Federal Reserve Bank of Chicago

The Evolution of Technological Substitution in Low-Wage Labor Markets

Daniel Aaronson and Brian J. Phelan

REVISED
March 2022

WP 2020-16

https://doi.org/10.21033/wp-2020-16

**Working papers are not edited, and all opinions and errors are the responsibility of the author(s). The views expressed do not necessarily reflect the views of the Federal Reserve Bank of Chicago or the Federal Reserve System.*

The Evolution of Technological Substitution in Low-Wage Labor Markets

Daniel Aaronson
Federal Reserve Bank of Chicago

Brian J. Phelan
DePaul University

March 2022

Abstract: This paper uses minimum wage hikes to evaluate the susceptibility of low-wage employment to technological substitution. We find that automation is accelerating and supplanting a broader set of low-wage routine jobs since the Financial Crisis. Simultaneously, low-wage interpersonal jobs are increasing and offsetting routine job loss. However, interpersonal job growth does not appear to be enough – as it was prior to the Financial Crisis – to fully offset the negative effects of automation on low-wage routine jobs. Employment losses are most evident among non-Asian people of color who experience outsized losses at routine jobs and smaller gains at interpersonal jobs.

JEL Codes: J15, J21, J24, J38, O33
Keywords: Low-wage automation, routine-biased technical change, minimum wage

The authors thank participants at EALE/SOLE 2020 World Conference and the Brookings Institution Conference on Automation, Labor Market Institutions, and the Middle Class. The views expressed here do not necessarily represent the views of the Federal Reserve Bank of Chicago or the Federal Reserve System.

Introduction

The fear of automation technology and its potential to displace a large portion of the global labor force is nearly ubiquitous. A 2018 survey from the Pew Research Center reports that almost 80 percent of respondents across 10 countries believe that robots and computers are likely to take over much of the work currently performed by humans sometime in the next 50 years and this change will cause more harm than good, including job loss and rising inequality (Pew 2018). Some studies argue that the Covid-19 pandemic could further accelerate the adoption of automation technology, especially among jobs that require physical interaction (Leduc and Liu 2020; Barrero, Bloom, and Davis 2020).

A certain unease about technology is warranted when accompanied by job loss, as there is robust evidence that workers who are displaced from their jobs tend to experience large declines in lifetime earnings and consequently may face material hardship (e.g. Ruhm 1991; Jacobson, LaLonde, and Sullivan 1993; Sullivan and Von Wachter 2009; Davis and Von Wachter 2011; Jolly and Phelan 2015; Aaronson et al 2019). Indeed, the literature examining the impact of automation technology on middle-skill workers has found both falling employment and falling earnings of affected workers (Autor and Dorn, 2013; Goos, Manning, and Salomons, 2014; Autor, 2019). Thus, it is well-accepted that the automation of middle-skill jobs has contributed to the rise in earnings inequality over at least the past 30 years.

Much less is known about the extent to which low-skill jobs are being automated and, if so, how it is impacting the low-wage workforce. Our reading is that, for many years, the literature largely assumed it was too costly for firms to automate the lowest-wage jobs (Bresnahan, Brynjolfsson, and Hitt, 2002; Manning, 2004; and Autor, Katz, and Kearney, 2008). However, Muro, Maxim, and Whiton (2019) argue that the lowest wage occupations are now the most susceptible to automation. Consistent with that conclusion, a handful of recent studies exploit minimum wage hikes as a shock to the relative price of low-wage jobs to examine whether higher labor costs are associated with elevated periods of technological adoption. These studies find some evidence of capital adoption or labor substitution patterns consistent with the automation of lower paid jobs.[1] However, the extent to which automation is impacting lower paid workers in the U.S. remains unsettled. A prime reason for this uncertainty is the nonexistence of nationally representative data on U.S. automation spending.

This paper makes two novel contributions to a budding literature on the extent and implications of automation among low-wage jobs. Since 2009, the price of technology has continued to fall and many localities have enacted sharp increases in their minimum wage.[2] On its face, these developments might suggest that the automation of low-wage jobs has accelerated and spread. Thus, our first contribution is to document how the labor market realignment associated with the automation of low-wage employment has changed over the first two decades of the century (i.e. pre- vs. post-Financial Crisis), including in response to increasingly common local minimum wage legislation. Our second contribution builds upon Lordan and Neumark (2018), by exploring heterogeneous effects and considering why some demographic groups may have been particularly hard hit by the automation of low-wage jobs.

Using both the Occupational Employment and Wage Statistics (OEWS) and American Community Survey (ACS), we show that the low-wage labor market implications of automation have widened since the Financial Crisis. Higher labor costs (via minimum wage hikes) continue to be associated with falling employment at jobs intensive in cognitively routine tasks, as they were in the first decade of the 21st century. However, the rate of job loss at cognitively routine occupations has increased since the Financial Crisis and job loss has spread to those intensive in manually routine tasks as well. Consequently, the total employment loss associated with an occupation's routineness – whether manual or cognitive – is twice as large now as it was in the decade prior to the Financial Crisis. The decline in routine task employment associated with

[1] Chen (2019), Cho (2018), Geng et al. (2018), Gustafson and Kotter (2018), Hau et al. (2018), and Qiu and Dai (2019) directly examine firm capital expenditures following minimum wage hikes outside the U.S. The majority, but not all, of these studies find that minimum wage hikes expedite the adoption of labor-saving capital. Aaronson and Phelan (2017) and Lordan and Neumark (2018) examine labor substitution patterns following minimum wage hikes. Both studies find evidence consistent with technological substitution.

[2] For example, the price of information technology hardware and services fell more than 20 percent between January 2010 and January 2018, as measured by the Consumer Price Index.

minimum wage hikes has been offset by an increase in the demand for jobs requiring interpersonal tasks. While the positive impact on jobs intensive in interpersonal tasks has also grown larger over time, there is some evidence to suggest that it does not seem to be enough – as it was prior to the Financial Crisis – to fully offset the negative effect of automation on low-wage routine jobs. We show that the falling cost of technology and rising minimum wage levels are related to these trends. An MSA-level analysis largely supports our main empirical findings at the state level. However, we show that automation appears to have had a larger impact on tasks among low-wage jobs in rural and smaller metropolitan areas.

We uncover a similar employment realignment pattern – declining employment at routine-intensive occupations and increasing employment at interpersonal-intensive occupations – across education, age, and sex sub-populations of low-wage workers. However, we find economically and statistically large differences by race that have arisen since the early 2010s. Among White and Asian American workers, who comprise more than 75 percent of those employed in the lowest wage jobs, the decrease in employment at routine jobs and increase at interpersonal jobs roughly offset, leading to no overall job loss following minimum wage hikes. However, non-Asian people of color experience larger employment declines from routine-intensive jobs and much smaller employment gains at interpersonal-intensive jobs. Consequently, non-Asian people of color, and especially Black workers, have experienced notable job loss as the automation of lower paid jobs accelerated over the last decade.

We decompose racial disparities in this job loss to better understand whether it is due to differential exposure to automation (due to elevated levels of employment in highly routinized jobs) or differential treatment. While we observe small differences in employment at highly-routinized low-wage jobs for non-Asian people of color, the vast majority of the differential impact is due to difference in treatment. Moreover, we show that this differential treatment by race, and the resulting overall job losses, are most egregious in states where racial resentment, as measured by Smith, Kreitzer, and Suo (2019)'s racial resentment index, has increased the most, consistent with discrimination.

In sum, we use a framework established in Lordan and Neumark (2018) and Aaronson and Phelan (2017) to add several important new findings to our understanding of automation's impact on the low wage labor market during the 2010s. First, the process of occupational reallocation in response to automation accelerated during the 2010s, as well as broadened into manually-routine jobs. Second, while the loss of routine jobs continues to be accompanied by growth in interpersonal-tasked jobs, it may no longer be enough to avoid a net decline in low-wage employment, as appeared to be the case prior to the Financial Crisis. Third, these employment declines have been particularly harmful outside of large cities and among non-Asian people of color who experience larger declines in employment at routine-intensive jobs but much smaller employment gains at interpersonal-intensive jobs.

Our paper also adds to a growing body of work that documents a variety of margins in which firms respond to minimum wage changes (see Clemens 2021 for an excellent review). One important example is Clemens, Kahn, and Meer (2020), who document that firms immediately upgrade skill requirements for entry level jobs after minimum wage hikes, in particular by requesting older and more educated workers with experience in customer service. This result is complementary to our own in that both papers highlight the extent to which firms replace inputs, either through capital-labor or labor-labor substitution, and increase demand for interpersonal tasks in response to higher labor costs.

I. Conceptual Framework

This section briefly discusses the differential impact that an exogenous wage increase may have on the demand for low-wage workers – with an emphasis on its effect via the adoption of automation technology. For example, consider a firm with a low-wage workforce that faces a legislated minimum wage hike. Within a standard competitive model, the firm has a few choices if the new minimum wage level is expected to exceed workers' marginal product. They may try to improve the productivity of the workforce, either through training of incumbents or upgrading, as in Phelan (2019) and Clemens, Kahn, and Meer (2021). If the wage increase is large enough, an alternative path may be through the adoption of labor-saving automation technology.

Automation need not lead to overall job loss, however. Ultimately, the extent of disemployment depends on whether worker skills are complements or substitutes to the emerging technology. A large, influential literature has shown that automation is especially likely to displace jobs with a heavy bent towards routine tasks (e.g. Autor, Levy, and Murnane 2003; Goos, Manning, and Salomons 2014), a finding consistent with the long secular decline in routine tasks in the U.S. (Jaimovich, Eksten, Siu, and Yedid-Levi 2020). Conversely, new production processes may require complementary job tasks.

A canonical example is a new technology like a self-scanner that shifts a task from a worker to a customer (Aaronson and Phelan, 2017). As firms introduce these labor-saving technologies, they simultaneously create jobs requiring new skills, such as maintaining new machinery or overseeing customer interactions with it. Consequently, in the short-run, employment growth in jobs that require non-routine skills may help offset the decline in routine jobs that are eliminated by automation. However, in the self-scanner example, some of this offsetting employment growth may not necessarily persist over longer periods of time as customers gradually adapt to the new technology. This is analogous to the reversal in skilled labor demand described in Beaudry, Green, and Sand (2016), where non-routine labor demand may increase in the short-run but ultimately fall in the longer-run.

In some circumstances, the adoption of automation technology can lead to a permanently higher level of non-routine employment. One familiar example occurs when automation technology eases a capacity constraint that otherwise limits production. Take the introduction of ordering kiosks or a smartphone-based ordering app that eliminates the need for cashiers at a café. Limited space behind the counter can be repurposed to increase coffee production (Aaronson and Phelan 2019). As wait times fall, fewer people skip their purchase and the café can profitably hire more employees to prepare orders – offsetting the decline in cashiers.

Offsetting non-routine employment growth could also arise from the composition of firms. In Aaronson, French, Sorkin, and To (2018), minimum wage hikes cause labor-intensive firms to fail at a higher rate since the increase in labor cost falls disproportionately on them. As production shifts to more capital-intensive incumbent and entrant firms, the tasks associated with their newly expanded employment would reflect their higher-tech production processes. Indeed, Dustmann, Lindner, Schonberg, Umkehrer, and von Berge (2020) find that minimum wages hikes are associated with workers reallocating their employment from smaller less-productive firms to larger more-productive firms.

Lastly, if the low-wage labor market is better characterized by monopsony, as some recent studies suggest (Krueger and Posner, 2018; Manning, 2020), minimum wage hikes would reduce employment at substitutable (i.e. routine) jobs but increase employment at all other types of low-wage employment.

Taken together, the adoption of automation technology due to an exogenous wage shock, such as a minimum wage hike, is likely to be characterized by falling low-wage employment at routine jobs. However, the employment effects on non-routine tasks are ambiguous. Thus, our empirical analysis focuses on how the composition of employment changes after significant increases to the cost of low-wage labor.

II. Data

Our primary data come from four sources: employment and wages from the Bureau of Labor Statistics' Occupation Employment and Wage Statistics (OEWS) and the Census Bureau's American Community Survey (ACS), state and local minimum wage levels from Vaghul and Zipperer (2019), and occupational tasks developed by Acemoglu and Autor (2011) based on the US Department of Labor's Occupation Information Network (O*NET). We discuss each in turn.

A. Occupation Employment and Wage Statistics (OEWS)

The OEWS contains data on employment levels and average wages for each detailed Standard Occupational Classification (SOC) occupation by state and metropolitan area. Each annual release of the OEWS is based on surveys of 1.2 million establishments. An establishment's participation in the survey takes place at one of six survey dates over the previous three years and therefore the data in a given year reflect a three-year moving average of occupational employment and wages. Our main analysis uses state-level occupational data from 2010 to 2018. We also estimate our empirical specifications using analogous data from 1999 to 2009 (and over the entire 1999-2018 period) in order to compare our new estimates to the pre-Financial Crisis period used in Aaronson and Phelan (2017) and understand why the effects have changed over time.

The OEWS data collection process underwent two changes between 2010 and 2018 that need to be accounted for. First, there were minor adjustments to the occupational coding systems in both 2012 and 2017.[3] To address these changes, we create consistent occupations over the full 2010 to 2018 period whenever possible. We also add occupation fixed effects to the empirical specifications to ensure that the variation used in estimation occurs within occupations and is not due to spurious SOC coding revisions. Second, the 2017 release of the OEWS began reporting occupational employment for an industry that was not previously surveyed, the "private household" industry.[4] This change led to an implausibly large increase, from 144 thousand in 2016 to 521 thousand in 2017, among "Personal and Home Care Aides" in California. Other states did not react this way. For example, Personal and Home Care Aide employment in Texas only increased from 189 thousand in 2016 to 197 thousand in 2017. After performing some additional tests comparing the similarity of annual state-level occupational employment levels in the OEWS and the ACS, we opt to exclude this one occupation in California from the analysis.[5]

---

[3] The OEWS largely adopted the 2010 SOC codes in 2010 but a few occupations were not updated until 2012. For more details, see the reply to question F.8 at https://www.bls.gov/oes/oes_ques.htm#Ques41, last accessed 12/4/19. Moreover, the OEWS combined 21 occupations into 10 more-aggregated occupations beginning with the 2017 data. See https://www.bls.gov/oes/changes_2017.htm, last accessed 12/4/19, for more details.

[4] See https://www.bls.gov/oes/2017/may/oes_tec.htm, last accessed 12/4/19, for more details.

[5] The correlation coefficient between the total state-level occupational annual employment for specific occupations such as cashiers and childcare workers in the OEWS and ACS is close to 0.9. However, the correlation coefficient for Personal and Home Care Aides is 0.6. When we exclude Personal and Home Care Aides in California, this correlation coefficient increases to 0.78. Our subsequent analysis, which uses the ACS to examine the employment response of minimum wage hikes, will not require any adjustments as the ACS is a nationally representative sample of individuals.

We also analyze OEWS occupational data for 328 metropolitan areas because minimum wage policies have become increasingly localized over the last decade. Metro areas present additional challenges, however. Some metro boundaries have changed since 2010 and many frequently cross state, city, and county boundaries.[6] We address these concerns by developing time-consistent metropolitan areas and show estimates on the subsample of metro areas contained within a single state. Since metro areas are smaller, non-exhaustive geographies than states, the metro area data are also necessarily based on fewer establishment surveys and therefore may generate noisier estimates.

Since the wage shocks we examine (i.e. minimum wage hikes) are likely to have larger effects on occupational employment at jobs that pay closer to the minimum wage, we group occupations within states (or metro areas) into wage bins according to the average 2010-2018 ratio of an occupation-state's average wage to the effective minimum wage.[7] This approach ensures that occupations within states remain in the same wage bin over the panel but occupations across states can be in different wage bins. The specific bins we use are average wage-to-minimum wage ratios between 1.0 to 1.5 (Wage Group 1), 1.5 to 2.0 (Wage Group 2), 2.0 to 2.5 (Wage Group 3), and 2.5 to 6.0 (Wage Group 4).[8]

B. American Community Survey (ACS)

We use the 2010 to 2018 ACS to supplement our analysis for two main reasons. First, the OEWS has at least two practical problems; its employment count is a three-year moving average and it excludes (at least until 2017) agriculture and private household services, two important low-wage industries. Neither is an issue in the ACS. Second, the ACS allows us to split the

[6] For example, 51 of the 328 metropolitan areas cross state lines.

[7] That is, $\overline{w2mw}_{js} = \frac{1}{9}\sum_{t=2010}^{2018} \frac{\overline{wage}_{jst}}{MW_{st}}$, where $\overline{wage}_{jst}$ is the average wage for occupation *j* in state *s* and year *t* from the OEWS and MW_{st} is the minimum wage in state s and year t. For the metro analysis, we look at wages and minimum wages at that geography.

[8] These bins differ slightly from our analysis of the 1999-2009 period in Aaronson and Phelan (2017), which use 1.00-1.75, 1.75-2.50, 2.50-3.00, and 3.00-6.00, because the minimum wage has become more binding since the Financial Crisis. Consequently, we change the bounds that make up our Wage Groups so that the *share* of employment in each of the new wage intervals is similar to the share of employment in the broader wage intervals used previously. For example, the share of employment in Wage Group 1 – the lowest paid occupations – was 21 percent in our earlier paper and 18 percent here.

sample by education, age, sex, and race and test whether these subsamples are more prone to changes in employment after minimum wage hikes. Thus, we use the ACS to both corroborate our estimates with the OEWS and extend our analysis to assess whether any demographic groups have been particularly harmed by the automation of low-wage jobs.

Practically, we transform the ACS into a panel of occupation-state-year employment counts to match the OEWS' structure. However, in a separate analysis, we go one step further and disaggregate these totals into industry as well.[9] We then mimic the OEWS analysis by grouping occupations within states into the same wage intervals using the average ratio of the wage-to-minimum wage over the 2010 to 2018 period. Relative to the OEWS, this process of grouping occupations to wage intervals is likely to be less precise, as some occupations have very few observations in a given state and an individual wage must be computed from an individual's reported annual earnings, weeks worked, and hours worked. Solely for the purpose of computing these average occupational wage calculations, we address this issue by excluding any individual whose wage-to-minimum wage ratio is more than two standard deviations away from the mean ratio for their reported occupation.[10]

C. Minimum Wage Data

Effective state, city, and county minimum wage levels come from Vaghul and Zipperer (2019).[11] As shown in Appendix Table A1, 29 out of the 51 states, including the District of Columbia, increased their minimum wage between 2010 and 2018. Moreover, many of these hikes were quite large and implemented over several years. For example, both Massachusetts and

---

[9] For industry, we use the detailed Census Industry Codes (CIC). However, since our emphasis is on low-wage employment, we classify only the 45 industries that employ at least 0.5 percent of workers paid less than 150 percent of the minimum wage and combine the remaining 224 CIC industries into a single industry. The results are not sensitive to reasonable perturbations of the 0.5 percent cutoff.

[10] This restriction means that if an individual reported a single hour of work but earned $20,000 as a cashier, their wage to minimum wage ratio of about 2,000 would not influence our computation of a cashier's wage to minimum wage, which tends to be closer to 1.5. Since some states have a small handful of observations for a given occupation, these outliers could otherwise have a very large influence on the state-occupation average wage-to-minimum wage.

[11] The minimum wage data is available at https://github.com/benzipperer/historicalminwage/releases, last accessed 11/12/19. We do not population-weight-adjust state minimum wage levels for city or county laws. However, we have looked at the sensitivity of our results to excluding states with frequent local adjustments and find that the results are unaffected. We also present results at the MSA level that account for local minimum wage activity.

California raised their minimum wage by 38 percent, from $8.00 to $11.00, over a period of three and four years, respectively. At the same time, ten states had predictable and small inflation-based increases in their minimum wage over the entire period.[12] We exclude these inflation-based adjustment states because they are unlikely to have the same effect as unanticipated and larger increases in the minimum wage.

The state-year minimum wages we use reflect those faced by the average respondent in the OEWS and ACS. Therefore, they differ slightly from each other. The OEWS-based analysis uses the minimum wage as of May of each calendar year, which we present in Table A1, to match the OEWS's survey collection month. The ACS-based analysis uses the average minimum wage level over the calendar year to reflect the ACS' full year survey.

D. Task Data

Data on the tasks performed at occupations come from Acemoglu and Autor (2011), who develop these measures from the O*NET database.[13] We transform their six measures – the extent to which an occupation is routine cognitive, routine manual, non-routine cognitive interpersonal, non-routine manual interpersonal, non-routine cognitive analytical, and non-routine manual physical – into six task shares. To compute these shares, each z-score value for each occupation is rescaled relative to the minimum value across all occupations. The six rescaled values are then summed up for each occupation separately and a task share is defined as the ratio of the rescaled value to the sum of all rescaled values.

We often further combine the six tasks into more aggregated measures. For example, we always combine non-routine cognitive interpersonal and non-routine manual interpersonal into a single interpersonal task share. For these combined task metrics, the task share is simply the sum of the two rescaled task measures divided by the sum of all six rescaled task measures. We also will show results based on the overall routineness of an occupation by combining routine

[12] These states are Arizona, Colorado, Connecticut, Florida, Missouri, Montana, Ohio, Oregon, Vermont, and Washington.
[13] The task data is available at https://economics.mit.edu/faculty/dautor/data/acemoglu, last accessed 11/12/19.

cognitive and routine manual tasks into a single measure of routineness, paralleling the approach taken in many studies looking at middle-skill automation (e.g. Autor, Katz, and Kearney 2008).

Table 1 presents the 25 occupations with the largest share of routine tasks and largest share of interpersonal tasks among occupations that land in Wage Group 1 (those occupations with an average wage-to-minimum wage ratio less than 1.5) for at least one state. Motion Picture Projectionists, Sewing Machine Operators, and Meat and Poultry Trimmers tend to have a disproportionately high share of routine tasks while Personal and Home Care Aides, Recreation Workers, and Child Care Workers tend to have a disproportionately high share of interpersonal tasks. The average occupation included among the top 25 routine-intensive low-wage occupation (Panel A) has nearly half of its tasks associated with routine cognitive or routine manual tasks and likewise the average occupation included among the top 25 interpersonal-intensive low-wage occupation (Panel B) has nearly half of its tasks associated with interpersonal tasks. Therefore, naturally, the importance of either routine or interpersonal tasks dwarfs non-routine tasks (either non-routine cognitive analytics or non-routine manual physical) among nearly all Wage Group 1 occupations.[14] Likewise, jobs that tend to have high levels of routine tasks have lower levels of interpersonal tasks. Among Wage Group 1 occupations, the correlation coefficient between the routine share and the interpersonal share of tasks is -0.84.

For each occupation, Table 1 also presents the cross-state average wage-to-minimum wage ratio, national employment in 2010, and the percent change in employment between 2010 and 2018. Between 2010 and 2018, employment grew by 21 percent among the top 25 low-wage interpersonal-intensive occupations but only four percent among the top 25 low-wage routine-intensive occupations. These divergent trends are even more pronounced among occupations where routine or interpersonal task share exceeds 50 percent (Figure 1). This shift in

[14] The low-wage occupation with the largest share of non-routine tasks (34 percent) is bicycle repairman. The average share of non-routine tasks among the 25 low-wage occupations with the highest share of non-routine tasks is 27.5 percent.

employment in the low-wage labor market mirrors the same secular patterns in routine and interpersonal tasks taking place among middle-skill jobs (Deming 2017; Autor 2019).

An increase in the relative price of labor to capital should be associated with declining growth in routine employment and possibly elevated growth in non-routine employment, escalating these secular employment trends. While our empirical analysis will directly estimate these effects using minimum wage hikes, it is instructive to simply examine employment trends separately for states that increased their minimum wage and states that did not during our timeframe.[15] Figures 2 to 4 present this comparison separately for occupations that are especially heavy in routine, interpersonal, and all other tasks, respectively. These figures highlight that employment trends were nearly identical in minimum wage and non-minimum wage hike states between 2010 and 2013, when there was a pause in minimum wage activity.[16] Once state and local minimum wage hikes begin again in earnest in 2014, relative employment in minimum wage states declined markedly in routine occupations (Figure 2) and increased, although with a bit more delay, in interpersonal occupations (Figure 3). Interestingly, there appears to be no difference in employment growth at all other non-routine, non-interpersonal occupations (Figure 4), suggesting there are not clear secular differences in the employment patterns of low-wage jobs between states that passed minimum wage legislation and states that did not. Thus, the raw data seem to suggest that minimum wage hikes are associated with declining employment in routine-intensive low-wage jobs but growing employment in low-wage interpersonal-intensive jobs.

