alter medium-to-higher wage employment as the marginal cost of low wage employment rises. The coefficient estimates in Columns (7) through (9) suggest that there is no significant change in the overall turnover in an establishment following an increase in the minimum wage. In summary, neither a change in the number of locations (e.g. through consolidation) nor an increase in turnover contribute to the reduction in head-count following an

[30] We are unable to extract the exact number of locations for each establishment due to data limitations. The most accurate information we have on business locations is at the three-digit ZIP-code level.

increase to the minimum wage in our sample.

Panel B of Table 7 analyzes how increases in the minimum wage affect establishment hiring policies. Our data allows us to identify the exact month when an employee was hired by an establishment. Hence, our measures of hiring reflect actual hiring and not imputed measures of hiring. Columns (1) through (3) report the coefficient estimates when the model is estimated with the fraction of *Low wage hires* (relative to total employment in month $t-1$) as the outcome variable. We find that firms reduce the rate of low wage hiring by a statistically significant 0.2%. Relative to the unconditional mean of 3%, a 0.2% reduction in low wage hiring represents an economically significant decline of 6.7%. Looking across Columns (1) through (3), we find that the coefficient estimates are unaffected by the inclusion of controls for time-varying state-level variables and firm-treatment specific time shocks.

Columns (4) - (9) replace the fraction of *Low wage hires* with the natural logarithm of *Low wage hires* and *Total hires*. We find an economically and statistically significant reduction on both fronts. The coefficient estimate in Column (5) suggests that establishments reduce *Low wage hires* by 5%, on average, in response to a 10% higher minimum wage. Column (8) shows that this translates into an approximately -3.1% response of *Total hires* to a 10% increase in the minimum wage. The results are robust to alternative definitions of *Low wage hires* (Columns (4) through (9) of Table IA.8) and display no evidence of pre-trends in a dynamic analysis (Figure IA.6 in the Internet Appendix).

Finally, in Table IA.9 in the Internet Appendix we examine exactly *who* is hired less often following an increase in the minimum wage. We split recent hires into three groups based on their age : (1) younger individuals (age ≤ 25), (2) older individuals (age > 25), and individuals whose age we do not know. We find that the magnitude of the employment effect is symmetric across all three groups and similar to the average effects documented in Panel B of Table 7. Thus, there is no robust evidence of employers actively substituting younger workers for more experienced older workers.

In summary, we find evidence that the establishments in our sample reduce employment following increases to the minimum wage. The reduction in employment manifests through reduced rates of hiring and not through increases in turnover of new or existing employees (Section 5) or the closing of locations.

6.4 Robustness

While our model has passed several falsification tests for employment pre-trends and unobservable confounders, time-varying factors at either the establishment or state level could still bias our results. To offer further assurance about the robustness of our results, we implement a synthetic control analysis at the establishment-level as a robustness check. The synthetic control model of Abadie et al. [2010] allows for a more flexible factor structure than difference-in-differences models and takes a data-driven approach to counterfactual selection. Synthetic control analyses have been used in several recent papers studying the minimum wage, including Dube and Zipperer [2015] and Jardim et al. [2017], and are useful for re-examining baseline difference-in-differences results from another perspective.[31]

Details on our procedure for selecting synthetic establishments and our methods for conducting statistical inference can be found in the Internet Appendix. Table IA.10 reports a summary of the analysis. Even under a synthetic control framework, we find a negative and significant effect of the minimum wage on establishment-level employment. The coefficient estimate of -0.01 for the fraction of low wage employees (Column (1)) is near-identical to the difference-in-differences estimates in Table 5. We also recover near-identical estimates for the fraction of low wage hires (Column (2)), and similarly find no impact on the rate of low wage turnover at establishments.

[31]The synthetic control approach is not without its own problems though. In particular, synthetic control models may be prone to over-fitting in the pre-treatment period, are theoretically only valid for a sufficiently long pre-treatment period, and the chosen synthetic controls are difficult to interpret as the number of treated and control units grow large. We prefer to use the difference-in-differences model for the baseline analysis because of its transparency and falsifiability.

7 What Explains the Reduction in Establishment Employment?

We now test additional predictions that are common in the literature on the minimum wage. We begin by testing for possible heterogeneity in the employment effect across firms in the tradable and non-tradable goods industries. We then examine whether firms actively substitute minimum wage labor for higher skilled labor.

Manning [2016], among others, argues that there could be significant differences in the employment effect across tradable and non-tradable goods industries. The idea is that because competition in the non-tradable goods industries is largely local, an increase in the minimum wage is a shock to the cost structure of all firms within the industry. This may make it easier for the firms to adjust on the price margin. Furthermore, non-tradable goods industries rely heavily on local demand (Mian and Sufi [2014]). To the extent an increase in the minimum wage increases the income of low-income households with a higher marginal propensity to consume, this may boost local demand and further increase the ability of firms to adjust on the price margin. Therefore, if firms in the non-tradable goods industries can adjust prices in response to a minimum wage hike, then they may have less pressure to adjust employment. This would predict a muted employment effect for firms in the non-tradable goods industries.

We classify our firms into non-tradable, tradable, and other (and construction) goods industries using the mapping in Mian and Sufi [2014]. The details on this mapping are provided in the Internet Appendix in Tables IA.11 and IA.12. Firms in the non-tradable goods industries make up approximately 60% of the sample (Figure IA.7).

7.1 Which Industries Reduce Employment?

To test whether there is heterogeneity in the employment effect across establishments in tradable and non-tradable industries, we estimate variants of the following triple-differences model:

$$\begin{aligned} Y_{f,s,t} = \alpha &+ \beta \times \text{NonTradable}_{I(f)} \times \text{Treated}_{\text{s}} \times \text{Post}_{t,\tau(s)} + \Gamma \times \text{Treated}_{\text{s}} \times \text{Post}_{t,\tau(s)} \\ &+ \delta_{f,s} + \delta_{tr(s),t} + \delta_{f,t} + \eta' X_{s,t-1} + \varepsilon_{f,s,t} \end{aligned} \tag{8}$$

where $\text{NonTradable}_{I(f)}$ is an indicator that takes a value of one if firm f belongs to a non-tradable goods industry.[32] The outcome variable is a measure of firm employment. The difference-in-differences coefficient, Γ, captures the baseline employment effect for firms in the tradable and other goods industries and is estimated using the variation described in Section 6.2. The triple-differences coefficient, β, measures the extent to which the employment effect is different for the subsample of firms in the non-tradable goods industries as opposed to the tradable and other goods industries. The coefficient sum, $\beta + \Gamma$, thus measures the total employment effect on firms in the non-tradable goods industries. The model includes our standard set of interactions and fixed effects, and the necessary non-tradable industry $\times$ month fixed effects are subsumed by $\delta_{f,t}$.

Table 8 presents the coefficient estimates from estimating Equation 8 where the outcome variable is either the fraction of *Low wage employees* or the natural logarithm of *Low wage employees*. As shown in Columns (1) through (3), there is a negative and statistically significant baseline effect (Γ) in the tradable and other goods industries. Relative to the unconditional mean of 44 percentage points, the coefficient estimate of -1.8 percentage points represents an approximately 4.1% reduction in low wage employment. We find a significant amount of heterogeneity across goods-producing industries. The estimate of the incremental effect for non-tradable firms, β, is both positive and statistically significant ($\beta = 0.015$), implying that non-tradable firms reduce employment by a smaller amount following an increase in the minimum wage. In fact, the net employment effect for firms in non-tradable goods industries is both statistically and economically not different from zero ($\Gamma + \beta = -0.003$).

[32]Our results are robust to excluding both construction and other industry firms from our sample.

In Columns (4) through (6) we repeat the analysis with the natural logarithm of *Low wage employees* as the outcome variable. We again find similar results: firms in the tradable and other goods industries reduce low wage employment in response to a higher minimum wage while firms in the non-tradable goods industries do not ($\beta + \Gamma = -0.12$ and insignificant). The same interpretation also holds when we repeat the analysis with the natural logarithm of total employment as the outcome variable, although the incremental effect (β) becomes marginally insignificant.

Note that even though we do not find a significant effect of the minimum wage on the level of employment at establishments in the non-tradable goods industries, these establishments could adjust the number of hours of their workers in response to the minimum wage hike. To test this, we re-estimate the baseline model (Equation 7) on the subsample of establishments in the non-tradable goods industries with measures of average employee hours as the outcome variable.[33] Table 9 presents the coefficient estimates. For firms in the non-tradable goods industries, we find negligible effects of the minimum wage on the natural logarithm of average hours for low wage and total employees. The coefficient estimates are all centered around zero with t-statistics in the neighborhood of 1. We obtain identical results when we implement a triple difference specification after including all firms in the sample.

7.2 Do Tradables Substitute Labor?

As a final test we examine whether firms in the tradable and other goods industries adjust on margins other than total employment. That is, conditional on firms being in an industry that reduces low wage labor, we evaluate if they substitute low wage employees for marginally higher skilled employees in response to a higher minimum wage. We use the natural logarithm of the stock of *Pseudo-low wage employees* (recall: $\omega_{i,t} \in (\$10,00\,,\,\$15.00]$) as our proxy for marginally higher skilled employees. As we saw in Section 4, existing *Pseudo-low wage employees* are no less likely to

[33] We use two measures of average employee hours. The first measure is the average number of hours as reported in our dataset (*AvgHours*). The disadvantage of this measure is that it has many missing values. Our dataset also reports annualized pay at each point in time with much greater regularity. The annualized pay is calculated as the total pay during a pay period times the number of pay periods during the year. We use the annualized pay, the frequency of the pay period and the hourly wage rate to calculate the implied number of hours worked during a period (*ImpHours*). This forms our second measure of number of hours.

be employed following an increase in the minimum wage. But this analysis does not say anything about the hiring rates of these employees, nor does it say anything specific about the establishments in the tradable goods industries.

The results from estimating the model with the natural logarithm of *Pseudo-low wage employees* as the outcome variable are presented in Table 10. In Columns (1) through (3) we estimate the model on the full sample (i.e. including non-tradable goods industries as well). We find that, on average, there is no substitution away from *Low wage employees* to *Pseudo-low wage employees*. The coefficient estimate on Γ is 0.00 (t-statistic = 0.01), implying no evidence of an unconditional substitution effect. Conditioning on the tradable and other goods industries, however, reveals a slightly different story. Columns (4) through (6) report the coefficient estimates after we confine the sample to firms in the tradable and other goods industries. We find a positive and weakly statistically significant substitution effect. As reported in Column (4), the response of *Pseudo-low wage* employment to a 10% increase in the minimum wage is approximately 2.1% (t-stat = 1.65). This effect is robust to the inclusion of time-varying state-level controls (Column (5)), but fades away in the most stringent specification which includes firm-treatment specific fixed effects (Column (6)). Note that the sample in this specification is roughly one-half that in Columns (1) through (3) because we are conditioning on tradable and other goods industries. This could suggest lower power in this specification.

The results in Tables 8, 9 and 10 are generally consistent with the predictions outlined at the beginning of the section. While the average effect of the minimum wage on employment is negative, this effect is confined to the tradable and other goods industries. Firms in the non-tradable goods industries exhibit no employment effects in response to a 10% minimum wage increase. Moreover, these firms do not appear to reduce the number of hours. We cannot rule out that these firms reduce benefits, training, or other programs that contribute to the marginal cost of labor. Finally, we find suggestive evidence that firms in the tradable goods sector substitute low wage labor for marginally higher skilled labor following a 10% increase in the minimum wage.

8 Conclusion

The effect of statutory minimum wages on employment is an important policy question. To answer this question, we use administrative wage data on one million hourly wage employees from over 300 firms spread across 23 two-digit NAICS industries and estimate the effect of six isolated minimum wage changes on employment. Our results suggest that the effect of minimum wages on employment is nuanced. We find that the proportion and the amount of low wage employees within firms declines in states that experience an increase in the minimum wage. This occurs through a reduction in hiring, and not through increases in turnover or the closing of locations. Existing low wage employees directly affected by an increase in the minimum wage are no less likely to remain employed as compared to their otherwise identical counterparts in states without changes to the minimum wage.

We find that the employment effect is relatively homogeneous across individual- and firm- level observables. However, there is significant heterogeneity across different types of goods-producing industries. On average, firms in the non-tradable goods industries do not reduce low wage employment. This is both in terms of head-count and employee hours. Firms in the tradable and other goods industries, on the other hand, exhibit negative employment effects in terms of head-count (but not hours). We also find some weak evidence that firms in the tradable and other goods industries substitute low-wage workers with marginally higher skilled workers following an increase in the minimum wage.

Our paper makes several contributions to the existing literature. First, we are unique in using administrative wage data to identify minimum wage employees across a number of industries to evaluate the employment effect. We can therefore speak to both the average effect of the minimum wage on employment and how this effect varies across industries. Second, our data also allows us to analyze the effect of the minimum wage on both existing employees and new hires. Third, we are able to highlight the channel through which firms adjust employment in response to higher minimum wages - e.g. turnover, hiring, hours, or changing the number of locations. Fourth, we are able to analyze how the minimum wage affects the composition of a firm's workforce (e.g. substitution effects) and how this varies across subsamples of the population. Finally, we are able to control for a wide variety of confounding factors while still ensuring that sufficient residual variation remains to identify our effects of interest.

Our results should be interpreted with the following caveats in mind. The employment effect of a minimum wage hike may depend on the status of the labor market (Clemens and Wither [2016]), the size of the minimum wage increase (Jardim et al. [2017]) and may differ across firms of different sizes. We estimate the employment effect during 2014-16 when the labor market was relatively benign, the average size of the minimum wage increase in our sample is 10%, and our sample predominantly consists of large firms. We also cannot speak to the total welfare effects of the minimum wage - although we can document that existing minimum wage workers seem to be better off in terms of wages and no worse off in terms of employment likelihood.

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Table 1: Descriptive Statistics - Minimum Wage Changes Analyzed

This table lists the minimum wage changes studied in our analysis There are a total of six treated states and twelve control states. The definition of treated and control states is provided in Section 3.2 of the text. MW Δ date refers to the year-month in which a treated state adjusts its minimum wage in our sample. Last MW Δ date refers to the year-month in which a treated state last adjusted its minimum wage prior to our sample period. Beginning (End) MW refers to the minimum wage at the beginning (end) of the sample period.

State Minimum Wage Changes						
State Postal Code	**MW Δ Date**	**Last MW Δ Date**	**Beginning MW**	**End MW**	**MW ΔSize**	**Control States**
CA	201407	200801	8.00	9.00	1.00	(NV,UT)
MA	201501	200801	8.00	9.00	1.00	(NH,PA,VA)
MI	201409	200907	7.40	8.15	0.75	(IL,IN,WI)
NE	201501	200907	7.25	8.00	0.75	(IA,KS)
SD	201501	200907	7.25	8.50	1.25	(ND)
WV	201501	200807	7.25	8.00	1.00	(KY)

Table 2: Descriptive Statistics - Macroeconomic Factors in Treatment and Control States

This table contains descriptive statistics at the state level as of the quarter immediately preceding a minimum wage change in each treated state. The definition of treated and control states is defined in Section 3. In the top portion of the table, each cell documents the difference between the value in the treated state and the average value in the control states. The bottom portion of the table reports the difference (and log population weighted difference) in means across the treated and control groups. t-statistics are reported below in mean differences, and both means and t-statistics are computed from regressions with the assumption of homoskedastic standard errors. All variables are defined in in the Appendix. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

State Postal Code	**Control States**	**Pop. (MM)**	**Pop. Growth**	**White Latino**	**Age/ ≤ 35**	**GDP PC (M)**	**GDPPC Growth%**	**Unemp Rate**	**HPI Growth**	**Democrat Vote**	**Union Rate**
CA	NV,UT	35.82	-0.56	-10.50	-3.31	12.31	1.99	1.80	0.60	21.69	7.25
MA	NH,PA,VA	-0.72	0.29	-0.33	1.11	12.62	0.15	0.63	-0.17	8.95	4.53
MI	IL,IN,WI	1.51	-0.04	-3.67	-1.57	-6.39	-0.15	1.20	0.60	2.76	2.00
NE	IA,KS	-1.12	0.35	-0.50	1.05	5.48	1.07	-1.10	0.98	-6.96	-1.75
SD	ND	0.11	-1.35	-3.00	-2.78	-24.43	-5.17	0.60	1.26	1.18	-0.10
WV	KY	-2.56	-0.51	6.00	-3.80	-2.14	0.97	0.80	-2.58	-2.26	-0.40
Diff. in Means		4.61	-0.15	-1.08	-1.06	0.59	0.06	0.43	0.10	2.00	1.45
Treated and Control		(1.05)	(-0.48)	(-0.30)	(-0.59)	(0.14)	(0.08)	(0.51)	(0.23)	(0.39)	(0.73)
Weighted Diff. in Means		-	-0.09	-1.57	-0.90	1.39	0.21	0.57	0.11	3.00	1.76
Treated and Control		-	(-0.30)	(-0.43)	(-0.52)	(0.34)	(0.32)	(0.69)	(0.25)	(0.59)	(0.91)

Table 3: Descriptive Statistics - Bound and Pseudo-Low Wage Employees

This table contains descriptive statistics on the one million Bound and Pseudo-low wage employees in our main sample. There are approximately 727,000 Bound employees (254,000 – 35% – of which earn exactly the minimum wage) and 273,000 Pseudo-low wage employees. The definition of Minimum Wage, Bound, and Pseudo-low Wage Employees is provided in Section 4 of the text. Wages are measured prior to treatment, and End Tenure, Turnover, and Voluntary | Turnover are measured as of the end of the sample period. All of variables are measured as of the month the employee enters the sample. Continuous variables are winsorized at the 5% and 95% levels in the descriptive statistics only. Estimated hours are computed by flooring raw hours at 10 and capping raw hours at 40 for the sample of employees reporting at least 10 hours of work per week.

Variable	**Mean**	**StDev**	**1st**	**25th**	**Median**	**75th**	**99th**
Bound (N=727,000)							
Hourly Wage	8.00	0.51	7.25	7.50	8.00	8.25	9.00
Estimated Hours	27.81	10.36	10	20	28	40	40
Age (Years)	30.91	12.65	19	21	25	39	61
Beginning Tenure (Months)	7.60	16.12	0	1	1	6	93
End Tenure (Months)	17.24	21.49	1	3	9	23	117
Turnover?	0.77	0.42	0	0	1	1	1
Voluntary | Turnover	0.76	0.42	0	1	1	1	1
End Tenure $\leq$ 3 Months?	0.29	0.45	0	0	0	1	1
End Tenure $\leq$ 6 Months?	0.42	0.49	0	0	0	1	1
Pseudo-Low Wage (N = 273,000)							
Hourly Wage	10.72	0.84	9.19	10.00	10.73	11.34	12.50
Estimated Hours	32.98	9.28	11	22	40	40	40
Age	35.91	13.58	19	24	32	48	61
Beginning Tenure	23.63	31.13	0	1	7	36	93
End Tenure	38.58	36.99	1	9	26	57	117
Turnover?	0.59	0.49	0	0	1	1	1
Voluntary | Turnover	0.82	0.39	0	1	1	1	1
End Tenure $\leq$ 3 Months?	0.14	0.35	0	0	0	0	1
End Tenure $\leq$ 6 Months?	0.22	0.41	0	0	0	0	1

Table 4: Descriptive Statistics - Establishment Employment

This table contains descriptive statistics on the 2,470 establishments (firm-state combinations) in our sample. The descriptive statistics are as of six months before a matched treated state increases its minimum wage. The sample is conditional upon establishments having at least one low wage employee and low wage employees constituting at least 5% of the workforce as of the month the establishment enters the sample. However, establishments are allowed to venture below the 5% floor after they enter the sample as evidenced in the below data. The establishments represent 339 distinct firms from 23 two-digit NAICS industries (20 BLS Industries, 12 BLS Supersectors). The median (first quartile) firm in the sample has an establishment in 8 (2) out of the 18 possible states in the sample. The definition of Low Wage Employees and Pseudo-Low Wage Employees at the establishment level is provided in Section 4.2 of the text. The definition of treated and control states is provided in Section 3.2 of the text. All variables are defined in Appendix A.

