THREE ESSAYS IN LABOR ECONOMICS

By

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ABSTRACT

This dissertation consists of three chapters analyzing labor markets of the United States, with a particular focus on the minimum wages, hours of work, and the relationships between wages and hours.

In chapter 1, I study the effects of minimum wages on the labor market outcomes of the elderly. In contrast to the groups that are more typically studied (e.g., teenagers), I find small, positive employment effects of minimum wages on those in their late sixties by using a variety of empirical specifications commonly used in the minimum wage literature. The point estimates of employment elasticities fall in the range of 0.1 to 0.3. The positive effects are not limited to the minimum wage workers; a broader class of workers including those who are paid wages well above the minimum wage are affected. To explain the results, I provide two pieces of evidence on labor-labor substitution. First, the industry-level employment elasticities of the young and elderly with respect to the minimum wage are negatively correlated. Second, I directly estimate the elasticity of substitution between young and older workers using the nested-CES production function framework. 2SLS estimates suggest that young and older workers are substitutes for each other. Although the estimated elasticity of substitution is small, it suggests that labor demand is shifted toward older workers when minimum wages are increased.

Chapter 2 examines short-run adjustments of working hours to minimum wage increases. By combining observations from the matched Current Population Survey and data regarding large-scale state-level minimum wage increases, I find negative effects on working hours. Large minimum wage increases reduce working hours by approximately 50 minutes per week. These effects are neither identical nor monotonic across working hours. Workers who worked part-time or overtime prior to the increases are negatively affected in terms of their working hours, while full-time workers are largely unaffected. Adjustments are related to a 40-hour workweek. There is a large shift from overtime to 40-hour per week positions for those working overtime in the previous year, while part-time workers are less likely to move to 40-hour per week positions after increases. These adjustments are consistent with the predictions from a labor demand model with a kinked labor

cost schedule caused by the overtime pay regulation.

Taken together, my first two chapters study how firms adjust their workforce in response to minimum wages. My results show that the adjustment of the headcount of teenage workers, the primary question in the minimum wage literature, may misrepresent the adjustments of the labor force to minimum wages.

Chapter 3 is my joint work with Steven J. Haider. In this chapter, we try to answer the following question: To what extent do workers earn a higher hourly wage if they work very long hours? Based on four decades of data from the Current Population Surveys and the Panel Study of Income Dynamics, our findings regarding this fundamental question about labor supply incentives are three-fold. First, the wage gap between those working very long hours (50+ hours per week) as compared to those working a standard work week (40 hours per week) has gone from being strongly negative in 1980 to being strongly positive in 2018. Second, at the individual level, a long-hours premium currently exists for about 95% of hourly workers and 40% of salary workers within their current job because of overtime regulations, but relatively few workers earn overtime pay. Third, if were to define the individual premium to be the entire within-occupation long-hours premium, then most workers would earn an hourly wage premium by working more hours, but it is unclear whether such a broad definition is appropriate.

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LIST OF ABBREVIATIONS

2SLS Two-Stage Least Squares

ADR Allegretto et al. (2011). See text.

BLS Bureau of Labor Statistics

CDLZ Cengiz et al. (2019). See text.

CEPR Centre for Economic Policy Research

CES Constant Elasticity of Substitution

CPS Current Population Survey

CPS-ORG Current Population Survey Outgoing Rotation Group

DoL Department of Labor

EAP Executive, Administrative, and Professional

FE Fixed Effects

FLSA Fair Labor Standard Act

FTE Full-Time Equivalent

HEC Highly Compensated Employees

IRS Internal Revenue Service

IV Instrumental Variable

ME Measurement Error

NBER National Bureau of Economic Research

NSW Neumark et al. (2004). See text.

NW Neumark and Wascher (1992). See text.

OASDI Old-Age, Survivors, and Disability Insurance

OLS Ordinary Least Squares

OTC Overtime pay, Tips, and Commission

PCE Personal Consumption Expenditures

PSID Panel Study of Income Dynamics

RHS Right Hand Side

SE Standard Error

SEO Survey of Economic Opportunity

SRC Survey Research Center

SSA Social Security Administration

TVA Tennessee Valley Authority

TWFE Two-Way Fixed Effects

USD United States Dollar

CHAPTER 1

MINIMUM WAGES AND THE ELDERLY

1.1 Introduction

Older workers tend to earn lower wages (Mincer, 1974; Haider and Loughran, 2010; Maestas, 2010). Due to the aging population, older workers are increasingly important in the low-wage labor markets in developed countries. For example, in the United States, the fraction of workers aged 65 or above among low-wage workers whose hourly wages are below 120 percent of the effective minimum wage increased from 4% in 1990 to 6.2% in 2019. During the same time, the fraction of teenagers declined from 26.9% to 14.7%.[1] Given the aging population, the increasing trend of the fraction of older workers is likely to continue.

Among the policy tools affecting low-wage labor markets, the minimum wage is perhaps the most ubiquitous and controversial among the public, policymakers, and academia. Despite the growing importance of older workers in the low-wage labor market and the vast economic literature examining the minimum wage or older workers, there exist very few papers that combine the two. This paper tries to fill that gap by analyzing the effects of minimum wage on the labor market outcomes of the elderly.

In this paper, I apply a variety of empirical strategies commonly used in the general minimum wage literature but focus on the elderly. I find small, positive employment effects of minimum wages on those in their late sixties instead of the negative effects on employment predicted by standard neoclassical economic theory. The point estimates of minimum wage-employment elasticities using commonly used methods fall in the range of 0.1 to 0.3. This agreement across methods is in sharp contrast to the results for teenagers, which tend to be negative or zero effects (Manning, 2021). These results suggest that the impacts on older workers are different. Further analysis regarding wage distribution confirms that older workers respond to minimum wages in a different manner. Unlike young and prime-age workers whose employment responses are limited to near minimum wages, a portion of the positive effects for older workers come from workers above the minimum

[1] My own calculation using Current Population Survey Outgoing Rotation Group.

wage.

These baseline employment effects raise two questions. First, why are employment effects on older workers positive? Second, why are workers whose hourly wage may not be directly raised by minimum wages also affected? To answer the first question, previous studies have tended to focus on labor supply decisions and interpret positive employment effects as a sign of delayed retirement (Borgschulte and Cho, 2020; Hampton and Totty, 2021). The logic behind this argument is as follows. By increasing workers' own wages, a higher minimum wage could provide more incentive for them to work longer. Although intuitive, this explanation has three limitations. First, the wage distribution analysis suggests that at least some portion of the positive responses come from a broader class of workers including those whose wages may not be increased by a minimum wage. Second, the same minimum wage increase will provide incentives to work more to teenagers and other younger low-wage workers as well as older workers, but older workers are the only population who actually do. Therefore, the labor supply argument is not enough to explain why older workers respond differently. Finally, in a competitive labor market with a binding minimum wage, labor demand primarily determines labor market outcomes; therefore, labor demand explanations should also be explored.

As an alternative explanation, I study a particular labor demand pathway; labor-labor substitution between young and older workers. In response to a higher minimum wage, employers may shift their demand to more productive workers since the relative price of the least skilled workers has increased (e.g., Clemens et al., 2021; Butschek, 2022). Older workers may be more productive than their younger counterparts earning minimum wages, perhaps because of their skills or because of their much lower probability of turnover (Allen, 2019). If labor demand is shifted toward older workers with higher wages and better productivity, the positive responses may extend to a broader class of workers.

I provide two pieces of evidence supporting this hypothesis. First, industry-level analysis suggests that the industry-specific minimum wage-employment elasticities of young and older workers are negatively correlated, providing suggestive evidence of labor-labor substitution. More formally, I estimate the elasticity of substitution between younger and older workers using a nested constant elasticity of substitution (CES) production function framework and a simulated-wage instrumental variable exploiting minimum wage changes. The estimated elasticities of substitutions are approximately 0.5, implying a small degree of substitution.

This paper contributes to the active, growing literature on the minimum wage in several ways. First, using a variety of specifications, I study a group of workers (aged 65-70) who are understudied in the literature. Compared to existing papers that examine a broader age range of older workers and provide weaker positive or mixed evidence of employment effects (Borgschulte and Cho, 2020; Cengiz et al., 2022), I focus on a smaller group and show more robust positive effects. Second, this paper provides a new explanation based on labor-labor substitution between demographic groups. Both industry-level analysis and the evidence from the nested-CES framework are consistent with labor demand shifts from younger minimum wage workers toward higher-wage, older workers.

This paper also extends another strand of labor economic literature examining the elasticity of substitution between demographic groups. Two demographic groups of primary interest in this study, older (aged 65-70) and younger (aged 16-21) workers, are often excluded from the scope of other papers. This paper also harnesses a different source of identifying variation. Unlike studies using labor supply shifters for identification, I exploit policy variation affecting the relative wage. By applying a simulated instrument to local labor market outcomes, I provide evidence of labor-labor substitution between workers of different ages.

The remainder of the paper is organized as follows. In section 1.2, I discuss the related literature. My empirical strategies are discussed in section 1.3. Section 1.4 introduces and describes the data. Section 1.5 presents the empirical findings on the employment effects of minimum wages. Section 1.6 explores the role of labor-labor substitution to explain the results in section 1.5. Section 1.7 concludes the study.

1.2 Related Literature

Over decades of analyzing the employment effects of minimum wages, labor economists have often studied the effects on specific low-wage demographic groups such as teenagers.[2] Surprisingly little attention has been paid to the effects on older populations even though a fraction of workers affected by minimum wages are increasing in their later stage of life, as recognized by Flinn (2010) and shown in Figure 1.1 below.

Recently, economists have started to examine the relationship between the minimum wage and the elderly in their sixties. By applying a canonical state-panel approach using the log of the minimum wage as a key regressor to CPS Basic Monthly files (1983-2016), Borgschulte and Cho (2020) report zero to small positive employment effects of minimum wages on older workers, with estimated minimum wage-employment elasticities ranging from 0 to 0.15. They further apply a border-county approach to the SSA's OASDI beneficiaries and provide evidence that a higher minimum wage makes Social Security beneficiaries delay the timing of claiming benefits, which is in line with positive employment effects. They provide two explanations for these effects. First, higher minimum wages provide more incentives to work for the elderly whose labor supply is fairly elastic. Second, demand may shift toward older workers. They find some suggestive evidence that labor demand has shifted from workers in their late fifties (ages 55-61) to those in their sixties (ages 62-70). In contrast to Borgschulte and Cho (2020), who use all observations, Cengiz et al. (2022) define relevant populations using machine learning techniques. Specifically, their machine learning technique predicts those who are more likely to earn minimum wages. Using an event-based approach, they report null employment effects on the elderly (ages 60-70) who are predicted to earn low wages.

Hampton and Totty (2021) examine the same question from a different angle, by focusing on those who earn near-minimum wage using longitudinal data. By using observations (aged 62-70) from SIPP (1978-2014) linked to IRS and OASDI data and the log of the minimum wage as

[2]For a broader review of minimum wage effects on employment, see Brown (1999), Neumark and Wascher (2008), Belman and Wolfson (2014), and Belman et al. (2015). Focusing on teenagers, a recent review by Manning (2021, Figure 3) shows that teen employment elasticities with respect to minimum wages estimated by commonly used specifications lie in the range of -0.3 to 0.1.

primary regressors, they find that a higher minimum wage increases employment and reduces the permanent exit from the employment of low-wage workers whose wage are equal to or lower than the minimum wage plus two dollars. According to their preferred estimates, a 10-percent increase in the minimum wage is associated with a 2 percent increase in employment (employment elasticity of approximately 0.2) and a 6.4 percent reduction in permanent employment exit evaluated at the sample mean. They provide evidence that a higher minimum wage delays Social Security benefit claimings, especially when the minimum wage is related to a binding earnings test. Unlike the effects on minimum wage workers, workers with slightly higher hourly wages are almost unaffected except for negative effects on part-time work. Hampton and Totty (2021) interpret these findings as a sign of delayed retirement.

Several additional papers examine older workers more broadly, with different age criteria. By using the American Community Survey (2011-2016), Clemens et al. (2021) find that increases in the minimum wage increase the fraction of older workers (ages 50-64) employed in low-wage occupations, while the same increases reduce the fraction of younger workers (ages 16-21). This suggests labor-labor substitution toward older workers. By using observations from CPS (1980-2015), in contrast, Lordan and Neumark (2018) show that higher minimum wages have an adverse effect on low-skilled older workers (ages 40+) in manufacturing and automatable jobs, suggesting labor-labor substitution from older workers. Borgschulte and Cho (2020) report that minimum wages reduce the number of hours worked and full-time work but not the overall employment of workers aged 55-61. Finally, by using Canadian data (1993-1999), Fang and Gunderson (2009) show the positive effects of large minimum wage increases on the employment of older workers (age 50 or above) who earned near-minimum wages prior to the increases.

There is a small but burgeoning literature on broader channels of adjustments responding to higher minimum wages, as recently reviewed by Clemens (2021). One line of adjustment to which this paper is more closely related is labor-labor substitution. When the wages of the least-skilled workers increase, firms may shift their demand toward higher-skilled workers, since the relative price of that labor falls. Although the argument that minimum wage stimulates employing more productive workers has a long history dating back to at least Webb (1912), empirical evidence on whether firms respond to a higher minimum wage in this way is mixed. Some papers have shown that labor demand is shifted toward higher skilled workers proxied by observable characteristics such as age, gender or education (Clemens et al., 2021; Fairris and Bujanda, 2008; Hirsch et al., 2015; Neumark and Wascher, 2011), while other papers do not find a detectable shift along the observable characteristics (e.g., Fairris and Bujanda, 2008; Giuliano, 2013; Butschek, 2022). By using the introduction of statutory minimum wage in Germany, Butschek (2022) reports evidence of labor-labor substitution toward higher productivity workers identified by the framework in Card et al. (2013) and he finds no evidence of labor-labor substitution using observable characteristics as a proxy for productivity. However, among these papers, only Clemens et al. (2021) consider older workers (under age 65).

This paper contributes to the minimum wage literature in several ways. First, I find that positive employment effects mainly come from those in their late sixties. By focusing on this population, I provide larger and more robust evidence of positive effects compared to existing studies. Second, based on these focal ages, I provide a new explanation based on the labor demand side argument; labor-labor substitution. Third, I examine the pattern of substitution of understudied pairs of population, young and older workers, stimulated by minimum wages. Given that these two demographic populations are among the most affected by minimum wage, understanding substitution between them is practically important.

This paper is also related to another active stream of literature studying complementarities or substitutability between younger and older workers. Due to the aging population, many developed countries are implementing labor market policies aiming to make older workers stay in the labor force longer (e.g. pension reform). One immediate question is whether this entails costs for the younger generation. The answer hinges on the substitutability between younger and older workers. If older and younger workers are substitutes for each other, making older workers work longer would hurt younger workers' labor market outcomes. The evidence is mixed. Studies using macroeconomic, country-level data tend to find null effects of delayed retirement on youth labor market outcomes (e.g. Gruber and Wise, 2010), while some recent studies using firm-level data provide evidence of the negative effects of delayed retirement on the labor market outcomes of younger workers (e.g., Bovini and Paradisi, 2019; Eckrote-Nordland, 2021). These studies, however, tend to focus on older workers below age 65 or younger workers completing their education. There is little evidence of substitution of teenagers or workers above age 65, although their labor market behaviors are relatively similar to each ohter.

One exception is Mohnen (2021) using U.S. commuting zone-level data. Using Bartik-type instruments, Mohnen (2021) finds that delayed retirement of older workers (aged 55+) reduces the employment and outcome of younger adults (aged 22-30). Mohnen (2021) also finds that the negative effects become larger for teenagers (aged 16-21) and smaller for prime-age workers (aged 31-44), suggesting that the youngest workers are closer substitutes for the oldest workers. The evidence in this paper is in line with Mohnen (2021).

Finally, this paper is also related to the literature examining substitutions between different demographic groups using the nested-CES production function framework. Labor economists have often estimated the elasticity of substitution using the effects of supply-side variations on wages (Welch, 1979; Katz and Murphy, 1992; Card and Lemieux, 2001; Borjas, 2003; Ottaviano and Peri, 2012). There are several differences between this paper and others. Existing papers tend to exclude the youngest or oldest workers from their analysis. These groups, however, are the primary focus of this paper. Building on the observation that the hourly wages of younger workers are more affected by local minimum wage changes, this paper focuses on the regional labor market instead of the national labor market approach used by much of the literature. Given that the price of young workers is heavily affected by minimum wage which is a regional labor market policy in the United States, the local market could be a better geographic unit for this population. I also use a new instrumental variable as a shifter for the relative wage between demographic groups and estimate the degree of substitutability between age groups that have typically been ignored in other papers.

1.3 Empirical Strategy

Much of the minimum wage literature can be characterized as attempts to estimate the following model:

$$y = \beta f(MW) + X'\gamma + \Pi + \varepsilon$$

where y is the outcomes of interest, β is the parameter of interest capturing treatment effects, $f(MW)$ is a function measuring the intensity or change of the minimum wage, X is a vector of controls, Π is a set of fixed effects, and ε is the idiosyncratic error term.

There is an ongoing debate over the identification strategy for minimum wage effects.[3] Important questions in this debate include the following: How can we define the treatment? What is the best way to define the treatment and control groups, and what is the best way to control different trends between states? The former is often related to the choice of $f(\cdot)$ while the latter is often related to the choice of Π. Answering these questions is hard, especially when researchers rely on different levels of geographic variation over different time frames. Researchers have proposed various ways to estimate β. Each method has advantages and disadvantages, that can vary depending on the research questions. Instead of adhering to one strategy, this study exploits a variety of specifications in the literature.

I begin with the canonical two-way fixed effects (TWFE) model in the spirit of Neumark and Wascher (1992). The regression specification follows Borgschulte and Cho (2020) for the context of older workers. In the rest of the paper, I call this the NW-type specification.

$$y_{ista} = \beta \ln MW_{st} + X'_{ista}\gamma + \phi_{sa} + \phi_{ta} + \varepsilon_{ista} \tag{1.1}$$

In equation (1.1), y_{ista} is the labor market outcomes of an individual i whose age is a and who resides in state s at time t. A key regressor is the log of the minimum wage, $\ln MW$. To account for age-specific differences across time and state, I use state-by-age specific fixed effects and time-by-age specific fixed effects. X_{ista} contains demographic controls including indicators for education level (five categories), race (four categories), gender, and marital status. This specification is closely

[3]For recent discussions, see Dube et al. (2010); Allegretto et al. (2011, 2017); Neumark et al. (2014); Meer and West (2016); Neumark and Wascher (2017); Cengiz et al. (2019)

related to one used by Borgschulte and Cho (2020) except that I employ individual-level observations and control for demographic characteristics instead of using state-time-age level aggregates. Using individual-level data allows me to exploit individual controls and is more convenient for subgroup analysis.

Although equation (1.1) is simple, intuitive, and easily interpretable, several studies argue that this specification is susceptible to different time trends across states (e.g., Allegretto et al., 2011; Cengiz et al., 2019). Violation of the parallel-trend assumption would threaten causal interpretation. As a remedy, Allegretto et al. (2011) propose a way to use geographic proximity for better counterfactuals and more aggressively control state-specific trends by including state-specific linear (or sometimes higher-degree polynomial) time trends and division-time-specific fixed effects. I call this the ADR-type specification. Specifically, I estimate the following:

$$y_{ista} = \beta \ln MW_{st} + X'_{ista}\gamma + \phi_{sa} + \phi_{sa} \cdot t + \phi_{ad(s)t} + \varepsilon_{ista} \tag{1.2}$$

which includes the state-age specific linear time trend ($\phi_{sa} \cdot t$) and division-time-age specific fixed effects ($\phi_{ad(s)t}$), following Borgschulte and Cho (2020). Equations (1.1) and (1.2) closely follow Borgschulte and Cho (2020)'s specifications. However, as pointed out by Meer and West (2016), with the dynamic effects, the inclusion of state-specific trends could wash out the treatment effects; hence the estimated parameters might be attenuated. Given the evidence that minimum wage effects may gradually affect employment through the hiring channel (e.g., Gopalan et al., 2021) rather than through instantaneous changes in the employment level, this concern is important.

Adjustments in employment often take time (Hamermesh, 1993), creating a potential for dynamic effects. Because of this concern, researchers have often added lags and/or leads of minimum wages (e.g., Neumark and Wascher, 1992; Meer and West, 2016; Dube, 2019) to their models. Dube (2019) argues that lagged minimum wage variables can mitigate the problem of state-specific time trends, and the coefficient of leading minimum wage terms can be used to determine the potential threat to the parallel trend assumptions. I estimate the distributed-lag models as a complementary specification for baseline employment analysis. Specifically, I include three years of leading and four years of lagged terms for minimum wages in equations (1.1) and (1.2), as shown in equations (1.1-D) and (1.2-D).

$$y_{ista} = \sum_{\tau=-3}^{4} \beta_\tau \ln MW_{s,t-\tau} + X'_{ista}\gamma + \phi_{sa} + \phi_{ta} + \varepsilon_{ista} \tag{1.1-D}$$

$$y_{ista} = \sum_{\tau=-3}^{4} \beta_\tau \ln MW_{s,t-\tau} + X'_{ista}\gamma + \phi_{sa} + \phi_{sa} \cdot t + \phi_{ad(s)t} + \varepsilon_{ista} \tag{1.2-D}$$

The next specification is based on the event-based approach used by Cengiz et al. (2022). I call it the CDLZ-type specification.

$$y_{ista} = \sum_{\tau=-3}^{4} \beta_\tau I^\tau_{st} + X'_{ista}\gamma + \phi_{sa} + \phi_{ta} + \Omega_{st} + \varepsilon_{ista} \tag{1.3}$$

Here, I^τ_{st} is a binary indicator variable equal to 1 when the large-scale state-level minimum wage increases occur τ years relative to time t in the state s. Given the different trends in outcomes across states, it provides a more transparent way to understand the effects. Since minimum wage effects could vary depending on the size of the increases (Clemens and Strain, 2021), focusing only on large-scale increases may provide a clearer picture. However, since increases with different sizes (larger than a certain threshold) are all treated as the same, it may not be possible to use important variations for identification. Furthermore, minimum wage increases often consist of a series of increases, which complicates the estimation and interpretation.

A key issue in equation (1.3) is how to define the treatments. I define treatment as a state-level minimum wage increase of 50 cents or larger in 2019 USD, excluding minimum wage increases enacted by the federal legislature. This definition of the treatments is close to that used by Cengiz et al. (2019, 2022). This gives me 172 treatments during a 40-year data period. The average increase is approximately 10% of the previous minimum wages. I control for large federal increases and small increases by including indicator variables for small changes and large federal changes with the same time window in Ω. Federal minimum wage increases are excluded for several reasons. Since federal minimum wage increases affect many states at the same time, it is difficult to find a relevant control, especially for earlier periods.[4] As a result, focusing only on the state-level increases may provide a more transparent source of variation. Furthermore, recent econometric literature on TWFE suggests that negative weights are likely to be assigned when larger portions of units are treated (e.g., de Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). Therefore, federal minimum wage increases are more likely to create a negative weight problem.

β in equations (1.1) and (1.2) and β_τ in equation (1.3) are not comparable. To compare estimates from different specifications, for the main analysis, I convert them into elasticities (or semi-elasticities in some cases). In the case of the CDLZ-type specification, I mainly report 3-year average elasticities.[5] For the main analysis, I also present the event-study figure together with the distributed-lag models including leads and lags of the minimum wages to examine the dynamic effects.

All the aforementioned specifications are built on the TWFE framework. Recently, the econometric literature on TWFE raises concerns about the interpretation of TWFE when treatment effects and timing are heterogeneous.[6] This concern could be more acute for the CDLZ-type specification since I aggregate minimum wage increases with different magnitudes into one treatment indicator. I complement the CDLZ-type specification by using a 'stacked-event approach' used by several recent papers on minimum wage (Cengiz et al., 2019, 2022; Clemens and Strain, 2021). The idea of this approach is simple. Suppose we are studying just one large-scale state-level minimum wage in-

[4]For instance, in the case of the 1979 federal minimum wage increase, 48 states and the District of Columbia experienced large-scale minimum wage increases due to action by the federal legislature. The remaining two states, Alaska and Connecticut, increased their own minimum wages at the same time.

[5]For equations (1.1) and (1.2), when y is measured by level (such as employment), the estimated $\hat{\beta}$ can be easily converted to elasticity by calculating $\hat{\beta}/\overline{y}$, where $\overline{y}$ is the average of y. In the case of equation (1.3), when y is measured by level, I calculate the 3-year average elasticity as follows. First, $\Delta\%y$ can be calculated by $\frac{\Delta y_{post} - \Delta y_{pre}}{\overline{y_{-1}}} = \frac{\left(\frac{1}{t+1}\sum_{\tau=0}^{t=2}\hat{\beta}_\tau - \frac{1}{3}\sum_{\tau=-1}^{-3}\hat{\beta}_\tau\right)}{\overline{y_{-1}}}$, in other words, the difference in the three-year average of pretreatment versus the three-year average of posttreatment outcomes relative to the control group normalized by the average of y_{ista} at period -1. Then this can be translated into elasticity by $\Delta\%y/\Delta\%MW$, where $\Delta\%MW$ is the percent change in the minimum wage for the treated. If y is measured in a log scale, I perform the same process without dividing by $\overline{y_{-1}}$. Instead of the average of $\hat{\beta}_\tau - \hat{\beta}_{-1}$ used by Cengiz et al. (2022), I compare the posttreatment coefficients against the 3-year average of the pretreatment to reduce the weight imposed on year -1. I additionally present the event-study figure for the primary outcome which shows $\hat{\beta}_\tau - \hat{\beta}_{-1}$, as a complementary analysis.

[6]For details, see de Chaisemartin and d'Haultfoeuille (2020); Goodman-Bacon (2021); Baker et al. (2022) among others.

crease (or, one ‘event’) with a set of clean control states that do not experience large-scale state-level increases (but we can allow control states to experience federal or small increases). Analyzing this one event is not associated with any of the pitfalls addressed by recent econometric literature, since those pitfalls are caused by the combination of heterogeneous effects and heterogeneous timing of the events. Expanding this idea, I first construct an event-by-event data set containing treatment state and clean controls with an 8-year, 32-quarter window. I restrict the treatment to state-level increases that do not experience any other nominal minimum wage changes during 3 years before the treatments. This gives me 52 ‘clean’ treatments. After creating this event-by-event data set, one can append (or ‘stack’) the data set and estimate the coefficient on the treatment indicators with the full set of event-specific state and time fixed effects. Baker et al. (2022) argue that this stacked-event approach is free from negative weighting problems. Specifically, I estimate the following:

$$y_{iksta} = \sum_{\tau=-3}^{4} \beta_\tau I_{st}^{\tau} + X_{ista}'\gamma + \phi_{ska} + \phi_{tka} + \Omega_{stk} + \varepsilon_{iksta} \quad (1.3\text{-S})$$

where k is an index for events. The fixed effects are all event-specific, while the treatment indicators aggregate event-specific effects by a single parameter. I use this stacked-event approach as a complementary method for the baseline employment analysis.

1.4 Data and Descriptive Findings

The main source of information for this paper is the NBER extract of the Current Population Survey Outgoing Rotation Group (CPS-ORG) for the years 1979-2019 (National Bureau of Economic Research, Various Years). The CPS-ORG has been the primary workhorse for minimum wage researchers during the last three decades due to its relatively larger size and precise information on hourly wages, which are essential for studying minimum wage.

The key variables for this paper include information on employment, hourly wages, working hours, and state minimum wages. I define employment as a binary variable that equals 1 if individuals work for pay, excluding self-employed workers, and 0 for individuals in all other categories including unemployed, self-employed, and those who are out of the labor force. Henceforth, whenever I use the terms 'employed' and 'employment', they exclude the self-employed and workers without pay unless explicitly noted. Information on hourly wages to the penny is available only for hourly paid workers. For salaried workers, I impute the hourly wages by dividing the weekly earnings by the usual weekly hours of work. If top-coded, I multiply their hourly wages by 1.4. Hourly wages are adjusted by R-CPI-U-RS obtained from the U.S. Bureau of Labor Statistics (2022). Of the two measures of weekly working hours in the CPS-ORG (usual hours of work and hours worked in the last week), I use usual hours of work for the analysis. The minimum wage information is downloaded from Vaghul and Zipperer (2021).

Although this study's main research question is the effect on the elderly (ages 65-70), I also present results for young (ages 16-21) and prime-aged (ages 30-54) workers for comparison; the rationale for these exact ages is discussed below. Young workers are the group that has been most extensively scrutinized by researchers, possibly because the majority of them work in minimum wage jobs. On the other hand, since most prime-age workers are not minimum wage workers, they are less likely to be affected by minimum wages. Including these two groups in the scope of the analysis facilitates understanding the effects on older workers and examining the validity of each specification.

Table 1.1 presents the key variables of interest for the three age groups. The labor market outcomes of older workers are often closer to those of younger workers, rather than to those of prime-age workers. As is well known, the employment-to-population ratio is high for prime-age adults, while it is much lower for younger and older groups. Few young workers are self-employed while older workers are more likely to be self-employed. Both part-time and minimum wage workers' proportions are calculated conditional on working. Approximately half of the young workers and more than a third of older workers work in part-time jobs, while the ratio is approximately 10 percent for the prime-age group. Furthermore, approximately half of the young workers and slightly less than one-fifth of older workers earn less than 1.2 times the effective minimum wage, while that ratio drops to 8.5 percent for prime-age workers.

To demonstrate the relative importance of the minimum wage for workers of different ages,

Table 1.1 Descriptive Statistics

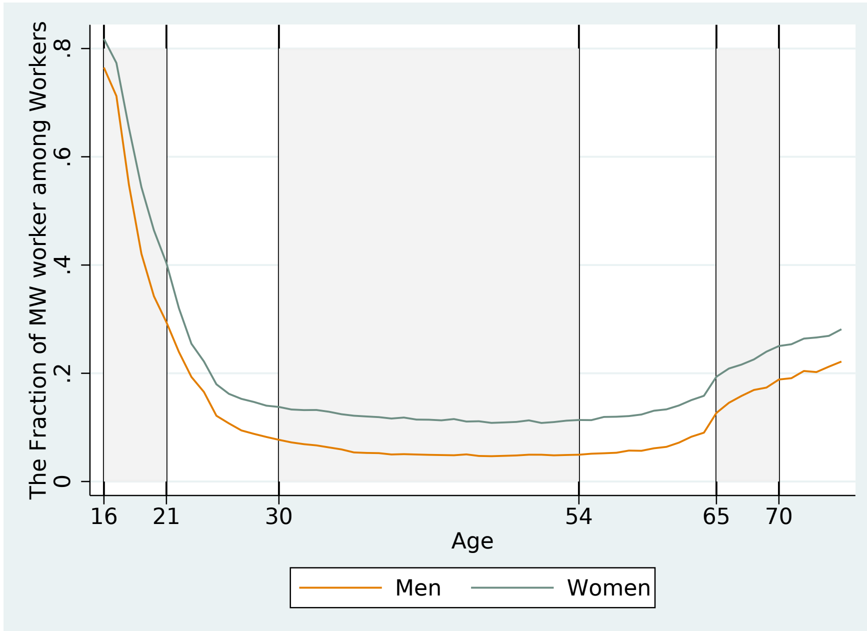
CPS-ORG, 1979-2019			
	Age 16-21	Age 30-54	Age 65-70
Panel A. Employment Variables			
Employed (Excluding Self-Employed)	0.451	0.691	0.180
Employed (Including Self-Employed)	0.460	0.784	0.240
Log Hourly Wage[b]	2.307	3.045	2.865
	(0.384)	(0.607)	(0.721)
Minimum Wage Workers[a,b]	0.511	0.085	0.185
Usual Hours of Work[b]	28.687	40.253	32.744
	(12.736)	(9.210)	(13.349)
Part-time[b]	0.513	0.108	0.372
Panel B. Demographic Variables			
Age	18.478	41.440	67.377
	(1.718)	(7.148)	(1.712)
Less than High School	0.490	0.130	0.251
High School Graduate	0.229	0.321	0.338
Some College	0.275	0.256	0.198
College Graduates	0.005	0.183	0.122
Advanced Degree	0.000	0.082	0.078
African-American	0.149	0.120	0.093
Hispanic	0.145	0.116	0.060
Observations	1428573	5770663	854730

Notes: [a] Minimum wage workers are defined as those whose hourly wages are lower than 1.2 times the effective minimum wage. Hence it includes subminimum wage workers. [b] is conditional on employment. All the results are weighted by the CPS earnings weight. Standard deviations are in parentheses. Standard deviations of the indicator variables are not included in the table.

I show the fraction of minimum wage workers among workers, defined as those whose hourly earnings are 120 percent of the minimum wage or lower, by age in Figure 1.1. This result is largely close to Figure 2.3 of Flinn (2010) and Figure 1 of Borgschulte and Cho (2020). As is widely known, a large portion of teenagers is paid the minimum wage. However, the importance of minimum wages measured by the fraction of minimum wage workers among the workforce drops quickly. The ratio becomes low and stable from approximately ages 30 to 60, but from the early sixties, the proportion of minimum wage workers starts to rise, and it ultimately exceeds 20 percent for workers in their seventies.

Table 1.1 and Figure 1.1 aggregate all the observations during the last four decades. The labor market outcomes of the elderly have seen large changes during the last four decades. Appendix

Figure 1.1 Minimum Wage Workers among Workers by Age



Notes: Source: CPS-ORG, 1979-2019. All the results are weighted by the CPS earnings weight. Minimum wage workers are defined as those whose hourly wage is 1.2 times the effective minimum wage or lower.

Figure A.2 shows the trends in the employment-to-population ratio, hourly wage, and fraction of minimum wage workers among workers for the three key age groups. In recent years, the elderly have been more likely to work, and if they work, they are more likely to earn higher wages. The median hourly wage (2019 USD) of older workers has increased dramatically from 1979 (approximately \$12) to 2019 (approximately \$19.8). It was closer to that of young workers in earlier periods, but it has caught up with the wages of prime-age workers in recent periods.

The fraction of minimum wage workers among all workers fluctuates with the real minimum wage, as is also captured by trends in the fraction of minimum wage workers among young workers. The same trend among older workers fluctuated together with that among younger workers until the mid-2000s but stabilized during the 2010s. Furthermore, although the fraction of minimum wage workers is still higher than that of prime-age workers, the gap in hourly wages almost disappears.

1.5 Effects of Minimum Wages on Employment

1.5.1 Baseline Employment Effects: Comparison across Ages and Methods

I begin with a set of age-by-age employment elasticities with respect to minimum wages using three specifications. Specifically, I estimate equations (1.1), (1.2), and (1.3) with the full set of age indicators interacting with the log of the minimum wage for equations (1.1) and (1.2) and treatment indicators for equation (1.3). Then, I convert age-specific coefficients to elasticities.

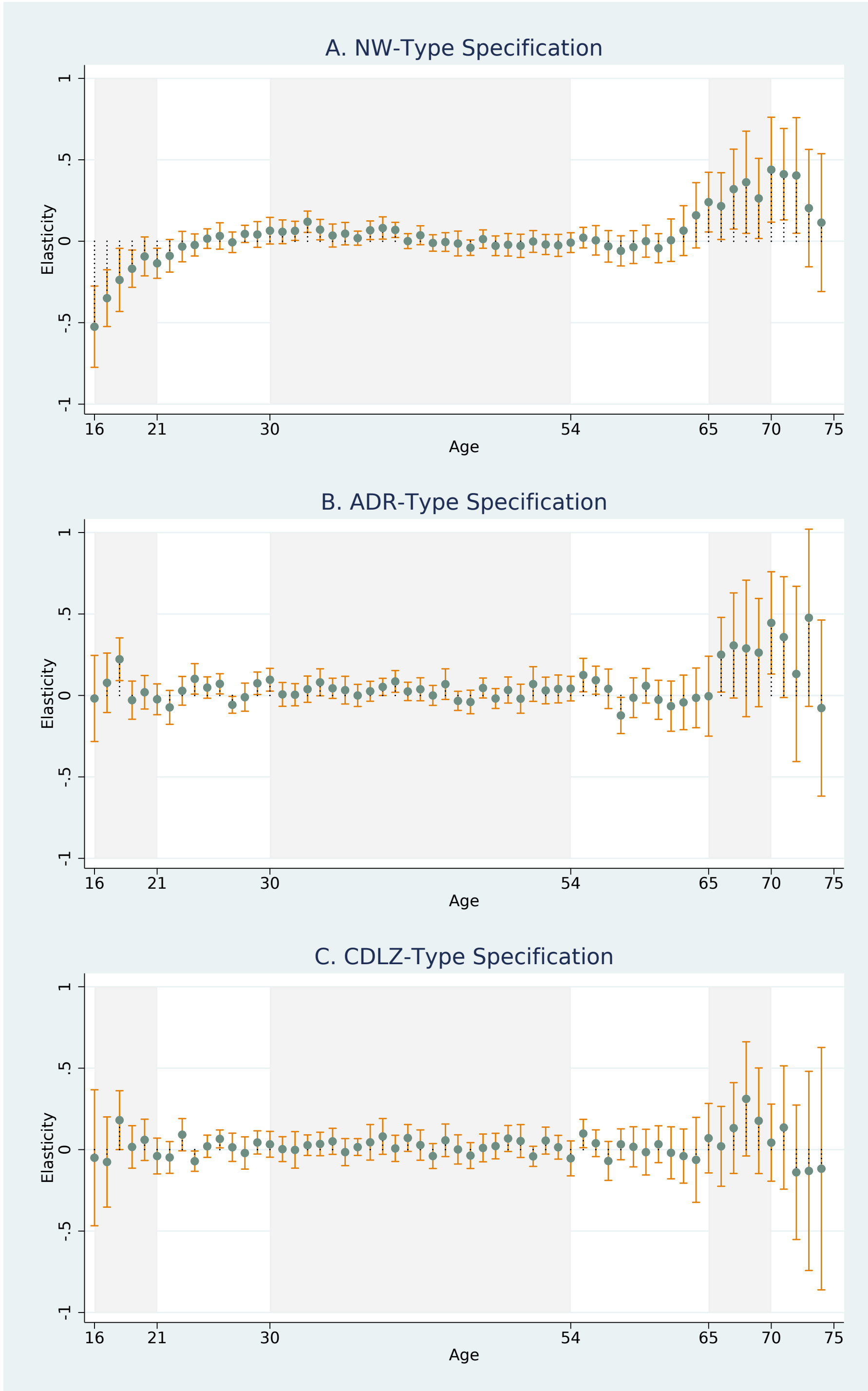
Figure 1.2 shows age-specific employment elasticities with respect to minimum wages from ages 16 to 74. As shown in Panel A, the employment effects of minimum wages are surprisingly different across ages. It shows negative employment elasticities significantly different from zero for teenagers, consistent with the literature using the log of minimum wage as a key regressor. However, as workers age, employment elasticity moves close to zero and then becomes relatively stable. The graph thus far is largely a mirror image of Figure 1.1. In contrast, from approximately age 60, employment elasticities become positive. For workers in their sixties, and especially their late sixties, the point estimates become positive, large, and generally significant, although the estimates are noisier.

Although the estimates in Panels B and C are different from those in Panel A regarding young workers, the results are similar for everyone else. Until approximately age 60, the point estimates in Panels B and C look like a horizontal line at zero with little fluctuations. However, from approximately the mid-sixties, employment elasticities deviate from zero and turn positive. This is clearer for estimates with the ADR-type specification, and less so for estimates with the CDLZ-type specification.[7]

Based on these results, most of my analysis focuses on three age groups: young workers (aged 16-21), prime-age workers (aged 30-54), and older workers (aged 65-70). My age criteria are slightly different from those in previous studies, but they are driven by similarities within each

[7]Using CPS Basic Monthly (1983-2016), Borgschulte and Cho (2020, Figure 3) provide similar age-specific employment and labor force participation elasticities (from age 50 to 70) using an empirical specification closer to the ADR-type specification. Their results do not show clear positive employment elasticities for people in their late sixties. The definition of employment in Borgschulte and Cho (2020) includes self-employed workers, while this paper does not. Later in Table 1.3, I show that the effects of the minimum wage on the self-employed and unemployed estimated by the ADR-type specification are negative (although not large). This offsets some of the positive effects on these ages.

Figure 1.2 Employment Elasticities by Age and Method



Notes: Source: CPS-ORG, 1979-2019. All the results are weighted by the CPS-ORG earnings weight. The dependent variable is a binary variable of employment (excluding self-employment). The estimated coefficients are converted to elasticities as described in section 1.3 Each dot shows point estimates of elasticity and each bar shows the 95% confidence interval. Robust standard errors are in parentheses and clustered at the state level. (Panels A and B are obtained from pooled regression of ages 16-74, while Panel C is obtained from separate age-by-age specific regression due to the lack of computing power.) See text for details.

Table 1.2 Employment Effects of Minimum Wages

	Dep var: Employed		
	Age 16-21 (N: 1,428,573)	Age 30-54 (N: 5,770,663)	Age 65-70 (N: 854,730)
	(1)	(2)	(3)
Panel A. Estimation using the *NW-type* Specification			
Elasticity w.r.t. Minimum Wage	-0.183***	0.027	0.166+
	(0.051)	(0.016)	(0.086)
Panel B. Estimation using the *ADR-type* Specification			
Elasticity w.r.t. Minimum Wage	0.026	0.031**	0.232**
	(0.037)	(0.011)	(0.069)
Panel C. Estimation using the *CDLZ-type* Specification			
3Y Average Effects			
Baseline			
Elasticity w.r.t. Minimum Wage	0.023	0.020	0.118*
	(0.067)	(0.019)	(0.058)
Stacked-Event Approach			
Elasticity w.r.t. Minimum Wage	-0.105	0.022	0.287
	(0.154)	(0.029)	(0.250)

Notes: All the results are weighted by the earnings weights (*earnwt*) in the CPS-ORG. Robust standard errors are in parentheses and clustered at the state-level. All the results include state-age specific fixed effects, time-age specific fixed effects, categorical variables of education, and race, and indicators for female and married observations. Panels A and B use the log of minimum wage for identification. Panel B includes state-age specific linear trends and division-time-age specific fixed effects from Panel A. Panel C uses an 8-year window of state-level large minimum wage increases and includes the indicator for small and federal minimum wage effects for the same window. In the stacked-event approach, some observations in clean control states are used multiple times. The total number of observations used in the stacked-event approach is 6,704,412 for column (1), 28,387,532 for column (2), and 4,196,649 for column (3). The unit of time is the quarter. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively.

group.[8] Each focal age range is shaded in Figures 1.1 and 1.2.

Table 1.2 presents the baseline employment effects of minimum wages for these three focal age groups. In Panel C, I report 3-year average elasticities from the baseline CDLZ-type specification and stacked-event approach. Appendix A.2 contains expanded tables including longer run effects and other complementary results. Columns (1), (2), and (3) show the effects on the young, prime-age, and elderly, respectively.

As widely found elsewhere and shown in Figure 1.2, in column (1), the point estimates of employment effects on young workers fall roughly within the range [-0.2, 0], and the NW-type

[8] Both Borgschulte and Cho (2020) and Hampton and Totty (2021) focus on ages 62-70, and Cengiz et al. (2022) study ages 60-70.

specification tends to produce more negative estimates.[9] As expected, the employment elasticities of prime-age workers are very close to zero, and all the estimates fall within a very narrow range [-0.05, 0.05]. Although some estimates are statistically significant, they are small and very close in magnitude to estimates from other specifications. The elasticities in this range are not economically important. The results thus far successfully replicate the stylized results in the literature.