Moreover, if we limit our sample of minimum wage states to only those six that adopt minimum wage hikes in every year over the 2015-2017 period,[17] which would address recent

[15] The minimum wage hike sample includes the 19 states that raised their minimum wage separate from CPI-based adjustments. The no hike samples include 22 states that did not increase their minimum wage over the period 2010-2018.
[16] The only non-CPI adjustment hikes introduced between 2010 and 2013 were in Illinois ($0.25 in 2011), Nevada ($0.70 in 2011), and Rhode Island ($0.35 in 2013).
[17] The six states are Alaska, Arkansas, Hawaii, Maryland, Massachusetts, and South Dakota. We do not include Michigan, Minnesota, and Washington, DC in this group because they increased their minimum wage in summer 2014. We also do not include California because several local areas within California enacted minimum wage hikes

criticism of difference-in-difference estimators when the timing of treatment varies (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2020), the figures look similar to what we present here (see Appendix Figure A1). The one difference is that the decrease in routine employment now also occurs with a delay. Given the importance of this critique of difference-in-differences estimation, we expand upon our empirical analysis in the robustness section of the paper, where we show our baseline estimates on this more limited sample of treatment states.

III. Empirical Methodology

Our empirical methodology examines how wages shocks stemming from minimum wage hikes affect occupational employment growth at jobs that differ in the extent to which they are associated with routine tasks. This examination of changes in the task-content of employment follows an earlier academic literature which assumes that automation technology is more likely to replace jobs with a larger share of tasks that are routine in nature (Autor, Levy, and Murnane 2003), often referred to as "routine-biased technological change" (Goos, Manning, and Salomons 2014). Under this framework, a minimum wage hike is associated with automation if it causes falling relative employment at low-wage routine jobs.

Our primary empirical specification regresses long differences in occupational employment on changes in the minimum wage and interactions between the change in the minimum wage and the routineness of a job. An emphasis on long-differences in the outcome variable has been advocated by researchers studying the longer-term effects of minimum wage hikes (e.g. Baker, Benjamin, and Stanger 1999; Meer and West 2016; and Sorkin 2015). It is especially appropriate for this analysis because the capital adoption necessary to automate certain jobs may take time to occur. Moreover, the structure of the OEWS data, which are based on surveys taking place over the past three years, means that employment changes will only reflect a

in 2014 and earlier. Later, we show regression results when these four additional states are included to the six state sample.

time series from independent surveys in long differences. Specifically, we estimate the following difference-in-differences regression model:

$$
\begin{aligned}
\Delta \ln Emp_{jst} = {} & \propto_s + \propto_t + \propto_j + \propto_k + \sum_{k=1}^{4} \sum_{z=-2}^{1} \beta_z^k \left(WG_{js}^k * \Delta lnMW_{s,t+z} \right) \\
& + \sum_{k=1}^{4} \sum_{z=-2}^{1} \beta_{z,T}^k \left(WG_{js}^k * \Delta lnMW_{s,t+z} * TaskShare_j \right) \\
& + \sum_{k=1}^{4} \gamma_1^k \left(WG_{js}^k * Year_t * TaskShare_j \right) \qquad (1) \\
& + \sum_{k=1}^{4} \gamma_2^k \left(WG_{js}^k * Year_t * lnEmp_{js,t-4} \right) + \varepsilon_{jst}
\end{aligned}
$$

where $\Delta lnEmp_{jst}$ is the change in the natural log of employment for occupation j in state s and year t from four years earlier. The minimum wage variables in the baseline regression specification, $\Delta lnMW_{s,t+z}$, are a set of four one-year changes in the natural log of the minimum wage in state s from two years prior (t-2) to one year post year t (t+1), where for example, $\Delta lnMW_{s,t-2} = lnMW_{s,t-2} - lnMW_{s,t-3}$. Thus, we estimate the effects of these hikes from one year before the hike until two years after the hike.[18] In the robustness section, we also include the lagged change in the minimum wage from three years prior to t ($\Delta lnMW_{s,t-3}$) and the leading change two and three years post t ($\Delta lnMW_{s,t+2}$ and $\Delta lnMW_{s,t+3}$). This lead and lag structure allows us to test the parallel trends assumption (associated with the lead coefficients) implicit in this difference-in-differences empirical specification and to examine the effects of a minimum wage several years after a hike.

The empirical specification also controls for state or metro area (α_s), year (α_t), occupation (α_j), and wage group (α_k) fixed effects; the task content of an occupation, TaskShare_j, where we

[18] Equation (1) is a long-difference distributed lag model, so named because it has a long difference in the outcome but one-year changes in the minimum wage like a distributed lag model. In this framework, the β coefficients reflect cumulative changes in the outcome up until a point in time – whereas a traditional distributed lag model reflects marginal changes in the outcome.

allow this effect to vary over time ($Year_t$) by wage group (WG^k_{js}); and the lagged natural log of the employment level from four years prior ($lnEmp_{js,t-4}$), where we also allow this effect to vary over time by group. Observations in the baseline version of equation (1) are weighted using the base year employment levels ($Emp_{js,t-4}$) and standard errors are clustered at the state or metro area level. Our use of sample weights is discussed in more detail in Appendix B.[19]

The key coefficients of interest, β^k_{zT}, describe the impact of a minimum wage hike on the change in employment of a particular task content *T*. Importantly, since we are regressing long-differences in outcomes on one-year changes in the minimum wage, β^k_{zT} reflect *cumulative* effects over the four-year period. Equation (1) ensures that the identification of β^k_{zT} takes place within occupations while still controlling for time trends in employment across tasks – such as the ongoing decline in routine jobs, which we document in Figure 1. To ease the interpretation of the β^k_{zT} coefficients, the $TaskShare_j$ variables are standardized to be z-scores. Thus, the β^k_{zT} coefficients represent the employment elasticity for a standard deviation increase in the specific task share *T*. We then estimate separate regressions for each task share, such as the extent to which an occupation is routine cognitive or routine manual.

The β^k_{zT} coefficients will be unbiased so long as state-level minimum wage changes are unrelated to unobserved employment trends associated with task *T* in state *s*. While we view this as a reasonable assumption, we also present estimates of Equation (1) that include state-by-year fixed effects but exclude the non-interacted $\Delta lnMW_{s,t-z}$ variables due to multicollinearity. The β^k_{zT} coefficients in that specification will be unbiased so long as the state-level minimum wage changes are unrelated to unobserved employment trends associated with task *T* in state *s* and year *t*. This assumption is even more likely to hold.

Our ACS analysis estimates Equation (1) with an occupation-industry-state-year panel that includes industry fixed effects. We also present an OEWS-comparable version of the ACS estimates without industry.

[19] Weights are used because we find strong evidence for heteroscedasticity and heterogeneity by occupation size (see Solon, Haider, and Wooldridge 2015).

IV. Results

A. OEWS State-level Estimates

Table 2 presents our basic estimates of the effect of a minimum wage hike on overall employment over the period 2010 to 2018. In the first four columns, grouped under Specification 1, we show how overall *cumulative* employment changed in the year before, year of, year after, and two years after a minimum wage hike – where each column represents the estimated effect on the collection of occupations in each wage grouping. The estimates provide some evidence that minimum wage hikes during the 2010s were associated with employment declines at the lowest wage occupations. While none of the coefficients in years after the hike are negative, the estimates for Wage Group 1 – those occupations with an average wage-to-minimum wage ratio less than 1.5 – imply that there was a positive leading effect. That is, employment in these occupations had been growing in states that increased their minimum wage prior to the hike. Thereafter, this relative employment advantage disappeared after the minimum wage increased and the change in employment growth, i.e. the difference in the coefficients from two years after the hike to the year prior to the hike, is -0.18 (0.10), which is statistically significant at the 10 percent level and economically on the higher side of the literature that has examined the overall employment effects of minimum wage hikes (Neumark and Wascher 2008; Dube, Lester, and Reich 2010; Neumark, Salas, and Wascher 2014; Allegreto, Dube, Reich, and Zipperer 2017; Cengiz, Dube, Lindner, and Zipperer, 2019). Overall employment at occupations in Wage Groups 2 to 4 (average wage to minimum wage ratio of 1.5 to 6) are not materially affected by the minimum wage hike.[20]

In columns (5) to (12), we begin to explore specific job tasks by adding the interaction between the routine cognitive share of an occupation and the minimum wage change (i.e. in

[20] The coefficients for the second lowest wage occupation group (Wage Group 2) is positive between the lead and two-year lagged coefficients, i.e. the opposite direction, although the change is small and not statistically significant. Like with Wage Group 1, there appears to be a leading effect of minimum wage hikes on occupational employment in Wage Group 4 and the change between the leading and two-year lag is negative although not statistically different from zero, -0.12 (0.09).

Equation (1), *TaskShare$_j$* is based on routine cognitive tasks). The first four of these columns (Specification 2) include state and year fixed effects and the latter four (Specification 3) include state-by-year fixed effects. The estimates strongly suggest that minimum wage hikes are associated with employment declines at the lowest paying jobs (i.e. Wage Group 1) that are intensive in routine cognitive tasks. This effect is evident one year after the hike, with an estimated elasticity of -0.10 (0.05), and more than doubles two years after the hike to -0.22 (0.06). In words, these estimates imply that an occupation with a routine cognitive share of tasks that is one standard deviation above average, such as Parking Enforcement Workers and Hotel Desk Clerks, experience relative employment declines of 2.2 percent for every 10 percent increase in the minimum wage. Occupations with routine cognitive tasks that are two standard deviations above average, such as Lobby Attendants and Gaming Dealers, would experience employment declines that are twice as large.

By contrast, there is no impact of minimum wage hikes on routine cognitive employment at higher paying occupations, i.e. jobs in Wage Group 2, 3, or 4. Moreover, the results are not materially affected whether we use state and year fixed effects or state-by-year fixed effects. We also find that the overall employment effect – the difference in the coefficients between the leading effect and the change two years after the hike – is more muted and statistically insignificant -0.12 (0.09) in this specification. Thus, while there is some evidence of overall employment declines, it is weakly statistically significant and not robust to the inclusion of routine task shares.

Table 3 presents a full set of β^{k}_{zT} coefficients for each of the *T* task categories. Each column reports the estimated elasticities from a different regression that includes state-by-year fixed effects (Specification 3 in Table 2). For ease of comparison, Column 1 repeats the cognitively routine estimates presented in Columns 9 and 10 of Table 2.

In Column 2, we show that minimum wage hikes are causing employment to decline at the lowest wage occupations intensive in routine *manual* tasks and the magnitude of the decline is quite similar to the observed decline at routine cognitive jobs. The point estimates imply that a

10 percent increase in the minimum wage causes employment to decline by 1.4 percent one year after the hike and 1.7 percent two years after the hike at occupations with a routine manual task share that is one standard deviation above average.[21] Again, no changes are occurring at occupations in Wage Groups 2, 3, or 4 (see Appendix Table A2 for Wage Groups 3 and 4 results). These patterns are consistent with wage shocks due to minimum wage hikes expediting the adoption of automation technology, which, in turn, supplant employment at routine cognitive and routine manual jobs. Moreover, the timing of the changes in employment, one and two years after the hike, is consistent with longer-term substitution effects. We observe a similar pattern of effects when routine manual and routine cognitive tasks are combined to form a single index of routineness (see column 3).

Although there is strong evidence of job loss among occupations intensive in routine tasks, minimum wage hikes are also associated with a significant offsetting increase in employment at jobs intensive in interpersonal tasks. Column 4 shows that a Wage Group 1 occupation with interpersonal tasks that are one standard deviation above average experiences employment growth of 1.9 percent and 2.4 percent one and two years after a 10 percent increase in the minimum wage.[22] While these coefficients are only statistically significant at the 8 and 6 percent level, respectively, the magnitude and timing relative to the change in employment at routine jobs is notable. And once again, no such effect shows up in Wage Groups 2, 3, or 4. Moreover, the remainder of Table 3 comfortingly suggests that minimum wage hikes tend not to affect employment at non-routine cognitive analytical or non-routine manual physical occupations, which are likely less automatable.

Changes Over Time

[21] For a point of reference, low-wage routine manual jobs that are about one standard deviation above average include Meat/Poultry Trimmers and Farmworkers while low-wage routine manual jobs that are about two standard deviations above average include Laundry/Dry Cleaning Workers and Garment Pressers.

[22] Occupations one standard deviation above average in interpersonal tasks include Manicurists and Restaurant/Cafe Hosts. Two standard deviation above average occupations include Recreation Workers and Personal/Home Care Workers.

Figure 5 (and Appendix Table A3) compares our results from 2010 to 2018 with identical regression specifications estimated on the 1999 to 2009 OEWS data. We find that minimum wage hikes have led to declining routine employment in both decades and the secular pattern of the effects are similar in that the estimated realignment away from routine tasks, and towards interpersonal tasks, grows in magnitude over the two years following a hike.

An important difference between the estimates is that the magnitude of the responses accelerated over time. To see this change, note that the rate of employment decline at routine jobs two years after the hike is larger in the post-Financial Crisis period than in the decade leading up to and including the Financial Crisis, whether routine is defined by cognitive tasks (Panel A), manual tasks (Panel B), or both (Panel C).[23] For example, when we combine routine cognitive and manual tasks together, the estimated two-year elasticities for Wage Group 1 in the post-Crisis period are two and a half times the size of the estimated effects in the pre-Crisis period: i.e. -0.22 (0.06) versus -0.08 (0.04), respectively. Similarly, the offsetting employment growth associated with Wage Group 1 occupations intensive in interpersonal tasks grew between the first two decades of the 21st century (Panel D of Figure 5). The estimated interpersonal elasticities two years after the hike are 0.24 (0.12) in the 2010-2018 period compared to -0.01 (0.07) in the 1999-2009 period.[24]

We find two other notable differences across the decades (see Appendix Table A3 for earlier decade details). First, the adverse impact of minimum wage hikes on overall Wage Group 1 employment appears to be larger post-Financial Crisis. Second, increases in the minimum wage in the 1999-2009 period affected the employment levels of routine and interpersonal

[23] This remains the case even after we account for any pre-trend that may be taking place. Over the pre-Crisis period the estimated Wage Group 1 elasticity two years after a minimum wage hike relative to the leading effect is -0.12 (0.05). This is smaller in magnitude than a comparable estimate of -0.22 (0.14) for the post-Crisis period. This combined effect for the pre-Crisis period is quite similar to the results in Aaronson and Phelan (2017), who estimate an elasticity of -0.13 (0.05). The small -0.01 differences between our current and past point estimates are due to the addition of occupation fixed effects and whether to winsorize the largest employment changes.

[24] The estimated effect on interpersonal tasks over the period 1999-2009 is less evident here than in Aaronson and Phelan (2017) because much of the offsetting employment growth in the pre-Crisis period was in cognitive interpersonal jobs but not manually interpersonal jobs. In this study, we combine cognitive and manual interpersonal tasks for simplicity and because the distinction between cognitive and manual interpersonal tasks looks less important in the 2010 to 2018 data.

occupations in Wage Group 2, whereas we find no such effects in the post-Crisis period. Thus, it is possible that some of the acceleration in the rate of automation that is apparent in Wage Group 1 occupations in the post-Crisis period may reflect a better "targeting" of occupations likely to be affected by minimum wage hikes than is the case in the earlier decade.

Why has the pace of automation accelerated recently? At least two concurrent trends could be at play: the price of automation technology has fallen substantially and the level of the effective minimum wage has risen. These trends are shown in Figure 6, which plots the producer price index (PPI) over the period 1999-2018 for two series that capture technology prices (left scale) - (i) computers and (ii) point-of-sale (POS) terminals - and the average state-level minimum wage (right scale).[25] Interestingly, the timing of change in these series is somewhat different across the three series, with the price of computers declining the most early in the period, the price of POS terminals declining the most late in the period, and the average minimum wage increasing during two distinct periods in the late aughts and the late teens.

We distinguish between these potential causes by estimating a series of regressions that introduce interaction terms between our ΔMW-X-TaskShare variables and the time trend variables related to either technology prices or the average minimum wage level.[26] The coefficients on these triple interaction terms describe how the employment reallocation of minimum wage hikes change over time as the average minimum wage level rises or technology prices fall between 1999 and 2018. Thus, we identify the potential importance of these different explanations solely using time series variation. While these tests reflect the best we can do with available data, we also acknowledge that the estimates are not without concern.[27]

[25] The specific series are the commodity-based electronic computer PPI (WPU1151) and the commodity-based Point of Sale Terminal PPI (WPU115406).

[26] Since it is likely that technology prices and the minimum wage level at the time of the two-year after hike are what matters, we use the prices and minimum wage levels lagged three years. Additionally, to ease the interpretation of these coefficients, we transform these minimum wage and technology price levels into z-scores.

[27] For example, there is no variation in technology prices across occupations or geographies, which may raise the possibility that we are understating their importance. To give one concrete example, suppose that technology needs to be customized to replace specific occupations, even when two occupations are similarly routine. In this case, equipment makers may opt to only develop the technology for larger occupations where they have a better chance of recouping their occupation-specific investment. This could lead to occupation-level variation in technology prices. Indeed, we find some support for the importance of occupation size, which we briefly discuss in Appendix B.

Table 4 presents the task content-specific results. For brevity, we only report the two-year after effect for Wage Group 1 occupations. Column (1) provides the average effect over the full 1999-2018 timeframe. The remaining columns consider alternative ways to capture the time trend and thereby potential channels of the secular change. In column (2), results include a simple linear time trend interacted with the full set of lags and leads of the minimum wage change (although again we report only the interaction with the two-year lagged minimum wage change). These estimates show that the decline in routine manual tasks and the increase in interpersonal tasks after a minimum wage hike has indeed gotten larger over time. There is no secular change in the decline in jobs intensive in routine cognitive tasks, implying the rate of decline at routine cognitive jobs has been steady over the 1999-2018 period.

In columns (3) and (4), we report interactions of the two-year lagged minimum wage change with the PPI for computers and the PPI for POS terminals. Falling producer prices for computer technology explain some of the rising employment at jobs intensive in interpersonal tasks (column 3). That said, the evidence is not overwhelming. Conversely, falling prices for POS terminals have contributed significantly to both the decrease in employment at jobs intensive in routine manual tasks and the rise in employment at jobs intensive in interpersonal tasks (column 4), two of the job tasks where we observe changes over the 1999-2018 period. Lastly, in column (5), we consider the role that rising minimum wage levels has had on the increased importance of automation in the low wage labor market. We find that a time trend based on the average state-level minimum wage explains much of the changing employment response at jobs intensive in routine manual tasks, any routine tasks, and interpersonal tasks – the very jobs where automation has likely expanded.[28]

Thus, we find evidence that both technology prices associated with POS terminals and high minimum wage levels have contributed to the accelerating rate of low-wage automation and

[28] The results are broadly similar when we use state-year variation in the minimum wage level, rather than the average state minimum wage level across all years. That specification introduces a panel aspect to the statistical model, rather than relying solely on the time-series.

show greater explanatory power, as measured by the adjusted R^2, than a simple linear time trend. Which of these two factors is more important? In results not shown, when we run a horserace with both sets of interaction terms, it is the minimum wage interactions that remains statistically significant and economically most relevant. Moreover, the adjusted R^2 associated with the minimum wage level is somewhat larger. Of course, other potential explanations are plausible as well, although those we have been able to explore did not pan out.[29] Therefore, we conclude that the increase in the rate of automation stems from a combination of rising minimum wage levels and falling technology prices, with perhaps somewhat more support for high minimum wage levels spurring greater adoption.

B. OEWS Metropolitan-level Estimates

Next, we turn to using sizable variation in city and county minimum wage policy during the 2010s to estimate the effects of minimum wage hikes on occupational employment at the MSA level. Panel A of Table 5 presents results on overall employment and Panels B and C on task share-specific employment using all MSAs (Column 1) available in the OEWS.[30] Like with the state-based results discussed above, there is no discernable impact on employment at higher paying jobs and therefore we move the estimated Wage Group 2, 3 and 4 results to Appendix Table A4.

The MSA findings have a similar but muted flavor to the state-based ones. For example, the MSA estimates imply a two-year post-hike elasticity of -0.12 (0.07) when both routine cognitive and routine manual tasks are combined to form an overall routine share of tasks, compared to -0.22 (0.06) at the state level. Likewise, the interactive task elasticity is 0.16 (0.08)

[29] Alternative explanations we considered include: a) changes in the composition of states that increased their minimum wage over time, b) the timing of the Financial Crisis and how it differentially impacts our 1999-2009 versus 2010-2018 estimates, and c) changes in composition of surviving firms, and in particular their ability to adopt to new technologies, after the Financial Crisis. We find no evidence that the sample of states or the timing of the Financial Crisis materially matter. We do not have the data to sufficiently test that changes in firm survival post-Financial Crisis played a role.

[30] The results in Panel A are from an empirical specification that includes MSA and year fixed effects while the results in Panel B and C are from empirical specifications that include MSA-by-year fixed effects.

at the MSA-level and 0.24 (0.12) at the state-level. This attenuation also impacts the overall employment response in Wage Group 1, which becomes essentially zero at the MSA-level.

We expected that smaller samples would reduce the precision of the estimates once we switched to the MSA data. However, the smaller point estimates are surprising. They could reflect measurement error introduced by MSAs that cross state or city lines. However, when we limit our data to only those OEWS metropolitan areas that are wholly contained in a state, the point estimates, while more precise, do not look any more similar to the state-level estimates (see Appendix Table A5).[31]

Alternatively, attenuated MSA results could reflect heterogeneity. A metro area analysis will necessarily place a greater emphasis on urban areas than a state-level analysis, and perhaps the realignment in employment that we observe is more likely to take place in rural locations and smaller cities. To test this hypothesis, we re-estimate our statistical models on the largest 25 metropolitan areas (Column 2) and excluding the 25 largest metropolitan areas (Column 3).[32] We find that the basic employment realignment is not at all evident in the largest metropolitan areas. However, when the largest cities are excluded, the estimated elasticity at low-wage routine occupations increases to -0.26 (0.09) two years after the hike (inclusive of the leading effect) and the estimated elasticity at low-wage interactive occupations increases to 0.23 (0.08), nearly the same as the state-level estimates. These results strongly suggest that low-wage automation that is spurred by minimum wage hikes is especially pertinent outside of the largest cities. Indeed, when we go one step further and examine the impacts in rural/non-metropolitan areas (Column 4), the estimated effects are especially large, albeit with imprecise standard errors that won't allow rejecting a difference with the more precisely estimated effects from smaller metropolitan areas.

[31] An alternative explanation that we cannot rule out is that the differing geographic boundaries – with many minimum wage hikes restricted to city limits but MSAs comprising much larger geographic areas – work to attenuate the estimated impact of the hike, even if the impact is actually taking place.

[32] The 25 largest metropolitan areas in the OEWS are Atlanta, Baltimore, Boston, Chicago, Dallas, Denver, Detroit, Houston, Los Angeles, Miami, Minneapolis, Nassau County Long Island, New York City, Orlando, Portland OR, Philadelphia, Phoenix, Pittsburgh, Riverside California, San Diego, San Francisco, Seattle, St. Louis, Tampa, and Washington, DC.

Metro size heterogeneity could arise for several reasons. One possibility is that high routine task jobs are more common in non-metro and small metro areas. However, we find little difference in the share of routine tasks by metropolitan status.[33] A second possibility is that minimum wage hikes have a larger bite in smaller cities and rural areas. But minimum wage bite, as measured by the Kaitz Index, does not differ by MSA size and our MSA-level results are similar if we use the Kaitz Index in place of our minimum wage variable. Lastly, we find no evidence that low-wage jobs in large MSAs had already been automated prior to the 2014-18 minimum wage hikes.[34] Therefore, we leave it to future research to better understand these metropolitan size differences.