Variable	**N**	**Mean**	**StDev**	**1st**	**25th**	**Median**	**75th**	**99th**
Employment Stock								
Total Employees	2,470	1,784	8,092	1	41	204	853	30,090
Hourly Employees	2,470	1,526	7,260	1	30	164	697	26,058
Low Wage Employees	2,470	735	3,760	1	12	74	309	11,042
Pseudo-Low Wage Employees	2,470	471	2,361	0	5	41	201	7,282
$\text{LowWage}_{f,s,t}/\text{Total}_{f,s,t}$	2,470	0.44	0.27	0.01	0.20	0.43	0.67	1.00
Employment Flow								
Total Hires	2,470	28.61	84.52	0	0	1	12	293
Low Wage Hires	2,470	19.56	61.65	0	0	3	19	460
$\text{LowWageHires}_{f,s,t}/\text{HourlyTotal}_{f,s,t}$	2,470	0.03	0.04	0.00	0.00	0.00	0.04	0.23
Employment Growth	2,470	(0.00)	0.05	-0.18	-0.01	0.00	0.01	0.15
Wages								
Average Annual Wages (Total Employees)	2,470	33,621	32,517	5,752	18,357	25,350	40,026	139,974
Average Annual Wages (Hourly Employees)	2,470	23,029	17,983	5,122	13,006	18,907	27,826	92,166
Average Annual Wages (Low Wage Employees)	2,470	11,925	5,055	3,527	8,535	11,104	14,287	28,858
Dollar Fraction Low Wage Employees	2,470	0.25	0.24	0.01	0.09	0.21	0.36	0.96

Table 5: Individual DD Regression - Individual Employment and Turnover

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

$$Y_{i,s,t} = \alpha + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} + \eta' X_{i,t} + \varepsilon_{i,s,t}$$

where δ_i are individual fixed effects, $\delta_{tr(s),f(i),,t}$ are treated$\times$ firm $\times$ time fixed effects, $\delta_{C(i),t}$ are treated $\times$ cohort $\times$ time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in employee tenure and lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{i,s,t}$, is either (1): an indicator for employment ($E_{i,s,t}$), (2) an indicator for voluntary turnover ($V_{i,s,t}$), or (3) an indicator for involuntary turnover ($I_{i,s,t}$), as defined in Appendix A. The variable Treated$_s$ is an indicator equal to one if state s is treated, and Post$_{\tau(s),t}$ is an indicator equal to one if for all months t after the month of treatment $\tau(s)$, and zero otherwise. A description of treated and control states is provided in Section 3.2. Standard errors are calculated by clustering two-dimensionally at state and month level, and t-statistics are reported below the coefficient estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Minimum Wage Employees						
Explanatory Variables	$E_{i,t}$ **(1)**	$E_{i,t}$ **(2)**	$I_{i,t}$ **(3)**	$I_{i,t}$ **(4)**	$V_{i,t}$ **(5)**	$V_{i,t}$ **(6)**
Treated$_s$ $\times$ Post$_{t,\tau(s)}$	0.005	0.004	-0.002*	-0.002	-0.002	-0.002
	(1.19)	(0.91)	(-1.69)	(-1.14)	(-0.69)	(-0.63)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$Firm $\times$ Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$Cohort$\times$ Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	No	Yes	No	Yes
N	2,418,459	2,414,220	2,418,459	2,414,220	2,418,459	2,414,220

Panel B: Bound Employees						
Explanatory Variables	$E_{i,t}$ **(1)**	$E_{i,t}$ **(2)**	$I_{i,t}$ **(3)**	$I_{i,t}$ **(4)**	$V_{i,t}$ **(5)**	$V_{i,t}$ **(6)**
Treated$_s$ $\times$ Post$_{t,\tau(s)}$	0.004	0.003	-0.002	-0.002	-0.002	-0.001
	(0.50)	(0.37)	(-0.98)	(-0.84)	(-0.28)	(-0.19)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$Firm $\times$ Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$Cohort$\times$ Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	No	Yes	No	Yes
N	7,615,770	7,602,483	7,615,770	7,602,483	7,615,770	7,602,483

Table 6: Establishment DD Regressions - Employment

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

$$Y_{f,s,t} = \alpha + \delta_{f,s} + \delta_{tr(s),t} + \delta_{f,t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} + [\eta' X_{s,t-1}] + \left\{\delta_{f,tr(s),t}\right\} + \varepsilon_{f,s,t}$$

where $\delta_{f,s}$ are firm-state (*establishment*) fixed effects, $\delta_{tr(s),t}$ are treated $\times$ time fixed effects, $\delta_{f,t}$ are firm$\times$ time fixed effects, $X_{s,t-1}$ is a vector of control variables including lagged realizations of quarterly HPI and GDP PC growth, and $\delta_{f,tr(s),t}$ are firm $\times$ treated $\times$ time fixed effects. The outcome variable, $Y_{f,s,t}$, is either a measure of the fraction of total firm employment or a measure of the level of total firm employment. The variables are defined in full in Appendix A. The variable Treated_s is an indicator equal to one if state s is treated and $\text{Post}_{t,\tau(s)}$ is an indicator equal to one for all months after the month of treatment. A description of treated and control states is provided in Section 3.2. Standard errors are calculated by clustering two-dimensionally at the state and month level. t-statistics are reported below the coefficient estimates, with *, **, and *** indicating statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Establishment Employment (Equally Weighted)

Explanatory	$\text{LowWage}_{f,s,t}/\text{Total}_{f,s,t-1}$			$\log(\text{LowWage})_{f,s,t}$			$\log(\text{Total})_{f,s,t}$		
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
$\text{Treated}_s \times \text{Post}_{t,\tau(s)}$	-0.010**	-0.009**	-0.009*	-0.048***	-0.044***	-0.052**	-0.026**	-0.025**	-0.028*
	(-2.50)	(-2.25)	(-1.80)	(-2.67)	(-2.59)	(-2.08)	(-2.17)	(-2.08)	(-1.75)
Firm × State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated × Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm × Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm × Treated × Time FE	No	No	Yes	No	No	Yes	No	No	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	60,903	59,570	59,570	60,903	59,570	59,570	60,903	59,570	59,570

Panel B: Establishment Employment (Low Wage Employee Headcount Weighted)

Explanatory	$\text{LowWage}_{f,s,t}/\text{Total}_{f,s,t-1}$			$\log(\text{LowWage})_{f,s,t}$			$\log(\text{Total})_{f,s,t}$		
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
$\text{Treated}_s \times \text{Post}_{t,\tau(s)}$	-0.008	-0.008*	-0.008	-0.043**	-0.039**	-0.055**	-0.017*	-0.017*	-0.025*
	(-1.60)	(-2.00)	(-1.33)	(-2.39)	(-2.29)	(-2.29)	(-1.89)	(-1.89)	(-1.79)
Firm × State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated × Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm × Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm × Treated × Time FE	No	No	Yes	No	No	Yes	No	No	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	60,903	59,570	59,570	60,903	59,570	59,570	60,903	59,570	59,570

Table 7: Establishment DD Regressions - How do Employers Reduce Employment?

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

$$Y_{f,s,t} = \alpha + \delta_{f,s} + \delta_{tr(s),t} + \delta_{f,t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} + [\eta' X_{s,t-1}] + \left\{\delta_{f,tr(s),t}\right\} + \varepsilon_{f,s,t}$$

where $\delta_{f,s}$ are establishment fixed effects, $\delta_{tr(s),t}$ are treated $\times$ time fixed effects, $\delta_{f,t}$ are firm$\times$ time fixed effects, $X_{s,t-1}$ is a vector of control variables including lagged realizations of quarterly HPI and GDP PC growth, and $\delta_{f,tr(s),t}$ are firm $\times$ treated $\times$ time fixed effects. The outcome variable, $Y_{f,s,t}$, is either a measure of firm turnover or firm hiring. The variables are defined in full in Appendix A. The variable Treated_s is an indicator equal to one if state s is treated and $\text{Post}_{t,\tau(s)}$ is an indicator equal to one for all months after the month of treatment. A description of treated and control states is provided in Section 3.2. Standard errors are calculated by clustering two-dimensionally at the state and month level. t-statistics are reported below the coefficient estimates, with *, **, and *** indicating statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Establishment Employment - Geographic Presence and Firm Turnover

Explanatory	$\log(\text{Number of Locations})_{f,s,t}$			$\log(1+\Delta\ \text{Number of Locations})_{f,s,t}$			$\log(\text{Turnover})_{f,s,t}$		
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
$\text{Treated}_s \times \text{Post}_{t,\tau(s)}$	-0.012	-0.011	-0.009	-0.0005	-0.001	-0.002	-0.024	-0.030	-0.040
	(-1.33)	(-1.38)	(-0.90)	(-0.10)	(-0.17)	(-0.29)	(-1.14)	(–1.58)	(-1.43)
Firm $\times$ State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Treated $\times$ Time FE	No	No	Yes	No	No	Yes	No	No	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	60,881	59,570	59,570	60,881	59,570	59,570	60,991	59,570	59,570

Panel B: Establishment Employment - Hiring

Explanatory	$\text{LowWageHires}_{f,s,t}/\text{Total}_{f,s,t-1}$			$\log(\text{LowWage Hires})_{f,s,t}$			$\log(\text{Total Hires})_{f,s,t}$		
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
$\text{Treated}_s \times \text{Post}_{t,\tau(s)}$	-0.002***	-0.002**	-0.002***	-0.055**	-0.050**	-0.064**	-0.030***	-0.031***	-0.042***
	(-2.63)	(-2.49)	(-2.70)	(-2.29)	(-2.17)	(-2.37)	(-3.75)	(-3.44)	(-4.20)
Firm $\times$ State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Treated $\times$ Time FE	No	No	Yes	No	No	Yes	No	No	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	60,903	59,570	59,570	60,903	59,570	59,570	60,903	59,570	59,570

Table 8: Establishment DD Regression - Explaining Industry Heterogeneity for Low Wage Employees

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

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

where $\delta_{f,s}$ are firm-state (*establishment*) fixed effects, $\delta_{tr(s),t}$ are treated $\times$ time fixed effects, $\delta_{f,t}$ are firm$\times$ time fixed effects, $X_{s,t-1}$ is a vector of control variables including lagged realizations of quarterly HPI and GDP PC growth, and $\delta_{f,tr(s),t}$ are firm $\times$ treated $\times$ time fixed effects. The outcome variable, $Y_{f,s,t}$, is a measure of firm employment. The variables are defined in full in Appendix A. The variable Treated_s is an indicator equal to one if state s is treated and $\text{Post}_{t,\tau(s)}$ is an indicator equal to one for all months after the month of treatment. A description of treated and control states is provided in Section 3.2. The variable $\text{NonTradable}_{I(f)}$ is an indicator equal to one if firm i is in the non-tradable goods industry, and zero otherwise. Standard errors are calculated by clustering two-dimensionally at the state and month level. t-statistics are reported below the coefficient estimates, with *, **, and *** indicating statistical significance at the 10%, 5%, and 1% level, respectively.

Explanatory Variables	$\text{LowWage}_{f,s,t}/\text{Total}_{f,s,t-1}$ **(1)**	**(2)**	**(3)**	$\log(\text{LowWage})_{f,s,t}$ **(4)**	**(5)**	**(6)**	$\log(\text{Total})_{f,s,t}$ **(7)**	**(8)**	**(9)**
$\text{Treated}_s \times \text{Post}_{t,\tau(s)}$	-0.018***	-0.018***	-0.019**	-0.079***	-0.075***	-0.108***	-0.041***	-0.040**	-0.045**
	(-2.57)	(-3.00)	(-2.38)	(-3.04)	(-3.13)	(-3.27)	(-2.18)	(-2.11)	(-2.14)
$\text{Treated}_s \times \text{Post}_{t,\tau(s)}$	0.015*	0.015**	0.017*	0.058*	0.058*	0.096**	0.030	0.028	0.032
$\times \text{NonTradable}_{I(f)}$	(1.88)	(1.88)	(1.70)	(1.76)	(1.76)	(2.40)	(1.30)	(1.22)	(1.60)
Firm $\times$ State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Treated $\times$ Time FE	No	No	Yes	No	No	Yes	No	No	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
$F: \beta + \Gamma = 0$	FTR	FTR	FTR	FTR	FTR	FTR	FTR	FTR	FTR
N	60,903	59,570	59,570	60,903	59,570	59,570	60,903	59,570	59,570

Table 9: Establishment DD Regressions - Do Non-Tradable Firms Reduce Hours?

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

$$Y_{f,s,t} = \alpha + \delta_{f,s} + \delta_{tr(s),t} + \delta_{f,t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} + \left[\eta' X_{s,t-1}\right] + \left\{\delta_{f,tr(s),t}\right\} + \varepsilon_{f,s,t}$$

where $\delta_{f,s}$ are firm-state (*establishment*) fixed effects, $\delta_{tr(s),t}$ are treated $\times$ time fixed effects, $\delta_{f,t}$ are firm$\times$ time fixed effects, $X_{s,t-1}$ is a vector of control variables including lagged realizations of quarterly HPI and GDP PC growth, and $\delta_{f,tr(s),t}$ are firm $\times$ treated $\times$ time fixed effects. The outcome variable, $Y_{f,s,t}$, is a measure of employee hours. The variables are defined in full in Appendix A. The variable Treated_s is an indicator equal to one if state s is treated and $\text{Post}_{t,\tau(s)}$ is an indicator equal to one for all months after the month of treatment. A description of treated and control states is provided in Section 3.2. The sample is restricted to employers in the non-tradable goods industries. Standard errors are calculated by clustering two-dimensionally at the state and month level. t-statistics are reported below the coefficient estimates, with *, **, and *** indicating statistical significance at the 10%, 5%, and 1% level, respectively.

Explanatory	log(AvgLowWageHours)			log(ImpLowWageHours)			log(AvgHours)			log(ImpHours)		
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
$\text{Treated}_s \times \text{Post}_t$	-0.004	-0.003	-0.005	-0.003	-0.001	-0.004	-0.005	-0.004	-0.008	-0.006	-0.004	-0.008
	(-0.67)	(-0.50)	(-0.83)	(-0.50)	(-0.17)	(-0.57)	(-0.71)	(-0.57)	(-1.33)	(-1.00)	(-0.67)	(-1.14)
Firm $\times$ State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Treated $\times$ Time FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	13,226	12,933	12,933	28,477	27,867	27,867	13,727	13,426	13,426	29,470	28,826	28,826

Table 10: Establishment DD Regressions - Do Tradable Firms Substitute Labor Types?

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

$$Y_{f,s,t} = \alpha + \delta_{f,s} + \delta_{tr(s),t} + \delta_{f,t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} + \left[\eta' X_{s,t-1}\right] + \left\{\delta_{f,tr(s),t}\right\} + \varepsilon_{f,s,t}$$

where $\delta_{f,s}$ are firm-state (*establishment*) fixed effects, $\delta_{tr(s),t}$ are treated $\times$ time fixed effects, $\delta_{f,t}$ are firm$\times$ time fixed effects, $X_{s,t-1}$ is a vector of control variables including lagged realizations of quarterly HPI and GDP PC growth, and $\delta_{f,tr(s),t}$ are firm $\times$ treated $\times$ time fixed effects. The outcome variable, $Y_{f,s,t}$, is a measure of *Pseudo-low wage* employment. The variables are defined in full in Appendix A. The variable Treated_s is an indicator equal to one if state s is treated and $\text{Post}_{t,\tau(s)}$ is an indicator equal to one for all months after the month of treatment. A description of treated and control states is provided in Section 3.2. The row Sample Choice denotes whether the model is estimated across all industries (Columns (1) through (3)) or only the tradable and other goods industries (Columns (4) through (6)). Standard errors are calculated by clustering two-dimensionally at the state and month level. t-statistics are reported below the coefficient estimates, with *, **, and *** indicating statistical significance at the 10%, 5%, and 1% level, respectively.

Explanatory	$\log(\text{PseudoLowWage})_{f,s,t}$					
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
$\text{Treated}_s \times \text{Post}_{t,\tau(s)}$	0.000	-0.002	-0.010	0.021*	0.021*	0.008
	(0.01)	(-0.17)	(-0.48)	(1.65)	(1.75)	(0.40)
Firm × State FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated × Time FE	Yes	Yes	No	Yes	Yes	No
Firm × Time FE	Yes	Yes	No	Yes	Yes	No
Firm × Treated × Time FE	No	No	Yes	No	No	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes
Sample Choice	All Industries - Full Sample			Tradable, Other, and Construction- Subsample		
N	60,903	59,570	59,570	28,385	27,777	27,777

Figure 1: Map of Treated and Control States

This figure plots 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.

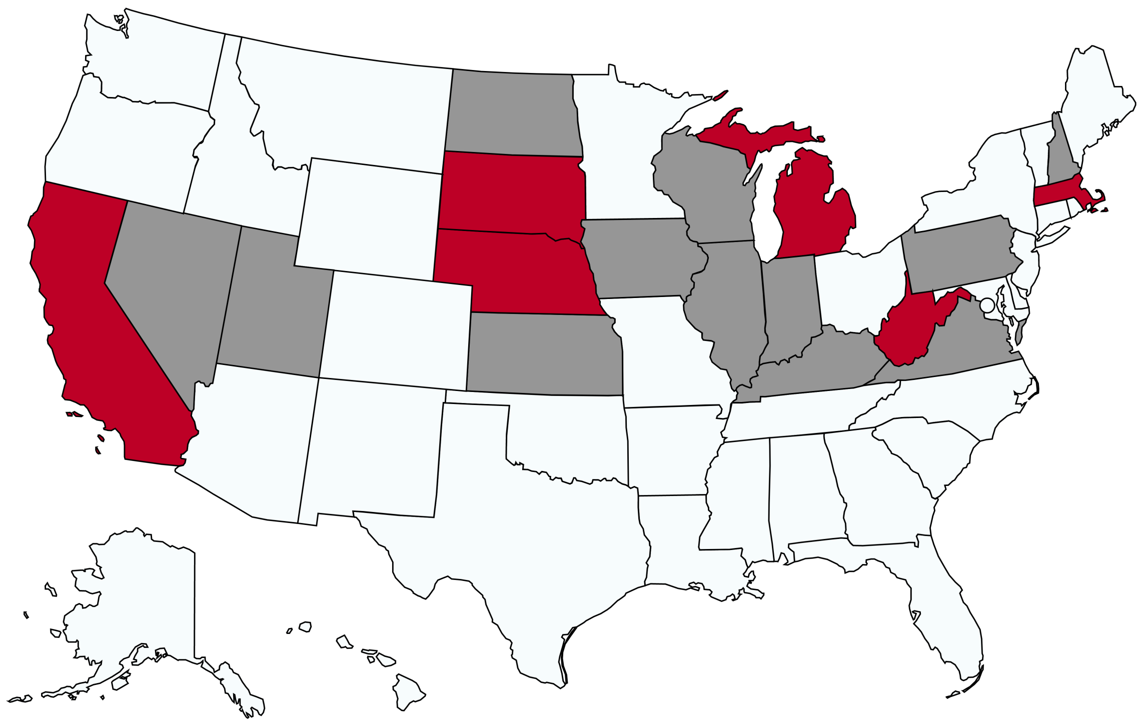


Figure 2: Macroeconomic Trends in Treated and Control States

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

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

where the $y_{s,t}$ is either the *Unemployment Rate, GDP PC, Population,* or *HPI* (logged levels and growth) of state s in year t, δ_s are state fixed effects, $\delta_{tr(s),t}$ are treated $\times$ year fixed, Treated$_s$ is a dummy variable that takes a value one if state s is a treated state, and $D(\tau)$ is a dummy variable equal to one for in year τ. 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 3.2 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 90% confidence intervals. Standard errors are clustered at the state level.

