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DISCUSSION PAPER SERIES

IZA DP No. 12137

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ABSTRACT

The Short-Run Effects of the Minimum Wage on Employment and Labor Market Participation: Evidence from an Individual-Level Panel

Neumark, Salas, and Wascher (2014) succinctly summarize the empirical challenges researchers of the minimum wage face: "the identification of minimum wage effects requires both a sufficiently sharp focus on potentially affected workers and the construction of a valid counterfactual control group for what would have happened absent increases in the minimum wage." The difficulty of addressing these two challenges is evident in the variety of empirical approaches seen in the literature. In this paper, I address the latter of the issues in a manner nearly absent in the minimum wage literature by taking advantage of individual-level longitudinal data to observe the impacts of minimum wage changes on unemployment and labor force participation. Using within-individual variation and short 4-month panels, I control for heterogeneity at the individual level that determines unemployment and labor force participation. Specifically, the empirical strategy controls any fixed individual-specific idiosyncrasies and differential exposure to time-invariant economic shocks. This differs significantly from previous literature that exploits within-state variation. The short-run impacts of the minimum wage are assessed using monthly data, instead of yearly or quarterly data, which allows for the analysis of contemporaneous minimum wage effects. There is no evidence of an increase in unemployment immediately following a minimum wage increase. In addition, it does not appear that employers are substituting full-time workers with part-time workers. That said, there is robust evidence that immediately following a minimum wage increase, labor force participation decreases.

JEL Classification: J2, J3, J6

Keywords: minimum wage, unemployment, labor force participation, individual fixed effects

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I. INTRODUCTION

Minimum wages have been studied so extensively that it is rare to find a fresh angle that piques labor economists' interest. Despite a mountain of literature, advances in empirical methods, and the availability of new data, definitive studies are scarcer than one would expect.[1] The literature looking at the employment impacts of minimum wages is so extensive that this is one of the few areas within economics that has been examined through meta-analyses (Belman and Wolfson 2014; Doucouliagos and Stanley 2009; Card and Krueger 1995).

Neumark, Salas, and Wascher (2014) succinctly state the empirical challenges minimum wage researchers face: "identification of minimum wage effects requires both a sufficiently sharp focus on potentially affected workers and the construction of a valid counterfactual control group for what would have happened absent increases in the minimum wage" (pg. 610). The difficulty of fully addressing these two challenges is evident in the variety of empirical methods used in the literature. In this paper, I present another new approach to tackling these challenges.

The contribution of this paper to the minimum wage literature can be summarized by two key features. First, this paper is the only paper to date that takes advantage of Integrated Public-Use Microdata Series' (IPUMS) recently revised Current Population Survey (CPS) individual identifiers to create a panel from which we can observe the impact of a minimum wage change *within* individuals. IPUMS' recent work on the individual identifiers allows individuals to be uniquely linked across months. The advantage of using this identifier is that it allows for the implementation of an identification strategy nearly absent in the minimum wage literature. In

[1] See Neumark and Wascher (2007) and Brown (1999) for comprehensive reviews of earlier minimum wage literature, including analysis and critique of empirical methods, and international evidence. Neumark (2018), Neumark and Wascher (2017), and Belman and Wolfson (2014) provide more recent reviews.

identifying employment effects from within-individual variation, I will be able to control for unobserved individual heterogeneity that determines unemployment and labor force participation. This strategy will also be able to control for differential exposure to economic shocks that may be correlated with minimum wage increases but are static in the short-run panel. We can think of this identification strategy as removing any confounding fixed individual-specific idiosyncrasies from minimum wage effects.

Minimum wage effects will be identified from individuals observed before and after a minimum wage change, mitigating concerns about changes in the sample composition over time from selective sample attrition or endogenous migration.

This type of panel sidesteps the need to defend a control group based on geographic proximity or observed demographics—an issue extensively debated in the panel and case study minimum wage literature.[2] Very few studies use an identification strategy in the spirit of the one used in this paper.[3]

The second key feature of this paper is that it focuses on the short-run impacts of the minimum wage using monthly data. In the CPS MORG files and other data sets that record changes across

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[2] For examples, see Clemens and Strain (2018); Clemens and Wither (2016); Allegretto, Dube, Reich, and Zipperer (2017); Sabia, Burkhauser, and Hansen (2012); Allegretto, Dube, and Reich (2011); and Dube, Lester, and Reich (2010). Neumark, Salas, and Wascher (2014) explore the use of synthetic control groups to identify control groups as an alternative to geographically proximate areas. Neumark (2018) summarizes these approaches.

[3] Two studies have used individual longitudinal data from Canada (Campolieti, Fang, and Gunderson 2005; Yuen 2003). Currie and Fallick (1996) use a similar empirical strategy, albeit with a smaller and less representative NLSY dataset. They identify individuals who are directly impacted by a change by looking at a sample of individuals initially at the minimum wage or between the old and new minimum wages. After including individual fixed effects, they find that affected individuals are less likely to be employed one year later relative to higher wage "unaffected" individuals. They are only able to observe employment to non-employment transitions. Neumark, Schweitzer, and Wascher (2004) manually match individuals in the CPS but can only match individuals in two samples, and many individuals cannot be matched. In a working paper, Clemens and Wither (2016) estimate regressions including individual fixed effects using one year of Survey of Income and Program Participation data during the Great Recession in 2008.

years or quarters, there is concern that unobserved spatial and temporal economic trends influence individual labor market outcomes and confound the impact of a minimum wage increase. Furthermore, analyzing changes from year to year obscures the timing of individual responses. In contrast, the CPS basic files allow researchers to narrow the observation window by tracking month-by-month changes in employment and labor force participation. Specifically, the 4 months on, 8 months off, and 4 months on sample design allows researchers to look at shorter windows around a minimum wage change and, in doing so, observe employment statuses immediately before and after a change. Focusing on a narrow temporal window in which larger economic phenomenon are relatively constant will aid in the identification of minimum wage effects, but I also include finer geographic and temporal controls as robustness checks.[4]

The sample period from 1990–2017 covers hundreds of effective state minimum wage changes experienced by hundreds of thousands of unique individuals. The size of the CPS samples, the substantial variability in the magnitude of minimum wage changes, and the number of changes in minimum wages over this period reinforce the generalizability of the results. These features of the data mean I have the necessary power to be able to estimate minimum wage impacts on smaller demographic subsamples likely to be impacted by the minimum wage.

One minor disadvantage of using the CPS basic survey is that I will not be able to show how minimum wage changes shift or compress the distribution of hourly wages at the minimum wage threshold. While standard practice in many studies, the purpose of analyzing the distribution is to identify the "treated" population—individuals whose hourly wage is raised by a minimum wage

[4] The characteristics of the CPS design and my empirical strategy prevent me from addressing longer-run impacts. For studies that address the longer-run impacts, see Clemens and Strain (2017); Allegretto, Dube, Reich, and Zipperer (2017); Allegretto, Dube, and Reich (2011); Dube, Lester, and Reich (2010); Burkhauser et al. (2000); and Baker, Benjamin, and Stanger (1999).

increase, thus motivating a subsequent employment analysis. I will defer to voluminous evidence in previous literature to inform my choice of sample population. Specifically, I will analyze populations determined relevant by previous literature—teens, youths, and the less educated.[5] The use of these populations is motivated by important questions regarding a minimum wage's ability to reduce poverty, the existence and extent of labor–labor substitution, and the general welfare of low-skilled workers. The labor market outcomes investigated in this paper are unemployment status, part-time vs. full-time status, and labor force participation.