C. ACS Estimates

Estimates derived from the ACS, presented in Table 6, are consistent with those from the OEWS.[35] We find the two-year-after employment elasticity among Wage Group 1 occupations is -0.16 (0.08), similar to the state-level OEWS results (Panel A). Moreover, we continue to see a notable reallocation of low-wage employment away from occupations intensive in routine cognitive and routine manual tasks and towards occupations intensive in interpersonal tasks. The estimated two-year-after task-based point estimates are somewhat larger in the ACS than the state-based estimated using the OEWS,[36] but, these differences go away when, like with the OEWS data, industry is not accounted for in the ACS (see Appendix Table A6). Moreover, the timing of the employment response in the ACS is similar to the OEWS, with most of the effect

[33] The average Wage Group 1 job in smaller (larger) cities is composed of 13.9 (13.6) percent routine manual tasks, 23.8 (23.7) percent routine cognitive tasks, 42.6 (42.5) percent interpersonal tasks, and 19.8 (20.1) percent non-routine tasks.

[34] A direct examination of previous technology adoption is not feasible. Instead, we examine variation in routine employment trends across large and small cities during a period – 2010 to 2014 – when minimum wage legislation was quiet. The absolute change in employment at highly routinized jobs (i.e. jobs with a routine share greater than 45 percent) in large and small cities was very similar over this period. By contrast, when the minimum wage increased substantially in several small and large cities between 2014 and 2018, the relative decline in routine share was far *larger* in larger cities (16 percent declines in the largest cities versus 5 percent in the smaller cities).

[35] Table 6 is based on a state-industry-occupation panel. In Appendix Tables A6, we use a state-occupation panel more directly comparable to the OEWS. We prefer the version that controls for industry because it improves precision and addresses a potential concern with our OEWS estimates.

[36] The ACS all routine tasks elasticity is -0.27 (0.08) compared to -0.22 (0.06) in the OEWS. The ACS interpersonal tasks elasticity is 0.32 (0.09) versus 0.24 (0.12) in the OEWS.

coming two years after the hike. This timing gives us greater confidence that the delayed results in the OEWS are not an artifact of its moving average data but instead reflect the time to adopt and implement new technology. In Panels B and C of Table 6 and Appendix Table A6, we show larger employment responses outside the 25 largest MSAs and in Appendix Table A7 we show no significant effect at higher wage occupations, again mimicking the results from the OEWS.

We can also show the same time trends in the ACS that we observe in the OEWS. Figure 7 plots the two-year after effect interaction with routine cognitive tasks, routine manual tasks, overall routine tasks, and interpersonal tasks using a range of sample years. While the ACS in its current form only goes back to 2005, the secular acceleration of employment declines at routine employment (and employment growth at interpersonal jobs) is still visible. The first point in each figure uses the full 2005-2018 period, giving us a sample of four-year changes from 2009-2018. As the earlier years are dropped one-by-one, the estimated effects consistently increase in magnitude.

D. Heterogeneity by Race

The key advantage of the ACS is that it allows us to explore heterogeneity by worker characteristics. Specifically, we stratify the ACS by education (high school diploma or less versus some college or more), age (under age 30 versus 30 or older), sex, and race (non-Asian people of color versus White and Asian American people).[37] Table 7 and Appendix Figures A2-A4 present the results for Wage Group 1 (see Appendix Table A8 for other Wage Groups).

The employment realignment associated with minimum wage hikes – decreasing employment at routine-intensive jobs (Panel B) and increasing employment at interpersonal-intensive jobs (Panel C) – is evident for each of the different subsamples. Moreover, while the prevalence of low-wage employment is much larger for less-education and younger workers, the estimated employment elasticities at routine and interpersonal jobs (two years after a minimum wage hike) are only slightly larger than their subgroup counterpart and none are statistically

[37] Non-Asian people of color include individuals that identify as neither White nor Asian on the ACS' race survey question. 64 percent of non-Asian people of color identify as Black and 26 percent as "some other race alone."

different than the estimates on the overall sample.[38] Likewise, the estimated overall employment effect is fairly similar by age, education, and sex two-years after a hike (Panel A).[39]

However, we find striking differences by race (panel A in Table 7 and Appendix Figures A2-A4). The estimated overall employment effect among non-Asian people of color is an economically large -0.64 (0.18) two-years after a minimum wage hike. By comparison, the overall employment elasticities for the Asian American and White samples are essentially zero. This sharp disparity suggests that all of the employment losses associated with automation are borne by non-Asian people of color, of which Black workers compose the majority. Indeed, the overall employment elasticity for Black workers is -0.77 (0.27) two years after a minimum wage hike, compared to -0.32 (0.46) for the remainder of the non-Asian people of color sample.[40] Moreover, this racial gap has largely arisen after the 2008-09 financial crisis recession (see Appendix Figure A2). In the decade leading up to and including that recession, there was virtually no difference by racial groups with regard to how job tasks respond to minimum wage hikes.[41]

The recent divergence in the overall employment effects of minimum wage hikes is surprising since our estimate of the employment elasticity for non-Asian people of color at routine-intensive jobs of -0.55 (0.18) is well balanced by the estimated employment elasticity at interpersonal jobs of 0.49 (0.21). However, unlike White and Asian American workers, these

[38] That said, the estimates for older and more educated workers oscillate signs between the one-year after and two-year after coefficients, creating some uncertainty about the results for these subsamples.

[39] There appears to be a short-term increase in employment among older workers, but this effect disappears two-years after the hike. There also appear to be differences by sex. When one accounts for the leading effect, the change in estimates are actually larger for men. Indeed, when we estimate the effects over time (Appendix Figure A2) men appear to have had a more negative employment effect from minimum wage hikes passed during the 2006-09 housing slowdown and recession, consistent with Clemens and Wither (2019).

[40] Despite the differences between Black workers and other non-Asian people of color, we continue to emphasize the overall non-Asian people of color sample due to sample size concerns when we focus solely on Black workers. Note, that people of color in this context solely refers to racial minorities. A large majority of Hispanics in the ACS identify as being White.

[41] Going back further in time, Bailey, DiNardo, and Stuart (2020) find that African American men were disproportionately impacted by the 1966 Fair Labor Standards Act, which raised the minimum wage to its highest level in the 20th century. In Appendix Figure A2, we show that the estimated effects for non-Asian people of color tend to be similar to White and Asian American workers over the full sample period 2009-2018 but become distinct – with larger overall employment losses – beginning with the 2012-2018 period, suggesting these racial disparities have arisen after the financial crisis.

gains and losses do not cancel each other out. We consider two broad explanations for this pattern: a) difference in treatment and b) difference in exposure.

Difference in treatment refers to the possibility that non-Asian people of color experience larger overall job losses from automation because the magnitude of treatment effects varies by race. We analyze this potential explanation through separate regressions by racial groupings, where we extend our basic framework and estimate separate effects by whether an occupation's routineness is above or below the median of routineness of all occupations. Differences in these coefficients tell us whether the disparities are being driven by employment declines at highly routinized jobs (larger negative coefficient on the interaction term at above median routine jobs), by employment gains at the low-routine/high-interpersonal jobs (larger negative coefficient on the interaction term at below median routine jobs), or equally between the two. This connection between low-routine and highly interpersonal jobs stems from the strong negative correlation between a low-wage job's routine and interpersonal share of tasks.

The results – for all Wage Group 1 workers, non-Asian people of color, and White and Asian American workers – are presented in Table 8. In the full Wage Group 1 sample (Column 1), the coefficients on the two-year after effect for above and below median routine jobs are nearly identical: -0.29 (0.12) and -0.28 (0.09), respectively. Thus, for the overall sample, the wage losses at highly routinized jobs (above median) are roughly being offset by the gains at highly interpersonal jobs (below median). However, non-Asian people of color (Column 2) experience economically larger employment losses at highly routinized jobs, -0.77 (0.24), and smaller gains at less-routinized/high-interpersonal jobs, -0.38 (0.21). By contrast, White and Asian American workers (Column 3) experience similar sized employment gains at less-routine/high-interpersonal jobs as non-Asian people of color but essentially no job loss at highly routinized jobs.

These patterns are visualized in a somewhat different way in Figure 8, which shows estimates of the overall employment effects of minimum wage hikes on subsamples based on the task intensity of employment as it becomes more/less routinized or interpersonal.[42] We start with a sample of the least routinized occupations (at or below the median of routineness) on the left, and then limit the sample to those occupations that are more routine (above 50th, 70th, and 90th percentile) in the final three sets of bars. The bars highlight the disparity by race in employment losses at highly routine occupations and employment gains at highly interpersonal jobs.

An alternative explanation as to why non-Asian people of color have experienced larger employment losses from automation could be because they are disproportionally employed in highly routinized occupations (Del Rio and Alonso-Villar 2015), what we refer to as difference in exposure. Indeed, during the 2010-18 period, non-Asian people of color were 4.4 percentage points more likely to work in jobs with an above median proportion of routine tasks and 6.7 percentage points less likely to work jobs with an above median proportion of interpersonal tasks, an allocation that is similar in both states that raised their minimum wage during the 2010s and those that did not.

However, racial disparities in the distribution of occupational job tasks are not large enough to explain the difference in employment effects that we observe. One simple way to see this point is to re-estimate our regressions but without the ten most racially segregated Wage Group 1 occupations.[43] The removal of these occupations decreases the overall sample of non-Asian people of color by 6.4 percent and the overall sample of people who are White and Asian American by 2.1 percent and therefore leaves a more balanced racial sample of treated workers in terms of their occupational employment. However, the exclusion of these segregated occupations does not materially alter our estimates. The two-year-after the minimum wage hike

[42] The empirical specification in Table 8 and Figure 8 are somewhat different. Table 8 is based on a statistical model with state-by-year fixed effects and interactions with the change in the minimum wage and an occupation's task intensity; regression coefficienst should be interpreted as relative effects. Figure 8 is based on estimates of the overall employment effects of minimum wage hikes, but we limit the sample by task. These estimates condition on state and year fixed effects and reflect overall employment effects for the different subsamples.

[43] The ten low-wage occupations with the highest proportion of non-Asian people of color include: Butchers, Maids and Housekeepers, Hand Packers and Packagers, Garment Pressers, Sewing Machine Operators, Packaging Machine Operators, Barbers, Graders and Sorters of Agricultural Products, and Food Cooking Machine Operators. The ten occupations with the highest proportion of White and Asian American people include: Dog Walkers, Personal Appearance Workers, Bartenders, Recreation and Fitness Workers, Counter Clerks, Stock Clerks, Chefs, Tailors and Dressmakers, Library Technicians, and Bank Tellers.

elasticity is -0.54 (0.19) for non-Asian people of color and -0.01 (0.09) for White and Asian American workers (compared to -0.64 (0.18) and 0.01 (0.09), respectively, with the full sample of occupations).[44]

To more precisely distinguish between the relative economic importance of difference in exposure versus difference in treatment, we perform two exercises. First, we compute the weighted average overall employment effect using the occupational distribution at above/below median routine occupations combined with the estimated overall employment elasticities at above/below median routine share occupations.[45] The weighted average elasticity for non-Asian people of color is -0.63 versus -0.67 when using the estimated employment effects on non-Asian people of color combined with the employment shares based on the White and Asian American sample versus the non-Asian people of color sample, respectively. This suggests that the difference in exposure explains only seven percent of the difference in the overall employment effect by racial groups. Second, we use the employment-based weights from the White and Asian American sample in the non-Asian people of color sample regressions, as in DiNardo, Fortin, and Lemieux (1996). The estimates will then be purged of pre-existing racial differences in the distribution of occupational employment. This adjustment has little impact. Indeed, the coefficient on the non-Asian people of color sample increases slightly from -0.64 (0.18) to -0.71 (0.31).

Taken together, we conclude that racial differences in the employment response to minimum wages are almost entirely due to differences in how groups of workers are being treated.

---

[44] Since the empirical specification includes occupation fixed effects, it is not surprising that the removal of a few highly segregated occupations has little impact. Another reason to be skeptical of the importance of differences in exposure is that a similar occupational employment pattern existed in the 1999-2009 period, but we find no differences in employment responses by racial groupings over that period.

[45] The weighted average employment elasticity is the product of the estimated elasticity at above median routine jobs and the share of employment at above median routine jobs plus the product of the estimated elasticity at below median routine jobs and the share of employment at median or below routine jobs. The above median shares for the non-Asian people of color sample and the White and Asian American sample are 39.4 and 44.6 percent, respectively. The shares differ from 50 percent because the median routine job, a cook, which in the ACS combines cooks in numerous settings (fast food, institution and cafeteria, etc.), is a large occupation. The elasticities at above and below median routine jobs by racial grouping are reported in Figure 8.

Racial Resentment and the Differential Treatment Effect by Racial Groupings

One potential explanation for the asymmetric treatment response to minimum wage hikes is employer or customer-based discrimination. If customers have discriminatory preferences and therefore would choose not to interact with employees of different races, as found in Holzer and Ihlanfedt (1998) and Bar and Zussman (2017), the creation of new interpersonal-intensive jobs could harm the employment opportunities of non-Asian people of color.[46]

Recent research finds that racial resentment in the U.S. has largely increased over the past 30 years, although this growth has been unevenly spread across U.S. states (Smith, Kreitzer, and Suo 2019). Using public opinion survey responses from 1988 to 2016, Smith et al (2019) show that racial animus has especially increased since 2000 in states, mostly in the East and West, where levels were relatively low during the late 20th century (see Appendix Figure A5).[47]

An increase in racial resentment could lead some employers to systematically discriminate against non-Asian people of color as minimum wage hikes expedited the automation of low-wage jobs away from routine and towards interpersonal tasks. To test this hypothesis, we re-estimate the overall employment effects of minimum wages but add an interaction between the change in the minimum wage and the Smith et al (2019) state-level change in racial resentment between 1988-2000 and 2004-2016.[48] These results are shown in Panel A of Table 9.

We find that increases in racial resentment are associated with worse employment elasticities for non-Asian people of color two years after minimum wage hikes (columns 1 and 2). The estimates imply that the average state-level increase in racial resentment between 1988-

[46] See Cook et al (2021) for an important historical example of the potential importance of customer discrimination. Alternatively, Small and Pager (2020) review research suggesting that standard corporate HR practices can lead to systematic racial differences in layoff decisions.
[47] The Smith, Kreitzer, and Suo (2019) index of racial resentment is available at https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/J6SEGJ, last accessed 10/15/21.
[48] The state-level indices of racial resentment from Smith et al (2019) are as of: 1988, 1990, 1992, 1994, 2000, 2004, 2008, 2012, and 2016. The selection of time periods is inconsequential to our results. For example, if we move 2004 or both 2004 and 2008 into the earlier period, the correlation coefficient between the state level changes (and the years we currently use) are 0.99 and 0.98, respectively. Likewise, we can shorten the pre-period and return the same results.

2000 and 2004-2016 (of 2.1 points on their index) reduced the estimated employment elasticity by -0.46 (0.23) per year. That is, an increase in racial resentment of this size decreases the two-year after employment elasticity from a statistically insignificant -0.19 (0.21) in a state with no change in racial resentment to -0.64 (0.18) in a state with an average change. For White and Asian American workers, the overall two-year after employment elasticity was -0.16 (0.15) in states that saw no change in racial resentment (columns 3 and 4) – an almost identical effect to the non-Asian people of color sample – but instead experience an average employment elasticity of 0.01 (0.09) in a state with an average increase in racial resentment.

Moreover, we find that elevated employment losses among non-Asian people of color in states with an above average increase in racial resentment is, unsurprisingly, due to differential treatment (see Panel B of Table 9). Non-Asian People of color lose a larger share of highly routinized jobs in states where the increase in racial animus was above average, -1.52 (0.60), relative to below average, -0.69 (0.24), and gain fewer low-routine/highly interpersonal jobs in states with above average changes in racial resentment, -0.06 (0.67), relative to below average states, -0.38 (0.22). White and Asian American workers experience the opposite – larger employment losses at highly routinized jobs and fewer employment gains at highly interpersonal jobs in states with smaller increases in racial resentment. These results suggest that racial discrimination may be impairing the employment opportunities of low-wage non-Asian people of color when automation causes a reallocation in job tasks away from the routine and toward the interpersonal.

V. Robustness

We perform a variety of robustness tests to check the sensitivity of our results to reasonable alternative empirical specifications.

Additional lags and leads

We extend our empirical model to include additional leads (both a two-year and three-year lead in the minimum wage change) and additional lags (a three-year lagged change in the minimum wage). The primary OEWS and ACS estimates are presented in Appendix Tables A9 and A10, respectively. Supportive of the parallel trends assumption, the two-year and three-year lead change coefficients are very similar to the one-year leading change coefficients. Additionally, the coefficients on most of the three-year lagged change in the minimum wage are similar to the two-year lagged change estimates, especially in the OEWS estimates, implying that the impact we document persists three years after as well (see columns 3 and 4 of Appendix Table A9). An exception is the ACS three-year after estimates on the White and Asian American sample (see Appendix Table A10), which are less robust. This is a reflection of weaker identification three years after a minimum wage change, especially in the ACS, and emphasizes why our preferred model, given the data in hand, extends to only two years post-hike.[49] Regardless, the results only reinforce that the employment effects on non-Asian people of color are distinct from White and Asian American people.

Variation in Treatment Timing

Several recent influential studies show that variation in treatment timing can bias treatment effect estimates when early adopters become a part of the control group for later adopters (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2020). As a simple exercise to make sure this is not a concern here, we limit our treatment sample in two ways and reestimate our main empirical specifications on these more-limited sample of treatment states. First, we limit our treatment sample to the 10 states (of the 19 that increased their minimum wage over the 2010-18 period) that increased their minimum wage every year over the 2015-2017 period, while leaving our control group unchanged (i.e. states that did not raise their minimum wage after 2010).[50] Next, we further limit this sample by excluding California, Michigan, Minnesota, and Washington, DC because some local areas in California increased their minimum wage prior to

[49] Very few increases in state minimum wages from three years prior are not also associated with hikes two years prior in our main 2010-18 period. For this reason, we also present estimates in these tables where we change our outcome to be a three-year change in employment, e.g. see column (5) in Appendix Table A9. Related, this lack-of-identification issue causes standard errors to increase when a third lag term is included.

[50] The nine states that increased their minimum wage each year between 2015 and 2018 are Alaska, Arkansas, California, Hawaii, Maryland, Massachusetts, Michigan, Minnesota, South Dakota, and Washington, DC.

2015 and the remaining three states increased their minimum wage during the summer of 2014 (which occurs between the 2014 and 2015 OEWS). These two new sets of estimates, presented in Appendix Table A11, are similar to our main results. For example, the two-year after coefficient for the employment elasticity by routine tasks is -0.22 (0.06) when all states are used and -0.22 (0.07) and -0.23 (0.07) when we limit the treatment states. Additionally, the two-year after effect by interpersonal tasks is 0.24 (0.12) when all states are used and 0.37 (0.11) and 0.53 (0.17) when the treatment states are limited.

Nonlinearities

Lastly, we look for non-linear treatment effects, as in Clemens and Strain (2018). These non-linearities could occur if capital-labor substitution occurs in the long-term, and inflation eats away at the real bite of the minimum wage (Sorkin 2015). We extend our main specification in two ways: a) add the square of the change in the natural log of the minimum wage and b) replace the log minimum wage with indicators for small (below median) and large (above median) hikes. As shown in Appendix Table A12, the quadratic term is insignificant and the point estimates on the small and large hike dummies are proportional to the difference in the average hike between small and large changes. Thus, we find no evidence of a non-linearity. However, we cannot rule out non-linearities associated with even smaller inflation-based adjustments as they are excluded from our empirical analysis.

VI. Conclusion

Using exogenous variation in occupational wages originating from minimum wage hikes, we find strong evidence that automation is changing the composition of jobs in the low-wage labor market, shifting employment from heavily routine occupations towards heavily interpersonal occupations. These dynamics have picked up considerably since the Financial Crisis; the estimated decline in lower paying routine jobs between 2010-2018 has more than doubled relative to the first decade of the 21st century and is spreading to a broader range of routine jobs. Similarly, employment growth in jobs that are intensive in interpersonal tasks has picked up recently but not enough to fully offset the decline in routine employment. Thus, we find some evidence that automation could decrease overall employment in the low-wage labor market. Although we end our analysis prior to the Covid-19 pandemic, this job loss could be even more severe during the pandemic, which may has seen the adoption of automation technology accelerate (Leduc and Liu 2020; Barrero, Bloom, and Davis 2020) and the growth in many interpersonal-intensive jobs slow.

We also explore heterogeneity in this employment realignment across demographic groups including by age, education, sex, and race. While all groups experience a movement away from jobs intensive in routine tasks and towards jobs that are intensive in interpersonal tasks, the overall job loss associated with the automation of lower paying jobs appears to be concentrated among non-Asian people of color, especially Black workers, who experience larger declines in employment at routine jobs and limited gains at interpersonal jobs. The pre-existing occupational distribution of non-Asian people of color cannot explain the magnitudes of these job losses. Instead, we find evidence that these differences are driven by differential treatment, of which increasing racial animosity could be a factor. Understanding the barriers or policies limiting the ability of non-Asian people of color to transition to interpersonal-intensive jobs strikes us as an especially important area of future research.

References

Aaronson, D., Agarwal, S., Hotchkiss, J.L. and Kelley, T., 2019. "Job displacement and financial outcomes." *Economics Letters*, *177*, 18-21.

Aaronson, D., French, E., Sorkin, I. and To, T. 2018. "Industry dynamics and the minimum wage: a putty-clay approach." *International Economic Review*, *59*(1), 51-84.

Aaronson, D. and Phelan, B.J., 2017. "Wage Shocks and the Technological Substitution of Low-wage Jobs." *The Economic Journal*, *129*(617), 1-34.

Aaronson, D. and Phelan, B.J, 2019. "Does automation always lead to a decline in low-wage jobs?," *Chicago Fed Letter*.

Acemoglu, D. and Autor, D, 2011, "Skills, Tasks and Technologies: Implications for Employment and Earnings," *Handbook of Labor Economics*, 4, 1043-1171.

Allegretto, S., Dube, A., Reich, M. and Zipperer, B., 2017. "Credible research designs for minimum wage studies: A response to Neumark, Salas, and Wascher." *ILR Review*, *70*(3), 559-592.

Autor, D., 2019. "*Work of the past, work of the future*." NBER Working Paper.

Autor, D. and Dorn, D., 2013. "The growth of low-skill service jobs and the polarization of the US labor market." *American Economic Review*, *103*(5), 1553-97.

Autor, D.H., Katz, L.F. and Kearney, M.S., 2008. "Trends in us wage inequality: revising the revisionists." *Review of Economics and Statistics*, 90(2), 300–23.

Autor, D.H., Levy, F. and Murnane, R.J., 2003. "The skill content of recent technological change: An empirical exploration." *The Quarterly Journal of Economics*, *118*(4), 1279-1333.

Bar, R. and Zussman, A., 2017. "Customer discrimination: evidence from Israel." *Journal of Labor Economics*, *35*(4), 1031-1059.

Baker, M., Dwayne B., and Stanger, S., "The highs and lows of the minimum wage effect: A time-series cross-section study of the Canadian law," *Journal of Labor Economics,* 17(2), 318-350.

Bailey, M., DiNardo J., and Stuart B., 2020, "The Economic Impact of a High National Minimum Wage: Evidence from the 1966 Fair Labor Standards Act," NBER working paper.

Barrero, J., Bloom N., and Davis S., 2020, "Covid-19 is Also a Reallocation Shock," Working paper.

Beaudry, P., Green, D. and Sand, B. 2016, "The Great Reversal in the Demand for Skill and

Cognitive Tasks," *Journal of Labor Economics*, 34(S1), S199-S247.