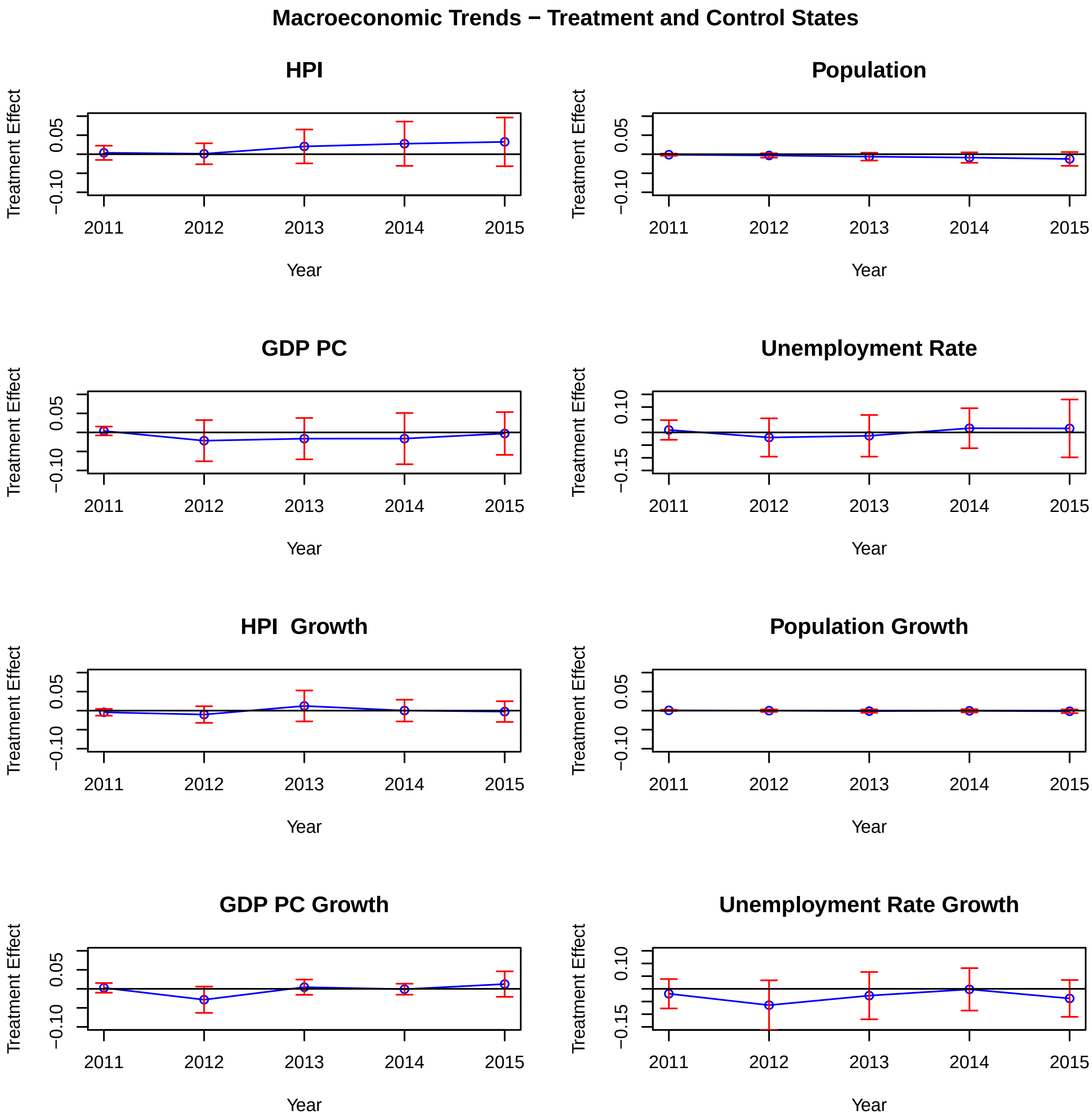


Figure 3: Evolution of Wages for Minimum Wage Employees

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

$$\omega_{i,s,t} = \alpha + \delta_s + [\delta_i] + \left[\delta_{tr(s),f(i),t}\right] + \left[\delta_{tr(s),C(i),t}\right] + \sum_{\tau=-12,\tau\neq-9}^{12} \Gamma_\tau \text{Treated}_s \times \text{D}(s,t,\tau) + [\eta' X_{i,t}] + \varepsilon_{i,s,t}$$

where $\omega_{i,s,t}$ is the wage rate of individual i in month t, δ_s are state fixed effects,δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treated $\times$ firm $\times$ time fixed effects, $\delta_{tr(s),C(i),t}$ are treated $\times$ cohort $\times$ time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in tenure and lagged realizations of quarterly HPI and GDP PC growth. The variable Treated$_s$ is a dummy variable that takes a value one if state s implements an increase to its minimum wage, 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 x-axis indicates the number of months (τ) from a minimum wage increase in event time. The blue dots correspond to the estimates of the Γ_τ coefficients, where the month corresponding to $\tau = -9$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the state and time level. The dashed gray line denotes either the observation weighted average increase in the minimum wage ($0.957) or the observation weighted-average increase in wages due to the minimum wage during the sample period ($0.864).

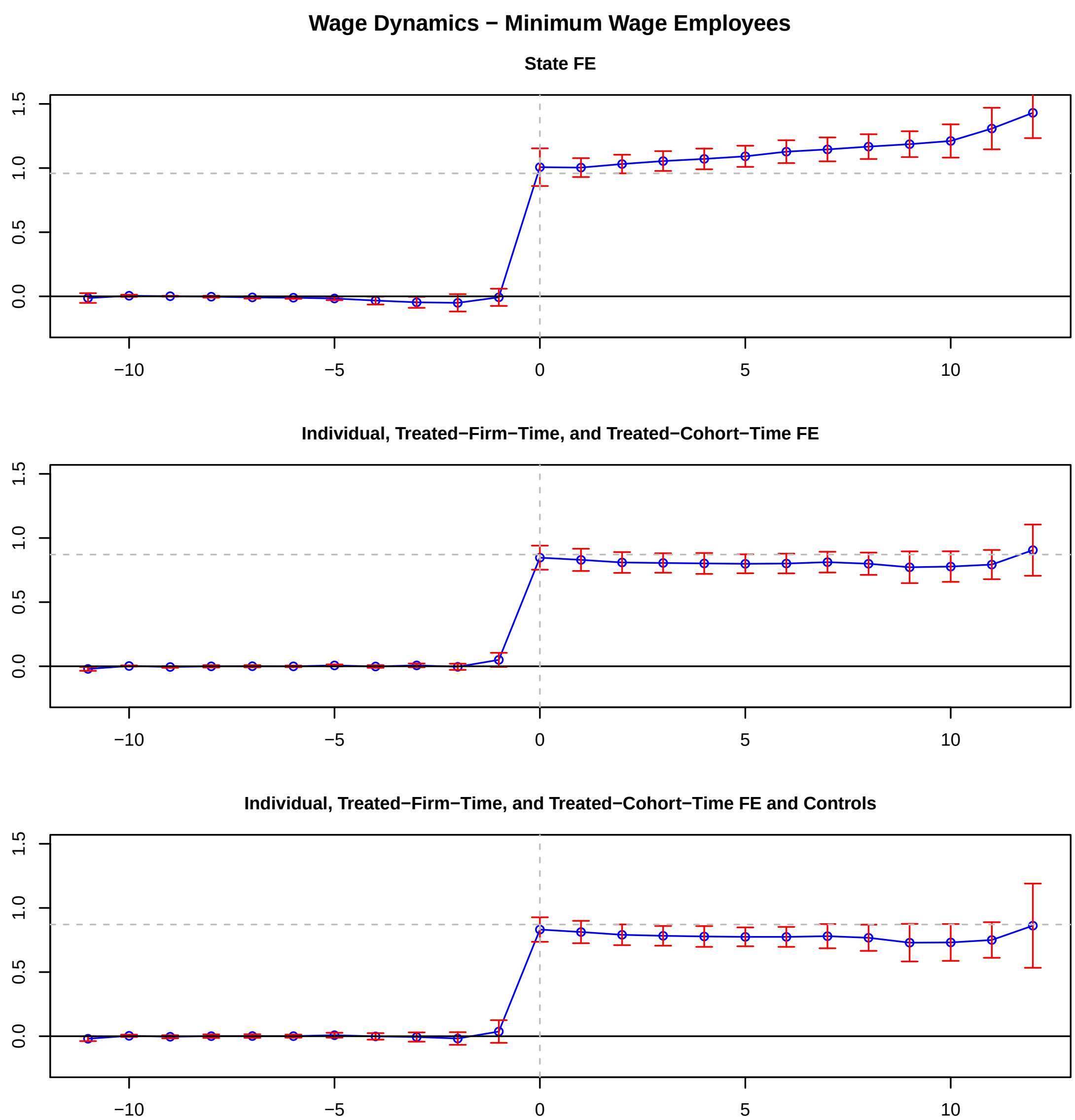


Figure 4: Evolution of Wages for Hourly-wage Employees

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

$$\omega_{i,s,t} = \alpha + [\delta_i] + \left[\delta_{tr(s),f(i),t}\right] + \left[\delta_{tr(s),C(i),t}\right] + \sum_{\tau=-12,\tau\neq-9}^{12} \Gamma_\tau \text{Treated}_s \times \text{D}(s,t,\tau) + [\eta' X_{i,t}] + \varepsilon_{i,s,t}$$

where $\omega_{i,s,t}$ is the wage rate of individual i in month t, δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treated $\times$ firm $\times$ time fixed effects, $\delta_{tr(s),C(i),t}$ are treated $\times$ cohort $\times$ time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in tenure and lagged realizations of quarterly HPI and GDP PC growth. The variable Treated$_s$ is a dummy variable that takes a value one if state s implements an increase to its minimum wage, 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 x-axis indicates the number of months (τ) from a minimum wage increase in event time. The blue dots correspond to the estimates of the Γ_τ coefficients, where the month corresponding to $\tau = -9$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the more robust of company and time or state and time level. The model is estimated separately for the the subsamples of Minimum Wage, Bound Employees, and Pseudo Low Wage Employees, and the coefficient estimates are plotted in the top, middle, and bottom panels, respectively. The dashed gray line denotes the observation weighted-average increase in wages due to the minimum wage during the sample period for each sub-group (excluding Pseudo-low wage employees).

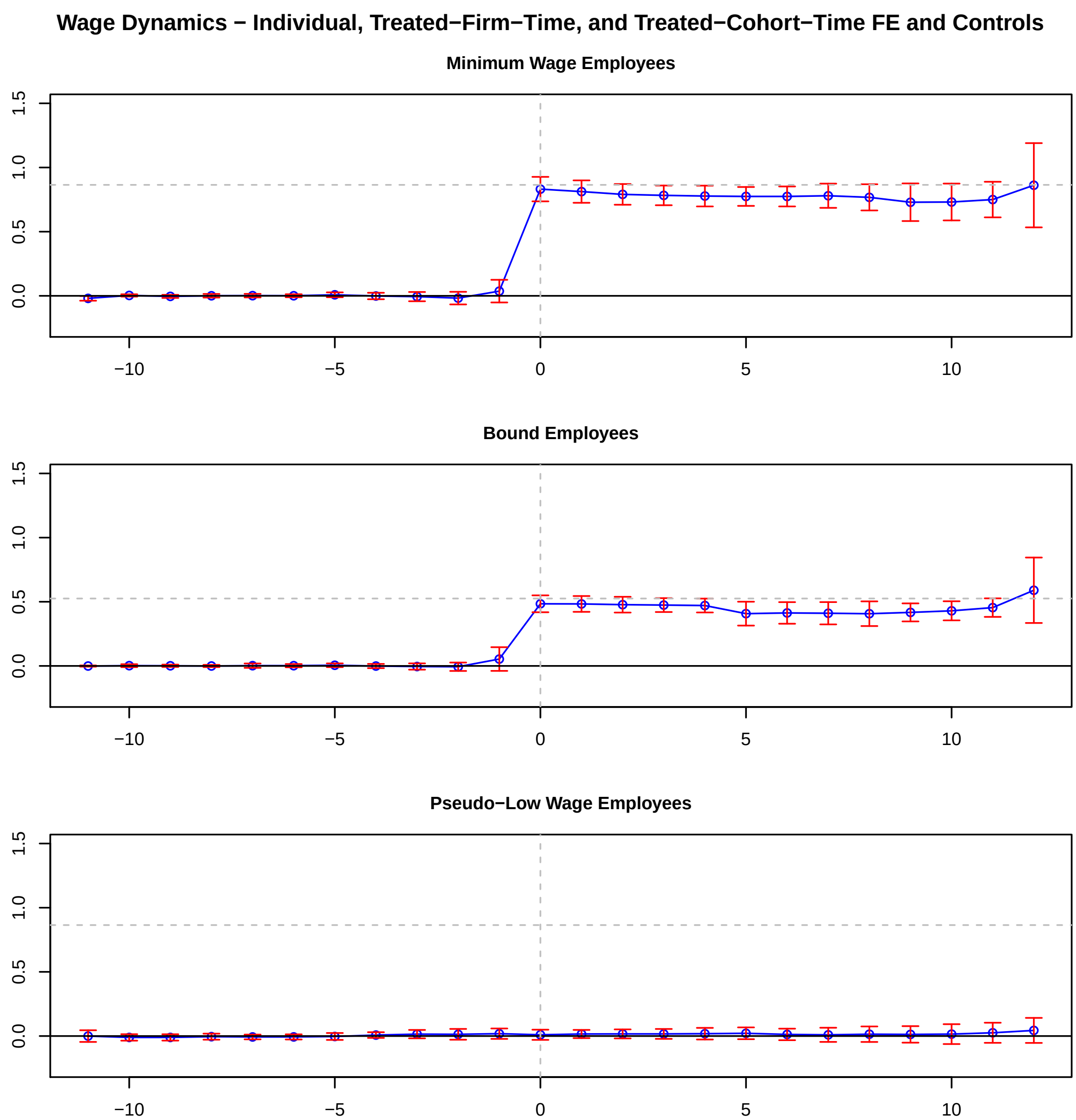


Figure 5: Evolution of Employment for Hourly-wage Employees

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

$$E_{i,s,t} = \alpha + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \sum_{\tau=-11}^{12} \Gamma_\tau \text{Treated}_s \times \text{D}(s,t,\tau) + \eta' X_{i,t} + \varepsilon_{i,s,t}$$

where $E_{i,s,t}$ is an indicator for employment, δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treatment × firm × time fixed effects, $\delta_{tr(s),C(i),t}$ are treatment × cohort × time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in tenure and lagged realizations of quarterly HPI and GDP PC growth. The variable Treated_s is a dummy variable that takes a value one if state s implements an increase to its minimum wage, 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 3.2 of the text. The model is estimated separately for the the subsamples of Minimum Wage, Bound, and Pseudo Low Wage Employees, and the coefficient estimates are plotted in the top, middle, and bottom panels, respectively. 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 $\tau = -9$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the state and time level.

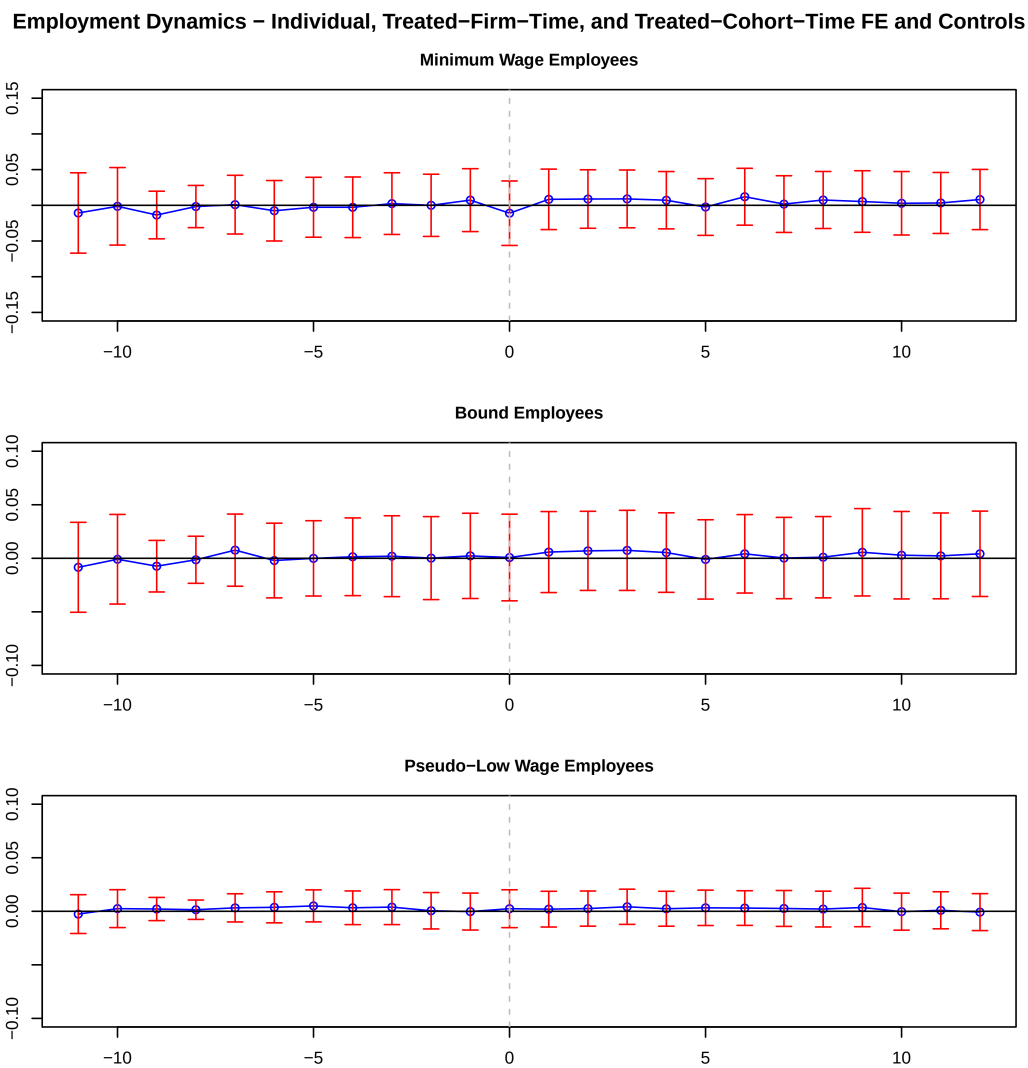


Figure 6: Evolution of Voluntary Turnover for Hourly-wage Employees

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

$$V_{i,s,t} = \alpha + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \sum_{\tau=-12,\tau\neq-9}^{12} \Gamma_\tau \text{Treated}_s \times \text{D}(s,t,\tau) + \eta' X_{i,t} + \varepsilon_{i,s,t}$$

where $V_{i,s,t}$ is an indicator for voluntary turnover, δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treatment $\times$ firm $\times$ time fixed effects, $\delta_{tr(s),C(i),t}$ are treatment $\times$ cohort $\times$ time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in tenure and lagged realizations of quarterly HPI and GDP PC growth. The variable Treated_s is a dummy variable that takes a value one if state s implements an increase to its minimum wage, 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 3.2 of the text. The model is estimated separately for the the subsamples of Minimum Wage, Bound, and Pseudo Low Wage Employees, and the coefficient estimates are plotted in the top, middle, and bottom panels, respectively. 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 $\tau = -9$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the state and time level.