This paper's primary results can be summed up in three conclusions. First, there is almost no evidence of a rise in unemployment immediately following a minimum wage increase. Second, it does not appear that employers are substituting full-time workers for part-time workers. Third, there is robust evidence that immediately following a minimum wage increase, there are fewer individuals in the labor force. I estimate statistically significant negative coefficients with magnitudes of more than .010 labor force participation percentage points in response to a 10% increase in the minimum wage for individuals ages 20 to 24. This translates to an elasticity of approximately -.14. I also find significant decreases in labor force participation for foreign-born individuals of between .006 and .012 labor force percentage points (elasticities of -.10 and -.16, respectively), depending on whether they have less than a high school degree or have exactly a high school degree.

The results suggest that the lack of an increase in unemployment among individuals in the labor force can be explained by individuals exiting the labor market. In the short run, the relevant labor market margin to study is labor force participation.

[5] While a large literature focuses on teens, I follow Belman et al. (2015) in looking at other demographic groups to emphasize the scope of the minimum wage effect across the labor market.

A standard static labor market model could explain these results if we consider the possibility that the excess supply of labor exits the labor market completely, leaving the equilibrium level of employment unchanged. The excess supply of labor generated by a higher minimum wage is composed of a mix of both new entrants and laid-off workers. A net decrease in labor force participation indicates that on average, recently laid-off individuals exiting the labor market outnumber individuals entering the labor force due to the minimum wage being raised to a level above their individual reservation wage. Alternatively, in the presence of monopsonistic or oligopsonistic labor markets, minimum wages do not have the same employment-decreasing impacts. Recent work has found evidence of employer collusion in the low-wage fast-food industry (Krueger and Ashenfelter 2018).

In the next section (II), I detail the variation in state minimum wages. In section III, I discuss the identification strategy and empirical methodology used in this study. In section IV, I present minimum wage effects on unemployment status, part-time status, and labor force participation. In section V, I discuss the empirical results by offering explanations grounded in the existing literature. In section VI, I offer concluding remarks.

II. DATA SOURCES AND SUMMARY OF MINIMUM WAGE VARIATION

The sample period spans 1990–2017 and covers all 50 U.S. states plus the District of Columbia. Monthly minimum wages by state for 1990–2011 are taken from Jeffrey Clemens' research webpage (http://econweb.ucsd.edu/~j1clemens/personalHome.html). Monthly minimum wages from 2012–2017 are gathered from state labor agencies and government websites. Combining both state and federal changes, a total of 516 effective minimum wage changes occurred during the sample period.

On average, approximately 18 state changes occur per year, and 10 changes per state over the sample period. Figure 1 shows the number of minimum wage increases by year. The number of effective state changes varies dramatically year-to-year. The spikes in the total number of changes coincide with binding increases in the federal minimum wage that impact many states simultaneously. For example, 1997's total contains the sum of binding federal increases, increases in states that were already beyond the new federal floor, and the multiple increases in the states of CA, DE, and VT.[6]

Appendix Table A shows the number of changes by state and the average size of the increase. The median number of increases in a state is 9, and 32 states experienced more than 7 increases. The largest increase in the minimum wage is $1.95, but the average increase is $0.48. Of the 516 total increases, 60 were of $0.75 or greater. A notable number of increases are the result of state laws that index the state minimum wage rate to inflation. In general, these increases are smaller: 57 of the increases were of $0.15 or less.[7]

Figure 2 shows the distribution of minimum wage changes by month. Most changes are effective on January 1, but a sizeable number of increases occur later in the year. Using monthly data directly links the month of a minimum wage change with a contemporaneous labor market outcome. The potentially confounding impact of this strong pattern is addressed by the inclusion of month fixed effects in my empirical specifications.

[6] The federal minimum wage increased on April 1, 1990, April 1, 1991, October 1, 1996, September 1, 1997, July 24, 2007, July 24, 2008, and July 24, 2009. Over the sample period, CA, DE, MD, MO, MT, NM, PE, and VT all experienced more than one increase in a single year at some point.

[7] In the two states where the state minimum wage follows a range of values, the lower end of the range is assumed (unless bounded by the federal minimum wage). These states are MN and NV. In some states, a higher state wage only applies to businesses that hire 2+ or 4+ employees. In these states, I assume the higher state rate. These states are IL, MI, NE, and VT. In two other states, MT and OH, firms must earn above a certain threshold for the higher state rate to apply ($110,000 for MT and $288,000 for OH). I assume the higher state rate in both states.

III. IDENTIFICATION AND EMPIRICAL METHODOLOGY

i. The Current Population Survey panel

The monthly Current Population Basic Surveys from 1990 through 2017 are downloaded from the Minnesota Population Center's Integrated Public Use Microdata Series website. A key feature of the basic surveys is that they report individual employment and labor force status in each month. The data set has 28,896,402 observations collected from a sample of 5,603,098 unique individuals ages 16–64. Of these, 702,653 unique individuals experience a minimum wage change.

Many studies use the CPS MORG files, as they contain detailed information on hourly wages, but as noted by Hoffman (2016), official Bureau of Labor Statistics labor market tabulations of employment and labor force participation are calculated from the full basic monthly survey. While the MORG files are a random selection from a nationally representative sample, differences between MORG and basic CPS files may arise if MORG files are used to analyze subgroups with small sample sizes in a state and year.[8] Addison, Blackburn, and Cotti (2013) suggest using MORG files to examine hourly wages, but the full CPS to examine employment.[9] The literature's focus on MORG samples and issues with the accuracy of CPS identifiers has meant that the longitudinal features of the CPS have been generally ignored. According to Drew, Flood, and Warren (2014),

> *Despite this longitudinal design, researchers have almost exclusively analyzed the CPS data as though it were a cross-sectional survey. There are several reasons for this: CPS*

[8] Hoffman (2016) compares full CPS monthly samples to MORG samples, showing that the two samples display noteworthy differences in summary labor market statistics.
[9] Allegretto, Dube, Reich, and Zipperer (2017) use the full CPS basic monthly files to study teen employment and the MORG files to study wages.

records are technically difficult to link across survey (especially for older files); the CPS's complex sample design complicates longitudinal analysis; identifying sequences of files containing variable relevant to a research problem can be laborious; the integration of variables over time is challenging; and data access is awkward, requiring manipulation of many different files. (pg. 122)

Drew, Flood, and Warren (2014) create a unique identifier for each individual so researchers can follow a person across monthly samples.[10] With identifiers, I can ensure the panel only contains individuals who are observed on both sides of a minimum wage change threshold, assuaging compositional concerns of selective sample attrition. The CPS basic files sample individuals up to 8 times, following a 4-8-4 cycle (surveyed for 4 consecutive months, not surveyed for 8 months, surveyed again for 4 consecutive months). I use the longitudinal aspect of the CPS to study the impact of a minimum wage change using within-individual exposure to a minimum wage increase. This identification strategy leverages month-to-month changes in labor force participation and employment to understand the short-run impact of minimum wage increases. A big advantage of this research design is that I do not need to identify alternative control groups.[11] Furthermore, I can analyze the impact of a minimum wage change in the months immediately before and after a minimum wage change—something which is not possible in studies that use changes in employment across quarters or years.

Figure 3 is a visual representation of the individuals who generate identifying variation. Since individuals are surveyed following a 4-8-4 cycle, an individual who experiences both pre and post minimum wage change periods looks like one of the three examples shown in the figure.

[10] Flood and Pacas (2016) discuss linking monthly files with the supplemental surveys.
[11] Neumark and Wascher (2007) thoroughly detail the difficulties associated with finding appropriate control groups in the minimum wage literature.