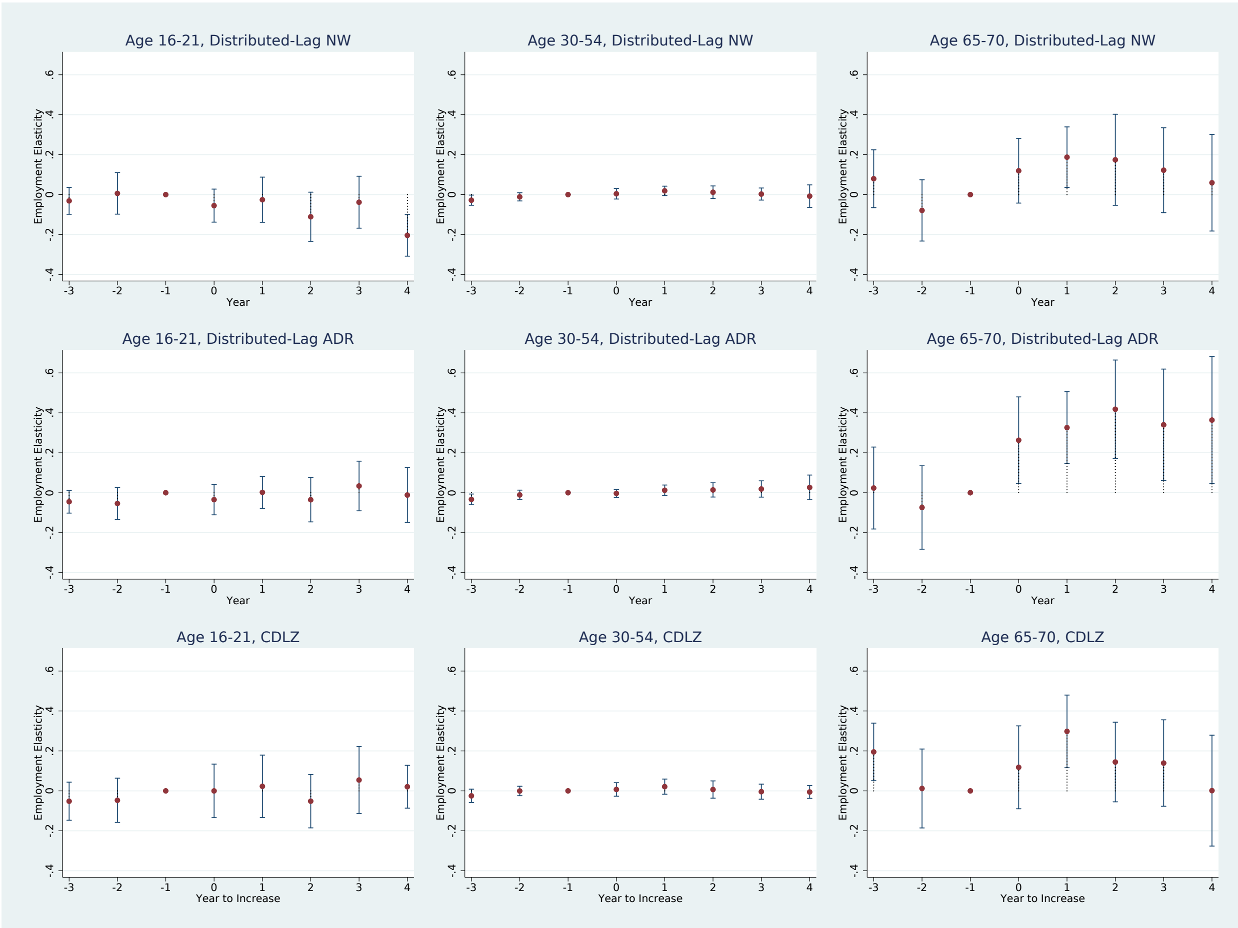
Next, I turn to the results in column (3) for the minimum wage effects on employment for the elderly. Unlike young and prime-age workers, all the specifications in column (3) report positive employment elasticities with respect to minimum wage. The point estimates are small and fall roughly within the interval [0.1, 0.3], and the majority are statistically significantly different from zero at the conventional level. This implies that if the minimum wage increases by 10 percent, the employment-to-population ratio increases by approximately 1 to 3 percent, or 0.2 to 0.5 percentage points. The effects are larger in Panel B, where estimates are more likely to be attenuated due to the inclusion of state-specific linear trends (Meer and West, 2016). The results using the stacked-event approach are aligned with estimates using other specifications, suggesting that positive estimates are not driven by negative weights or other pitfalls of TWFE.

Although the magnitudes of employment elasticities are not large, these estimates are larger than those in the literature. By combining specifications similar to those in Panels A and B and the CPS Basic Monthly (1983-2016), Borgschulte and Cho (2020, Table 3) report employment elasticities on the elderly (age 62-70) in the range [0, 0.15], depending on the specification and definition of employment.[10] With the same age range, 62-70, Hampton and Totty (2021) report employment elasticity of approximately 0.2 using workers earning near-minimum wages. Since minimum wage workers constitute a small portion of the older workforce, the implied overall employment elasticities are much smaller than 0.2. By focusing on the older ages, I can more clearly demonstrate the positive employment effects on the elderly. Furthermore, by adding new results using the event-based and stacked-event approaches, I can address that these results are not

[9]Wolfson and Belman (2019) and Manning (2021) provide a more detailed discussion of the elasticities in the literature and their sensitivities to specifications.

[10]Note that their 'employment' includes the self-employed, and their 'wage and salary employed' is identical to my 'employment' variable.

Figure 1.3 Dynamic Effects on Employment



Notes: The first row shows the results using the distributed-lag NW-type specification, and the second row shows estimates from the distributed-lag ADR-type specification. Each dot shows employment elasticities calculated by the joint sum of the coefficients up to that term divided by the sample average, normalized to make the -1 year term to zero. The third row shows the estimates from the standard event-based approach calculated by $\left((\beta_\tau - \beta_{-1})/\overline{y_{-1}}\right) \cdot (1/\Delta MW)$. All the estimates include the same set of covariates as in Table 1.2.

driven by negative weights or other pitfalls of the TWFE specification.

Figure 1.3 explores the dynamic effects and pretrend by using distributed-lag models and event-based approaches. In the distributed-lag models, I present the cumulative elasticities calculated by the joint sum of the coefficients from the three-year leading terms up to that year divided by the sample average, following Dube (2019). The elasticities are further normalized to make the -1 year term to zero.[11]

Regarding the elderly, the three methods produce similar results. Positive effects on employment grow in the short run (approximately 2 years) but disappear in the medium run. The two-year

[11] For instance, the elasticities in the year 2 are calculated by $(\sum_{t=-3}^{2} \hat{\beta}_t - \sum_{t=-3}^{-1} \hat{\beta}_t)/\overline{y} = (\hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_2)/\overline{y}$.

cumulative employment elasticities in the first two rows and employment elasticities to large-scale increases a year after the events are in the range of 0.2 to 0.4, compared to the year -1, and they are statistically significant at the conventional level. After that, the positive effects gradually disappear, although they persist longer in the ADR-type specifications. Figure 1.3 also does not show any noticeable pretrends, except for the estimate on the 3 years prior to the increase from the CDLZ-type specification.[12]

I present additional complementary analysis in Appendices A.3 and A.4. Appendix A.3 shows the effects on other important labor market outcomes: weekly working hours and hourly wages. Conditional on working, I do not find any detectable effects on the working hours and hourly wages of the elderly. Appendix A.4 examines various issues including event-by-event estimates, heterogeneity by employment status in the previous year, and heterogeneity by education. Although the effects are often noisy due to the smaller sample size, in general, I do not find evidence of negative employment effects on the elderly.

In summary, although a larger fraction of the elderly, especially less-educated workers, earn the minimum wage, there is no evidence of disemployment effects as suggested by standard neoclassical theory, consistent with previous studies (Borgschulte and Cho, 2020; Hampton and Totty, 2021; Cengiz et al., 2022). Instead, this subsection provides evidence of small, positive effects of the minimum wage on elderly employment for those in their late sixties. These positive effects are robust to a variety of specifications. Given the standard labor demand theory, which implies negative labor demand elasticities with respect to wages, these results require further investigation.

1.5.2 Employment Effects across the Wage Distribution

Another approach to examining the employment effects of the minimum wage is to examine the effects on wage distribution. This has become a useful tool for the anatomy of employment effects in recent minimum wage studies (e.g., Cengiz et al., 2019; Derenoncourt and Montialoux, 2021; Forsythe, 2022). I begin by applying the bunching approach proposed by Cengiz et al. (2019).

[12]Note that the employment elasticities in Table 1.2 are calculated by the difference between the 3-year average pre-period and post-period, instead of using a year before the treatment as a base. The event-study figure shows that it is a conservative estimate.

This method is based on the analysis using wage bin-by-bin aggregate and estimates the effects of minimum wage increases on the number of workers in the wage bins relative to the new minimum. A detailed discussion of this method and regression specification is provided in Appendix A.5.

Although this method provides a powerful tool to characterize the effects along the wage distribution, it is highly data-demanding. The basic unit of the bunching approach is the state-quarter-bin level aggregate. Subgroups such as the elderly often lack sufficient numbers of observations. To overcome this problem and compare the results across specifications, I complement the analysis by using a simpler alternative approach to estimating the effects on the cumulative distribution. Specifically, I use a set of binary indicator variables, $I(\text{Employed and hourly wage } \leq c)$ with a different set of criteria. For c, I specify 10 to 150 percent of the median wage with a 10 percent interval for c. The average of minimum-to-median ratio is 0.45, ranging from 0.287 to 0.795. For 90 percent of the population, the minimum-to-median ratio falls in the range of 0.363 to 0.559. (See Appendix Figure A.3 for the distribution of the ratio.) The average median hourly wage is approximately \$18. It is calculated from all workers including part-time or part-year workers and non-prime-wage workers.

The primary focus of the analysis in this subsection is to examine who in the wage distribution responds to minimum wage changes. For that purpose, instead of rescaling the coefficients by different values across c to convert them into elasticities, I normalize the coefficients with the same ratio.[13] These normalized semi-elasticities show the change in the probability of working with wages lower than c relative to employment.[14]

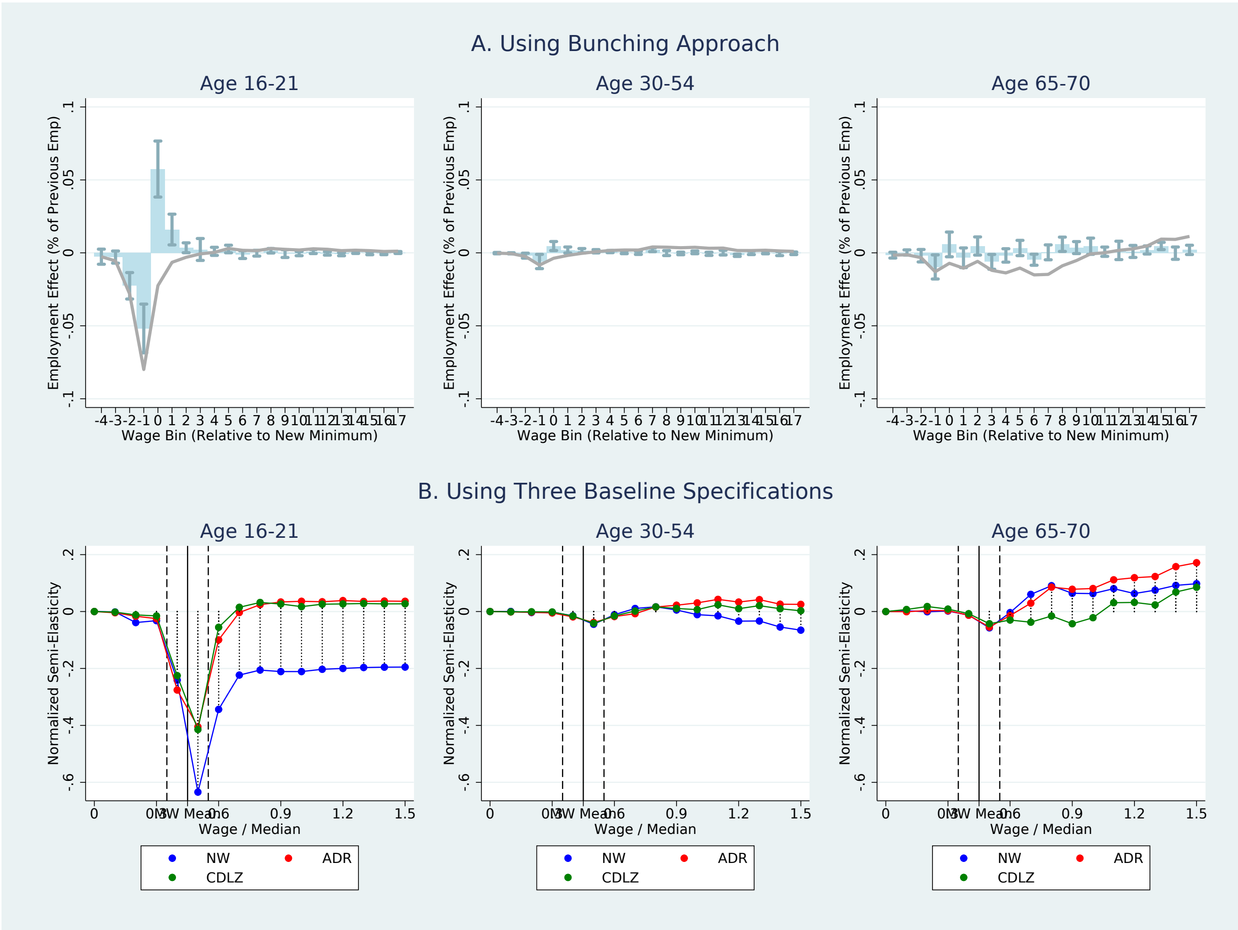
Panel A in Figure 1.4 shows the results using the bunching approach. Here, the wage bins are numbered relative to the new minimum.[15] The gray lines show the cumulative effects up to that wage bin. The definition of the treatment is identical to that in equation (1.3). Following the

[13]This is the baseline average employment-to-population ratio of the age group for the NW- and ADR-type specifications and the average employment-to-population ratio of the treated state before the treatment for the CDLZ-type specification.

[14]For example, the normalized effect at a certain c is 0.1, which means that a 10 percent increase in minimum wage increases the number of workers below c by 1 percent of the total employment.

[15]Specifically, 0 indicates the 25-cent wage bin containing the new minimum wage and the three bins above it (or, ‘just-above-minimum’ in the example), and the wage bin -1 implies four 25-cent wage bins below the new minimum.

Figure 1.4 Effect of Minimum Wage along the Wage Distribution



Notes: In Panel A, each bar shows the employment effects on the wage bin normalized by the employment-to-population ratio of each age group before the treatment. The gray lines in Panel A show the cumulative effects up to that wage bin. In Panel B, the blue, green, and red dots show the estimates for the young (16-21), prime-age (30-54), and elderly (65-70), respectively. See text for details. The black solid line shows the average minimum wage-median wage ratio (approximately 45 percent of the median wage), and the black dotted lines show the 5th and 95th percentiles of the minimum to median ratio, respectively.

original method in Cengiz et al. (2019), this shows the effects relative to the previous employment level, not elasticities. Given the average sizes of minimum wage increases, multiplying by 10 approximates the employment elasticities. Appendix A.2 presents larger graphs for each panel. Panel B in Figure 1.4 shows the effects on the number of workers below a certain wage using three baseline specifications. Intuitively, these lines are analogous to the gray lines in Panel A.

The left panels show the large negative effects on the lower part of the wage distribution of the young workers for all the specifications. The magnitude of the missing jobs (negative effects below the new minimum) is smaller in Panel B, since 50 percent of the median wage is higher than the minimum wage in some cases. Within Panel B, the negative effects are larger in the NW-type specification. These large negative effects are offset by positive effects on the workers above the minimum (Panel A) or 50 percent of the median (Panel B). In Panel B, the size of the positive effects on the number of workers above 50 percent of the median is similar across the specifications. Therefore, the long-lasting debates over teenager employment effects boil down to measuring the effects on employment below the new minima. In the case of prime-age workers, the magnitude of the missing jobs is much smaller than that for young workers, and it is offset by the positive effects above the minimum wage. For all three specifications, the effects on the number of workers below 70 percent of the median are virtually zero, which implies the workers are just shifted. However, in the case of the NW-type specification, the negative effects grow for higher values of c, which raises some concerns regarding the validity of the specification.[16] Except for that, for both young and prime-age workers, I do not find any detectable effects on the region above 70 percent of the median wage. Seventy percent of the median is approximately 4 dollars above the average minimum wage; hence, workers below 70 percent of the median can be characterized as near-minimum wage workers.

Once again, the results differ for the elderly. All the specifications show negative effects on the lower part of the distribution that are larger than those on prime-age workers. Focusing on near-minimum wages (70 percent of the median or up to 5 dollars above the new minimum), the cumulative effects are much smaller than the total positive employment effects. In the NW- and ADR-type specifications of Panel B, the minimum wage has positive effects on the number of workers below 70 percent of the median, but these are only approximately half of the total employment effects in section 1.5.1. In Panel A and the CDLZ-type specification of Panel B, the effects on near-minimum wage workers are even negative, and the positive effects come instead from workers above this region. For all the specifications, however, the effects up to 17 dollars above the new minimum (roughly \$25) or 130 percent of the median hourly wage (roughly \$23.5) become closer to the total employment effects in section 1.5.1. This shows that the responses of

[16]Note that the overall effects on the prime-age workers are zero. This means that these negative effects are offset by positive effects on high-wage workers.

near-minimum wage workers cannot explain all the positive effects identified in section 1.5.1 and that the positive effects extend to workers who are paid well above the minimum wage.

Overall, this subsection shows that positive employment effects on older populations are not limited to near-minimum wage workers but extend more broadly to low- or even medium-wage workers. The effects on near-minimum wage workers are not sufficient to explain the overall positive employment effects on the elderly, and this may explain the difference between my estimates and those in Hampton and Totty (2021) or Cengiz et al. (2022). Hampton and Totty (2021) analyze near-minimum wage workers, and Cengiz et al. (2022) examines the effects on individuals who are more likely to work in minimum wage jobs.[17] This feature raises a question. These wage ranges (70 to 130 percent of the median-wage) seem to be too high to be affected by spillover effects.

1.5.3 Where Do the Additional Employees Come From? Effects on Various Labor Market Status

This subsection examines how the minimum wage affects labor market status: employment, unemployment, self-employment, and being out of the labor force. To compare the exact magnitudes, I present semi-elasticities. Since each category is mutually exclusive, the coefficients should sum up to zero. Appendix A.2 contains a table with usual elasticities. Focusing on the older population, Table 1.3 reveals that an increase in employment is associated with a decrease in the fraction of the elderly who are out of the labor force, while the effects are not precisely estimated. The effects on unemployed or self-employed workers are not clear. This shows that a higher minimum wage attracts workers who are out of the labor force, suggesting that higher minimum wages increase the labor supply.

1.6 Why Positive?

This section provides an economic explanation for the empirical findings in the last section. I find small, positive employment effects of minimum wages on older workers in line with recent papers (Borgschulte and Cho, 2020; Hampton and Totty, 2021). Existing papers often interpret

[17] Hampton and Totty (2021) also report null employment effects on workers who earned 5-10 dollars above the minimum wage before the increases. The slope of the effects in Panel B is relatively flatter, which may lead to insignificant estimates in this range.

Table 1.3 Effects of Minimum Wage on Various Labor Force Status

	Employed (E) (1)	Unemployed (U) (2)	Self-Employed (S) (3)	Out of the Labor Force (OLF) (4)
Panel A. Age 16-21 (N: 1,428,573)				
Estimation using the *NW-type* Specification				
Semi-Elasticity	-0.083***	-0.001	0.000	0.083**
	(0.023)	(0.011)	(0.002)	(0.029)
Estimation using the *ADR-type* Specification				
Semi-Elasticity	0.012	-0.010	-0.004	0.002
	(0.017)	(0.010)	(0.003)	(0.015)
Estimation using the *CDLZ-type* Specification				
3Y Average Effects				
Semi-Elasticity	0.010	0.002	0.006*	-0.018
	(0.027)	(0.012)	(0.003)	(0.021)
Panel B. Age 30-54 (N: 5,770,663)				
Estimation using the *NW-type* Specification				
Semi-Elasticity	0.019	-0.007	0.005	-0.017
	(0.011)	(0.006)	(0.006)	(0.010)
Estimation using the *ADR-type* Specification				
Semi-Elasticity	0.022**	-0.009	-0.000	-0.013*
	(0.008)	(0.006)	(0.004)	(0.005)
Estimation using the *CDLZ-type* Specification				
3Y Average Effects				
Semi-Elasticity	0.014	0.002	-0.016**	0.000
	(0.013)	(0.005)	(0.005)	(0.007)
Panel C. Age 65-70 (N: 854,730)				
Estimation using the *NW-type* Specification				
Semi-Elasticity	0.030+	-0.000	0.033***	-0.063**
	(0.016)	(0.003)	(0.009)	(0.022)
Estimation using the *ADR-type* Specification				
Semi-Elasticity	0.042**	-0.004	-0.015	-0.024
	(0.012)	(0.004)	(0.011)	(0.014)
Estimation using the *CDLZ-type* Specification				
3Y Average Effects				
Semi-Elasticity	0.025*	-0.004	0.000	-0.021
	(0.012)	(0.004)	(0.011)	(0.018)

Notes: Robust standard errors are in parentheses and clustered at the state level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. See notes to Table 1.2 for more details.

the results as a sign of delayed retirement and increased labor supply that come from wage effects. However, several important questions remain. First, the analysis of the wage distribution suggests that at least some portion of the positive effects come from workers whose hourly wage may not be directly determined by the minimum wage. This calls into question the explanatory power of the labor supply responses. Second, higher minimum wages create even larger incentives for young or other minimum wage workers, while they generally do not show positive responses. Third, under the binding minimum wage in standard neoclassical economics, employment outcomes are more likely to be determined by labor demand than by movement along the supply curve.

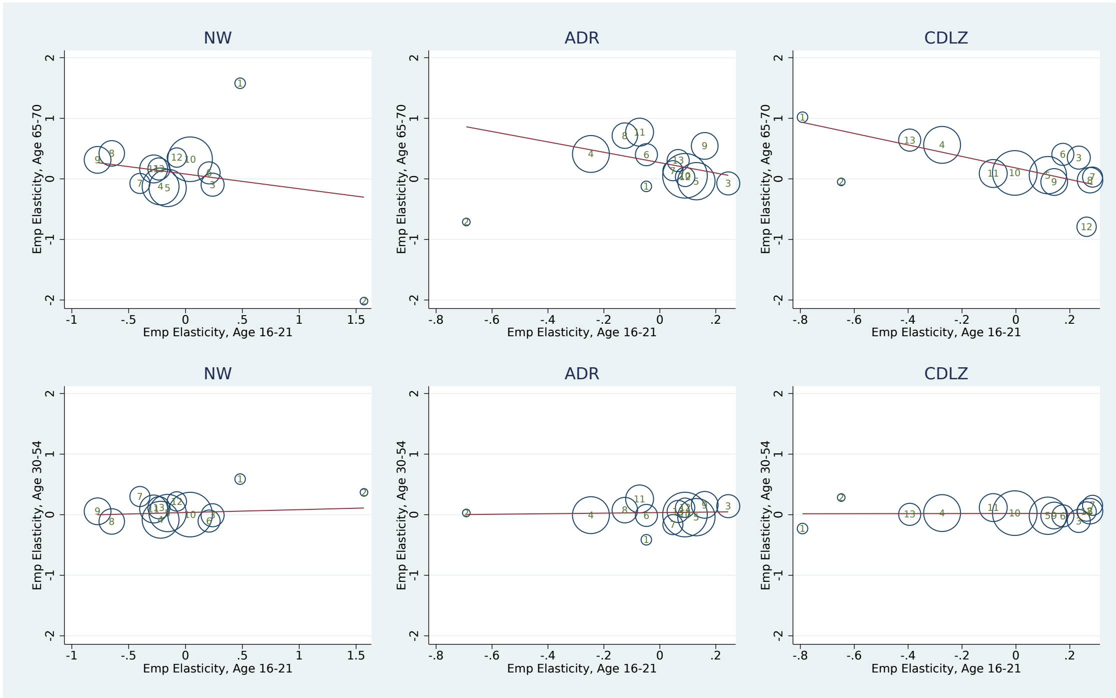
This section seeks an alternative, possibly complementary explanation for the positive employment effects. The key idea is as follows. The minimum wage is a wage rate for the least-skilled workers such as teenagers. A higher minimum wage increases the relative price of the least-skilled workers, and employers may shift their demand toward better-skilled workers. With this labor-labor substitution, effects are not necessarily limited to the least skilled population; rather, small effects can be found among a broader class of workers. This section focuses on the substitution between young and older workers.

1.6.1 Industry-Level Analysis

I begin with a descriptive examination of the possibility of labor-labor substitution based on an industry-level approach. Specifically, I classify industries into 13 consistent categories based on the method proposed by Pollard (2019). Detailed information on this classification is provided in Appendix A.1.1. Using the consistent industry information at hand, I examine the industry-specific employment elasticities with respect to minimum wages for the three age groups. After estimating the industry-age-specific employment elasticities, I plot the prime-age and elderly industry-level employment elasticities against those of the young. If minimum wage causes labor-labor substitution between workers of different ages, the relationship will be negative. In contrast, if everyone in the same industry is affected in the same manner regardless of their ages, the relationships will be positive.

The results are shown in Figure 1.5. The first row shows the relationship between industry-

Figure 1.5 Industry-Level Analysis



Notes: Linear fitted lines are weighted by the fraction of workers employed in each industry among the total workforce. The size of each circle reflects the size of industry.

level employment elasticities with respect to minimum wages of the young and elderly, and the bottom row shows the same relationship between the young and prime-age. The linear fitted lines are weighted by the fraction of workers employed in each industry among the total workforce. The top row shows a negative relationship between young and elderly employment elasticities, implying that the number of older workers increased in industries that reduced the number of young workers. This relationship is especially clear in the ADR- and CDLZ-type specification with a slope of approximately negative 1 and becomes weaker in the NW-type specification. In contrast, the employment effects on young and prime-age workers are not correlated, primarily due to the null effects on the prime-age workers.

However, this analysis has several limitations. First, industry-level estimates are highly imprecise and noisy. Furthermore, different methods often identify different industries as positively or negatively affected industries. This problem is especially severe for some industries that do not employ many younger or older workers (e.g., agriculture (industry 1) or mining (industry 2)). Despite these limitations, industry-level analysis can be taken as suggestive evidence of labor-labor substitution between young and older workers.

1.6.2 Estimating the Elasticity of Substitution Using Minimum Wage Changes

To more formally examine the possibility of labor-labor substitution, this subsection estimates the elasticity of substitution using a simple nested-CES production function framework. Labor economists have long been interested in estimating the elasticity of substitution between workers with different skills and demographics, and the nested-CES framework has been a powerful weapon in labor economists' arsenal (e.g., Welch, 1979; Katz and Murphy, 1992; Card and Lemieux, 2001; Borjas, 2003; Ottaviano and Peri, 2012). It greatly simplifies the nature of substitution without losing too much detail.

Due to the lack of observations especially for older workers, I impose the simplest possible structure to determine the elasticity of substitution between groups. Consider the following state or state-industry-level aggregate production function using capital and a variety of labor inputs aggregated into L:

$$Y = F(L_y, L_p, L_o, K) = AK^{\alpha} L^{1-\alpha}$$

L_y, L_p and L_o are the employment of the young, prime-age, and older workers, respectively. Aggregated labor input, L, is a CES aggregate of prime-age and non-prime-age workers. In other words:

$$L = \left[\theta_{np} L_{np}^{\frac{\sigma_p - 1}{\sigma_p}} + (1 - \theta_{np}) L_p^{\frac{\sigma_p - 1}{\sigma_p}}\right]^{\frac{\sigma_p}{\sigma_p - 1}}$$

and the labor of non-prime-age workers, L_{np} is again a CES aggregate of young and older workers:

$$L_{np} = \left[\theta L_y^{\frac{\sigma - 1}{\sigma}} + (1 - \theta) L_o^{\frac{\sigma - 1}{\sigma}}\right]^{\frac{\sigma}{\sigma - 1}}$$

The CES production function imposes the same rate of substitution between factors in the same layer. If I put young, prime-age, and older workers in the same layer, I impose the restriction of an identical rate of substitution between different age groups. Given the analysis in the previous subsection, this restriction may not be plausible. To avoid this problem, I impose an additional layer of non-prime and prime-age workers.

In a competitive market equilibrium, wage equals the value of the marginal productivity of labor. It can be derived that

$$\ln(L_y/L_o) = \sigma \ln(\theta/(1-\theta)) - \sigma \ln(w_y/w_o) \tag{1.4}$$

In other words, the relationship between relative wages and relative employment reveals the elasticity of substitution, holding relative productivity constant. (see Appendix A.6 for details). For the estimation, I use the following specification:

$$\ln(L_y/L_o)_{st} = -\sigma \ln(w_y/w_o)_{st} + \phi_s + \phi_t + \varepsilon_{st} \tag{1.5}$$

where ϕ_s and ϕ_t are state and time fixed effects, respectively.

An econometric challenge in consistently estimating σ from equation (5) is controlling for unobservable relative productivity. Researchers can observe relative wages and relative employment, which are equilibrium prices and quantities. Positive productivity shock for a certain group increases labor demand for that group, which increases both wages and employment. If the set of fixed effects in equation (5) can control for relative productivity, the OLS estimate can consistently estimate σ. Otherwise, equation (5) has an omitted variable bias problem which creates a positive bias in the estimates.

The literature often relies on the assumption that relative productivity is time-invariant or follows a linear trend (e.g., Katz and Murphy, 1992; Ottaviano and Peri, 2012 for the elasticity of substitution between domestic workers and immigrants). Other studies use supply shifters such as immigration (e.g., Borjas, 2003; Ottaviano and Peri, 2012 for upper levels) as instrumental variables for relative supply, setting the relative wage as a dependent variable. The last method can be understood as estimating the labor demand curve using shifts in the labor supply.

In this paper, I take a different approach, utilizing an instrumental variable of the relative wage ratio in equation (1.5). I propose an instrumental variable exploiting the change in minimum wages in the spirit of simulated eligibility instruments (e.g, Currie and Gruber, 1996 for Medicaid expansion; Gruber and Saez, 2002 for the effects of taxes). I calculate the simulated wage by applying future minimum wages to the wage distribution of the base year (1979). To calculate the simulated average wage with future minimum wages, I use the wage distribution of the comparison year below the minimum wage.[18] Specifically, I calculate the simulated wage as follows. First, I calculate the conditional average above the threshold using the base year and below the threshold using the comparison year. Second, I calculate the weighted sum of these two conditional averages, using the fraction of workers above and below the threshold in the base year as weights. This method is in line with the "tail pasting" method of DiNardo et al. (1996) with simplification. A detailed discussion of this method is provided in Appendix A.7.

The key identifying assumption of this simulated wage instrument is that it is uncorrelated with future changes in relative productivity or other unobservable components. Since I only exploit minimum wage changes to simulate the average wage, future productivity shock is unlikely to affect the simulated wage instrument. Using this simulated wage instrumental variable, I estimate equation (1.5) using 2SLS.

One important issue in estimating equation (1.5) is the choice of the unit of the labor market. The literature tends to focus on the national-market approach (e.g., Card and Lemieux, 2001; Borjas, 2003; Ottaviano and Peri, 2012), with the idea that internal migration will equate the regional wages of workers with a certain set of skills. However, given the evidence showing that the wages of teenagers and other young workers are highly affected by minimum wages, young workers' wages are more likely to be determined locally than nationally. Furthermore, the elderly are the population with the lower internal migration rate (Benetsky et al., 2015). Therefore, for the analysis related to non-prime-age workers, a regional market approach may be appropriate. Based on this concern, I use the state as the unit of analysis.

Table 1.4 shows the estimated elasticity of substitution between young workers and workers in other age groups. Unlike analyses thus far, I use the year instead of the quarter for the unit of time

[18] In practice I use 1.2 times the maximum minimum wage of the base and comparison years for the threshold.

Table 1.4 Elasticity of Substitution

	Dep: ln Rel Emp					
	Young and Elderly Coefficient = $-\sigma$			Young and Prime-Age		
	OLS	Simulated IV	Simulated IV	OLS	Simulated IV	Simulated IV
	(1)	(2)	(3)	(4)	(5)	(6)
ln Rel Wage	-0.030	-0.637*	-0.649*	0.167*	-0.025	0.010
	(0.033)	(0.249)	(0.294)	(0.071)	(0.199)	(0.138)
First-stage	-	1.275	1.058	-	1.801	1.899
First-stage F	-	24.61	21.15	-	209.10	198.72
Obs	2,091	2,091	2,091	2,091	2,091	2,091
State-FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
State-Linear Trend	N	N	Y	N	N	Y

Notes: Each cell is weighted by the total number of workers at the state-year level. Robust standard errors are clustered at the state level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively.

to maximize the number of observations in each cell. The coefficients in columns (1) through (3) imply a negative of the elasticity of substitution between young and older workers. The larger (in absolute terms) this value is, the more substitutable these groups are. For comparison, I additionally use the coefficients estimated from the same equation using young and prime-age workers. Since I place the prime-age and non-prime-age workers in different layers, the interpretation of the coefficients can differ from the elasticity of substitution in the production function (see Appendix A.6 for details). Note that if the estimated coefficients between the young and prime-age workers are similar or larger in absolute terms, the nesting structure is no longer plausible. Hence, it can be used to evaluate the structure.

In columns (1) and (4), the OLS estimates are close to zero or even positive, suggesting that workers of different ages may not be substitutes in production. In contrast, the columns using instrumental variables show that OLS estimates are positively biased. The 2SLS estimate for young and prime-age workers is close to zero, while that for young and older workers shows a small degree of substitution. The results are robust to the inclusion of state-specific linear time trends. The first stage is very strong for the relationship between the young and prime-age workers, and weaker but still stronger than the conventional threshold in the weak-IV test in columns (2) and (3). This suggests that young and older workers are substitutes for each other, but that the degree of substitution is not high.

Table 1.4 uses state-year level aggregates. The role of the minimum wage, however, varies greatly across industries. In some industries, it may not affect both relative wages and employment, while in others, it may play a substantial role in determining both. To use this industry-level variation, in Table 1.5, I use state-year-industry level aggregates, instead of state-year level aggregates. One challenge in this analysis is the insufficient number of observations. Simulated instruments work well when there are enough observations both below and above the minimum wage in both the base and comparison year. To minimize the cells with too few observations, I focus on two industry groups: Leisure and Hospitality and the Wholesale and Retail Trade. These two industries have been studied extensively in the minimum wage literature. All other industries are classified into just one group. I further exclude some cells if they lack observations either above or below the minimum wages.

Table 1.5 shows the results. Panels A and C use state-year level aggregates, and Panels B and D use 5-year aggregates. A 5-year aggregate has more observations in each cell, and it may be complicated by multiple minimum wage changes during the 5-year interval. Columns (1) and (3) show the OLS estimates.

Columns (1) and (2) use state, time, and industry-specific fixed effects. In column (2), all the estimates are imprecisely estimated. In columns (3) through (5), I add state-industry-specific fixed effects and time-industry-specific fixed effects. Since I am using the state-industry level aggregates, state-industry-specific fixed effects may be a better choice of unit-specific fixed effects. Therefore, these are my preferred estimates. In column (4), the estimated coefficients are positive for the young and prime-age groups and negative for the young and elderly groups. Since the estimated coefficients in Panels A and B imply the negative of the elasticity of substitution, this suggests that young and older workers are substitutable. Adding state-industry-specific fixed effects in column (5) does not greatly change the results. In sum, the estimated elasticity of substitution is approximately 0.3 to 0.6, which is close to the estimates in Table 1.4.

Overall, using a nested-CES framework, I estimate the small degree of elasticity of substitution

Table 1.5 Elasticity of Substitution, Industry Level

	Dep: ln Rel Emp				
	OLS	Simulated IV	OLS	Simulated IV	Simulated IV
	(1)	(2)	(3)	(4)	(5)
	Panel A. The Young and Elderly				
ln Rel Wage	0.097**	-0.279	0.038	-0.299*	-0.366*
	(0.032)	(0.840)	(0.027)	(0.142)	(0.156)
First-stage	-	0.166	-	0.919	0.825
First-stage F	-	21.98	-	51.20	56.40
Obs	4,749	4,749	4,748	4,748	4,748
	Panel B. The Young and Elderly (5-Year Aggregate)				
ln Rel Wage	0.101	0.467	-0.077	-0.410*	-0.681*
	(0.075)	(0.532)	(0.054)	(0.182)	(0.271)
First-stage	-	0.264	-	1.383	1.116
First-stage F	-	33.05	-	32.57	19.28
Obs	1,189	1,189	1,189	1,189	1,189
	Panel C. The Young and Prime-Age				
ln Rel Wage	0.195**	1.356	0.147*	0.203	0.151
	(0.063)	(1.034)	(0.056)	(0.156)	(0.106)
First-stage	-	0.331	-	1.427	1.422
First-stage F	-	44.29	-	259.56	139.84
Obs	6,185	6,185	6,185	6,185	6,185
	Panel D. The Young and Prime-Age (5-Year Aggregate)				
ln Rel Wage	0.397**	0.645	0.333**	0.541+	0.118
	(0.117)	(0.437)	(0.115)	(0.304)	(0.194)
First-stage	-	0.691	-	1.456	1.598
First-stage F	-	108.49	-	93.99	88.07
Obs	1,224	1,224	1,224	1,224	1,224
State-FE	Y	Y	N	N	N
Time FE	Y	Y	N	N	N
Ind FE	Y	Y	N	N	N
State-Ind-FE	N	N	Y	Y	Y
Time-Ind-FE	N	N	Y	Y	Y
State-Ind Linear Trend	N	N	N	N	Y

Notes: Each cell is weighted by the total number of workers at the state-time-industry level. Robust standard errors are clustered at the state level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively.

between young and older workers. However, the magnitude of the estimated coefficients is fairly small. As a comparison, in Katz and Murphy (1992), the elasticity of substitution between better- and less-educated prime-age workers is approximately 1.4. In Ottaviano and Peri (2012), the elasticity of substitution between younger (1-20 years of potential experience) and older (21-40 years of potential experience) prime-age workers with the same education level is approximately 3 to 4.

Several potential issues may affect these lower estimates. First, I use aggregates of workers with any education, while better-educated older workers may not be a substitute for the young workers. Second, using the local market may provide attenuated estimates if wages across local markets are equalized. Although the migration rate of the elderly is generally low, their wages might be affected by the migration of prime-age workers or the location choice of the employers. In Borjas (2003) in the immigration context, wage effects using the local labor market approach are approximately one-third of the estimated effects using the national market approach. Furthermore, workers may be able to move across industries. Industries employing young workers often require minimal training or formal education. This can also explain the lower elasticity of substitution in Table 1.5.

One important question remains. Both industry-level analysis in section 1.6.1 and direct estimation of elasticity of substitution in section 1.6.2 suggest that although older workers are substitutes for young workers, prime-age are not. Immediate question is to ask why. There are several possible explanations. First, compared to prime-age workers, older workers tend to work in a relatively similar set of industries to young workers. In Appendix A.8, I measure the similarity of industry distribution between groups of workers using the index of congruence proposed by Welch (1999). This analysis shows that, compared to the prime-age workers, the industry distribution of older workers is relatively similar to that of young workers. This partially explains why older workers are a closer substitute. Furthermore, the labor market behaviors of older workers are relatively similar to those of young workers. As shown in Table 1.1, labor market behaviors of young workers are characterized by shorter weekly working hours and a higher portion of part-time workers. Focusing on that characteristics, older workers might be a natural candidate for substitutes in production.

1.7 Discussion and Conclusion

This paper examines the effects of minimum wages on the labor market outcomes of older workers and explains the effects based on labor-labor substitution. I find positive effects on employment of the older population aged 65-70 and these positive effects are not limited to minimum wage workers but a broader class of workers are affected. Further analysis suggests that these positive effects may come from demand shifted from the young minimum wage workers, although the degree of substitution is not high. This pattern of labor-labor substitution is consistent with evidence from managers' surveys (Hirsch et al., 2015) which find that managers consider hiring older/more experienced workers when the minimum wage is higher.

These results have several policy implications. First, when analyzing the effects of minimum wages, just focusing on teenagers may overstate the possible welfare loss or detrimental effects on employment. If minimum wages stimulate firms to adjust their composition of the workforce, we may need to consider a broader class of workers for a more comprehensive analysis of the minimum wages.

Second, evidence of labor-labor substitution suggests that firms' responses could be larger than what can be measured by the change in the number of workers. As reviewed by Clemens (2021), recent and burgeoning literature examines various channels of adjustments to minimum wages. Together with chapter 2, this paper provides evidence in line with Clemens (2021).

Third, my results have policy implications in an era of the aging population and frequent minimum wage increases. In the United States, many state legislatures are raising their own minimum wages to $15 or even higher. When the minimum wage becomes higher, employers may search for more reliable, experienced workers. The results in section 1.6 show that older workers are good candidates for those employers. In the era of the aging population, it may be even easier to adjust their workforce, since there are relatively more older workers in the labor market.

From a broader perspective, it has policy implications related to the elderly. Due to the aging population, many developed countries are considering and/or implementing labor market policies aiming to increase the labor force participation and labor supply of the older population. To achieve the goal of increasing employment of the older population and reducing the welfare burden, however, analysis of the labor demand for older workers is necessary as well. This paper sheds light on this issue by assessing how minimum wages affect the labor demand for older workers.

CHAPTER 2

MINIMUM WAGE INCREASES, HOURS OF WORK, AND OVERTIME PAY REGULATION: EVIDENCE FROM THE MATCHED CURRENT POPULATION SURVEY

2.1 Introduction

Over the past four decades, economists have extensively studied the effects of minimum wages on the number of jobs. However, extensive margin analysis focusing on the number of jobs may provide a limited picture of the effects of minimum wages on low-wage workers. Employers may prefer to adjust working hours instead of (or together with) the number of workers, especially in the short run (Hamermesh, 1993, Chapter 7). Therefore, a change in the number of jobs may misrepresent the change in labor demand.

Minimum wage earners work relatively few hours and are disproportionately likely to work part-time, but there is substantial heterogeneity within this group. Most teenagers, the group that has been most extensively studied, work in part-time jobs, while the majority of prime-age workers, a group that comprises the larger portion of minimum wage workers, work in full-time jobs. However, little research has studied whether and how minimum wage effects differ by workers' working hours. In their review of minimum wage effects on various populations, Belman et al. (2015) point out the dearth of research analyzing the differential effects on part-time and full-time workers, writing, "this is a gap in the literature where further research is likely to be productive (p.603)." This paper fills the gap by examining the minimum wage effects on part-time, full-time, and overtime workers.

In this paper, I pay particular attention to overtime pay regulation. The Fair Labor Standards Act (FLSA), the law regulating minimum wage and overtime pay, requires firms to pay at least one-and-a-half times straight rate wages for hours exceeding 40 hours per week for many minimum wage workers. Therefore, employers face a kinked labor cost schedule, which provides an economic rationale for differential effects on part-time, full-time, and overtime workers.

Specifically, I examine the short-run adjustments of labor market outcomes, especially working hours, after the minimum wage increases using observations from the matched Current Population Survey Outgoing Rotation Group (CPS-ORG hereafter) over a 15-year period (2005-2019) and focusing on large-scale state-level minimum wage changes. The results indicate that the minimum wage increases reduce the working hours of affected workers by approximately 50 minutes per week, roughly 3.3 percent of the average baseline number of work hours (implied own-wage elasticity is approximately -0.6). These effects are neither identical nor monotonic across the number of hours of work. On the one hand, the working hours of part-time and overtime workers are reduced by approximately 4.4 and 6 percent, respectively. On the other hand, full-time workers with similar wages are largely unaffected in terms of the number of working hours. These negative effects on working hours offset much of wage gain, leading to insignificant effects on weekly earnings.

Economic theory suggests that bunching is likely to arise at convex kink points (Moffitt, 1990; Saez, 2010; Kleven, 2016). I estimate the minimum wage effects on the probability of working part-time, full-time excluding 40 hours, exact 40 hours (the kink point), and overtime. I find a large shift from overtime to 40-hour workweeks for those working overtime in the previous year. Furthermore, the transition from part-time work to a 40-hour workweek is less likely to occur when the minimum wage is increased, although full-time workers are unaffected. These findings are consistent with the predictions that arise from a kinked labor cost function.

These findings speak to several lines of the literature. First, this paper contributes to the study of the intensive margin effects of minimum wage increases. Through the lens of longitudinal data, the results clarify how minimum wage increases affect those who are earning a minimum wage. This study also extends the literature by addressing an understudied but theory-guided dimension of heterogeneity based on the initial level of working hours. Given that minimum wage workers from various demographic groups work different numbers of hours, it provides a useful guideline for understanding and predicting the effects of minimum wage increases. Finally, it contributes to the literature on overtime work by examining the effects of wages on overtime incidence using plausibly exogenous variations in wages.

The remainder of the paper is organized as follows: Section 2.2 introduces the background and related literature. Section 2.3 presents a conceptual framework that shows the effect of the minimum wage increases under overtime pay regulations. Section 2.4 introduces the data and summary statistics. The empirical strategy is discussed in Section 2.5. Section 2.6 shows the empirical findings. Section 2.7 summarizes the paper and presents a simple back-of-the-envelope calculation examining the total hours lost if all states increased their minimum wages. The results suggest that even though the size of the overtime minimum wage worker population is small, their practical importance is not negligible.

2.2 Background and Literature Review

Enacted in 1938, the FLSA has regulated the price and quantity of labor in the U.S. by setting the minimum wage and overtime pay rules.[1] The minimum wage is perhaps the most extensively scrutinized policy in the field of labor economics.[2] The minimum wage literature has primarily been concerned with the effects of the minimum wage on the number of low-wage workers, especially teenagers, and workers employed in fast-food restaurants. An extensive margin analysis examining the effects on the number of workers has attracted attention in academia for two main reasons. First, this question is helpful for understanding the costs and benefits of minimum wages. Second, examining whether a minimum wage reduces employment is widely considered a means for assessing so-called Neoclassical economics based on the downward-sloping demand curve.

As noted by Stewart and Swaffield (2008, p.150), however, economic theory predicts a reduction in labor usage, not necessarily a reduction in the number of workers. Firms may decide to adjust working hours, as well as the headcount of workers. Therefore, examining only the extensive margin responses may provide a limited picture of the overall effects on labor demand.

This concern provides motivation for the studies on the intensive margin analysis of minimum wages, starting from Zavodny (2000).[3] Compared to the existing literature on the extensive margin analysis, there are far fewer studies on the intensive margin, and the evidence is mixed.[4] Similar

[1] Brown and Hamermesh (2019) provide a review of economic research on the FLSA

[2] I do not attempt to review the vast body of minimum wage literature in its entirety here, and several high-quality reviews are available (e.g., Neumark and Wascher, 2008; Belman and Wolfson, 2014).

[3] Belman and Wolfson (2014, Chapter 3) provide a detailed review of this subject.