Bresnahan, T.F., Brynjolfsson, E. and Hitt, L.M. 2002. "Information technology, workplace organization and the demand for skilled labor: firm-level evidence." *Quarterly Journal of Economics*, 117(1), 339–76.

Callaway, B. and Sant'Anna, P.H. 2020. "Difference-in-differences with Multiple Time Periods." *Journal of Econometrics*, forthcoming.

Cengiz, D., Dube, A., Lindner, A. and Zipperer, B., 2019. "The effect of minimum wages on low-wage jobs." *The Quarterly Journal of Economics*, *134*(3), 1405-1454.

Chen, Y., 2019, "What Do Establishments Do When Wages Increase? Evidence from Minimum Wages in the United States," Working Paper.

Cho, D., 2018. "Downward Wage Rigidity, Corporate Investment, and Firm Value." Working Paper.

Clemens, J. 2021. "How do Firms Respond to Minimum Wage Increases? Understanding the Relevance of Non-employment Margins." *Journal of Economic Perspectives*, *35*(1), 51-72.

Clemens, J., Kahn, L., and Meer, J., 2021, "Dropouts Need Not Apply: The Minimum Wage and Skill Upgrading," *Journal of Labor Economics* 39LS1, S107-S149.

Clemens, J. and Wither, M. 2019. "The Minimum Wage and the Great Recession: Evidence of Effects on the Employment and Income Trajectories of Low-skilled Workers." *Journal of Public Economics*, *170*, 53-67.

Clemens, J. and Strain, M.R. 2018. "The Short-Run Employment Effects of Recent Minimum Wage Changes: Evidence from the American Community Survey." *Contemporary Economic Policy*, *36*(4), 711-722.

Cook, L., M. Jones, T. Logan, and D. Rose, 2021, "Competition and Discrimination in Public Accommodations: Evidence from the Green Books," Working Paper.

Davis, S. and von Wachter, T., 2011, "Recessions and the Cost of Job Loss," *Brookings Papers on Economic Activity*.

Del Río, C. and Alonso-Villar, O., 2015. "The evolution of occupational segregation in the United States, 1940–2010: Gains and losses of gender–race/ethnicity groups." *Demography,* 52(3), 967-988.

Deming, D.J., 2017. "The growing importance of social skills in the labor market." *The Quarterly Journal of Economics*, *132*(4), 1593-1640.

DiNardo, J., Fortin, N.M. and Lemieux, T., 1996. "Labor Market Institutions and the

Distribution of Wages, 1973-1992: A Semiparametric Approach." *Econometrica*, *64*(5), 1001-1044.

Dube, A., Lester, T.W. and Reich, M., 2010. "Minimum wage effects across state borders: Estimates using contiguous counties." *The Review of Economics and Statistics*, *92*(4), 945-964.

Dustmann, C., Lindner, A., Schönberg, U., Umkehrer, M. and Vom Berge, P., 2020. "Reallocation Effects of the Minimum Wage." *CReAM Discussion Paper*.

Geng, H. G., Huang, Y., Lin, C., & Liu, S., 2018. "Minimum Wage and Corporate Investment." Working Paper.

Goodman-Bacon, A., 2021. "Difference-in-differences with Variation in Treatment Timing." *Journal of Econometrics*, forthcoming.

Goos, M., Manning, A. and Salomons, A., 2014. "Explaining job polarization: Routine-biased technological change and offshoring." *American Economic Review*, *104*(8), 2509-26.

Gustafson, M. and Kotter, J.D., 2018. "Minimum wage and corporate policy." Working Paper.

Hau, H., Huang, Y., & Wang, G., 2018. "Firm Response to Competitive Shocks: Evidence from China's Minimum Wage Policy." *Review of Economic Studies*, Forthcoming.

Holzer, H.J. and Ihlanfeldt, K.R., 1998. "Customer discrimination and employment outcomes for minority workers." *The Quarterly Journal of Economics*, *113*(3), 835-867.

Jaimovich, N., I. Eksten, H. Siu, and Y. Yedid-Levi, 2020, "The Macroeconomics of Automation: Data, Theory, and Policy Analysis," Working paper.

Jolly, N.A. and Phelan, B.J., 2015. "Job Displacement's long-run effect on access to employer-provided health insurance and other fringe benefits." *Economics Letters*, *130*, 100-104.

Jacobson, L.S., LaLonde, R.J. and Sullivan, D.G., 1993. "Earnings losses of displaced workers." *American Economic Review*, 685-709.

Krueger, A.B. and Posner, E., 2018. "A Proposal for Protecting Low-Income Workers from Monopsony and Collusion." *The Hamilton Project Policy Proposal*, *5*.

Leduc, S. and Liu, Z., 2020, May. "Can Pandemic-Induced Job Uncertainty Stimulate Automation?" Federal Reserve Bank of San Francisco Working Paper No. 2020-19.

Lordan, G. and Neumark, D., 2018. "People versus machines: The impact of minimum wages on automatable jobs." *Labour Economics*, *52*, 40-53.

Manning, A., 2004. "We can work it out: the impact of technological change on the demand for low-skill Workers." *Scottish Journal of Political Economy*, 51(5), 581–608.

Manning, A., 2020. “Monopsony in labor markets: a review.” *ILR Review*, Forthcoming.

Meer, J. and West, J., 2016, “Effects of the minimum wage on employment dynamics." *Journal of Human Resources,* 51(2), 500-522.

Muro, M., Maxim, R. and Whiton, J., 2019. “Automation and artificial intelligence: How machines are affecting people and places.” Brookings Institution Report, available online at: https://www.brookings.edu/wp-content/uploads/2019/01/2019.01_BrookingsMetro_Automation-AI_Report_Muro-Maxim-Whiton-FINAL-version.pdf#page=29

Neumark, D. and Wascher, W.L., 2008. *Minimum wages*. MIT press.

Neumark, D., Salas, J.I. and Wascher, W., 2014. “Revisiting the Minimum Wage—Employment Debate: Throwing Out the Baby with the Bathwater?” *ILR Review*, 67(3 Suppl), 608-648.

Qiu, Y. and Dai, X., 2019. “Do Increases in State Minimum Wages Accelerate Technology Adoption?” Working Paper.

Pew Research Center, 2018. “In Advanced and Emerging Economies Alike, Worries about Job Automation.” Available at https://www.pewresearch.org/global/2018/09/13/in-advanced-and-emerging-economies-alike-worries-about-job-automation/, last accessed 11/13/19.

Phelan, B., 2019, “Hedonic-Based Labor Supply Substitution and the Ripple Effect of Minimum Wages,” *Journal of Labor Economics* 37(3), 905-948.

Ruhm, C., 1991, “Are Workers Permanently Scarred by Job Displacements?” *American Economic Review*, 81, 319–24.

Small, M. and D. Pager, 2020, “Sociological Perspectives on Racial Discrimation,” Journal of Economic Perspectives 34(2), 49-67.

Smith, C.W., Kreitzer, R.J. and Suo, F. 2020. “The Dynamics of Racial Resentment across the 50 US States.” *Perspectives on Politics*, *18*(2), 527-538.

Solon, G., Haider, S.J. and Wooldridge, J.M., 2015. “What are We Weighting For?.” *Journal of Human Resources*, 50(2), 301-316.

Sorkin, I. 2015. “Are There Long-Run Effects of the Minimum Wage?," *Review of Economic Dynamics*, 18(2), 306-333.

Sullivan, D. and T. von Wachter, 2009, “Job Displacement and Mortality: An Analysis Using Administrative Data,” *The Quarterly Journal of Economics*, 124(3), 1265-1306.

Vaghul, K. and Zipperer, B., 2019, “Historical State and Sub-State Minimum Wages, Through December 31, 2019,” Version 1.2.0, available at https://github.com/benzipperer/historicalminwage/releases.

Table 1: Top 25 Routine and Interpersonal Intensive Low-Wage Occupations

Occupation	Average Wage-to-Minimum Wage	Share Routine	Share Inter-personal	2010 National Employment Levels	Employment Growth 2010-2018
Panel A: Top 25 Routine Intensive Low-Wage Occupations					
Graders and Sorters, Agricultural Products	1.35	57%	25%	32,470	7%
Cutters and Trimmers, Hand	1.74	56%	25%	17,120	-40%
Motion Picture Projectionists	1.45	52%	27%	8,690	-58%
Textile and Garment Pressers	1.32	51%	23%	56,480	-32%
Sewing Machine Operators	1.49	51%	28%	147,040	-7%
Shoe Machine Operators and Tenders	1.63	51%	29%	890	-20%
Gaming and Sports Book Writers and Runners	1.54	51%	31%	12,230	-27%
Textile Weaving Machine Operators	1.81	50%	31%	20,940	-1%
Meat, Poultry, and Fish Cutters and Trimmers	1.56	49%	33%	160,330	-5%
Shoe and Leather Workers and Repairers	1.60	49%	27%	4,820	25%
Cashiers	1.29	48%	38%	3,354,170	8%
Slaughterers and Meat Packers	1.64	48%	31%	86,020	-27%
Gaming Cage Workers	1.63	47%	35%	12,780	17%
Laundry and Dry-Cleaning Workers	1.38	47%	37%	204,790	4%
Maids and Housekeeping Cleaners	1.39	46%	40%	865,980	7%
Gaming Dealers	1.30	46%	35%	73,830	15%
Service Station Attendants	1.41	45%	32%	86,070	30%
Textile Winding Machine Setters	1.79	44%	33%	26,700	12%
Cooks, Institution and Cafeteria	1.58	44%	39%	387,700	3%
Tellers	1.67	44%	39%	556,300	-16%
Gaming Change Persons and Booth Cashiers	1.53	43%	38%	13,910	46%
Painter and Plasterers Helpers	1.67	43%	28%	11,090	-24%
Farmworkers and Laborers	1.23	43%	32%	222,820	28%
Textile Dyeing Machine Operators	1.68	43%	34%	11,580	-27%
Switchboard Operators	1.73	42%	40%	138,180	-49%
Average Routine Intensive Occupation	**1.39**	**47%**	**37%**	**260,517**	**4%**
Panel B: Top 25 Interpersonal Intensive Low-Wage Occupations					
Door-to-Door Salespeople	1.69	1%	79%	5,600	-2%
Residential Advisors	1.69	13%	68%	65,140	66%
Personal and Home Care Aides	1.33	24%	66%	681,430	225%
Recreation Workers	1.59	17%	64%	293,440	21%
Locker Room and Coatroom Attendants	1.41	30%	59%	15,930	10%
Child Care Workers	1.37	18%	58%	611,260	-8%
Tour Guides and Escorts	1.66	17%	55%	28,930	34%
Recreational Protective Service Workers	1.33	31%	54%	117,530	17%
Amusement and Recreation Attendants	1.28	21%	53%	254,670	23%
Bartenders	1.42	27%	52%	495,350	27%
Hosts and Hostesses	1.26	29%	52%	329,030	27%
Manicurists and Pedicurists	1.35	31%	51%	47,430	125%
Funeral Attendants	1.60	26%	50%	29,590	18%
Nonfarm Animal Caretakers	1.43	25%	49%	135,070	48%
Retail Salespersons	1.61	30%	46%	4,155,210	7%
Floral Designers	1.64	25%	46%	47,860	-10%
Bakers	1.59	30%	46%	140,800	28%
Waiters and Waitresses	1.38	35%	46%	2,244,470	15%
Physical Therapist Aides	1.63	34%	45%	45,910	3%
Receptionists and Information Clerks	1.71	36%	44%	997,110	5%
Transportation Attendants, Except Air	1.60	34%	44%	24,030	-6%
Food Concession Attendants	1.24	36%	44%	446,630	6%
Hotel, Motel, and Resort Desk Clerks	1.40	37%	43%	222,550	17%
Food Preparation Workers	1.34	35%	42%	802,630	1%
Nursing Aides and Attendants	1.65	35%	42%	1,473,990	2%
Average Interpersonal Intensive Occupation	**1.50**	**31%**	**48%**	**548,464**	**21%**

Notes: This table presents the top 25 occupations with the highest routine share of tasks and highest interpersonal share of tasks. The table is limited to the lowest paying occupations, which we define to be the occupations that are classified as Wage Group 1 for at least one state. The 2010 employment levels come from the OEWS and represent national totals in the U.S., except Personal Home Care Aides, which excludes California (see text for more details).

Table 2: Employment Effect of a Minimum Wage Hike, by Routine Cognitive Share of Tasks

Occupation Employment and Wage Statistics, 2010-2018

	Specification 1:				**Specification 2:**				**Specification 3:**			
	Wage Group 1	**Wage Group 2**	**Wage Group 3**	**Wage Group 4**	**Wage Group 1**	**Wage Group 2**	**Wage Group 3**	**Wage Group 4**	**Wage Group 1**	**Wage Group 2**	**Wage Group 3**	**Wage Group 4**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ΔMW Next Year	0.19***	-0.09	-0.02	0.13**	0.18***	-0.10	0.01	0.13**				
	(0.07)	(0.06)	(0.12)	(0.05)	(0.06)	(0.06)	(0.11)	(0.05)				
ΔMW This Year	0.06	0.05	-0.04	0.04	0.07	0.05	-0.05	0.04				
	(0.08)	(0.06)	(0.06)	(0.06)	(0.08)	(0.06)	(0.06)	(0.06)				
ΔMW Last Year	0.09*	-0.04	-0.01	0.12**	0.10**	-0.05	-0.03	0.12**				
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)	(0.06)				
ΔMW 2Yrs Ago	0.01	0.02	-0.06	0.00	0.05	0.01	-0.05	-0.01				
	(0.08)	(0.05)	(0.08)	(0.08)	(0.07)	(0.05)	(0.06)	(0.08)				
ΔMW Next Year X RoutineSh					0.00	0.00	-0.07	-0.08	0.01	-0.03	-0.09	-0.07
					(0.08)	(0.08)	(0.10)	(0.04)	(0.08)	(0.09)	(0.11)	(0.04)
ΔMW This Year X RoutineSh					-0.03	0.01	-0.01	0.01	-0.03	0.02	-0.04	0.01
					(0.04)	(0.03)	(0.05)	(0.02)	(0.04)	(0.03)	(0.04)	(0.02)
ΔMW Last Year X RoutineSh					-0.10*	0.04	0.02	0.04	-0.09*	0.03	0.00	0.04
					(0.05)	(0.05)	(0.05)	(0.03)	(0.05)	(0.05)	(0.05)	(0.03)
ΔMW 2Yrs Ago X RoutineSh					-0.22***	0.03	-0.07	0.09	-0.21***	0.04	-0.09	0.08
					(0.06)	(0.05)	(0.08)	(0.05)	(0.07)	(0.05)	(0.08)	(0.06)
State FE and Year FE	**Yes**				**Yes**				**No**			
State-by-Year FE	**No**				**No**				**Yes**			

Notes: This table reports the β_z^k and $\beta_{z,T}^k$ coefficients and standard errors from Equation (1). Specification 1 excludes the ΔMW-X- TaskSh interaction term; Specification 2 is Equation (1); and Specification 3 includes state-by-year fixed effects and includes the non-interacted ΔMW variables. Wage groups 1, 2, 3, and 4 includes occupations with the ratio of the average wages to the minimum wage of 1.0-1.5, 1.5-2.0, 2.0-2.5, and 2.5-6.0, respectively The sample size is $N = 95,781$ for all specifications. * p<0.10, ** p<0.05, and *** p<0.01.

Table 3: Employment Effects by Task Shares
Occupation Employment and Wage Statistics, 2010-2018

	Routine Cognitive	Routine Manual	Overall Routine	Interpersonal	Nonroutine Cognitive	Nonroutine Manual
	(1)	(2)	(3)	(4)	(5)	(6)
Wage Group 1						
ΔMW Next Year X Task Share	0.01	0.03	0.02	-0.09	0.02	0.14
	(0.08)	(0.21)	(0.14)	(0.13)	(0.14)	(0.09)
ΔMW This Year X Task Share	-0.03	-0.10	-0.07	0.03	0.05	0.07
	(0.04)	(0.10)	(0.06)	(0.09)	(0.12)	(0.07)
ΔMW Last Year X Task Share	-0.09*	-0.14*	-0.13**	0.19*	-0.09	-0.02
	(0.05)	(0.07)	(0.06)	(0.10)	(0.09)	(0.08)
ΔMW 2Yrs Ago X Task Share	-0.21***	-0.17***	-0.22***	0.24*	0.01	0.05
	(0.07)	(0.06)	(0.06)	(0.12)	(0.14)	(0.10)
Wage Group 2						
ΔMW Next Year X Task Share	-0.03	0.03	0.01	-0.03	-0.07	0.11**
	(0.09)	(0.07)	(0.08)	(0.06)	(0.08)	(0.05)
ΔMW This Year X Task Share	0.02	-0.08	-0.07	0.00	0.08	0.01
	(0.03)	(0.09)	(0.09)	(0.10)	(0.05)	(0.07)
ΔMW Last Year X Task Share	0.03	0.03	0.06	-0.10	0.00	0.07
	(0.05)	(0.06)	(0.08)	(0.08)	(0.06)	(0.07)
ΔMW 2Yrs Ago X Task Share	0.04	0.05	0.08	-0.12**	0.06	0.02
	(0.05)	(0.03)	(0.07)	(0.05)	(0.08)	(0.05)

Notes: Each column varies by the task share used in the interaction term, ΔMW-X-Task Share. All specifications are otherwise identical to Specification 3 in Table 2. Each column presents the results from a different regression. The results from Wage Group 3 and Wage Group 4 are presented in Appendix Table A2. See the notes to Table 2 for Wage Group definitions. The sample size is $N = 95,781$ for each regression. *p<0.10, **p<0.05, and ***p<0.01.

Table 4: Trends in the Two-Year After Effects in the OEWS by Task Content

		Time Trends			
	Overall Effects	**Linear**	**Computer PPI Level**	**POS PPI Level**	**Average Minimum Wage Level**
	(1)	(2)	(3)	(4)	(5)
Panel A: Routine Cognitive Tasks					
ΔMW 2Yrs Ago X Task Share	-0.11**	-0.04	-0.10**	-0.09**	-0.09
	(0.04)	(0.08)	(0.04)	(0.04)	(0.06)
ΔMW 2Yrs Ago X Task Share X Time Trend		-0.01	-0.02	0.04	-0.05
		(0.01)	(0.05)	(0.05)	(0.05)
Partial R^2 of Time Trend (x1000)		0.003	0.001	0.003	0.010
Panel B: Routine Manual Tasks					
ΔMW 2Yrs Ago X Task Share	-0.06*	0.10	-0.03	-0.03	-0.05
	(0.04)	(0.07)	(0.04)	(0.05)	(0.06)
ΔMW 2Yrs Ago X Task Share X Time Trend		-0.02**	0.11	0.12**	-0.09***
		(0.01)	(0.07)	(0.06)	(0.03)
Partial R^2 of Time Trend (x1000)		0.017	0.022	0.028	0.022
Panel C: All Routine Tasks					
ΔMW 2Yrs Ago X Task Share	-0.11***	0.02	-0.09**	-0.08*	-0.09
	(0.04)	(0.07)	(0.04)	(0.04)	(0.06)
ΔMW 2Yrs Ago X Task Share X Time Trend		-0.01*	0.05	0.09	-0.08**
		(0.01)	(0.06)	(0.06)	(0.04)
Partial R^2 of Time Trend (x1000)		0.012	0.005	0.017	0.022
Panel D: Interpersonal Tasks					
ΔMW 2Yrs Ago X Task Share	0.10	-0.18	0.06	0.04	0.08
	(0.08)	(0.14)	(0.09)	(0.09)	(0.10)
ΔMW 2Yrs Ago X Task Share X Time Trend		0.03***	-0.14*	-0.18**	0.19***
		(0.01)	(0.07)	(0.09)	(0.05)
Partial R^2 of Time Trend (x1000)		0.053	0.033	0.058	0.097

Notes: This table reports the two-year after elasticity of a minimum wage hike on employment at jobs that vary by task content and then how these elasticities vary over time using different variables to capture the time trend. All regressions use the 1999-2019 OEWS data with N=299,321. The partial R^2 of the time trend is presented in basis points and solely refers to the explanatory power of the interaction terms between the two year after effect and the time trend for Wage Group 1 occupations. *p<0.1; **p<0.05; and *** p<0.01

Table 5: MSA-Level Employment Effects by Task Share

Occupation Employment and Wage Statistics, 2010-2018

	MSA-Level Estimates			
	All MSAs	**Limit to Twenty-Five Largest MSAs**	**Exclude Twenty-Five Largest MSAs**	**Rural Areas**
	(1)	(2)	(3)	(4)
Panel A: Overall Employment Effects				
ΔMW Next Year	0.07	0.04	0.07	-0.01
	(0.14)	(0.17)	(0.09)	(1.19)
ΔMW This Year	0.09	0.11	0.02	-0.47
	(0.06)	(0.09)	(0.07)	(0.77)
ΔMW Last Year	0.02	0.01	0.05	-0.69
	(0.09)	(0.14)	(0.06)	(0.77)
ΔMW 2Yrs Ago	0.09	0.19	0.01	-0.59
	(0.07)	(0.13)	(0.06)	(1.01)
Panel B: Employment by Routine Tasks				
ΔMW Next Year X Routine Share	0.02	0.03	0.09	0.35
	(0.07)	(0.09)	(0.06)	(0.35)
ΔMW This Year X Routine Share	-0.05*	-0.03	-0.06	0.03
	(0.03)	(0.04)	(0.05)	(0.25)
ΔMW Last Year X Routine Share	0.03	0.11	-0.06	0.27
	(0.04)	(0.07)	(0.04)	(0.39)
ΔMW 2Yrs Ago X Routine Share	-0.12*	-0.07	-0.17***	-0.66**
	(0.07)	(0.13)	(0.06)	(0.30)
Panel C: Employment by Interpersonal Tasks				
ΔMW Next Year X Interpersonal Share	-0.19	-0.16	-0.16**	-0.30
	(0.12)	(0.13)	(0.08)	(0.46)
ΔMW This Year X Interpersonal Share	-0.05	-0.10	0.08	-0.06
	(0.08)	(0.11)	(0.06)	(0.42)
ΔMW Last Year X Interpersonal Share	0.00	-0.12	0.14**	-0.54
	(0.08)	(0.14)	(0.07)	(0.58)
ΔMW 2Yrs Ago X Interpersonal Share	0.16*	0.08	0.23***	0.85
	(0.08)	(0.17)	(0.08)	(0.53)

Notes: This table reports results using a panel of MSA-occupations. Each column-panel is a separate regression. The results for Wage Groups 2-4 are economically and statistically insignificant and therefore, presented in Appendix Table A4. See the notes for Table 2 for Wage Group definitions. Panel A is from an empirical specification includes MSA and year fixed effects while Panel B and C are from empirical specifications that include MSA-by-year fixed effects. $N =$ for all MSAs; $N = 38,898$ when we limit the sample to the 25 largest MSAs; $N = 286,727$ when we exclude the 25 largest MSAs; and $N = 89,615$ when we limit the sample to rural areas. *p<0.10, **p<0.05, and ***p<0.01.