The number months before or after a minimum wage change is on the horizontal axis, with "month 0" representing the month of a minimum wage change. A change is represented by a vertical dashed red line. The 3 months preceding and following a minimum wage change are labeled {-3, -2, -1, 0, 1, 2}. Each horizontal blue line represents one of the three types of individuals who experience a minimum wage change at some point during a consecutive 4-month survey period. All three types of individuals are observed at least 1 month immediately prior to and after a minimum wage change.[12]

The panel is unbalanced. Within the first 4-month rotation period, 69.5% of the individuals are observed in four consecutive months, and 10.7% are only observed for one month in a 4-month block. These individuals will not be included in the individual fixed-effects regressions.

Many individuals experience a minimum wage change in the first 4-month rotation period but not the second (and vice versa). To avoid including their second 4-month rotation in their post period (or avoid treating the first 4-month period as a pre period in the opposite scenario), the observations in the second 4-month rotation period are ignored. This creates a more balanced panel, avoids double counting a single individual if they experience more than one minimum wage change, and prevents confusing an individual's treatment period with a control period.

Most of the overall minimum wage variation is between individuals (as opposed to within individuals), so a large sample size is necessary for implementing the individual fixed-effects estimator. Appendix Table B provides summary statistics on the final subsamples to be used, including the number of unique individuals in each subsample.

[12] States that had at least two changes in close temporal proximity are AK, CA, DE, FL, IA, MD, MO, MT, NH, NM, OR, PA, RI, and VT. Only 1 state had two minimum wage changes with overlapping 4-month windows. I edited this to correct for overlap.

ii. Methodology

Based on a total of 516 minimum wage changes, I observe hundreds of thousands of individuals before and after a minimum wage increase. Selection into or out of employment or labor force participation at the time of a minimum wage increase may be driven by individual and market-level factors. My empirical strategy allows for the differencing out of temporally-invariant unobserved heterogeneity unique to an individual that could bias the estimated impact of a minimum wage change. Labor market factors that are constant within 4-month windows will be controlled for, but I will also include month fixed effects to control for trends within a year. For larger macroeconomic factors to confound the results, they need to vary within an individual's 4-month window and coincide with minimum wage changes while at the same time be uncorrelated with monthly trends.[13]

To address any remaining concerns of heterogeneity in labor market trends not captured in the baseline specifications, I estimate a specification including state-specific linear month trends and a saturated specification with state–month period effects (the most flexible of specifications). While there is discussion in the literature on the use of these finer controls, I stay agnostic, comparing specifications with and without these controls.[14]

The baseline specification includes only individual fixed-effects

[13] Year fixed effects are not included in specifications with individual fixed effects due to their high collinearity with minimum wage changes. To illustrate this using Figure 3, all minimum wage change implemented on January 1st (the red line in Figure 3) will be absorbed by the year dummy. Including year dummies would only leave variation from changes in the minimum wage that occur at other times of the year.

[14] Allegretto, Dube, and Reich (2011) suggest the use of state-specific linear trends or regional-specific time dummies to correct for bias generated from unobserved heterogeneity in geographic employment trends. Neumark, Salas, and Wascher (2014) challenge the sensitivity of their models by including different types of trends. Dube, Lester, and Reich (2016); Allegretto, Dube, Reich, and Zipperer (2017); and Neumark and Wascher (2017) continue this debate. In this paper, this debate is moot, since results do not vary dramatically between specifications.

$$y_{ismt} = \alpha + \beta MW_{smt} + \varphi_i + \varepsilon_{ismt} \quad (1)$$

where y_{ismt} is a dichotomous employment or labor force outcome for individual i, in state s, and during month m and year t. The variable of interest, MW_{smt}, is the natural log of the effective minimum wage. φ_i represents indicators for each individual.[15] All observations are weighted using CPS individual weights. Individual employment outcomes within a state are correlated, so standard errors are clustered at the level of the minimum wage variation—the state. Clustering is necessary since clusters of individuals by state, rather than individuals themselves, are assigned the "treatment" of a minimum wage increase.

In Figure 2, we observe a disproportionate number of minimum wage changes in the month of January. The seasonally unadjusted unemployment rate is also consistently highest in January. Month fixed effects are included to nonparametrically control for national month-to-month trends in employment. I therefore add τ_m to equation (1), as shown in (2)

$$y_{ismt} = \alpha + \beta MW_{smt} + \varphi_i + \tau_m + \varepsilon_{ismt} \quad (2)$$

Appendix Table A shows that minimum wage changes are not randomly distributed across states. Because some states experience many minimum wage increases while others experience few, a potential concern is that these states may have fundamentally different labor markets and that these differences follow trends that coincide with the state minimum wage.[16] States that change

[15] Controls for gender, race, Hispanic origin, and, to a slightly lesser extent, age and marital status will be absorbed as part of an individual fixed-effect. Studies focusing on the longer run have also added a variety of controls to account for larger macroeconomic trends. Some of these are the employment-to-population ratio, private sector employment, the teen share of the population in the state, non-seasonally adjusted state unemployment rate, or average adult wages into the specification. Neumark and Wascher (2007) compare and discuss the use of these types of controls.

[16] This is one of the motivators behind Allegretto, Dube, and Reich's (2010) use of census region dummies and trends.

their minimum wage often create more variation and are overrepresented in the data. In this sample, state fixed effects are absorbed by the individual fixed effects, but I can add state-specific linear trends, as shown in equation 3:

$$y_{ismt} = \alpha + \beta MW_{smt} + \varphi_i + \tau_m + \delta_s \cdot m + \varepsilon_{ismt} \qquad (3)$$

Given the very short time span, the inclusion of state-linear trends may be sufficient to control for confounding economic trends (Addison, Blackburn, and Cotti 2013), but state-by-month dummies can also be added in support of the previous specification.[17] A month dummy for every state allows for the most flexible controls of unobservable seasonality and state-specific heterogeneity in yearly economic trends, but they will also absorb variation from minimum wage changes that occur on a fixed schedule. These are minimum wage changes that occur at the same time of the year, every year, like smaller minimum wage adjustments that track inflation.

Minimum wages have often increased in years when unemployment rates are at their highest.[18] This is seen in Figure 1 for the recession of the early 1990s and the Great Recession. I do not include year fixed effects in any specification since they are collinear with minimum wage changes that occur between December and January in the individual fixed effects specifications. Doing so would only leave variation from changes in the minimum wage that occur at other times of the year.

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[17] Meer and West (2016) show that estimates of fixed effects regression including state-specific time trends attenuate estimates of the minimum wage effect on employment levels. The results in this paper show that the addition of these trends does not seriously alter any of this paper's conclusions and in most cases have little impact on the estimates' magnitude or statistical significance—likely due to the short length of the individual panels.

[18] Addison, Blackburn, and Cotti (2013) specifically use a sample period in which they can assess the differential impact of increasing minimum wages during a recessionary period.

The identifying assumption for these specifications is that, after conditioning on the individual and state-specific temporal trends, minimum wage changes are uncorrelated with differences in remaining unobserved labor market characteristics.

These models differ from the canonical model with state and year fixed effects in a few key ways. The individual fixed effects model uses a different source of identifying variation, the panels are short in length, and I control for average trends in unemployment within a year by including finer month dummies.

iii. The target population

Previous studies have found that correctly identifying the treatment group for whom the minimum wage is binding is crucial for determining the presence of adverse employment effects (Sabia, Burkhauser, and Hansen 2012; Neumark and Wascher 2007; Neumark and Wascher 2002). Teens are a subset of minimum wage workers and the focus of a large portion of the minimum wage literature, but a more comprehensive look at the impact of minimum wages on employment requires expanding the analysis to include other populations of interest. For example, while a large percentage of teens are earning the minimum wage, teenagers comprise only 3% of all employment and only 19% of those earning less than 1.1 times the minimum wage (Belman and Wolfson 2014). I focus on various populations of interest in the literature shown to be affected by minimum wage increases, as determined by changes in their hourly wage distribution.