[4] Although debates and disagreements continue, review papers and meta-analyses suggest that median teen em-

to studies of the extensive margins, the majority of the intensive margin studies focus on specific subgroups with low wages. Focusing on the evidence from the United States, studies using two-way fixed effects (TWFE, hereafter) and state-panel data to examine the intensive margin effects on teenagers have found negative effects, although Allegretto et al. (2011) find that negative effects disappear when controlling for regional heterogeneity more aggressively by adding division-specific time controls and state-specific time trends.[5] Estimates for other populations have also varied from no effects (Dube et al., 2007 for the fast-food industry, Orrenius and Zavodny, 2008 for immigrants with lower educational attainments) to large elasticities of -1 (Sabia, 2008 for single mothers with lower educational attainments and Orazem et al., 2002 for retail sectors). Cengiz et al. (2019, Appendix Table A) provide the intensive margin analysis of the entire population of low-wage workers, not limited to specific groups. By applying the bunching method to the CPS-ORG, they find no negative effects on the number of full-time equivalents (FTE), which can be understood as total unconditional working hours.[6]

A few other studies have used longitudinal data to examine intensive margin responses, as I do in this paper. With longitudinal data, researchers can focus on workers who are actually earning a minimum wage instead of relying on age or other demographic variables to identify the population of interest, instead of relying on demographic information. The closest study is Neumark et al. (2004, NSW for short), which use the matched CPS-ORG (1979-1997) and report the negative effects on working hours of low-wage incumbent workers. Compared to NSW, the present paper focuses on larger minimum wage changes. Since the minimum wage effect is likely to be nonlinear (Clemens and Strain, 2021), focusing on more salient changes provides better ways to understand the effects. I further extend the literature by exploring a new dimension of heterogeneity that results from differences in initial working hours.

Two recent studies apply administrative panel data to this question. Gopalan et al. (2021) use

ployment elasticities in the literature lie in the narrow bound of [-0.2, 0]. See Wolfson and Belman (2019) and Congressional Budget Office (2019).

[5]Unconditional hours elasticity ranging from -0.2 to -0.4 (Orrenius and Zavodny, 2008; Sabia, 2009) and smaller conditional hours elasticities (from 0 to -0.1)

[6]Cengiz et al. (2019) measured the number of FTEs by the weighted sum of working hours divided by 40.

administrative payroll panel data and report no effects on the working hours of minimum wage workers, although they do find reductions in the number of new hires. In contrast, Jardim et al. (2022) find that the minimum wage increase in Seattle reduces working hours, while it does not affect the number of jobs. While these papers focus on a single or small number of minimum wage increases, the present paper studies a broader set of minimum wage increases. My key findings, the absence of extensive margin responses accompanied by large intensive margin responses in the short run, are in line with those of Jardim et al. (2022).

Although the effect on the relative share of part-time and full-time workers among the workforce has been an important topic in the literature,[7] little research has examined whether and how those who work part-time and full-time are affected differently. In a review of minimum wage effects on various populations, Belman et al. (2015) note the dearth of work on this subject, mentioning that they are aware of only one study using British data (Connolly and Gregory, 2002). Studies on overtime workers are even scarcer, partly reflecting the fact that overtime work is not common among minimum wage workers. One notable exception is Cengiz et al. (2022), who examine the effects of minimum wage increases on the share of part-time and overtime workers. Cengiz et al. (2022) find that minimum wage increases reduce the share of part-time workers among the high-probability group identified by machine learning. This study extends the literature by explicitly considering differential effects on workers who are working different amounts of hours prior to the minimum wage increases.

This work contributes to the small body of economic studies on overtime pay regulation. Although the minimum wage literature has largely ignored the issue of overtime work, the overtime pay literature has focused on the relationship between overtime pay and minimum wages, starting from Trejo (1991). He analyzes overtime pay regulation from two perspectives: the fixed-wage (Labor Demand) model and the fixed-job (Employment Contract) model. In the former, a straight rate wage is given and fixed; hence, imposing an overtime pay premium creates a kink in the wage schedule and overtime pay discourages overtime work for everyone. The prediction based

[7] Card and Krueger (1995, pp. 48-49.), and Dube et al. (2007) find an increase in the proportion of full-time workers. Ressler et al. (1996) report the opposite finding based on a time-series method.

on the fixed-job model is very different. In this framework, employers and workers care about the combination of total (weekly) hours and earnings. If the government newly introduces the overtime pay or increases the premium, workers and employers can nullify its effect and maintain the same combination of hours and earnings by adjusting the straight wage rate – except when they cannot legally lower the straight wage rate, as is the case for minimum wage earners. Although they yield different predictions regarding the effects of overtime pay regulation, these two perspectives agree on the effect on the minimum wage workers: overtime pay regulation is binding for them. Hence, employers of minimum wage workers face a kinked labor cost schedule caused by the overtime pay regulation. By using minimum wage as a source of plausibly exogenous variations in wages, this paper provides evidence of how employers adjust labor hours to wage increases when they face a kinked cost schedule.

2.3 Conceptual Framework

This section presents a labor demand model with a kinked labor cost adopted from the textbook model of Cahuc et al. (2014, Chapter 2). Consider a production function with a single labor input, $Y = f(L)$,[8] where L is total labor use. Define $L = N \cdot e(H)$ where N is the number of workers and $e(H)$ is the measure of efficiency per worker.[9]

Suppose that employing a worker entails a fixed cost, F. For simplicity, ignore additional fixed costs associated with full-time work (e.g. health insurance) and other fringe benefits. Let w be the given hourly wage. If workers work longer than $\bar{H}$, employers need to pay overtime premium b for additional hours. In the United States, $\bar{H} = 40$ and $b = 0.5$ The labor cost per worker is $LC(H) = F + wH + bw(H - \bar{H})\boldsymbol{I}(H > \bar{H})$.

The employer's maximization problem given wage rate is

$$\max_{N,H} \pi = pf(L) - N \cdot LC(H)$$

[8] It can be understood as the short-run adjustments when capital is fixed.

[9] If $Y = L, e(H)$ can be understood as the production function in Barzel (1973). He studies the shape of the $e(H)$ function under the learning-by-doing and work fatigue. If $e(H) = H$, workers and hours are perfectly substitutable.

Define cost per efficiency, $\omega = \dfrac{LC(H)}{e(H)}$. Rewrite the firm's problem as

$$\max_{N,H} \pi = pf(L) - \omega L$$

The problem can be solved in two steps, as discussed in Cahuc et al. (2014). Firms first find H which minimizes ω, then choose optimal L^* by choosing N. The solution is

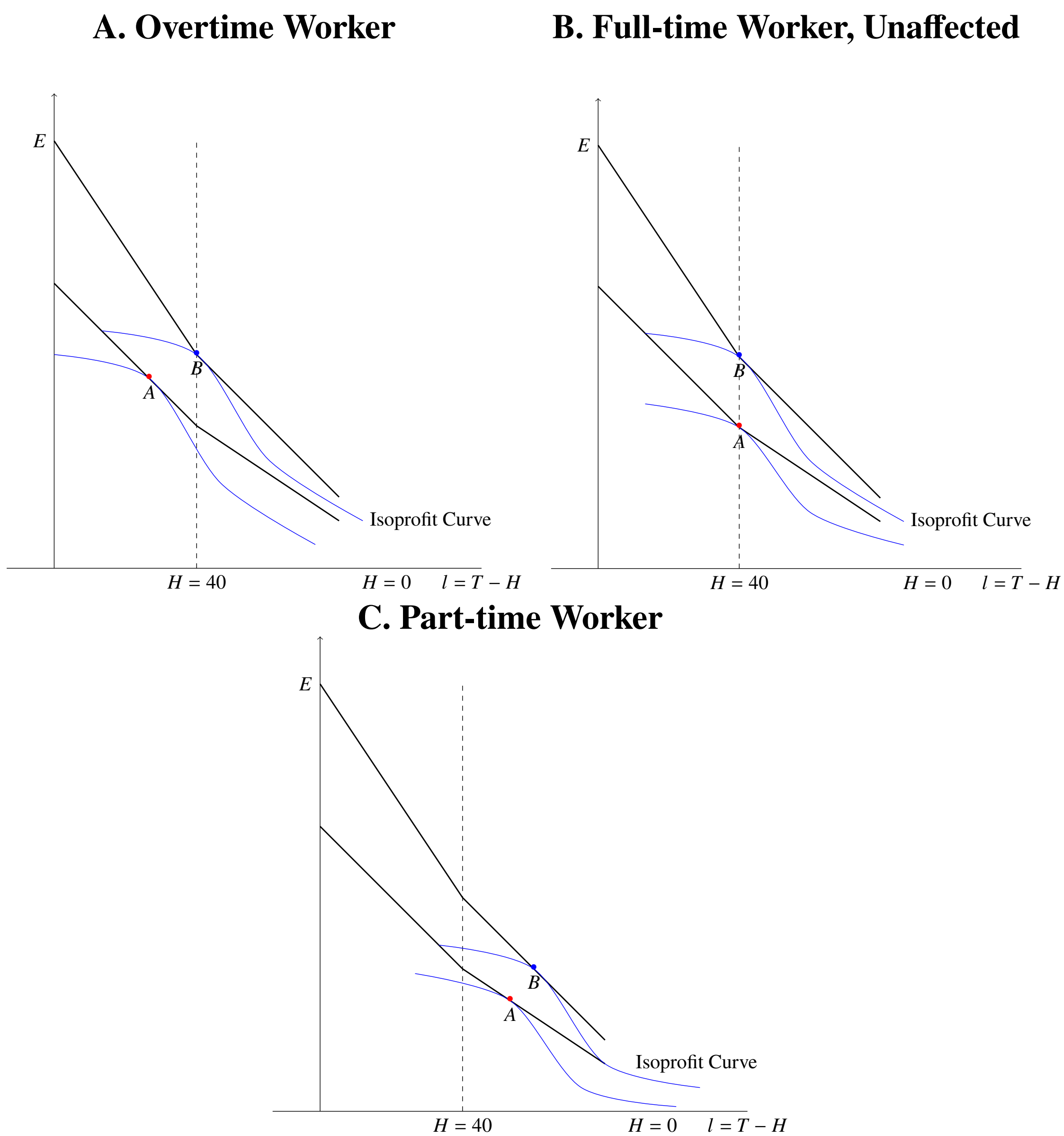
$$H^* = \begin{cases} \dfrac{\eta_H^e(H_p^*)F}{w(1-\eta_H^e(H_p^*))} & \text{if } F \leq \delta_p \\ \bar{H} & \text{if } \delta_p \leq F \leq \delta_o \\ \dfrac{(F - bw\bar{H})\eta_H^e(H_o^*)}{w(1+b)(1-\eta_H^e(H_o^*))} & \text{if } F \geq \delta_o \end{cases}$$

where $\delta_p = \dfrac{w(1-\eta_H^e(H_p^*))\bar{H}}{\eta_H^e(H_p^*)}$, $\delta_o = \dfrac{w(1+b-\eta_H^e(H_o^*))\bar{H}}{\eta_H^e(H_o^*)}$, and $\eta_H^e(\cdot)$ is the elasticity of efficiency with respect to hours and measures the productivity gain from working more hours. η_H^e should be in the [0,1] interval. $\eta_H^e = 1\ \forall H$ implies that workers and hours are a perfect substitute ($e(H) = H$), then there is no interior solution. In this case, the optimal working hour is either $\bar{H}$ or the largest possible H. Since $\bar{H}$, b, and w are fixed parameters, main determinants of working hours are fixed costs and elasticity of efficiency. Smaller fixed costs and lower elasticities of efficiency promote the hiring of part-time workers.

What would happen if the wage is increased to $w' > w$? Suppose that the new profit level is large enough to compensate for variable costs. Then employers will continue to hire workers at least in the short run. The above equation shows that $\dfrac{\partial \delta_p}{\partial w} > 0$, $\dfrac{\partial \delta_o}{\partial w} > 0$ and $\dfrac{\partial H^*}{\partial w} \leq 0$. Therefore, more workers are likely to work part-time, and some of the overtime workers will move to 40-hour per week jobs. Some full-time workers may become part-time workers, but for those who are in $F \in [\delta_p(w'), \delta_o(w)]$ will not be affected.

Figure 2.1 illustrates some examples. I assume the isoprofit curve to be convex at least near H^*. Panel A shows the case of an overtime worker who moved to the kink point, $H = 40$. Panel B shows the case of a full-time worker whose work hours have not changed. Finally, Panel C shows the case of a part-time worker. The intuition is simple. By rotating the slope of the labor cost, the

Figure 2.1 Minimum Wage Increases and Working Hours with Overtime Pay Regulation



standard price effects argument works for the part-time and overtime workers. However, a larger change in wages is required to move the workers clustered at the kink point.

In summary, the model predicts negative effects on working hours for those previously working part-time and overtime, while smaller negative effects for those working full-time. The effects on the overall size of the bunching are ex ante unclear, since some full-time workers will lose their hours while overtime workers will move to the kink point.[10]

2.4 Data

The primary source of information for this study is the NBER extract of the CPS-ORG for the years 2005-2019 (National Bureau of Economic Research, Various Years), a nationally representative, large scale survey.[11] I construct a two-year panel data using the structure of the CPS-ORG.[12] The strengths of the CPS-ORG include its large sample size and relatively precise information on hourly wages.[13] However, its short panel lengths prevent the study of longer-run changes.

I exclude workers whose hours or wages are imputed (following Cengiz et al., 2019), self-employed workers, and workers whose growth in wages or hours exceed 1000 percent (following NSW). I exclude observations whose first-year wages are lower than 50 percent of the effective minimum wage, which is the maximum of state- and federal-level minimum wages. Monthly state-level effective minimum wage data are obtained from Vaghul and Zipperer (2019). I further restrict my sample to be balanced, requiring the respondents to be between 16 and 64 years of age in both interviews. State-level information is downloaded from U.S. Bureau of Labor Statistics (2023). Inflation is adjusted by using R-CPI-U-RS downloaded from U.S. Bureau of Labor Statistics (2022).

The key variables are the measures of hourly wages and hours. I use straight rate wages

[10] Δ Bunching $= \displaystyle\int_{\delta_o(w)}^{\delta_o(w')} f(F)dF - \int_{\delta_p(w)}^{\delta_p(w')} f(F)dF$ where $f(F)$ is the pdf of fixed costs

[11] Changes in overtime pay regulation may change the effects of minimum wages on overtime work by altering the bindingness of overtime pay. To minimize this concern, I select a period between changes in overtime regulations; 2005 and 2020.

[12] When the respondents enter the survey, they are interviewed for the first four months, there are no interviews for the next eight months, and the respondents are interviewed again in the final four months. In the fourth and eighth interviews, the respondents are asked for more detailed information on earnings, forming the Outgoing Rotation Group.

[13] In the CPS-ORG, the respondents are asked about their hourly earnings directly if they are paid hourly. Therefore, the hourly earnings information for them is free from the measurement errors in hours.

excluding overtime pay, tips, and commissions whenever this information is available. However, the salaried workers report only their earnings - including overtime pay, tips, and commissions - in whatever time frame is convenient for them (annual, monthly, biweekly, weekly, or others), and the CPS converts reported earnings to weekly earnings. The hourly wages of salaried workers are calculated by dividing the weekly earnings by the weekly working hours; for the working hours, I use the usual number of hours worked per week.

Based on the hourly wages, I restrict my sample to low-wage workers whose initial hourly wage was lower than the initial minimum wage plus five dollars. I divide these workers into 12 groups: four groups based on wages times three groups based on working hours in the first year. The four wage groups are roughly similar in size. The first group consists of the workers whose first-year wage is lower than the effective minimum wage plus 50 cents (in 2019 USD). Given the definition of treatment discussed in the next section, they are directly affected if treated. In the second wage group, the wage minus the minimum wage of workers is in (0.5, 2]. In the treated states, they are a mixture of directly affected (those whose hourly wage is larger than the minimum wage plus 50 cents but smaller than the new minimum) and indirectly affected by spillover effects. These two groups are the primary focus. Throughout the paper, I refer to the former as the lowest wage group (or Group 1), and the latter as the lower wage group (or Group 2). The third (Group 3) and fourth (Group 4) groups consist of workers whose first-year wages minus the minimum wage are in (2, 3.5] and (3.5, 5], respectively. Workers in these groups are still low-wage workers, but are less likely to be affected by minimum wage changes.

I classify the workers as part-time, full-time, and overtime workers based on the usual working hours in the year they enter the survey. Overtime workers are defined as those who work more than 40 hours per week. The distinction between part-time and full-time workers is rather unclear. In this study, I follow the definition of the CPS and Dunn (2018) and define part-time workers as those who work less than 35 hours per week.[14]

[14] If workers worked less than 35 hours per week, the CPS asks the reason. Alternative definitions of part-time workers are possible. For instance, Cengiz et al. (2019) define part-time workers as those who work less than 30 hours per week.

Table 2.1 Descriptive Statistics

			Wage Group		
	All	Group 1	Group 2	Group 3	Group 4
	$\tilde{w} \leq 5^b$	$\tilde{w} \leq 0.5$	$0.5 < \tilde{w} \leq 2$	$2 < \tilde{w} \leq 3.5$	$3.5 < \tilde{w} \leq 5$
Panel A. Year 1 Economic Variables					
Log Wage (2019 USD)	2.330	2.108	2.276	2.410	2.530
	(0.197)	(0.192)	(0.104)	(0.092)	(0.091)
Paid Hourly	0.853	0.813	0.905	0.860	0.826
Weekly Hours of Work	32.404	28.195	30.832	34.189	36.479
	(12.020)	(13.272)	(11.766)	(11.083)	(10.143)
Part-time	0.426	0.603	0.497	0.350	0.248
Full-time	0.504	0.339	0.449	0.575	0.655
Overtime	0.071	0.058	0.054	0.075	0.097
Panel B. Year 2 Economic Variable					
Work in Second Year	0.812	0.747	0.795	0.837	0.870
Log Wage (2019 USD)[a]	2.460	2.320	2.391	2.507	2.604
	(0.302)	(0.331)	(0.266)	(0.273)	(0.262)
Paid Hourly[a]	0.827	0.822	0.870	0.821	0.792
Weekly Hours of Work[a]	34.193	30.942	32.914	35.453	37.014
	(10.879)	(12.095)	(10.945)	(10.221)	(9.323)
Part-time	0.283	0.375	0.327	0.239	0.187
Full-time	0.469	0.325	0.419	0.532	0.601
Overtime	0.060	0.046	0.048	0.066	0.081
Change Industry[a]	0.294	0.268	0.300	0.307	0.300
Panel C. Year 1 Demographic Variable					
Age	34.872	31.305	33.544	36.498	38.192
	(13.820)	(13.849)	(13.927)	(13.473)	(12.982)
Female	0.585	0.592	0.593	0.576	0.577
< High School	0.234	0.352	0.258	0.183	0.146
High School Graduates	0.349	0.301	0.351	0.370	0.373
Some College	0.309	0.267	0.305	0.327	0.336
College Graduates	0.088	0.063	0.072	0.098	0.118
Higher than B.A.	0.020	0.016	0.014	0.021	0.028
African American	0.125	0.113	0.127	0.131	0.129
Hispanic	0.247	0.275	0.262	0.240	0.211
Other Non-white Races	0.086	0.099	0.088	0.080	0.077
Teenagers	0.139	0.268	0.172	0.077	0.042
Panel D. Year 2 Wage Distribution					
$w^2 \leq MW^1 + 0.5^a$	0.147	0.437	0.124	0.047	0.028
$w^2 \leq MW^1 + 2^a$	0.371	0.723	0.561	0.177	0.078
$w^2 \leq MW^1 + 3.5^a$	0.590	0.827	0.777	0.561	0.213
$w^2 \leq MW^1 + 5^a$	0.764	0.880	0.864	0.747	0.574
$w^2 \leq MW^2 + 0.5^a$	0.165	0.474	0.151	0.056	0.031
Panel E. Treatment					
Experience Treatment	0.144	0.152	0.139	0.140	0.145
Experience Small-scale Increases	0.222	0.266	0.212	0.199	0.213
Experience Increase by Federal[b]	0.093	0.067	0.084	0.096	0.125
Observations	91308	20465	24050	24713	22080

Notes: Variables with [a] are measured conditional on working in the second year. $\tilde{w} = w^1 - MW^1$, the first-year wage minus the first-year minimum wage. All the results are weighted by the CPS earnings weight (*earnwt*). The first row of the variable shows the means and the second shows the standard deviations. The standard deviations of the indicator variables are omitted. [b] includes both the small and large scale increases by federal one.

Table 2.1 presents descriptive statistics summarizing the data. Even among low-wage workers, there are substantial differences in patterns in working hours. In general, the workers with lower wages are more likely to work part-time and fewer hours, with a larger standard deviation, suggesting greater variation among them. Although approximately 60 percent of the minimum wage workers are part-time workers, only approximately 25 percent of the upper wage groups work part-time.

The low wage workers are not strongly attached to the labor market: approximately 20 percent of the workers did not work in the second year. Both higher wages and a larger number of working hours suggest that the workers are more strongly attached to the labor market. While more than 25 percent of Group 1 workers do not work in the second year, the percentage drops to approximately 17 percent for Group 4 workers. Appendix Table B.1 shows that approximately 25 percent of the part-time workers do not work in the second year, while more than 90 percent of the overtime workers do.

Panel D shows the evolution of wages. Approximately half of the workers in the lowest wage group remain in minimum wage positions in the second year, and approximately 10 percent or fewer of the non-minimum low-wage workers become minimum wage workers in the second year. These findings are in line with those of Carrington and Fallick (2001). The minimum wage workers caught up to their counterparts in the higher wage groups. The difference in the average wage between the first and second groups shrinks from 0.168 log points to 0.071 log points, while the gap between the second and third groups is almost stable (0.134 log points to 0.116 log points).

In Appendix Table B.1, I present the summary statistics by the hour group. Three features of Appendix Table B.1 are worth mentioning. First, there is a strong correlation between current working hours and future working hours. Conditional on working, approximately three-quarters of the part-time workers continue to work part-time in the second year, and most of the full-time workers continue to work full-time. The fraction decreases for the overtime workers; nevertheless, more than half of the overtime workers work overtime in the subsequent year. Second, although most of the low-wage workers are paid hourly (85 percent), approximately half of the overtime workers are salaried. This finding partly reflects salaried workers who are not near-minimum wage workers but are included in this category due to the large positive measurement error in the number of hours worked. However, it may also reflect overtime pay concerns. As noted by Chung and Haider (2020), larger portions of salaried workers are not subject to overtime pay due to the FLSA structure, so employers may have the incentive to change their method of payment. Finally, the part-time workers are generally younger, and the share of female workers is much higher for this population. Approximately one-quarter of the part-time workers are teenagers, while less than 3 percent of the overtime workers are younger than 20.

2.5 Identification Strategy

To identify the effects of the minimum wage increases, I compare those workers whose initial labor market behaviors are similar but who experience different minimum wage evolutions. I define the relevant workers based on their initial wage, instead of using demographic or industry information. This method enables me to analyze a larger proportion of the minimum wage workers. The effects on hiring are beyond the scope of this paper.[15] Furthermore, due to the short panel length ($T = 2$) of the CPS-ORG, I could assess effects only in the short-run. This limits the analysis, but, given that working hours are adjusted more quickly (Belman and Wolfson, 2010), it may provide a reliable picture on the intensive margin responses.

During the sample period, 2005-2019, there were hundreds of nominal minimum wage increases including states that index their minimum wages to inflation. The magnitude of the nominal increases range from 4 cents to 1.95 dollars.[16] Among the numerous increases, I focus my attention on those increases that are at least 50 cents in 2019 USD and define them as 'treatments'.[17] This definition gives me 107 treatments. The definition is similar to that of Cengiz et al. (2019, 2022). The median size of the minimum wage increases of the treatment is approximately 10 percent of

[15]Several recent papers argue that new hiring is a key channel of employment adjustments. See Meer and West (2016) and Gopalan et al. (2021)

[16]Increases of 4 cents were implemented in Alaska (2018), Florida (2009), Missouri (2017), and Ohio (2017) and an increase of 1.95 dollars was implemented in Arizona (2017)

[17]The minimum wage effects could be much clearer in the large-scale increases. For a detailed discussion, see Clemens and Strain (2021).

the previous minimum wage. The following difference-in-differences specification is used:

$$y_{ijhst} = \alpha + \sum_j \beta_j D_{st} \times \boldsymbol{I}(w^1 \in \text{Group } j) + X'_{ist}\delta + W'_{st}\eta + \Omega + \lambda_{jh} + \rho_{js} + \rho_{jt} + e_{ist} \qquad (2.1)$$

Variables y_{ijhst} represent the labor market outcomes of individual i residing in state s at time t in wage group j and hour group h. The unit of time is month. Variable D is a treatment indicator that equals 1 if individual i living in state s experiences the treatments defined above in the month (t) of the second-year interview or in the preceding 11 months.[18] w^1 refers to real hourly wages in the first-year interview, and the wage group is defined based on their wages compared to the effective minimum wages. From here, all the superscripts imply the year of the interview (1 or 2).

Parameters β_js are the key parameters of interest that measure the treatment effects on the workers in wage group j. To compare those who experience large scale minimum wage changes and no changes, I additionally put indicators for federal- and small-scale state- level increases that interact with each wage group indicator in Ω.

Vector X contains a set of individual covariates including gender, a quartic in age, the categorical variables of education and race, and an indicator for metropolitan residents measured in the initial year. W is a set of state-level controls including state-level unemployment and the logs of the population, measured in the second interview year. Three sets of fixed effects - wage-group-by-hour-group fixed effects (λ_{jh}), wage-group-by-state fixed effects (ρ_{js}), and wage-group-by-time fixed effects (ρ_{jt}) - are included. By putting ρ_{js} and ρ_{jt}, β_js can be understood as parameters in the standard TWFE regression using only the observations in wage group j. Therefore, β_js are identified using variations within the wage group across treated and untreated states. Putting λ_{jh} further controls the differences in the labor market outcomes related to the differential initial labor market behaviors, hence identification is made within the groups.

This empirical strategy has several issues. The wage group is defined based on the wage relative to the minimum wage in the first year. This method is in line with studies using longitudinal data

[18] It is intended to capture the minimum wage increases between the two interviews in the CPS. Since the exact interview date is unknown, some minimum wage changes may have taken effect merely a few days prior to the first interview or after the second interview.

and aggregating states with different minimum wages (e.g., NSW and Lopresti and Mumford, 2016). However, the substantial variations in state-level minimum wages in recent years raise an important issue. For instance, a worker in Mississippi (which is bound by a federal minimum wage of $7.25) earning $7.50 in 2019 and another worker in the District of Columbia (whose minimum wage was $14 in 2019) earning $ 14.25 dollars per hour in 2019 are classified into the same wage group. Both the absolute value of wages and wages relative to the minimum may play a role, and if so, it is questionable whether a worker in D.C. could be an appropriate counterfactual for a worker in Mississippi and vice versa. Furthermore, the minimum wage effects could be larger when high minimum wage states are treated, which introduces the possibility of heterogeneous effects.

If both the treated and the untreated states contain a good mixture of high- and low-wage states in each period, this problem may not be a major concern. The minimum wage increases are, however, unevenly distributed over time and across states. Recent minimum wage increases have been led by high minimum wage states, and a large proportion of workers in the control group reside in the states that are bound by federal minimum wages. In contrast, during the periods of federal minimum wage increases (2007-2009), only a handful of high minimum wage states comprise the control group.[19]

Appendix B.3 includes two exercises conducted to examine how different compositions of treated and control groups affect the estimates. First, I include the minimum wage increases by the federal level changes in treatments and estimate the main analysis of the paper. Second, I estimate the same equations using only observations from the years 2011-2019, a period with no federal level minimum wage increases. In the years 2011-2019, there is a stronger tendency for higher minimum wage states to further increase their own minimum wages. Appendix B.3 shows that the adjustments become larger when the treated group includes more high minimum wage states and become smaller when federal level increases are included.

Another issue is related to the uneven distribution of the minimum wage increases and their heterogeneous effects. Equation (2.1) is in the TWFE framework. Recent econometric literature

[19]For instance, as of July 2007, the year of the federal minimum wage increases, only three states (Alaska, Minnesota, and Wisconsin) and the District of Columbia did not experience any nominal changes in minimum wages within a year.

has shown that the parameter of interest, β_j, can be understood as a weighted sum of average treatment effects in each time t, and some weights could be negative (see, e.g., de Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021.) Negative weights are more likely to arise when a large share of states are treated, and when some states are treated in many periods. These negative effects are likely to be an issue if the treatment effects are heterogeneous. Both concerns are relevant for this study. As shown in Appendix Figure B.1, the minimum wage increases are highly clustered in the years 2007-2009 and 2016-2019. Furthermore, the former coincided with the Great Recession, the period in which the minimum wage effects could be different from other years.

To determine whether this problem affected the estimates, I use the estimator provided by de Chaisemartin and d'Haultfoeuille (2020). The key intuition of their alternative estimator is to compare outcomes of the 'joiners' and the 'stable group'.[20] Since most of the minimum wage increases are clustered in January (in the beginning of the year), - and July if federal level increases are also considered - I am able to use only a small fraction of the observations. The results using the alternative estimator are shown in Appendix B.5. The results are generally larger in magnitude but highly imprecise possibly due to the small sample size.

So far, I have not permitted the effects to vary across the hour groups. To examine whether the minimum wage effects are different across the initial working hours, I revise Equation (2.1) as follows

$$\begin{aligned} y_{ijhst} &= \alpha + \sum_j \sum_h \beta_{jh} D_{st} \times \boldsymbol{I}(w^1 \in \text{Group } j) \times \boldsymbol{I}(h^1 \in \text{Group } h) \\ &\quad + X'_{ist}\delta + W'_{st}\eta + \Omega + \lambda_{jh} + \rho_{js} + \rho_{jt} + e_{ist} \end{aligned} \tag{2.2}$$

Here, β_{jh} captures the effects of the large-scale increases in the effective minimum wages ("treatment") on labor market outcomes whose wage group is j and hour group is h. The conceptual framework expects the effects to be different in magnitude across hours of work, and even opposite in sign for the probability of working at the kink point. By estimating β_{jh}s, Equation (2.2) can

[20]The joiners are those who start to receive the treatments or stop receiving treatments and the stable group refers to those who are treated both before and after or who are untreated in both periods

examine whether aggregating everyone regardless of their working hours ignores such differential adjustments by hours.

This study focuses on the short-run changes. One potential source of misspecification is dynamic effects of minimum wage changes. Technically, it is possible to analyze using an event-study framework including lags and/or leads of minimum wage changes. However, interpreting the coefficients for the past minimum wage changes is clear only under the assumption that the past minimum wage increases do not affect their wage relative to the minimum wage *and* part-time, full-time, and overtime status in the first-year observation. Given the evidence provided by Belman and Wolfson (2010) that the employers adjust hours quickly (within a year), this assumption is restrictive. Instead of imposing stronger assumptions, I focus on the short-run effects of the minimum wage changes. However, I test whether the estimates are sensitive to the inclusion of lags and leads in Appendix B.4 by using the event-study specification. The results are generally robust to the inclusion of leads and lags. Coefficients for the future minimum wage increases are generally close to zero and flat, suggesting parallel pre-trend. Analysis in Appendix B.4 further confirms that intensive margin responses are completed within a year.

2.6 Empirical Findings

I first explore the effects of the minimum wage increases on the incumbent workers by estimating Equation (2.1). In Table 2.2, the first three columns are estimated conditional on working in both the first and second years, and the latter two columns condition just on working in the initial year. Column (1) shows that hourly wages increased by approximately 5 percent for the workers in the lowest wage group. Note that the median minimum wage increase is approximately 10 percent in treated states, which implies a wage elasticity of approximately 0.5. The estimates for the upper wage groups show positive but smaller effects on the hourly wages, suggesting small spillover effects.

Does the minimum wage affect employment outcomes? Columns (2) through (5) examine this question. Columns (2) and (3) show the intensive margin responses using two measures: usual weekly hours of work and log of weekly working hours. Conditional on working, increased wages

Table 2.2 Minimum Wage Effects across Wage Groups

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} = w^1 - MW^1 \leq 0.5)$	0.052***	-0.864**	-0.033*	0.015	-0.335
	(0.013)	(0.271)	(0.013)	(0.015)	(0.445)
$D_{st} \times I(0.5 < \tilde{w} \leq 2)$	0.031***	0.161	0.009	-0.010	-0.274
	(0.009)	(0.204)	(0.007)	(0.012)	(0.404)
$D_{st} \times I(2 < \tilde{w} \leq 3.5)$	0.024+	0.024	0.006	-0.000	0.087
	(0.014)	(0.243)	(0.009)	(0.011)	(0.357)
$D_{st} \times I(3 < \tilde{w} \leq 5)$	0.010	-0.104	0.000	-0.000	-0.067
	(0.012)	(0.231)	(0.009)	(0.008)	(0.331)
Observations	74,150	74,150	74,150	91,308	91,308
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: $\tilde{w} = w^1 - MW^1$, the initial wage minus the initial minimum wage. Each number in the cell shows β_j. All the regressions include a quartic in age, dummy for high-school graduates, some college, B.A., above B.A., African-American, Hispanic, and other nonwhite races, and metropolitan residents as the individual controls. State-month level unemployment rates and logs of the population are included. Standard errors are clustered at state level. All results are weighted using earnings weight (*earnwt*). The period unit is month.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

lowered the weekly working hours. Workers in the lowest wage group lost hours by approximately 50 minutes or 3.3 percent. It suggested that employers responded to the wage shock along the intensive margin in the short run. Combining with the wage elasticity of 0.5 from the column (1), implied own-wage elasticity of working hours, the ratio of the percent change of the working hours to the percent change of the wages, is approximately -0.6. In contrast to large adjustments for the workers in the lowest wage group, there are no detectable effects on the upper wage groups. In summary, only narrowly defined near-minimum wage workers are affected, suggesting that precise information of the affected population is important. Much of the wage gain is offset by the loss in hours, leading to insignificant effects on weekly earnings.[21]

[21] The effects on the log weekly earnings (including overtime pay, tips, and commissions for both hourly paid and salaried workers) are -0.002 (se 0.017) for workers in the lowest wage group, and 0.032 (se 0.013) for those in the lower wage group. In principle, the effects on log weekly earnings should be equal to the sum of the effects on the hourly wage and weekly working hours. Estimated coefficients deviate from the simple sum of two coefficients since different measures are used for weekly earnings and hourly wages of hourly paid workers. In the CPS-ORG, hourly wages of the hourly paid workers do not include overtime pay, tips, and commissions, while weekly earnings do. These definitions suggest that the product of hourly wage and weekly working hours should be larger or equal to the reported weekly earnings. However, in data, the average of the latter is larger than that of the former for the minimum wage workers. Since the CPS converts reported earnings in whatever time frame is convenient for respondents (biweekly, monthly, quarterly, annual, etc) into weekly earnings, it may reflect the fact that minimum wage workers are weakly attached to

The minimum wage increases, however, have no effect on the number of workers, at least for the incumbent workers analyzed in this study, as is clear in column (4). The absence of extensive margin responses for incumbent workers is consistent with recent studies (see Cengiz et al., 2019; Gopalan et al., 2021; Jardim et al., 2022). However, it does not necessarily imply the absence of employment effects if employers prefer to adjust the size of the workforce by hiring channels rather than firing (Gopalan et al., 2021).

Column (5) examines the effects on hours of work, unconditional on working in the second year. It can be understood as the effects on total working hours of incumbent workers. Together with the extensive margin analysis, it does not show significant effects. In summary, Table 2.2 shows the intensive margin responses without extensive margins in the short run, and this finding is in line with those from the Seattle minimum wage increases (Jardim et al., 2022).

Finally, the effects on two upper wage groups are neither economically nor statistically significant, except for small and weak positive effects on the wage of the third group, as expected. In later analyzes, I only present the effects on the bottom two groups for the sake of convenience. Full tables are available in Appendix B.2.

Table 2.3 shows the disaggregated results by initial working hours, following Equation (2.2). Again, column (1) shows clear, positive wage effects. Wage effects were approximately 5.6 percent for part-time workers and 3.5 percent for full-time workers. Large, positive wage effects on overtime workers partly reflect a reduction in working hours. As discussed above, a large share of overtime workers are salaried workers. Since the wage of salaried workers is calculated by dividing weekly earnings by weekly hours, a reduction in hours may create a spurious positive effect on wages.

Columns (2) and (3) show the evidence of intensive margin adjustments in the short-run. In columns (2) and (3), part-time and overtime minimum wage workers are negatively affected on hours, while full-time workers are nearly unaffected. Part-time workers lose their hours by approximately one hour or by 4.4 percent, and overtime workers lose hours by approximately 3.3 hours or 6 percent. Implied own-wage elasticity of working hours is approximately -0.8 and -0.5,

the labor market so they are less likely to work full-year or full-month.

Table 2.3 Minimum Wage Effects across Wage and Hours Group

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in (0, 35))$	0.056***	-0.995**	-0.044*	0.027+	-0.092
	(0.014)	(0.371)	(0.018)	(0.016)	(0.486)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [35, 40])$	0.035*	-0.160	-0.009	-0.008	-0.418
	(0.015)	(0.229)	(0.010)	(0.019)	(0.695)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 > 40)$	0.113*	-3.334***	-0.060*	0.018	-2.167+
	(0.044)	(0.883)	(0.024)	(0.027)	(1.103)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in (0, 35))$	0.032**	-0.128	0.000	-0.006	-0.357
	(0.011)	(0.295)	(0.011)	(0.022)	(0.596)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [35, 40])$	0.030**	0.518+	0.019+	-0.016	-0.211
	(0.010)	(0.298)	(0.010)	(0.013)	(0.517)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 > 40)$	0.026	-0.173	0.006	0.003	0.036
	(0.029)	(0.979)	(0.027)	(0.033)	(1.446)
Observations	74,150	74,150	74,150	91,308	91,308
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, in other words, initial wage minus initial minimum wage. h^1 refers to working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

respectively. The 95 percent confidence interval for the estimate for part-time or overtime workers rule out the point estimate for full-time workers. This pattern is consistent with the prediction from Section 2.3.

An alternative explanation for negative effects on part-time workers might be related to their lower productivity. This is in line with larger negative effects on the working hours of less experienced workers Jardim et al. (2022).[22] However, this explanation is not readily applicable to the results in Table 2.3 for two reasons. First, the productivity story cannot explain the effects on overtime workers. They are more strongly attached to the labor market and possibly more productive than part-time and full-time workers. Second, the productivity explanation expects that lower-hour workers will be hit harder. However, further analysis in Appendix B.6 does not support this hypothesis. In Appendix B.6, I divide the part-time workers into more detailed groups and estimate Equation (2.2). The results suggest that negative effects are almost the same in magnitude for part-time workers with different hours of work. For instance, in Appendix Table B.9, those who

[22] Jardim et al. (2022) define the less experienced workers by those whose total working hours in the three quarters prior to the increase are lower than the median.

work 20 hours per week or less lose their hours by approximately 4.9 percent, and those who work between 20 and 35 lose their hours by 4.7 percent. Therefore, differences only appear between part-time and full-time. In contrast to the productivity explanation, the kinked labor demand model fits well into the results. In Section B.6, elasticity is roughly identical for all part-time workers regardless of their initial working hours. The large difference in responses is expected to be found only from the kinked point, as shown in Table 2.3 and Appendix B.6.

Unlike columns (2) and (3), column (4) does not show any clear sign of employment effects along the extensive channel. Except for the weak positive effects on the probability of working of the part-time workers, all estimates are neither statistically significant nor economically meaningful. These estimates lead to the estimates for unconditional hours of work statistically indistinguishable from 0 in column (5), except for the overtime workers.

Full-time workers in the lower wage group are positively affected in terms of hours, although the size is small and estimates are not highly precise. They gain hours by approximately 30 minutes per week or 2 percent. Combining negative estimates on the lowest part-time workers and positive effects on the lower group full-time workers suggested the possibility of labor-labor substitution discussed by Clemens et al. (2021) and Neumark and Wascher (2011), although the evidence is not strong. Note that wage effects are larger for the lowest group workers, while productivity could be higher for the full-time workers in the lower wage group. Given the wage effects, workers who are more productive become relatively cheaper, and employers may shift hours to those workers.

The measurement issues which may affect the estimates are worth discussing. Since overtime workers may be exposed to temporary shocks to the working hours or positive, nonclassical measurement errors in working hours, one may wonder whether regression to the mean may derive the results. As shown in Appendix Table B.1, a larger portion of overtime workers turn to full-time workers in the following year, consistent with the regression to the mean. However, I do not compare overtime workers with other workers, but compare overtime workers experiencing minimum wage increases with those who are not. Therefore, regression to the mean is less likely to affect the results unless minimum wage increases are correlated to the aforementioned factors

Table 2.4 Minimum Wage Effects on Hours-of-Work Distribution

	Work in Both Year			
	$h^2 \in (0,35)$	$h^2 \in [35,40)$	$h^2 = 40$	$h^2 > 40$
	(1)	(2)	(3)	(4)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in (0,35))$	0.025	0.013	-0.036*	-0.001
	(0.016)	(0.012)	(0.016)	(0.006)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [35,40])$	0.003	-0.004	0.002	-0.001
	(0.014)	(0.012)	(0.015)	(0.008)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 > 40)$	0.005	0.018	0.074+	-0.097**
	(0.021)	(0.016)	(0.042)	(0.033)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in (0,35))$	0.011	0.012	-0.016	-0.007
	(0.016)	(0.011)	(0.017)	(0.005)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [35,40])$	-0.001	0.002	-0.018	0.017+
	(0.014)	(0.015)	(0.017)	(0.010)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 > 40)$	0.038	-0.002	-0.097*	0.060+
	(0.026)	(0.014)	(0.038)	(0.035)
Observations	74,150	74,150	74,150	74,150
Controls	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, in other words, initial wage minus initial minimum wage. h^1 refers to working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

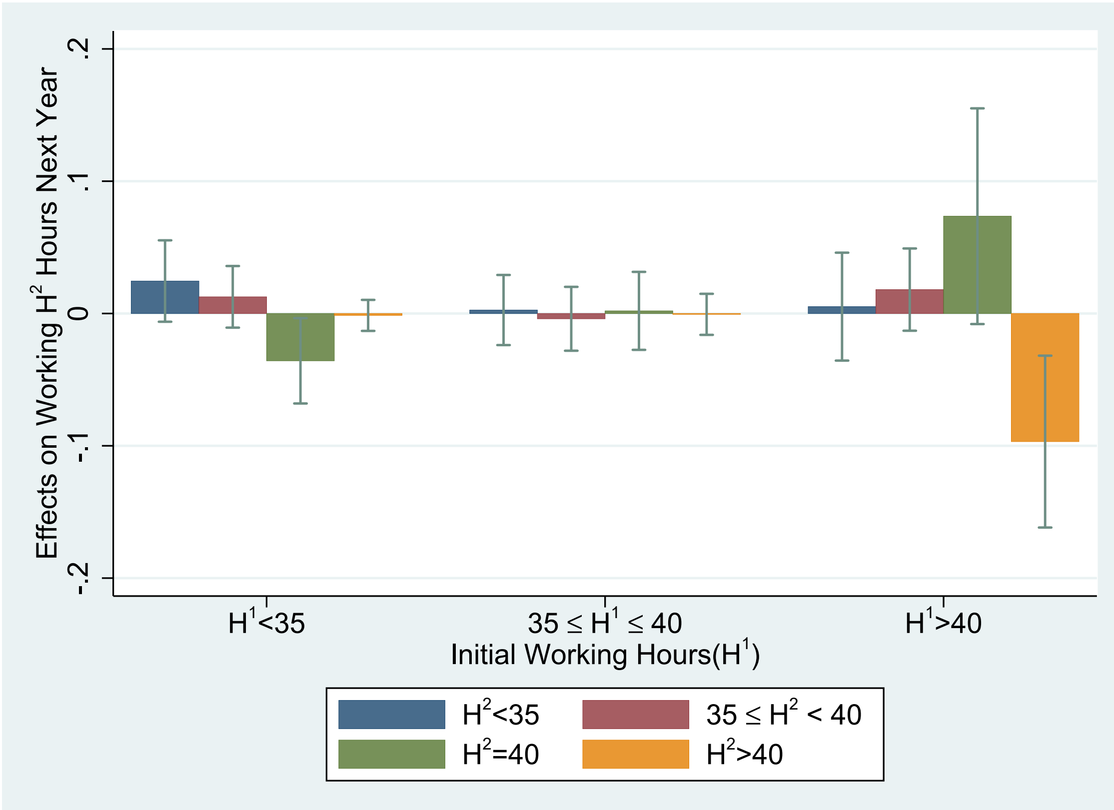
affecting working hours.[23]

So far, I have examined the effects on working hours. Although the patterns are largely consistent with the predictions, I do not examine the effects related to the kink point itself. Here, I provide more direct evidence involved with the kink by showing the effects on the probability of working a specific amount of hours, e.g., overtime or 40-hour work week. I estimate the Equation (2.2) by using $\boldsymbol{I}(h^2 > 40), \boldsymbol{I}(h^2 = 40), \boldsymbol{I}(h^2 \in [35,40))$, and $\boldsymbol{I}(h^2 \in (0,35))$ as outcome variables. The first outcome, $\boldsymbol{I}(h^2 > 40)$, can be connected to the overtime pay literature, since it estimates the effects on the overtime incidence. Table 2.4 shows the estimates, and Figure 2.2 visualizes the effects. The full table including the estimates from equation (2.1) is in the Appendix B.2.