Table 6: Employment Effects by Task Share

Wage Group 1 Estimates from the American Community Survey, 2010-2018

		Employment Effects by Task Content			
	Overall Employment Effect	Routine Cognitive Tasks	Routine Manual Tasks	All Routine Tasks	Inter-personal Tasks
	(1)	(2)	(3)	(4)	(5)
Panel A: Full Sample					
ΔMW Next Year	0.06	-0.09	-0.07	-0.09	0.09
	(0.10)	(0.09)	(0.10)	(0.10)	(0.10)
ΔMW This Year	0.07	0.01	0.03	0.03	0.00
	(0.10)	(0.08)	(0.10)	(0.07)	(0.11)
ΔMW Last Year	0.06	0.08	0.13	0.12*	-0.13**
	(0.08)	(0.07)	(0.08)	(0.07)	(0.06)
ΔMW 2Yrs Ago	-0.16**	-0.21***	-0.25**	-0.27***	0.32***
	(0.08)	(0.07)	(0.09)	(0.08)	(0.09)
Panel B: Exclude the 25 Largest MSAs					
ΔMW Next Year	0.15	-0.06	0.00	-0.04	0.01
	(0.13)	(0.14)	(0.11)	(0.13)	(0.13)
ΔMW This Year	-0.01	0.01	-0.03	-0.01	0.08
	(0.13)	(0.08)	(0.11)	(0.09)	(0.14)
ΔMW Last Year	0.17	0.11	0.09	0.12	-0.13
	(0.16)	(0.08)	(0.12)	(0.09)	(0.09)
ΔMW 2Yrs Ago	-0.08	-0.27**	-0.26**	-0.31***	0.35***
	(0.09)	(0.10)	(0.12)	(0.11)	(0.10)
Panel C: 25 Largest MSAs Only					
ΔMW Next Year	-0.14	0.15	-0.08	0.05	0.07
	(0.19)	(0.09)	(0.18)	(0.12)	(0.16)
ΔMW This Year	-0.08	-0.09	-0.19	-0.11	0.15
	(0.19)	(0.09)	(0.18)	(0.13)	(0.18)
ΔMW Last Year	-0.04	0.13	0.11	0.18	-0.05
	(0.17)	(0.19)	(0.19)	(0.21)	(0.20)
ΔMW 2Yrs Ago	0.10	-0.12	0.32	-0.01	-0.19
	(0.26)	(0.14)	(0.23)	(0.19)	(0.23)

Notes: This table is based on a panel of occupation-industry-state (Panel A and B) or occupation-industry-MSA (Panel C) employment levels computed from the American Community Survey. Each column-panel is a separate regression. The results for Wage Group 2, 3, and 4 for Panel A and Panel B are presented in Appendix Table A7. See notes for Table 2 for Wage Group definitions. The Panel A, B, and C specifications use $N = 241,011$, $N = 221,584$, and $N = 76,610$ observations, respectively *p<0.10, **p<0.05, and ***p<0.01.

Table 7: Employment Effects by Background Characteristics

American Community Survey, 2010-2018

	By Education		By Age		By Race		By Sex	
	High School or Less	**Some College or More**	**Under Age 30**	**Aged 30+**	**Non-Asian People of Color**	**White & Asian Americans**	**Women**	**Men**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Overall Employment Effect								
ΔMW Next Year	0.05	0.20	0.26	-0.15	0.42*	0.00	0.01	0.25*
	(0.15)	(0.19)	(0.17)	(0.13)	(0.23)	(0.12)	(0.13)	(0.13)
ΔMW This Year	0.11	-0.06	-0.26	0.28**	0.32	0.02	0.12	-0.05
	(0.15)	(0.25)	(0.15)	(0.13)	(0.24)	(0.09)	(0.12)	(0.17)
ΔMW Last Year	0.00	0.12	-0.13	0.29**	0.32**	-0.01	0.00	0.20
	(0.11)	(0.19)	(0.14)	(0.13)	(0.12)	(0.08)	(0.08)	(0.17)
ΔMW 2Yrs Ago	-0.02	-0.12	-0.19	-0.05	-0.64***	0.01	-0.20**	-0.13
	(0.20)	(0.17)	(0.16)	(0.15)	(0.18)	(0.09)	(0.10)	(0.14)
Panel B: Employment Effects by Routine Tasks								
ΔMW Next Year	-0.07	-0.07	0.05	-0.17	0.07	-0.12	-0.10	-0.16
	(0.14)	(0.11)	(0.12)	(0.19)	(0.17)	(0.10)	(0.11)	(0.12)
ΔMW This Year	0.04	-0.06	-0.01	0.00	0.00	-0.02	0.05	0.17
	(0.10)	(0.10)	(0.11)	(0.13)	(0.17)	(0.08)	(0.08)	(0.11)
ΔMW Last Year	-0.02	0.21	-0.04	0.29**	-0.09	0.18**	0.07	0.08
	(0.12)	(0.14)	(0.09)	(0.13)	(0.16)	(0.08)	(0.08)	(0.14)
ΔMW 2Yrs Ago	-0.32***	-0.23*	-0.31***	-0.37***	-0.55***	-0.26***	-0.23***	-0.37*
	(0.11)	(0.13)	(0.10)	(0.13)	(0.18)	(0.10)	(0.08)	(0.19)
Panel C: Employment Effects by Interpersonal Tasks								
ΔMW Next Year	-0.01	0.15	-0.07	0.17	-0.14	0.13	0.14	0.13
	(0.14)	(0.13)	(0.11)	(0.15)	(0.15)	(0.12)	(0.11)	(0.19)
ΔMW This Year	0.07	0.02	-0.01	0.05	0.01	0.06	-0.08	0.04
	(0.11)	(0.14)	(0.10)	(0.14)	(0.18)	(0.11)	(0.10)	(0.21)
ΔMW Last Year	0.07	-0.22*	0.14	-0.30**	0.18	-0.21**	-0.07	-0.10
	(0.11)	(0.12)	(0.09)	(0.14)	(0.17)	(0.09)	(0.08)	(0.13)
ΔMW 2Yrs Ago	0.31**	0.34**	0.34**	0.44***	0.49**	0.34***	0.28***	0.43**
	(0.13)	(0.13)	(0.13)	(0.13)	(0.21)	(0.09)	(0.09)	(0.17)

Notes: This table presents results stratified by education, age, race, and sex from empirical specifications that include state-by-year fixed effects. Each column-panel is a separate regression. The results for Wage Groups 2 and 3 are presented in Appendix Table A8. See the notes of Table 2 for the Wage Group definitions The occupation-industry-state-year employment levels for each subgroup are computed from the sample of individuals in the American Community Survey. The total number of observations in each regression includes: high school or less $N = 126,709$; some college or more $N = 192,863$; under aged 30 $N = 103,103$; aged 30+ $N = 211,750$; non-Asian people of color $N = 80,233$; White and Asian American $N = 220,914$; female $N = 158,522$, and male $N = 151,637$.
* p<0.10, ** p<0.05, and *** p<0.01.

Table 8: Employment Effects by Routine Share at High & Low Routine Share Jobs Wage Group 1 Estimates from the American Community Survey

		Workers by Race	
	All Workers	**Non-Asian People of Color**	**White & Asian Americans**
	(1)	(2)	(3)
ΔMW Next Yr X Routine Share X Above Median	-0.13	0.13	-0.21
	(0.15)	(0.27)	(0.16)
ΔMW This Yr X Routine Share X Above Median	0.14	0.10	0.10
	(0.14)	(0.28)	(0.18)
ΔMW Last Yr X Routine Share X Above Median	0.11	-0.06	0.11
	(0.11)	(0.32)	(0.12)
ΔMW 2Yrs Ago X Routine Share X Above Median	-0.29**	-0.77***	-0.15
	(0.12)	(0.24)	(0.16)
ΔMW Next Yr X Routine Share X Below Median	-0.04	0.07	-0.04
	(0.17)	(0.18)	(0.19)
ΔMW This Yr X Routine Share X Below Median	-0.07	-0.10	-0.12
	(0.12)	(0.13)	(0.15)
ΔMW Last Yr X Routine Share X Below Median	0.15	-0.11	0.25
	(0.10)	(0.27)	(0.11)
ΔMW 2Yrs Ago X Routine Share X Below Median	-0.28***	-0.38*	-0.37***
	(0.09)	(0.21)	(0.10)
N	241,011	80,233	220,914

Notes: This table presents results when the routine share of tasks are interacted with a dummy variable for having either above or below median levels of routine tasks. Negative coefficients on the minimum-wage/routine-share/above-median interaction terms largely reflect jobs losses at highly routinized jobs while negative coefficients on the below-median interaction largely reflect job gains at low-routinized/highly interpersonal jobs. All specifications include state-by-year fixed effects. *p<0.1; **p<0.05; *** p<0.01

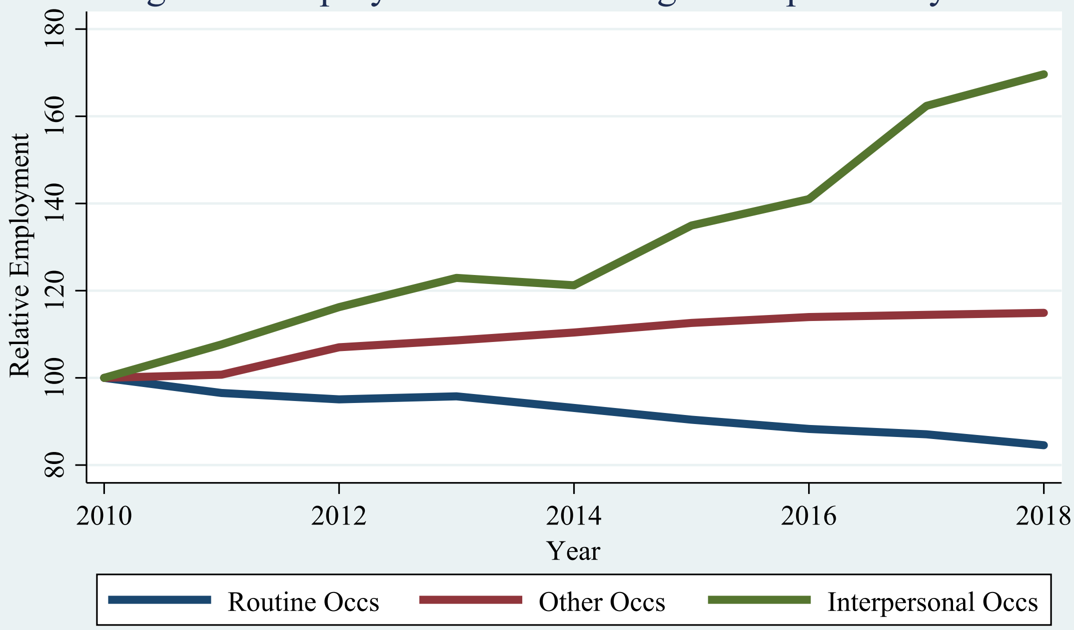
Table 9: Employment Effects of Minimum Wages & Racial Resentment

Wage Group 1 Estimates from the American Community Survey

	Non-Asian People of Color		White & Asian Americans	
	Spec. 1	**Spec. 2**	**Spec. 1**	**Spec. 2**
	(1)	(2)	(3)	(4)
Panel A: Overall Employment Effects				
ΔMW Next Yr	0.39*	-0.08	0.00	-0.05
	(0.23)	(0.29)	(0.12)	(0.22)
ΔMW This Yr	0.32	1.16**	0.03	0.12
	(0.25)	(0.56)	(0.09)	(0.15)
ΔMW Last Yr	0.33*	0.40**	-0.01	-0.17
	(0.12)	(0.18)	(0.08)	(0.18)
ΔMW 2Yrs Ago	-0.64***	-0.19	0.01	-0.16
	(0.18)	(0.21)	(0.09)	(0.15)
ΔMW Next Yr X Δ Racial Resentment		0.23		0.02
		(0.14)		(0.06)
ΔMW This Yr X Δ Racial Resentment		-0.41*		-0.04
		(0.21)		(0.04)
ΔMW Last Yr X Δ Racial Resentment		0.02		0.05
		(0.09)		(0.04)
ΔMW 2Yrs Ago X Δ Racial Resentment		-0.22**		0.06
		(0.11)		(0.04)
N	79,151		219,929	
Panel B: Employment Effects by Routine Share at High & Low Routine Jobs				
	Non-Asian People of Color		**White & Asian Americans**	
	Sample 1	**Sample 2**	**Sample 1**	**Sample 2**
ΔMW Next Yr X Routine Share X Above Median	-0.11	0.07	-0.37	-0.01
	(0.43)	(0.26)	(0.25)	(0.20)
ΔMW This Yr X Routine Share X Above Median	0.29	0.08	0.31	-0.12
	(1.10)	(0.29)	(0.31)	(0.19)
ΔMW Last Yr X Routine Share X Above Median	0.11	0.01	0.17	0.05
	(1.26)	(0.28)	(0.28)	(0.12)
ΔMW 2Yrs Ago X Routine Share X Above Median	-1.52**	-0.60**	-0.08	-0.17
	(0.60)	(0.24)	(0.46)	(0.19)
ΔMW Next Yr X Routine Share X Below Median	0.55	0.00	0.30	-0.35**
	(0.48)	(0.20)	(0.20)	(0.14)
ΔMW This Yr X Routine Share X Below Median	-0.02	-0.06	-0.36*	0.07
	(0.49)	(0.13)	(0.20)	(0.16)
ΔMW Last Yr X Routine Share X Below Median	-0.36	-0.04	0.41*	0.21*
	(0.71)	(0.29)	(0.22)	(0.10)
ΔMW 2Yrs Ago X Routine Share X Below Median	-0.06	-0.38*	-0.42*	-0.23**
	(0.67)	(0.22)	(0.23)	(0.08)
N	24,673	55,560	106,232	114,682

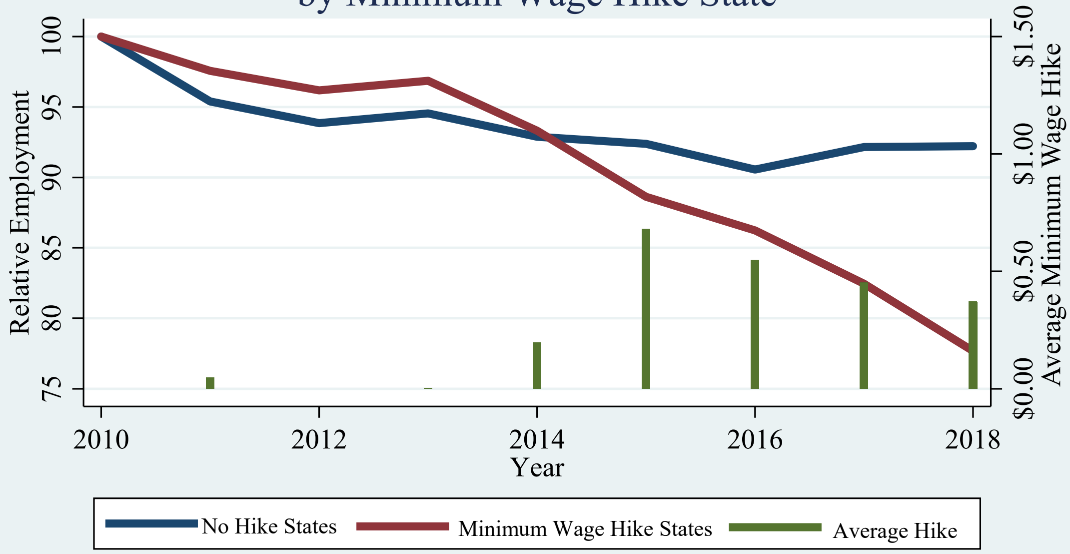
Notes: Panel A presents results of the effect of minimum wages on low-wage employment and how those effects change as racial resentment within a state increases. Panel B presents results on the effect of minimum wage on routine employment at high and low routine jobs, except that we estimate separate effects at states where the increase in racial resentment was above average (Sample 1) and below average (Sample 2). Both sets of results are estimated separately by our racial groupings. The state-level change in racial resentment comes from an index of racial resentment developed in Smith, Kreitzer, and Suo (2019). The change in racial resentment is the difference in the average state-level of racial resentment over the period 2004, 2008, 2012, and 2016 versus the average over the period 1988, 1990, 1992, 1994, 2000. Positive changes in racial resentment reflect increased resentment over time. The analysis excludes Washington, D.C. because there is no measure of racial resentment for it in Smith, Kreitzer, and Suo (2019). *p<0.1; **p<0.05; *** p<0.01

Figure 1: Employment in Low-Wage Occupations by Task



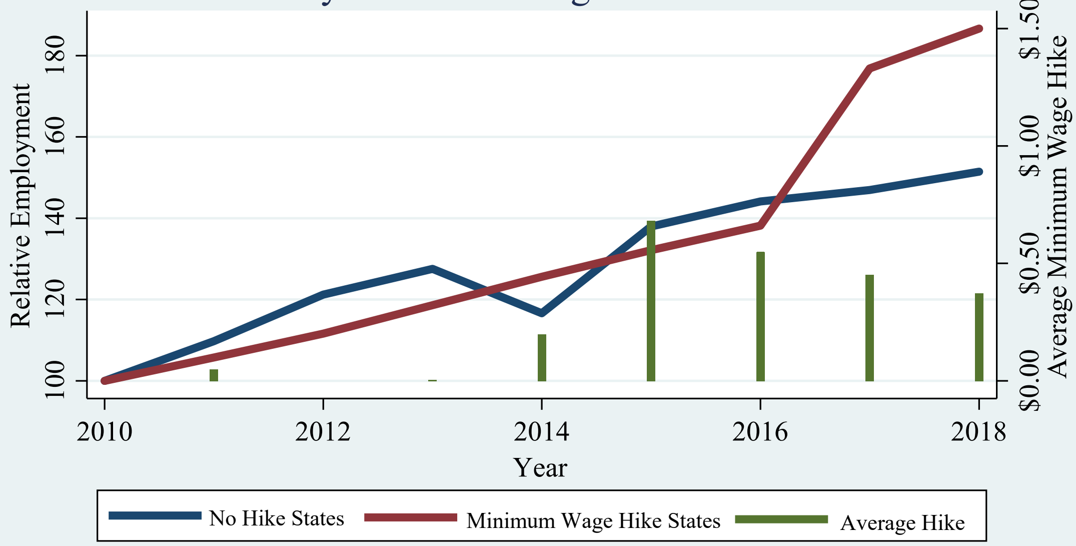
Notes: A low-wage occupation is in Wage Group 1 for at least one state. A routine (interpersonal) occupation has a routine (interpersonal) task share of at least 50 percent. Empoyment levels from Occupation Employment and Wage Statistics, 2010-2018.

Figure 2: Employment in Low-Wage Routine Occupations by Minimum Wage Hike State



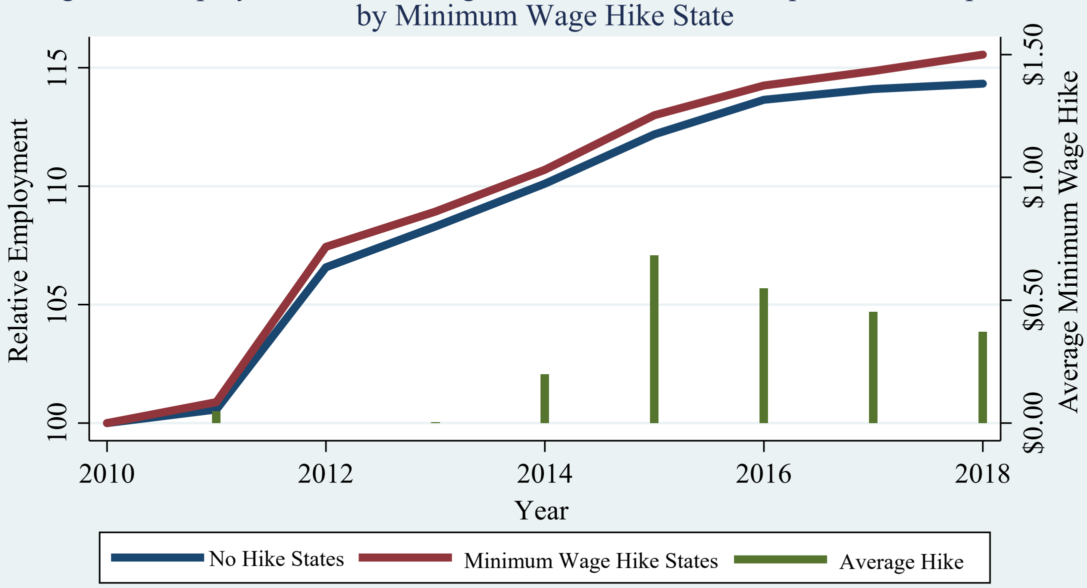
Note: This figure is limited to occupations where routine tasks compose more than 50 percent of the total tasks. The average minimum wage hike is an employment-based weighted average for those states that increased their minimum wage. 17 states increased their minimum wage, separate from automatic inflation-based adjustments, between 2010 and 2018. We exclude 10 states with automatic annual inflation-based adjustments. Employment data from OEWS 2010-2018.

Figure 3: Employment in Low-Wage Interpersonal Occupations by Minimum Wage Hike State



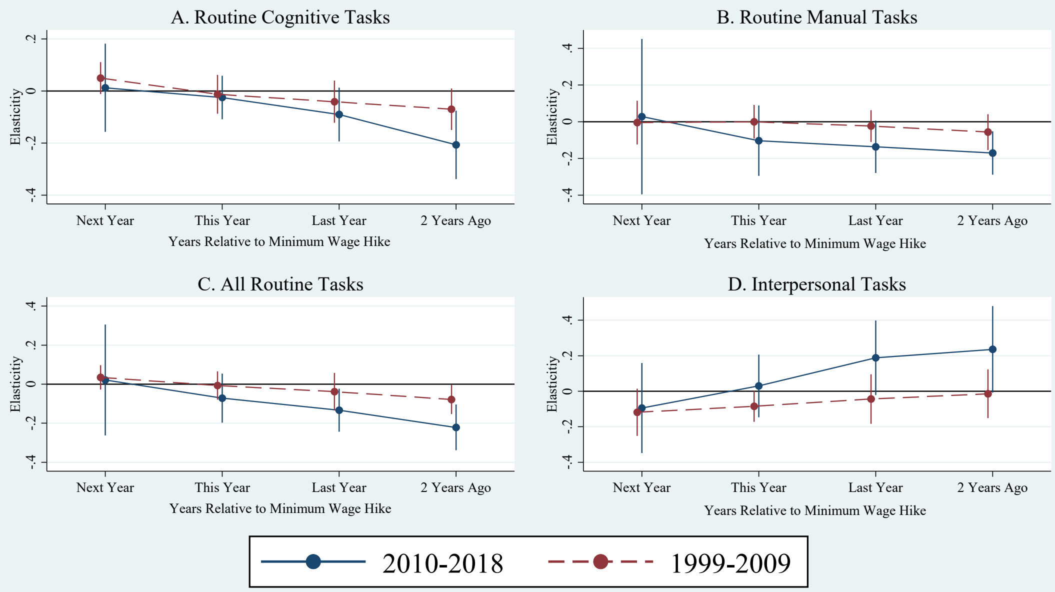
Note: This figure is limited to occupations where interpersonal tasks compose more than 50 percent of the total tasks. The average minimum wage hike is an employment-based weighted average for those states that increased their minimum wage. 17 states increased their minimum wage, separate from automatic inflation-based adjustments, between 2010 and 2018. We exclude 10 states with automatic annual inflation-based adjustments. Employment levels from OEWS 2010-2018.