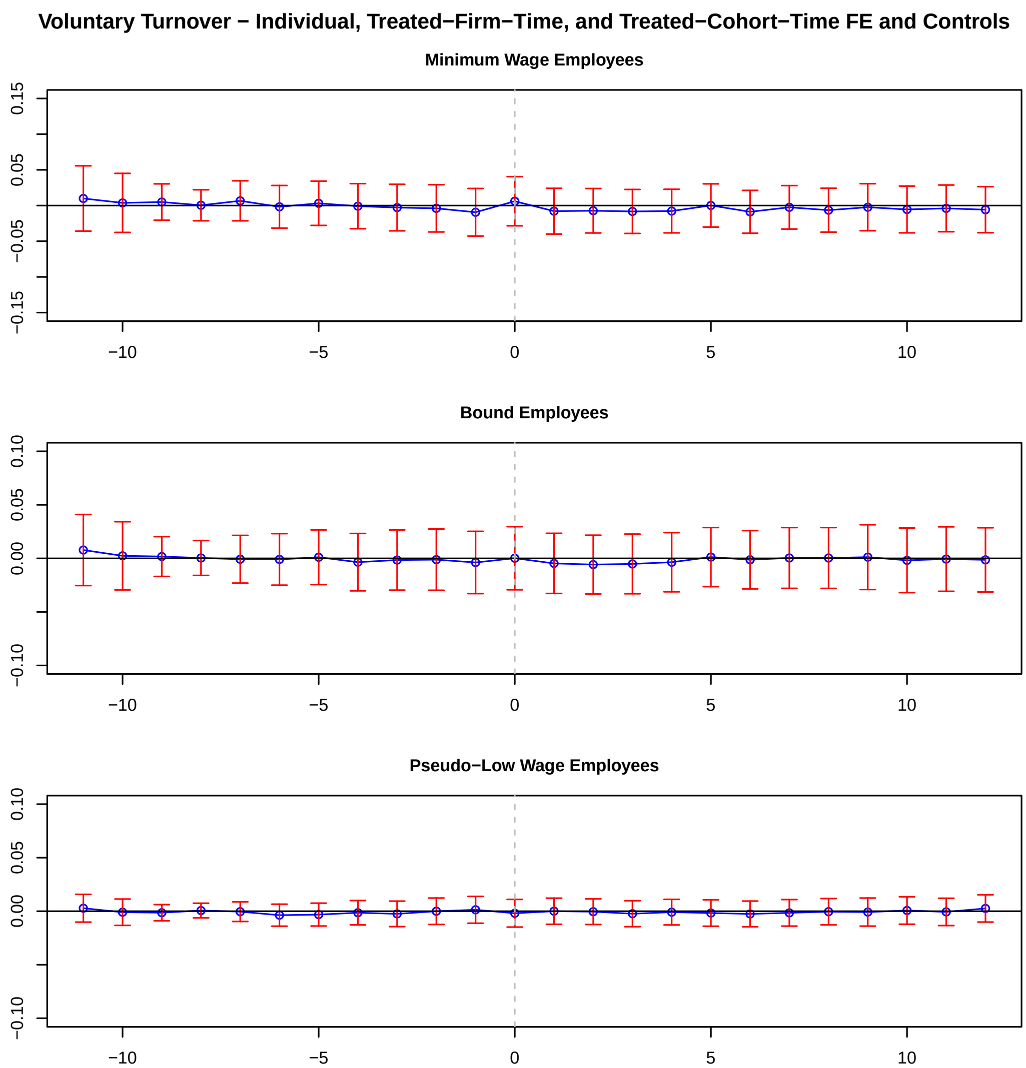


Figure 7: Evolution of Involuntary Turnover for Hourly-wage Employees

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

$$I_{i,s,t} = \alpha + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \sum_{\tau=-12,\tau\neq-9}^{12} \Gamma_\tau \text{Treated}_s \times \text{D}(s,t,\tau) + \eta' X_{i,t} + \varepsilon_{i,s,t}$$

where $I_{i,s,t}$ is an indicator for involuntary turnover, δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treatment $\times$ firm $\times$ time fixed effects, $\delta_{tr(s),C(i),t}$ are treatment $\times$ cohort $\times$ time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in tenure and lagged realizations of quarterly HPI and GDP PC growth. The variable Treated_s is a dummy variable that takes a value one if state s implements an increase to its minimum wage, 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 3.2 of the text. The model is estimated separately for the the subsamples of Minimum Wage, Bound, and Pseudo Low Wage Employees, and the coefficient estimates are plotted in the top, middle, and bottom panels, respectively. 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 $\tau = -9$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the state and time level.

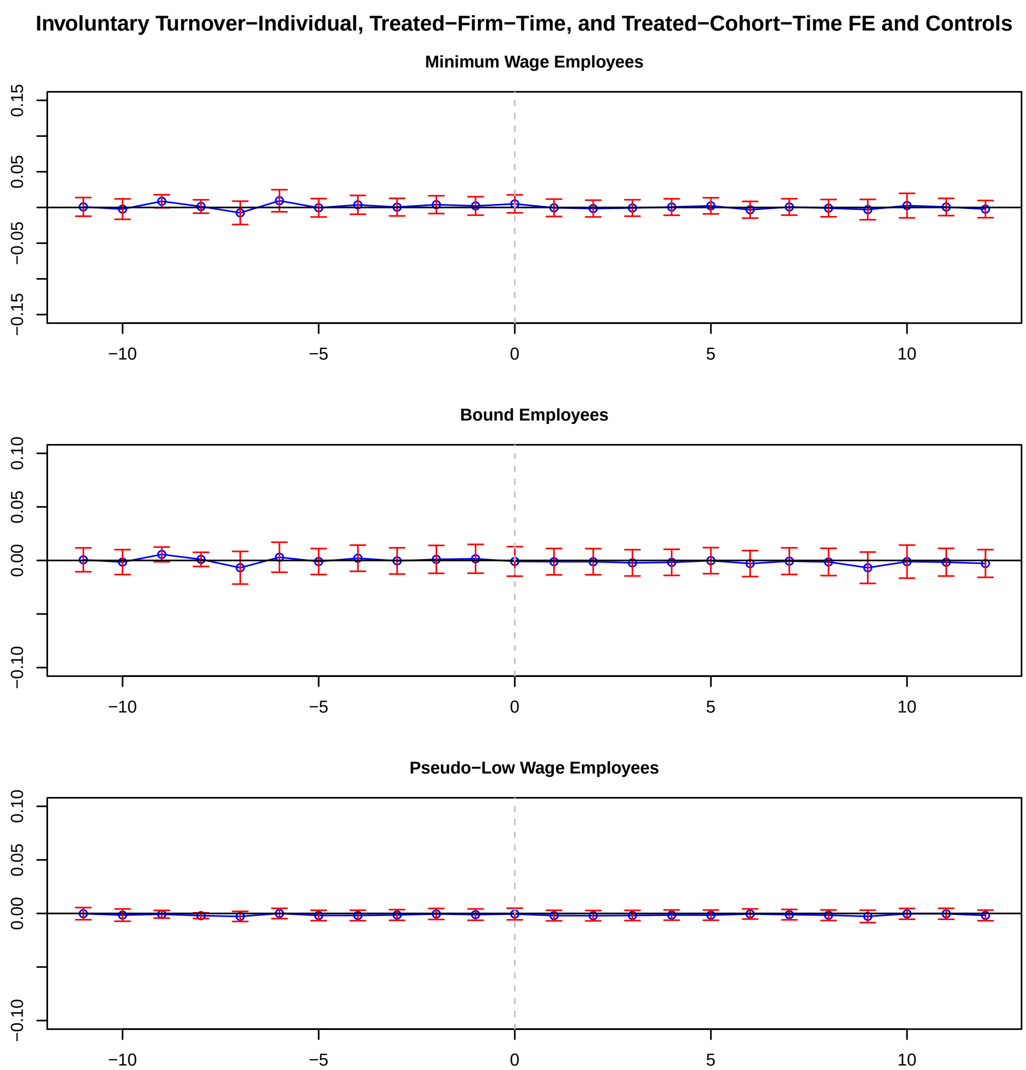
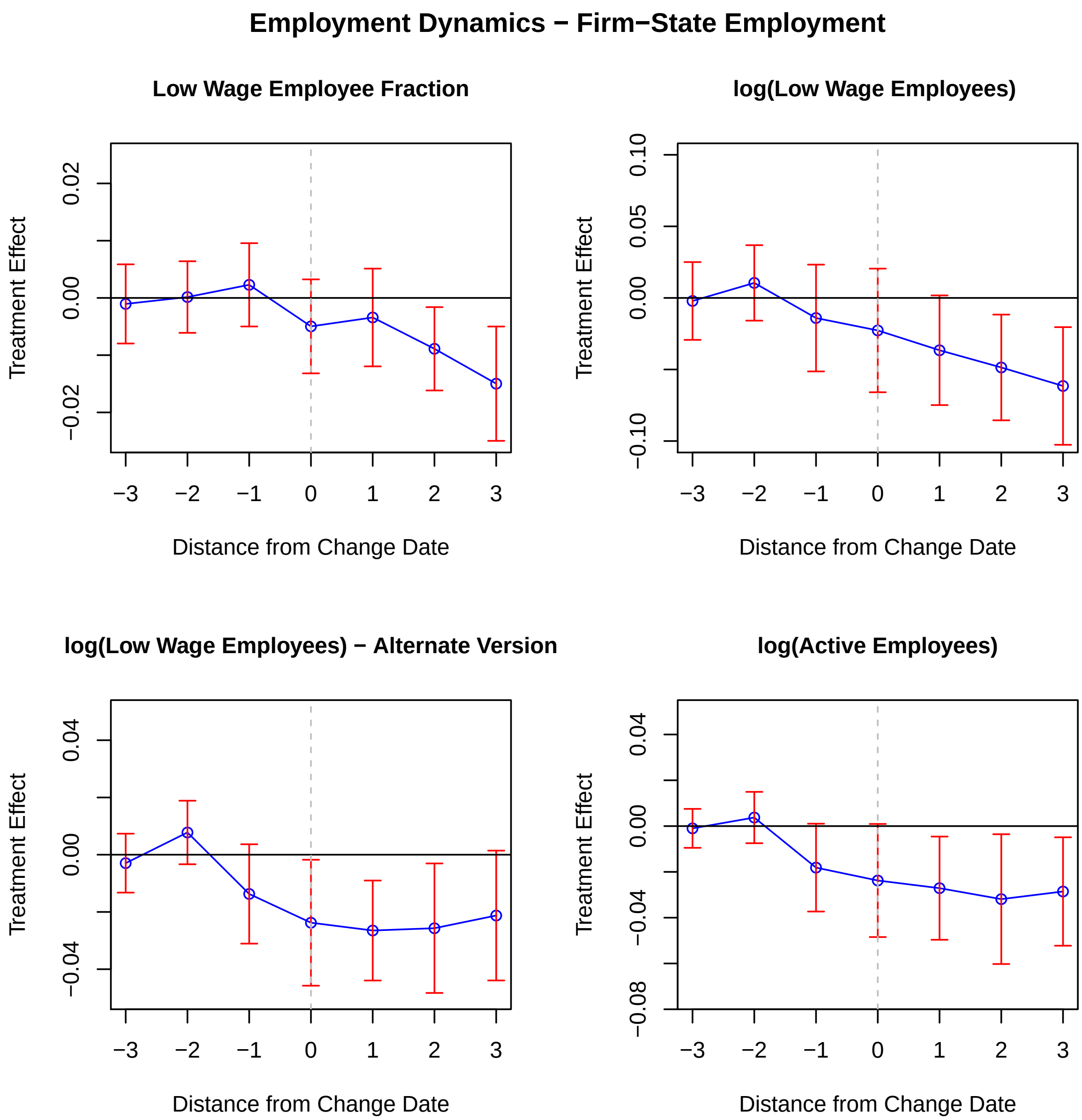


Figure 8: Evolution of Establishment Employment

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

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

where$\delta_{f,s}$ are are establishment fixed effects, $\delta_{tr(s),t}$ are treated$\times$ month fixed effects, $\delta_{f,t}$ are firm $\times$ month fixed effects, and $X_{s,t-1}$ is a vector of control variables including lagged realizations of state HPI and GDP PC growth. The outcome variable, $Y_{f,s,t}$ is a measure of low wage employment. The variable Treated$_s$ is a dummy variable that takes a value one if state s implements an increase to its minimum wage, 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 $\{\Gamma_\tau\}_{\tau=-3}^{3}$ coefficients, where the quarters corresponding to $\tau = -3$ is excluded as the reference level. The vertical red bars indicate confidence 90% confidence intervals, where standard errors are clustered at the state and time level.



Appendix A: Variable Definitions and Subgroup Definitions

Variables are sorted by whether they are reported in State-level, Individual-level, or Establishment-level tables and figures. Within each group, variables are sorted alphabetically.

State-Level Variables

1. Democratic Vote - the percent of individuals in the state that voted for the Democratic party in the 2012 presidential election.

2. Difference Employment-Population - the representativeness of the number of *Bound and Pseudo-low wage employees* in the state in our sample, relative to the representativeness of the state in terms of population: $\frac{LowWage_s}{\sum_{s \in SampleStates} LowWage_s} - \frac{Population_s}{\sum_{s \in SampleStates} Population_s}$. This variable has mean-zero by construction.

3. GDP Growth - the annual GDP growth rate.

4. GDP PC Growth - in state-level regressions, this variable is the annual GDP per-capita growth rate. In individual and *establishment* level regressions, this variable is the quarterly GDP per-capita growth rate.

5. HPI Growth - the annual Housing Price Index growth rate.

6. Median Tenure (t-12) - The median job tenure of the employees in the state at the beginning of the sample period.

7. Median Tenure (t+12) - The median job tenure of the employees in the state at the end of the sample period

8. Population Growth - the annual population growth rate

9. Turnover - the percentage of employees in the state that leave their initial place of employment by the end of the sample period.

10. Union Rate - the union affiliation rate for the state as of 2014 and as reported by the BLS.

Individual-Level Variables

1. Employment ($E_{i,t}$) - an indicator equal to one if employee i in month t remains employed

2. Involuntary Turnover ($V_{i,t}$) - an indicator equal to one if individual i has involuntarily left their employment by month t

3. Voluntary Turnover ($V_{i,t}$) - an indicator equal to one if individual i has voluntarily left their employment by month t

Establishment-Level Variables

1. Average Annual Wages (Hourly Employees) - the average annual wages for the establishment across all its hourly employees.

2. Average Annual Wages (Low Wage Employees) - the average annual wages for the establishment across all its Low Wage Employees.

3. Average Annual Wages (Total Employees) - the average annual wages for the establishment across all its employees.

4. Dollar Fraction Low Wage Employees **-** the annual wages of *Low wage employees* in *establishment* f, s at time t divided by the total annual wages for the *establishment* at time t

5. Employment Growth - the month-over-month percent growth in total employees.

6. Hourly Employees - the total number of hourly employees the establishment employs.

7. $\log(\text{Age} < 25\text{LowWageHires})_{f,s,t}$- the natural logarithm of the number of *Low wage employees* hired at establishment f, s in month t that were of age 24 or less.

8. $\log(\text{Age} \geq 25\text{LowWageHires})_{f,s,t}$- the natural logarithm of the number of *Low wage employees* hired at establishment f, s in month t that were of age 25 or more.

9. $\log(\text{AgeUnkownLowWageHires})_{f,s,t}$- the natural logarithm of the number of *Low wage employees* hired at establishment f, s in month t that have missing age fields in the data.

10. $\log(\text{AvgHours})_{f,s,t}$- the natural logarithm of the average number of hours the employees in establishment f, s work in each pay period in month t, reported directly from the data source.

11. $\log(\text{AvgLowWageHours})_{f,s,t}$- the natural logarithm of the average number of hours the *Low wage employees* in establishment f, s work in each pay period in month t, reported directly from the data source.

12. $\log(1{+}\text{Change in Number of Locations})_{f,s,t}$- the natural logarithm of the one plus the month-over-month change in the number of distinct three-digit ZIP codes for which establishment f, s has employees in state s in month $t - 1$ to t.

13. $\log(\text{ImpgHours})_{f,s,t}$- the natural logarithm of the imputed average number of hours the employees in establishment f, s work in each pay period in month t. Imputation is done by taking pay-period wages divided by pay-period hourly pay rate if the employee is hourly, and setting this equal to an equal to a 40 hour work week for salaried employees.

14. $\log(\text{ImpgLowWageHours})_{f,s,t}$- the natural logarithm of the imputed average number of hours the *Low wage employees* in establishment f, s work in each pay period in month t. Imputation is done by taking pay-period wages divided by pay-period hourly pay rate.

15. $\log(\text{LowWage})_{f,s,t}$- the natural logarithm of the number of *Low wage employees* in establishment f, s at time t.

16. $\log(\text{LowWageAlernate})_{f,s,t}$- the natural logarithm of the number of employees in establishment f, s at time t which make $\omega_{i,t} \leq \$15.00$ an hour.

17. $\log(\text{LowWageHires})_{f,s,t}$- the natural logarithm of the number of *Low wage employees* hired at establishment f, s at time t.

18. $\log(\text{Number of Locations})_{f,s,t}$- the natural logarithm of the number of distinct three-digit ZIP codes for which establishment f, s has employees in state s in month t

19. $\log(\text{PseudoLowWage})_{f,s,t}$- the natural logarithm of the number of *Pseudo-low wage employees* in establishment f, s at time t.

20. $\log(\text{Total})_{f,s,t}$- the natural logarithm of the total number employees in establishment f, s at time t.

21. $\log(\text{TotalHires})_{f,s,t}$- the natural logarithm of the total number employees hired at establishment f, s at time t.

22. $\log(\text{Turnover})_{f,s,t}$- the natural logarithm of the total number of employees which attritioned from establishment f, s at time t.

23. $\text{LowWage}_{f,s,t}/\text{Total}_{f,s,t-1}$(Fraction Low Wage Employees)- the number of *Low wage employees* in establishment f, s at time t divided by the total employees in the establishment at time $t-1$

24. $\text{LowWageHires}_{f,s,t}/\text{Total}_{f,s,t-1}$(Fraction Low Wage Hires)- the number of *Low wage employees* hired in establishment f, s in month t divided by the total employees in the establishment at time $t-1$.