The results in Table 2.4 and Figure 2.2 are again consistent with the prediction. First, the

[23] Another issue is measurement error in hours which may affect the results by misclassifying salaried workers. In Appendix B.7, I additionally replicate the analysis using only hourly paid workers in the first year. For part-time and full-time workers, results are almost identical to those in Tables 2.2 and 2.3. However, I lose precision for overtime workers due to the smaller sample size. Note that more than half of the overtime workers are salaried workers. 95 percent confidence interval of the coefficient on the minimum wage overtime workers' hours of work includes both negative 3 and positive 3, and wage effects are also imprecisely estimated.

Figure 2.2 The Effects on Hours-of-Work Distribution by Initial Working Hours



Notes: It shows the coefficients and 95% confidence intervals of the effects of minimum wage increases on the directly affected workers' hours-of-work distribution by workers' initial working hours. H^1 and H^2 refer to the hours of work at the first and second interview, respectively. See text for details.

probability of working 40 hours per week decreases for part-time workers by 3.6 percentage points when the minimum wage is increased. Figure 2.2 visually emphasizes that they are instead working a smaller number of hours. This result is consistent with the prediction of Section 2.3, which suggests that part-time work would be more prevalent, and some workers would move out from the kink point.

The effects on overtime workers are also consistent with the prediction. They are less likely to work overtime and shift to 40-hour per week positions. Estimates show that the probability of staying in overtime positions decreases by 9.7 percentage points, which is larger than one-fifth of the total probability of staying in overtime jobs. This result provides evidence that higher (straight-rate) wages are likely to reduce the incidence of overtime work. However, effects on becoming overtime workers for part-time or full-time workers are estimated to be zero. Since becoming an overtime worker is very uncommon for part-time and full-time workers, this result is reasonable. The prediction in Section 2.3 showed that larger bunching at the kink is only relevant for potential overtime workers; hence, part-time and full-time workers are not affected. This further explains the lack of evidence on the effects of the share of overtime workers in Cengiz et al. (2022).

Finally, the bottom half of column (4) of Table 2.4 shows the shift from 40-hour positions to overtime from full-time and overtime workers in the lower wage group. The last row of column (4) shows that overtime workers in the lower wage group are more likely to stay in the overtime positions by 6 percentage points. Combined with the adverse effects of working overtime from the lowest wage group, it may suggest the labor-labor substitution toward more productive workers. However, the effects on working hours are minimal for these groups in Table 2.3, suggesting that the overall effects could be small.

2.7 Discussion and Conclusion

In this study, I explore the effects of minimum wage increases on short-run adjustments in labor market outcomes by combining longitudinal data from the CPS-ORG and 107 large-scale state-level minimum wage increases. The results show that employers adjust hours in response to minimum wage increases, and different adjustments were made for different types of workers. Part-time and overtime workers lose working hours, and negative effects on working hours offset much of wage gain. Full-time workers do not experience negative effects on hours and they also enjoy wage gains. I find that minimum wage increases and overtime pay regulation interact with each other and discourage the use of overtime workers. Affected overtime workers shift toward 40-hour per week positions, and part-time workers are more likely to stay in part-time jobs.

The analysis in this paper suggests that part-time and overtime minimum wage workers experience negative effects on their labor-market outcomes. To see how my estimates can be connected to the existing literature focusing on specific demographic subgroups, I show the portion of demographic subgroups among minimum wage part-time, full-time, and overtime workers in Table 2.5, and the share of part-time, full-time, and overtime workers among selected groups in Table 2.6.

Table 2.5 Demographic Subgroup by Hours of Work

	$w^1 \leq MW + 0.5$, first year observation		
	Part-time	Full-time	Overtime
Teenager	0.403	0.070	0.028
Low-educated Prime-age Men	0.017	0.114	0.120
Low-educated Prime-age women	0.050	0.124	0.062
Low-educated Single Mothers	0.191	0.216	0.132
Low-educated Male Immigrants	0.017	0.114	0.120
Better-educated Prime-age Men	0.072	0.167	0.355
Better-educated Prime-age Women	0.185	0.277	0.206
Observations	12608	6664	1193

Notes: All results are weighted by earnings weight *earnwt*. 'Low-educated' is defined as those whose education level is lower than a high-school diploma, and 'Better-educated' is defined as those who have a high-school diploma or higher educational attainment (including some college, B.A., and advanced degree). Prime-age is 25-55. Immigrants are defined as those whose place of birth is not United States.

Table 2.6 Hours of Work by Demographic Subgroup

	$w^1 \leq MW + 0.5$, first year observation							
	Whole	Teenagers	Low-educated				Better-educated	
			Prime-age Men	Prime-age Women	Single Mothers	Male Immigrants	Prime-age Men	Prime-age Women
Part-time	0.603	0.905	0.187	0.398	0.610	0.186	0.358	0.513
Full-time	0.339	0.089	0.689	0.554	0.355	0.689	0.470	0.432
Overtime	0.058	0.006	0.124	0.048	0.035	0.125	0.171	0.055
Observations	20465	5856	959	1455	2032	934	2456	4681

Notes: See notes for Table 2.5

Only the first-year observations of the lowest wage group workers were used.

On the one hand, negative effects on the part-time workers suggest that teenagers and single mothers might be the most vulnerable to the minimum wage increases. Teenagers have been the most extensively studied in the literature, but analysis suggests that they are not representative of minimum wage workers. Rather, they are the group most concentrated in part-time jobs, so they might be the most likely to be negatively affected in terms of working hours. Hence, focusing on teenagers may provide a biased view, given the differential effects of minimum wages depending on the hours of work. Another group among whom part-time work is prevalent is single mothers; this suggests that they may be negatively affected as well.

On the other hand, negative effects on the working hours of overtime workers point to a very different group to be affected by the minimum wages. Prime-age men make up nearly half of the minimum wage overtime workers. Although most prime-age men are full-time workers, there may exist a small minority experiencing large negative effects on their working hours.

Then how many workers would be affected? I use the simple back-of-the-envelope-calculation to see how many workers would be affected, and how many hours would be reduced. According to the BLS minimum wage report, in 2018, the number of hourly paid workers whose hourly earnings are at or below the federal minimum wage is approximately 1.7 million (U.S. Bureau of Labor Statistics, 2019). 885,000 of them were part-time workers and 50,000 worked overtime. However, this calculation substantially underestimates the number of affected workers for two reasons: first, in 2018, 29 states and the District of Columbia had their own minimum wage higher than the federal; second, hourly paid workers made up only a small fraction of minimum wage overtime workers.

I calculate the fraction of hourly paid workers whose hourly earning is at or below federal minimum wage using first-year observations of the lowest wage group workers in 2018. In my sample, only around 16 percent of part-time workers and 6 percent of overtime workers in the lowest wage group were hourly paid workers at or below the federal minimum wage. Using these percentages, I estimate the number of the part-time and overtime workers in the lowest wage group to be roughly 5.5 million and 800,000, respectively. Appendix Table A1 showed that 75 percent of part-time workers and 90 percent of overtime workers continued to work in the following year.

Combining the above information with estimates in Table 3, a back-of-the-envelope calculation suggests that if all states increase their minimum wage by 10 percent (approximately the median of the size of increases in the data), the sum of hours lost would be approximately 4 million hours per week ($5.5 \times 0.75 \times 1$) for the part-time workers, and slightly more than 2 million hours per week ($0.8 \times 0.9 \times 3.3$) for overtime workers. In summary, although the number of the overtime minimum-wage workers is relatively small, their importance is non-negligible because each overtime worker loses a relatively large number of hours if subjected to a minimum wage increase.

Overall, the results in this paper suggest that economists need to more broadly consider the effects of the minimum wages on various economic outcomes, rather than focusing on the change in the head count of workers. Economists have widely considered that testing whether the minimum wage reduces the number of jobs is testing neoclassical economics itself. However, when firms can change output prices and other nonwage aspects of jobs (including working hours and overtime benefits discussed in this paper), minimum wage does not necessarily reduce the number of jobs. Recently, Clemens (2021) points out that "margins including nonwage job attributes can have first-order implications for analyses of minimum wages (p.52.)" and review the small body of economic literature on other margins, including output prices, noncash compensations and other job attributes. This paper is in line with Clemens (2021)'s argument and suggests that minimum wage literature needs to expand the scope of outcomes of interest to a variety of aspects of jobs.

CHAPTER 3

DOES WORKING LONG HOURS PAY? THE LONG-HOURS WAGE GAP, 1979-2018

Disclaimer

This chapter was co-authored with Steven J. Haider. Haider has approved that this work be included as a chapter in my dissertation.

3.1 Introduction

It has long been recognized that the hourly wage rate may vary systematically with the number of hours worked. Perhaps the most noted regularity is that there exists an hourly wage decrement for part-time work as compared to full-time work, and to the extent that this variability characterizes the menu of wage-hours packages faced by an individual, such tied wage-hours offers can have profound effects on labor supply decisions.[1]

Less studied is the differential hourly wage paid to those working very long hours.[2] The popular press has recently featured articles that have noted a substantial change in the relative hourly wage paid to those working long hours, which we reproduce in Panel A of Figure 3.1 (Miller, 2019). Between 1979 and 2018, the hourly wage differential paid to those working long hours went from a 10% decrement to a 10% premium for men and women (details provided below). Several recent papers have examined the extent to which differential pay by hours is important to the gender pay gap (Cortés and Pan, 2019; Goldin, 2014; Denning et al., 2022), patterns of household specialization (Cha, 2010; Chung, 2020), and labor supply more generally (Bick et al., 2022).

In this paper, we first document the long-hours wage gap and its change over the last four decades, paying particular attention to numerous measurement issues that arise. We then directly examine the extent to which the long-hours wage gap reflects the actual wage-hours package faced by individuals. To do so, we make use of detailed information on overtime availability, overtime pay, and within-occupation wage variation.

Our findings are three-fold. First, the wage gap between those working very long hours (50+

[1] For example, see Moffitt (1984); Lundberg (1985); Aaronson and French (2004); Hirsch (2005).

[2] Important exceptions include Kuhn and Lozano (2008); Cortés and Pan (2019), and Cha and Weeden (2014).

hours per week) as compared to those working a standard work week (40 hours per week) has gone from being strongly negative in 1980 to being strongly positive in 2018, and this trend becomes even stronger when important measurement issues are addressed. Second, at the individual level, a long-hours premium currently exists for about 95% of hourly workers and 40% of salary workers within their current job because of overtime regulations, but only about 20% of hourly workers and 10% of salary workers earn overtime pay. Third, if were to define the individual premium to be the entire within-occupation long-hours premium, then most workers would earn an hourly wage premium by working more hours, but it is unclear whether such a broad definition is appropriate.

We make several contributions to the literature. First, we carefully examine the potential role of measurement issues in estimating the hourly wage premium for working long hours, providing novel results regarding the differential amount of measurement error in hours for hourly workers and salary workers. This differential measurement error appreciably affects our results. Second, while other studies have examined the potential hourly wage premium paid to those working long hours, previous studies have not examined the importance of overtime regulations, and many studies even excluded overtime pay from the analysis. Third, we provide novel evidence on the availability of overtime from PSID.

The rest of this chapter is organized as follows. The second section reviews the relevant literature, and the third section describes our data. The fourth section considers whether those who work long hours earn more than those who do not, making use of four decades of CPS data. The fifth section then carefully considers whether a premium exists for individuals, examining the existence of overtime pay on the current job and the potential for additional pay by switching jobs within ones own occupation. The final section concludes and discusses our results.

3.2 Background

The key regulation that directly affects whether those who work long hours are paid more is the overtime regulations, usefully summarized in Brown and Hamermesh (2019). The federal overtime regulations, which have changed very little since being established as part of the Fair Labor Standards Act of 1938, specify that qualifying workers be paid at least a 50% hourly wage premium for every hour worked over 40 hours per week. Several states have passed stricter regulations that mandate the 50% hourly wage premium be paid on hours beyond 8 hours per day (e.g., Alaska, California, Colorado, and Nevada). The primary changes that have been made to the regulations are with respect to which workers qualify and the earnings level at which salary workers are presumed to be exempt from the regulations.

As stressed in Brown and Hamermesh (2019), there is very little research on the effects of overtime legislation in the United States, presumably because the law has changed so little. One important exception, using variation induced by a California state law that requires firms to pay overtime on all hours worked over 8 hours per day, finds that the application of the law induces fewer individuals to work more than 8 hours per day (Hamermesh and Trejo, 2000). Two other important exceptions suggest that the effects of overtime legislation may be more nuanced than a simple shift in labor demand away from long work weeks. Trejo (1991), using micro-level CPS data from the 1970s, finds that firms adjust both the number of individuals working over 40 hours per week and the straight-time hourly wage of affected workers, and Trejo (2003), using aggregate-level data from the 1970s and 1980s and industry-level variation based on law changes, finds little evidence that overtime law changes affected hours worked.

Several papers have examined the premium associated with working long hours more broadly. Kuhn and Lozano (2008) analyzes why individuals work long hours, directly examining the extent to which changes in working long hours is due to changes in the financial incentive to do so. Their results suggest that there the long-hours wage premium increased during the 1980s and 1990s, especially for salary workers.[3] Several papers have directly linked these incentives to work long hours to the gender gap in wages (Goldin, 2014; Cha and Weeden, 2014; Cortés and Pan, 2019; Denning et al., 2022).

Numerous papers have examined the strong effects that such varying wages can have on labor supply (e.g., Moffitt, 1984; Lundberg, 1985; Biddle and Zarkin, 1989; Aaronson and French, 2004).

[3] Other papers that document the change in the long hours premium include Cha and Weeden (2014), Weeden et al. (2016), and Cortés and Pan (2019). Bell and Freeman (2001) examine the extent to which within-occupation wage inequality leads to individuals within an occupation to working more hours.

More recently, Bick et al. (2022) has estimated a labor supply model that focuses on the possibility that there is a premium or penalty to working more than 40 hours a week.

Thus far, we have primarily focused on the contemporaneous returns to working long hours. Ben-Porath (1967) developed a model in which contemporary investments in human capital would pay off in later periods, and Imai and Keane (2004) and Keane (2011) provide structural estimates of such a model. Gicheva (2013) provides direct reduced form evidence that working long hours leads to higher wage growth.

3.3 Data

We primarily rely on two data sets for our analysis. The first is the Current Population Survey Outgoing Rotation Group (CPS-ORG) for the years 1979 to 2018. The CPS-ORG is a nationally representative, large-scale survey about the labor market. Compared to other surveys, it has several advantages. First, it contains a better quality measure of hourly wages, especially for hourly-paid workers for whom it directly asks their hourly wage to the penny. Another advantage of using CPS-ORG is its fairly consistent measure of hourly wages over time. Because of these advantages, it is widely used by studies on the premium associated with working long hours (e.g., Kuhn and Lozano, 2008; Cha and Weeden, 2014; Weeden et al., 2016; Bick et al., 2022).

For our analysis, we restrict our sample to workers aged 18-64 who are not self-employed and who report working 35-65 hours a week. This hours restriction is intended to abstract from the well-studied decrement paid to the part-time worker, as well as from the very few individuals who work extraordinarily long hours.[4] For hourly-paid workers, we use the direct report of the hourly wage. For salary workers, we divide weekly earnings by weekly hours of work; we directly consider the possibility of the so-called "division bias" of this adjustment below. Our baseline analysis adjusts top-coded weekly earnings by multiplying them by 1.4, following Cha and Weeden (2014); we examine the importance of this assumption in the next section. All analyses of the CPS is weighted, although none of our key regression results are affected by this decision. Wages are adjusted to 2018 dollars with the Personal Consumption Expenditure (PCE) deflator.

[4]Among workers who work 35 hours or more, only 1.24% work more than 65 hours.

Table 3.1 Sample Descriptives, CPS-ORG Select Years

	1979-81	1989-91	1999-01	2009-11	2016-18
Female	0.39	0.43	0.44	0.45	0.45
Age	36.70	37.15	38.98	41.07	41.05
	(12.42)	(11.20)	(11.18)	(11.78)	(12.17)
Education	12.82	13.29	13.45	13.86	14.11
	(2.77)	(2.71)	(2.73)	(2.77)	(2.77)
Real hourly wage	18.98	19.71	22.48	25.01	26.39
	(10.69)	(12.25)	(15.24)	(17.33)	(18.64)
Hourly worker?	0.55	0.56	0.56	0.53	0.54
Weekly hours 35-40	0.81	0.77	0.77	0.81	0.80
Weekly hours 41-50	0.15	0.18	0.17	0.14	0.14
Weekly hours 51-65	0.04	0.06	0.06	0.05	0.05
Fraction top-coded	0.01	0.01	0.01	0.02	0.04
Fraction imputed	0.17	0.04	0.33	0.36	0.41
Observations	440,296	424,590	367,691	364,453	360,841

Notes: These results are based on household heads from the CPS-ORG. All results are weighted. Standard deviations are in parentheses. Standard deviations of the indicator variables are not included in the table.

Following many of the existing studies, we use the CPS-ORG files provided by National Bureau of Economic Research (Various Years). While these files provide a well-documented and well-considered effort at harmonizing key variables over time, they do not include all of the underlying variables in the CPS-ORG. We supplement these files with data and code provided by the Centre for Economic Policy Research (CEPR). The CEPR similarly provides an excellent source of data and code for the CPS-ORG (Center for Economic and Policy Research, 2020).

The second data set is the Panel Study of Income Dynamics (PSID), a longitudinal study that began in 1968 with approximately 5,000 families. It has since followed these families and their direct descendants, interviewing them annually from 1968 through 1997 and biennially starting in 1999. In each wave, the core PSID survey collects information on labor supply, family structure, and income. As expected with a data collection effort that began so long ago, the questions and structure have changed over time. The key structural issue for our purposes is that population representative statements regarding the labor supply of women can only be made consistently after 1978.[5]

[5]While key labor supply information was collected for the head of household in every year regardless of gender, key labor supply information was only consistently collected for spouses starting in 1979. Combining this feature of the data with the PSID definition that the male spouse (when present) is the head of household implies that labor supply

Table 3.2 Overtime Pay in Main Job among Household Heads, PSID Select Years

	1979-83	1989-93	1999-03	2009-13
Panel A: Hourly Workers				
N	6,015	7,214	3,708	4,543
Female	0.23	0.29	0.31	0.33
Age	38.6	38.3	40.9	41.2
Education	12.1	12.6	13.0	13.4
Nominal hourly wage	7.64	11.46	17.82	28.1
Wage ratio for overtime on main job				
0†	0.07	0.06	–	–
(0.00,1.25]	0.06	0.08	0.10	0.07
(1.25,1.75]	0.82	0.74	0.79	0.76
(1.75,9.99]	0.03	0.06	0.01	0.01
Missing	0.02	0.06	0.10	0.15
Wage ratio for additional hours				
0†	0.65	–	–	–
(0.00,1.25]	0.08	–	–	–
(1.25,1.75]	0.23	–	–	–
(1.75,9.99]	0.02	–	–	–
Missing	0.02	–	–	–
Panel B: Salary Workers				
N	4,886	5,173	2,787	2,757
Female	0.21	0.26	0.25	0.26
Age	39.4	39.9	42.1	42.0
Education	14.5	14.8	15.1	15.4
Nominal hourly wage	10.72	16.96	28.18	37.01
Overtime wage on main job				
0†	0.71	0.76	0.81	0.83
(0.00,1.25]	0.07	0.05	0.05	0.07
(1.25,1.75]	0.19	0.13	0.11	0.09
(1.75,9.99]	0.01	0.02	0.01	0.01
Missing	0.02	0.03	0.03	0.01
Wage ratio for additional hours				
0†	0.80	–	–	–
(0.00,1.25]	0.10	–	–	–
(1.25,1.75]	0.06	–	–	–
(1.75,9.99]	0.01	–	–	–
Missing	0.04	–	–	–

Notes: These results are based on households heads from the PSID. All results are weighted. †This category represents workers who reported they would not be paid for any additional hours worked or who reported that no additional hours were available.

Similar to our CPS-ORG sample restrictions, we analyze workers aged 18-64 who are not self-employed and who report working 35-65 hours a week. Similar to the CPS-ORG, hourly workers are asked their hourly wage, which we use directly. Salary workers are allowed to give their salary over a time period which is convenient for them, and we compute their hourly wages based on a standardized hourly schedule (see the appendix for details). All analysis of the PSID is weighted.[6]

See the Appendix C for further details on all of our data.

3.4 How Has the Long-Hours Wage Gap Changed?

In this section, we document the change in the long-hours wage gap over the last four decades making use of the CPS-ORG.

3.4.1 Baseline Results

We begin by following the analysis strategy in Cha and Weeden (2014). Specifically, we estimate the following log-wage equation

$$\ln w_{it} = \beta_{0t} + \beta_{1t} long_{it} + \beta_{2t} Z_{it} + \epsilon_{it} \tag{3.1}$$

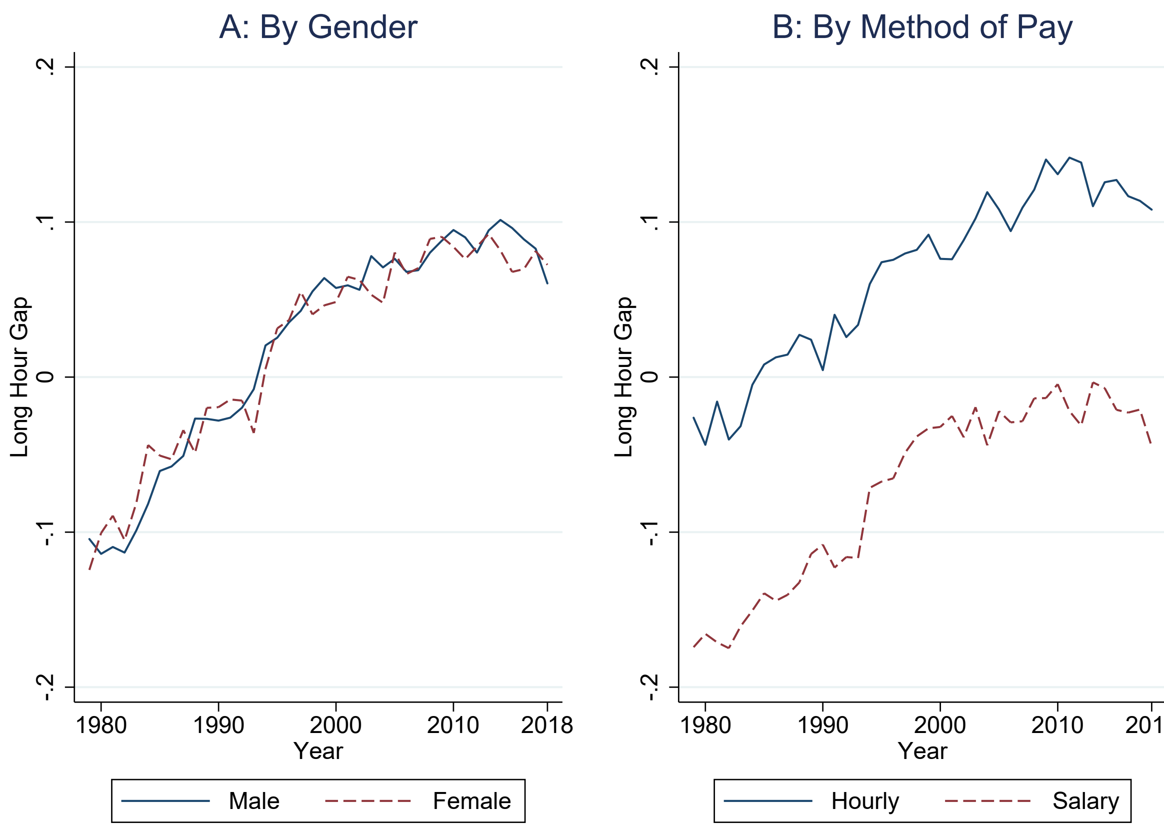
for each year t. The variable $long_{it}$ is an indicator variable for whether individual i works 50 hours or more in year t, and therefore, the parameter β_{1t} measures the log-wage gap between individuals working 50-65 hours relative to those working 35-49 hours in year t. The vector Z_{it} contains a quartic age and indicator variables for race, region, metropolitan area, and public sector workers. We also include four indicator variables for educational status (high-school graduates, some college education, college graduates, and advanced degrees). Importantly, in the NBER data, the hourly wage variable for hourly workers explicitly *excludes* pay stemming from overtime, tips and commission, whereas the hourly wage variable for salary workers explicitly *includes* overtime, tips and commission. We consider the empirical importance of this difference in the next subsection.

Turning to the results, Panel A of Figure 3.1 shows the long-hours wage gap β_{1t} for males and

information for married females is missing in many of the early years of the PSID.

[6]We use PSID data from the underlying nationally representative sample (i.e., the SRC subsample) and the "poverty" sample (i.e., the SEO subsample) to provide for a larger sample size. We thus apply weights throughout because the sampling probabilities are directly related to our outcome of interest, and not surprisingly, key PSID results are affected by this decision.

Figure 3.1 The Long-Hours Wage Gap, CPS-ORG 1979-2018



Notes: These results represent the regression-adjusted gap between in log-hourly wages between those individuals working 50-65 hours per week and those working 35-49 hours per week.

females separately. Our results closely mimic those of Cha and Weeden (2014). Whereas those who working long hours had wages about 10% less than those working 35-49 hours around 1980, long-hour workers earned about 10% more per hour around 2010. Furthermore, the gaps were very similar for males and females in each year. Based on this finding, we will pool males and females and include a gender indicator for the rest of our analysis.

Panel B of Figure 3.1 shows the same long-hours wage gap, but separates between workers who report being paid hourly versus those who report being paid a salary. While the trend in the long-hours wage gap is very similar between the two groups, the level is much different. Hourly workers who worked long hours were paid an hourly wage about 4% less than those working 35-49 hours around 1980, whereas the similar gap was about 18% less for salary workers. Both groups saw this gap move in the positive direction by about 15 percentage points over the following 40 years. Thus, by the end of our sample period, hourly long-hour workers were paid a wage about 10% more than hourly individuals working 35-49 hours, and salary long-hour workers were paid a wage about 5% less than their 35-49 hour salary counterparts.

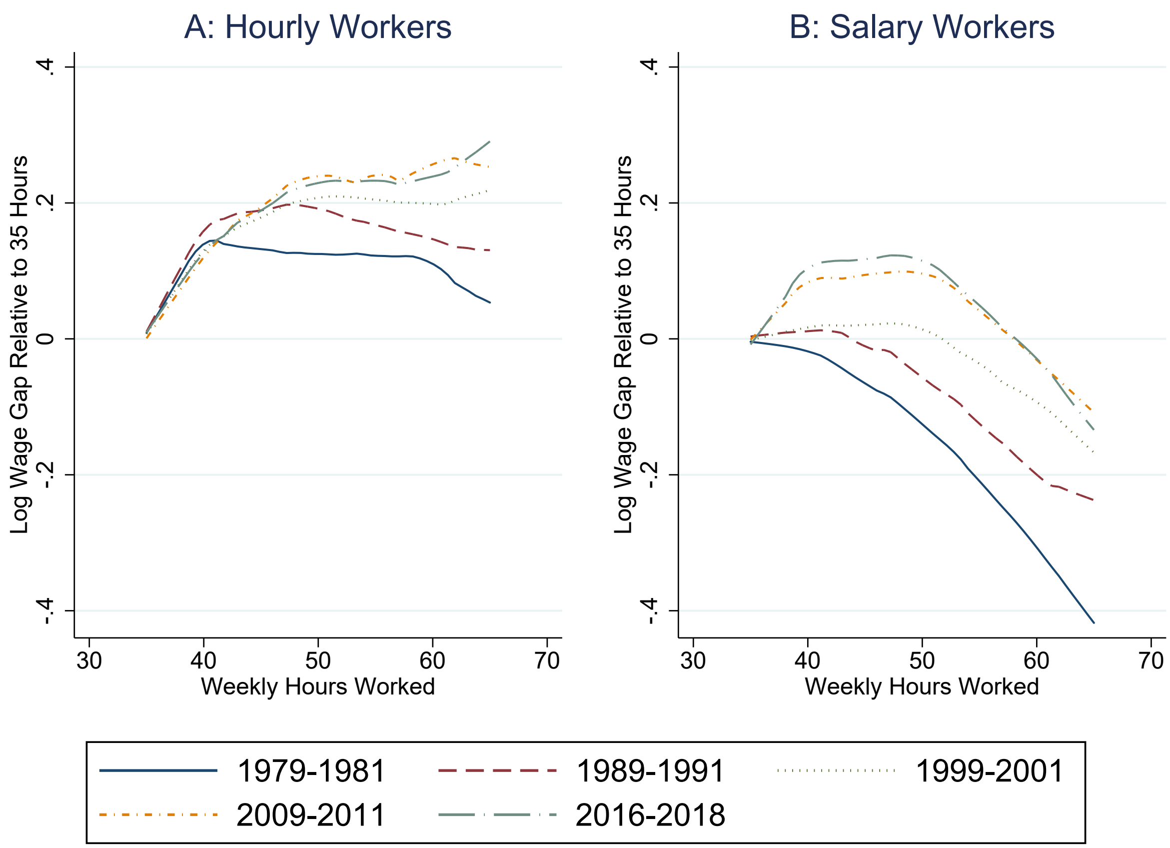
To provide further insight into the relationship between the hourly wage and hours worked, we show the (log) wage-hours locus for select years, separately for hourly workers and for salary workers. To do so, we first estimate a log-wage equation for each three year period similar to equation (3.1), but additionally include a gender indicator and year indicators. We then graph the relationship between the residuals from these log-wage equations and usual hours worked, normalizing the values to be zero for 35 hours of work.[7] Thus, the graphs show the log-wage for each hours worked as compared to those working 35 hours.

Turning to the results for hourly workers in Panel A of Figure 3.2, over the last decade wages increased strongly with hours worked when hours are increased from 35 to 50 hours: Hourly workers who were working 50 hours per week were being paid a wage over 20% more than those who were working 35 hours. For the 2009-11 years and the 2016-18 years, wages went up by another 5% when hours increased from 50 to 65. The monotonic relationship between wages and hours worked during the last decade represents an important departure from earlier years. In fact, for the years 1979-81, there was a monotonic decline in the wage with respect to hours worked for those who were working more than 40 hours.

The relationship between log wages and hours worked is substantially different for salary workers, as shown in Panel B of Figure 3.2. During the last decade, the hourly wage increased as workers increased hours from 35 to 40 hours by about 10%, and then the wage was relatively constant until 50 hours of work. After 50 hours of work, the wage dropped precipitously with hours worked, so that those who were working 65 hours per week were earning about 20% less than those working 50 hours a week. In earlier years, there was no increase in the wage even for those who increased hours between 35 hours a week.

[7] We use a local linear smoother with a kernel of 3.

Figure 3.2 The Log-Wage / Hours Locus, CPS-ORG Select Years



Notes: These results plot the residual from a log-wage regression against hours, normalizing the residuals to be zero at 35 hours.

3.4.2 Measurement Issues

Several important measurement issues arise regarding the results presented thus far. The first issue is that measurement error in hours will affect the results for hourly workers differently than for salary workers. For hourly workers, the hourly wage is elicited directly, so the measurement error concern is that the key explanatory variable, the usual weekly hours worked variable, is measured with error. The results for salary workers are plagued by this problem, but they are also plagued by an additional one. Salary workers are asked about their weekly earnings over whatever time frame is convenient, such as daily, weekly, monthly, or annually, and then converted to weekly earnings. These weekly earnings are then converted to an hourly wage based on the worker's usual weekly hours worked. Therefore, the dependent variable for salary workers is adjusted to an hourly wage based on the same error-ridden measure of usual hours worked. The use of the error-ridden hourly wage on the left-hand side can lead to an additional bias, often called "division bias", when compared to the series for hourly workers.

Fortunately, there are numerous, high-quality validation studies of the measurement error in the responses to questions on hours worked, summarized in Bound et al. (2001). To incorporate such information directly, we present one additional measure of how wages vary with hours, the wage-hours elasticity α_{1t} that is obtained from a regression of the log-wage on log-hours:

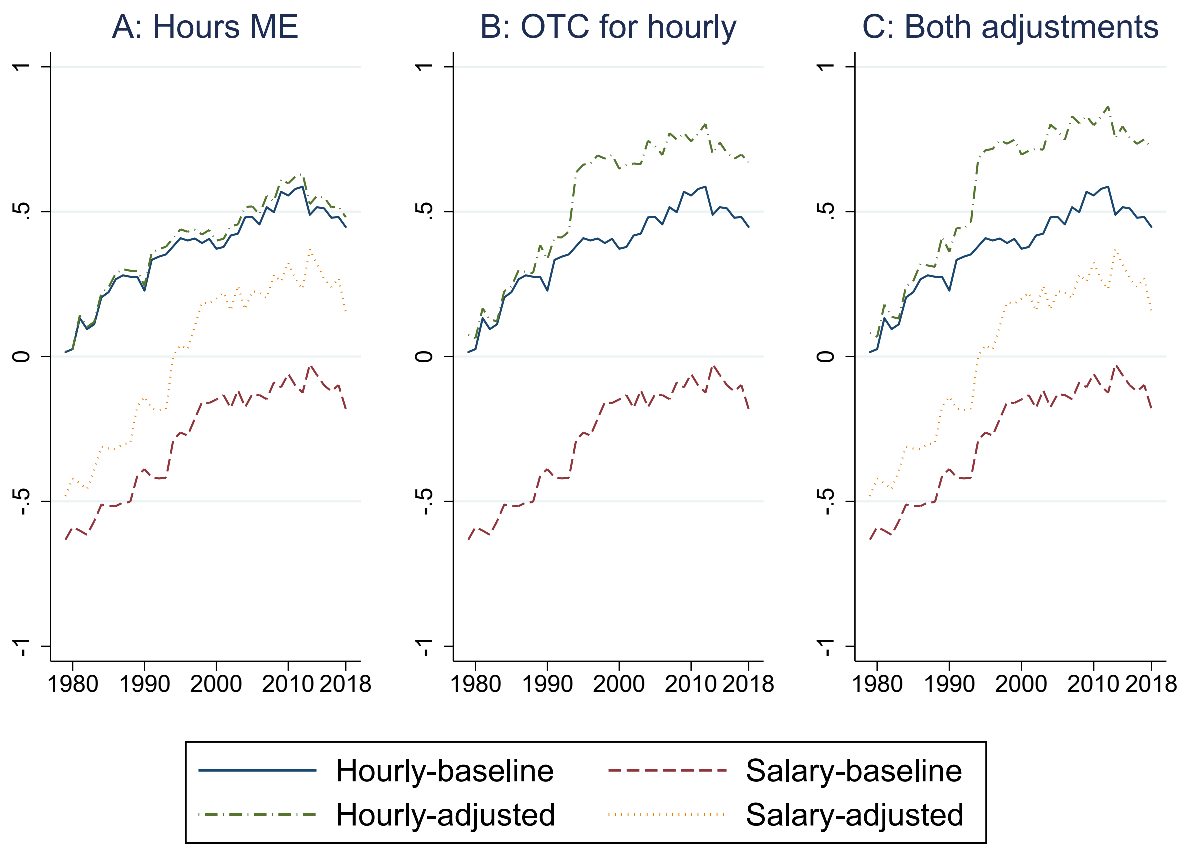
$$\ln w_{it} = \alpha_{0t} + \alpha_{1t} \ln h_{it} + \alpha_{2t} Z_{it} + \epsilon_{it} \tag{3.2}$$

The wage-hours elasticity can be adjusted for measurement error based simply on the "reliability ratio," which is frequently reported in validation studies. The adjustment for hourly workers only accounts for the fact that the key independent variable h_{it} is measured with error, but the adjustment for salary workers additionally takes into account that the wage variable w_{it} is obtained by using the same error-ridden measure of hours that is used as the key independent variable. These adjustments allow for mean reversion in measurement error by only relying on the assumption that the measurement error in hours is uncorrelated with the idiosyncratic component of wages, which is weaker than the more standard "classical measurement error" assumption.[8] An additional benefit of presenting the wage-hours elasticity is that it is used in several other studies of how wages vary with hours (e.g., Cortés and Pan, 2019; Denning et al., 2022).

What value should be used for the reliability ratio for hours worked? While most studies do not report the reliability ratio separately for hourly and salary workers, it would seem likely that weekly hours would be reported with greater accuracy for hourly workers because hourly workers must routinely report their hours worked. Using the same data analyzed in Angrist and Krueger (1999), we find results that are consistent with this conjecture. Whereas Angrist and Krueger (1999) report an overall reliability ratio of 0.87, we find that the reliability ratio is 0.93 for hourly workers and

[8] Letting b be the reliability ratio for hours, it can be shown that the regression of hourly wages on the noisy hours measure is $b\alpha_{1t}$ for hourly workers and $b\alpha_{1t} - b$ for salary workers. Thus, by assuming a value for b, we can then solve for α_{1t}. See the appendix for further details.

Figure 3.3 The Wage-Hours Elasticity and Measurement Issues, CPS-ORG 1979-2018



Notes: These results plot wage-hours elasticities. "Baseline" results use the same data as that from Section 3.5.1. The "Adjusted" series in Panel A adjusts for measurement error in hours. The "Adjusted" series in Panel B includes overtime, tips and commission earnings for hourly workers. The "Adjusted" series in Panel C applies both adjustments.

0.71 for salary workers.[9]

Panel A of Figure 3.3 shows a baseline wage-hours elasticity series that uses the data from the previous section and ignores these measurement error concerns. As expected given the results in the previous two figures, the series for hourly and salary workers are strongly increasing, but the hourly worker series is shifted upwards as compared to the salary worker series. In the later years of our sample, the elasticity is positive and significantly greater than zero for hourly workers, but it is slightly negative (and statistically indistinguishable from zero) for salary workers.

Panel A of Figure 3.3 also shows an alternative series that adjusts for measurement error based

[9]Our results are based on re-analyzing their data, but simply separating between hourly and salary workers. The estimated difference of 0.22 (SE=0.03) is significantly different from zero at conventional levels. Alan Krueger generously provided the data and code that allowed us to produce these results.

on the formulas in the appendix and our reliability ratio estimates from CPS validation data. The results indicate that the elasticity for hourly workers were attenuated towards zero by 7% because of the measurement error in hours (i.e., the reliability ratio is 0.93). The results for salary workers are more complicated than simple attenuation/amplifcation because of the two adjustments that are being applied. The net adjustment for salary workers shifts the series upwards by 0.29 log points on average, reducing the mean gap between hourly and salary workers from 0.65 log points without any measurement error adjustments to 0.38 log points after applying the adjustments.[10] Overall, adjusting for measurement error suggests that the trend towards a larger premium to working long hours is even greater than that documented in the previous figures, and especially so for salary workers. Indeed, by the end of our sample period, the wage-hours elasticity is positive even for salary workers.

A second measurement issue is that the NBER-provided wage for hourly workers explicitly excludes overtime, tips, and commission, whereas the wage for salary workers explicitly includes such earnings. This omission not only affects the comparisons between hourly and salary workers, but it also ignores a key incentive to working additional hours: Individuals may receive overtime pay or earn more commission. CEPR provides an additional wage series that includes overtime, tips, and commissions for hourly workers from 1994 onwards.[11] We present the results for this hourly wage series in Panel B of Figure 3.3 (and repeat the wage series from Panel C for salary workers for comparison).

As expected, the benefits for working longer hours are much larger for hourly workers starting in 1994 when overtime, tips, and commission can be included, reaching a log-point gap of about .7 at the end of our sample period. The results in Panel B also foreshadow results in our next section:

[10]See Bound et al. (2001) for a summary of validation studies on self-reported hours worked. Using the overall reliability ratio from the CPS validation data (0.870, based on Angrist and Krueger, 1999), the gap between hourly and salary workers is 0.60 on average. Using the reliability ratio from the PSID validation data (0.683, based on Bound et al., 1994), the same gap is 0.48 on average.

[11]This wage series for hourly workers is constructed as follows. For the period 1979-1993, CEPR calculate an hourly wage by dividing usual weekly earnings by usual hours per week, and then use whatever wage is larger–this calculated hourly wage or the reported hourly wage. Before 1989, this correction has little effect on the estimated wage (Schmitt, 2003) Starting in 1994, the CPS directly asks workers “Do you usually receive overtime pay, tips, or commissions?” and, if the response is affirmative, the amount. The responses to these questions are used to construct overtime, tips, and commission per usual hour of work and added to the hourly wage.

While there is a substantial premium with working long hours captured in the overtime, tips, and commission part of pay (the point we just focused on here), there is a substantial premium for working long hours in the portion of pay that is supposed to exclude such pay. Thus, even for hourly workers, the premium associated with long hours is not just about receiving reported overtime pay.

In the appendix, we show results for two additional measurement issues that matter very little. One stems from survey non-response. Like many household surveys, the CPS-ORG provides "allocated" or "imputed" data when a respondent does not provide an answer to a question. Such non-response is frequent and increasing over time.[12] For our sample, the fraction with imputed wages or hours is less than 20% in the early years, but increased to over 40% in the later years (see Table 3.1). Despite the large change in the fraction of data that are imputed, throwing out imputed data matters very little to our results, which should not be surprising because work hours is used in the imputation process (Hirsch and Schumacher, 2004). The other stems from changes in top-coding over time. Overall, top-coded data are less of an issue than imputed data, with only about 1% of workers top-coded in the early years and about 4% in the last years (see Table 1), although top-coding varies tremendously between hourly and salary workers.[13] Just as with imputations, our results suggest that top-coding affects our results very little.

3.5 Do Individuals Face a Long-Hours Premium?

While we have documented a substantial change in the relative pay of those working long hours, the results do not necessarily tell us whether a particular individual receives a higher hourly wage (or any wage) for working additional hours. In this section, we attempt to answer that question directly: How does an individual's hourly wage vary when they work more hours?

The first two subsections focus on answering this question by only considering whether the wage changes when an individual remains in their current job. The third section additionally considers whether an individual could increase their hourly wage through working additional hours by sorting to a different employer.

[12] See Hirsch and Schumacher (2004) for a detailed analysis of survey non-response in the CPS-ORG. See Meyer et al. (2015) for a useful discussion of trends in non-response and household survey quality more generally.

[13] No more than 0.2% of hourly workers top-coded in any year, but upwards of 10% of salary workers are top-coded in several years.

3.5.1 Overtime Evidence from the CPS-ORG

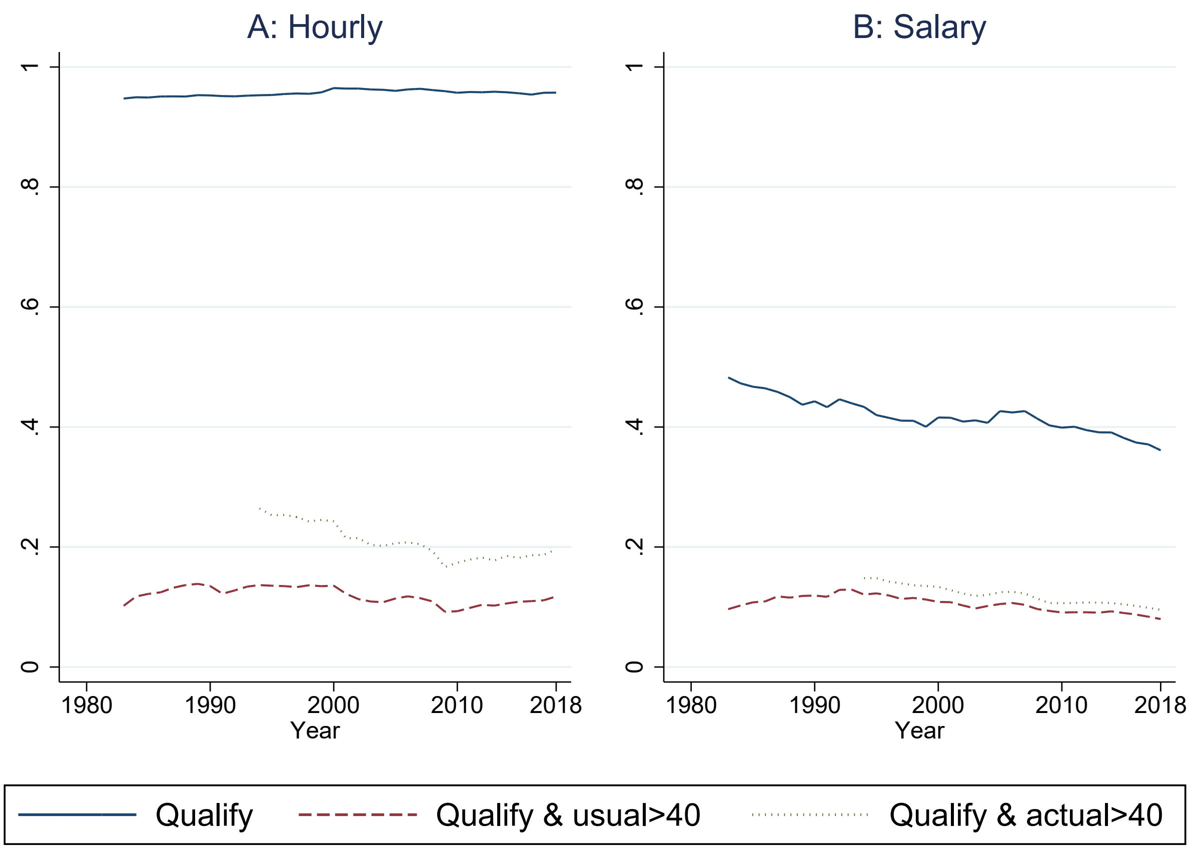
Using the CPS-ORG, we estimate the percentage of individuals who are "covered by and nonexempt from" overtime pay regulations–those individuals who are covered by the overtime provision of the Fair Labor Standards Act (FLSA) and do not fall into one of the exempt categories. In principle, these individuals must be paid at least time-and-a-half if they work more than 40 hours per week. To do so, we follow the Department of Labor's (DoL) procedures used in numerous analyses of overtime law changes.[14] This procedure entails using industry, occupation, sector and location information to first assess who is covered by the relevant overtime provisions of the FLSA (e.g., workers in religious occupations and most federal workers are not covered), and then exclude covered workers who are exempt from the overtime provisions because they are working in a particular occupation/industry (e.g., certain agricultural, fishery, and transportation workers) or because they are salaried workers in occupations in which they are likely to meet the "executive, administrative, and professional" (EAP) or "highly compensated employees" (HEC) duties test.[15]

Figure 3.4 shows the fraction of full-time hourly (Panel A) and salary (Panel B) workers that are estimated to qualify for overtime payments (i.e., workers covered and non-exempt from FLSA overtime pay requirements) to being paid at least time-and-a-half when working more than 40 hours per week. Most hourly workers are subject to being paid overtime, with 96% being subject from 2005 to 2018. The percent for salary workers is much less and declining: 43% were subject to paying overtime wages in 2005, whereas only 36% were in 2018. The decline in salary workers subject to being paid overtime wages is due to the fact that the salary test for the EAP provision was

[14]These procedures were used in several regulatory impact analysis to assess the effects of proposed changes in the overtime provision, including the 2004 increase in the salary test (Department of Labor, 2004), the contested 2016 Obama-era increase in the salary test (Department of Labor, 2016), and the most recent increase the salary test (Department of Labor, 2019). While the methods appear to be very similar for all three analyses, we follow the most recent.