Figure 4: Employment in Low-Wage Non-Routine/Non-Interpersonal Occupations by Minimum Wage Hike State



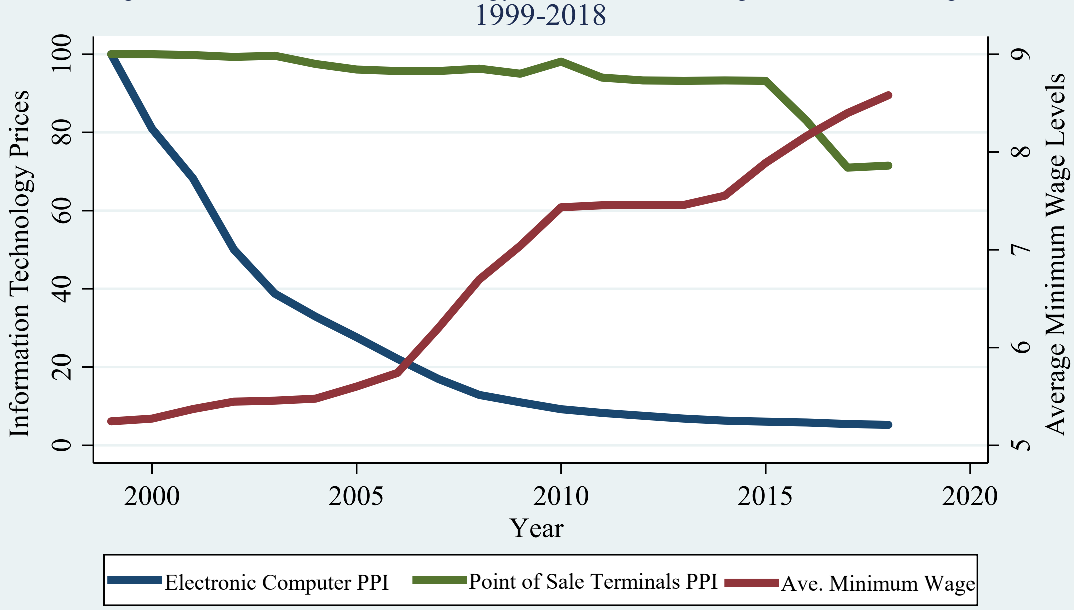
Note: This figure is limited to occupations where neither routine tasks nor interpersonal tasks compose more than 50 percent of the total tasks. The average minimum wage hike is an employment-based weighted average for those states that increased their minimum wage. 17 states increased their minimum wage, separate from automatic inflation- based adjustments, between 2010 and 2018. We exclude 10 states with automatic annual inflation-based adjustments. Employment levels from OEWS 2010-2018.

Figure 5: Effect of Minimum Wage Hikes by Task Intensity
1999-2009 vs. 2010-2018



Notes: This figure presents the estimated elasticity prior to and following a minimum wage hike for Wage Group 1 occupations using 1999-2009 and 2010-2018 OEWS data. The standard error bars capture the 95% confidence interval for each estimated elasticity.

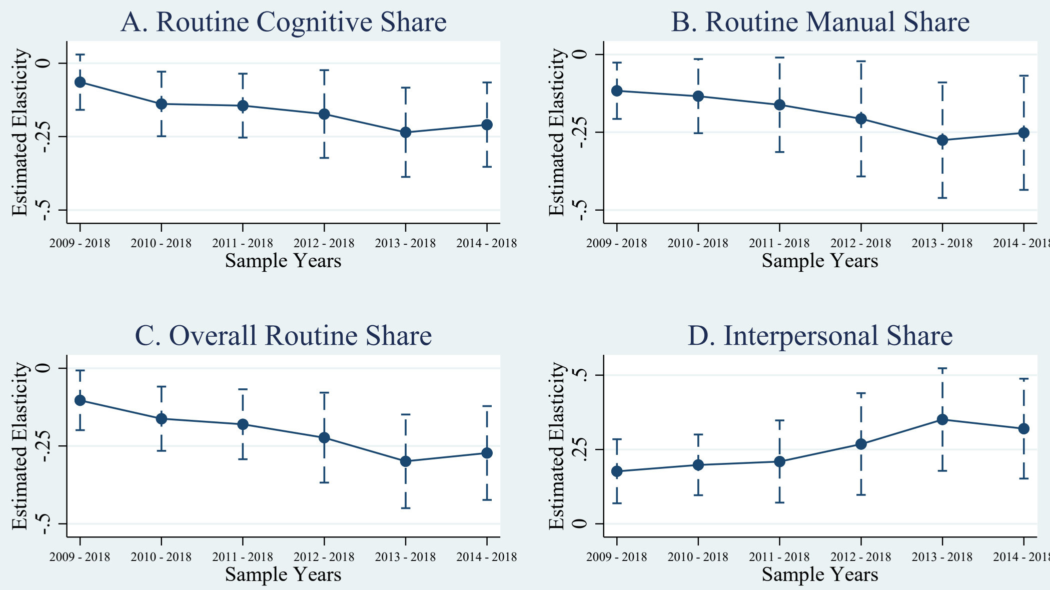
Figure 6: Information Technology Prices and Average Minimum Wages
1999-2018



Notes: The Electronic Computer PPI (WPU1151) and Point of Sale Terminal PPI (WPU115406) are commodity-based price indices and were collected from the Federal Reserve Bank of St. Louis FRED database. The average minimum wage is the weighted average state-level minimum wage as of January of each year.

Figure 7: Employment Effects of Minimum Wage Hikes in the ACS

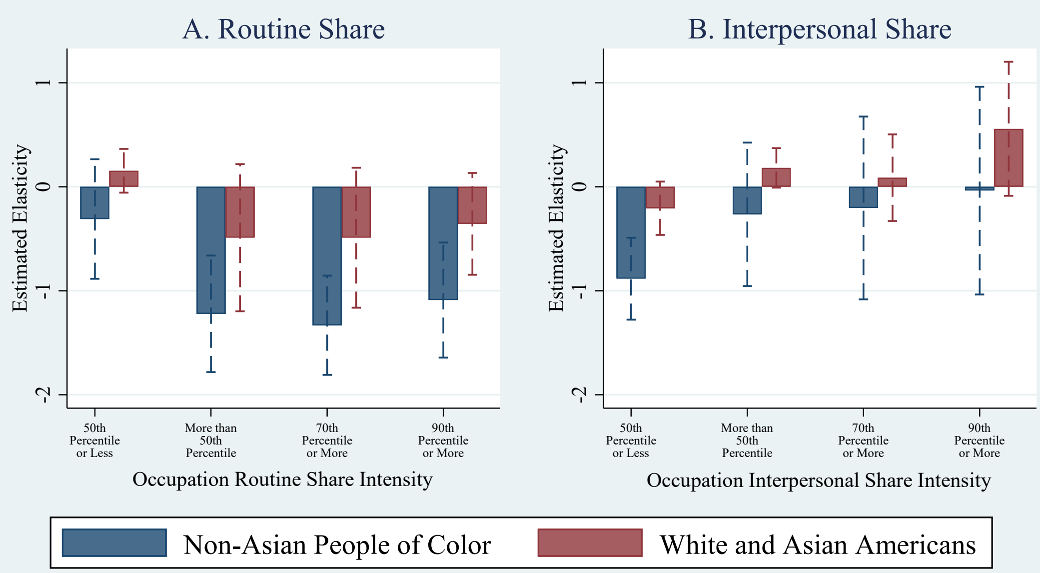
Two-Year After Effects by Task Intensity over Time



Notes: This figure plots to two-year after effects of minimum wage hikes by Task Share as the sample period changes. Sample years reflect years of four-year changes and standard error bars reflect 95% confidence intervals.

Figure 8: Two-Year After Employment Effects of Minimum Wage Hikes

For Low-Wage Occupations in the ACS by Task-Intensity



Notes: These figures plot two-year after elasticities on the overall employment effects of minimum wage hikes as the sample of occupations becomes more routine and more interpersonal. The estimates come from empirical analyses of ACS data 2010-2018 estimated separately by race. The standard error bars reflect 95% confidence intervals.

NOT FOR PUBLICATION

Appendix A: Additional Results

Table A1: State-Level Minimum Wage Changes, 2010-2018

	States														
Year	**AK**	**AZ**	**AR**	**CA**	**CO**	**CT**	**DE**	**DC**	**FL**	**HI**	**IL**	**ME**	**MD**	**MA**	**MI**
2010	\$7.75	\$7.25	\$7.25	\$8.00	\$7.25	\$8.25	\$7.25	\$8.25	\$7.25	\$7.25	\$8.00	\$7.50	\$7.25	\$8.00	\$7.40
2011		\$7.35			\$7.36				\$7.25		\$8.25				
2012		\$7.65			\$7.64				\$7.67						
2013		\$7.80			\$7.78				\$7.79						
2014		\$7.90			\$8.00	\$8.70			\$7.93						
2015	\$8.75	\$8.05	\$7.50	\$9.00	\$8.23	\$9.15	\$7.75	\$9.50	\$8.05	\$7.75			\$8.00	\$9.00	\$8.15
2016	\$9.75	\$8.05	\$8.00	\$10.00	\$8.31	\$9.60	\$8.25	\$10.50	\$8.05	\$8.50			\$8.25	\$10.00	\$8.50
2017	\$9.80	\$10.00	\$8.50	\$10.50	\$9.30	\$10.10		\$11.50	\$8.10	\$9.25		\$9.00	\$8.75	\$11.00	\$8.90
2018	\$9.84	\$10.50	\$8.50	\$11.00	\$10.20			\$12.50	\$8.25	\$10.10		\$10.00	\$9.25		\$9.25
2019	\$9.89	\$11.00	\$9.25	\$12.00	\$11.10		\$8.75	\$13.25	\$8.46			\$11.00	\$10.10	\$12.00	\$9.45
	MN	**MO**	**MT**	**NE**	**NV**	**NJ**	**NY**	**OH**	**OR**	**RI**	**SD**	**VT**	**WA**	**WV**	
2010	\$7.25	\$7.25	\$7.25	\$7.25	\$7.55	\$7.25	\$7.25	\$7.30	\$8.40	\$7.40	\$7.25	\$8.06	\$8.55	\$7.25	
2011			\$7.35		\$8.25			\$7.40	\$8.50			\$8.15	\$8.67		
2012			\$7.65					\$7.70	\$8.80			\$8.46	\$9.04		
2013		\$7.35	\$7.80					\$7.85	\$8.95	\$7.75		\$8.60	\$9.19		
2014		\$7.50	\$7.90			\$8.25	\$8.00	\$7.95	\$9.10	\$8.00		\$8.73	\$9.32		
2015	\$8.00	\$7.65	\$8.05	\$8.00		\$8.38	\$8.75	\$8.10	\$9.25	\$9.00	\$8.50	\$9.15	\$9.47	\$8.00	
2016	\$9.00		\$8.05	\$9.00			\$9.00			\$9.60	\$8.55	\$9.60		\$8.75	
2017	\$9.50	\$7.70	\$8.15			\$8.44	\$9.70	\$8.15	\$9.75		\$8.65	\$10.00	\$11.00		
2018	\$9.65	\$7.85	\$8.30			\$8.60	\$10.40	\$8.30	\$10.25	\$10.10	\$8.85	\$10.50	\$11.50		
2019	\$9.86	\$8.60	\$8.50			\$8.85	\$11.10	\$8.55	\$10.75	\$10.50	\$9.10	\$10.78	\$12.00		

Notes: This table excludes 22 states in which the minimum wages did not change between 2010-2019. The empirical analysis also excludes the following states that had automatic CPI adjustments over most of the period of analysis. These states include: Arizona, Colorado, Connecticut, Florida, Missouri, Montana, Ohio, Oregon, Vermont, and Washington.

Table A2: Employment Effects by Task Shares for Wage Group 3 and 4
Occupation Employment and Wage Statistics, 2010-2018

	Routine Cognitive	Routine Manual	Overall Routine	Interpersonal	Nonroutine Cognitive	Nonroutine Manual
	(1)	(2)	(3)	(4)	(5)	(6)
Wage Group 3						
ΔMW Next Year X Task Share	-0.09	-0.05	-0.17	0.14	0.09	-0.04
	(0.11)	(0.11)	(0.18)	(0.12)	(0.12)	(0.04)
ΔMW This Year X Task Share	-0.04	-0.05	-0.10	0.02	0.13**	0.00
	(0.04)	(0.08)	(0.09)	(0.07)	(0.06)	(0.03)
ΔMW Last Year X Task Share	0.00	-0.04	-0.05	0.04	0.15*	-0.06*
	(0.05)	(0.05)	(0.08)	(0.05)	(0.08)	(0.03)
ΔMW 2Yrs Ago X Task Share	-0.09	0.01	-0.11	0.03	0.06	0.02
	(0.08)	(0.07)	(0.12)	(0.09)	(0.12)	(0.04)
Wage Group 4						
ΔMW Next Year X Task Share	-0.07	-0.20**	-0.18***	0.19***	0.17**	-0.18***
	(0.04)	(0.08)	(0.06)	(0.06)	(0.08)	(0.06)
ΔMW This Year X Task Share	0.01	0.01	0.01	0.00	-0.01	-0.01
	(0.02)	(0.08)	(0.05)	(0.07)	(0.06)	(0.07)
ΔMW Last Year X Task Share	0.04	0.02	0.05	0.00	-0.06*	-0.03
	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
ΔMW 2Yrs Ago X Task Share	0.08	0.10*	0.13**	-0.09**	-0.08	0.05
	(0.06)	(0.05)	(0.05)	(0.04)	(0.05)	(0.06)

Notes: This table shows the Wage Group 3 and 4 coefficients for the regressions presented in Table 3. See Table 3 for more details. *p<0.10, **p<0.05, and ***p<0.01.

Table A3: Employment Effects by Task Shares using the 1999-2009 OEWS

	Overall Employment	Routine Cognitive	Routine Manual	Overall Routine	Interpersonal
	(1)	(2)	(3)	(4)	(5)
Wage Group 1					
ΔMW Next Year	0.07	0.05	0.00	0.04	-0.12*
	(0.06)	(0.03)	(0.06)	(0.03)	(0.07)
ΔMW This Year	0.07	-0.01	0.00	-0.01	-0.09
	(0.06)	(0.04)	(0.05)	(0.04)	(0.04)
ΔMW Last Year	0.10	-0.04	-0.02	-0.04	-0.04
	(0.06)	(0.04)	(0.04)	(0.05)	(0.07)
ΔMW 2Yrs Ago	0.09	-0.07*	-0.06	-0.08**	-0.01
	(0.05)	(0.04)	(0.05)	(0.04)	(0.07)
Wage Group 2					
ΔMW Next Year	0.06	0.04*	-0.05	-0.01	0.00
	(0.05)	(0.03)	(0.03)	(0.04)	(0.05)
ΔMW This Year	0.03	0.04	-0.06*	-0.02	0.02
	(0.06)	(0.03)	(0.03)	(0.04)	(0.04)
ΔMW Last Year	0.04	0.01	-0.05	-0.04	0.02
	(0.08)	(0.03)	(0.04)	(0.05)	(0.05)
ΔMW 2Yrs Ago	-0.01	-0.01	-0.11	-0.12	0.13*
	(0.09)	(0.05)	(0.07)	(0.07)	(0.07)

Notes: This table shows results using the 1999-2009 sample period following the specifications from Table 3. See Table 3 for more details. N=151,948 for the 1999-2009 period. *p<0.10, **p<0.05, and ***p<0.01.

Table A4: MSA-Level Estimates from the OEWS for Wage Group 2-4, 2010-2018

	Overall Employment Effect		Employment Effects by Task Content: Routine Cognitive Tasks		Routine Manual Tasks		Combined Routine Cognitive & Manual Tasks		Interpersonal Tasks	
	All MSAs	Exclude 25 Largest MSAs	All MSAs	Exclude 25 Largest MSAs	All MSAs	Exclude 25 Largest MSAs	All MSAs	Exclude 25 Largest MSAs	All MSAs	Exclude 25 Largest MSAs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Wage Group 2										
ΔMW Next Year	-0.04	-0.06	0.00	-0.11***	0.00	-0.02	0.01	-0.12	-0.02	0.08
	(0.13)	(0.06)	(0.04)	(0.04)	(0.05)	(0.06)	(0.06)	(0.08)	(0.05)	(0.07)
ΔMW This Year	-0.05	-0.02	0.02	-0.03	-0.13**	-0.09*	-0.10*	-0.11*	0.09	0.06
	(0.10)	(0.10)	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)
ΔMW Last Year	-0.05	-0.11	0.06	-0.02	-0.08	-0.09*	-0.01	-0.10**	0.01	0.08
	(0.10)	(0.07)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)
ΔMW 2Yrs Ago	0.05	-0.01	-0.08	-0.11**	0.04	-0.15**	-0.05	-0.24***	-0.04	0.15**
	(0.08)	(0.07)	(0.07)	(0.05)	(0.09)	(0.05)	(0.07)	(0.06)	(0.08)	(0.06)
Wage Group 3										
ΔMW Next Year	-0.02	-0.03	-0.13**	-0.11	-0.11	-0.08	-0.24***	-0.20***	0.12	0.12**
	(0.12)	(0.08)	(0.05)	(0.07)	(0.08)	(0.08)	(0.07)	(0.06)	(0.08)	(0.06)
ΔMW This Year	-0.09	-0.01	-0.04	-0.05	0.00	-0.07	-0.04	-0.12**	-0.04	0.10**
	(0.07)	(0.07)	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
ΔMW Last Year	-0.04	-0.09	-0.04	-0.05	0.05	0.00	-0.01	-0.06	-0.02	0.04
	(0.12)	(0.07)	(0.04)	(0.04)	(0.06)	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)
ΔMW 2Yrs Ago	-0.06	-0.13*	-0.08*	-0.06	0.00	-0.04	-0.09*	-0.12*	0.03	0.07
	(0.08)	(0.07)	(0.04)	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)	(0.06)	(0.06)
Wage Group 4										
ΔMW Next Year	0.02	0.10	0.00	-0.05	-0.16***	-0.07	-0.10***	-0.08**	0.13***	0.06
	(0.13)	(0.08)	(0.03)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
ΔMW This Year	-0.12*	-0.10	-0.05**	-0.05	-0.07**	-0.12***	-0.08***	-0.12***	0.07***	0.11***
	(0.07)	(0.12)	(0.02)	(0.04)	(0.03)	(0.04)	(0.02)	(0.03)	(0.02)	(0.03)
ΔMW Last Year	0.02	-0.02	0.03	0.04	-0.04	-0.13**	-0.01	-0.05	0.03	0.09**
	(0.09)	(0.07)	(0.03)	(0.05)	(0.03)	(0.05)	(0.03)	(0.04)	(0.03)	(0.04)
ΔMW 2Yrs Ago	0.07	-0.07	0.08**	0.09**	0.01	-0.14***	0.06	-0.02	-0.05	0.07
	(0.09)	(0.06)	(0.03)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)

Notes: This table shows the Wage Group 3 and 4 coefficients for the regressions presented in Table 5. See Table 5 for more details. *p<0.10, **p<0.05, and ***p<0.01.

Table A5: **MSA-Level Estimates, Excluding MSAs that Cross State Borders**

		Employment Effects by Task Content			
	Overall Employment Effect	**Routine Cognitive Tasks**	**Routine Manual Tasks**	**All Routine Tasks**	**Inter-personal Tasks**
	(1)	(2)	(3)	(4)	(5)
Wage Group 1					
ΔMW Next Year	-0.09	-0.03	0.07	0.02	-0.10
	(0.10)	(0.06)	(0.09)	(0.09)	(0.12)
ΔMW This Year	0.05	-0.09***	0.01	-0.06*	0.03
	(0.05)	(0.03)	(0.04)	(0.03)	(0.04)
ΔMW Last Year	-0.03	-0.04	0.06*	0.01	0.03
	(0.08)	(0.03)	(0.04)	(0.03)	(0.05)
ΔMW 2Yrs Ago	0.01	-0.15**	-0.05	-0.13*	0.18**
	(0.05)	(0.06)	(0.07)	(0.07)	(0.08)
Wage Group 2					
ΔMW Next Year	-0.21***	-0.02	-0.03	-0.04	-0.01
	(0.08)	(0.06)	(0.06)	(0.07)	(0.06)
ΔMW This Year	-0.09	0.02	-0.12*	-0.09	0.07
	(0.10)	(0.04)	(0.07)	(0.07)	(0.07)
ΔMW Last Year	-0.15***	0.07*	-0.11**	-0.03	0.06
	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)
ΔMW 2Yrs Ago	-0.05	-0.10	0.04	-0.07	-0.03
	(0.06)	(0.08)	(0.10)	(0.08)	(0.08)

Notes: This table presents results analogous to Table 5 when 51 of the 328 metro areas that cross state boundaries are excluded. The sample size is $N = 270,622$ for all of these specification. See Table 5 for more details. *p<0.10, **p<0.05, and ***p<0.01.

Table A6: Employment Effects in the American Community Survey 2010-2018
Employment Summed to Occupation-State-Year Level

	Overall Employment Effect	Routine Cognitive Tasks	Routine Manual Tasks	All Routine Tasks	Inter-personal Tasks
	(1)	(2)	(3)	(4)	(5)
Panel A: Full Sample of Individuals					
ΔMW Next Year	0.14	-0.04	-0.02	-0.03	0.04
	(0.10)	(0.06)	(0.09)	(0.07)	(0.09)
ΔMW This Year	0.16	-0.06	-0.11	-0.08	0.15*
	(0.11)	(0.08)	(0.10)	(0.07)	(0.09)
ΔMW Last Year	0.08	-0.02	-0.02	-0.01	0.06
	(0.08)	(0.06)	(0.09)	(0.05)	(0.10)
ΔMW 2Yrs Ago	-0.07	-0.16*	-0.25**	-0.22**	0.28**
	(0.08)	(0.09)	(0.12)	(0.10)	(0.11)
Panel B: Results Excluding Individuals from 25 Largest MSAs					
ΔMW Next Year	0.15	-0.07	-0.05	-0.06	0.07
	(0.14)	(0.09)	(0.14)	(0.11)	(0.14)
ΔMW This Year	0.13	-0.03	-0.13	-0.07	0.16
	(0.12)	(0.09)	(0.12)	(0.09)	(0.13)
ΔMW Last Year	0.15	0.00	-0.03	-0.01	0.04
	(0.14)	(0.07)	(0.11)	(0.06)	(0.11)
ΔMW 2Yrs Ago	-0.02	-0.22*	-0.29**	-0.27**	0.33***
	(0.09)	(0.11)	(0.13)	(0.11)	(0.12)
Panel C: Results for Individuals Living in the 25 Largest MSAs					
ΔMW Next Year	0.02	-0.01	-0.01	-0.02	-0.03
	(0.19)	(0.22)	(0.19)	(0.18)	(0.20)
ΔMW This Year	0.10	-0.18	-0.25**	-0.20*	0.31**
	(0.26)	(0.17)	(0.10)	(0.11)	(0.13)
ΔMW Last Year	-0.14	-0.01	0.05	0.03	0.02
	(0.26)	(0.06)	(0.14)	(0.07)	(0.15)
ΔMW 2Yrs Ago	0.14	0.06	0.08	0.04	0.02
	(0.21)	(0.12)	(0.28)	(0.16)	(0.22)

Notes: This table removes the industry component of the ACS panel used in Table 6 to be comparable to the OEWS analysis. See Table 6 for more details. The sample sizes are $N = 67,667$, $N = 66,150$, and $N = 18,798$ for Panel A, B, and C, respectively. *p<0.10, **p<0.05, and ***p<0.01.