I use three samples classified by age: individuals ages 16–19 (the teen sample), individuals ages 20–24 , and individuals ages 25–29.[19] Other subsamples include individuals ages 23–29 with

[19] Samples restricted to individuals over 30 years old are also estimated. These results are briefly discussed in footnotes.

exactly a high school degree and with less than a high school degree, and individuals who were born outside of the United States with exactly a high school degree and with less than a high school degree.

The analysis of these samples is further motivated by the relevance of minimum wage policy to poverty reduction, the existence and extent of labor–labor substitution, and the general welfare of low-skilled workers.

IV. RESULTS

In this section, I present estimates for individual unemployment, part-time status, and labor force participation based on equations (1–3) for each sample.[20] I begin with the results for unemployment. An unemployed individual is determined using the CPS definition. Individuals not in the labor force are excluded.

i. Unemployment impacts by age

In the spirit of the "canonical model" referenced in the literature, Tables 1 and 2 display estimates from a specification that only includes state and month fixed effects. The entries in Table 1 are the β estimates (i.e., the minimum wage effects) stratified by age group. This model is based on previous literature that uses within-state variation in minimum wages to identify employment effects. The estimates represent a weighted average of many before and after within-state comparisons, after differencing out a common national-level month time trend.

From Table 1 we see the minimum wage coefficient is positive and statistically different from 0 at the 1% level for all age groupings. Not surprisingly, the impact is most pronounced for the

[20] Estimates based on (3) but replacing the linear trends with state-by-month indicators are mentioned but not reported due to their strong similarity to estimates from (3).

youngest age group and diminishes with age. A 10% increase in the minimum wage is associated with a .004 point increase in the probability of unemployment for teens. With a (weighted) mean unemployment level of .1778, this implies a percentage increase of .25% in unemployment. This estimate falls within the typical range of estimates given similar empirical strategies (Belman and Wolfson 2014; Neumark and Wascher 2007). The issues with this type of specification are well-documented in the literature.[21] Most studies differ in that they do not have the capacity to include month fixed effects. I add them instead of year fixed effects in order to compare these results with my individual fixed effects results later.

Table 2 displays the results from a specification based on the canonical model with only state and time fixed effects but stratifying the sample by immigrant status and education. Save for a small and statistically insignificant coefficient for immigrants with less than a high school degree, all subsamples display positive statistically significant coefficients. Individuals ages 23–29 with exactly a high school degree show the largest increase in unemployment following a minimum wage change.

Turning to the individual fixed effects specifications, Table 3 reports the coefficients based on equations (1), (2), and (3) in Panels A, B, and C, respectively. All the coefficients are now *negative* and statistically significant. There is consistent evidence that minimum wage effects are greatest for the youngest age group in all panels. The coefficients decrease in absolute value with age.[22] A 10% increase in the minimum wage is associated with a .009 percentage point *decrease*

[21] A recent examination of the two-way fixed-effects specification by Goodman-Bacon (2018) highlights some of these concerns. Other sources of bias include bias from selective sample attrition and selective cross-state migration.
[22] Reinforcing this pattern, restricting the sample to only individuals older than 30 years old generates the smallest magnitudes in all specifications.

in the probability of being unemployed (Panel A) and .014 points in the second specification (Panel B).[23]

The regression estimates the average of all within-individual comparisons. Individuals can only be treated once. There does not appear to be evidence of increased unemployment in the months immediately following an increase in the minimum wage. Non-negative employment impacts are documented in the literature (Belman and Wolfson 2014; Neumark and Wascher 2007). More recently, Clemens and Strain (2018) suggest that the size of the minimum wage increase plays a role in the sign of the effect. Specifically, smaller changes in the minimum wage tend to have positive employment impacts, while larger changes tend to have negative impacts.

Panel C in Table 3 reports the results from equation (3), which includes controls for state-specific linear trends in unobserved factors that may be both correlated with minimum wages and unemployment.[24] The addition of state-month linear trends does not change the conclusions from Panel B. These results confirm that using within-individual comparisons coupled with a short time frame minimizes the role of confounding state trends. We do not see evidence of an unemployment effect immediately following an increase in the minimum wage in Table 3.

The differences in the results between the state fixed effects and individual fixed effects models stem from the sources of variation in each model, the time periods covered, and the remaining unobserved factors left in the error term. In the model with state fixed effects, the within variation is calculated using the difference between a state's minimum wage and its mean wage

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[23] The sample sizes differ for the two reasons. The first is that in the individual fixed effects regressions, individuals observed once must be excluded. The second is that only individuals who are observed on both sides of a minimum wage change can create variation that is identified separately from an individual fixed effect.

[24] The results from the saturated model with state-by-month indicators is estimated, but the results are not reported because the estimates are very similar to those in Panels B and C. The 16–19-year-old sample estimate is larger by .0021 compared to the specification with state-linear trends. For the 20–24-year-old sample, the estimate using linear trends is larger by .0034. For the older group, the estimates differ by less than .001.

in each time period. While federal changes in the minimum wage are arguably more exogenous to a state's unobservable determinants of unemployment, the frequency and magnitude of state legislated changes are likely correlated with a host of idiosyncratic unobserved factors that are changing over time. Some of these factors include demographic changes (including migration), shifts in industry composition, changes in a state's political leanings, and other statewide economic policies. Over longer periods of time, these factors cannot be controlled for using state fixed effects, even with the addition of linear trends. Within-state variation in the minimum wage is still correlated with within-state variation from these unobservable factors.

In the individual fixed effects model, the within variation is calculated using an individual's deviation from their individual mean in each time period. Coupled with a shorter 4-month time period, most of the time-varying confounders listed above will not vary for an individual. That said, other statewide policies may be enacted contemporaneously with a minimum wage change. Month fixed effects are included, since it is still possible that other employment-related policy changes tend to be implemented at the same time of the year as a minimum wage increase.

ii. Unemployment impacts by education level

Table 4 displays the results from specifications (1), (2), and (3) using samples of individuals ages 23–29 and immigrants. All specifications include individual fixed effects. Separate regressions are run for individuals ages 23–29 with less than a high degree, individuals ages 23–29 with exactly a high school degree, individuals who were born outside of the United States with less than a high school degree, and foreign-born individuals with exactly a high school degree.

In contrast to Table 2, the coefficients are negative, and some are statistically significant. The coefficients are larger in magnitude for individuals who did not earn a high school diploma. The inclusion of month indicators (Panel B) increases the magnitude of the impact, and two estimates become statistically significant. In all three specifications, immigrants without a high school degree display larger impacts in absolute value compared to all 23–29-years-olds without a high school degree. Interestingly, the relatively more educated individuals experience smaller absolute value decreases in unemployment.

The results from Panel B are robust to the inclusion of state-specific linear month trends in Panel C. Like Table 3, there is no evidence that an increase in the minimum wage leads to an increase in unemployment. Almost all the coefficients are negative, and some are statistically significant at the 5% level in Panels B and C. For immigrants without a high school degree, the point estimates are -.0844 and -.0688, respectively.[25]

iii. Part-time status by age

It is possible that the previous results mask unemployment effects because employers are reducing worker hours while keeping them employed to avoid paying for benefits enjoyed by full-time workers, thus reducing labor costs. To investigate this possibility, I estimate whether part-time employment increases following a minimum wage increase. While work hours may fluctuate week to week, movement from full-time to part-time status is a reliable indicator that work hours are decreasing.