25. Low Wage Employees- the total number of *Low wage employees* employees the establishment employs.

26. Low Wage Hires - the total number of *Low wage employees* employees the establishment hired in the month.

27. Pseudo Low Wage Employees- the total number of *Pseudo-low wage employees* employees the establishment employs.

28. Total Employees- the total number of employees the establishment employs.

29. Total Hires - the total number of new hires by the establishment in the month.

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 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 state. The definition of treated and control states is given in Section 3.2. 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: $\text{NEW MW}_s = \text{BOP MW}_s + \Delta\text{MW}_{tr(s)} \ \ \forall s \in \text{ControlStates}$. $\omega_{i,t}$ 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**
Minimum wage employees	Individual Level	Employees making exactly the minimum wage in the pre-treatment period.	$\omega_i = \text{BOP MW}_s$
Bound employees	Individual Level	Employees making either exactly the minimum wage, or above the minimum wage but below the new minimum wage in the pre-treatment period. These employees' wages are directly affected by minimum wage increases.	$\omega_i < \text{NEW MW}_s$
Pseudo-low wage employees	Individual Level	Employees making moderately above the new minimum wage in the pre-treatment period.	$\omega_i \in (\text{NEW MW}_s + \$1, \text{NEW MW}_s + \$3.50]$
Low Wage Employees	Establishment Level	Employees making less than or equal to \$10 at any point in time (dynamic measure).	$\omega_{i,t} \leq \$10$
Pseudo -low Wage Employees	Establishment Level	Employees making between \$10 and \$15 at any point in time (dynamic measure).	$\omega_{i,t} \in (\$10, \$15]$

Appendix B: Data Appendix

In this Appendix we compare the employment data we use throughout the analysis to data on the U.S. population as of March 2015. Our employment data comes from Equifax Inc.'s TheWorkNumber database. 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.[34] 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.[35] Figure B.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 B.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 repre-

[34]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.

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

sented in a similar proportion to their population weights.[36]. As shown in Figure B.3, our data is geographically representative of the distribution of employment across U.S. states.

Figures B.4 and B.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 B.6).

[36]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 granular identifiers for a business establishment are at the firm-state and the firm-3 digit ZIP level. In contrast, the QCEW identifies establishments 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 B.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".

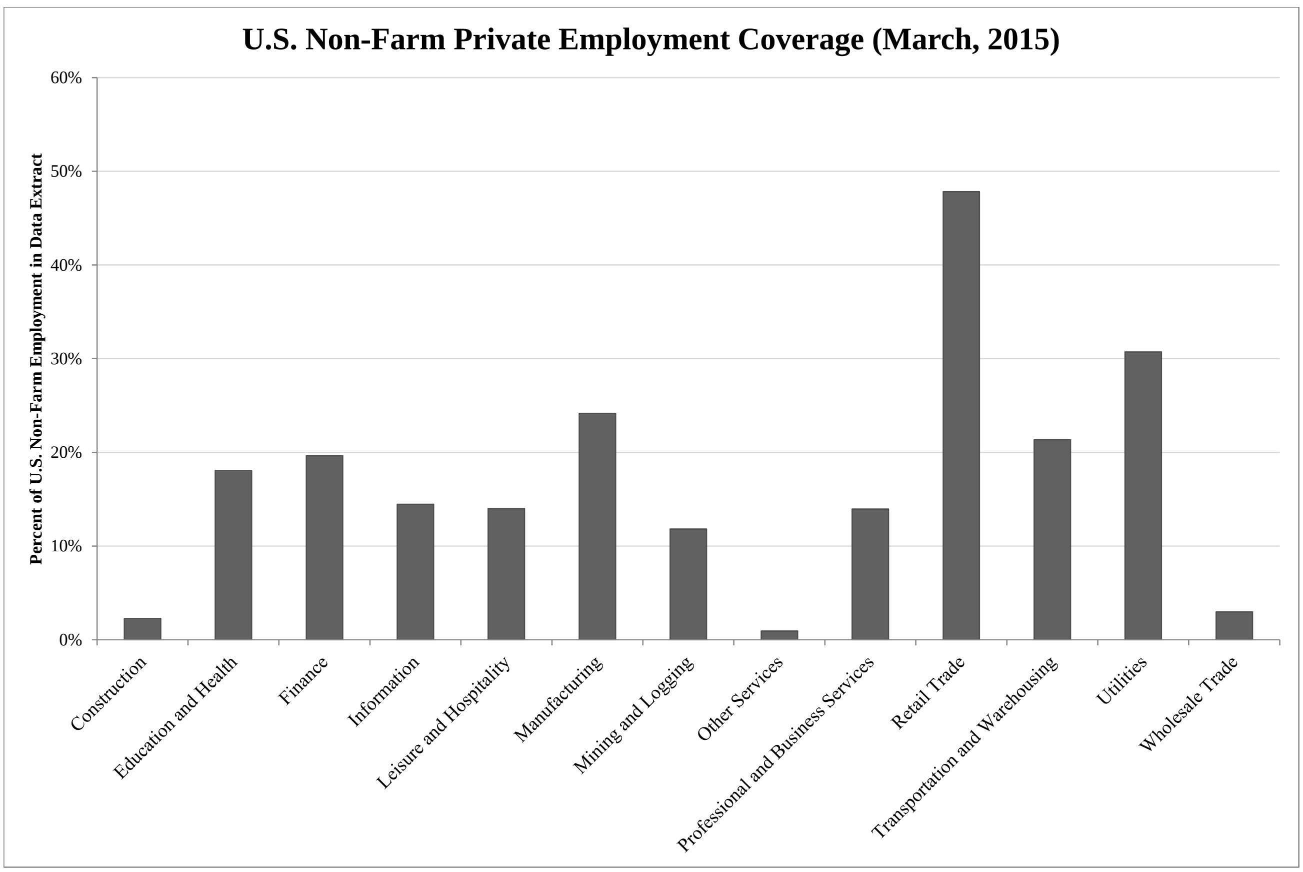


Figure B.2: Distribution of Employment Data

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".

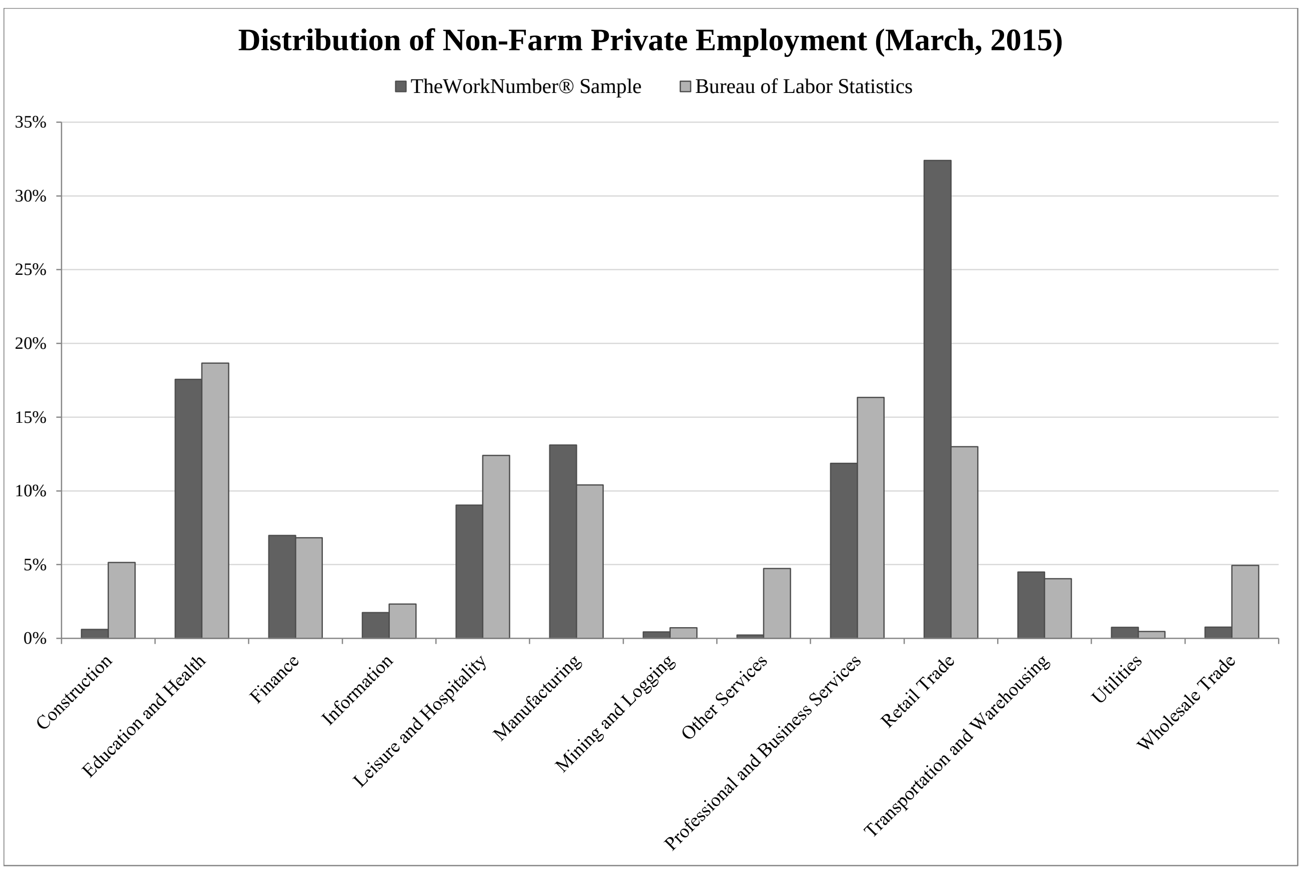


Figure B.3: State Distribution of Employment Data

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.

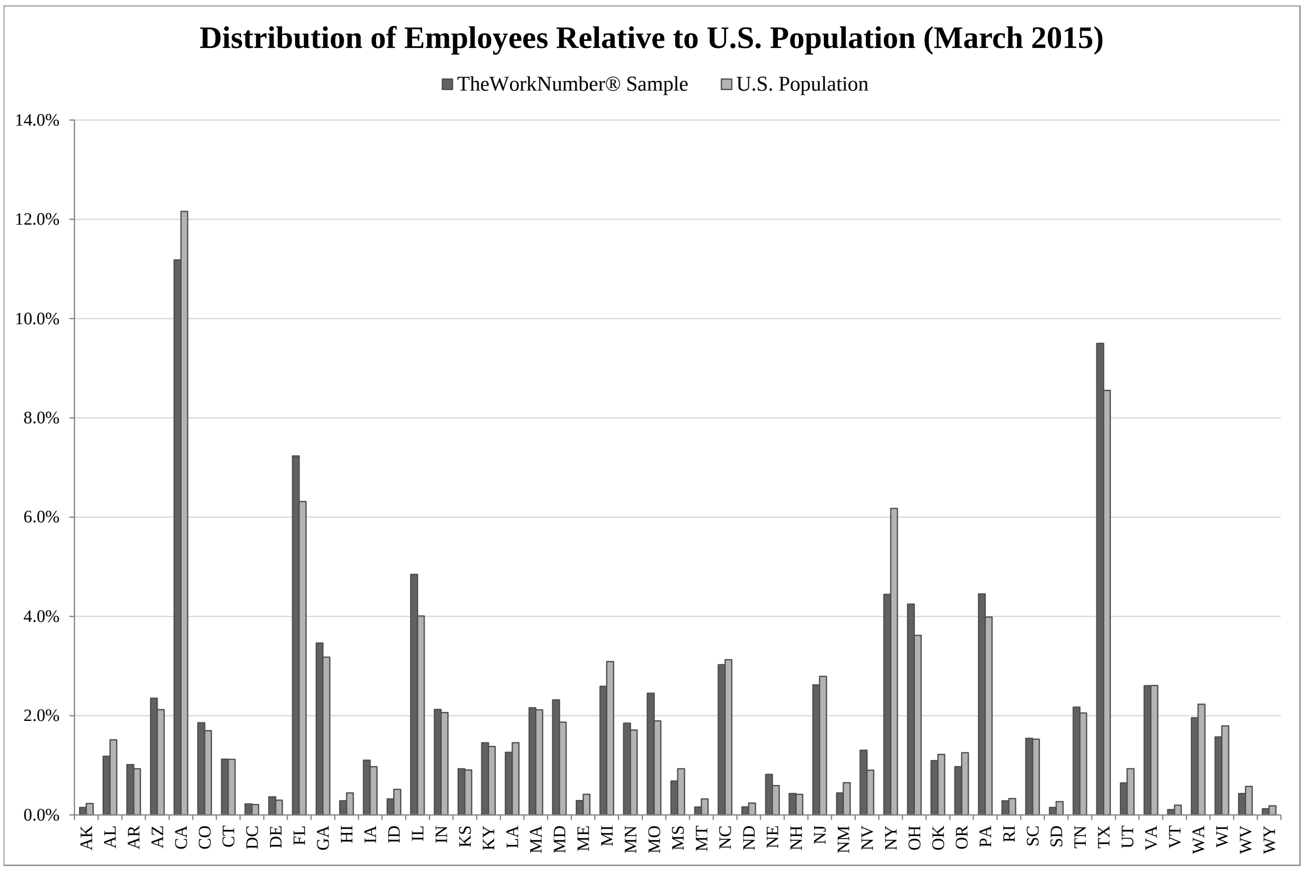


Figure B.4: Median Incomes of Employment Data

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.

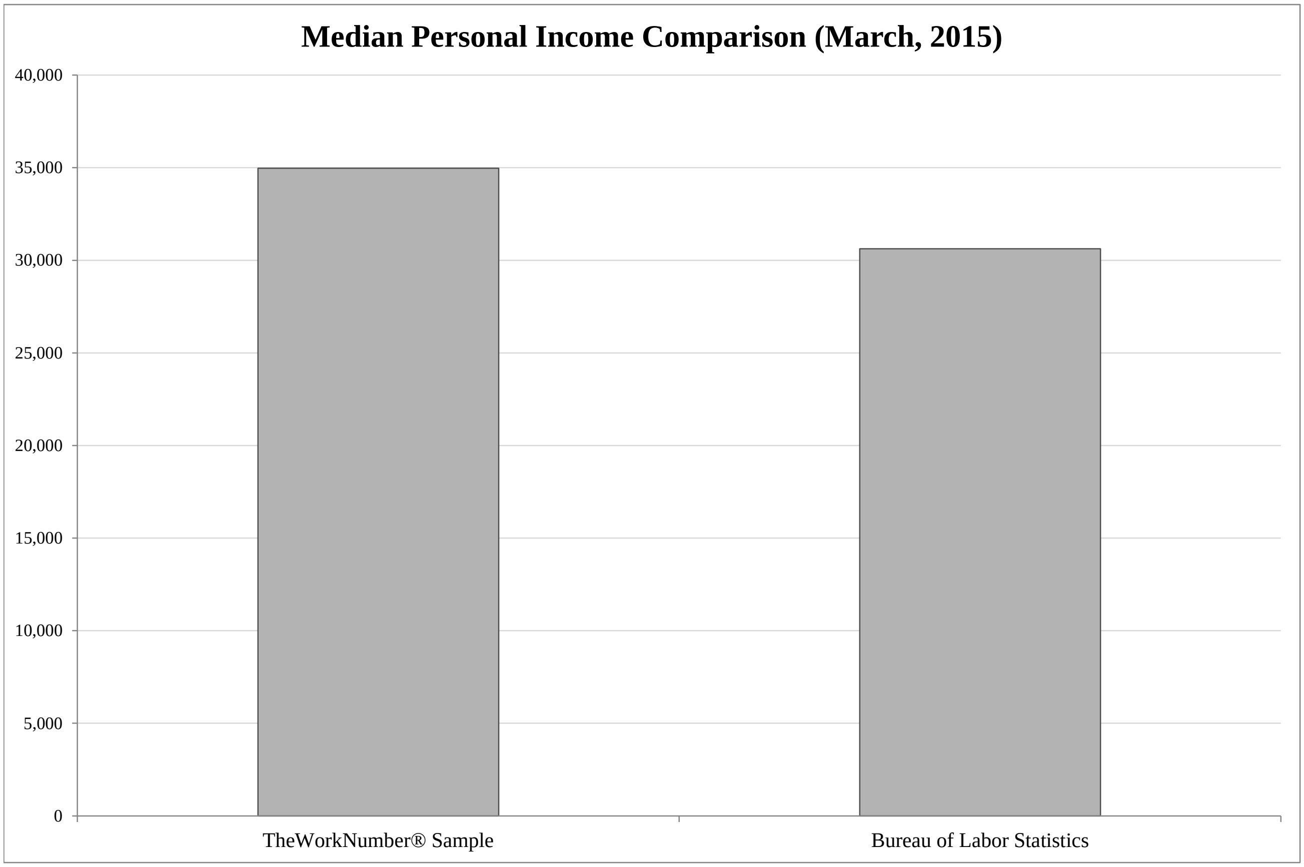


Figure B.5: Median Tenure of Employment Data

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).