Table A7: Employment Effects in the ACS, Wage Group 2-4

		Employment Effects by Task Content			
	Overall Employment Effect	Routine Cognitive Tasks	Routine Manual Tasks	All Routine Tasks	Inter-personal Tasks
	(1)	(2)	(3)	(4)	(5)
Panel A: Full Sample of Individuals					
Wage Group 2					
ΔMW Next Year	-0.06	0.00	-0.02	-0.02	0.06
	(0.09)	(0.09)	(0.07)	(0.08)	(0.07)
ΔMW This Year	0.04	-0.02	-0.05	-0.05	0.05
	(0.10)	(0.08)	(0.09)	(0.08)	(0.08)
ΔMW Last Year	-0.01	-0.05	-0.10*	-0.12*	0.16**
	(0.10)	(0.10)	(0.06)	(0.07)	(0.07)
ΔMW 2Yrs Ago	-0.16	0.09	-0.03	0.05	-0.03
	(0.10)	(0.12)	(0.11)	(0.10)	(0.10)
Wage Group 3					
ΔMW Next Year	0.02	0.14**	-0.05	0.06	0.02
	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)
ΔMW This Year	0.10	0.07	0.05	0.06	-0.05
	(0.11)	(0.08)	(0.06)	(0.05)	(0.04)
ΔMW Last Year	-0.08	0.04	-0.06	-0.03	0.04
	(0.08)	(0.07)	(0.05)	(0.07)	(0.07)
ΔMW 2Yrs Ago	-0.20	0.02	-0.11**	-0.02	0.05
	(0.10)	(0.05)	(0.05)	(0.05)	(0.05)
Wage Group 4					
ΔMW Next Year	0.02	-0.03	-0.04	-0.04	0.05
	(0.07)	(0.05)	(0.06)	(0.05)	(0.05)
ΔMW This Year	0.00	0.03	-0.04	0.00	0.01
	(0.07)	(0.04)	(0.05)	(0.03)	(0.04)
ΔMW Last Year	0.02	0.01	0.04	0.04	-0.03
	(0.06)	(0.07)	(0.05)	(0.05)	(0.04)
ΔMW 2Yrs Ago	0.09**	-0.04	-0.02	-0.04	0.02
	(0.04)	(0.05)	(0.06)	(0.05)	(0.04)
Panel B: Excluding Individuals from the 25 Largest MSAs					
Wage Group 2					
ΔMW Next Year	-0.03	0.01	0.04	0.03	-0.01
	(0.13)	(0.11)	(0.07)	(0.09)	(0.07)
ΔMW This Year	0.15	-0.08	-0.11	-0.13**	0.09
	(0.11)	(0.07)	(0.08)	(0.05)	(0.07)
ΔMW Last Year	0.06	-0.18**	-0.06	-0.16*	0.18**
	(0.12)	(0.08)	(0.07)	(0.08)	(0.08)
ΔMW 2Yrs Ago	-0.09	0.20*	0.03	0.16	-0.11
	(0.09)	(0.11)	(0.10)	(0.11)	(0.10)
Wage Group 3					
ΔMW Next Year	0.06	0.08	-0.05	0.01	0.04
	(0.07)	(0.08)	(0.07)	(0.07)	(0.06)
ΔMW This Year	0.15	0.04	0.03	0.03	-0.01
	(0.12)	(0.09)	(0.07)	(0.07)	(0.06)
ΔMW Last Year	-0.03	-0.02	-0.03	-0.04	0.03
	(0.14)	(0.07)	(0.06)	(0.06)	(0.08)
ΔMW 2Yrs Ago	-0.11	0.00	-0.10	-0.04	0.05
	(0.12)	(0.06)	(0.06)	(0.07)	(0.07)
Wage Group 4					
ΔMW Next Year	0.11	-0.05	-0.04	-0.06	0.06
	(0.10)	(0.07)	(0.08)	(0.08)	(0.07)
ΔMW This Year	0.05	0.07*	-0.07	0.00	0.02
	(0.09)	(0.04)	(0.06)	(0.05)	(0.06)
ΔMW Last Year	0.01	-0.01	0.10*	0.05	-0.06
	(0.10)	(0.08)	(0.06)	(0.07)	(0.06)
ΔMW 2Yrs Ago	0.17**	0.00	-0.01	-0.01	0.01
	(0.07)	(0.06)	(0.05)	(0.05)	(0.05)

Notes: This table shows the Wage Group 2, 3, and 4 coefficients for the regressions presented in Table 6. See Table 6 for more details. *p<0.10, **p<0.05, and ***p<0.01.

Table A8: Heterogeneity in Effects within ACS Sample, Wage Group 2-3

	By Education		By Age		By Race		By Sex	
	High School or Less	Some College or More	Under Aged 30	Aged 30+	Non-Asian People of Color	White & Asian Americans	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Overall Employment Effect								
Wage Group 2								
ΔMW Next Year	0.05	-0.07	-0.26	0.02	0.33	-0.11	-0.01	-0.03
	(0.15)	(0.13)	(0.17)	(0.09)	(0.22)	(0.10)	(0.12)	(0.10)
ΔMW This Year	0.11	-0.03	0.15	-0.05	0.22	-0.05	0.03	0.02
	(0.13)	(0.10)	(0.13)	(0.09)	(0.21)	(0.09)	(0.15)	(0.06)
ΔMW Last Year	0.02	0.02	-0.17	0.09	0.25	-0.05	0.09	-0.15
	(0.11)	(0.16)	(0.15)	(0.09)	(0.17)	(0.11)	(0.10)	(0.13)
ΔMW 2Yrs Ago	-0.13	-0.14	-0.24*	-0.19	-0.23	-0.17	-0.12	-0.16
	(0.16)	(0.11)	(0.13)	(0.11)	(0.16)	(0.11)	(0.11)	(0.14)
Wage Group 3								
ΔMW Next Year	0.14	-0.04	0.19	-0.07	0.16	0.00	0.17**	-0.16
	(0.17)	(0.09)	(0.16)	(0.08)	(0.19)	(0.10)	(0.08)	(0.12)
ΔMW This Year	0.20	-0.04	0.05	0.11	0.63**	0.04	0.11	0.14
	(0.12)	(0.14)	(0.24)	(0.10)	(0.28)	(0.09)	(0.11)	(0.13)
ΔMW Last Year	-0.15	0.03	-0.36	-0.01	0.14	-0.07	-0.11	-0.06
	(0.10)	(0.12)	(0.23)	(0.09)	(0.26)	(0.08)	(0.11)	(0.08)
ΔMW 2Yrs Ago	-0.21	-0.27***	0.04	-0.24**	-0.37	-0.19**	-0.19	-0.22**
	(0.16)	(0.08)	(0.15)	(0.11)	(0.35)	(0.08)	(0.15)	(0.09)
Panel B: Employment Effects by Routine Tasks								
Wage Group 2								
ΔMW Next Year	0.05	-0.12	-0.15	0.01	-0.08	-0.03	-0.02	-0.05
	(0.10)	(0.08)	(0.09)	(0.09)	(0.13)	(0.07)	(0.07)	(0.11)
ΔMW This Year	-0.15	-0.02	-0.15**	-0.02	0.12	-0.10	-0.04	-0.03
	(0.09)	(0.12)	(0.07)	(0.11)	(0.12)	(0.07)	(0.10)	(0.11)
ΔMW Last Year	-0.14	-0.13	-0.06	-0.15	-0.19	-0.10	-0.15**	-0.05
	(0.12)	(0.08)	(0.13)	(0.09)	(0.16)	(0.09)	(0.07)	(0.09)
ΔMW 2Yrs Ago	0.01	0.12	0.09	0.00	-0.07	0.03	0.10	-0.24*
	(0.14)	(0.10)	(0.09)	(0.12)	(0.20)	(0.10)	(0.10)	(0.13)
Wage Group 3								
ΔMW Next Year	-0.03	0.12	-0.11	0.08	0.24	0.03	0.12	0.03
	(0.13)	(0.08)	(0.24)	(0.06)	(0.16)	(0.07)	(0.08)	(0.12)
ΔMW This Year	0.08	0.03	0.08	0.08	0.05	0.08	-0.04	0.09
	(0.14)	(0.08)	(0.23)	(0.05)	(0.09)	(0.06)	(0.06)	(0.11)
ΔMW Last Year	0.04	-0.05	0.10	-0.08	-0.04	-0.06	0.07	-0.07
	(0.09)	(0.07)	(0.10)	(0.07)	(0.13)	(0.07)	(0.10)	(0.07)
ΔMW 2Yrs Ago	-0.14	0.01	-0.18	-0.01	-0.36**	0.02	-0.04	-0.08
	(0.14)	(0.08)	(0.19)	(0.07)	(0.16)	(0.06)	(0.08)	(0.06)
Panel C: Employment Effects by Interpersonal Tasks								
Wage Group 2								
ΔMW Next Year	0.09	0.09	0.28***	-0.01	0.16	0.03	-0.04	0.23**
	(0.07)	(0.09)	(0.09)	(0.07)	(0.13)	(0.07)	(0.08)	(0.09)
ΔMW This Year	0.13	0.03	0.09	0.07	0.01	0.08	0.06	0.02
	(0.09)	(0.13)	(0.09)	(0.09)	(0.12)	(0.08)	(0.12)	(0.08)
ΔMW Last Year	0.12	0.16*	0.08	0.13	0.18	0.14	0.14**	0.15
	(0.11)	(0.09)	(0.13)	(0.08)	(0.16)	(0.09)	(0.06)	(0.10)
ΔMW 2Yrs Ago	0.03	-0.11	-0.04	0.04	0.04	0.02	-0.08	0.17
	(0.13)	(0.12)	(0.10)	(0.11)	(0.19)	(0.09)	(0.09)	(0.11)
Wage Group 3								
ΔMW Next Year	0.07	-0.04	0.15	0.01	0.06	0.02	-0.07	0.04
	(0.09)	(0.08)	(0.19)	(0.05)	(0.13)	(0.06)	(0.08)	(0.09)
ΔMW This Year	-0.07	0.02	0.00	-0.05	-0.28	-0.02	0.07	-0.09
	(0.08)	(0.09)	(0.15)	(0.05)	(0.18)	(0.06)	(0.07)	(0.08)
ΔMW Last Year	0.05	-0.01	0.05	0.06	-0.24*	0.11	-0.18*	0.17**
	(0.10)	(0.06)	(0.09)	(0.08)	(0.12)	(0.08)	(0.09)	(0.08)
ΔMW 2Yrs Ago	0.14	-0.01	0.06	0.08	0.40***	0.01	0.05	0.11*
	(0.12)	(0.07)	(0.14)	(0.07)	(0.11)	(0.06)	(0.10)	(0.06)

Notes: This table shows the Wage Group 2 and 3 coefficients for the regressions presented in Table 7. See Table 7 for more details. *p<0.10, **p<0.05, and ***p<0.01.

Table A9: Add Earlier/Later Changes in the Minimum Wage into OEWS Analysis
Wage Group 1 Estimates

	Baseline Estimates	Include 2-Year Lead	Include 3-Year Lead	Include 3-Year Lag	Include New Leads and Lags I	Include New Leads and Lags II	Outcome is 3-Year Change in Ln Employment: 2-Year Lead	Outcome is 3-Year Change in Ln Employment: 3-Year Lead
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Overall Employment Effects								
ΔMW in 3Yrs			0.13			0.11		0.12**
			(0.08)			(0.07)		(0.05)
ΔMW in 2Yrs		0.18***	0.18***		0.23***	0.21***	0.19***	0.16***
		(0.04)	(0.05)		(0.05)	(0.05)	(0.04)	(0.05)
ΔMW Next Year	0.19***	0.17**	0.15**	0.19***	0.15**	0.15**	0.10*	0.10*
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.05)	(0.05)
ΔMW This Year	0.06	0.11	0.11	0.06	0.12*	0.13*	0.12*	0.11*
	(0.08)	(0.08)	(0.07)	(0.08)	(0.07)	(0.06)	(0.06)	(0.06)
ΔMW Last Year	0.09*	0.08	0.07*	0.10**	0.10**	0.09**	0.01	0.02
	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)
ΔMW 2Yrs Ago	0.01	0.06	0.09	-0.01	0.01	0.06	0.06	0.09
	(0.08)	(0.06)	(0.07)	(0.06)	(0.05)	(0.07)	(0.07)	(0.08)
ΔMW 3Yrs Ago				0.06	0.17**	0.13*		
				(0.09)	(0.08)	(0.06)		
Panel B: Employment Effects by Overall Routine Tasks								
ΔMW in 3Yrs X Overall Routine Share			-0.02			0.03		0.05
			(0.11)			(0.10)		(0.07)
ΔMW in 2Yrs X Overall Routine Share		-0.04	-0.04		-0.10	-0.11	0.03	0.01
		(0.07)	(0.08)		(0.07)	(0.08)	(0.09)	(0.10)
ΔMW Next Year X Overall Routine Share	0.02	0.05	0.05	0.06	0.12	0.11	-0.01	-0.02
	(0.14)	(0.13)	(0.14)	(0.14)	(0.14)	(0.14)	(0.07)	(0.07)
ΔMW This Year X Overall Routine Share	-0.07	-0.08	-0.07	-0.06	-0.07	-0.07	-0.15***	-0.16**
	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)
ΔMW Last Year X Overall Routine Share	-0.13**	-0.12**	-0.12**	-0.16***	-0.14***	-0.14***	-0.11**	-0.11**
	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
ΔMW 2Yrs Ago X Overall Routine Share	-0.22***	-0.23***	-0.23***	-0.12**	-0.11**	-0.10	-0.15***	-0.14***
	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)
ΔMW 3Yrs Ago X Overall Routine Share				-0.26***	-0.30***	-0.31***		
				(0.09)	(0.09)	(0.10)		
Panel C: Employment Effects by Interpersonal Tasks								
ΔMW in 3Yrs X Interpersonal Share			-0.06			-0.08		-0.10
			(0.12)			(0.11)		(0.07)
ΔMW in 2Yrs X Interpersonal Share		-0.08	-0.06		-0.05	-0.02	-0.14	-0.09
		(0.10)	(0.10)		(0.10)	(0.10)	(0.10)	(0.10)
ΔMW Next Year X Interpersonal Share	-0.09	-0.05	-0.04	-0.12	-0.09	-0.07	0.04	0.05
	(0.13)	(0.10)	(0.10)	(0.13)	(0.11)	(0.11)	(0.07)	(0.07)
ΔMW This Year X Interpersonal Share	0.03	0.02	0.03	0.01	0.01	0.02	0.09	0.11
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
ΔMW Last Year X Interpersonal Share	0.19*	0.21*	0.22**	0.21**	0.22**	0.23**	0.17*	0.18*
	(0.10)	(0.09)	(0.09)	(0.10)	(0.09)	(0.09)	(0.09)	(0.09)
ΔMW 2Yrs Ago X Interpersonal Share	0.24*	0.23*	0.22*	0.16	0.17	0.14	0.17	0.15
	(0.12)	(0.12)	(0.13)	(0.11)	(0.11)	(0.12)	(0.10)	(0.10)
ΔMW 3Yrs Ago X Interpersonal Share				0.18**	0.17*	0.19**		
				(0.09)	(0.08)	(0.09)		
N	95,781	95,781	95,781	95,781	95,781	95,781	115,678	115,678

Notes: This table reports estimates when the change in the minimum wage from two-years before and three-years after is added to the main empirical specifications in the OEWS, columns (1)-(6), or when the outcome is changed to be the three-year change in the natural log of employment (instead of the four-year change), columns (7)-(8). * p<0.1; ** p<0.05; and *** p<0.01

Table A10: Add Earlier/Later Changes in Minimum Wage into ACS Estimates

Wage Group 1 Estimates by Race

	Non-Asian People of Color						White and Asian Americans					
	Baseline Weighted Estimates	Include 3-Year Lead	Include 3-Year Lag	Include New Leads/Lags: Full Sample	Include New Leads/Lags: Large Occupations	Outcome is 3Yr Change in Ln Emp	Baseline Weighted Estimates	Include 3-Year Lead	Include 3-Year Lag	Include New Leads/Lags: Full Sample	Include New Leads/Lags: Large Occupations	Outcome is 3Yr Change in Ln Emp
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Overall Employment Effects												
ΔMW in 3Yrs		0.06		0.06	-0.05	-0.02		-0.08		-0.08	-0.08	-0.01
		(0.11)		(0.11)	(0.09)	(0.13)		(0.09)		(0.09)	(0.09)	(0.06)
ΔMW in 2Yrs		-0.31*		-0.33*	-0.30	-0.05		0.18*		0.17	0.13	-0.01
		(0.17)		(0.17)	(0.21)	(0.17)		(0.10)		(0.10)	(0.09)	(0.07)
ΔMW Next Year	0.42*	0.25	0.38	0.17	0.05	-0.50**	0.00	0.00	-0.01	-0.01	0.08	0.01
	(0.23)	(0.24)	(0.23)	(0.24)	(0.34)	(0.22)	(0.12)	(0.12)	(0.12)	(0.12)	(0.18)	(0.10)
ΔMW This Year	0.32	0.17	0.26	0.07	0.35	0.16	0.02	0.05	0.00	0.03	0.22	0.08
	(0.24)	(0.24)	(0.26)	(0.26)	(0.28)	(0.18)	(0.09)	(0.09)	(0.09)	(0.09)	(0.13)	(0.11)
ΔMW Last Year	0.32**	0.29**	0.33**	0.28**	-0.11	-0.18	-0.01	-0.03	-0.02	-0.03	0.01	-0.01
	(0.12)	(0.14)	(0.12)	(0.14)	(0.25)	(0.19)	(0.08)	(0.08)	(0.08)	(0.08)	(0.16)	(0.09)
ΔMW 2Yrs Ago	-0.64***	-0.78***	-0.53***	-0.66**	-0.30	-0.52*	0.01	0.05	0.03	0.07	-0.07	-0.06
	(0.18)	(0.17)	(0.20)	(0.17)	(0.19)	(0.30)	(0.09)	(0.10)	(0.10)	(0.10)	(0.16)	(0.13)
ΔMW 3Yrs Ago			-0.39	-0.47	-0.94**				-0.09	-0.08	0.04	
			(0.43)	(0.41)	(0.45)				(0.17)	(0.16)	(0.20)	
Panel B: Employment Effects by Overall Routine Tasks												
ΔMW in 3Yrs X Routine Share		0.00		0.01	0.05	-0.01		-0.12		-0.12	-0.04	-0.17***
		(0.13)		(0.13)	(0.09)	(0.09)		(0.10)		(0.10)	(0.13)	(0.06)
ΔMW in 2Yrs X Routine Share		-0.23**		-0.24**	-0.06	0.08		-0.04		-0.04	-0.04	0.10
		(0.11)		(0.11)	(0.11)	(0.12)		(0.11)		(0.11)	(0.13)	(0.06)
ΔMW Next Year X Routine Share	0.07	0.13	0.07	0.12	-0.04	0.10	-0.12	-0.07	-0.12	-0.07	-0.13	0.00
	(0.17)	(0.20)	(0.18)	(0.20)	(0.21)	(0.18)	(0.10)	(0.12)	(0.11)	(0.12)	(0.14)	(0.09)
ΔMW This Year X Routine Share	0.00	0.06	0.01	0.06	-0.02	-0.11	-0.02	0.03	-0.02	0.03	0.11	0.03
	(0.17)	(0.21)	(0.17)	(0.22)	(0.19)	(0.13)	(0.08)	(0.08)	(0.08)	(0.08)	(0.11)	(0.11)
ΔMW Last Year X Routine Share	-0.09	-0.01	-0.10	-0.01	-0.09	-0.10	0.18**	0.17**	0.18**	0.17**	0.18**	-0.14
	(0.16)	(0.17)	(0.15)	(0.16)	(0.19)	(0.20)	(0.08)	(0.08)	(0.08)	(0.08)	(0.09)	(0.09)
ΔMW 2Yrs Ago X Routine Share	-0.55***	-0.60***	-0.57***	-0.62***	-0.41**	-0.57***	-0.26***	-0.26***	-0.26**	-0.25*	-0.42**	0.12
	(0.18)	(0.17)	(0.19)	(0.19)	(0.20)	(0.16)	(0.10)	(0.10)	(0.12)	(0.13)	(0.16)	(0.10)
ΔMW 3Yrs Ago X Routine Share			0.05	0.06	-0.40				-0.01	-0.02	0.04	
			(0.23)	(0.23)	(0.24)				(0.16)	(0.15)	(0.22)	
Panel C: Employment Effects by Interpersonal Tasks												
ΔMW in 3Yrs X Interpersonal Share		0.11		0.11	0.11	0.06		0.19**		0.21**	0.08	0.18**
		(0.19)		(0.20)	(0.20)	(0.13)		(0.09)		(0.10)	(0.12)	(0.07)
ΔMW in 2Yrs X Interpersonal Share		0.28**		0.28**	0.13	0.01		-0.05		-0.06	-0.07	-0.09
		(0.11)		(0.11)	(0.13)	(0.12)		(0.10)		(0.11)	(0.14)	(0.06)
ΔMW Next Year X Interpersonal Share	-0.14	-0.22	-0.14	-0.23	-0.14	-0.22	0.13	0.09	0.13	0.07	0.22*	-0.03
	(0.15)	(0.19)	(0.16)	(0.19)	(0.23)	(0.22)	(0.12)	(0.13)	(0.13)	(0.13)	(0.12)	(0.10)
ΔMW This Year X Interpersonal Share	0.01	-0.14	0.01	-0.14	-0.04	0.10	0.06	0.00	0.07	0.01	-0.15	-0.06
	(0.18)	(0.26)	(0.18)	(0.26)	(0.21)	(0.15)	(0.11)	(0.11)	(0.11)	(0.11)	(0.13)	(0.13)
ΔMW Last Year X Interpersonal Share	0.18	0.10	0.18	0.10	0.20	0.15	-0.21**	-0.14	-0.23**	-0.16	-0.09	0.21**
	(0.17)	(0.21)	(0.18)	(0.21)	(0.21)	(0.18)	(0.09)	(0.11)	(0.10)	(0.11)	(0.10)	(0.09)
ΔMW 2Yrs Ago X Interpersonal Share	0.49**	0.59***	0.49**	0.58***	0.26	0.56**	0.34***	0.29**	0.29**	0.21	0.41***	-0.05
	(0.21)	(0.21)	(0.20)	(0.21)	(0.28)	(0.21)	(0.09)	(0.12)	(0.12)	(0.14)	(0.15)	(0.09)
ΔMW 3Yrs Ago X Interpersonal Share			0.01	0.03	0.70*				0.14	0.22	0.10	
			(0.27)	(0.31)	(0.36)				(0.19)	(0.17)	(0.24)	
N	80,233	80,233	80233	80,233	17,078	97,018	220,914	220,914	220,914	220,914	17,667	267,573

Notes: This table reports estimates when the two-year lead and three-year lagged change in the minimum wage is added to the main empirical specifications in the ACS or when the outcome is changed to be the three-year change in the natural log of employment (instead of the four-year change). “Large Occupations” are ones with employment levels (at the occupation-industry-state-year level) greater than 50. *p<0.1; **p<0.05; and *** p<0.01

Table A11: Limiting the Treatment Sample to States that Increased Minimum Wages Each Year 2015-2017
Wage Group 1 Estimates from OEWS

	Routine Cognitive Tasks		Routine Manual Tasks		Overall Routine Tasks		Interpersonal Tasks	
	Limited Treatment States 1	**Limited Treatment States 2**	**Limited Treatment States 1**	**Limited Treatment States 2**	**Limited Treatment States 1**	**Limited Treatment States 2**	**Limited Treatment States 1**	**Limited Treatment States 2**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔMW Next Yr X Task Share	0.01	-0.02	0.14	-0.15	0.07	-0.08	-0.16	-0.08
	(0.11)	(0.16)	(0.19)	(0.24)	(0.15)	(0.21)	(0.13)	(0.17)
ΔMW This Yr X Task Share	-0.11**	-0.06	-0.01	-0.09	-0.07	-0.08	0.16*	0.01
	(0.04)	(0.07)	(0.05)	(0.13)	(0.04)	(0.10)	(0.09)	(0.11)
ΔMW Last Yr X Task Share	-0.13*	-0.10	-0.20	-0.30***	-0.19**	-0.23***	0.32**	0.41**
	(0.07)	(0.09)	(0.12)	(0.08)	(0.09)	(0.08)	(0.15)	(0.17)
ΔMW 2Yrs Ago X Task Share	-0.22***	-0.16***	-0.15*	-0.23*	-0.22***	-0.23***	0.37***	0.53***
	(0.06)	(0.05)	(0.08)	(0.12)	(0.07)	(0.07)	(0.11)	(0.17)
N	75,808	66,293	75,808	66,293	75,808	66,293	75,808	66,293