Table 5 is identical to Table 3, but now the outcome variable is an indicator equal to 1 if an individual reported they usually work full-time hours but are working part-time because of economic reasons. The indicator is 0 if the individual works a full-time schedule (defined as 35+

[25] Estimates from an individual fixed effects logit model with indicators for month report minimum wage coefficients that are negative in all subsamples.

hours). All other statuses are left out of this sample, including individuals who work part-time for non-economic reasons.[26]

In Panel A, the estimated impacts are smaller compared to Table 3. While some estimates are positive for the 20–24-year-old group, they are still not statistically distinguishable from 0. There is once again evidence that magnitudes are largest for the 16–19-year-old sample and that the impact decreases in absolute value with the older samples. The addition of month indicators does little to change the results. In Panel C, the decrease in part-time employment for teens is larger and now significant. There is scant evidence supporting the conclusion that part-time employment increases immediately following a minimum wage increase.[27]

iv. Part-time status by education level

Table 6 displays the results for part-time employment from specifications (1), (2), and (3) using the same samples of individuals ages 23–29 and immigrants used in Table 4.

Overall, the results in Table 6 are consistent in sign and magnitude with the results by age in Table 5. While generally not significant, in the case of immigrants with a high school diploma, we see some evidence that they are being moved from full- to part-time status. Looking at Panel A, a 10% increase in the minimum wage is associated with .005 percentage point increase in the

[26] The "hours of work" variables in the CPS are not appropriate for this analysis. The "usual hours worked in a week" variable uses language that solicits a typical workweek. If an individual was recently laid off, their answer may reflect the entire year's employment or simply some longer span of time. The "total number of hours worked last week" variable is unreliable because decreases could be a consequence of various non-employer related factors like employee sickness or vacation. Hours of work could also decrease more in jobs with greater exposure to economic shocks.

[27] Unreported estimates from specification (3) with state–month indicators instead of linear trends are very similar to those in Table 5, Panel C. When restricting the sample to only individuals over the age of 30, the coefficients turn positive, providing some evidence of increased part-time work. That said, none of the coefficients are significant in any of the specifications, and the coefficients are an order of magnitude smaller than coefficients for the 16–19-year-old group.

probability of working part-time relative to full-time for this group. However, the coefficients are negative in almost all the other estimates.

v. Labor force participation by age

The lack of unemployment effects and movement in part-time status could also be explained by workers exiting the labor force altogether. In other words, perhaps newly unemployed workers or workers facing hour reductions transition directly out of the labor force after a minimum wage increase. These individuals may receive more utility from not working and engaging in other activities compared to working part-time or searching for a new job. To follow the unemployment and part-time status analyses, I now look at how labor force participation changes on either side of a minimum wage increase. Table 7 displays the results of specifications (1), (2), and (3) where the outcome variable is a dichotomous variable equal to 1 if the individual is in the labor force and 0 if not.

In Panel A of Table 7, we observe significant drops in labor force participation immediately following an increase in minimum wage. A 10% increase in the minimum wage is associated with about a .0157 percentage point decrease in the probability of labor force participation for teens. This association disappears with the inclusion of month dummies in Panel B. In contrast, the decrease in labor force participation for the 20–24-year-old group is robust to the inclusion of additional controls.

We observe statistically significant decreases in labor force participation for the 25–29-year-old group, but magnitudes are smaller compared to the 20–24-year-old group. The most consistent evidence of a decrease in labor force participation is in the 20–24-year-old group. A 10% increase in the minimum wage is associated with about a .0102 decrease in labor force participation percentage points. From a baseline labor force participation rate of .7554, this translates to an elasticity of approximately -.135.[28]

vi. Labor force participation by education level

Table 8 displays the results for labor force participation from specifications (1), (2), and (3) using the same samples of individuals ages 23–29 and immigrants used in Tables 4 and 6.

As seen in Table 7, there is consistent and robust evidence of decreases in labor force participation immediately after a minimum wage increase. For the 23–29-year-old group, the magnitudes are in a similar range. There is little evidence of a difference in response for individuals ages 23–29 without a high school diploma and those with exactly a high school diploma.

Foreign-born individuals do see differences by education level. The coefficients are almost twice as large for foreign-born individuals with exactly a high school diploma compared to those with less education. There are significant decreases in labor force participation for foreign-born individuals of between .006 and .012 (elasticities of .10 and .16, respectively), depending on whether they have less than a high school degree or have exactly a high school degree.

Together with Table 7, Table 8 displays consistent evidence of a minimum wage effect. These results offer one possible explanation for why we do not observe increases in unemployment or part-time work.

[28] Restricting the sample to individuals ages 30 and older, the decreases in labor force participation are larger in magnitude than those for the 25–29-year-old group, but smaller than the coefficients for 20–24-year old group. Estimates in all specifications are significant at the 1% level.

V. DISCUSSION

The results of this paper can be summarized into three conclusions. First, I find nearly no evidence of increases in unemployment immediately following a minimum wage increase once I control for individual fixed effects. Using a difference-in-differences approach and a similarly aged sample, Clemens and Strain (2018) show that the impact on employment can be positive and significant after focusing on states that change their minimum wage by less than $1 and on states that index their minimum wage increases.[29] Given only 17 of the 516 minimum wage changes in my sample period are over $1 and the average change is $0.48, the results in this paper could reflect the same phenomenon. Alternatively, Meer and West (2016) provide evidence that minimum wage impacts may not be observed immediately after a change but may manifest over longer periods of time through reduced employment growth.

Second, it does not appear that employers are substituting full-time workers for part-time workers. Third, there is robust and consistent evidence that immediately following a minimum wage increase, individuals are less likely to be in the labor force. The impacts are largest for the 20–24-year-old age group and foreign-born individuals with exactly a high school diploma.

Much of the discussion in the literature focuses on the unemployment impacts of the minimum wage, but the results suggest that the appropriate labor market margin to focus on is labor force participation. While we cannot know precisely why an individual leaves the labor market, there is robust evidence that decreases in labor force participation occur with increases in the minimum wage after most individual-level and market-level factors are controlled for. This

[29] The authors use ACS data from 2013–2015 but note that results hold when they use the CPS instead.

conclusion is further supported if we assume that individual-level and local labor market factors do not change significantly over the 4-month period in which we observe individuals.

An explanation based on a standard static labor market model fits if we consider the possibility that the excess supply of labor exits the labor market completely, leaving the equilibrium level of employment unchanged. Since the excess supply of labor generated by a higher minimum wage is composed of a mix of both new entrants and laid-off workers, a net decrease in labor force participation indicates that, on average, recently laid-off individuals exiting the labor force outnumber individuals entering the labor force resulting from the minimum wage being raised to a level above their individual reservation wage.

We see evidence that labor force participation decreases for most groups, yet more for some groups than for others. There is no evidence to conclude on the presence of labor–labor substitution, but this could be a subject of future research.

A resurgent explanation for the lack of unemployment effects is based on the presence of monopsonic competition in the market for lower-skilled labor. Krueger and Ashenfelter (2018) explore the possibility of oligopsonistic competition in the labor market by analyzing a database of franchise contracts. "No-poaching" clauses in franchise contracts are common in high-turnover and lower-paid industries—precisely the ones most likely to be impacted by a minimum wage increase. No-poaching agreements prohibit franchises from hiring employees from affiliated companies and severely reduce competition for labor amongst employers. As seen in the case of monopsony, under oligopsony, an increase in the minimum wage can increase employment.