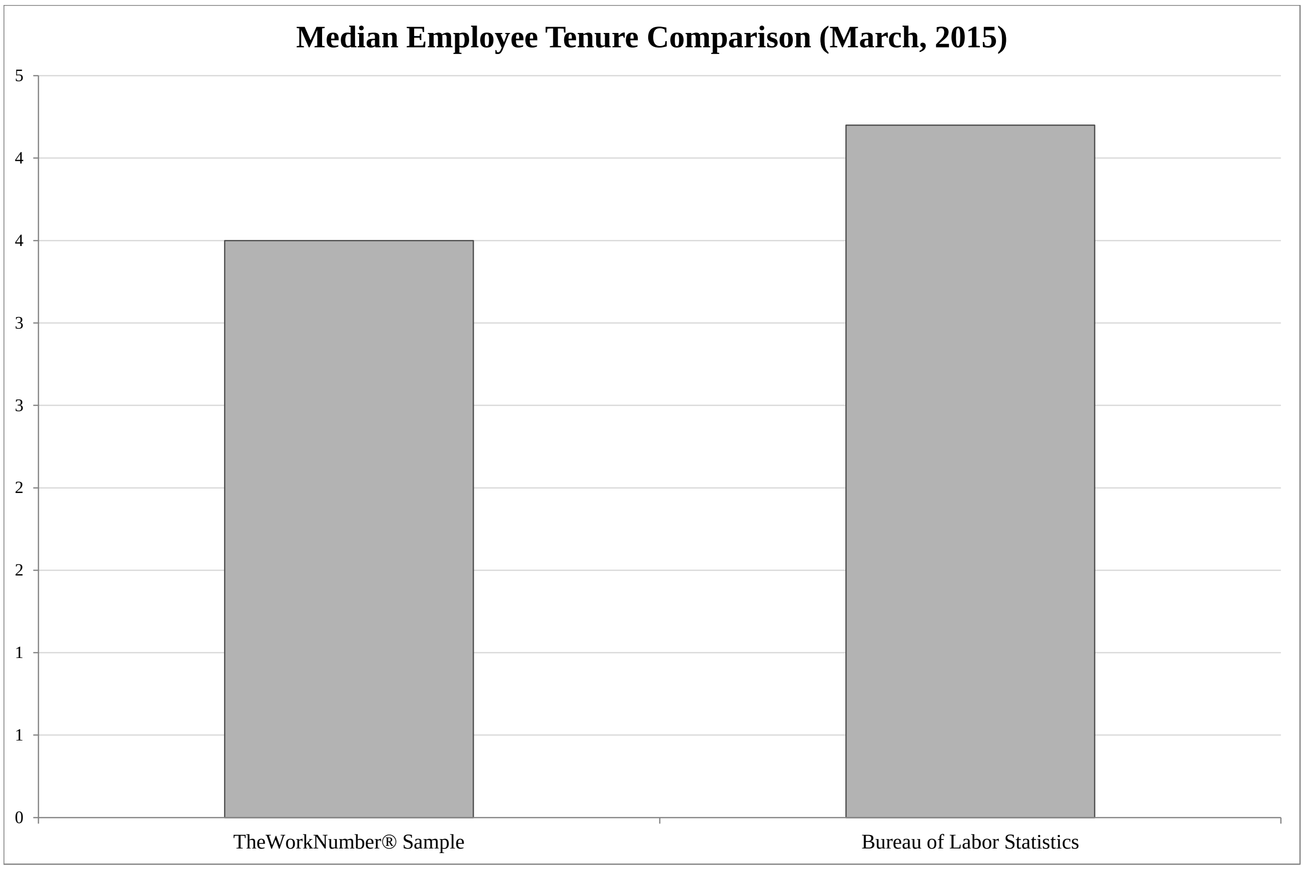
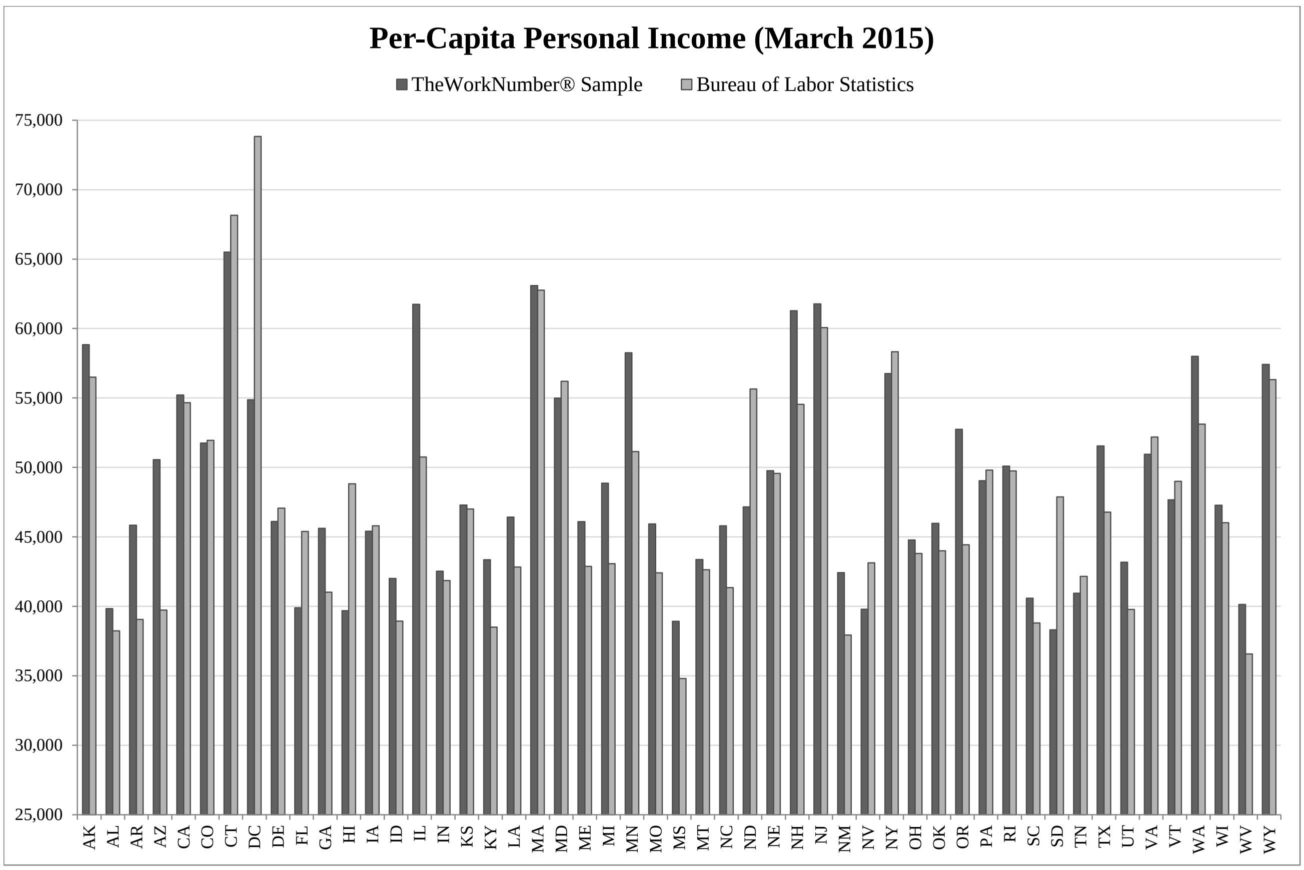


Figure B.6: State Per Capita Personal Income of Employment Data

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.



Internet Appendix - Not Intended for Publication

Table IA.1: List of Candidate Treatment States and Control States

This table lists year-over-year minimum wage changes for the set of states with at least one minimum wage increase of 75 cents over the sample period (candidate treated states) and the control states. The table is split between treated states (Panel A), non-treated states with at least one 75 cent increase (Panel B), and control states (Panel C). To be considered a treated state, a candidate treated state must have (1) only implemented one increase to the minimum wage of at least $0.75 between 2010 and 2015, and (2) implemented its minimum wage change of at least $0.75 in an isolated manner. A minimum wage change is considered isolated if there are no other minimum wage changes in the twelve months following or the 24 months preceding the minimum wage change. All treated states implemented their $0.75 or more increases between 2014 and 2015. Alaska is eliminated from the analysis because of the use of geographic variation in the identification strategy.

State	EOY 2009 Minimum Wage	**Changes to Minimum Wage by Year** 2010	2011	2012	2013	2014	2015	EOY 2015 Minimum Wage
Panel A: Treated States								
CA	8.00	-	-	-	-	1.00	-	9.00
MA	8.00	-	-	-	-	-	1.00	9.00
MI	7.40	-	-	-	-	0.75	-	8.15
NE	7.25	-	-	-	-	-	0.75	8.00
SD	7.25	-	-	-	-	-	1.25	8.50
WV	7.25	-	-	-	-	-	0.75	8.00
Panel B: States with 75+ cent Increase that are Eliminated by Conditions								
AK	7.25	0.50	-	-	-	-	1.00	8.75
DC	8.25	-	-	-	-	1.25	1.00	10.50
MD	7.25	-	-	-	-	-	1.00*	8.25
MN	7.25	-	-	-	-	0.75	1.00	9.00
NJ	7.25	-	-	-	-	1.00	0.13	8.38
NY	7.25	-	-	-	-	0.75	0.75	8.75
RI	7.40	-	-	-	0.35	0.25	1.00	9.00
Panel C: Control States								
IA	7.25	-	-	-	-	-	-	7.25
IL	8.00	0.25	-	-	-	-	-	8.25
IN	7.25	-	-	-	-	-	-	7.25
KS	7.25	-	-	-	-	-	-	7.25
KY	7.25	-	-	-	-	-	-	7.25
ND	7.25	-	-	-	-	-	-	7.25
NH	7.25	-	-	-	-	-	-	7.25
NV	7.55	-	0.70	-	-	-	-	8.25
PA	7.25	-	-	-	-	-	-	7.25
UT	7.25	-	-	-	-	-	-	7.25
VA	7.25	-	-	-	-	-	-	7.25
WI	7.25	-	-	-	-	-	-	7.25

*Maryland 2015: Represents two different minimum wage increases within the same year (75 cents and 25 cents).

Table IA.2: Within Treatment Time Series Comparisons

This table contains differences in means between treated and control states at the treatment-state level over various yearly estimation windows: 2012-2014, 2005-2015, and 1995-2015. For each estimation window, the interior cells contain the difference in time series means between treated states and control states for each treatment-state pairing and each variable of interest (measured yearly). An indication of statistical significance is reported below each mean difference, where , **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively and standard errors are computed from regressions with the assumption of homoskedasticity. The definition of treated and control states is defined in Section 3.

Treated State	**Control States**	**Estimation: 2012-2014**			**Estimation: 2005-2015**			**Estimation: 1995-2015**		
		ΔGDP PC%	**ΔUR%**	**ΔHPI%**	**ΔGDP PC%**	**ΔUR%**	**ΔHPI%**	**ΔGDP PC%**	**ΔUR%**	**ΔHPI%**
CA	(NV,UT)	2.20 (*)	-0.07	0.03	1.30	-0.06	-0.32	1.63 (*)	-0.13	0.34
MA	(NH,PA,VA)	-0.18	0.00	0.44	0.37	0.01	-0.40	0.45	-0.01	0.16
MI	(IL,IN, WI)	0.37	-0.13	1.45 (**)	-0.35	-0.10	-0.27	-0.47	-0.03	-0.02
NE	(IA,KS)	-0.02	0.15	0.13	0.19	0.02	0.01	0.14	0.02	-0.02
SD	(ND)	-8.69	-0.13	-0.48	-3.80 (*)	0.01	-0.04	-1.89	0.04	0.00
WV	(KY)	-0.25	0.53	-2.56 (**)	0.66	0.16	0.41	0.54	-0.09	-0.08

Table IA.3: Difference in Industry Composition Across Treated and Control States

This table displays the difference in industry composition between the treated states and the matched control states in our sample. Industry composition is defined as the fraction of total establishments in the treated or control state that belongs to each BLS industry. If treated and control states have the exact same industry composition, then the difference in each cell should be zero. The definition of treated and control states is provided in Section 3.2 of the text.

Difference in Industry Composition (%Treated - %Control)						
BLS	**Treated States**					
Industries	**CA**	**MA**	**MI**	**NE**	**SD**	**WV**
Admin and Support	-0.3	0.2	0.3	0.4	4.4	1.4
Agriculture	-0.2	-0.1	-0.4	0.0	0.0	-0.2
Arts and Rec.	-0.5	1.0	-0.4	-0.1	-0.1	0.6
Construction	0.3	0.4	0.0	0.0	0.0	0.0
Educational Services	-3.7	0.9	0.3	0.3	1.6	1.4
Finance	-0.3	2.7	-1.8	1.6	-1.6	2.3
Health Care	1.0	-1.0	1.0	1.2	0.5	0.6
Hotels and Food	-1.7	-1.7	1.0	0.5	-6.3	-0.9
Information	1.4	0.3	-1.1	-1.5	0.0	-0.2
Management	0.0	0.3	0.0	0.0	0.0	0.0
Manufacturing	0.5	0.2	2.6	-1.5	4.6	-0.9
Mining and Oil	0.0	-0.1	0.0	0.0	0.0	-0.1
Other Services	0.1	0.7	0.6	-0.1	2.0	0.1
Professional Services	0.3	0.5	0.1	-1.0	1.0	-0.8
Public Administration	0.0	0.0	0.0	0.0	0.0	0.0
Real Estate	-0.2	0.0	0.1	1.1	0.0	-1.4
Retail Trade	4.2	-4.2	-2.3	-1.5	-0.1	-1.2
Transportation and Utilities	-0.6	-0.3	-0.4	0.7	-0.8	-0.7
Wholesale Trade	-0.3	0.1	0.4	-0.1	0.4	0.0
Average abs (%Treated − %Control)	0.83	0.77	0.68	0.61	1.23	0.68

Table IA.4: Bound Employees - Heterogeneity Across States

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

$$Y_{i,s,t} = \alpha + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s)C(i),t} + \sum_{s' \in TreatedStates} \Gamma_{s'} \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} \times 1\{s = s'\} + \eta' X_{i,t} + \varepsilon_{i,s,t}$$

where δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treated× firm × time fixed effects, $\delta_{tr(s),C(i),t}$ are treated × cohort × time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in employee tenure and lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{i,s,t}$, is either (1): an indicator for employment ($E_{i,s,t}$), (2) an indicator for voluntary turnover ($V_{i,s,t}$), or (3) an indicator for involuntary turnover ($I_{i,s,t}$), as defined in Appendix A. The variable Treated_s is an indicator equal to one if state s is treated, and $\text{Post}_{\tau(s),t}$ is an indicator equal to one if for all months t after the month of treatment $\tau(s)$, and zero otherwise. The variable $1\{s = s'\}$ is an indicator that equals one if state s is equal to state s' and zero otherwise. A description of treated and control states is provided in Section 3.2. The variable The model is estimated on the subsample of *Bound employees.* Standard errors are calculated by clustering two-dimensionally at the state and month 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)**	$I_{i,t}$ **(2)**	$V_{i,t}$ **(3)**
$\text{Treated}_s \times \text{Post}_{t,\tau(s)} \times 1\{s = \text{CA}\}$	0.007	-0.005	-0.002
	(0.50)	(-0.49)	(-0.42)
$\text{Treated}_s \times \text{Post}_{t,\tau(s)} \times 1\{s = \text{MA}\}$	0.005	-0.002	-0.003
	(0.31)	(-0.16)	(-0.59)
$\text{Treated}_s \times \text{Post}_{t,\tau(s)} \times 1\{s = \text{MI}\}$	-0.000	0.001	-0.001
	(-0.16)	(0.09)	(-0.25)
$\text{Treated}_s \times \text{Post}_{t,\tau(s)} \times 1\{s = \text{NE}\}$	-0.001	0.001	-0.001
	(-0.32)	(0.08)	(-0.10)
$\text{Treated}_s \times \text{Post}_{t,\tau(s)} \times 1\{s = \text{SD}\}$	0.007	-0.004	-0.006
	(0.25)	(-0.06)	(-0.77)
$\text{Treated}_s \times \text{Post}_{t,\tau(s)} \times 1\{s = \text{WV}\}$	0.009	-0.004	-0.005
	(0.43)	(-0.25)	(-0.82)
Individual FE	Yes	Yes	Yes
Treated ×Firm × Time FE	Yes	Yes	Yes
Treated ×Cohort× Time FE	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes
N	7,602,483	7,602,483	7,602,483

Table IA.5: Bound Employees - Heterogeneity by Ex-ante Tenure

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

$$Y_{i,s,t} = \alpha + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} + \beta \text{Treated}_s \times \text{Post}_{t,\tau(s)} \times \text{Z}_i + \delta_{\text{Z}_i,t} + \eta' X_{i,t} + \varepsilon_{i,s,t}$$

where δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treated$\times$ firm $\times$ time fixed effects, $\delta_{tr(s),C(i),t}$ are treated $\times$ cohort $\times$ time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in employee tenure and lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{i,s,t}$, is either (1): an indicator for employment ($E_{i,s,t}$), (2) an indicator for voluntary turnover ($V_{i,s,t}$), or (3) an indicator for involuntary turnover ($I_{i,s,t}$), as defined in Appendix A. The variable Treated$_s$ is an indicator equal to one if state s is treated, and Post$_{\tau(s),t}$ is an indicator equal to one if for all months t after the month of treatment $\tau(s)$, and zero otherwise. A description of treated and control states is provided in Section. The variable Z$_i$ is a dummy variable capturing the ex-ante individual characteristic LowTenure, as defined as below median tenure. Standard errors are calculated by clustering two-dimensionally at the state and month 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)**
Treated$_s$ $\times$ Post$_{t,\tau(s)}$	0.003	0.002	-0.001	-0.000	-0.002	-0.002
	(0.34)	(0.19)	(-0.10)	(-0.00)	(-1.13)	(-0.82)
Treated$_s$ $\times$ Post$_{t,\tau(s)}$	0.004	0.004	-0.003	-0.003	-0.001	-0.001
$\times$LowTenure$_i$	(0.57)	(0.62)	(-0.57)	(-0.62)	(-0.40)	(-0.44)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$Firm $\times$ Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$Cohort$\times$ Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	No	Yes	No	Yes
N	7,609,656	7,599,918	7,609,656	7,599,918	7,609,656	7,599,918

Table IA.6: Bound Employees - Heterogeneity by Ex-Ante Wages

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

$$Y_{i,s,t} = \alpha + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} + \beta \text{Treated}_s \times \text{Post}_{t,\tau(s)} \times \text{Z}_i + \delta_{\text{Z}_i,t} + \eta' X_{i,t} + \varepsilon_{i,s,t}$$

where δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treated× firm × time fixed effects, $\delta_{tr(s),C(i),t}$ are treated × cohort × time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in employee tenure and lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{i,s,t}$, is either (1): an indicator for employment ($E_{i,s,t}$), (2) an indicator for voluntary turnover ($V_{i,s,t}$), or (3) an indicator for involuntary turnover ($I_{i,s,t}$), as defined in Appendix A. The variable Treated$_s$ is an indicator equal to one if state s is treated, and Post$_{\tau(s),t}$ is an indicator equal to one if for all months t after the month of treatment $\tau(s)$, and zero otherwise. A description of treated and control states is provided in Section. The variable Z$_i$ is a dummy variable capturing the ex-ante individual characteristic LowWage, as defined as below median wage. Standard errors are calculated by clustering two-dimensionally at the state and month 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)**
Treated$_s$ × Post$_{t,\tau(s)}$	0.001	-0.000	0.001	0.002	-0.002	-0.002
	(0.08)	(-0.02)	(0.20)	(0.26)	(-0.75)	(-0.63)
Treated$_s$ × Post$_{t,\tau(s)}$ ×LowWage$_i$	0.007	0.007	-0.006	-0.006	0.000	-0.000
	(0.75)	(0.75)	(-0.91)	(0.26)	(-0.75)	(-0.63)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated ×Firm × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated ×Cohort× Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	No	Yes	No	Yes
N	7,615,770	7,602,413	7,615,770	7,602,413	7,615,770	7,602,413

Table IA.7: Bound Employees - Heterogeneity of Treatment - Firm Size

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

$$Y_{i,s,t} = \alpha + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} \\ + \beta \text{Treated}_s \times \text{Post}_{t,\tau(s)} \times \text{Z}_i + \delta_{\text{Z}_i,t} + \eta' X_{i,t} + \varepsilon_{i,s,t}$$

where δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treated× firm × time fixed effects, $\delta_{tr(s),C(i),t}$ are treated × cohort × time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in employee tenure and lagged realizations of quarterly HPI and GDP PC growth. The outcome variable, $Y_{i,s,t}$, is either (1): an indicator for employment ($E_{i,s,t}$), (2) an indicator for voluntary turnover ($V_{i,s,t}$), or (3) an indicator for involuntary turnover ($I_{i,s,t}$), as defined in Appendix A. The variable Treated$_s$ is an indicator equal to one if state s is treated, and Post$_{\tau(s),t}$ is an indicator equal to one if for all months t after the month of treatment $\tau(s)$, and zero otherwise. A description of treated and control states is provided in Section. The variable Z$_i$ is a dummy variable capturing the ex-ante firm characteristic LowFraction, as defined as below median fraction of *Low wage employees* to total employees. Standard errors are calculated by clustering two-dimensionally at the state and month 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)**
Treated$_s$ × Post$_{t,\tau(s)}$	0.004	0.003	-0.002	-0.001	-0.002	-0.002
	(0.50)	(0.37)	(-0.27)	(-0.18)	(-0.98)	(-0.85)
Treated$_s$ × Post$_{t,\tau(s)}$	0.000	0.002	-0.003	-0.004	0.003	0.002
×LowFraction$_{f(i)}$	(0.05)	(0.25)	(-0.46)	(-0.65)	(0.79)	(0.70)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated ×Firm × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated ×Cohort× Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	No	Yes	No	Yes
N	7,615,770	7,602,413	7,615,770	7,602,413	7,615,770	7,602,413

Table IA.8: Firm-State DD Robust Regressions - Employment

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

$$Y_{f,s,t} = \alpha + \delta_{f,s} + \delta_{tr(s),t} + \delta_{f,t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} + \left[\eta' X_{s,t-1}\right] + \left\{\delta_{f,tr(s),t}\right\} + \varepsilon_{f,s,t}$$

where $\delta_{f,s}$ are firm-state (*establishment*) fixed effects, $\delta_{tr(s),t}$ are treated $\times$ time fixed effects, $\delta_{f,t}$ are firm$\times$ time fixed effects, $X_{s,t-1}$ is a vector of control variables including lagged realizations of quarterly HPI and GDP PC growth, and $\delta_{f,tr(s),t}$ are firm $\times$ treated $\times$ time fixed effects. The outcome variable, $Y_{f,s,t}$, is either a measure of total firm employment or a measure of total firm hiring. The variables are defined in full in Appendix Appendix A. The variable Treated$_s$ is an indicator equal to one if state s is treated and Post$_{t,\tau(s)}$ is an indicator equal to one for all months after the month of treatment. A description of treated and control states is provided in Section 3.2. Standard errors are calculated by clustering two-dimensionally at the state and month level. t-statistics are reported below the coefficient estimates, with *, **, and *** indicating statistical significance at the 10%, 5%, and 1% level, respectively.