[15]More specifically, in certain "named" occupations, such as certain medical, legal and educational occupations must meet just the EAP duties test to be exempt from the overtime provisions. In the remaining occupations, workers must meet a "salary test" and the EAP duties test to be exempt. In these remaining occupations, from 2004 rule change, highly compensated employees (HCE), those who earn above $100,000, face a less-stringent duties test. Although work duties are not provided in the CPS-ORG to assess the duties on an individual basis, the DoL provides estimated probabilities that an individual would meet the duties test based on their occupation. We randomly select individuals by occupation according to these probabilities to meet the duties test. Because these probabilities are assigned at the occupation level, within-occupation comparisons should not be made regarding with respect to coverage/exemptions.

Figure 3.4 Overtime Coverage and Overtime Work, CPS-ORG 1979-2018



Notes: "Qualify" denotes workers who are covered and non-exempt from FLSA overtime regulations. "Qualify & usual>40" denotes workers who qualify for overtime pay and report usually working more than 40 hours per week. "Qualify & actual>40" denotes workers who qualify for overtime and report actually working more than 40 hours during the reference week.

fixed in nominal dollars at $455/week throughout this time period.[16] Taken together, these results suggest that 68% of salary and hourly workers in our sample were subject to be paid overtime, and thus their hourly wage was much higher when working more than 40 hours a week.

Of course, just because about 68% of workers were subject to being paid overtime wages does not mean they had the option of doing so. To provide evidence on how many individuals might have had the opportunity to earn overtime wages, Figure 3.4 also shows the fraction of workers who are subject to being paid overtime wages and who report usually working over 40 hours of week; these workers face a strong incentive to work long hours on their current job. Among hourly

[16]The criteria for salary levels test were fixed at $155/week from 1975 to 2004 and changed to $455/week in 2005. See Department of Labor (2004).

workers, this percentage was roughly constant from 2005 to 2018 at about 11%. This percentage was slightly lower for salary workers and declining (presumably due to the decline in coverage), ranging from 10% to 8% from 2005 to 2018.[17]

One final issue is that "usually" work overtime might be a fairly stringent requirement for workers to ever be allowed to earn an overtime wage. All individuals in the Outgoing Rotation Group are still asked the standard monthly CPS question about the actual number of hours they worked last week on their main job. Such a variable will capture those who usually work over 40 hours, as well as those happened to work 40 hours the previous week. As is apparent in Figure 3.4, this change increases the percentage of hourly workers who earned an overtime wage by a sizeable amount (from 11.8% to 19.7% in 2018), but much less so for salary worker (from 7.6% to 9.2% in 2018).[18]

So, do individuals earn a wage premium for working long hours? When just considering the premium that arises within jobs due to overtime regulations, it appears that 20% of hourly workers and 10% of salary workers do according to CPS data.

3.5.2 Overtime Evidence from the PSID

While the PSID has a much smaller sample size than the CPS, it possesses one substantial benefit for our purposes: It directly asks individuals about their pay if they worked additional hours. Hourly workers are asked directly about their overtime wage rate, and salary workers are first asked if they would be paid more if they worked more hours, with those who respond affirmatively being further asked their hourly wage rate for those additional hours.[19]

[17] As a useful check on the data, we compare these results to the fraction of workers who self-report that they usually are paid overtime, tips, and commission (OTC). For salary workers, these percentages are fairly similar: in 2018, 9.7% report receiving OTC and 7.5% are subject to being paid overtime and report usually working more than 40 hours a week. For hourly workers, the gap in the percentages is larger: the same percentages are 19.3% and 11.8%. At least some of this gap (and the difference in gaps) can be explained by OTC explicitly including tips and commission and the differential propensity to earn tips and commissions between hourly and salary workers. For example, 2.7% of hourly workers and 1.3% of salary workers in our sample are in "predominantly tipped" occupations (e.g., bartenders and wait staff), according to an occupational designation used in EPI (2014) and CBO (2014).

[18] As another potential indicator of the rationing of work hours, we examined second job holdings. While individuals who were subject to being paid overtime were significantly more likely to hold a second job (a regression adjusted difference of 0.9% (SE=0.1) compared to an overall rate of 3.9%), the overall rate is low enough to indicate that holding a second job is not an important part of alleviating potential hours constraints.

[19] Hourly workers are asked, "What is your hourly wage rate for overtime?" (Question B16, 1989). Salary workers are asked two questions: "If you were to work more hours than usual during some week, would you be paid for those

The results for PSID head-of-households are shown in Table 3.2. Starting with hourly workers in Panel A, we see that most workers report being paid in the "time-and-a-half" range if they were to work overtime in every time period. For example, 76% of hourly workers would earn in the time-and-a-half range for the years 2009-13. For the same time period, 7% of workers report not making a higher hourly wage for working overtime hours, which is fairly similar to the 4% estimate based on the CPS data. There is little evidence for large changes over time in the PSID.

Turning to salary workers, 83% of workers report that they would not receive any wages for working overtime for the years 2009-13. Only 10% report that they would be paid a higher hourly wage for working overtime hours during this last time period, a percentage that is much less than the 40% obtained with the CPS estimates, with most of these workers reporting an amount in the time-and-a-half range.[20] For salary workers, there is a clear downward trend in workers reporting that they would receive an overtime premium for working more than 40 hours a week, just as was found in the CPS-ORG.

While these overtime questions tell us about whether an individual would earn a higher hourly wage if they worked more, they do not tell us whether individuals have the option to work more. Thus, even though virtually all hourly workers report the existence of an overtime wage, none of them may have overtime work allowed by their employer. Before 1989, however, the PSID directly asked workers whether they could work more hours, and for those answering affirmatively, the wage they would be paid.[21] Importantly, the question does not specify whether the availability of more hours is at their main job, and thus, even among those who report that they could work more hours (and are currently working at least 40 hours) may not be paid the overtime wage. For our initial period (1979-84), 65% of hourly workers and 80% of salary worker report that they could not work

extra hours of work?" (Question B14, 1989) and "About how much would make per hour for those extra hours?" (Question B15, 1989). Cherry (2004) uses these PSID questions as well to examine whether working long hours increases pay for salary workers.

[20] From 1970 to 1992, workers were asked about the exact wage they would earn for additional hours worked. Most reported a wage ratio that was very close to 1, 1.5, or 2. Starting in 1993, workers were asked the wage ratio directly, with a ratio of 1.0 (i.e., "straight time"), 1.5 (i.e., "time-and-a-half"), and 2.0 (i.e., "double time") being selected by most workers. Workers put in the "Not Applicable" category include those who did not respond to the question and those who's hourly wage and/or overtime wage was top-coded.

[21] "Now thinking about your job(s) over the past year, was there more work available on (any of) your job(s) so that you could have worked more if you had wanted to?" (Question B91, 1987).

more hours for a positive wage. For hourly workers, 25% report that they could earn a premium for working additional hours, whereas the same quantity for salary workers is 8%. Despite the different time periods, these percentages are reasonably close to the estimates we obtained from the CPS, where about 20% of hourly workers and 10% of salary workers were earning a higher wage when working more than 40 hours per week.

3.5.3 Long-Hours Premium within Occupations

In the previous two sections, we have taken a fairly narrow view of a potential long-hours premium for an individual, focusing just on the premium that may arise for an individual working more hours at their current employer. However, different firms may adopt different production technologies, which could allow some firms to require an individual to work more hours, but then pay the individual a higher hourly wage. In such situations, individuals may have a long-hours premium available to them, but the premium would be obtained by moving to another firm. Indeed, this possibility has been the focus of most papers that examine whether or not a long-hours premium exists by examining the relationship between wages and hours within occupation (e.g., Cortés and Pan, 2019; Denning et al., 2022).

We start by estimating variants of the wage-hours elasticity in eq. (3.2). To do so, we pool across years 2012 through 2018 because a consistent occupational code is available for these years. We additionally include year indicators in all of these models to net out systematic wage changes across years.

Turning to the results in Table 3.3, column (1) shows the elasticity using the wage measurement that excludes overtime, tips and commission, but does not include occupation fixed effects. As mentioned previously, the wage measure for hourly workers in the NBER extract excludes these earnings for hourly workers, and thus, many studies that estimate the elasticity use this wage measure (e.g., Denning et al., 2022). The coefficient in column (1) implies that a 10% increase in hours worked leads to a 5% increase in the hourly wage (0.499, SE=0.007). Column (2) uses a wage measure that includes overtime, tips and commission, and as expected, the coefficient implies a stronger relationship (0.711, SE=0.007): a 10% increase in hours worked leads to a 7% increase

Table 3.3 The Wage-Hours Elasticity, CPS-ORG 2012-18

	(1)	(2)	(3)	(4)	(5)
Panel A: Hourly Workers (N=450,431)					
Log hours	0.499	0.711	0.610	0.656	0.654
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Panel B: Salary Workers (N=386,915)					
Log hours	–	-0.103	-0.200	0.126	0.122
	–	(0.007)	(0.006)	(0.009)	(0.009)
OTC included?	N	Y	Y	Y	Y
Occupation FE?	N	N	Y	Y	N
ME adjustment?	N	N	N	Y	Y
Occupation x College FE?	N	N	N	Y	Y

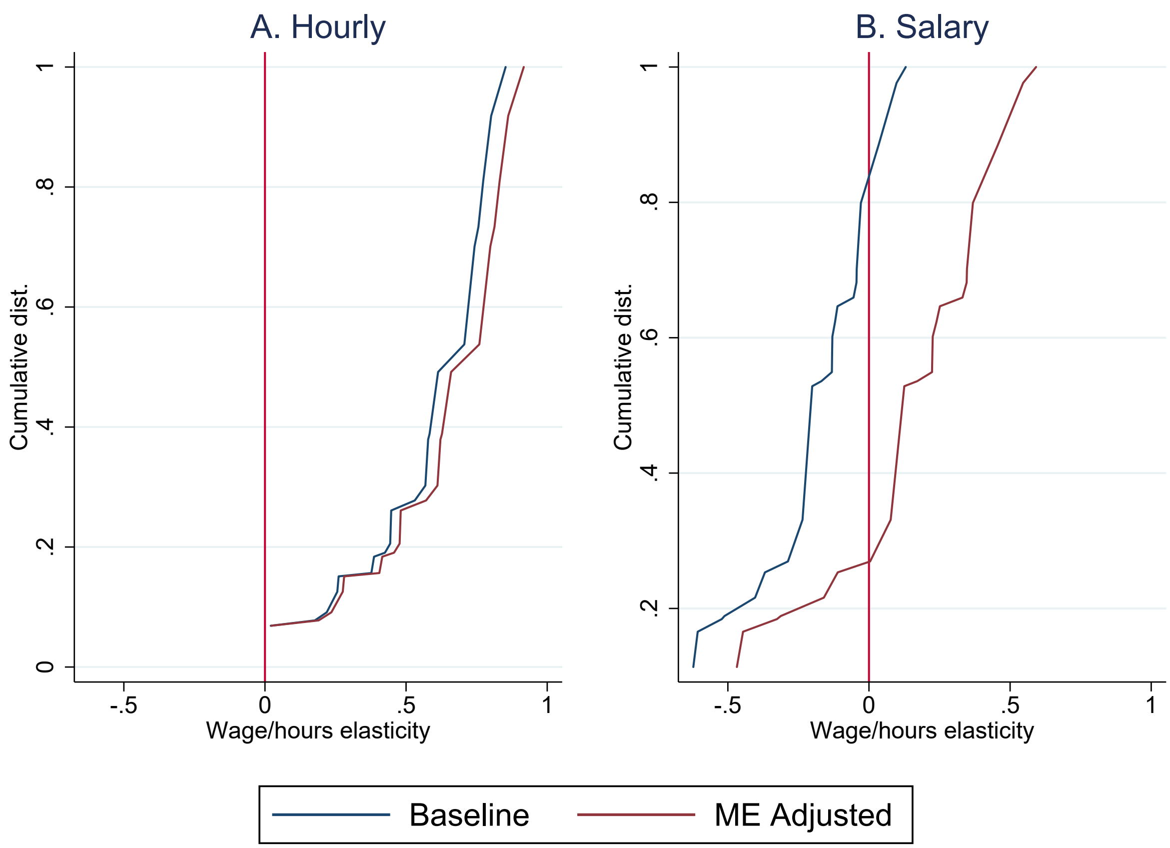
Notes: These results are based on household heads from the CPS-ORG, 2012-18. All results are weighted. Standard errors are in the parentheses.

in the hourly wage for hourly workers. The same coefficient for salary workers is negative (-0.103, SE=0.007), implying that a 10% increase in hours worked leads to a 10% decline in the hourly wage. These results suggest that the omitted the overtime, tips and commission for hourly workers appreciably changes the results, just as was observed in the previous section.

Column (3) includes the occupation fixed effects, and thus looks at the relationship between hours worked and the hourly wage within occupation. For both hourly and salary workers, the coefficients appreciably move more negative in both cases, but the qualitative findings remain: There appears to be a strong positive relationship between hours worked and the hourly wage for hourly workers (0.610, SE=0.007) and a strong negative relationship for salary workers (-0.200, SE=0.006). Column (4) then applies the measurement error adjustment described in the previous section, which moves the salary estimate to be strongly positive. Based on this column, there is a strong premium within occupations for hourly workers (0.656, SE=0.007) and salary workers (0.126, SE=0.009).[22]

[22]This finding is in contrast to those reported in Denning et al. (2022), from which they conclude there is no long-hours premium. The explanation for the differences are several fold. First, our estimates for hourly workers include overtime, tips and commission, which represents an important financial inducement to work long hours and makes the results for hourly workers more comparable to salary workers. Second, their adjustment for measurement error assumes that measurement error is classical. Our measurement error adjustment allows for mean reversion, consistent with the findings of the validation studies (see Bound et al. (2001) for a useful summary. Third, we restrict our analysis to fulltime workers (those working 35 to 65 hours), where they include all workers.

Figure 3.5 The Distribution of the Wage-Hours Elasticity, 2012-18 Pooled Results



Notes: These graphs present the cumulative distribution of the wage-hours elasticity across individuals, based on regression that allow the elasticity to vary at the major occupation code level. "Baseline" uses the wage measure that includes overtime, tips, and commission for everyone. "ME Adjusted" applies the measurement adjustments discussed in Section 3.5.

Despite the fact that we are finding overall positive returns, it is possible that the results are masking significant heterogeneity. To examine whether such heterogeneity exists, we allow the premium to vary across 23 major occupational groups, but still control for detailed occupational fixed effects.[23] We then use these occupation premiums in conjunction with the number of workers in each occupational group to construct the cumulative distribution function of the returns across all fulltime workers.

Turning to the results in Figure 3.5, we see that the returns to working long hours are positive for all hourly workers, both with and without adjustments for measurement error. For salary workers,

[23]We use the 23 "detailed" occupational groups that are defined by the Census Bureau. See Appendix Table 2 for the occupational specific results.

the returns are negative for about 85% of workers if we do not adjust for measurement error. When adjusting for measurement error, the premium is negative for less than 30% of workers. Thus, looking within occupation, there appears to be a positive premium for the vast majority of our sample.

Finally, the interpretation that these elasticities represent a premium for an individual hinges on workers being able to switch jobs within an occupational group, and it is readily apparent that some occupations encompass a fair degree of heterogeneity.[24] As a simple examination of the extent to which this heterogeneity matters, we further subdivide occupations into two groups of workers: Individuals who have at least a college degree and those who do not. We then repeat the regression in Table 3.3 with these occupation $\times$ college degree fixed effects, reporting the results in Column (5). As is readily apparent, neither the coefficient for hourly or salary workers changes much at all.

3.6 Conclusion and Discussion

To what extent do individuals get paid a higher hourly wage for working more hours? Surprisingly few papers have tackled this fundamental question regarding the incentives to work. We first examine whether individuals who work long hours indeed get a higher hourly wage. Using the CPS-ORG, we find a strong secular trend towards such individuals receiving a higher hourly wage, especially after important measurement issues are addressed. Among hourly workers in the last years of our data, individual working long hours receive an hourly wage that is about .7 log points higher than those working 40 hours a week. Among salary workers, the premium is about .2 log points in the latter years of our sample.

Does this translate into a premium for working long hours at the individual level? Just considering the premium that exists on ones current job based on federal overtime regulations, about 95% of hourly workers and 40% of salary workers would qualify for an overtime premium. Despite this large fraction, relatively few individuals earn overtime pay (about 20% among hourly workers and 10% among salary workers). Such a narrow view overlooks the possibility that individuals might

[24]For example, 10% of occupations are highly segregated by education, with the fraction being college educated being either less than 4% or greater than 96%. At the same time, 10% of occupations are near parity in education, with the fraction being college education between 42% and 58%.

be able to switch employers. While we find strong evidence that a premium exists within narrow occupational categories for all hourly workers and most salary workers, it is difficult to assess with our data the extent to which such options are feasible.

Despite the evidence we are able to provide, several important questions remain. First, to what extent do firms provide the opportunity for individuals to work overtime hours? While we find relatively few individuals earn overtime pay, we do not know whether this stems from individual choices or employer constraints. Second, we provide little concrete evidence on the extent to which the within-occupation premium represents a real premium across individuals. Third, we mainly consider the contemporaneous benefits of working long hours in this paper. Of course, working long hours might serve as an investment in future higher earnings.

So, to what extent do individual get paid a higher hourly wage for working more hours? While we have made substantial progress in answering this question, it would appear far more information is needed given the importance of this question to understanding labor supply.

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APPENDIX A

APPENDIX TO CHAPTER 1

A.1 Data Appendix

A.1.1 Industry Classification

The industry classification of the CPS-ORG has experienced multiple changes throughout the sample period (1979-2019). Several changes were made to the 3-digit detailed industry codes, and the same detailed industry is often classified as different major industry classifications. Therefore, industry classification of the CPS-ORG is highly inconsistent across time. To overcome this problem, this paper relies on the consistent industry code proposed by Pollard (2019, Table C-5). This method aggregates detailed industries into 47 industries (excluding armed forces) and 13 large industries. To maximize the number of observations in each cell, I use 13 major industry codes. Table A.1 shows the list of industries

Table A.1 List of Industries

Number	Industries
1	Agriculture, Forestry, Fishing, and Hunting
2	Mining
3	Construction
4	Manufacturing
5	Wholesale and Retail Trade
6	Transportation and Utilities
7	Information
8	Financial Activities
9	Professional and Business services
10	Education and Health services
11	Leisure and Hospitality
12	Other Services
13	Public administration

Notes: Source: Pollard (2019)

A.1.2 Additional Descriptive Figures and Tables

This subsection presents complementary figures depicting the data. Figure A.1 first shows the evolution of the employment-to-population ratio by age. This ratio increases dramatically over age 16-21, which is the age range examined in this study. It is relatively flat from age 25 to early 50 but starts to decline at approximately age 50. It declines faster in the early 60s. Only fewer than 10 percent of the elderly older than 70 work.

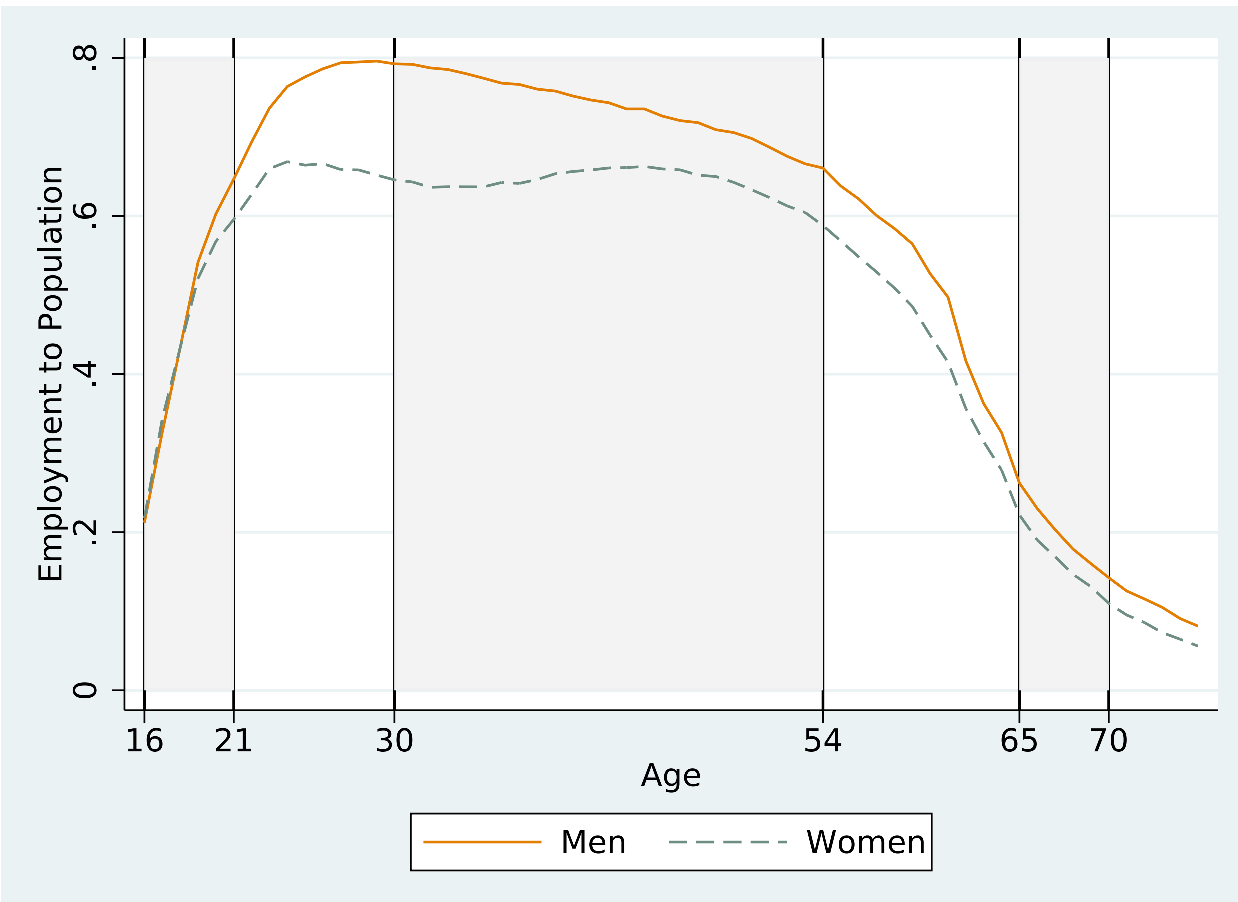
Figure A.1 is drawn using all observations from 1979 to 2019. However, it may mask a drastic change in labor market outcomes during the last four decades. Figure A.2 shows the trends in three labor market outcomes: the employment-to-population ratio, median hourly wage, and fraction of minimum wage workers among workers. In all three panels, red lines show the trends among prime-age workers, blue lines show those among the young and green lines show the trends among the elderly.

As shown in the left panel, the employment-to-population ratio of the young workers has decreased from approximately the early 2000s. It was approximately half during the 1980s and 1990s, but it has dropped to approximately 40 percent in recent years. In contrast, this ratio starts to increase for the elderly at more or less the same time.

The middle panel, showing the trend in median hourly wage, may be the most striking. The median hourly wage of older workers saw drastic increases, unlike those of prime-age or young workers. The median hourly wage of older workers was very close to that of young workers in earlier periods. It started to increase from the late 1990s and recently has become closer to the median hourly wage of prime-age workers. As a result, the fraction of the minimum wage workers becomes lower, although it still exceeds that of prime-age workers.

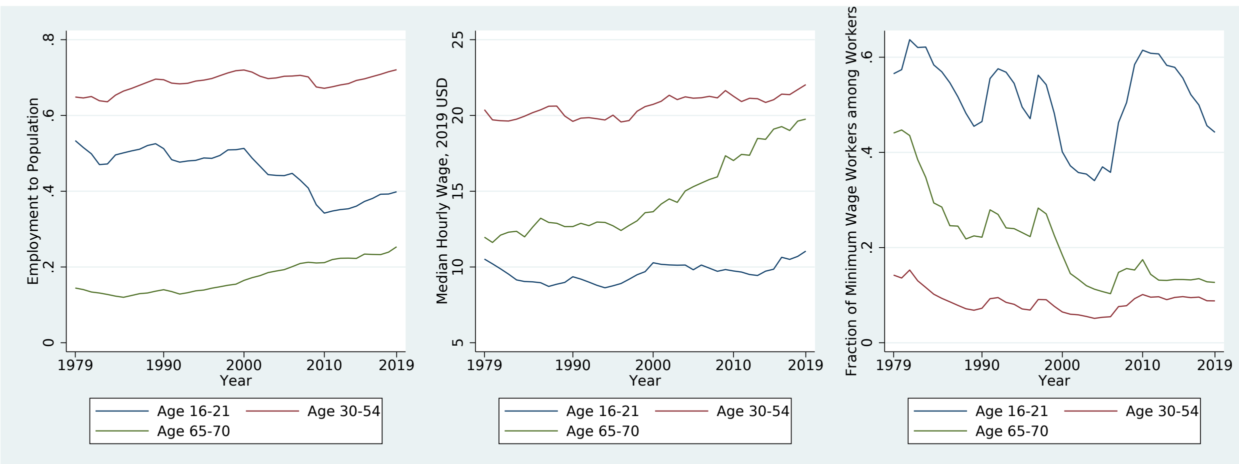
Figure A.3 shows the population weighted distribution of minimum wage to median wages. Most states' minimum-wage-to-median-wage ratio ratios, or in other words, the famous Kaitz index, falls roughly in the range of 0.35 to 0.55, with a longer right tail.

Figure A.1 Employment-to-Population Ratio by Age



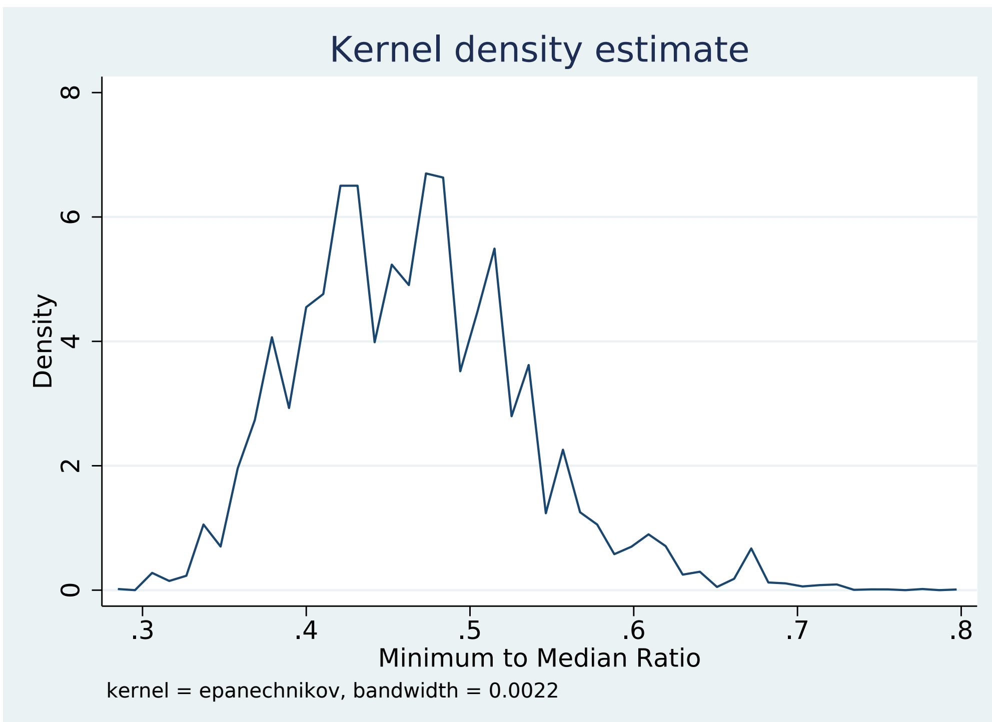
Notes: Source: CPS-ORG, 1979-2019. All the results are weighted by the CPS earnings weight.

Figure A.2 Trends in Labor Market Outcomes by Age, 1979-2019



Notes: Source: CPS-ORG, 1979-2019. The left and right panels are weighted by the CPS earnings weight, and the middle panel is unweighted. Minimum wage workers are defined as those whose hourly wage is the effective minimum wage * 1.2 or lower.

Figure A.3 Distribution of Minimum to Median Wage



Notes: Source: CPS-ORG, 1979-2019. All the results are weighted by the CPS earnings weight.

A.2 Expanded Tables and Figures

Appendix A.2 presents the expanded tables and figures. Table A.2 shows the full version of Table 1.2. Panels A and B are the same with the table in the body, while Panel C shows the employment elasticity with respect to federal minimum wage increases as well as 5-year average effects. First, in the longer run, the effects tend to disappear, as shown in Figure 1.3. This suggests that employment effects persist only in the short run. Second, responses to the federal level are noisy and negative.

Table A.3 contains the elasticity with respect to minimum wage for each outcome variable in Table 1.3. Due to the smaller fraction of self-employed workers among the young or older workers, its responses measured by elasticities are much more inflated.

Figure A.4 shows the results in Panel A of Figure 1.4 one by one, and Figure A.5 shows the extended version of Panel B of Figure 1.4 with 95-percent confidence intervals.

Table A.2 Expanded Version of Table 1.2

	Dep var: Employed		
	Age 16-21 (N: 1,428,573)	Age 30-54 (N: 5,770,663)	Age 65-70 (N: 854,730)
	(1)	(2)	(3)
Panel A. Estimation using *NW-type* Specification			
Elasticity w.r.t. Minimum Wage	-0.183***	0.027	0.166+
	(0.051)	(0.016)	(0.086)
Panel B. Estimation using *ADR-type* Specification			
Elasticity w.r.t. Minimum Wage	0.026	0.031**	0.232**
	(0.037)	(0.011)	(0.069)
Panel C. Estimation using *CDLZ-type* Specification			
3Y Average Effects			
Elasticity w.r.t. Minimum Wage	0.023	0.020	0.118*
	(0.067)	(0.019)	(0.058)
Elasticity w.r.t. Minimum Wage (Federal)	0.027	0.004	-0.221
	(0.084)	(0.025)	(0.179)
5Y Average Effects			
Elasticity w.r.t. Minimum Wage	0.042	0.014	0.071
	(0.060)	(0.015)	(0.070)
Elasticity w.r.t. Minimum Wage (Federal)	0.068	0.023	-0.120
	(0.094)	(0.024)	(0.185)

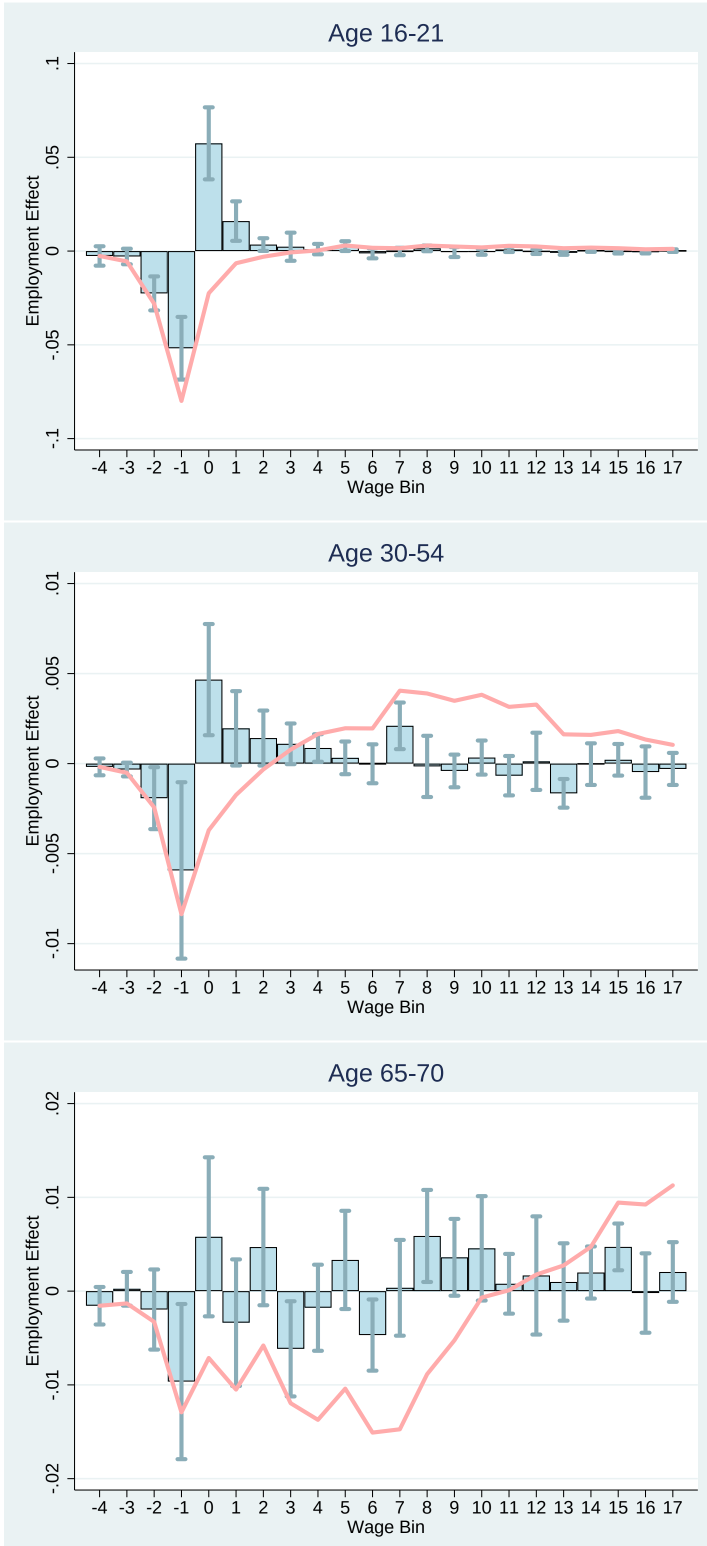
Notes: All the results are weighted by the earnings weight (*earnwt*) in CPS-ORG. Robust standard errors are in parentheses and clustered at the state-level. The unit of time is the quarter. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. see notes to Table 1.2 for details.

Table A.3 Expanded Version of Table 1.3

	Wage/Salary Employment (E) (1)	Unemployed (U) (2)	Self-Employed (S) (3)	All Others (NILF) (4)
Panel A. Age 16-21 (N: 1,428,573)				
Estimation using the *NW-type* Specification				
Semi-Elasticity	-0.083***	-0.001	0.000	0.083**
	(0.023)	(0.011)	(0.002)	(0.029)
Ela w.r.t. MW	-0.183***	-0.010	0.025	0.183**
	(0.051)	(0.135)	(0.168)	(0.063)
Estimation using the *ADR-type* Specification				
Semi-Elasticity	0.012	-0.010	-0.004	0.002
	(0.017)	(0.010)	(0.003)	(0.015)
Ela w.r.t. MW	0.026	-0.119	-0.378	0.005
	(0.037)	(0.117)	(0.280)	(0.032)
Estimation using the *CDLZ-type* Specification				
3Y Average Effects				
Semi-Elasticity	0.010	0.002	0.006*	-0.018
	(0.027)	(0.012)	(0.003)	(0.021)
Ela w.r.t. MW	0.023	0.028	0.732*	-0.035
	(0.067)	(0.179)	(0.301)	(0.041)
Panel B. Age 30-54 (N: 5,770,663)				
Estimation using the *NW-type* Specification				
Semi-Elasticity	0.019	-0.007	0.005	-0.017
	(0.011)	(0.006)	(0.006)	(0.010)
Ela w.r.t. MW	0.027	-0.170	0.053	-0.100
	(0.016)	(0.169)	(0.062)	(0.060)
Estimation using the *ADR-type* Specification				
Semi-Elasticity	0.022**	-0.009	-0.000	-0.013*
	(0.008)	(0.006)	(0.004)	(0.005)
Ela w.r.t. MW	0.031**	-0.231	-0.001	-0.072*
	(0.011)	(0.166)	(0.040)	(0.030)
Estimation using the *CDLZ-type* Specification				
3Y Average Effects				
Semi-Elasticity	0.014	0.002	-0.016**	0.000
	(0.013)	(0.005)	(0.005)	(0.007)
Ela w.r.t. MW	0.020	0.057	-0.169**	0.001
	(0.019)	(0.164)	(0.057)	(0.039)
Panel C. Age 65-70 (N: 854,730)				
Estimation using the *NW-type* Specification				
Semi-Elasticity	0.030+	-0.000	0.033***	-0.063**
	(0.016)	(0.003)	(0.009)	(0.022)
Ela w.r.t. MW	0.166+	-0.002	0.469***	-0.085**
	(0.086)	(0.246)	(0.129)	(0.029)
Estimation using the *ADR-type* Specification				
Semi-Elasticity	0.042**	-0.004	-0.015	-0.024
	(0.012)	(0.004)	(0.011)	(0.014)
Ela w.r.t. MW	0.232**	-0.346	-0.208	-0.032
	(0.069)	(0.381)	(0.154)	(0.020)
Estimation using the *CDLZ-type* Specification				
3Y Average Effects				
Semi-Elasticity	0.025*	-0.004	0.000	-0.021
	(0.012)	(0.004)	(0.011)	(0.018)
Ela w.r.t. MW	0.118*	-0.365	0.003	-0.030
	(0.058)	(0.318)	(0.157)	(0.025)

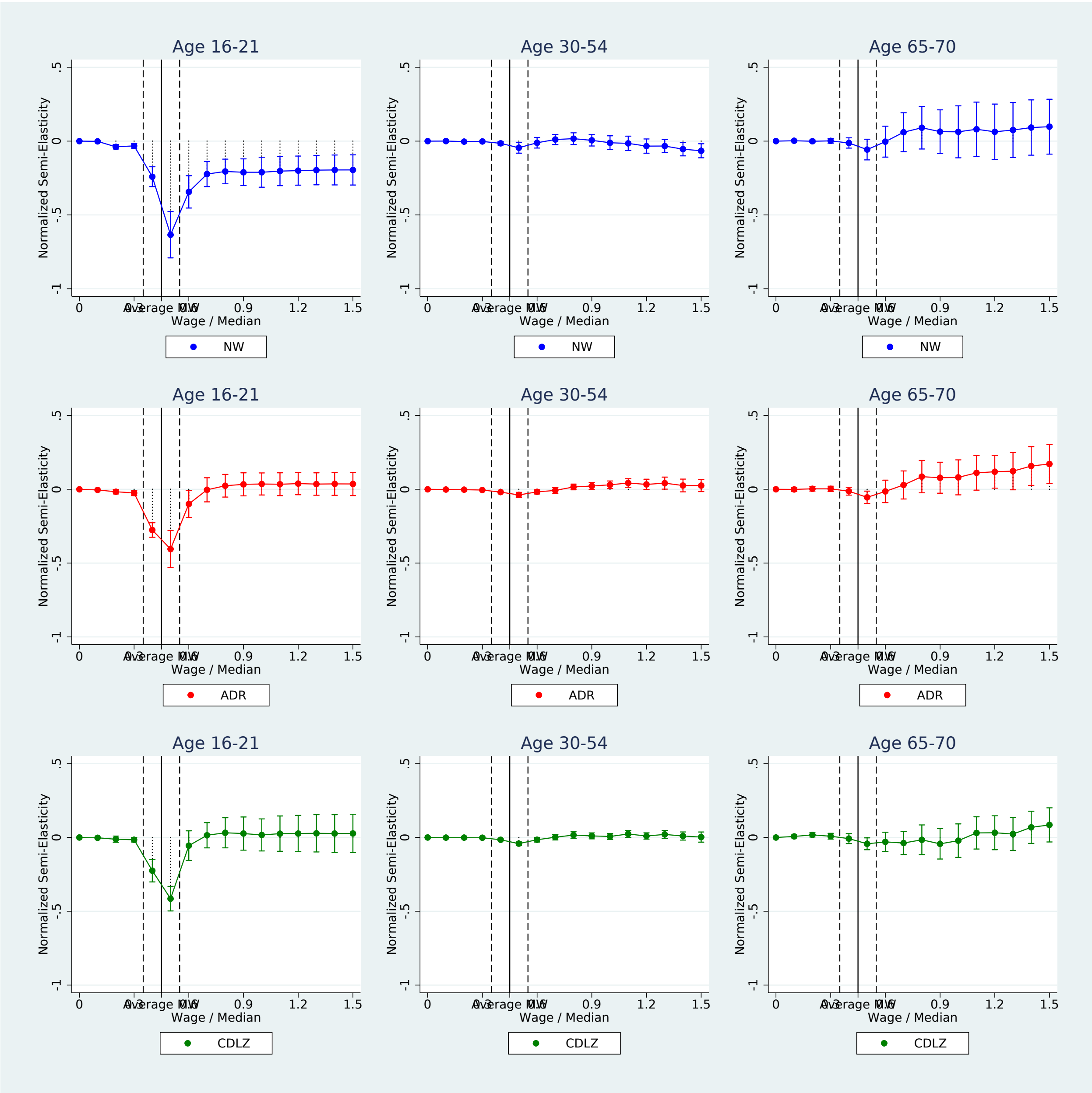
Notes: Robust standard errors are in parentheses and clustered at the state level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. See notes to Table 1.2 for more details.

Figure A.4 Results using Bunching Approach by Age



Notes: Each bar shows the employment effects on the wage bin normalized by the employment-to-population ratio of each age group before the treatment. The red line shows the cumulative effects up to that wage bin.

Figure A.5 Expanded version of Figure 1.4



Notes: The blue, green, and red dots show the estimates for the young (16-21), prime-age (30-54), and elderly (65-70), respectively. Each bar shows a 95 percent confidence interval. See text for details. The black solid line shows the average minimum wage-median wage ratio (approximately 45 percent of the median wage), and the black dotted lines show the 5th and 95th percentiles of the minimum to median ratio, respectively.

A.3 Effects on Other Labor Market Outcomes

Examining the effects on the number of jobs may not provide a comprehensive picture of the effects on the labor market, since other labor market outcomes may also respond to the minimum wages. This subsection explores other important labor market outcomes such as wages and hours, using three baseline specifications.

Table A.4 shows the effects of minimum wages on log hourly wages and log weekly working hours. All the reported coefficients are elasticities of hourly wages with respect to minimum wages, in other words, $\hat{\beta}$ from equations (1.1) and (1.2) for Panels A and B and $\sum_{\tau=0}^{2} \hat{\beta}_\tau - \sum_{\tau=-3}^{-1} \hat{\beta}_\tau$ divided by minimum wage change for Panel C.

The positive effects of minimum wages on the hourly wage of the affected are well-established in the literature. Consistent with the literature, column (1) of Table A.4 reports positive and significant effects on the hourly wage of young workers. Hourly wage elasticity with respect to minimum wage falls in the [0.1, 0.2] interval, which is consistent with the consensus in the literature (Belman and Wolfson, 2014, Ch 5). The estimates in column (3) are positive but close to zero and insignificant, as expected from the small share of minimum wage workers among prime-age workers.

The results in column (5) also show that the effects on older workers' hourly wage are close to zero, conditional on working. There are several explanations for the zero wage effects. First, although a larger fraction of older workers earn minimum wage compared to prime-age workers, it is still much lower than that of teenagers. Therefore, the zero wage effects could be a result of a smaller fraction of minimum wage workers. Second, given the small positive effects reported in the previous subsection, a higher minimum wage may increase the fraction of lower wage workers, which would lead to insignificant or even negative wage effects among those who work. Finally, higher minimum wages may move workers from high-paying jobs with longer hours to low-paying jobs with shorter-hours.