VI. CONCLUSION

In order to apply the empirical strategy of using individual fixed effects, I restrict each individual's sample frame to a narrow 4-month window. I use a data set that spans decades, and the minimum wage estimates in this study are averaged from thousands of before and after comparisons. The empirical results presented leave little ambiguity about the short-term impacts of increasing the minimum wage. The lack of significant or noteworthy negative employment effects should not be too surprising given a literature that finds both evidence for and against disemployment effects. To cite Neumark and Wascher (2007), "Based on our review of the literature . . . the majority of the U.S. studies that found zero or positive effects of the minimum wage on low-skill employment were either short panel data studies or case studies of the effects of a state-specific change in the minimum wage on a particular industry." The changes in labor force participation immediately following a minimum wage increase are robust and suggest an interesting avenue for future work.

Although it would be convenient to be able to sum up the unemployment, part-time status, and labor force participation status results in a single story, even a superficial reading of the minimum wage literature will quickly raise doubts about this possibility. Future work will use this empirical framework to estimate the effects of minimum wage changes on households instead of individuals. Yet there are other questions that cannot be addressed using this paper's empirical framework; for example, are changes in labor market outcomes occurring outside of the 4-month sample frame? To what extent do employers anticipate increases in the minimum wage? Are changes too gradual to be detected empirically? These questions boil down to understanding employers' ability to adapt to changes in labor costs and their decision to reoptimize in the face of higher labor costs.

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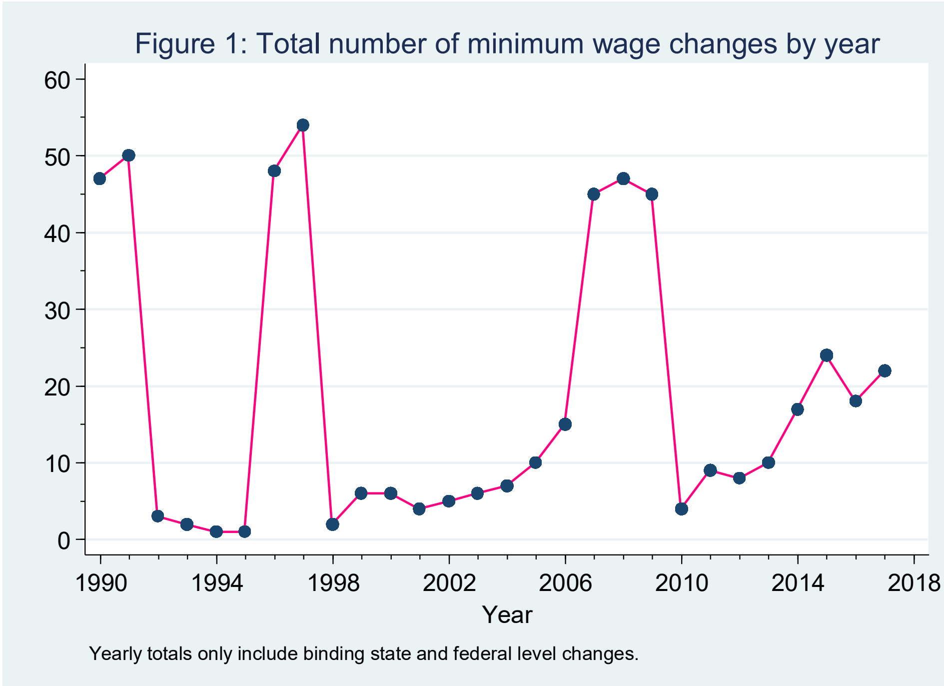
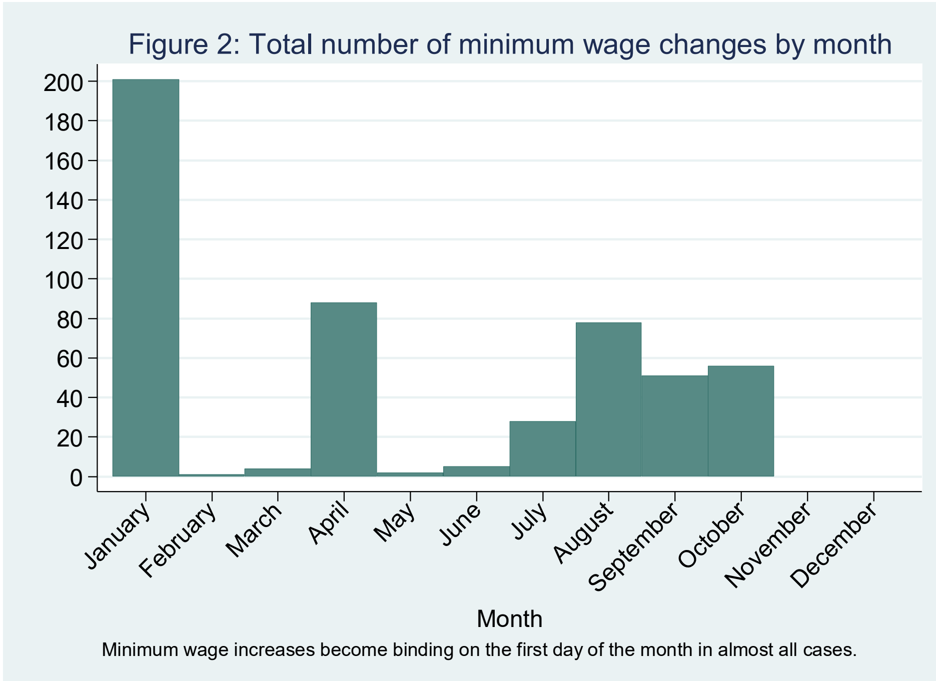
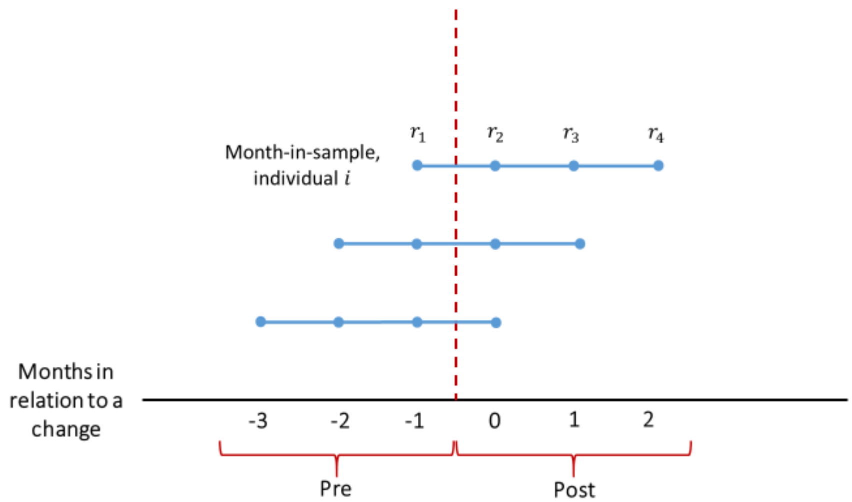


Note: Totals are effective changes, meaning changes that bind, as opposed to statutory changes. The number of changes is very large in years when the federal rate increases. For example, 1997's total number contains binding federal increases, increases in states already that were already above the new federal floor, and multiple increases in some states like CA, DE, and VT. CA, DE, MD, MO, MT, NM, PE, and VT all experience more than one increase in a single year at some point over the sample period. The federal minimum wage increased on April 1st, 1990, April 1st, 1991, October 1st, 1996, September 1st, 1997, July 24th, 2007, July 24th, 2008, and July 24th, 2009.