Explanatory Variables	log(LowWage Alternate) **(1)**	**(2)**	**(3)**	log(Lowwage Alternate Hires) **(4)**	**(5)**	**(6)**	LowWage Alternate Hires/Total **(7)**	**(8)**	**(9)**
$D(s,t)$	-0.023**	-0.023**	-0.030*	-0.029***	-0.030***	-0.039***	-0.002***	-0.002**	-0.002***
	(-2.09)	(-2.09)	(-1.76)	(-4.14)	(-3.75)	(-3.9)	(-2.00)	(-2.00)	(-2.00)
Firm $\times$ State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated State $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Treated State $\times$ Time FE	No	No	Yes	No	No	Yes	No	No	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	60,903	59,570	59,570	60,903	59,570	59,570	60,903	59,570	59,570

Table IA.9: Establishment DD Regressions - Who is Hired Less?

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

$$Y_{f,s,t} = \alpha + \delta_{f,s} + \delta_{tr(s),t} + \delta_{f,t} + \Gamma \times \text{Treated}_s \times \text{Post}_{t,\tau(s)} + \left[\eta' X_{s,t-1}\right] + \left\{\delta_{f,tr(s),t}\right\} + \varepsilon_{f,s,t}$$

where $\delta_{f,s}$ are firm-state (*establishment*) fixed effects, $\delta_{tr(s),t}$ are treated $\times$ time fixed effects, $\delta_{f,t}$ are firm$\times$ time fixed effects, $X_{s,t-1}$ is a vector of control variables including lagged realizations of quarterly HPI and GDP PC growth, and $\delta_{f,tr(s),t}$ are firm $\times$ treated $\times$ time fixed effects. The outcome variable, $Y_{f,s,t}$, is a measure of firm hiring. The variables are defined in full in Appendix A. The variable Treated$_s$ is an indicator equal to one if state s is treated and Post$_{t,\tau(s)}$ is an indicator equal to one for all months after the month of treatment. A description of treated and control states is provided in Section 3.2. Standard errors are calculated by clustering two-dimensionally at the state and month level. t-statistics are reported below the coefficient estimates, with *, **, and *** indicating statistical significance at the 10%, 5%, and 1% level, respectively.

Explanatory	log(Age $<$ 25 LowWage Hires)$_{f,s,t}$			log(Age $\geq$ 25 LowWage Hires)$_{f,s,t}$			log(AgeUnknown LowWage Hires)$_{f,s,t}$		
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
$D(s,t)$	-0.036*	-0.033*	-0.039	-0.041*	-0.039*	-0.050*	-0.034**	-0.030**	-0.039**
	(-1.80)	(-1.65)	(-1.44)	(-1.86)	(-1.86)	(-1.79)	(-2.27)	(-2.31)	(-2.17)
Firm $\times$ State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Time FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Firm $\times$ Treated $\times$ Time FE	No	No	Yes	No	No	Yes	No	No	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	60,903	59,570	59,570	60,903	59,570	59,570	60,903	59,570	59,570

Table IA.10: Establishment Synthetic Control Estimates

This table contains the coefficient estimates from our synthetic control analysis. We describe our method for forming synthetic controls at the end of the Internet Appendix. Estimates represent the average estimated treatment effect for the treated group over the post-treatment period. p-values are calculated using the method in Acemoglu et al. [2016], with *, **, and *** indicating statistical significance at the 10%, 5%, and 1% level, respectively.

Explanatory Variables	$\text{LowWage}_{f,s,t}$ / $\text{Total}_{f,s,t-1}$ **(1)**	$\text{LowWageHires}_{f,s,t}$ / $\text{Total}_{f,s,t-1}$ **(2)**	$\text{LowWage}_{f,s,t}$ / $\text{LowWage}_{f,s,t-1}$ **(3)**	$\text{Turnover}_{f,s,t}$ / $\text{Total}_{f,s,t-1}$ **(4)**	$\text{LowWageTurnover}_{f,s,t}$ / $\text{Total}_{f,s,t-1}$ **(5)**
D	-0.010**	-0.002**	-0.008***	-0.002	-0.002
	(p=0.03)	(p=0.01)	(p=0.00)	(p=0.40)	(p=0.47)
N Establishments	1,450	1,450	1,450	1,450	1,450

Table IA.11: 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]. As of March 2015, 52.2% of establishments belong to the non-tradable goods industries, 9.7% belong to the tradable goods industries, 2.1% belong to the construction industries, and 33.0% belong to the other goods industries.

Three-Digit NAICS	Industry Name	Classification	%Establishments
441	Motor Vehicle and Parts Dealers	Non Tradable	2.93
442	Furniture and Home Furnishings Stores	Non Tradable	2.30
443	Electronics and Appliance Stores	Non Tradable	0.72
445	Food and Beverage Stores	Non Tradable	6.04
446	Health and Personal Care Stores	Non Tradable	1.91
447	Gasoline Stations	Non Tradable	0.60
448	Clothing and Clothing Accessories Stores	Non Tradable	13.40
451	Sport. Goods, Hobby, Mus. Instr., & Book Stores	Non Tradable	6.38
452	General Merchandise Stores	Non Tradable	6.59
453	Miscellaneous Store Retailers	Non Tradable	3.11
722	Food Services and Drinking Places	Non Tradable	11.10
211	Oil and Gas Extraction	Tradable	0.43
311	Food Manufacturing	Tradable	2.38
312	Beverage and Tobacco Product Manufacturing	Tradable	0.98
315	Apparel Manufacturing	Tradable	0.94
322	Paper Manufacturing	Tradable	0.26
323	Printing and Related Support Activities	Tradable	0.13
324	Petroleum and Coal Products Manufacturing	Tradable	0.72
325	Chemical Manufacturing	Tradable	1.49
326	Plastics and Rubber Products Manufacturing	Tradable	0.68
333	Machinery Manufacturing	Tradable	0.09
334	Computer and Electronic Product Manufacturing	Tradable	0.21
335	Elec. Equip., Appliance, and Component Manuf.	Tradable	0.09
336	Transportation Equipment Manufacturing	Tradable	1.02
339	Miscellaneous Manufacturing	Tradable	0.26

Table IA.12: 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]. As of March 2015, 52.2% of establishments belong to the non-tradable goods industries, 9.7% belong to the tradable goods industries, 2.1% belong to the construction industries, and 33.0% belong to the other goods industries.

Three-Digit NAICS	Industry Name	Classification	%Establishments
236	Construction of Buildings	Construction	0.13
321	Wood Product Manufacturing	Construction	0.13
444	Building Mat., Garden Equip., + Supplies Dealers	Construction	1.36
531	Real Estate	Construction	0.55
424	Merchant Wholesalers, Nondurable Goods	Other	0.17
454	Nonstore Retailers	Other	0.30
481	Air Transportation	Other	0.77
484	Truck Transportation	Other	0.21
485	Transit and Ground Passenger Transportation	Other	1.06
486	Pipeline Transportation	Other	0.13
488	Support Activities for Transportation	Other	0.72
492	Couriers and Messengers	Other	1.06
512	Motion Picture and Sound Recording Industries	Other	0.30
515	Broadcasting (except Internet)	Other	0.34
517	Telecommunications	Other	0.43
518	Data Processing, Hosting, and Related Services	Other	0.09
522	Credit Intermediation and Related Activities	Other	2.34
523	Securities, Commodity Contracts, and Other Inv.	Other	0.98
524	Insurance Carriers and Related Activities	Other	0.17
532	Rental and Leasing Services	Other	2.04
551	Management of Companies and Enterprises	Other	0.04
561	Administrative and Support Services	Other	5.83
562	Waste Management and Remediation Services	Other	0.26
611	Educational Services	Other	5.19
621	Ambulatory Health Care Services	Other	0.26
622	Hospitals	Other	2.08
623	Nursing and Residential Care Facilities	Other	1.28
624	Social Assistance	Other	0.09
713	Amusement, Gambling, and Recreation Indus	Other	1.36
721	Accommodation	Other	4.21
812	Personal and Laundry Services	Other	1.32
813	Religious, Grantmaking, Civic, Etc.	Other	0.09

Figure IA.1: Establishment Industry Composition

This figure plots the BLS Industry distribution for the 2,470 establishments in our sample. The establishments represent 339 distinct firms. 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". The y-axis lists the frequency of establishments in the sample, and the x-axis lists BLS Industries.

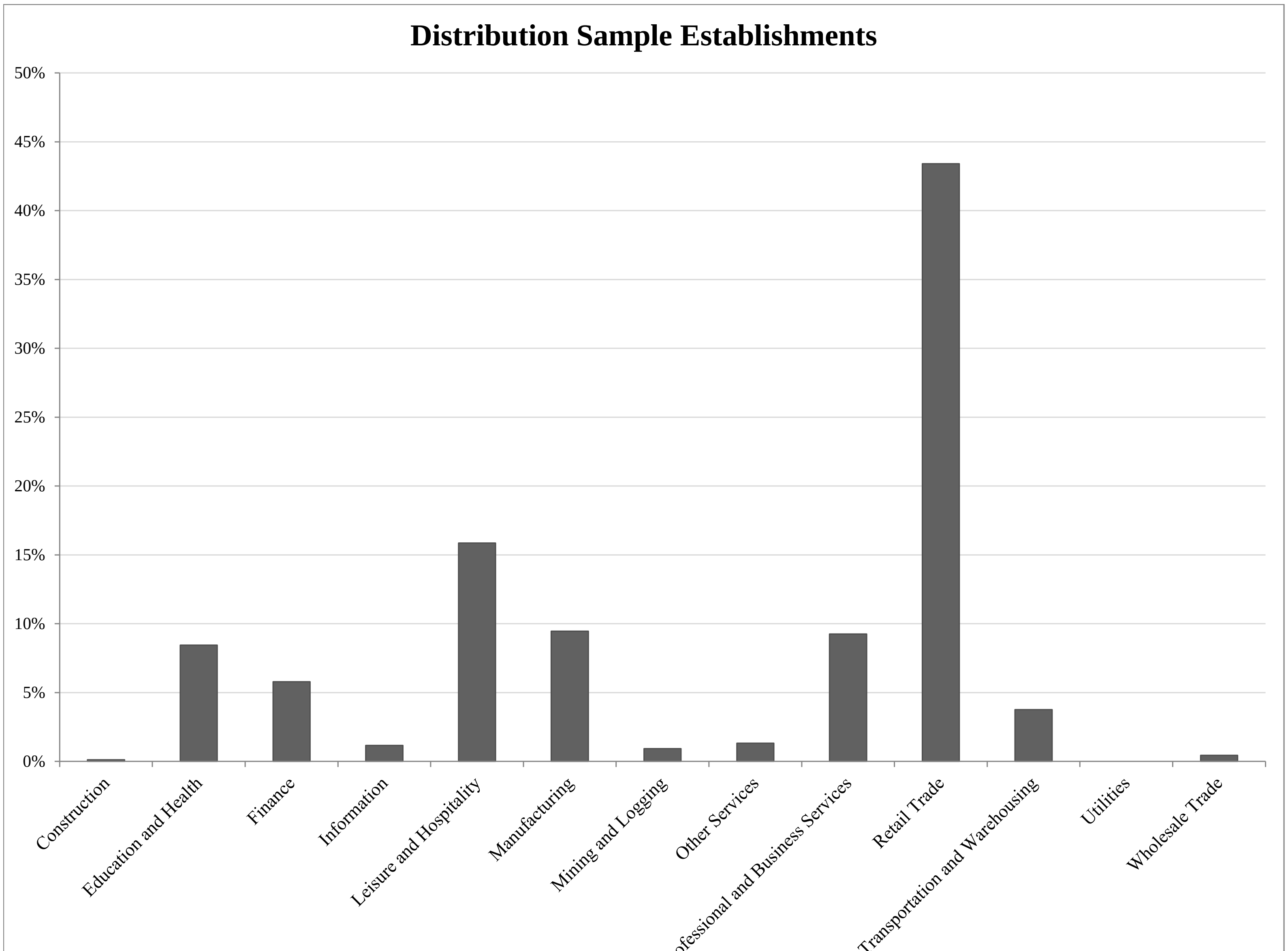


Figure IA.2: Establishment Employment State Distribution

This figure plots the distribution of employment across states for our sample of 2,470 establishments. The y-axis denotes the percent of employment, and the x-axis lists the treated and controls states (with T denoting treated states). The distribution for sample establishment employments is given by the dark gray bars, and the distribution for the U.S. population is given by the light gray bars.

Figure IA.3: Evolution of Hours for Hourly-wage Employees

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

$$H_{i,s,t} = \alpha + \delta_i + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \sum_{\tau=-11}^{12} \Gamma_\tau \text{Treated}_s \times \text{D}(s,t,\tau) + \eta' X_{i,t} + \varepsilon_{i,s,t}$$

where $H_{i,s,t}$ is an indicator for average weekly hours, δ_i are individual fixed effects, $\delta_{tr(s),f(i),t}$ are treatment $\times$ firm $\times$ time fixed effects, $\delta_{tr(s),C(i),t}$ are treatment $\times$ cohort $\times$ time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in tenure and lagged realizations of quarterly HPI and GDP PC growth. The variable Treated_s is a dummy variable that takes a value one if state s implements an increase to its minimum wage, 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 3.2 of the text. The model is estimated separately for the the subsamples of Minimum Wage, Bound, and Pseudo Low Wage Employees, and the coefficient estimates are plotted in the top, middle, and bottom panels, respectively. 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 $\tau = -9$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the state and time level.

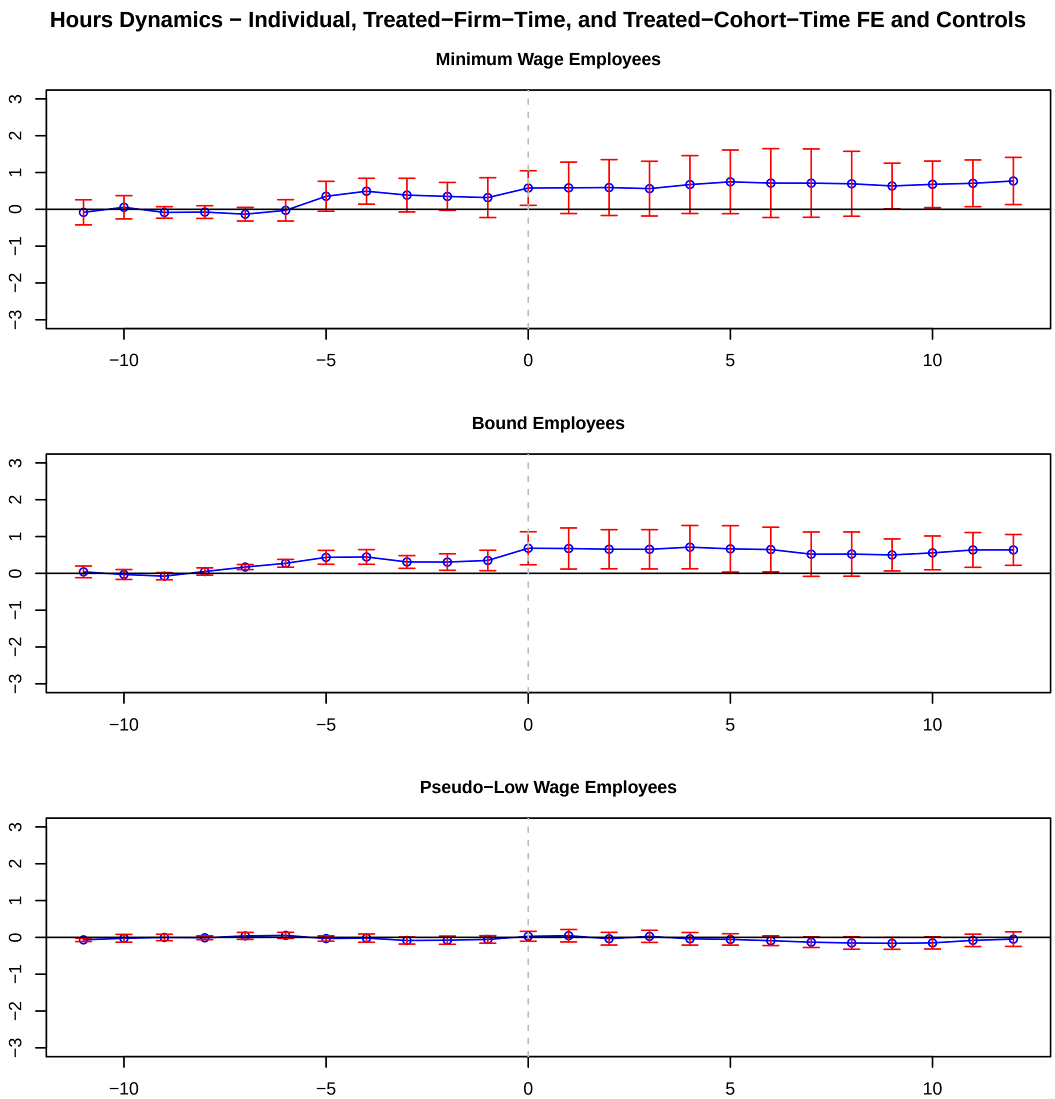


Figure IA.4: Evolution of Employment – Triple Differences

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

$$Y_{i,s,t} = \alpha + \delta_i + \delta_{s,t} + \delta_{tr(s),f(i),t} + \delta_{tr(s),C(i),t} + \delta_{Bound,tr(s),t} + \sum_{\tau=-12,\tau\neq-9}^{12} \Gamma_\tau \text{Treated}_s \times \text{D}(s,t,\tau) \times \text{Bound}_i + \eta' X_{i,t} + \varepsilon_{i,s,t}$$

where $Y_{i,s,t}$ is an indicator for employment, voluntary turnover, or involuntary turnover, δ_i are individual fixed effects, $\delta_{s,t}$ are state $\times$ time fixed effects, $\delta_{Bound,t}$ are *Bound employee* $\times$ treatment $\times$ time fixed effects, $\delta_{tr(s),f(i),t}$ are treatment $\times$ firm $\times$ time fixed effects, $\delta_{tr(s),C(i),t}$ are treatment $\times$ cohort $\times$ time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in tenure and lagged realizations of quarterly HPI and GDP PC growth. The variable Treated_s is a dummy variable that takes a value one if state s implements an increase to its minimum wage, $D(s,t,\tau)$ is a dummy variable equal to one for all individuals in state s, τ months relative to the treated month, and Bound_i is an indicator equal to one if individual i is a *Bound employee*. The sample includes all *Bound* and *Pseudo-low wage employees.* The definition of treated and control states is provided in Section 3.2 of the text. 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 $\{\Gamma_\tau\}_{\tau=-11}^{12}$ coefficients, where the month corresponding to $\tau=-9$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the state and time level.