In Appendix A.4.2, I conduct several analyses to answer this question. However, it is not clear which explanation makes sense. If a smaller bite of the minimum wage is the reason for zero wage effects, wage effects should be larger for subgroups with larger fraction of minimum wage workers. In Appendix Figure A.10, I show the fraction of minimum wage workers by education and age, analogous to Figure 1.2. This shows that approximately 40 percent of older workers without a high school diploma earn less than or equal to 120 percent of the effective minimum wage. However, as shown in Appendix Table A.10, wage effects are not larger for this group, which provides less support for the explanation. If positive employment effects and compositional shifts are the reason for the zero wage effects, groups with positive employment effects will show lower wage effects. This is also not true in the analysis by education level. Therefore, it is not clear why the wage effects are smaller.

Hours of work is another important labor market outcome. The results in column (2) and (4) show negative effects on young workers from the NW-type specification and zero effects on prime-age workers. Focusing on older workers, the results in columns (6) suggest that a minimum wage does not change the weekly working hours of employed workers. In summary, conditional on working, a higher minimum wage does not alter the labor market characteristics of older workers, although it increases their employment.

Table A.4 Effects of Minimum Wages on Wages and Hours

	Sample: Employed					
	Age 16-21		Age 30-54		Age 65-70	
	Log Hourly Wages	Log Weekly Hours	Log Hourly Wages	Log Weekly Hours	Log Hourly Wages	Log Weekly Hours
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Estimation using *NW-type* Specification						
Elasticity w.r.t. Minimum Wage	0.128***	-0.090**	0.006	-0.000	-0.024	-0.021
	(0.024)	(0.027)	(0.021)	(0.010)	(0.051)	(0.033)
Obs	655,520	622,277	3,887,157	3,817,307	144,503	138,456
Panel B. Estimation using *ADR-type* Specification						
Elasticity w.r.t. Minimum Wage	0.190***	0.023	0.009	-0.010*	0.027	0.041
	(0.021)	(0.031)	(0.016)	(0.005)	(0.044)	(0.053)
Observations	655,520	622,275	3,887,157	3,817,307	144,461	138,406
Panel C. Estimation using *CDLZ-type* Specification						
3Y Average Effects						
Elasticity w.r.t. Minimum Wage	0.164***	0.032	0.014	0.002	0.057	0.024
	(0.022)	(0.027)	(0.016)	(0.007)	(0.065)	(0.038)
Elasticity w.r.t. Minimum Wage (Federal)	-0.032	0.053	0.003	-0.010	0.074	0.035
	(0.046)	(0.039)	(0.023)	(0.011)	(0.096)	(0.087)
5Y Average Effects						
Elasticity w.r.t. Minimum Wage	0.152***	0.061+	0.017	0.002	0.057	0.020
	(0.023)	(0.036)	(0.014)	(0.007)	(0.054)	(0.037)
Elasticity w.r.t. Minimum Wage (Federal)	-0.015	0.051	0.012	-0.012	0.070	0.049
	(0.047)	(0.048)	(0.024)	(0.011)	(0.088)	(0.075)
Obs	655,520	622,277	3,887,157	3,817,307	144,503	138,456

Notes: Columns (1), (3), and (5) report elasticities of hourly wages with respect to minimum wages, and columns (2), (4), and (6) report elasticities of working hours with respect to minimum wages. Robust standard errors are in parentheses and clustered at the state level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. See notes to Table 1.2 for more details.

A.4 Robustness Check and Heterogeneity of Employment Effects

This section presents more detailed results regarding the employment effects analysis in section 1.5.1.

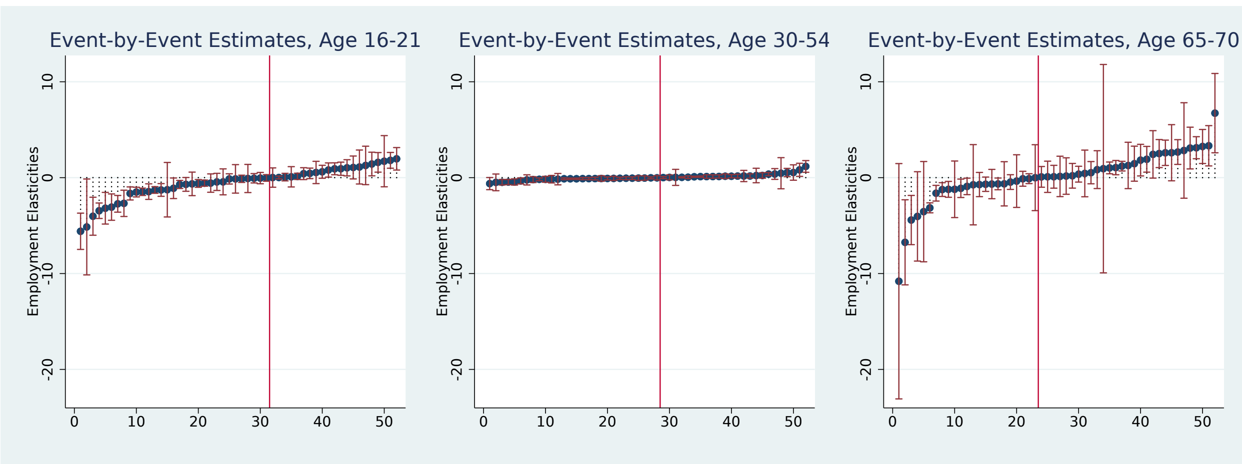
A.4.1 Event-by-Event Estimates

Figure A.6 first presents event-by-event estimates for 52 ‘clean’ treatments. Events are aligned by the size of the elasticities, and the red vertical line shows the place of zero. In the case of the prime-age, estimates are generally much smaller. In the case of the young and the elderly, some of the estimates become noisy and larger in magnitude, possibly due to the lack of observations. This analysis, however, also shows that for more of the events, the elderly employment elasticities are positive.

Figures A.7 through A.9 show the event-by-event estimates, classified by the time. Most state-level minimum wage increases are clustered in certain periods, providing a natural way of classification. I classify them into 4 time periods: 1987-1993, 1996-2003, 2005-2007, and 2014-2015. Events after 2015 are excluded since I do not have their full 8-year window observations. This shows that, in general, negative effects are more prominent for minimum wage increases during the Great Recession, as shown by Clemens and Wither (2019). In contrast, from the late 80s to the early 90s, effects tend to be more positive.

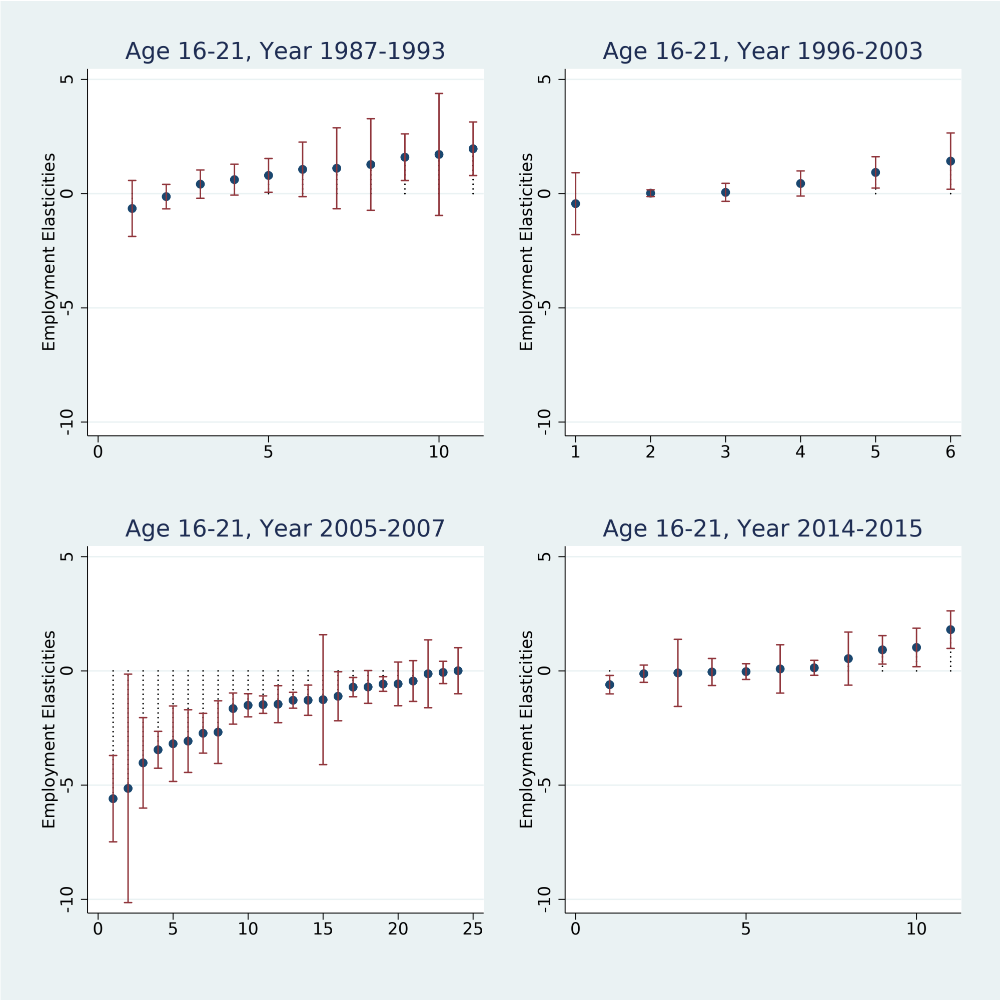
In recent periods, employment elasticities with respect to the minimum wages of prime-age workers are close to zero and often negative. In contrast, in the case of older populations. estimates tend to be more positive and often significantly different from zero. This is especially interesting for several reasons. First, as shown in Appendix Figure A.2, the hourly wage of older workers is higher in this period. This may support the analysis in section 1.5.2, which reports positive employment effects on workers above the minimum. Second, although the average hourly wages of prime-age and older workers become closer in this period, they are affected differently. Third, this period sees relatively more frequent minimum wage changes, hence identification exploits more variation. The effects of recent minimum wage changes require future study.

Figure A.6 Event-by-Event Estimates



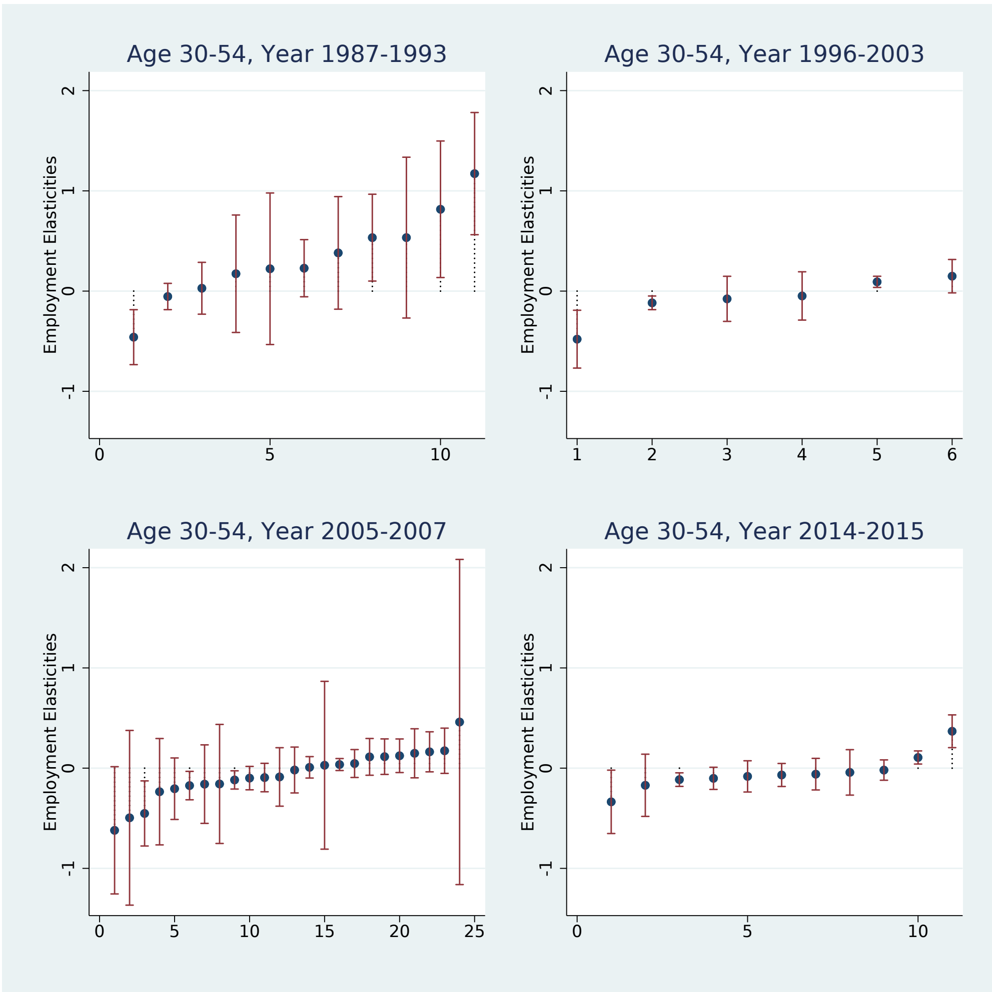
Notes: Each dot shows the point estimates of employment elasticity with respect to minimum wage, and each bar shows 95 percent confidence interval. Red vertical line is located between the smallest positive estimates and the largest negative estimates.

Figure A.7 Event-by-Event, Young (16-21)



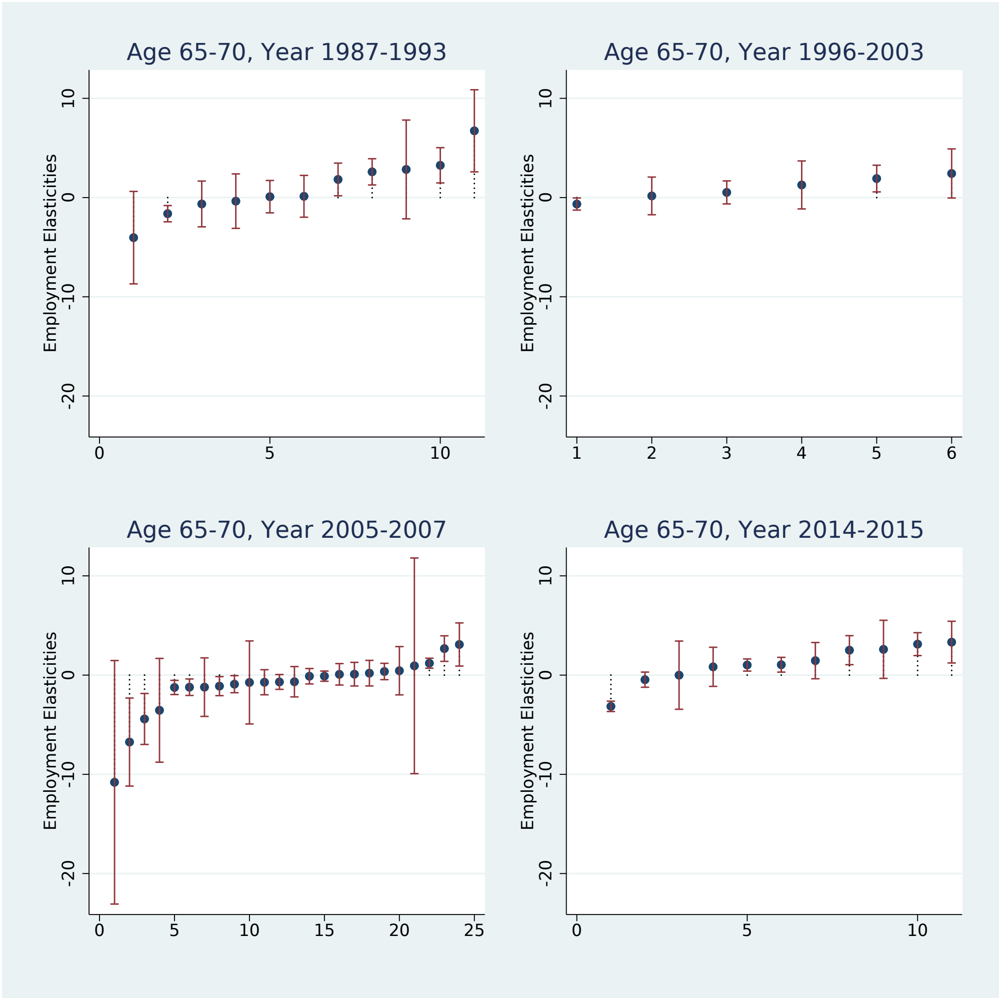
Notes: Each dot shows the point estimates of employment elasticity with respect to minimum wage, and each bar shows 95 percent confidence interval.

Figure A.8 Event-by-Event, Prime-Age (30-54)



Notes: Each dot shows the point estimates of employment elasticity with respect to minimum wage, and each bar shows 95 percent confidence interval.

Figure A.9 Event-by-Event, Elderly (65-70)



Notes: Each dot shows the point estimates of employment elasticity with respect to minimum wage, and each bar shows 95 percent confidence interval.

A.4.2 Heterogeneity by Education

This subsection addresses heterogeneity based on education. Figure A.10 shows the fraction of minimum wage workers among workers by age and education. It resembles Figure 1.1 but starts from 25 since analyzing workers with a higher degree but below age 25 is hardly meaningful. There are two notable observations. First, the fraction of minimum wage workers shows an increasing trend from age 60, and the increase becomes dramatic from age 65 for all education levels. This finding is consistent with the analysis for all workers in Figure 1.1, but Figure A.10 shows that it is not limited to low-educated workers and applies to workers of all education levels, including those with advanced degrees. Among workers aged 65-70, the fraction of minimum wage workers is approximately 10 percent for those who have a bachelor's degree or higher. Second, although increasing trends are common for everyone, they are more dramatic for workers without a bachelor's degree. In the case of high school dropouts, the fraction of minimum wage workers exceeds 30 percent after age 65.

Figure A.10 motivates analysis in this section. Based on the observation in Figure A.10, I divide the population into four groups: High school dropouts, high school graduates, some college, and bachelor's degree or above. Tables A.5 through A.7 show the heterogeneity of employment effects by education level for the young, prime-age, and elderly, respectively.

Table A.5 shows the results for young workers. Column (4) is included for completeness but does not appear to deliver meaningful findings given the small sample size. Although the overall patterns differ across the specifications, all the specifications show that the results are more negative for high school dropouts and workers with some college education. A large portion of workers with some college education in this population may not have completed their schooling. Hence, these workers may be susceptible to adjustments responding to higher minimum wages.

Table A.6 shows the same results for prime-age workers. Most of the estimates of employment elasticities with respect to minimum wage fall in the range of [-0.05, 0.05], although some are statistically different from zero. The only exception is high school dropouts using the NW-type specification, and interestingly, this result shows positive employment elasticity.

In Table A.7, although heterogeneity analysis is not highly robust to the choice of specification, none of the results show evidence of disemployment effects. Even high-school dropouts whose share of minimum wage workers is above 30 percent do not show disemployment effects. However, specifications do not provide robust evidence on who is more positively affected by employment. High school graduates are the group all three specifications show relatively similar estimates in the [0.1, 0.25] interval. Except for this group, the results differ by specification. The NW-type specification and CDLZ-type specifications tend to show larger positive effects on lower-educated workers, and the ADR-type specification shows the opposite result.

Results so far suggest the following. Although a larger fraction of the elderly, especially less-educated workers, earn minimum wages, there is no evidence of disemployment effects suggested by standard neoclassical theory. Results instead suggest small positive effects, but these effects may not be limited to the least-skilled workers who are more likely to earn minimum wages.

Next Tables A.8 through A.10 show the effects on hourly wages. Table A.8 shows the positive wage elasticities for all the specifications in columns (1) and (3), with larger and clearer effects on the wages of those who do not have a high school diploma. The effects in columns (2) and (3) are smaller in Panel A, while Panels B and C show larger and clearer effects on hourly wages.

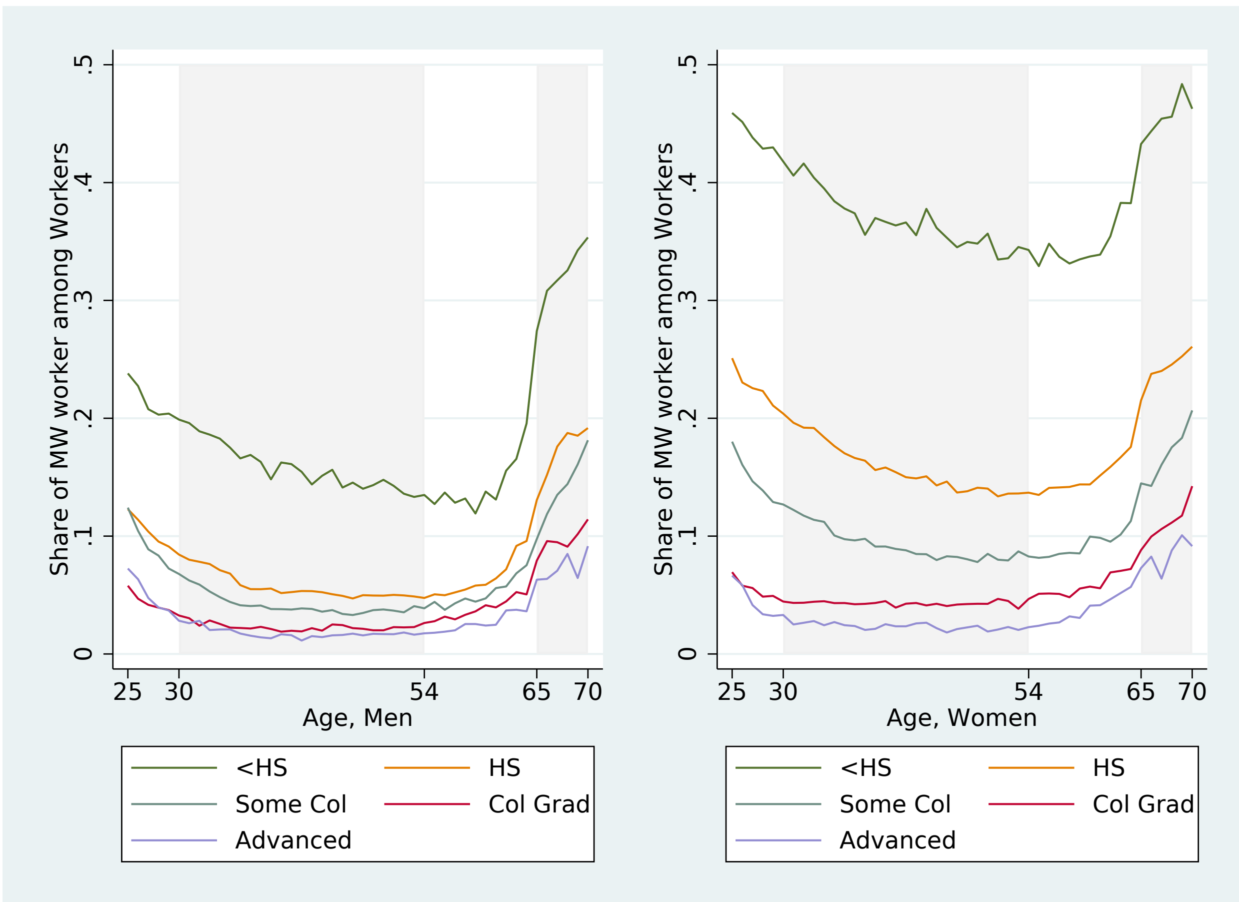
Table A.9 shows the effects on hourly wages of prime-age workers. Somewhat surprisingly, the effects on less-educated workers are generally negative in Panel A, while they are closer to zero in the other specifications. Together with Figure 1.4, this may cast some doubts on the validity of this specification.

Finally, Table A.10 shows the effects on older workers. One goal of this analysis is to determine why the effects on the hourly wage of older workers are close to zero. As mentioned in section 1.5.2, there are several explanations: (1) only a small fraction of workers are affected; (2) positive employment responses increase the fraction of lower-wage workers; and (3) workers may move to lower-wage part-time jobs.

As discussed above and shown in Figure A.10, the fraction of minimum wage workers is substantial for high school dropouts. The sample average of the fraction of minimum wage workers among high school dropouts work is 0.38. Given that the same ratio for the young workers is approximately 50 percent, this bite is not small. If a smaller 'bite' is the culprit of the zero wage effects, we can expect to see clearer effects for this group. In column (1) of Table A.10, however, this is not the case. In general, wage effects are not larger for high-school dropouts. If there is any difference, middle-skilled workers such as high school graduates or those with some college education may experience larger effects.

However, it is also not clear whether the increased portion of lower wage workers could explain the zero wage effects results. A composition change would imply that large employment effects cause lower wage effects; hence one would expect to see negative relationships between the wage and employment effects. In Table A.7, patterns are not highly consistent across the methods, but larger employment effects are found among high school graduates. However, this group shows larger wage effects in Table A.10. Therefore, it is not clear why wage effects are smaller for older workers.

Figure A.10 Share of Minimum Wage Workers, by Age and Education



Notes: Source: CPS-ORG, 1979-2019. All the results are weighted by the CPS earnings weight. Minimum wage workers are defined as those whose hourly wage is the effective minimum wage * 1.2 or lower.

Table A.5 Employment Effects by Education, Age 16-21

	Dep var: Employed Sample: Age 16-21			
	High School Dropouts	High School Graduates	Some College	College Degree or Above
	(1)	(2)	(3)	(4)
Panel A. Estimation using *NW-type* Specification				
Elasticity w.r.t. Minimum Wage	-0.316***	-0.035	-0.216***	0.146
	(0.087)	(0.039)	(0.059)	(0.196)
Obs	716,626	325,030	377,878	7,610
Panel B. Estimation using *ADR-type* Specification				
Elasticity w.r.t. Minimum Wage	0.023	0.138**	-0.053	0.146
	(0.072)	(0.041)	(0.053)	(0.196)
Obs	716,626	325,030	377,878	7,610
Panel C. Estimation using *CDLZ-type* Specification				
3Y Average Effects				
Elasticity w.r.t. Minimum Wage	-0.079	0.118*	0.051	-0.180
	(0.122)	(0.055)	(0.062)	(0.323)
Elasticity w.r.t. Minimum Wage (Federal)	0.142	0.013	-0.061	0.368
	(0.134)	(0.086)	(0.086)	(0.486)
5Y Average Effects				
Elasticity w.r.t. Minimum Wage	-0.015	0.114*	0.044	-0.116
	(0.126)	(0.044)	(0.050)	(0.272)
Elasticity w.r.t. Minimum Wage (Federal)	0.149	0.040	0.027	0.552
	(0.133)	(0.090)	(0.100)	(0.362)
Obs	716,632	325,583	378,570	7,610

Notes: All the results are weighted by the CPS earnings weight. Standard errors are in parentheses and clustered at the state-level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. see notes to Table 1.2.

Table A.6 Employment Effects by Education, Age 30-54

	Dep var: Employed Sample: Age 30-54			
	High School Dropouts	High School Graduates	Some College	College Degree or Above
	(1)	(2)	(3)	(4)
Panel A. Estimation using *NW-type* Specification				
Elasticity w.r.t. Minimum Wage	0.202***	0.036	-0.001	-0.017
	(0.048)	(0.023)	(0.017)	(0.013)
Obs	729,309	1,905,989	1,477,447	1,657,918
Panel B. Estimation using *ADR-type* Specification				
Elasticity w.r.t. Minimum Wage	0.040	0.047*	0.049*	0.011
	(0.046)	(0.020)	(0.023)	(0.016)
Obs	729,264	1,905,989	1,477,445	1,657,916
Panel C. Estimation using *CDLZ-type* Specification				
3Y Average Effects				
Elasticity w.r.t. Minimum Wage	0.044	0.020	0.015	0.023
	(0.037)	(0.027)	(0.016)	(0.021)
Elasticity w.r.t. Minimum Wage (Federal)	0.041	0.039	0.006	-0.030
	(0.080)	(0.033)	(0.034)	(0.029)
5Y Average Effects				
Elasticity w.r.t. Minimum Wage	0.002	0.034+	0.015	0.002
	(0.037)	(0.020)	(0.014)	(0.016)
Elasticity w.r.t. Minimum Wage (Federal)	0.110	0.040	0.020	-0.006
	(0.105)	(0.033)	(0.030)	(0.027)
Obs	729,309	1,905,989	1,477,447	1,657,918

Notes: All the results are weighted by the CPS earnings weight. Standard errors are in parentheses and clustered at the state-level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. see notes to Table 1.2.

Table A.7 Employment Effects by Education, Age 65-70

	Dep var: Employed Sample: Age 65-70			
	High School Dropouts	High School Graduates	Some College	B.A or Above
	(1)	(2)	(3)	(4)
Panel A. Estimation using *NW-type* Specification				
Elasticity w.r.t. Minimum Wage	0.162	0.200+	-0.000	0.066
	(0.172)	(0.110)	(0.096)	(0.094)
Obs	226,463	293,819	163,429	171,019
Panel B. Estimation using *ADR-type* Specification				
Elasticity w.r.t. Minimum Wage	0.014	0.234+	0.242+	0.265+
	(0.233)	(0.133)	(0.143)	(0.141)
Observations	226,457	293,819	163,396	170,974
Panel C. Estimation using *CDLZ-type* Specification				
3Y Average Effects				
Elasticity w.r.t. Minimum Wage	0.428*	0.132	0.045	0.105
	(0.194)	(0.128)	(0.140)	(0.112)
Elasticity w.r.t. Minimum Wage (Federal)	-0.590+	-0.102	-0.241	-0.146
	(0.340)	(0.244)	(0.275)	(0.285)
5Y Average Effects				
Elasticity w.r.t. Minimum Wage	0.190	0.127	0.009	0.073
	(0.212)	(0.119)	(0.162)	(0.109)
Elasticity w.r.t. Minimum Wage (Federal)	-0.274	-0.204	-0.048	0.014
	(0.343)	(0.250)	(0.258)	(0.244)
Obs	226,463	293,819	163,429	171,019

Notes: All the results are weighted by the CPS earnings weight. Standard errors are in parentheses and clustered at the state-level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. see notes to Table 1.2.

Table A.8 Effects on Hourly Wage by Education, Age 16-21

	Dep var: ln Hourly Wages Sample: Age 16-21, employed			
	High School Dropouts	High School Graduates	Some College	College Degree or Above
	(1)	(2)	(3)	(4)
Panel A. Estimation using *NW-type* Specification				
Elasticity w.r.t. Minimum Wage	0.221***	0.055+	0.084*	0.178
	(0.022)	(0.032)	(0.035)	(0.166)
Obs	236,184	200,898	213,581	4,572
Panel B. Estimation using *ADR-type* Specification				
Elasticity w.r.t. Minimum Wage	0.270***	0.153***	0.135***	0.631+
	(0.029)	(0.032)	(0.032)	(0.325)
Observations	236,077	200,284	212,930	3,538
Panel C. Estimation using *CDLZ-type* Specification				
3Y Average Effects				
Elasticity w.r.t. Minimum Wage	0.192***	0.168***	0.139***	0.131
	(0.033)	(0.026)	(0.027)	(0.251)
Elasticity w.r.t. Minimum Wage (Federal)	-0.039	-0.093	0.017	-0.306
	(0.057)	(0.062)	(0.060)	(0.592)
5Y Average Effects				
Elasticity w.r.t. Minimum Wage	0.173***	0.152***	0.141***	0.115
	(0.034)	(0.029)	(0.026)	(0.196)
Elasticity w.r.t. Minimum Wage (Federal)	0.006	-0.072	0.010	-0.275
	(0.053)	(0.064)	(0.056)	(0.479)
Obs	236,184	200,898	213,581	4,572

Notes: All the results are weighted by the CPS earnings weight. Standard errors are in parentheses and clustered at the state-level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. see notes to Table 1.2.

Table A.9 Effects on Hourly Wage by Education, Age 30-54

	Dep var: ln Hourly Wages Sample: Age 30-54, employed			
	High School Dropouts	High School Graduates	Some College	College Degree or Above
	(1)	(2)	(3)	(4)
Panel A. Estimation using *NW-type* Specification				
Elasticity w.r.t. Minimum Wage	-0.130***	-0.046*	-0.040*	0.058+
	(0.028)	(0.022)	(0.019)	(0.033)
Obs	385,325	1,256,552	1,029,024	1,216,256
Panel B. Estimation using *ADR-type* Specification				
Elasticity w.r.t. Minimum Wage	0.006	0.002	-0.003	0.027
	(0.041)	(0.025)	(0.021)	(0.024)
Observations	384,679	1,256,552	1,029,010	1,216,230
Panel C. Estimation using *CDLZ-type* Specification				
3Y Average Effects				
Elasticity w.r.t. Minimum Wage	0.069*	0.025	0.008	0.012
	(0.031)	(0.021)	(0.020)	(0.023)
Elasticity w.r.t. Minimum Wage (Federal)	-0.053	0.003	0.016	-0.003
	(0.063)	(0.029)	(0.034)	(0.035)
5Y Average Effects				
Elasticity w.r.t. Minimum Wage	0.073**	0.026	0.011	0.017
	(0.026)	(0.019)	(0.019)	(0.018)
Elasticity w.r.t. Minimum Wage (Federal)	0.016	0.018	0.035	-0.010
	(0.068)	(0.025)	(0.032)	(0.038)
Obs	385,325	1,256,552	1,029,024	1,216,256

Notes: All the results are weighted by the CPS earnings weight. Standard errors are in parentheses and clustered at the state-level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. see notes to Table 1.2.

Table A.10 Effects on Hourly Wage by Education, Age 65-70

	Dep var: ln Hourly Wages Sample: Age 65-70, employed			
	High School Dropouts (1)	High School Graduates (2)	Some College (3)	College Degree or Above (4)
Panel A. Estimation using *NW-type* Specification				
Elasticity w.r.t. Minimum Wage	-0.066	-0.033	0.023	-0.071
	(0.069)	(0.047)	(0.058)	(0.069)
Obs	25,607	47,344	31,831	39,721
Panel B. Estimation using *ADR-type* Specification				
Elasticity w.r.t. Minimum Wage	0.040	0.143*	0.145	-0.118
	(0.168)	(0.069)	(0.089)	(0.087)
Observations	23,770	46,608	30,265	38,256
Panel C. Estimation using *CDLZ-type* Specification				
3Y Average Effects				
Elasticity w.r.t. Minimum Wage	0.171	0.130+	0.311*	-0.174
	(0.115)	(0.067)	(0.148)	(0.145)
Elasticity w.r.t. Minimum Wage (Federal)	0.162	-0.165	0.026	0.223
	(0.192)	(0.156)	(0.188)	(0.206)
5Y Average Effects				
Elasticity w.r.t. Minimum Wage	0.177+	0.132*	0.258+	-0.164
	(0.101)	(0.058)	(0.135)	(0.122)
Elasticity w.r.t. Minimum Wage (Federal)	0.141	-0.111	0.001	0.196
	(0.211)	(0.142)	(0.162)	(0.170)
Obs	25,607	47,344	31,831	39,721

Notes: All the results are weighted by the CPS earnings weight. Standard errors are in parentheses and clustered at the state-level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. see notes to Table 1.2.

A.4.3 Heterogeneity by Employment Status in the Previous Year

In Tables A.11 through A.13, I use the 2-year panel nature of the CPS-ORG. To construct longitudinal data using the CPS-ORG, I use household identifier variables (*hhid, hhnum*, and *lineno*) together with the month of the interview and state identifiers. Although this set of variables is standard in the literature (see Lefgren and Madrian, 1999), it often creates multiple matches. Although it is possible to overcome this issue for the period after 2004 (see Chung, 2022 and Section B.1 for more detail), I rely on the standard method for consistency. Fewer than 0.3 percent of the total observations have this problem. Approximately 70 percent of the total observations are matched.

Using matched observations, I classify them into two groups: those who worked in the first year and those who did not. Then I estimate the minimum wage effects on each group. However, since I am conditioning on the first-year labor market outcomes and use the second-year labor market outcomes as a dependent variable, using a standard event-study design with an 8-year window may create a problem. Since past minimum wage increases may affect the first-year labor market outcomes, I may end up with conditioning on the results. To avoid this problem, I modify the specification as follows and focus on the short-run changes.

$$y_{ista} = \beta I_{st} + X'_{ista}\gamma + \phi_{sa} + \phi_{ta} + \Omega_{st} + \varepsilon_{ista} \tag{A.1}$$

Here I_{st} captures the state-level minimum wage increases between two interviews. Hence, I estimate the short-run adjustments in labor market outcomes by comparing the labor market outcomes of those who experience large increases and those who do not. Again, the effects are converted into elasticity.

Table A.11 through A.13 show the results for observations who did and did not work in the previous year separately, for the young, prime-age and elderly, respectively. The employment-to-population ratio is much lower for those who did not work in the first year. I present both coefficients and elasticities to avoid the potential risk of exaggerating very small changes.

In Tables A.11 and A.13, the effects tend to be more negative in column (2), which suggests more negative effects on inflows into employment, consistent with Gopalan et al. (2021). The results are again very different in Table A.13, in column (1), the effects on all the matched observations show elasticity in the bound of [0.1, 0.3], similarly to the results in Table 1.2. In later columns, in all the specifications, positive responses measured by elasticity are clear for entrants ([0.3, 0.7]) rather than incumbents ([-0.1, 0.1]), although the sizes of the coefficients themselves are often reversed and estimates are generally imprecise, possibly due to the smaller sample size. In the case of those who did not work in the first year, the second year average employment rate is just approximately 4 percent, while the same ratio for those who worked in the last year is above 70 percent. This suggests that a higher minimum wage may foster unretirement behavior. However, the estimates are highly noisy and suffer from insufficient observations, necessitating further research.

Table A.11 Minimum Wage Effects by Previous Employment Status, Age 16-21

	Outcome: Employment		
	All Matched	Do Not Work in the 1st Year	Work in the 1st Year
	(1)	(2)	(3)
Panel A. Estimation using the *NW-type* Specification			
ln MW	-0.095**	-0.057*	-0.044
	(0.029)	(0.024)	(0.032)
Elasticity w.r.t. Minimum Wage	-0.200**	-0.199*	-0.059
	(0.060)	(0.085)	(0.043)
Obs	345,545	200,843	144,662
Panel B. Estimation using the *ADR-type* Specification			
ln MW	0.024	-0.008	0.054
	(0.033)	(0.031)	(0.054)
Elasticity w.r.t. Minimum Wage	0.051	-0.028	0.071
	(0.070)	(0.109)	(0.071)
Obs	345,244	200,530	144,471
Panel C. Estimation using the *CDLZ-type* Specification			
I_{st}	-0.004	-0.002	0.001
	(0.005)	(0.007)	(0.006)
1Y Elasticity w.r.t. Minimum Wage	-0.076	-0.067	0.009
	(0.084)	(0.207)	(0.063)
Obs	345,545	200,843	144,662

Notes: Robust standard errors are in parentheses and clustered at the state-level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. Panel C includes indicators for small increases and large federal level increases, together with demographic controls, as in Table 1.2 and state-age and time-age specific fixed effects. Panels A and B include the same set of variables as in Table 1.2. see notes to Table 1.2 for detail.

Table A.12 Minimum Wage Effects by Previous Employment Status, Age 30-54

	Outcome: Employment		
	All Matched	Do Not Work in the 1st Year	Work in the 1st Year
	(1)	(2)	(3)
Panel A. Estimation using the *NW-type* Specification			
$\ln MW$	0.005	-0.013	0.002
	(0.014)	(0.011)	(0.007)
Elasticity w.r.t. Minimum Wage	0.007	-0.066	0.002
	(0.020)	(0.057)	(0.008)
Obs	1,974,017	598,087	1,375,930
Panel B. Estimation using the *ADR-type* Specification			
$\ln MW$	0.015+	-0.036*	0.003
	(0.009)	(0.015)	(0.007)
Elasticity w.r.t. Minimum Wage	0.022+	-0.183*	0.003
	(0.013)	(0.076)	(0.008)
Obs	1,974,017	598,063	1,375,928
Panel C. Estimation using the *CDLZ-type* Specification			
I_{st}	0.002	-0.003	0.000
	(0.002)	(0.002)	(0.001)
1Y Elasticity w.r.t. Minimum Wage	0.020	-0.132	0.004
	(0.026)	(0.087)	(0.009)
Obs	1,974,017	598,087	1,375,930

Notes: Robust standard errors are in parentheses and clustered at the state-level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. Panel C includes indicators for small increases and large federal level increases, together with demographic controls, as in Table 1.2 and state-age and time-age specific fixed effects. Panels A and B include the same set of variables as in Table 1.2. see notes to Table 1.2 for detail.

Table A.13 Minimum Wage Effects by Previous Employment Status, Age 65-70

	Outcome: Employment		
	All Matched	Do Not Work in the 1st Year	Work in the 1st Year
	(1)	(2)	(3)
Panel A. Estimation using the *NW-type* Specification			
ln MW	0.044*	0.013	0.018
	(0.019)	(0.008)	(0.031)
Elasticity w.r.t. Minimum Wage	0.242*	0.362	0.025
	(0.102)	(0.224)	(0.042)
Obs	319,243	253,331	65,912
Panel B. Estimation using the *ADR-type* Specification			
ln MW	0.050*	0.024+	-0.033
	(0.019)	(0.014)	(0.064)
Elasticity w.r.t. Minimum Wage	0.275*	0.674+	-0.046
	(0.108)	(0.395)	(0.087)
Obs	319,243	253,331	65,630
Panel C. Estimation using the *CDLZ-type* Specification			
I_{st}	0.003	0.003+	0.007
	(0.004)	(0.002)	(0.009)
1Y Elasticity w.r.t. Minimum Wage	0.117	0.650+	0.080
	(0.178)	(0.339)	(0.111)
Obs	319,243	253,331	65,912

Notes: Robust standard errors are in parentheses and clustered at the state-level. +, *, **, *** are statistically significant at 10%, 5%, 1%, and 0.1%, respectively. Panel C includes indicators for small increases and large federal level increases, together with demographic controls, as in Table 1.2 and state-age and time-age specific fixed effects. Panels A and B include the same set of variables as in Table 1.2. see notes to Table 1.2 for detail.

A.5 Note on the Bunching Approach

This section applies a brand-new tool in the minimum wage literature: the bunching approach proposed by Cengiz et al. (2019). The basic idea of the method is related to the bunching. Labor economists have long recognized that a substantial fraction of workers are clustered at the exact minimum (e.g. DiNardo et al., 1996). If the minimum wage is increased, some of the jobs below the new minimum wage level will be eliminated and some portion of those will be recovered at the level exactly at or slightly above the new minimum wage level, creating a bunching at the new minimum. Then, employment effects can be identified by comparing the size of missing and excess jobs near the minimum wages and the size of missing jobs and excess jobs can be obtained by comparison with a hypothetical distribution. This method is useful to decompose the overall employment effects into bin-by-bin effects and observe where the effects occur.

In practical estimation, the key issue is how to construct a hypothetical distribution. Cengiz et al. (2019) relies on states that do not experience minimum wage increases. Therefore, we are in the difference-in-differences framework. To estimate it, they use an event-study design using state-level minimum wage increases. The unit of observation is the employment-to-population ratio by \$0.25 wage bin. A more detailed discussion is provided in Cengiz et al. (2019).

$$\frac{E_{sjt}}{N_{st}} = \sum_{\tau=-3}^{4} \sum_{k=-4}^{17} \alpha_{\tau k} I_{sjt}^{\tau k} + \mu_{sj} + \rho_{jt} + \Omega_{sjt} + \varepsilon_{sjt} \tag{A.2}$$

Equation (A.2) represents the regression specification. Unlike previous estimates, the unit of analysis is state-period-wage-bin level aggregates. E_{sjt} shows an estimate of the number of employees of state s, time t and wage bin j. The first component of the RHS shows the treatment dummies. $I_{sjt}^{\tau k}$ is equal to 1 if the increased minimum wage τ years from period t and the wage bin is in the interval k and $k+1$ dollars relative to the new one. I define 'treatment' as minimum wage increases larger than \$0.50 in 2019 USD, as in equation (3). μ_{sj} and ρ_{jt} captures state-by-wage-bin effects and period-by-wage-bin effects. Ω includes controls such as small and federal level minimum wage increases.[1] Following Cengiz et al. (2019) to obtain precise information on hourly wages, workers with imputed information are excluded, unlike in the analysis in other parts of the paper.

There are several issues related to estimating the number of workers. To estimate the number of workers and residents, I rely on the CPS earnings weight *earnwt*, as in Cengiz et al. (2019). The sum of *earnwt* in the raw CPS-ORG across all years is equal to 12 times total population, so I can estimate the population at the state-quarter level very precisely by summing them across quarters and dividing this by 3. But since we exclude all imputed workers, the number of workers is underestimated. To correct for this issue, Cengiz et al. (2019) relies on additional information from Quarterly Census of Employment and Wages (QCEW) data. Specifically, Cengiz et al. (2019) calculates $\frac{E_{sjt}}{N_{st}} = \frac{E_{sjt}^{CPS}}{E_{st}^{CPS}} * \frac{E_{st}^{QCEW}}{N_{st}^{CPS}}$, where superscript CPS shows estimates using CPS information and $QCEW$ represents information from the QCEW, respectively. If the distribution of excluded

[1] Here, I try to keep the details as close to Cengiz et al. (2019) as possible. Unlike equation (1.3), Ω include indicators for wage bins within the $[MW, MW+4]$ and $[MW-4, MW]$ for a year before the increase (*pre*), 2-3 years before the increase (*early*), and for five years after the increases (*post*). In sum, I include six ({early, pre, post} $\times$ {above, below}) for small and federal minimum wage increases.

observation is unrelated to wage bin and working time status, this method can provide reliable information for estimating the number of workers in the wage bin. I follow this procedure and multiply the number of workers by $\frac{E_{st}^{QCEW}}{E_{st}^{CPS}}$.