Note: Totals are effective changes. Most minimum wage increases occur on the first on the month.

Figure 3: Graphical representation of the identifying variation



Note: Figure 3 shows the three types of individuals that generate identifying minimum wage variation. The horizontal axis is the number of months before or after a minimum wage change, with "0" representing the month of a minimum wage change. The red vertical segmented line is drawn to align with the first day of month 0. The 3 months preceding and 3 months following a minimum wage change are labeled {-3, -2, -1, 0, 1, 2}. Each horizontal blue line represents one of three types of individuals who experience a minimum wage change at some point during a consecutive 4-month survey period. r_1, r_2, r_3, r_4 denote an individual's CPS month-in-sample number.

Table 1: Unemployment effects of the minimum wage by age, without individual fixed-effects

	Age group		
	16-19	20-24	25-29
MW	.0423***	.0346***	.0233***
	(.0089)	(.0049)	(.0045)
State FE	x	x	x
Month FE	x	x	x
Mean	.1778	.1000	.0694
Adj. R^2	.0071	.0032	.0021
n	575,670	1,061,657	1,230,933

Note: The outcome variable unemployed is a dichotomous variable equal to 1 if the individual is unemployed and 0 if not. The sample includes individuals who are in the labor force. MW is the natural log on the effective state minimum wage. Standard errors are reported below the coefficient in parenthesis. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All observations are weighted using individual sample weights and standard errors are clustered at the state level. The weighted means of each age grouping's unemployment level are reported below the regression results for reference.

Table 2: Unemployment effects of the minimum wage by education & immigrant status, without individual fixed-effects

	23-29		Immigrant	
	No HS	HS	No HS	HS
MW	.0468***	.0625***	-.0080	.0193***
	(.0107)	(.0073)	(.0109)	(.0069)
State FE	x	x	x	x
Month FE	x	x	x	x
Mean	.1488	.0953	.0881	.0682
Adj. R^2	.0091	.0046	.0047	.0021
n	158,832	460,035	318,759	314,924

Note: The outcome variable unemployed is a dichotomous variable equal to 1 if the individual is unemployed and 0 if not. The sample includes individuals who are in the labor force. MW is the natural log on the effective state minimum wage. "No HS" stands for "no high school degree" and includes individuals who did not complete high school. An immigrant is defined to be an individual who was born outside of the US, regardless of age. Standard errors are reported below the coefficient in parenthesis. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All observations are weighted using individual sample weights and standard errors are clustered at the state level. The weighted means of each age grouping's unemployment level are reported below the regression results for reference.

Table 3:
Unemployment effects of the minimum wage by age

Panel A

	Age group		
	16-19	20-24	25-29
MW	-.0944**	-.0296*	-.0257**
	(.0379)	(.0151)	(.0109)
Individual Fe	x	x	x
Month FE			
Adj. R^2	.5934	.6016	.6407
n	501,865	969,313	1,146,128

Panel B

	Age group		
	16-19	20-24	25-29
MW	-	-	-
	.1422***	.0572***	.0374***
	(.0391)	(.0189)	(.0133)
Individual FE	x	x	x
Month FE	x	x	x
Adj. R^2	.5942	.6019	.6409
n	501,865	969,313	1,146,128

Panel C

	Age group		
	16-19	20-24	25-29
MW	-	-.0571***	-.0377***
	.1549***		
	(.0417)	(.0180)	(.0130)
Individual FE	x	x	x
Month FE	x	x	x
Linear trends	x	x	x
Adj. R^2	.5942	.6019	.6409
n	501,865	969,313	1,146,128

Note: The outcome variable unemployed is a dichotomous variable equal to 1 if the individual is unemployed and 0 if not. The sample includes individuals who are in the labor force. Each panel represents a different specification. MW is the natural log on the effective state minimum wage. Standard errors are reported below the coefficient in parenthesis. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All observations are weighted using individual sample weights and standard errors are clustered at the state level.

Table 4:
Unemployment effects of the minimum wage by education & immigrant status

Panel A	23-29		Immigrant	
	No HS	HS	No HS	HS
MW	-.0526	-.0058	-.0118	.0008
	(.0645)	(.0225)	(.0275)	(.0327)
Individual FE	x	x	x	x
Month FE				
Linear trend				
Adj. R^2	.6228	.6418	.5596	.6240
n	141,039	421,424	295,561	292,606

Panel B	23-29		Immigrant	
	No HS	HS	No HS	HS
MW	-.0689	-.0506**	-.0844**	-.0418
	(.0775)	(.0234)	(.0316)	(.0341)
Individual FE	x	x	x	x
Month FE	x	x	x	x
Linear trend				
Adj. R^2	.6233	.6422	.5602	.6242
n	141,039	421,424	295,561	292,606

Panel C	23-29		Immigrant	
	No HS	HS	No HS	HS
MW	-.0574	-.0533**	-.0688***	-.0402
	(.0761)	(.0251)	(.0250)	(.0313)
Individual FE	x	x	x	x
Month FE	x	x	x	x
Linear trend	x	x	x	x
Adj. R^2	.6234	.6422	.5604	.6243
n	141,039	421,424	295,561	292,606

Note: The outcome variable unemployed is a dichotomous variable equal to 1 if the individual is unemployed and 0 if not. The sample includes individuals who are in the labor force. Each panel represents a different specification. MW is the natural log on the effective state minimum wage. "No HS" stands for "no high school degree" and includes individuals who did not complete high school. An immigrant is defined to be an individual who was born outside of the US, regardless of age. Standard errors are reported below the coefficient in parenthesis. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All observations are weighted using individual sample weights and standard errors are clustered at the state level.

Table 5:
Part-time status effects of the minimum wage by age

Panel A			
		Age group	
	16-19	20-24	25-29
MW	-.0464	.0124	-.0013
	(.0417)	(.0128)	(.0105)
Individual FE	x	x	x
Month FE			
Linear trends			
Adj. R^2	.1727	.1914	.2110
n	92,017	532,843	824,400

Panel B			
		Age group	
	16-19	20-24	25-29
MW	-.0665	.0066	-.0013
	(.0427)	(.0137)	(.0102)
Individual FE	x	x	x
Month FE	x	x	x
Linear trends			
Adj. R^2	.1733	.1916	.2112
n	92,017	532,843	824,400

Panel C			
		Age group	
	16-19	20-24	25-29
MW	-.0805*	.0075	-.0001
	(.0424)	(.0136)	(.0108)
Individual FE	x	x	x
Month FE	x	x	x
Linear trends	x	x	x
Adj. R^2	.1732	.1917	.2112
n	92,017	532,843	824,400

Note: The outcome variable part-time is a dichotomous variable equal to 1 if the individual is working part-time and 0 if full-time. The sample includes individuals who are in the labor force. Each panel represents a different specification. MW is the natural log on the effective state minimum wage. Standard errors are reported below the coefficient in parenthesis. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All observations are weighted using individual sample weights and standard errors are clustered at the state level.