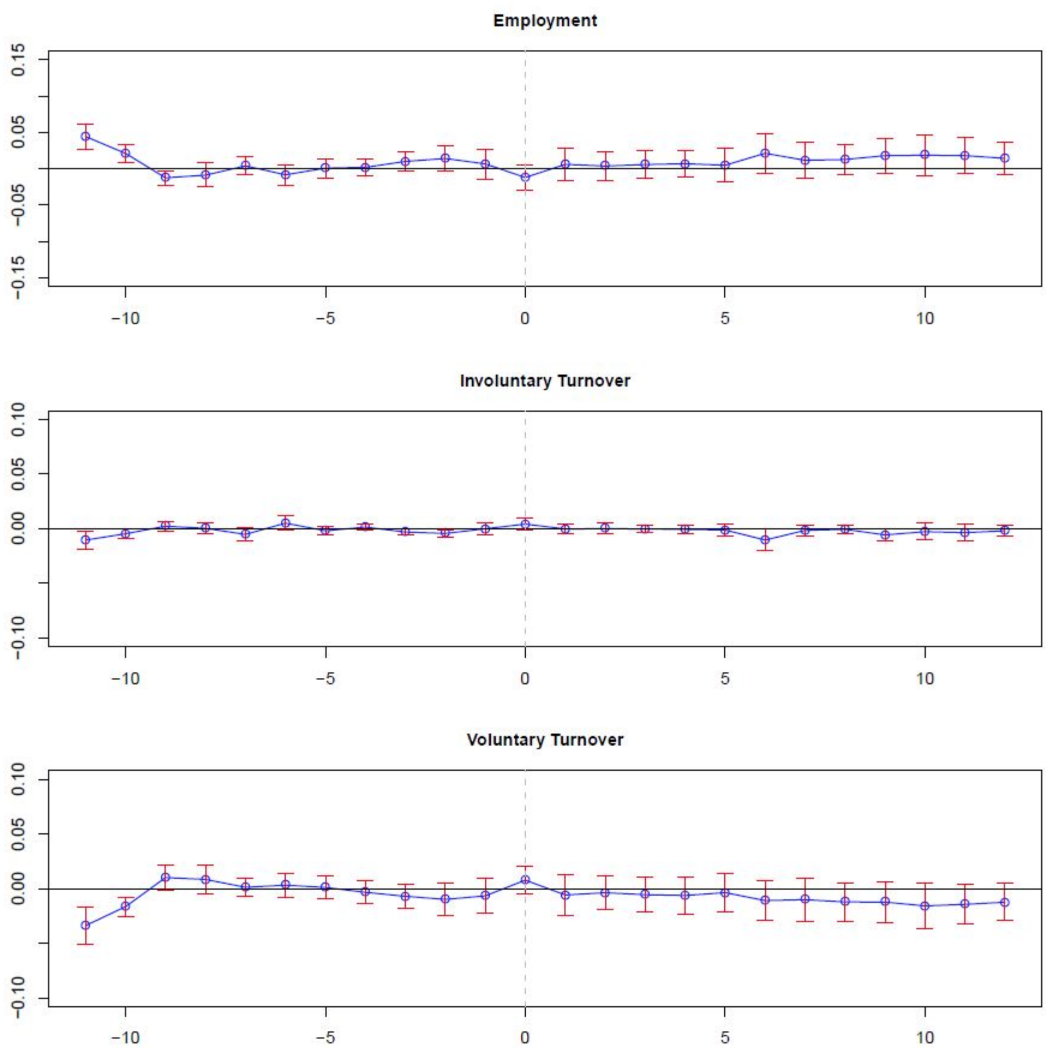


Figure IA.5: Evolution of Employment – Border Counties

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

$$E_{i,s,t,b} = \alpha + \delta_i + \delta_{b,f(i),t} + \delta_{b,C(i),t} + \sum_{\tau=-11}^{12} \Gamma_\tau \text{Treated}_s \times \text{D}(s,t,\tau) + \eta' X_{i,t} + \varepsilon_{i,s,t,b}$$

where $E_{i,s,t}$ is an indicator for employment, δ_i are individual fixed effects, $\delta_{b,f(i),t}$ are border county × firm × time fixed effects, $\delta_{b,C(i),t}$ are border county × cohort × time fixed effects, and $X_{i,t}$ is a vector of control variables including a quadratic in tenure and lagged realizations of quarterly HPI and GDP PC growth. Border counties are defined as groupings of counties that share a common border. The variable Treated$_s$ is a dummy variable that takes a value one if state s implements an increase to its minimum wage, 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 3.2 of the text. The model is estimated separately for the the subsamples of Minimum Wage, Bound, and Pseudo Low Wage Employees, and the coefficient estimates are plotted in the top, middle, and bottom panels, respectively. The model is only estimated for individuals in border counties. 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 $\tau = -9$ is excluded as the reference level. The vertical red bars indicate confidence 95% confidence intervals, where standard errors are clustered at the state and time level.

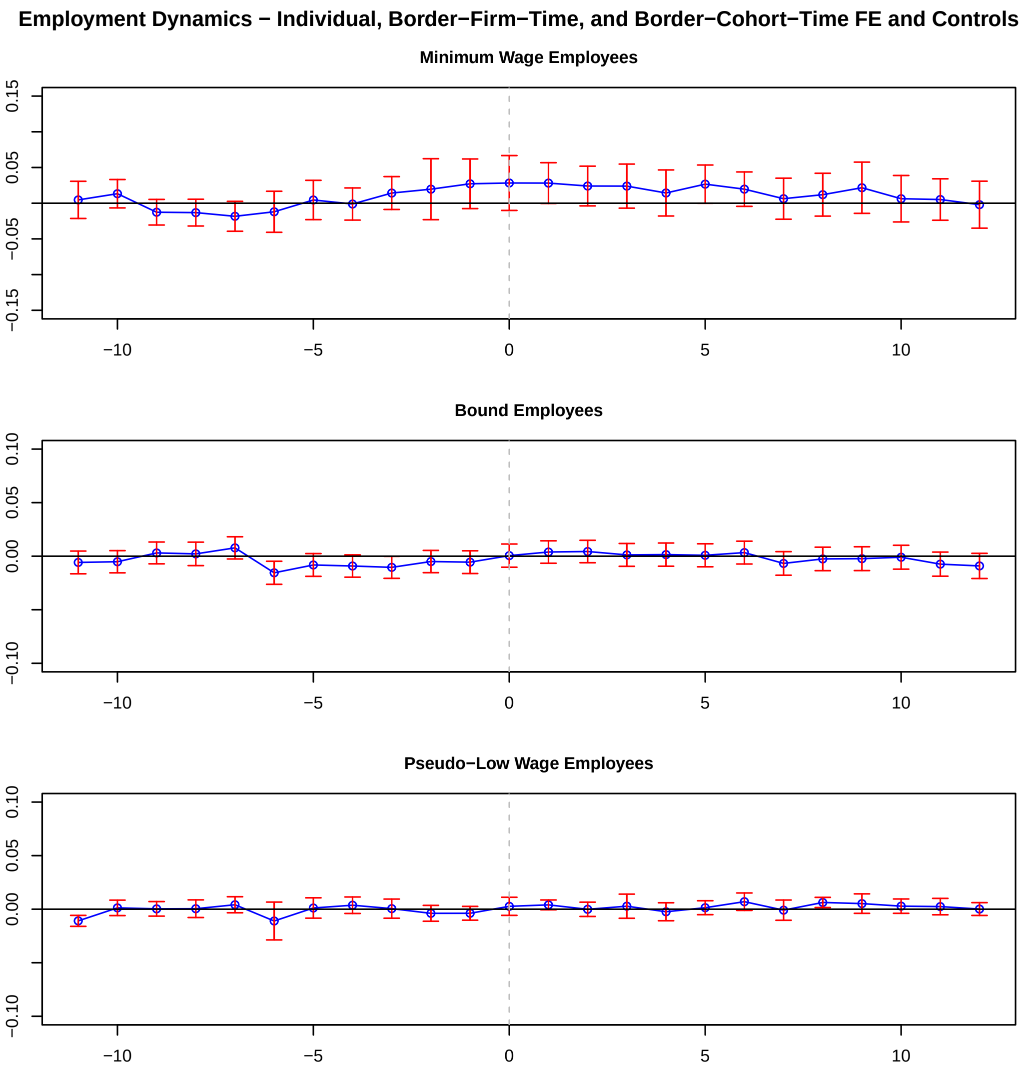


Figure IA.6: Evolution of Establishment Employment Flow

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

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

where$\delta_{f,s}$ are are establishment fixed effects, $\delta_{tr(s),t}$ are treated$\times$ month fixed effects, $\delta_{f,t}$ are firm $\times$ month fixed effects, and $X_{s,t-1}$ is a vector of control variables including lagged realizations of state HPI and GDP PC growth. The outcome variable, $Y_{f,s,t}$ is a measure of low wage employment. The variable Treated$_s$ is a dummy variable that takes a value one if state s implements an increase to its minimum wage, 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 $\{\Gamma_\tau\}_{\tau=-3}^{3}$ coefficients, where the quarters corresponding to $\tau = -3$ is excluded as the reference level. The vertical red bars indicate confidence 90% confidence intervals, where standard errors are clustered at the state and time level.

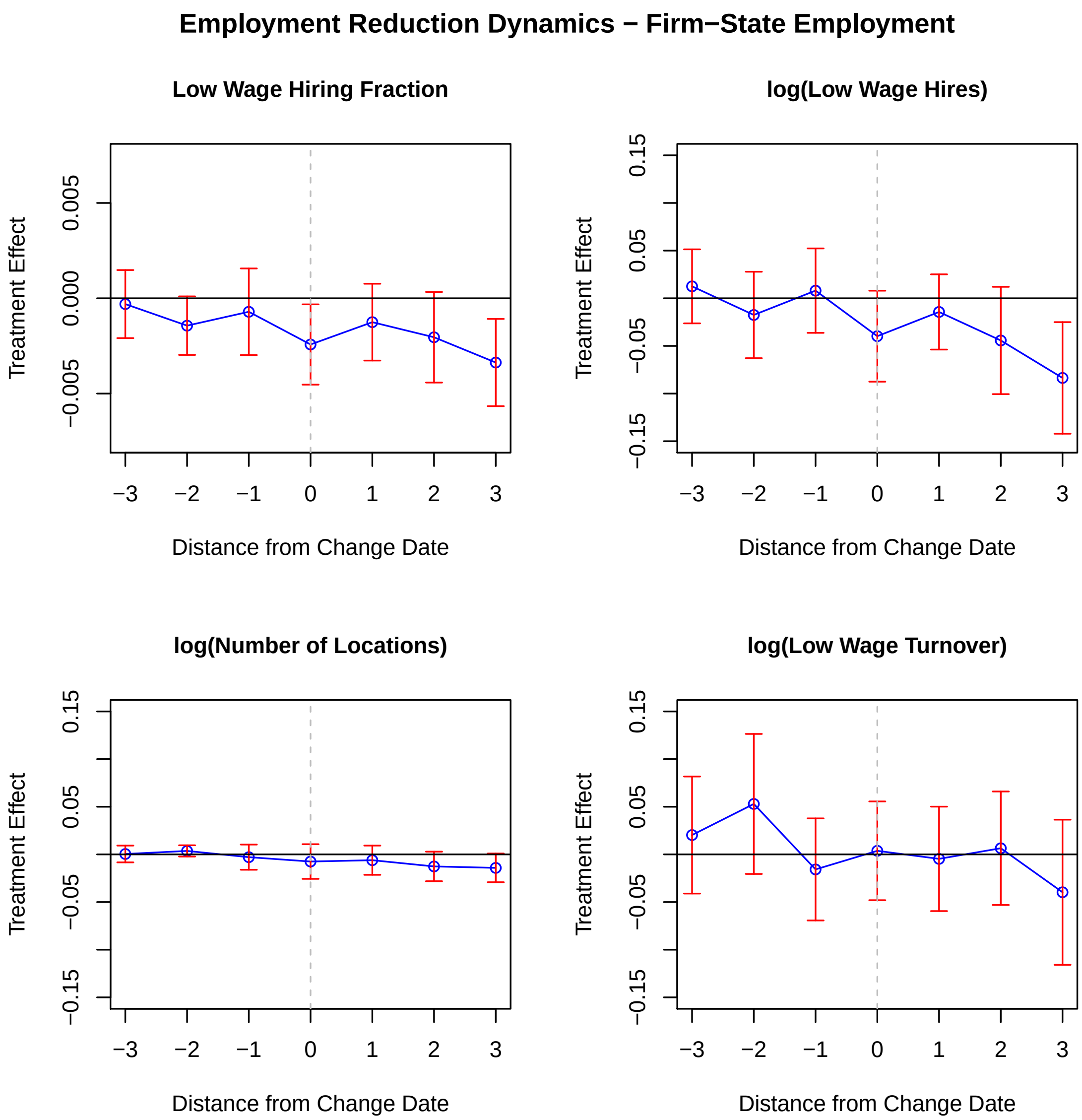
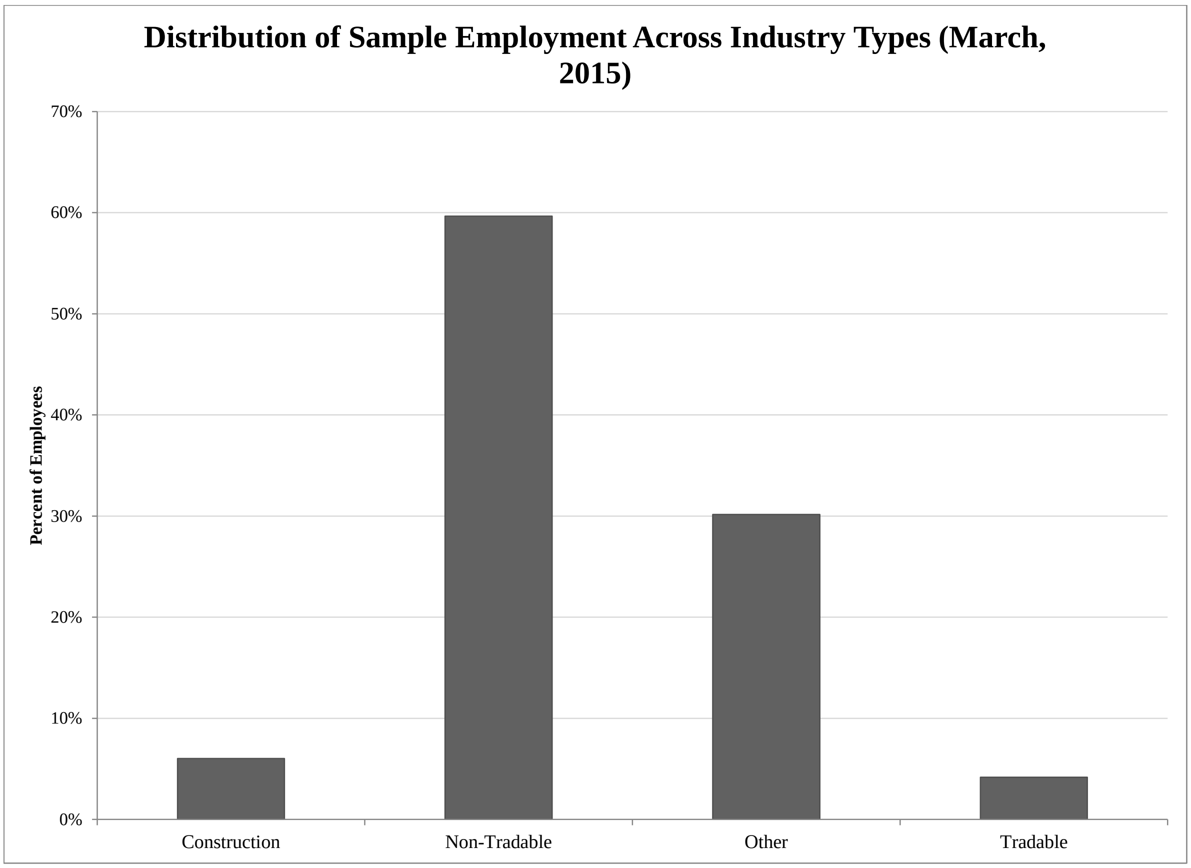


Figure IA.7: Distribution of Employment Across Goods Industries

This figure describes the distribution of sample establishment employment in across goods industries. The x-axis corresponds to types of goods industries (non-tradable, tradable, construction, and other). The definition for goods industries and their mappings to three-digit NAICS codes is provided in Tables IA.11 and IA.12. The y-axis corresponds to the percent of establishments in each type of goods industry.



Calculating Our Synthetic Control Estimates

In this Appendix we document our method for calculating average treatment effects using the synthetic control method of Abadie et al. [2010]. Our process can be broken down into three steps: (1) synthetic control selection, (2) point estimation, and (3) inference. We detail each of the three steps below. We also note that the synthetic control method requires a balanced panel to select controls and compute estimates. Thus, we limit our set of establishments to those that belong to the sample for the period beginning 9 months prior to treatment and ending 9 months after treatment. We also limit our analysis to outcome variables which are fractions so we can easily translate these into estimates for the entire treatment group without scaling issues.

1. **Synthetic Control Selection:** We form a different synthetic control for each treated unit in our sample. For a given treated establishment, we start by restricting the set of potential "donor establishments" to control establishments located in the same geographic region. Then, following the applied literature on synthetic control (e.g. Gobillon and Magnac [2016]), we allocate weights to donor establishments by matching on pre-treatment values of the outcome variable. We allow for the weights to change for the same treated establishment for each outcome variable in our sample. Throughout the analysis, we use the two-step optimization procedure in the `R` package `Synth` to form the optimal weights and weighting matrix. Following Acemoglu et al. [2016], we discard establishments with poor pre-treatment synthetic fits as defined by pre-treatment root-mean squared errors being greater than $\sqrt{3}$ times the average root-mean squared error.

2. **Point Estimation:** We follow Acemoglu et al. [2016] and construct a single point estimate for the entire treated group for the effect of the policy intervention on the outcome variable. The formula for the point estimate is given by:

$$\hat{\phi}(\text{Treated}) = \frac{\sum_{i \in \text{Treated}} \left(\frac{\sum_{t=T_0+1}^{T} Y_{i,t} - \hat{Y}_{i,t}^{N}}{\hat{\sigma}_i} \right)}{\sum_{i \in \text{Treated}} \hat{\sigma}_i^{-1}},$$

where $Y_{i,t}$ is the outcome variable in month t for treated establishment i, $\hat{Y}_{i,t}^{N}$ is the synthetic control estimate of the counterfactual outcome, T_0+1 is the first period of the policy intervention, and $\hat{\sigma}_i = \sqrt{\sum_{t=1}^{T_0}\left(Y_{i,t}-\hat{Y}_{i,t}^{N}\right)^2/T_0}$ is the pre-treatment root-mean squared error of the outcome variable and the synthetic control estimate of the counterfactual outcome. This point estimate is a "mean" treatment effect for the treated group over the post-treatment period, where the "mean" is weighted by how well the synthetic control matches the treated unit in the pre-treatment period.

3. **Inference:** Again, following Acemoglu et al. [2016], we conduct statistical inference on our point estimates by forming the empirical distribution of $\hat{\phi}$ from placebo treatment groups. Specifically, let N_T denote the size of the treatment group and let N_C denote the size of the control group. To form our empirical distribution, we randomly select $10,000$ placebo groups of control establishments of size N_T from the set of $\binom{N_C}{N_T}$ possible combinations and compute $\hat{\phi}$(Placebo) for each placebo group. The synthetic control for each control establishment j in the placebo group is constructed as in Step (1), but with assigning the unit j a placebo treatment status. We then use this distribution of $10,000$ $\hat{\phi}$(Placebo)'s to form the empirical distribution of $\hat{\phi}$. We then compute the p-value of $\hat{\phi}$(Treated) for each outcome variable by ranking $\hat{\phi}$(Treated) relative to the empirical distribution.