The employment effects are estimated as follows. Note that the dependent variable is employment-to-population ratio by wage bin. Therefore, the estimates $\alpha_{\tau k}$ will capture the effects of minimum wage increase τ years from period t and k dollars from the wage bin on the share of workers in that cell in percentage point terms. The change in the employment-to-population share between time -1 (year prior to the minimum wage increases) and year τ is calculated by $\alpha_{\tau k} - \alpha_{-1k}$ and it will show the change in number of workers per capita in that wage cell. It is normalized by calculating $\Delta a_{\tau k} = \frac{\alpha_{\tau k} - \alpha_{-1k}}{\overline{EPOP}_{-1}}$ where $\overline{EPOP}_{-1}$ is average employment-to-population ratio of treated states of the year prior to the minimum wage increases, corresponding to $\bar{y}_{-1}$ in equation (1.3). Therefore, $\Delta\alpha_{\tau k}$ implies changes in the number of jobs in the wage bin and working times relative to the total workforce. The average effects on k wage for the following five years is $\Delta a_k = \frac{1}{5}\sum_{\tau=0}^{5}\Delta a_{\tau k}$.

Although there are numerous advantages to this approach, it has serious shortcomings. Since it decomposes the workforce into 117 wage bins, we need a very large sample with precise information on hours. The CPS-ORG meets the criteria for the overall low-wage workforce, but even the CPS-ORG is small for a variety of subgroups such as elderly. Consequently, a large portion of wage bins do not contain any observations. Specific results using the bunching approach are in Figure 1.4 and Figure A.4.

A.6 Note on the Nested-CES Production Function

In the competitive labor market, the wage of each type of worker equals the marginal product. Therefore, from the production function described in section 1.6.2, we have:

$$\begin{aligned}
\ln w_y &= \ln[(1-\alpha)AK^{\alpha}L^{-\alpha}] + \tfrac{1}{\sigma_p}\ln(L) + \ln\theta_{np} + [-\tfrac{1}{\sigma_p} + \tfrac{1}{\sigma}]\ln(L_{np}) + \ln\theta - \tfrac{1}{\sigma}\ln L_y \\
\ln w_o &= \ln[(1-\alpha)AK^{\alpha}L^{-\alpha}] + \tfrac{1}{\sigma_p}\ln(L) + \ln\theta_{np} + [-\tfrac{1}{\sigma_p} + \tfrac{1}{\sigma}]\ln(L_{np}) + \ln(1-\theta) - \tfrac{1}{\sigma}\ln L_o \\
\ln w_p &= \ln[(1-\alpha)AK^{\alpha}L^{-\alpha}] + \tfrac{1}{\sigma_p}\ln(L) + \ln(1-\theta_{np}) - \tfrac{1}{\sigma_p}\ln L_p
\end{aligned}$$

From young and older workers' wages, this can be expressed as:

$$\ln(w_y/w_o) = \ln(\theta/(1-\theta)) - \frac{1}{\sigma}\ln(L_y/L_o)$$

Rearrange it. Then, obtain equation (1.4).

For comparison, from young and prime-age workers' wages,

$$\begin{aligned}
\ln(w_y/w_p) &= \ln(\theta_{np}/(1-\theta_{np})) + \tfrac{1}{\sigma_p}\ln(L_{np}/L_p) + \ln\theta - \tfrac{1}{\sigma}\ln(L_y/L_{np}) \\
&= \ln(\theta_{np}/(1-\theta_{np})) + [\tfrac{1}{\sigma_p} + \tfrac{1}{\sigma}]\ln(L_{np}/L_p) + \ln\theta - \tfrac{1}{\sigma}\ln(L_y/L_p)
\end{aligned}$$

Therefore, without proper control of the relative workforce between prime- and non-prime-age workers, regression of the relative wage between young and prime-age workers on the relative employment between the young and prime-age cannot be interpreted as a parameter in the production function.

A.7 Note on the Simulated Wage Instrument

This section explains how I calculate the simulated wage instrument based on DiNardo et al. (1996)'s "tail pasting" approach. Specifically, the simulated average wage is calculated as follows.

Let base year be 0 and comparison year be 1. Define the minimum wage at base year be MW_0 and comparison year be MW_1. Let $MW^L = \max\{MW_0, MW_1\}$. For the wage distribution above the MW^L, I use the base year's wage distribution and calculate the conditional average.[2] For the wage distribution below the MW^L, I use the wage distribution of the comparison year and calculate the conditional average. Then, I calculate the simulated average wage by the weighted sum of these two conditional averages using the base year's fraction of workers above and below the MW^L as weights. This is in line with the method in DiNardo et al. (1996) with some simplifications.

Specifically, I assume that wage distribution above MW^L is not affected by the minimum wage, and that wage distribution below the MW^L is determined by the real value of the minimum wage. Then, for wages above MW^L,

$$[1 - \boldsymbol{I}(w \leq MW^L)]f(w|t = 0, MW_0) = [1 - \boldsymbol{I}(w \leq MW^L)]f(w|t = 0; MW_1)$$

where $f(w|t)$ is the probability density function of the wages at time t. $f(w|t = 0, MW_0)$ is the actual wage distribution at time 0, and $f(w|t = 0, MW_1)$ is a counterfactual wage distribution at time 0 if minimum wage is changed to MW_1. Additionally, for a wage below MW^L,

$$[\boldsymbol{I}(w \leq MW^L)]f(w|t = 1; MW_1) = [\boldsymbol{I}(w \leq MW^L)]\psi(MW_1)f(w|t = 0; MW_1)$$

where ψ is a reweighting function defined below. Then, the counterfactual wage density will be

$$\begin{aligned} f(w|t = 0; MW_1) &= \boldsymbol{I}(w \leq m_L)\psi(MW_1)f(w|t = 1; MW_1) \\ &\quad +[1 - \boldsymbol{I}(w \leq MW^L)]f(w|t = 0; MW_0) \end{aligned}$$

Using this counterfactual wage distribution, a simulated average wage can be obtained from

$$\begin{aligned} E[w|t = 0; MW_1] &= \int wf(w|t = 0; MW_1)dw \\ &= \int \boldsymbol{I}(w \leq MW^L)\psi(MW_1)wf(w|t = 1; MW_1)dw \\ &\quad + \int [1 - \boldsymbol{I}(w \leq MW^L)]wf(w|t = 0; MW_0)dw \\ &= \psi(MW_1)P(w_1 \leq MW^L)E[w|t = 1, w \leq MW^L; MW_1] \\ &\quad +P(w_0 > MW^L)E[w|t = 0, w > MW^L; MW_0] \end{aligned}$$

where the reweighting function $\psi(MW_1)$ is equal to $\dfrac{P(w_0 \leq MW^L)}{P(w_1 \leq MW^L)}$. The reweighting function will adjust for the difference in the fraction of workers below MW^L to ensure that the counterfactual wage density integrates to 1.[3]

Do these simulated wages match the fluctuations in actual wages well? I show the evolution of the actual and simulated average wage over time using two example states: California and Michigan. Figure A.11 shows the evolution of the two average wages together with the real minimum wages.

[2] As mentioned above, in practice I use 1.2 times maximum of the minimum wage for the threshold.

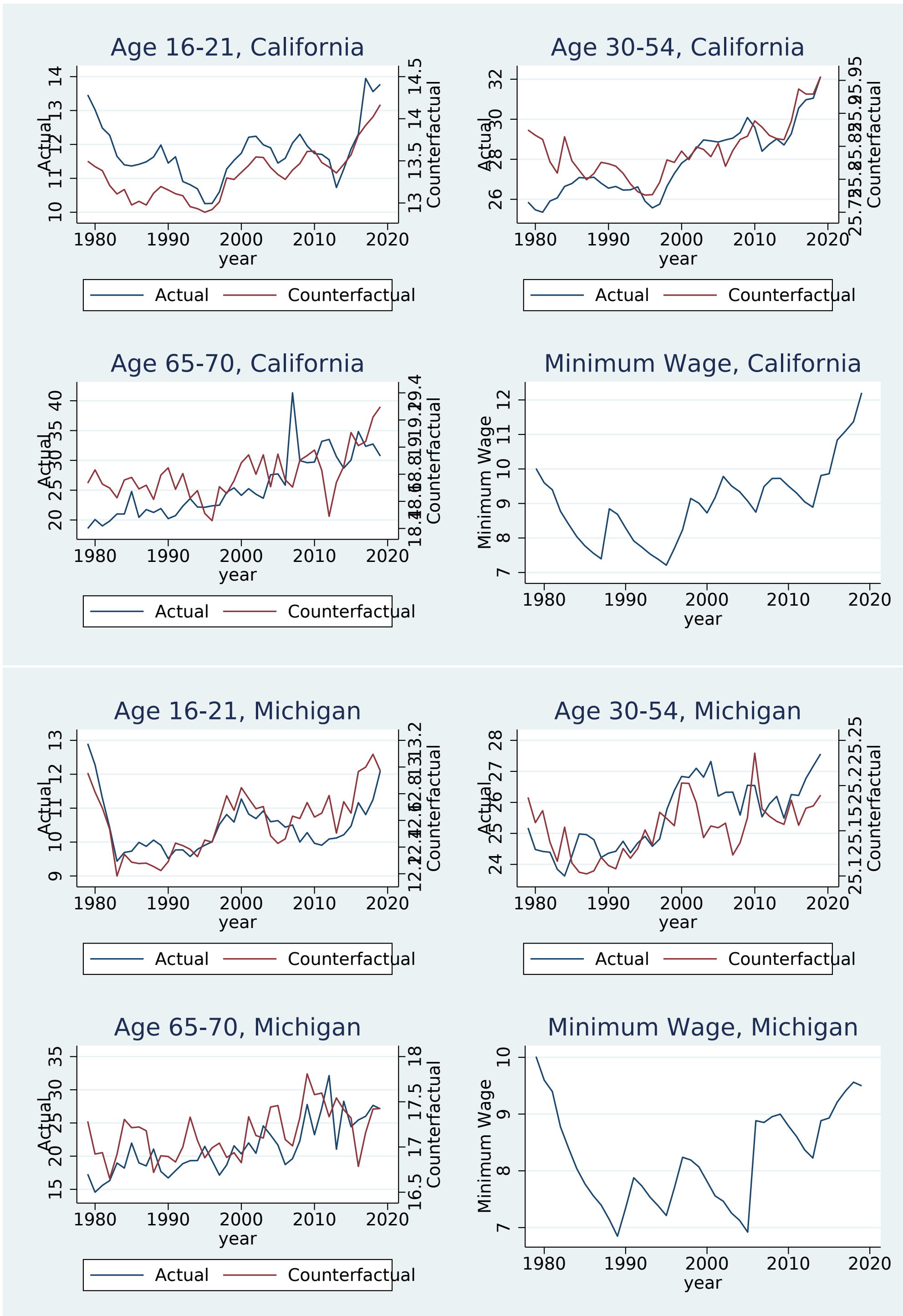
[3] The original method in DiNardo et al. (1996) also adjusts for the difference in observable characteristics. I do not apply that process. Since I use state-age or state-age-industry level as my unit of analysis, the sample size for each unit is much smaller. Therefore I use the simpler approach.

First the counterfactual wage fluctuates much less than the actual wage rates. By way of construction, the simulated wage rate holds the wage above the minimum wage constant. Therefore, especially for workers such as prime-age workers whose fraction affected by the minimum wage is small, it fluctuates little. On the other hand, if a larger fraction of workers is affected by the minimum wages, it fluctuates relatively more.

In Figure A.11, for young workers, trends closely follow those of the actual wage with little fluctuations. Note that the left axis shows the actual wage and the right axis shows the simulated wage. In contrast, the simulated wage rate deviates from the trends in actual wage much more for prime-age or older workers. For instance, in Michigan, the real value of the minimum wage dropped significantly in the 1980s and was relatively stable by the mid 2000s. Movement in the simulated wages of prime-age workers reflect these trends in minimum wages. The actual wages of prime-age workers, however, were relatively stable in the 1980s and 1990s and then experienced a sharp increase in around 2000.

The explanatory power of these simulated wages for actual wages comes from the fact that the simulated wages successfully follow the trend in one particular group - young workers. Since the average wage rate of young workers is greatly affected by the minimum wage, and the simulated wage rate can capture this, the actual relative wage rate between the young workers and the other groups can be explained by the simulated relative wage rate between the groups.

Figure A.11 Evolution of Actual and Simulated Wage, California and Michigan



A.8 Distribution of Industry across Ages

This subsection examines how similar industry distribution of workers in different ages are. For that purpose, I use the index of congruence used by Welch (1999) and Borjas (2003). The index for any two groups k and l is defined by

$$G_{kl} = \frac{\sum_c (q_{kc} - \bar{q}_c)(q_{lc} - \bar{q}_c)/\bar{q}_c}{\sqrt{(\sum_c (q_{kc} - \bar{q}_c)^2/\bar{q}_c)(\sum_c (q_{lc} - \bar{q}_c^2/\bar{q}_c)}}$$

where q_{hc} implies the portion of workers employed in industry c among group $h(= k, l)$ workers, and $\bar{q}_c$ is the fraction of the entire workforce employed in the industry. The index has a value of 1 if workers in two groups have identical industry distribution and -1 if they are employed in completely different industries.

Table A.14 shows the index of congruence across age and education groups. Overall, industry distribution of the young, prime-age, and elderly are different. $G_{young,prime} = -0.976$, $G_{young,elderly} = -0.106$ and $G_{prime,elderly} = -0.096$. It starts to show some similarity as decomposing them into education groups. The industry distribution of young workers by education groups are very similar with each other, suggesting that they are close substitutes. The elderly without high school diplomas are the group whose industry distribution is the most similar with the young workers ($G = 0.356$), followed by the elderly with high school diplomas ($G = 0.288$). The same index with prime-age high school dropouts is 0.232 and high school graduates is -0.118. It suggests that at least the less educated elderly have some potential to be substitutes to the young workers.

There are other interesting patterns in Table H1. The Index of Congruence shows that industry distribution of the better educated elderly is relatively similar to that of prime-age workers, unlike the less-educated elderly. The index between older workers with advanced degrees and prime-age workers with advanced degrees is even 0.924.

Panels B and C in Table H1 show the index for full-time and part-time workers.[4] Industry distribution of full-time young workers becomes much more similar to that of low-educated prime-age and elderly workers. G between the young apd prime-age and older high-school dropouts become 0.574 and 0.545, respectively. However, part-time young workers work in relatively different industries. It might be the result of the disproportionately high fraction of young part-time workers in 'Leisure and Hospitality' (32.7 percent) and 'Wholesale and Retail Trade' (30 percent).

In sum, there are some possibilities that the less-educated elderly could be a closer substitute for the young workers, compared to less-educated prime-age workers. The indexes are higher for full-time workers who drive the positive employment effects.

[4] $\bar{q}_c$ is calculated only by using full-time and part-time workers, respectively.

Table A.14 Index of Congruence in Industry across Age and Education Groups

		Young	Prime-Age (Age 30-54)					
		All	All	<HS	HSG	Some Col	Col Grad	Advanced
			Panel A. All Workers					
Young (Age 16-21)	All	-	-0.976	0.232	-0.118	-0.762	-0.635	-0.547
Elderly (Age 65-70)	All	-0.106	-0.096	-0.370	-0.473	0.024	0.185	0.379
	<HS	0.356	-0.412	0.575	0.139	-0.534	-0.597	-0.349
	HSG	0.288	-0.413	-0.136	-0.016	-0.113	-0.257	-0.232
	SC	-0.132	-0.028	-0.607	-0.413	0.300	0.365	0.288
	CG	-0.371	0.212	-0.753	-0.674	0.355	0.726	0.661
	Adv.	-0.422	0.328	-0.556	-0.723	0.225	0.582	0.924
			Panel B. Full-Time Workers					
Young (Age 16-21)	All	-	-0.948	0.574	0.291	-0.609	-0.642	-0.648
Elderly (Age 65-70)	All	-0.290	0.076	-0.439	-0.606	0.101	0.328	0.594
	<HS	0.545	-0.461	0.838	0.350	-0.605	-0.716	-0.438
	HSG	0.270	-0.338	0.057	0.283	0.097	-0.378	-0.326
	SC	-0.319	0.127	-0.704	-0.438	0.549	0.488	0.348
	CG	-0.539	0.288	-0.830	-0.786	0.375	0.825	0.699
	Adv.	-0.549	0.361	-0.589	-0.821	0.084	0.574	0.936
			Panel C. Part-Time Workers					
Young (Age 16-21)	All	-	-0.976	-0.050	-0.684	-0.938	-0.867	-0.773
Elderly (Age 65-70)	All	-0.653	0.487	0.179	0.770	0.417	0.320	0.168
	<HS	-0.255	0.135	0.705	0.626	-0.065	-0.165	-0.212
	HSG	-0.350	0.152	0.103	0.708	0.115	-0.005	-0.168
	SC	-0.611	0.439	-0.032	0.687	0.445	0.343	0.152
	CG	-0.718	0.600	-0.235	-0.532	0.638	0.617	0.422
	Adv.	-0.884	0.904	-0.179	0.307	0.901	0.928	0.924

Notes: See text for details. The number in each cell shows the similarity in industry distribution across groups of workers. -1 means that workers are working in a completely different set of industries, and 1 means that the industry distribution is identical. All results are weighted by CPS earnings weight (*earnwt*) variable.

APPENDIX B

APPENDIX TO CHAPTER 2

B.1 Data Appendix

B.1.1 Matching CPS-ORG

The standard way to construct longitudinal data using the CPS-ORG is to rely on household identifier variables (*hhid*, *hhnum*, and *lineno*) together with the month of the interview (*intmonth*) and state information to match first- and second-year observations. Lefgren and Madrian (1999) provides a detailed discussion. However, this set of variables generally creates multiple matches since the combination cannot uniquely identify each observation in the data set. From 2004 Q2, fortunately, researchers can uniquely identify each observation by adding additional information from *hrhhid2*, another household identifier, to the enumerated variables. By using additional information, approximately 78 percent of observations aged 16-64 in the raw data were matched. Because of the age restriction (their age should be in the 16 to 64 range in both years of interviews), approximately 4 percent of the sample was dropped. Among the matched, an additional 3 percent is dropped since the changes in age are too large or small (larger than 2 or smaller than 0) or the reported sex varies. Approximately 72 percent of observations in raw data survive the criteria. I use observations who enter the interview from 2005 to 2018, and the second-year sample consists of observations from 2006 to 2019.

B.1.2 Additional Summary Statistics

Table B.1 shows the summary statistics by the hour group. The key features are discussed above. All four wage groups are aggregated for Table B.1.

Table B.1 Summary Statistics by Working Hours

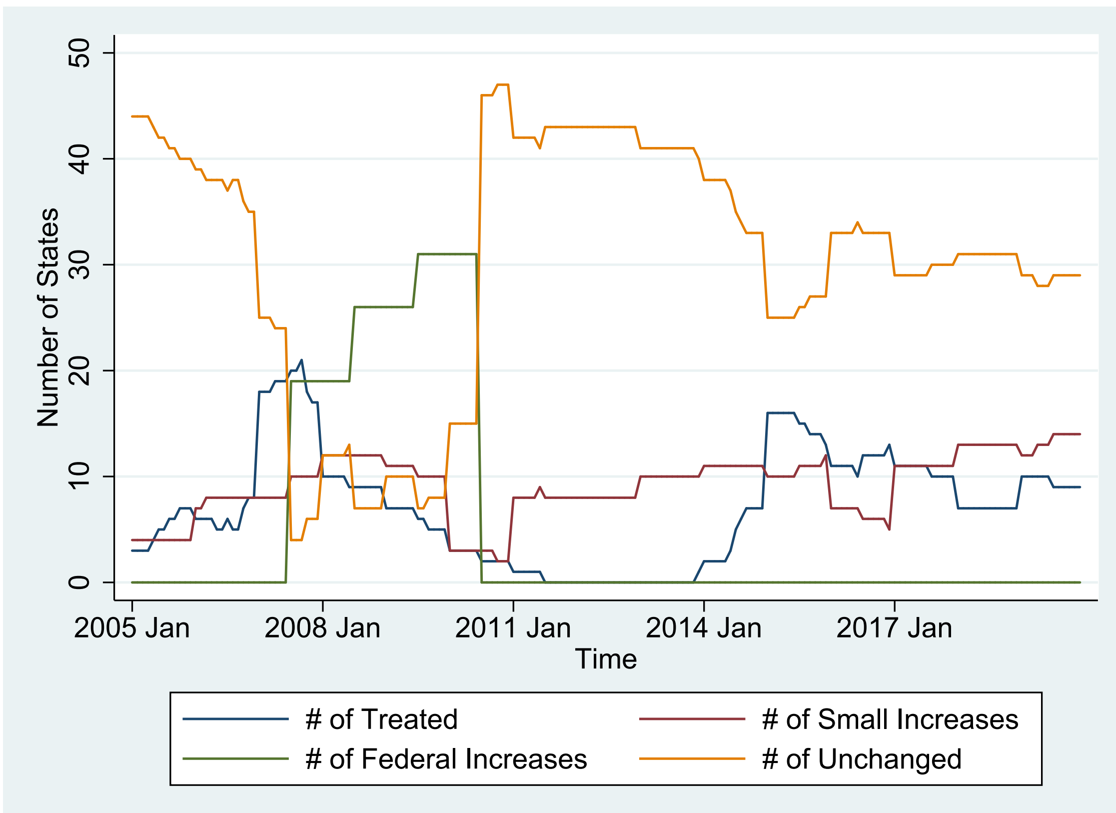
	Hours Group		
	Part-time	Full-time	Overtime
Panel A. Year 1 Economic Variable			
Log Wage (2019 USD)	2.280	2.370	2.347
	(0.194)	(0.186)	(0.231)
Paid Hourly	0.932	0.834	0.513
Panel B. Year 2 Economic Variable			
Work in the Second Year	0.744	0.856	0.902
Log Wage (2019 USD)^a	2.388	2.494	2.586
	(0.278)	(0.290)	(0.397)
Paid Hourly^a	0.903	0.815	0.534
Part-time	0.544	0.096	0.043
Full-time	0.188	0.719	0.373
Overtime	0.012	0.041	0.487
Panel C. Year 1 Demographic Variable			
Age	31.151	37.482	38.679
	(14.417)	(12.734)	(12.192)
Female	0.652	0.556	0.387
< High School	0.260	0.219	0.191
High School Graduates	0.286	0.403	0.345
Some College	0.356	0.277	0.257
College Graduates	0.083	0.084	0.140
Above B.A.	0.015	0.017	0.066
African American	0.112	0.141	0.088
Hispanic	0.176	0.309	0.238
Other	0.083	0.087	0.093
Teenager	0.275	0.041	0.023
Panel D. Wage Group			
Wage Group 1	0.334	0.158	0.194
Wage Group 2	0.313	0.239	0.204
Wage Group 3	0.219	0.304	0.285
Wage Group 4	0.134	0.298	0.317
Observations	39731	45125	6452

Notes: See notes for Table 2.1. Variables with ^{a} are measured conditional on working in the second year.

B.1.3 Number of Treated and Control States Each Year

The minimum wage increases are unevenly distributed over time and across states. Figure B.1 shows the number of states who experience treatment, small-scale state-level increases, federal-level increases within a year as well as those whose minimum wages have remained unchanged by each month from 2005 January to 2019 December. The figure shows that the minimum wage increases are highly concentrated on two periods: 2007-2010 and 2015-2019. The first era coincides with the Great Recession and federal level increases in 2007, 2008, and 2009. The major driving force of the second period is the state-level changes.

Figure B.1 Number of States Experiencing the Treatments



Notes: Number of states that experienced treatments, small-scale state-level increases, and federal increases within a year. The orange line shows the number of states whose minimum wage emained unchanged within a year.

B.2 Full Tables

This section shows the full versions of Table 2.3 and 2.4. Most of the estimates for two upper wage groups are statistically indistinguishable from zero and small in magnitude. In Panel B of Table B.3, I additionally present the results using equation 2.1. In Panel B of Table B.3, estimates in columns (3) and (4) are close to zero and insignificant. The estimate in column (3) is diluted due to the null effects on the full-time workers. In Panel B of column (4), the estimate becomes closer to zero because of the zero effects on part-time and full-time workers. As explained in Table B.1, a very minor fraction of part-time or full-time workers become overtime workers in the second year, hence it is not surprising to see null average effects on those groups. It could explain the discrepancy between finding in this paper and that of Cengiz et al. (2022) where the effects on the fraction of overtime workers are close to zero.

Table B.2 Full Version of Table 2.3

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in (0,35))$	0.056***	-0.995**	-0.044*	0.027+	-0.092
	(0.014)	(0.371)	(0.018)	(0.016)	(0.486)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [35,40])$	0.035*	-0.160	-0.009	-0.008	-0.418
	(0.015)	(0.229)	(0.010)	(0.019)	(0.695)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 > 40)$	0.113*	-3.334***	-0.060*	0.018	-2.167+
	(0.044)	(0.883)	(0.024)	(0.027)	(1.103)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in (0,35))$	0.032**	-0.128	0.000	-0.006	-0.357
	(0.011)	(0.295)	(0.011)	(0.022)	(0.596)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [35,40])$	0.030**	0.518+	0.019+	-0.016	-0.211
	(0.010)	(0.298)	(0.010)	(0.013)	(0.517)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 > 40)$	0.026	-0.173	0.006	0.003	0.036
	(0.029)	(0.979)	(0.027)	(0.033)	(1.446)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in (0,35))$	0.029*	0.152	0.019	-0.010	-0.125
	(0.015)	(0.348)	(0.015)	(0.016)	(0.481)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in [35,40])$	0.030+	-0.129	-0.002	0.008	0.208
	(0.016)	(0.283)	(0.011)	(0.011)	(0.402)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 > 40)$	-0.040	0.561	0.005	-0.004	0.330
	(0.027)	(0.883)	(0.018)	(0.017)	(1.125)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in (0,35))$	0.002	-0.103	-0.003	-0.015	-0.374
	(0.010)	(0.449)	(0.021)	(0.014)	(0.445)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in [35,40])$	0.006	0.012	0.003	0.006	0.159
	(0.013)	(0.197)	(0.007)	(0.009)	(0.415)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 > 40)$	0.066*	-0.882	-0.006	0.007	-0.643
	(0.033)	(0.697)	(0.015)	(0.019)	(1.249)
Observations	74,150	74,150	74,150	91,308	91,308
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, i.e., the initial wage minus the initial minimum wage. h^1 refers to the working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table B.3 Full Version of Table 2.4

	Work in Both Year			
	$h^2 \in (0,35)$	$h^2 \in [35,40)$	$h^2 = 40$	$h^2 > 40$
	(1)	(2)	(3)	(4)
Panel A. Across Wage and Hours Group				
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in (0,35))$	0.025	0.013	-0.036*	-0.001
	(0.016)	(0.012)	(0.016)	(0.006)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [35,40])$	0.003	-0.004	0.002	-0.001
	(0.014)	(0.012)	(0.015)	(0.008)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 > 40)$	0.005	0.018	0.074+	-0.097**
	(0.021)	(0.016)	(0.042)	(0.033)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in (0,35))$	0.011	0.012	-0.016	-0.007
	(0.016)	(0.011)	(0.017)	(0.005)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [35,40])$	-0.001	0.002	-0.018	0.017+
	(0.014)	(0.015)	(0.017)	(0.010)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 > 40)$	0.038	-0.002	-0.097*	0.060+
	(0.026)	(0.014)	(0.038)	(0.035)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in (0,35))$	0.014	-0.012	0.004	-0.005
	(0.016)	(0.012)	(0.013)	(0.005)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in [35,40])$	0.007	-0.006	0.011	-0.012*
	(0.013)	(0.011)	(0.018)	(0.006)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 > 40)$	0.008	0.011	-0.032	0.013
	(0.015)	(0.009)	(0.036)	(0.029)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in (0,35))$	-0.013	-0.001	0.017	-0.004
	(0.024)	(0.016)	(0.018)	(0.009)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in [35,40])$	-0.007	0.006	0.006	-0.005
	(0.010)	(0.008)	(0.019)	(0.010)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 > 40)$	-0.020+	-0.010	0.098*	-0.069+
	(0.011)	(0.014)	(0.044)	(0.035)
Panel B. Across Wage Group				
$D_{st} \times I(\tilde{w} = w^1 - MW^1 \leq 0.5)$	0.015	0.007	-0.014	-0.008
	(0.012)	(0.011)	(0.013)	(0.006)
$D_{st} \times I(0.5 < \tilde{w} \leq 2)$	0.007	0.007	-0.022	0.008
	(0.010)	(0.010)	(0.013)	(0.006)
$D_{st} \times I(2 < \tilde{w} \leq 3.5)$	0.009	-0.007	0.005	-0.008
	(0.010)	(0.008)	(0.011)	(0.005)
$D_{st} \times I(3.5 < \tilde{w} \leq 5)$	-0.010	0.003	0.018	-0.011
	(0.009)	(0.008)	(0.016)	(0.010)
Observations	74,150	74,150	74,150	74,150
Controls	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, i.e., the initial wage minus the initial minimum wage. h^1 refers to the working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

B.3 Alternative Treatment Definitions

This section explores how the estimates are affected by using an alternative definition of the treatments and different time periods. The goal of these exercises is to understand the relationship between the estimated effects and the different compositions of the treated groups and the role of initial minimum wages.

Tables B.4 and B.5 replicate Tables 2.2 and 2.3 with the treatments defined as an increase in effective minimum wages exceeding 50 cents in 2019 USD including federal minimum wages. Unlike the analysis in the body, the treatments include both state- and federal level changes.

The results in Tables B.4 and B.5 are largely similar to those in Table 2.2 and 2.3, but the negative estimates on the working hours decreased in absolute terms. Several factors may play a role. First, the wage effects in Column (1) are smaller with the alternative definition. This finding may reflect the gap in the initial wage that is not fully controlled by the group-by-state fixed effects. Additionally, the state-level changes may have larger wage effects. If the underlying wage effects are smaller, it is not surprising to see smaller effects on the hours. Second, compared to Tables 2.2 and 2.3, the treatment groups in Tables B.4 and B.5 have more states with lower initial minimum wages. If the absolute wage level, together with wage relative to minimum, plays a role, it may explain the patterns in Tables B.4 and B.5.

In Table B.6 and B.7, I additionally estimated Equations (2.1) and (2.2) using observations from years 2011-2019. That period saw no federal minimum wage increases. Further, the average minimum wages of the treated states before the treatment are higher during this time period.[1] These results show that the negative impacts on working hours are strengthened, and the wage effects are much clearer.

By comparing the patterns in Tables 2.2, 2.3, B.4, B.5, B.6, and B.7, we can learn that the negative effects on the working hours are larger when the high-minimum wage states are treated. If the high-minimum wage states are treated, their new minimum could be binding for more workers, and it may cause larger negative effects on the working hours. Table B.4 shows that when the treated group contained a treatment for low-wage states bound by federal minimum wages, the negative effects on hours are rather modest, and the wage gains far exceed the loss in hours. However, when the states with higher minimum wages further increase their minimum, as many states recently do, the loss in working hours could offset wage gains, raising a question of whether increasing the minimum wage could boost the earnings of the low-wage workers.

[1] The unweighted average real minimum wage of the treated states one month before the increase is \$9.32 for the state-level treatment during the years 2005-2019, \$8.96 for the state- and federal increases during the years 2005-2019, and \$10.26 for the state-level increases during the years 2011-2019.

Table B.4 Replication of Table 2.2, Using Federal and State Increases as the Treatments

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} = w^1 - MW^1 \leq 0.5)$	0.040**	-0.481+	-0.018+	0.010	-0.090
	(0.012)	(0.242)	(0.010)	(0.011)	(0.374)
$D_{st} \times I(0.5 < \tilde{w} \leq 2)$	0.014+	-0.085	0.001	-0.008	-0.335
	(0.007)	(0.167)	(0.006)	(0.010)	(0.293)
$D_{st} \times I(2 < \tilde{w} \leq 3.5)$	0.007	-0.103	-0.001	-0.005	-0.280
	(0.011)	(0.208)	(0.009)	(0.010)	(0.350)
$D_{st} \times I(3 < \tilde{w} \leq 5)$	0.003	-0.094	-0.002	0.000	-0.045
	(0.010)	(0.164)	(0.007)	(0.007)	(0.300)
Observations	74,150	74,150	74,150	91,308	91,308
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, i.e., the initial wage minus the initial minimum wage. h^1 refers to the working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table B.5 Replication of Table 2.3, Using Federal and State Increases as the Treatments

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in (0, 35))$	0.040**	-0.807*	-0.032*	0.011	-0.271
	(0.013)	(0.332)	(0.015)	(0.012)	(0.414)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [35, 40])$	0.032*	0.179	0.002	0.004	0.124
	(0.014)	(0.212)	(0.009)	(0.016)	(0.553)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 > 40)$	0.080+	-1.321	-0.019	0.039	0.497
	(0.047)	(1.407)	(0.030)	(0.028)	(1.600)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in (0, 35))$	0.012	-0.490+	-0.017	-0.008	-0.560
	(0.009)	(0.279)	(0.011)	(0.018)	(0.513)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [35, 40])$	0.013	0.388+	0.019+	-0.012	-0.237
	(0.009)	(0.218)	(0.010)	(0.010)	(0.391)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 > 40)$	0.032	-0.461	-0.001	0.036	1.085
	(0.027)	(0.893)	(0.024)	(0.030)	(1.571)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in (0, 35))$	0.006	0.054	0.008	-0.004	0.039
	(0.013)	(0.314)	(0.016)	(0.015)	(0.454)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in [35, 40])$	0.011	-0.238	-0.006	0.003	-0.139
	(0.013)	(0.211)	(0.008)	(0.010)	(0.377)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 > 40)$	-0.013	0.234	-0.002	-0.062*	-2.878+
	(0.023)	(0.693)	(0.015)	(0.027)	(1.454)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in (0, 35))$	-0.006	0.002	-0.003	-0.015	-0.230
	(0.009)	(0.346)	(0.017)	(0.011)	(0.380)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in [35, 40])$	0.003	-0.014	0.001	0.005	0.070
	(0.010)	(0.148)	(0.006)	(0.008)	(0.348)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 > 40)$	0.030	-0.871	-0.012	0.016	-0.289
	(0.030)	(0.591)	(0.013)	(0.016)	(1.067)
Observations	74,150	74,150	74,150	91,308	91,308
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, i.e., the initial wage minus the initial minimum wage. h^1 refers to the working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table B.6 Replication of Table 2.2, Years 2011-2019

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} \leq 0.5)$	0.062***	-1.590***	-0.061**	0.004	-1.240*
	(0.017)	(0.379)	(0.018)	(0.018)	(0.468)
$D_{st} \times I(0.5 < \tilde{w} \leq 2)$	0.060***	0.146	0.012	-0.013	-0.387
	(0.008)	(0.326)	(0.013)	(0.016)	(0.459)
$D_{st} \times I(2 < \tilde{w} \leq 3.5)$	0.043*	-0.313	-0.003	0.001	-0.173
	(0.018)	(0.334)	(0.014)	(0.016)	(0.538)
$D_{st} \times I(3.5 < \tilde{w} \leq 5)$	0.032*	-0.333	-0.012	-0.002	-0.234
	(0.012)	(0.361)	(0.013)	(0.013)	(0.564)
Observations	46,856	46,856	46,856	57,103	57,103
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, i.e., the initial wage minus the initial minimum wage. h^1 refers to the working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table B.7 Replication of Table 2.3, Years 2011-2019

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in (0,35))$	0.058**	-1.661***	-0.072**	0.025	-0.625
	(0.017)	(0.460)	(0.023)	(0.019)	(0.540)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [35,40])$	0.052*	-0.820*	-0.037*	-0.033	-1.765**
	(0.022)	(0.397)	(0.017)	(0.021)	(0.653)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 > 40)$	0.153*	-5.001***	-0.097**	-0.007	-4.484**
	(0.063)	(1.265)	(0.030)	(0.038)	(1.363)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in (0,35))$	0.060***	-0.027	0.008	-0.012	-0.514
	(0.012)	(0.534)	(0.022)	(0.029)	(0.658)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [35,40])$	0.058***	0.506	0.020	-0.018	-0.253
	(0.011)	(0.386)	(0.016)	(0.018)	(0.550)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 > 40)$	0.077+	-1.200	-0.010	0.017	-0.292
	(0.040)	(1.284)	(0.040)	(0.027)	(1.300)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in (0,35))$	0.060***	-0.268	0.006	-0.003	-0.228
	(0.014)	(0.538)	(0.022)	(0.026)	(0.799)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in [35,40])$	0.041+	-0.319	-0.007	0.007	0.030
	(0.023)	(0.297)	(0.013)	(0.019)	(0.692)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 > 40)$	-0.028	-0.493	-0.014	-0.025	-1.646
	(0.030)	(1.035)	(0.022)	(0.030)	(2.126)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in (0,35))$	0.016	-1.336+	-0.067*	-0.040+	-2.096**
	(0.018)	(0.695)	(0.032)	(0.020)	(0.618)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in [35,40])$	0.026+	0.133	0.007	0.010	0.471
	(0.015)	(0.297)	(0.011)	(0.018)	(0.798)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 > 40)$	0.108**	-0.974	-0.005	0.032*	0.386
	(0.032)	(0.861)	(0.020)	(0.016)	(1.048)
Observations	46,856	46,856	46,856	57,103	57,103
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, i.e., the initial wage minus the initial minimum wage. h^1 refers to the working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

B.4 Event-Study Framework Including Lags and Leads

In this subsection, I tested how estimates are affected by the inclusion of the lags and leads of minimum wage increases using the following event-study specifications.

$$\begin{aligned} y_{ijhst} &= \alpha + \sum_{\tau=-2}^{2} \sum_{j} \beta_{j\tau} \times D_{st\tau} \boldsymbol{I}(w^1 \in \text{Group } j) \\ &\quad + X'_{ist}\delta + W'_{st}\eta + \Omega + \lambda_{jh} + \rho_{js} + \rho_{jt} + e_{ist} \end{aligned} \tag{B.1}$$

$$\begin{aligned} y_{ijhst} &= \alpha + \sum_{\tau=-2}^{2} \sum_{j} \sum_{h} \beta_{jh\tau} \times D_{st\tau} \boldsymbol{I}(w^1 \in \text{Group } j) \times \boldsymbol{I}(h^1 \in \text{Group } h) \\ &\quad + X'_{ist}\delta + W'_{st}\eta + \Omega + \lambda_{jh} + \rho_{js} + \rho_{jt} + e_{ist} \end{aligned} \tag{B.2}$$

Here, Ω included the indicators for the past and future small-scale and federal minimum wage increases interacted with the indicators for each wage group. The specification follows the event-study specifications, but the interpretation is not straightforward as mentioned. Since information on minimum wage increases is often publicly available several years prior to the actual increases, $\beta_{j\tau}$s and $\beta_{jh\tau}$s for the future minimum wage increases could be interpreted as the effects of the expected minimum wage increases. However, interpretations are not clear for the coefficients for the past minimum wage increases. The groups are defined based on the labor market outcomes during the first-year, which might have already been affected by past minimum wage increases. Therefore, it cannot be understood as the medium-run effects of minimum wage increases without imposing stronger assumptions. These coefficients, especially for the intensive margin responses, might be better understood as additional effects in the following years.

All the regressions included the aforementioned controls and workers in Groups 3 and 4, while I report the coefficients for only the bottom two groups. The top left panel shows the estimates from Equation (B.1) and the other three panels show them from Equation (B.2). Year 0 is the case when minimum wage increases occur between first and second year interviews. Year 1 and 2 show the 1 and 2 years after the minimum wage increases, respectively.

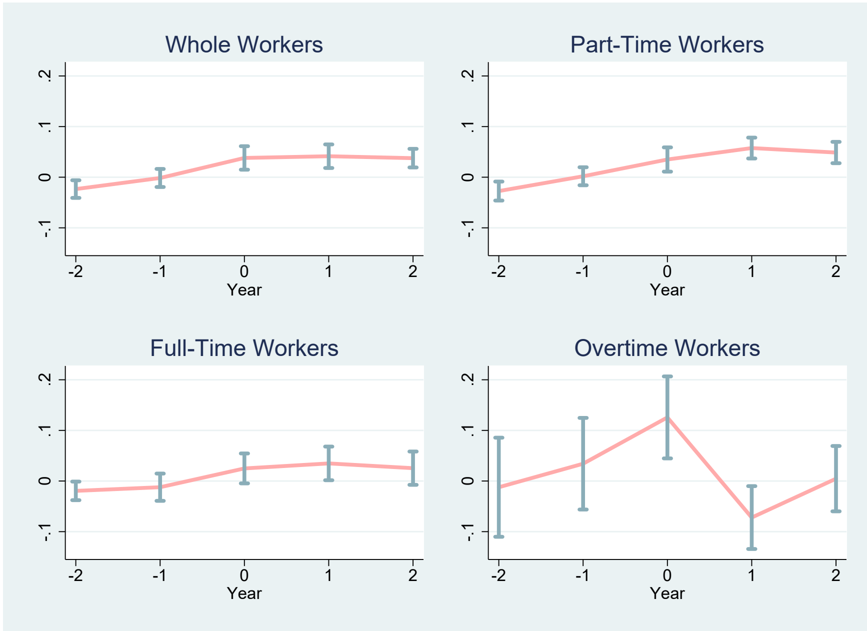
The first two figures show the wage effects. They show that part-time and full-time workers enjoy persistent wage gains, while some of the positive effects on overtime workers are offset. However, given the evidence that some of the overtime workers are moved to full-time work (Table 2.4), the estimates are difficult to interpret.

Figures B.4 through B.7 show the intensive margin response. The figures demonstrate an interesting pattern of adjustments. Effects are found only for the first year, and there are no additional adjustments in the following years. It is consistent with the evidence that the working hours are adjusted quickly (Belman and Wolfson, 2010).

The next two figures show the extensive margin responses. They do not show any clear instantaneous effects (year 0) as Tables 2.2 and 2.3 do. However, Figure B.8 suggested the possibility of extensive margin responses in the year following the minimum wage increases. In this case, the employers may adjust hours first, and then move to the firing channel. This effect is found only in part-time workers.

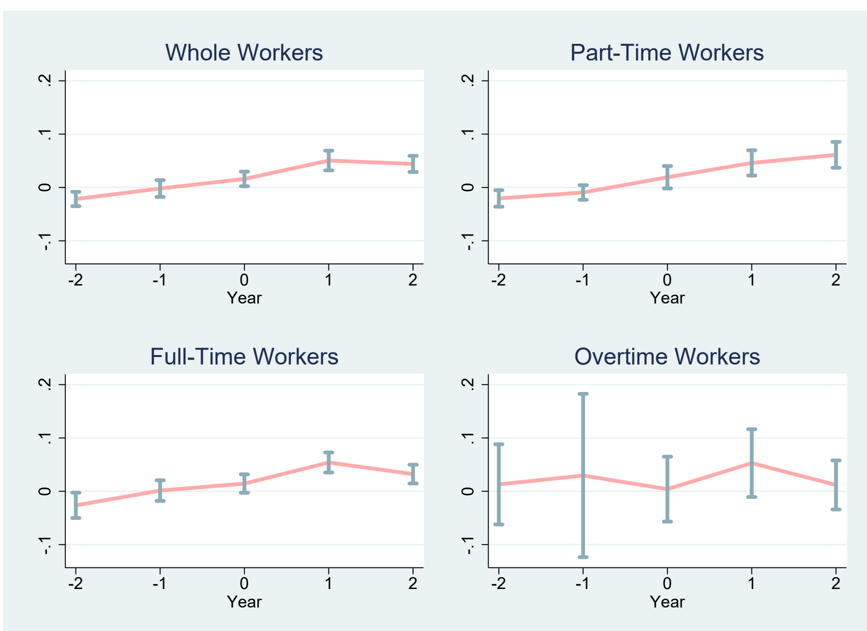
Figures B.10 through B.13 show the effects on the probability of working overtime and 40 hours per week.