Table 6:
Part-time status effects of the minimum wage by education & immigrant status

Panel A	23-29		Immigrant	
	No HS	HS	No HS	HS
MW	-.0885*	-.0102	.0459	.0523***
	(.0488)	(.0249)	(.0282)	(.0194)
Individual FE	x	x	x	x
Month FE				
Linear trend				
Adj. R^2	.2275	.2051	.2429	.2372
n	89,342	289,102	201,621	208,496
Panel B	23-29		Immigrant	
	No HS	HS	No HS	HS
MW	-.0899*	-.0277	-.0122	.0154
	(.0473)	(.0258)	(.0284)	(.0158)
Individual FE	x	x	x	x
Month FE	x	x	x	x
Linear trend				
Adj. R^2	.2281	.2053	.2438	.2386
n	89,342	289,102	201,621	208,496
Panel C	23-29		Immigrant	
	No HS	HS	No HS	HS
MW	-.0665	-.0223	-.0083	.0196
	(.0464)	(.0268)	(.0290)	(.0170)
Individual FE	x	x	x	x
Month FE	x	x	x	x
Linear trend	x	x	x	x
Adj. R^2	.2282	.2053	.2438	.2378
n	89,342	289,102	201,621	208,496

Note: The outcome variable part-time is a dichotomous variable equal to 1 if the individual is working part-time and 0 if full-time. The sample includes individuals who are in the labor force. Each panel represents a different specification. MW is the natural log on the effective state minimum wage. "No HS" stands for "no high school degree" and includes individuals who did not complete high school. An immigrant is defined to be an individual who was born outside of the US, regardless of age. Standard errors are reported below the coefficient in parenthesis. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All observations are weighted using individual sample weights and standard errors are clustered at the state level.

Table 7:
Labor force status effects of the minimum wage by age

Panel A			
		Age group	
	16-19	20-24	25-29
MW	-.1537***	-.1491***	-.0264**
	(.0401)	(.0258)	(.0127)
Individual FE	x	x	x
Month FE			
Linear trends			
Adj. R^2	.6571	.7037	.7822
n	1,150,434	1,305,630	1,393,894
Panel B			
		Age groups	
	16-19	20-24	25-29
MW	.0026	-.1024***	-.0197
	(.0352)	(.0217)	(.0126)
Individual FE	x	x	x
Month FE	x	x	x
Linear trends			
Adj. R^2	.6568	.7044	.7822
n	1,150,434	1,305,630	1,393,894
Panel C			
		Age group	
	16-19	20-24	25-29
MW	.0070	-.1021***	-.0226*
	(.0352)	(.0221)	(.0124)
Individual FE	x	x	x
Month FE	x	x	x
Linear trends	x	x	x
Adj. R^2	.6568	.7044	.7822
n	1,150,434	1,305,630	1,393,894

Note: The outcome variable labor force status is a dichotomous variable equal to 1 if the individual is in the labor force part-time and 0 if not. The sample includes individuals who are in the labor force. Each panel represents a different specification. MW is the natural log on the effective state minimum wage. Standard errors are reported below the coefficient in parenthesis. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All observations are weighted using individual sample weights and standard errors are clustered at the state level.

Table 8:
Labor force status effects of the minimum wage by education & immigrant status

Panel A

	23-29		Immigrant	
	No HS	HS	No HS	HS
MW	-.0806*	-.0870***	-.0732**	-.1182***
	(.0478)	(.0296)	(.0343)	(.0269)
Individual FE	x	x	x	x
Month FE				
Linear trend				
Adj. R^2	.7593	.7648	.8122	.8098
n	212,797	525,939	461,378	394,312

Panel B

	23-29		Immigrant	
	No HS	HS	No HS	HS
MW	-.0751	-.0851***	-.0633*	-.1219***
	(.0488)	(.0293)	(.0357)	(.0251)
Individual FE	x	x	x	x
Month FE	x	x	x	x
Linear trend				
Adj. R^2	.7593	.7648	.8122	.8098
n	212,797	525,939	461,378	394,312

Panel C

	23-29		Immigrant	
	No HS	HS	No HS	HS
MW	-.0905*	-.0849***	-.0641	-.1220***
	(.0475)	(.0280)	(.0385)	(.0269)
Individual FE	x	x	x	x
Month FE	x	x	x	x
Linear trend	x	x	x	x
Adj. R^2	.7593	.7648	.8122	.8099
n	212,797	525,939	461,378	394,312

Note: The outcome variable labor force status is a dichotomous variable equal to 1 if the individual is in the labor force part-time and 0 if not. The sample only include individuals who are in the labor force. Each panel represents a different specification. MW is the natural log on the effective state minimum wage. "No HS" stands for "no high school degree" and includes individuals who did not complete high school. An immigrant is defined to be an individual who was born outside of the US, regardless of age. Standard errors are reported below the coefficient in parenthesis. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. All observations are weighted using individual sample weights and standard errors are clustered at the state level.

APPENDIX TABLES

Table A: Total number of effective changes and average change by state, 1990-2017

State	Total	Average ($)	State	Total	Average ($)
Alabama	7	.56	Montana	15	.32
Alaska	10	.60	Nebraska	9	.63
Arizona	13	.51	Nevada	8	.61
Arkansas	10	.52	New Hampshire	10	.36
California	11	.57	New Jersey	10	.51
Colorado	15	.40	New Mexico	8	.52
Connecticut	17	.34	New York	12	.53
Delaware	13	.38	North Carolina	7	.56
District of Columbia	12	.64	North Dakota	7	.55
Florida	14	.34	Ohio	13	.37
Georgia	7	.56	Oklahoma	7	.56
Hawaii	10	.54	Oregon	19	.34
Idaho	7	.56	Pennsylvania	7	.51
Illinois	10	.49	Rhode Island	12	.46
Indiana	7	.56	South Carolina	7	.56
Iowa	7	.56	South Dakota	10	.53
Kansas	7	.56	Tennessee	7	.56
Kentucky	7	.56	Texas	7	.56
Louisiana	7	.56	Utah	7	.56
Maine	13	.40	Vermont	21	.30
Maryland	11	.54	Virginia	7	.56
Massachusetts	11	.66	Washington	20	.36
Michigan	9	.62	West Virginia	9	.60
Minnesota	8	.49	Wisconsin	8	.45
Mississippi	7	.56	Wyoming	7	.56
Missouri	12	.36			

Note: Totals and averages are calculated using only effective changes. A change indicates that the minimum wage increased either due to a binding federal or state minimum wage law. Averages are in dollars.

Table B: Summary statistics by subsample

	16-19	20-24	25-29	23-29 No HS	23-29 HS	Immigrant No HS	Immigrant HS
Unemployed	.17	.10	.07	.15	.09	.09	.07
Part-time	.05	.03	.02	.05	.02	.04	.02
Participation	.47	.76	.83	.68	.81	.66	.75
Female	.49	.51	.51	.47	.47	.49	.52
Hispanic	.14	.15	.14	.41	.16	.76	.49
Black	.13	.11	.11	.13	.13	.06	.10
Age	17.44	22.02	27.02	26.01	26.01	37.83	39.54
Children	.04	.28	.74	1.16	.83	1.33	1.14
Married	.02	.18	.45	.43	.41	.59	.63
High school	.32	.85	.85	0	1	0	1
Some college	.14	.56	.58	0	0	0	0
Hourly wage	6.95	9.13	11.94	8.86	10.81	10.03	12.25
Individuals i	384,632	469,248	486,924	82,002	193,847	153,940	135,635
Sample $i \times m$	1,214,618	1,408,522	1,496,549	232,354	572,858	485,339	419,132

Note: Except for mean age, mean number of children, and mean hourly wage, all values are in percentages. “HS” stands “high school degree” (no more and no less). “No HS” stands for “no high school degree” and includes individuals who did not complete high school. An immigrant is defined to be an individual who was born outside of the US, regardless of age. This table includes individuals who are only observed for one month. Sample sizes after removing individuals who are only observed for one month are reported in the regression tables.