Figure B.2 Effects on the Log Wage, $w^1 - MW^1 \leq 0.5$



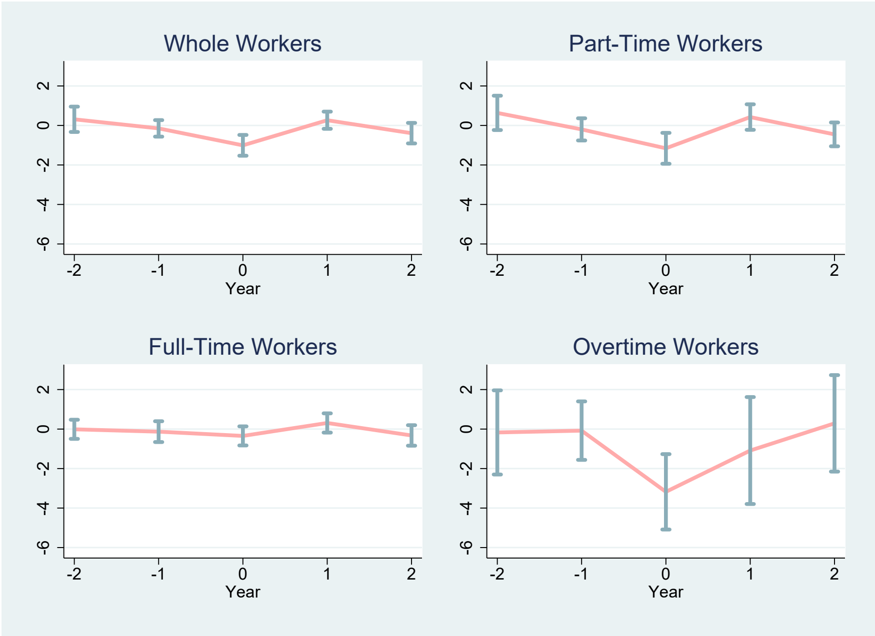
Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{1\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{1h\tau}$ in equation (B.2) for $h \in$ {Part-time, Full-time, Overtime}, respectively.

Figure B.3 Effects on the Log Wages, $0.5 \leq w^1 - MW^1 \leq 2$



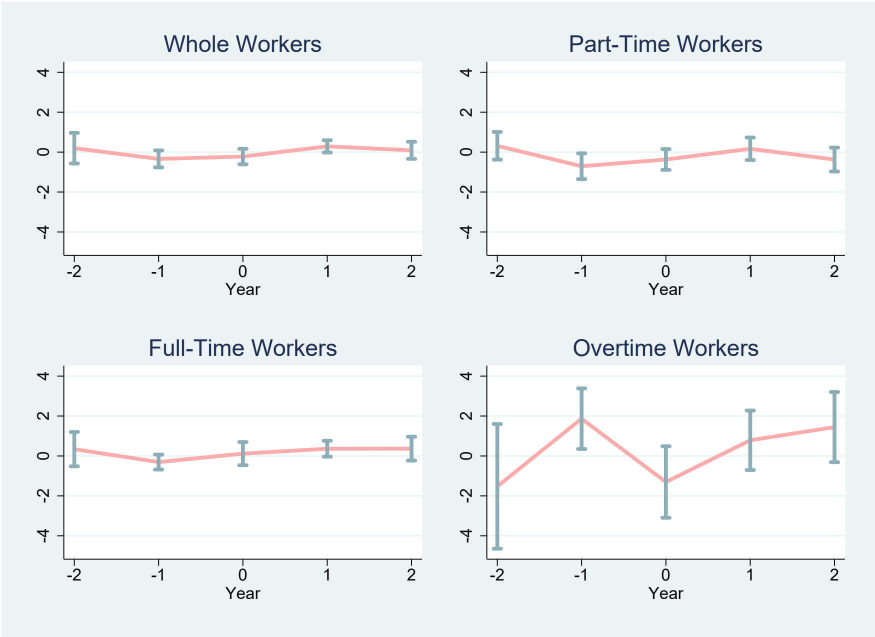
Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{2\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{2h\tau}$ in equation (B.2) for $h \in$ {Part-time, Full-time, Overtime}, respectively.

Figure B.4 Effects on the Hours of Work, $w^1 - MW^1 \leq 0.5$



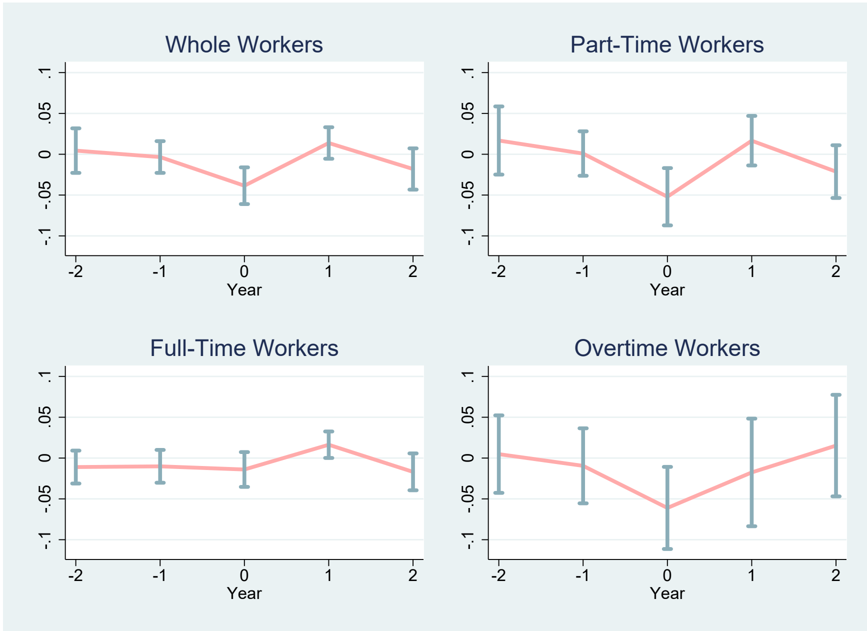
Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{1\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{1h\tau}$ in equation (B.2) for $h \in$ {Part-time, Full-time, Overtime}, respectively.

Figure B.5 Effects on the Hours of Work, $0.5 \leq w^1 - MW^1 \leq 2$



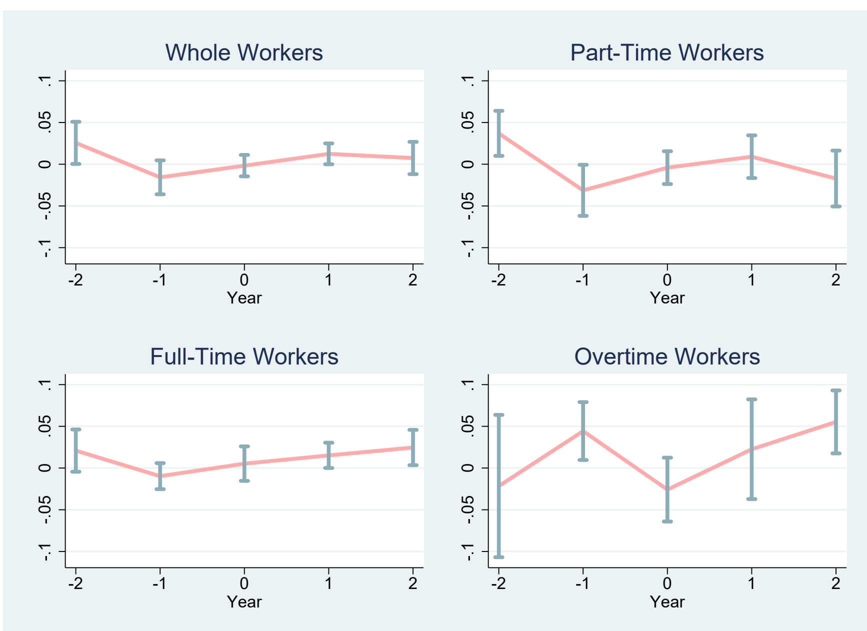
Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{2\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{2h\tau}$ in equation (B.2) for $h \in$ {Part-time, Full-time, Overtime}, respectively.

Figure B.6 Effects on the Log Hours of Work, $w^1 - MW^1 \leq 0.5$



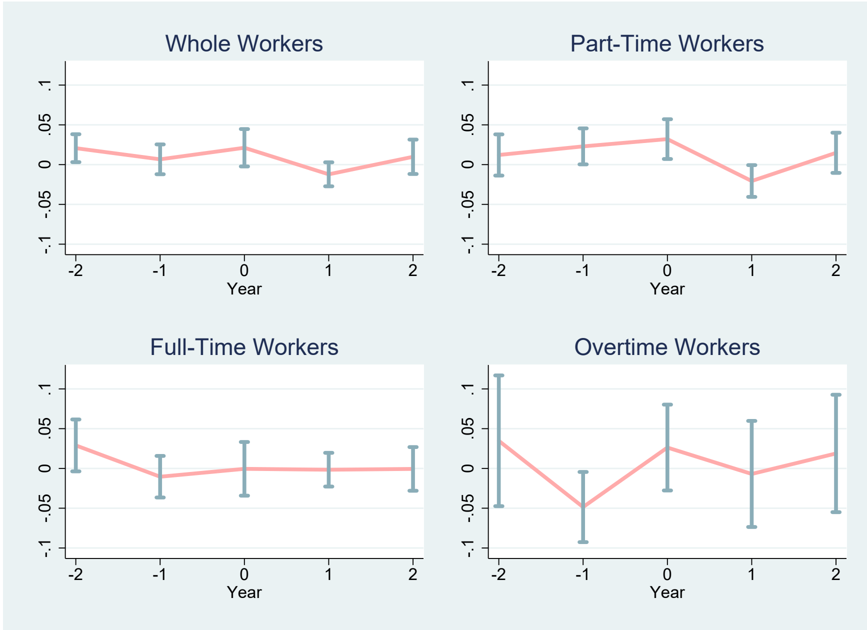
Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{1\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{1h\tau}$ in equation (B.2) for $h \in$ {Part-time, Full-time, Overtime}, respectively.

Figure B.7 Effects on the Log Hours of Work, $0.5 \leq w^1 - MW^1 \leq 2$



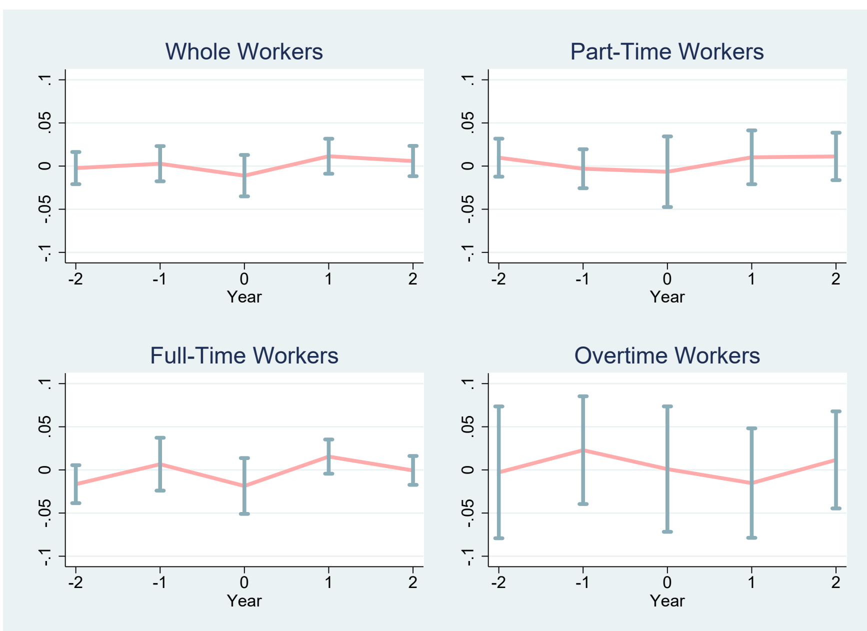
Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{2\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{2h\tau}$ in equation (B.2) for $h \in$ {Part-time, Full-time, Overtime}, respectively.

Figure B.8 Effects on Work, $w^1 - MW^1 \leq 0.5$



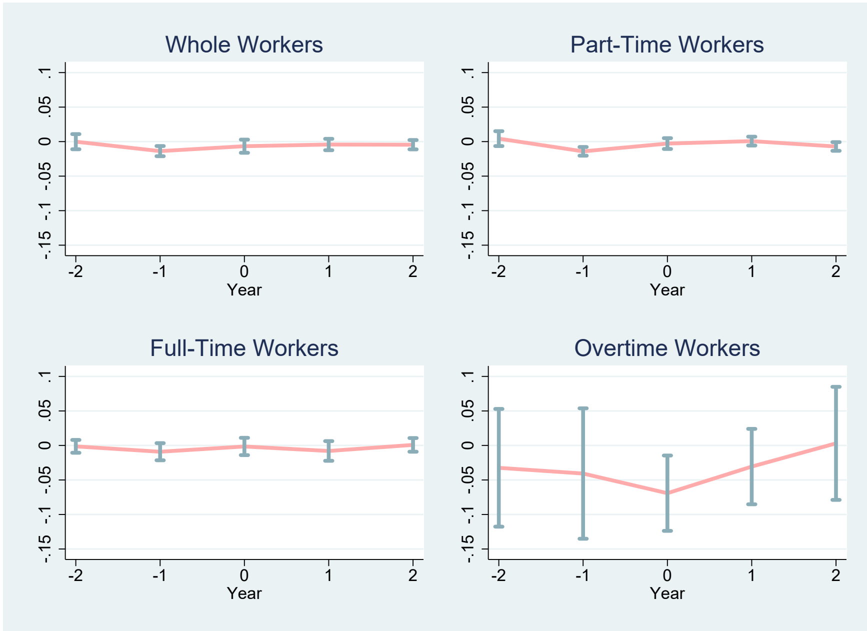
Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{1\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{1h\tau}$ in equation (B.2) for $h \in$ {Part-time, Full-time, Overtime}, respectively.

Figure B.9 Effects on Work, $0.5 \leq w^1 - MW^1 \leq 2$



Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{2\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{2h\tau}$ in equation (B.2) for $h \in$ {Part-time, Full-time, Overtime}, respectively.

Figure B.10 Effects on Overtime, $w^1 - MW^1 \leq 0.5$

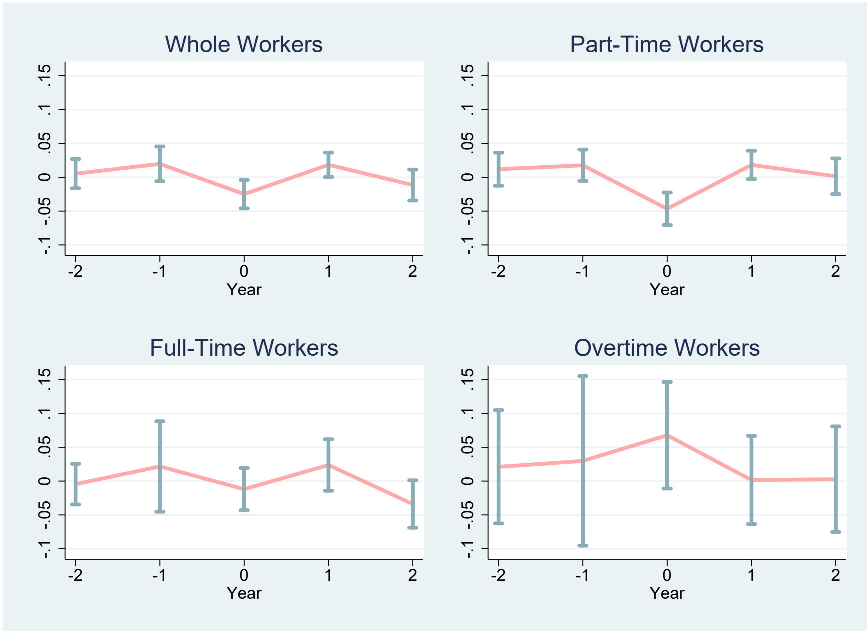


Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{1\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{1h\tau}$ in equation (B.2) for $h \in$ {Part-time, Full-time, Overtime}, respectively.

Figure B.11 Effects on Overtime, $0.5 \leq w^1 - MW^1 \leq 2$

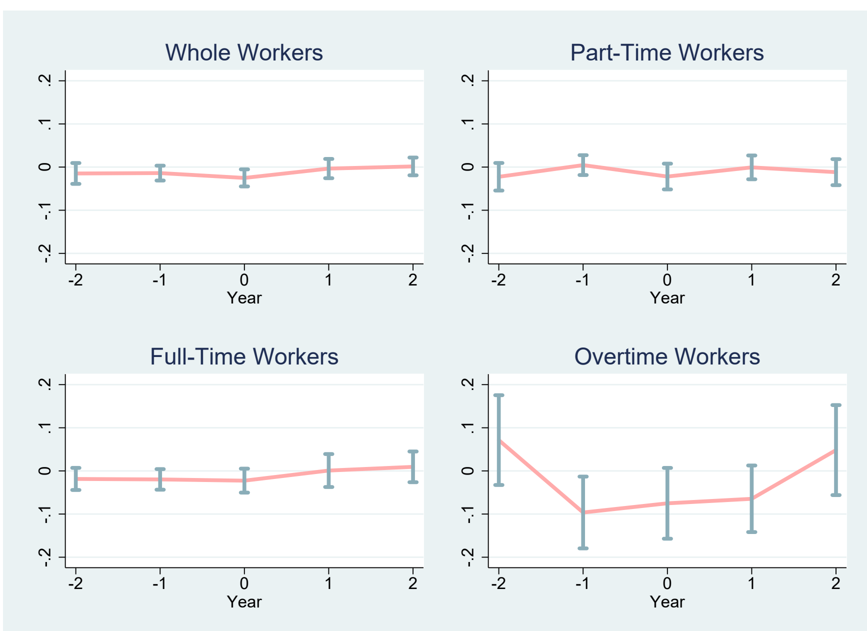
Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{2\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{2h\tau}$ in equation (B.2) for $h \in$ {Part-time, Full-time, Overtime}, respectively.

Figure B.12 Effects on $h = 40$, $w^1 - MW^1 \leq 0.5$



Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{1\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{1h\tau}$ in equation (B.2) for $h \in \{\text{Part-time, Full-time, Overtime}\}$, respectively.

Figure B.13 Effects on $h = 40, 0.5 \leq w^1 - MW^1 \leq 2$



Notes: The top left panel shows point estimates and 95 percent confidence interval of $\beta_{2\tau}$ in equation (B.1), and the other three panels show the same point estimates and 95 percent confidence interval of $\beta_{2h\tau}$ in equation (B.2) for $h \in \{\text{Part-time, Full-time, Overtime}\}$, respectively.

B.5 Robustness Check of the TWFE Results

In this subsection, I explored the effects using an alternative estimator proposed by de Chaisemartin and d'Haultfoeuille (2020). In this paper's framework, treatments were not staggered since treatments can be turned 'on' and 'off' in the same state. de Chaisemartin and d'Haultfoeuille (2020)'s estimator can be used in this situation unlike most other estimates proposed in the literature relying on the staggered assumption. To use the estimator provided by Stata's *did_multiplegt* package, I made a few revisions to my model. Since their package is designed for the TWFE model, I first change the regression equation to

$$y_{ihst} = \alpha + \beta D_{st} + X'_{ist}\delta + W'_{ist}\eta + \Omega + \lambda_h + \rho_s + \rho_t + e_{ist}$$

using observations in wage bin j. The next revision is to change the unit of time. All my estimates so far use the month as the unit of time. Since the estimator proposed by de de Chaisemartin and d'Haultfoeuille (2020) compares joiners in period t to a stable group that is not treated in both t and $t-1$ and leavers to another stable group who are treated in both periods, if some periods have neither joiners nor leavers, I lose them in the estimation. However, the minimum wage increases are not evenly distributed monthly, so many periods have neither joiners nor leavers. To minimize this concern, I changed the temporal unit to quarters, instead of months.

In general, the estimates increase substantially in absolute magnitude if I use an alternative estimator, and it is very imprecisely estimated partly due to the smaller sample size. However, the results are qualitatively similar, except for the positive effects on the working hours of the lower wage group in Panel B.

Table B.8 TWFE and the Alternative Estimator

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
Panel A. $\tilde{w} \leq 0.5$					
TWFE	0.052***	-0.806**	-0.030*	0.019	-0.130
	(0.014)	(0.259)	(0.012)	0.019	-0.130
Observations	15,310	15,310	15,310	20,508	20,508
Alternative Estimator	0.079	-1.939	-0.121	-0.005	-1.957
	(0.064)	(1.806)	(0.087)	(0.070)	(2.578)
Observations	5,158	5,158	5,158	7,265	7,265
Panel B. $0.5 < \tilde{w} \leq 2$					
TWFE	0.030**	0.087	0.006	-0.012	-0.365
	(0.009)	(0.229)	(0.007)	(0.014)	(0.416)
Observations	19,117	19,117	19,117	24,102	24,102
Alternative Estimator	0.069	1.575+	0.042	0.036	2.841+
	(0.053)	(0.948)	(0.039)	(0.039)	(1.619)
Observations	6,987	6.987	6,987	8,950	8,950

Notes: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1 . Standard errors of alternative estimators are calculated by 500 iterations of Bootstrap.

B.6 Results Using a More-Detailed Hours Bin

This subsection presents the results using a more-detailed classification of the part-time workers and re-estimating Equation (2.2). In Table B.9, I divide part-time workers into two groups: those who work 20 hours or less and those who work more than 20 hours per week. The first group ($h^1 \in (0, 20]$) has 22,147 workers and the second has 17,584 workers. For Table B.10, I divide the part-time workers into three groups: $0 < h^1 < 15, 15 \leq h^1 < 25$, and $25 \leq h^1 < 35$. The three groups have 7,892, 16,956, and 14,883 workers respectively. Although the first group is smaller in total size, its size is roughly similar to the third group if I consider only the workers in the lowest wage group.

Tables B.9 and B.10 show that the results for the part-time and overtime workers are almost identical, so I focus on variations within the part-time workers. As briefly mentioned in Section 2.6, the effects are almost the same in Column (3). It suggests that the relationship between the initial hours and intensive margin effects are not continuous; rather, a large, discrete jump occurred between part-time and full-time, whereas the effects are almost identical among the part-time workers.

Table B.9 Replication of Table 2.3 using More Detailed Hours Bin

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in (0,20])$	0.054**	-0.947*	-0.049*	0.029	-0.087
	(0.016)	(0.427)	(0.024)	(0.026)	(0.627)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in (20,35))$	0.059***	-1.244**	-0.047**	0.019	-0.425
	(0.015)	(0.362)	(0.014)	(0.013)	(0.477)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [35,40])$	0.035*	-0.135	-0.008	-0.008	-0.426
	(0.015)	(0.226)	(0.009)	(0.019)	(0.672)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 > 40)$	0.113*	-3.408***	-0.063**	0.017	-2.276*
	(0.044)	(0.854)	(0.022)	(0.027)	(1.104)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in (0,20])$	0.031*	0.032	0.007	-0.002	-0.064
	(0.015)	(0.393)	(0.020)	(0.024)	(0.692)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in (20,35))$	0.032*	-0.325	-0.008	-0.012	-0.735
	(0.014)	(0.581)	(0.021)	(0.029)	(0.810)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [35,40])$	0.030**	0.405	0.014	-0.017	-0.327
	(0.010)	(0.299)	(0.009)	(0.013)	(0.516)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 > 40)$	0.026	-0.299	0.000	0.002	-0.091
	(0.029)	(0.954)	(0.025)	(0.033)	(1.438)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in (0,20])$	0.014	0.285	0.037+	-0.023	-0.299
	(0.019)	(0.369)	(0.019)	(0.018)	(0.482)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in (20,35))$	0.044**	0.296	0.015	0.008	0.585
	(0.016)	(0.473)	(0.018)	(0.024)	(0.706)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in [35,40])$	0.030+	-0.128	-0.002	0.008	0.195
	(0.016)	(0.277)	(0.011)	(0.011)	(0.397)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 > 40)$	-0.040	0.584	0.006	-0.004	0.350
	(0.027)	(0.894)	(0.018)	(0.017)	(1.113)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in (0,20])$	0.008	-0.448	-0.017	-0.030	-0.793
	(0.016)	(0.605)	(0.033)	(0.026)	(0.673)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in (20,35))$	-0.003	0.153	0.007	0.000	0.175
	(0.015)	(0.559)	(0.019)	(0.016)	(0.598)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in [35,40])$	0.006	-0.046	0.000	0.006	0.149
	(0.013)	(0.196)	(0.007)	(0.009)	(0.414)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 > 40)$	0.066*	-0.899	-0.007	0.007	-0.645
	(0.033)	(0.677)	(0.014)	(0.019)	(1.236)
Observations	74,150	74,150	74,150	91,308	91,308
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, i.e., the initial wage minus the initial minimum wage. h^1 refers to the working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table B.10 Replication of Table 2.3 using More Detailed Hours Bin

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in (0, 15))$	0.050+	-0.991	-0.054	0.006	-0.694
	(0.027)	(0.646)	(0.041)	(0.039)	(0.902)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [15, 25))$	0.048***	-0.819*	-0.031+	0.048*	0.536
	(0.012)	(0.381)	(0.016)	(0.022)	(0.649)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [25, 35))$	0.070***	-1.092**	-0.043**	0.016	-0.387
	(0.015)	(0.386)	(0.015)	(0.014)	(0.500)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [35, 40])$	0.035*	-0.141	-0.008	-0.008	-0.466
	(0.015)	(0.232)	(0.010)	(0.019)	(0.674)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 > 40)$	0.113*	-3.467***	-0.067**	0.016	-2.342*
	(0.044)	(0.823)	(0.020)	(0.027)	(1.111)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in (0, 15))$	0.026+	-0.091	-0.005	-0.016	-0.412
	(0.015)	(0.619)	(0.037)	(0.030)	(0.631)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [15, 25))$	0.027+	0.088	0.012	0.009	0.212
	(0.015)	(0.540)	(0.020)	(0.033)	(0.796)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [25, 35))$	0.039**	-0.346	-0.009	-0.018	-0.914
	(0.015)	(0.481)	(0.017)	(0.026)	(0.874)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [35, 40])$	0.030**	0.312	0.009	-0.018	-0.425
	(0.010)	(0.309)	(0.010)	(0.013)	(0.515)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 > 40)$	0.026	-0.358	-0.003	0.001	-0.164
	(0.029)	(0.938)	(0.024)	(0.033)	(1.442)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in (0, 15))$	0.020	0.404	0.069	-0.066*	-0.758
	(0.028)	(0.892)	(0.056)	(0.025)	(0.684)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in [15, 25))$	0.015	0.089	0.006	0.007	0.212
	(0.020)	(0.479)	(0.019)	(0.018)	(0.496)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in [25, 35))$	0.046**	0.196	0.011	-0.002	0.167
	(0.017)	(0.450)	(0.017)	(0.024)	(0.739)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in [35, 40])$	0.030+	-0.113	-0.001	0.008	0.202
	(0.017)	(0.264)	(0.010)	(0.011)	(0.387)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 > 40)$	-0.040	0.622	0.009	-0.004	0.366
	(0.027)	(0.892)	(0.017)	(0.016)	(1.110)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in (0, 15))$	0.007	0.006	-0.021	-0.063	-0.901
	(0.043)	(1.469)	(0.098)	(0.054)	(1.434)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in [15, 25))$	0.008	-0.601	-0.021	-0.016	-0.687
	(0.015)	(0.639)	(0.025)	(0.030)	(0.852)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in [25, 35))$	-0.005	0.012	0.002	0.002	0.096
	(0.016)	(0.576)	(0.021)	(0.018)	(0.660)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in [35, 40])$	0.006	-0.023	0.002	0.006	0.152
	(0.013)	(0.189)	(0.007)	(0.009)	(0.416)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 > 40)$	0.066*	-0.880	-0.006	0.007	-0.643
	(0.033)	(0.688)	(0.015)	(0.019)	(1.243)
Observations	74,150	74,150	74,150	91,308	91,308
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, i.e., the initial wage minus the initial minimum wage. h^1 refers to the working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

B.7 Results using Hourly Paid Workers

Appendix section B.7 shows the effects only using hourly paid workers. I defined hourly paid workers as those who were paid hourly in the first year, regardless of their second-year method of payment. The effects on part-time and full-time workers were almost identical. Given that most part-time and full-time low-wage workers were hourly-paid, this result is not surprising. However, the effects on overtime workers were different. There were no significant results for overtime workers. Note that more than half of the overtime workers are salaried, and the ratio was even higher for directly affected workers. Among 1,200 workers in the lowest wage group overtime workers, only 400 were paid hourly in the first year.

Table B.11 Replication of Table 2.2, Hourly Paid Only

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} \leq 0.5)$	0.048***	-0.755*	-0.034*	0.022	-0.037
	(0.013)	(0.343)	(0.016)	(0.016)	(0.492)
$D_{st} \times I(0.5 < \tilde{w} \leq 2)$	0.031**	0.080	0.005	-0.012	-0.428
	(0.009)	(0.218)	(0.007)	(0.012)	(0.406)
$D_{st} \times I(2 < \tilde{w} \leq 3.5)$	0.019	-0.073	0.003	-0.002	-0.029
	(0.014)	(0.252)	(0.011)	(0.012)	(0.416)
$D_{st} \times I(3.5 < \tilde{w} \leq 5)$	0.003	-0.052	0.001	0.002	0.070
	(0.013)	(0.226)	(0.009)	(0.010)	(0.379)
Observations	62,942	62,942	62,942	78,266	78,266
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, i.e., the initial wage minus the initial minimum wage. h^1 refers to the working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table B.12 Replication of Table 2.3, Hourly Paid Only

	Work in Both Years			Work in First Year	
	Log Wage	Hours of Work	Log Hour	Work	Hours of Work
	(1)	(2)	(3)	(4)	(5)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in (0, 35))$	0.060***	-1.144*	-0.050*	0.025	-0.296
	(0.014)	(0.438)	(0.022)	(0.017)	(0.537)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 \in [35, 40])$	0.028+	0.009	-0.004	0.014	0.469
	(0.016)	(0.287)	(0.012)	(0.022)	(0.772)
$D_{st} \times I(\tilde{w} \leq 0.5, h^1 > 40)$	0.011	-0.742	-0.018	0.034	0.584
	(0.040)	(1.765)	(0.039)	(0.059)	(2.309)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in (0, 35))$	0.031**	-0.110	-0.001	-0.009	-0.446
	(0.011)	(0.314)	(0.011)	(0.025)	(0.648)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 \in [35, 40])$	0.035**	0.379	0.015	-0.013	-0.213
	(0.012)	(0.321)	(0.012)	(0.016)	(0.559)
$D_{st} \times I(0.5 < \tilde{w} \leq 2, h^1 > 40)$	-0.028	-0.738	-0.013	-0.048	-2.637
	(0.034)	(1.436)	(0.039)	(0.052)	(1.765)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in (0, 35))$	0.028+	0.068	0.015	-0.012	-0.195
	(0.016)	(0.362)	(0.016)	(0.017)	(0.512)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 \in [35, 40])$	0.017	-0.211	-0.006	0.008	0.150
	(0.017)	(0.274)	(0.012)	(0.013)	(0.485)
$D_{st} \times I(2 < \tilde{w} \leq 3.5, h^1 > 40)$	-0.036+	0.392	0.004	-0.023	-0.573
	(0.020)	(0.545)	(0.019)	(0.022)	(1.107)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in (0, 35))$	-0.002	-0.299	-0.008	-0.011	-0.459
	(0.011)	(0.451)	(0.022)	(0.014)	(0.464)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 \in [35, 40])$	0.002	-0.027	0.003	0.009	0.234
	(0.016)	(0.211)	(0.008)	(0.011)	(0.472)
$D_{st} \times I(3.5 < \tilde{w} \leq 5, h^1 > 40)$	0.030	0.790	0.026+	0.007	1.102
	(0.031)	(0.605)	(0.013)	(0.030)	(1.534)
Observations	62,942	62,942	62,942	78,266	78,266
Controls	Y	Y	Y	Y	Y
Group-by-State FE	Y	Y	Y	Y	Y
Group-by-Period FE	Y	Y	Y	Y	Y

Notes: See notes for Table 2.2. $\tilde{w} = w^1 - MW^1$, i.e., the initial wage minus the initial minimum wage. h^1 refers to the working hours in the first year. *** p<0.001, ** p<0.01, * p<0.05, + p<0.1

APPENDIX C

APPENDIX TO CHAPTER 3

C.1 Data Appendix

C.1.1 CPS-ORG

We construct our analysis file for the CPS-ORG based on the NBER data files (National Bureau of Economic Research, Various Years). We restrict our sample to individuals aged 18 to 64 who report usually working 35 to 65 hours per week. We additionally exclude self-employed workers, workers without pay, and those who never worked based on *class* (until the year 1993) and *class94* (from the year 1994). Hourly versus salary paid workers is determined by *paidhre*.

As mentioned in section 3.3, we construct hourly wages in different ways for hourly-paid and salaried workers. We used the reported hourly wage for hourly workers (*earnhre*). For salary workers, we constructed hourly wages by dividing weekly earnings (*earnwke*) by usual hours of work (*uhourse*). Wages are adjusted for inflation based on the Personal Consumption Expenditures (PCE) deflator, downloaded via St. Louis Fed [1] . We dropped outlier wage observations, defined as those whose real hourly wage (1979 dollars) was smaller than 1 or larger than 100, following Cha and Weeden (2014).

As background characteristics, we classified individuals into four race categories (white, black, Hispanic, and others), five education categories (high school dropout, high school graduate or equivalent, some college education, college graduate, and advanced degree), and four region categories (Northeast, Midwest, South, and West).

To identify observations with imputed data, we rely on *I25c* and *I25d*. They are indicator variables showing which observation is imputed. But for the years 1994 and 1995, information on which observations are imputed is unavailable (see Hirsch and Schumacher, 2004). To adjust for top-coding, we multiplied top-coded observations by 1.4, following previous literature such as Cha and Weeden (2014). The thresholds for top-coding are 999 (until the year 1998), 1923 (during the years 1989-1997), and 2884 (from the year 1998) for weekly paid workers.

We augment the NBER version of the CPS-ORG with the CEPR version (Center for Economic and Policy Research, 2020). Specifically, we merge two data sets using the combination of the following variables: *hhid*, *hhnum*, *lineno*, *minsamp*, *intmonth* and *hrhhid2* as well as information on their age and state. We only lost around 1000 observations out of more than 6 million observations.

C.1.2 PSID

We only use the SEO (Survey of Economic Opportunity) and SRC (Survey Research Center) samples of the PSID, not the immigrant sample. All information about wages and hours is taken from the Labor Section of the survey. Information in the labor section is only collected for the head and wife of the household. We place no restrictions on individuals entering or leaving the sample; instead, we use all available observations.

[1] It is available at `https://fred.stlouisfed.org/series/DPCERD3A086NBEA`

C.2 Measurement Error Results

The set-up of our measurement error model follows that found in other places, including Angrist and Krueger (1999). Letting quantities with asterisks denote the error-free variables, define w to be log of the hourly wage, h to be the log of usual weekly hours, and e to be the log of weekly earnings, such that

$$e^* = w^* + h^* \tag{C.1}$$

$$h = h^* + \nu \tag{C.2}$$

$$w^* = \gamma_0 + \gamma_1 h^* + \eta \tag{C.3}$$

where ν is the measurement error in hours and η is the idiosyncratic component of log wages. The parameter γ_1 is the parameter of interest. The primary assumption we make is that $cov(\nu, \eta) = 0$. This assumption is weaker than the traditional "classical measurement error" assumption, which would additionally assume that $E[\nu] = cov(\nu, h^*) = 0$. This weaker assumption is necessary given the well-documented mean reversion observed in hours (e.g., see Bound et al., 2001).

Based on these assumption and letting $b(y|x)$ denote the regression of y on x, the key result for hourly workers is

$$\text{plim } b(w^*|h) = \frac{cov(w^*, h)}{var(h)} = \frac{cov(\gamma_1 h^* + \eta, h)}{var(h)} = \gamma_1 \frac{cov(h^*, h)}{var(h)} \tag{C.4}$$

The quantity $\frac{cov(h^*,h)}{var(h)}$ is often referred to as the reliability ratio, which is simply the regression of the error-free log-hours measure h^* on the error-ridden log-hours measure h. The key result for salary workers includes an additional term, which is often referred to as "division bias", because the error-ridden hours measure is used on the left-hand side to obtain an estimate of the hourly wage:

$$\text{plim } b(e^* - h|h) = \frac{cov(\gamma_1 h^* + \eta + h^* - h^* - \nu, h)}{var(h)} = \gamma_1 \frac{cov(h^*, h)}{var(h)} - \frac{cov(\nu, h)}{var(h)} \tag{C.5}$$

The last term is simply one less the reliability ratio.

Estimates of the reliability ratio are provided in many validation studies because of this direct connection to correcting for measurement errors in linear regression models. Based on our own analysis of the Angrist and Krueger (1999) data, we use a reliability ratio estimate of 0.93 for hourly workers and 0.71 for salary workers. Based on Table 4 of Bound et al. (2001), we present additional results in a footnote that use a value of 0.870 based on CPS data (Angrist and Krueger, 1999) and 0.6828 based on the PSID Validation Study (Bound et al., 1994)

Two other issues are worth noting regarding these adjustments. First, in all of these cases, an estimate of the reliability ratio is only available for one point in time. Thus, we apply the same reliability ratio estimate to all years of the data. Second, the estimates we report above are overstated in that we are applying them to regressions that include other covariates. With that said, results reported in Angrist and Krueger (1999) imply that the adjustment for covariates tends to have little effect on the reliability ratio for hours worked (see their Table 11).

Appendix Figure C.1 provides figures for the results of our analysis of imputations and top-coding. Panel A compares the baseline results from Figure 3.3 to those we where simply drop the imputed data. Despite the rather large fraction of imputed data–and a fraction that has strongly increased over time–excluding the imputed data has little effect on the trends. The reason for this is that hours of work are included in the imputation process, and thus the correlation between wages and hours is maintained in the imputed data.

Panel B of Appendix Figure C.1 provides an analysis of the importance of top-coding. While many fewer observations are affected by top-coding than are affected by imputations (only about 1% of workers top-coded in the early years and about 4% in the last years – see Table 3.1), top-coding varies tremendously for hourly versus salary workers (0.2% of hourly workers top-coded in any year, but upwards of 10% of salary workers are top-coded in several years). Following the practice of others using the CPS-ORG, we simply adjusted for top-coding by multiplying top-coded observations by 1.4. Such an adjustment is fairly crude, especially given the changes in top-coding over time.

To examine the potential importance of our top-coding adjustment, Panel B of Appendix Figure C.1 compares our baseline results on the long-hours premium to one that makes use of the adjustment in the CEPR data. The CEPR adjustment is based on assuming a distribution for earnings and then choosing an adjustment based on the extent that data are top-coded each year.[2] As is clear from the results, using the CEPR wage variable has little effect on our results.

C.3 Determining Overtime Status

We started with the coding that was used for the Regulatory Impact Analysis for the Department of Labor (2019) Final Rule (and its appendix) for the 2019 Overtime Law Change. The only provisions we ignored were the ones concerning certain amusement workers (only 2.8 percent of amusement workers were considered to meet the necessary qualifications) and sailors on foreign vessels (estimated to be 13,290 nationally, many of whom would likely have been captured under other exemptions such as those in sea carrier occupations). We applied these provisions back through 2005.

Specifically, we estimated the fraction of workers covered by overtime pay in the following way. We first identify workers covered by the FLSA. Although most wage and salary workers are covered, some workers such as religious workers and most federal employees (with the exception of postal workers, TVA workers, and Library of Congress workers) are not covered by the FLSA.

Second, we identify workers exempt for industry and/or occupation reasons based on information provided by Department of Labor (2019). For example, Agricultural workers, fishermen, employees of rural newspapers, and transportation workers are not subject to overtime pay regulations.

To be exempt from overtime pay, covered workers should pass three tests: salary basis test (must be paid a fixed salary level), salary level test (they should receive salary earnings higher than a certain threshold), and duties test. However, workers with named occupations such as lawyers, doctors, and teachers do not have a salary level test. For other workers, to pass the salary level test, their weekly earnings should be greater than $155 until 2004 and $455 afterward.

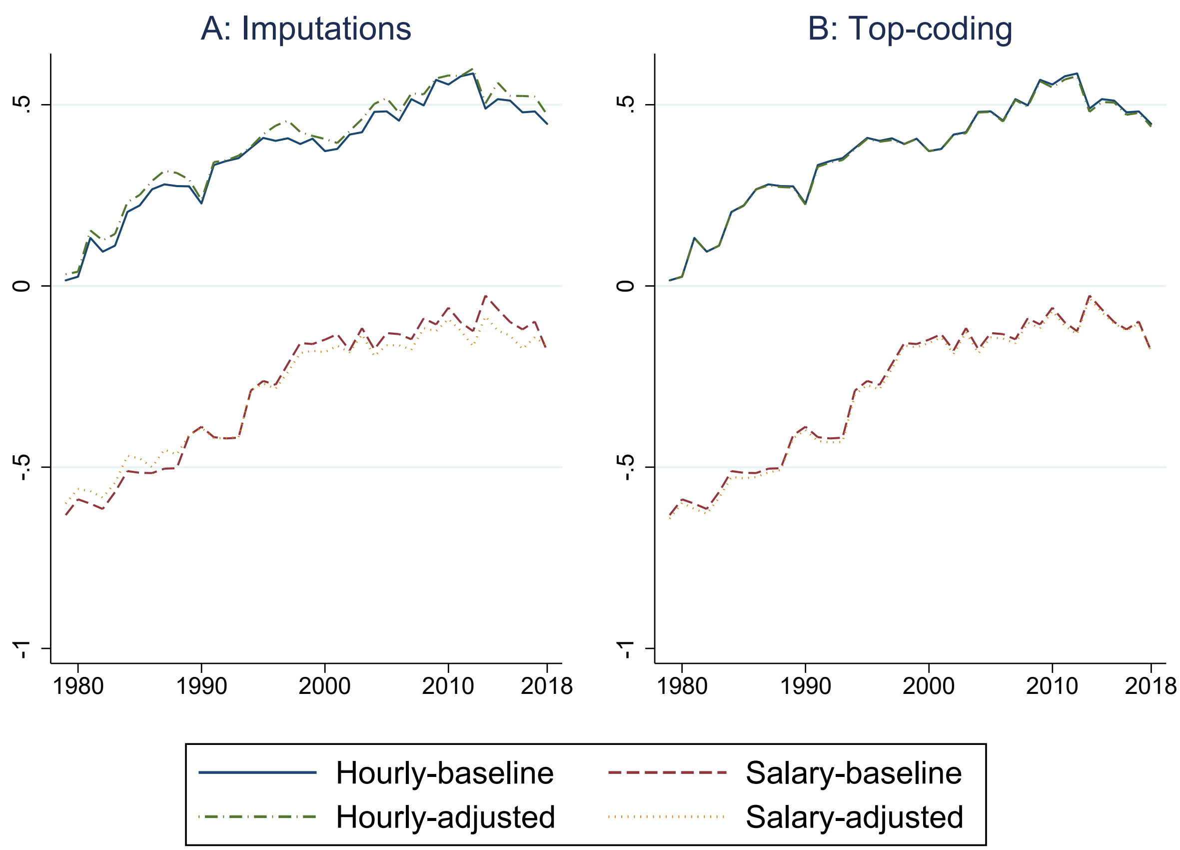
The information on whether specific workers pass the duties test is unavailable from CPS-ORG. However, Department of Labor (2004, 2019) provide occupation-level probabilities of the passing duties test. We use this information to estimate the number of workers eligible for overtime pay.

[2] Specifically, the CEPR adjustment is based on a log-normal distributional assumption. In the appendix, we show a third approach to assess whether changing top-coding affects our results. This approach is to apply the maximum amount of top-coding observed in any year (0.2% for hourly workers and 10.4% for salary workers) to every year. All basic trends that we report are the same when we instead use such consistently top-coded data; see Appendix Figure C.1.

Finally, we made more miscellaneous adjustments. From 1993, a special salary test for hourly paid workers in computer occupations is introduced. We further utilize recent state-level changes in overtime regulations.

C.4 Additional Tables and Figures

Figure C.1 The Wage-Hours Elasticity and Measurement Issues, 1979-2018



Notes: These results plot wage-hours elasticities. "Baseline" results use the same data as that from Section 3.5.1. The "Adjusted" series in Panel A adjusts for imputations. The "Adjusted" series in Panel B adjusts for top-coding. See text for details.

Table C.1 CPS-ORG Wording for Key Questions

NBER Variable	Questions
Panel A: Before Redesign	
Hourly vs. salary (paidhre)	Is ... paid by the hour on this job?
Hours worked (uhourse)	How many hours per week does ... usually work at this job?
Hourly wage for hourly worker (earnhre)	How much does ... earn per hour?
Weekly wage for hourly worker (earnwke)	[usual hours multiplied by hourly wage]
Weekly wage for salary worker (earnwke)	How much does ... usually earn per week at this job before deductions? Include overtime, tips, and commissions.
Panel B: After Redesign	
Hourly vs. salary (paidhre)	[Coded hourly if answer to (A) is hourly or if answer to (B) is yes] (A) For your (Main) job, what is the easiest way for you to report total earnings before taxes or other deductions: hourly, weekly, annually, or on some other basis? (B) (Even though you told me it is easier to report your earnings....), are you paid at an hourly rate on (this) job?*
Hours worked (uhourse)	How many hours per week do you usually work at your (main) job?
Hourly wage for hourly worker (earnhre)	[If answer to (A) is hourly or (B) is yes] (C)(Excluding overtime pay, tips, and commissions) What is your hourly rate of pay on (this) job?
Weekly wage for hourly worker (earnwke)	[(C) multiplied by (D) and then added to (E); (F) is used as a check] (D) How many hours do you usually work per week at this rate? [this refers to (C)] (E) How much do you usually received just in overtime pay, tips, or commissions, before taxes or other deductions? (F) I have estimated your total weekly earnings (,for your main job) as ... before taxes or other deductions. Does that sound correct?
Weekly wage for salary worker (earnwke)	(Including overtime pay, tips, and commissions) What are your usual (weekly/ bi-weekly/monthly/annual) earnings on (this) job, before taxes or other deductions?

Notes: * (B) is only asked if the response to (A) was not hourly.