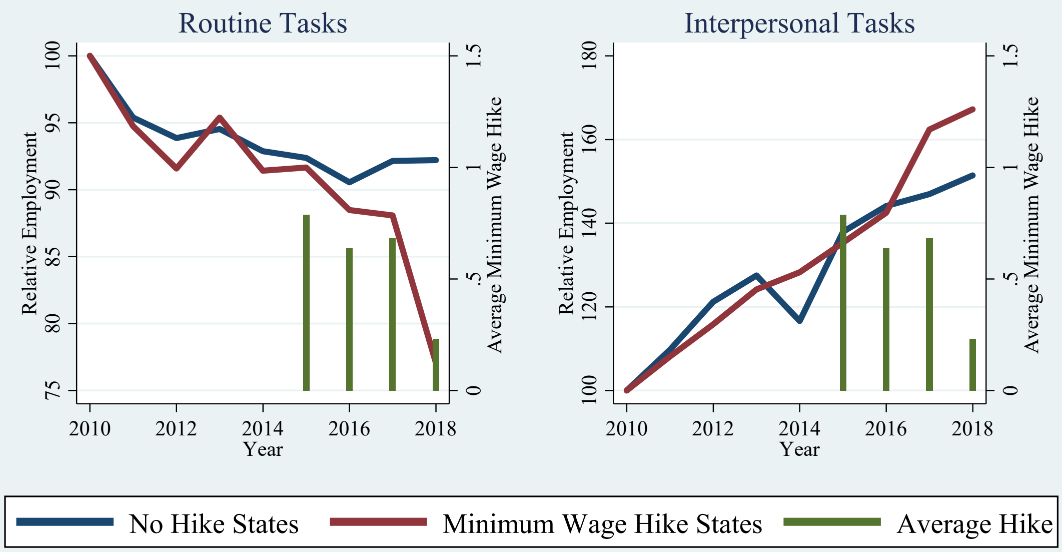
Notes: This table presents the estimates from specifications using the OEWS that limits the treatment sample to those states that did not increase their minimum wage until 2015 and the increased it each year from 2015-2017 (though some also increased in 2018). These "Limited Treatment States 1" include: Alaska, Arkansas, California, Hawaii, Maryland, Michigan, Minnesota, New Jersey, South Dakota, and Washington D.C. "Limited Treatment States 2" further excludes Michigan, Minnesota, and Washington, DC because while they increased their minimum wage between the May 2014 and May 2015 OEWS, they actually increased their minimum wage in summer 2014. It also excludes California because localities in California increased their minimum wage prior to 2015. *p<0.1; **p<0.05; *** p<0.01

Table A12: Non-Linear Effects of Minimum Wage Hikes in the OEWS

	Baseline Model	Include Squared ΔMW	Use Indicators For Small/ Large Hikes
	(1)	(2)	(3)
Panel A: Routine Cognitive Tasks			
ΔMW 2Yrs Ago X Task Share	-0.21***	-0.17	
	(0.07)	(0.30)	
ΔMW 2Yrs Ago Squared X Task Share		-0.01	
		(0.03)	
Small ΔMW 2 Yrs Ago X Task Share			-0.01
			(0.01)
Large ΔMW 2 Yrs Ago X Task Share			-0.02**
			(0.01)
Panel B: Routine Manual Tasks			
ΔMW 2Yrs Ago X Task Share	-0.17***	-1.44*	
	(0.06)	(0.80)	
ΔMW 2Yrs Ago Squared X Task Share		0.12	
		(0.07)	
Small ΔMW 2 Yrs Ago X Task Share			-0.06*
			(0.03)
Large ΔMW 2 Yrs Ago X Task Share			-0.04**
			(0.02)
Panel C: All Routine Tasks			
ΔMW 2Yrs Ago X Task Share	-0.22***	-0.90	
	(0.06)	(0.55)	
ΔMW 2Yrs Ago Squared X Task Share		0.06	
		(0.05)	
Small ΔMW 2 Yrs Ago X Task Share			-0.04*
			(0.02)
Large ΔMW 2 Yrs Ago X Task Share			-0.03***
			(0.01)
Panel D: Interpersonal Tasks			
ΔMW 2Yrs Ago X Task Share	0.24*	0.03	
	(0.12)	(0.47)	
ΔMW 2Yrs Ago Squared X Task Share		0.03	
		(0.04)	
Small ΔMW 2 Yrs Ago X Task Share			0.01
			(0.02)
Large ΔMW 2 Yrs Ago X Task Share			0.03*
			(0.02)

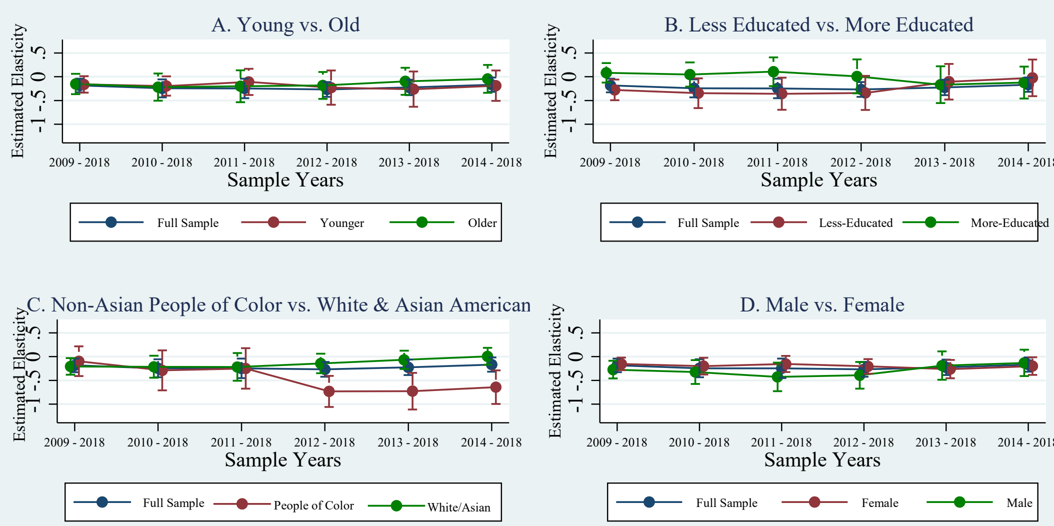
Notes: This table compares estimates when minimum wage hikes are included linearly with two non-linear specifications: including a quadratic in the change in the minimum wage (Column 2) and including two indicators for a "small" and "large" hike (Column 3), where a small (large) hike is one smaller (larger) than the median change over the period of analysis, i.e. 0.07 log points. *p<0.1; **p<0.05; and *** p<0.01

Figure A1: Employment in Low-Wage Routine and Interpersonal Occupations
Limited Minimum Wage Hike States



Note: This figure is limited to occupations where tasks intensity is more than 50 percent of the total tasks (either routine or interpersonal). The minimum wage hike states are limited to those states that did not increase their minimum wage until 2015 and then increased it every year 2015-2017 (some also increased in 2018). We further exclude California because several localities in CA had increased their minimum wages prior to 2015. The average minimum wage hike is a employment-based weighted average for those states

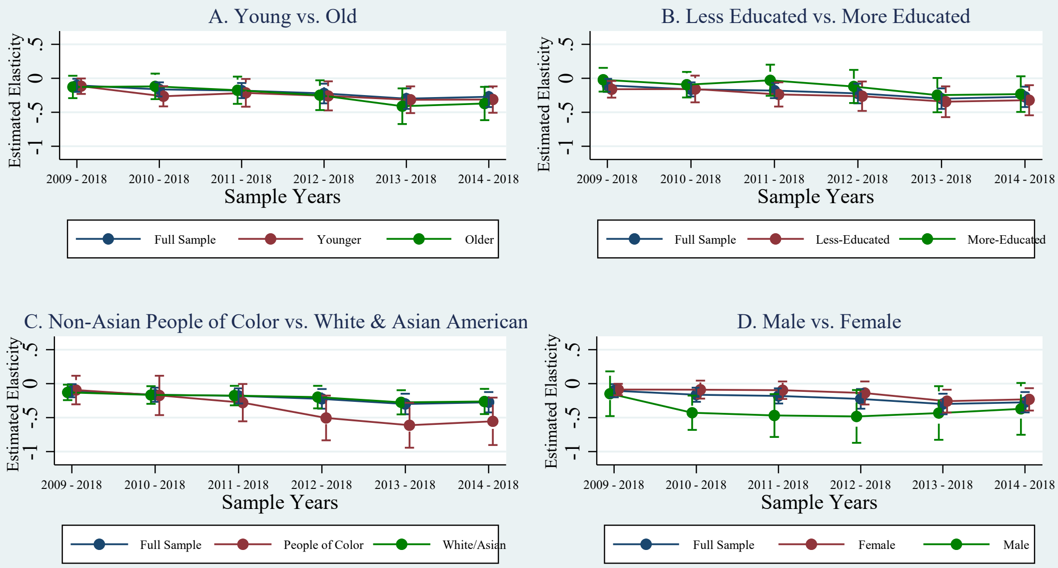
Figure A2: Overall Employment Effects of Minimum Wage
Two-Year After Effect - By Demographic Groups Over Time



Notes: This figure presents the two-year after effects of the overall employment effects of minimum wage hikes on specific demographic groups over time. Young workers are less than 30. Less-educated workers have a high school diploma or less. Non-Asian people of color include all non-white/non-Asian American workers. The sample years are sample years of four-year employment changes and the standard error bars reflect 95% confidence intervals.

Figure A3: Employment Effects of Minimum Wage by Routine Intensity

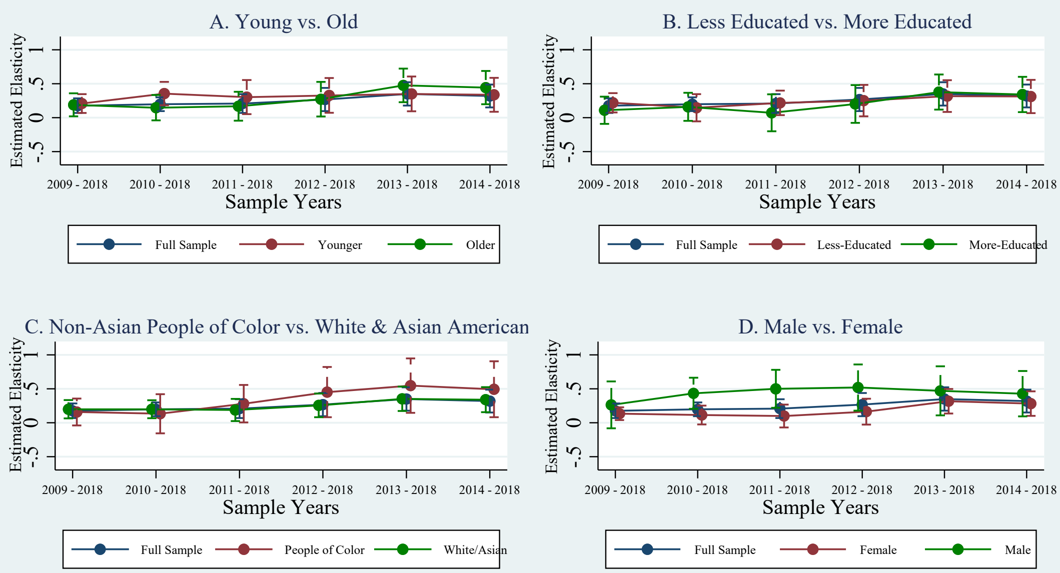
Two-Year After Effect - By Demographic Groups Over Time



Notes: This figure presents the two-year after effect of minimum wage hikes on employment as the routine share of employment increases. See the notes to Figure A3 for the definitions of the demographic groups. Standard error bars reflect 95% confidence intervals and sample years reflect years of year-year changes in employment.

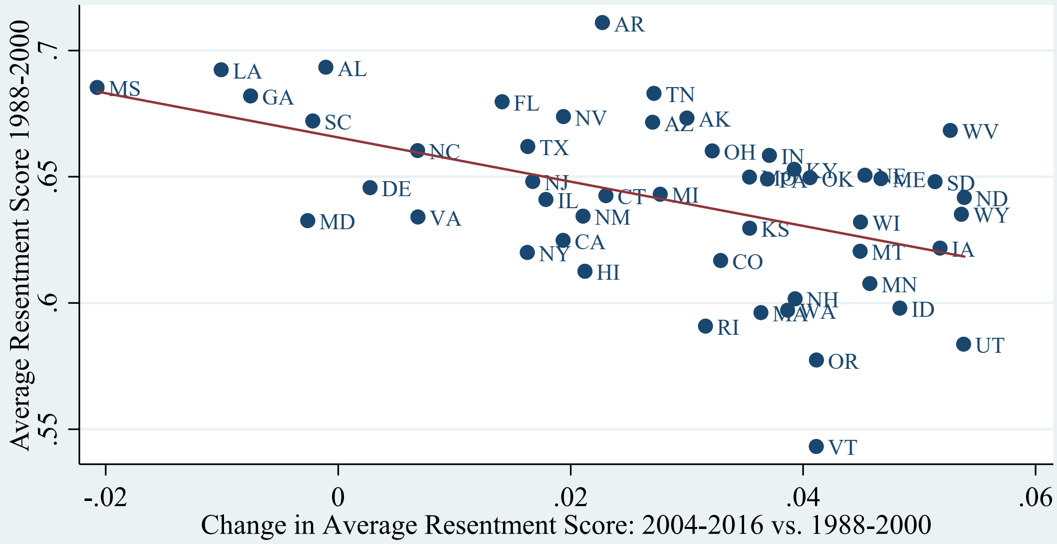
Figure A4: Employment Effects of Minimum Wage by Interpersonal Intensity

Two-Year After Effect - By Demographic Groups Over Time



Notes: This figure presents the two-year after effect of minimum wage hikes on employment as the interpersonal share of employment increases. See the notes to Figure A3 for the definitions of the demographic groups. Standard error bars reflect 95% confidence intervals and sample years reflect years of year-year changes in employment.

Figure A5: Changes in Racial Resentment by State Over Time
2004-2016 vs. 1988-2000



Note: This figure plots the average racial resentment score by state over the period 1988-2000 (measured as the average score in 1988, 1990, 1992, 1994, and 2000) against the change in the average resentment score since 2000, which uses state-level measures of racial resenting from 2004, 2008, 2012, and 2016. Positive changes reflect an increase in racial resentment. All state-level racial resentment values are from Smith, Kreitzer, and Suo (2019).

NOT FOR PUBLICATION

Appendix B: Sample Weights

Our empirical analyses utilize employment-based weights at the occupation-state employment level in a base year. Solon, Haider, and Wooldridge (2015) (hereafter referred to as SHW), state that two primary reasons to use weights in a regression analysis are a) heteroscedasticity associated with occupation size and b) heterogeneity associated with occupation size. We find that both of these justifications apply to our analysis.

Heteroskedasticity. Following SHW, we use the modified Breusch-Pagan test of Wooldridge (2013) which regresses the squared residual on the inverse of the occupation-state year base employment level. We consistently find highly statistically significant coefficients in both the OEWS and ACS, across all task types, and for both the worker of color and White and Asian American samples in the ACS. Other recent studies that found similar heteroskedasticity also use weights, e.g. Buchmueller and Carey (2018), Goodman-Bacon (2018), and Gavrilova, Kamada, and Zoutman (2019).

Heterogeneity by Occupation Size. In the presence of heterogeneity by size, SHW argue that weighted estimates will be closer to the average partial effect (see also Chalfin and McCrary (2018), and Chodorow-Reich, Gopinath, Mishra, and Narayanan (2020)). In our context, it is plausible that larger occupations will be more susceptible to automation. One practical reason is that capital equipment suppliers are more likely to target bigger occupations, at least initially, when automation technology requires customization. Related to size issues, the usual measurement error concerns with smaller states and occupations are alleviated with weighting. Indeed, we find heterogeneity by occupation size with larger highly-routinized occupations experiencing larger employment declines than similarly routinized smaller occupations.

Despite this support for using weights in our analysis, we present unweighted estimates for curious readers in Appendix Table B1. First, note that the weighted and unweighted estimates are broadly similar in several outcomes (overall employment, routine cognitive tasks, and overall routine tasks), although the standard errors of the unweighted coefficients are roughly double the size of the comparable standard errors in the weighted regressions. That said, the unweighted estimates are much weaker for routine manual and interpersonal tasks. As we argued already, one culprit driving this difference is heteroskedasticity. But we also find strong evidence of heterogeneity in the effect of minimum wages by occupation size. When we split the sample by employment levels – those with occupation-state-year base employment levels greater than or less than 10,000 employees – the employment response at larger occupations strongly reflects the baseline weighted estimates (column 4). There does not appear to be as robust of an employment response at smaller occupations, beyond routine cognitive tasks (column 5). Moreover, for our context, the larger occupations are more common among Wage Group 1 occupations, accounting for a quarter of all state-occupation-year observations but 80 percent of all employment.

We also show how weights impact the ACS results, by racial grouping (see Appendix Table B2). Interestingly, the difference between weighted and unweighted estimates arises among White and Asian American workers. Again, we find precision declines notably without weights in both groups. Both heteroskedacity and heterogeneity by occupation size are evident. Therefore, we find the weighted estimates to be more compelling.

Appendix References

Buchmueller, T.C., and Carey, C, 2018, "The effect of prescription drug monitoring programs on opioid utilization in Medicare." *American Economic Journal: Economic Policy,* 10(1), 77-112.

Chalfin, A. McCrary, J., 2018,. "Are US cities underpoliced? Theory and evidence." *Review of Economics and Statistics,* 100(1), 167-186.

Chodorow-Reich, G., Gopinath, G., Mishra, P., and Narayanan, A., 2020, "Cash and the economy: Evidence from India's demonetization." *The Quarterly Journal of Economics,* 135(1), 57-103.

Gavrilova, E., Kamada, T., and Zoutman, F., 2019, "Is legal pot crippling Mexican drug trafficking organisations? The effect of medical marijuana laws on US crime," *The Economic Journal,* 129(617), 375-407.

Goodman-Bacon, Andrew, 2018, "Public insurance and mortality: evidence from Medicaid implementation," *Journal of Political Economy,* 126(1), 216-262.

Solon, G., Haider, S.J. and Wooldridge, J.M., 2015. "What are We Weighting For?." *Journal of Human Resources*, 50(2), 301-316.

Wooldridge, J.M., 2013, *Introductory Econometrics: A Modern Approach. 5th ed.* Mason, OH: South-Western.

Table B1: Effect of Using Employment-Based Weights on OEWS Estimates
Wage Group 1 Estimates

		Unweighted Estimates			
	Baseline Weighted Estimates	Full Sample	Exclude Large Emp Changes	Limit to Large Occupations	Limit to Small Occupations
	(1)	(2)	(3)	(4)	(5)
Panel A: Overall Employment Effects					
ΔMW Next Year	0.19***	0.20	0.10	-0.07	0.24
	(0.07)	(0.14)	(0.11)	(0.10)	(0.16)
ΔMW This Year	0.06	0.21	0.14	0.09	0.22
	(0.08)	(0.13)	(0.10)	(0.11)	(0.15)
ΔMW Last Year	0.09	0.12	0.01	0.17	0.10
	(0.05)	(0.09)	(0.10)	(0.11)	(0.11)
ΔMW 2Yrs Ago	0.01	0.06	0.08	0.14	0.00
	(0.08)	(0.18)	(0.16)	(0.12)	(0.21)
Panel B: Employment Effects by Routine Cognitive Tasks					
ΔMW Next Year X Routine Cognitive Share	0.01	-0.08	-0.01	0.06	-0.06
	(0.08)	(0.11)	(0.07)	(0.10)	(0.14)
ΔMW This Year X Routine Cognitive Share	-0.03	-0.02	-0.06	-0.14	0.03
	(0.04)	(0.14)	(0.09)	(0.10)	(0.17)
ΔMW Last Year X Routine Cognitive Share	-0.09*	-0.17	-0.16	-0.22***	-0.16
	(0.05)	(0.11)	(0.10)	(0.06)	(0.13)
ΔMW 2Yrs Ago X Routine Cognitive Share	-0.21***	-0.31**	-0.18**	-0.30***	-0.28
	(0.07)	(0.13)	(0.09)	(0.09)	(0.17)
Panel C: Employment Effects by Routine Manual Tasks					
ΔMW Next Year X Routine Manual Share	0.03	-0.19	-0.05	0.00	-0.22
	(0.21)	(0.16)	(0.08)	(0.20)	(0.19)
ΔMW This Year X Routine Manual Share	-0.10	-0.09	-0.04	-0.12	-0.07
	(0.10)	(0.12)	(0.07)	(0.10)	(0.14)
ΔMW Last Year X Routine Manual Share	-0.14*	-0.13	-0.12	-0.23	-0.12
	(0.07)	(0.09)	(0.08)	(0.15)	(0.11)
ΔMW 2Yrs Ago X Routine Manual Share	-0.17***	-0.03	-0.03	-0.31***	0.02
	(0.06)	(0.16)	(0.11)	(0.11)	(0.20)
Panel D: Employment Effects by Overall Routine Tasks					
ΔMW Next Year X Overall Routine Share	0.02	-0.19	-0.05	0.02	-0.20
	(0.14)	(0.13)	(0.08)	(0.15)	(0.17)
ΔMW This Year X Overall Routine Share	-0.07	-0.08	-0.06	-0.15	-0.04
	(0.06)	(0.14)	(0.08)	(0.10)	(0.17)
ΔMW Last Year X Overall Routine Share	-0.13**	-0.19**	-0.17**	-0.25**	-0.18
	(0.06)	(0.09)	(0.09)	(0.10)	(0.12)
ΔMW 2Yrs Ago X Overall Routine Share	-0.22***	-0.19	-0.12	-0.34***	-0.15
	(0.06)	(0.15)	(0.10)	(0.09)	(0.20)
Panel E: Employment Effects by Interpersonal Tasks					
ΔMW Next Year X Interpersonal Share	-0.09	0.24	0.06	-0.13	0.28
	(0.13)	(0.16)	(0.10)	(0.15)	(0.20)
ΔMW This Year X Interpersonal Share	0.03	0.04	0.05	0.16	-0.01
	(0.09)	(0.14)	(0.09)	(0.15)	(0.17)
ΔMW Last Year X Interpersonal Share	0.19*	0.14*	0.16*	0.33**	0.10
	(0.10)	(0.08)	(0.09)	(0.15)	(0.11)
ΔMW 2Yrs Ago X Interpersonal Share	0.24*	0.14	0.08	0.43**	0.05
	(0.12)	(0.14)	(0.10)	(0.17)	(0.17)
N	95,781	95,781	92,432	8,710	87,071

Notes: This table reports estimates from a series of empirical specifications examining the differences between estimates that use and do not use state-year-occupation employment level weights. Each column within each panel represents the Wage Group 1 estimates from a different regression. Among the different unweighted estimates "Large Employment Changes" are employment changes larger than 1 log point and "Large/Small Occupations" are occupations with employment levels greater than/less than 10,000. While there are many fewer "Large Occupations," there are large Wage Group 1 occupations in each state and they represent more than 80 percent of the total employment among Wage Group 1 occupations. *p<0.1; **p<0.05; and *** p<0.01

Table B2: Effect of Using Employment-Based Sample Weights on ACS Estimates
Wage Group 1 Estimates by Race

	Non-Asian Workers of Coloe				White and Asian Americans			
		Unweighted Estimates				Unweighted Estimates		
	Baseline Weighted Estimates	Full Sample	Exclude Large Emp Changes	Limit to Large Occupations	Baseline Weighted Estimates	Full Sample	Exclude Large Emp Changes	Limit to Large Occupations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Overall Employment Effects								
ΔMW Next Year	0.42*	0.25	0.36	-0.25	0.00	0.07	0.26	-0.15
	(0.23)	(0.31)	(0.30)	(0.47)	(0.12)	(0.20)	(0.18)	(0.22)
ΔMW This Year	0.32	-0.13	-0.29	-0.09	0.02	0.00	0.03	0.33*
	(0.24)	(0.31)	(0.34)	(0.55)	(0.09)	(0.17)	(0.15)	(0.19)
ΔMW Last Year	0.32**	0.67**	0.52	0.27	-0.01	-0.38*	-0.43***	-0.02
	(0.12)	(0.31)	(0.31)	(0.35)	(0.08)	(0.23)	(0.15)	(0.25)
ΔMW 2Yrs Ago	-0.64***	-0.80**	-0.73**	-0.50	0.01	0.21	0.18	-0.36*
	(0.18)	(0.30)	(0.31)	(0.54)	(0.09)	(0.26)	(0.17)	(0.19)
Panel B: Employment Effects by Overall Routine Tasks								
ΔMW Next Year X Routine Share	0.07	0.27	0.19	0.28	-0.12	-0.03	-0.18	-0.27
	(0.17)	(0.21)	(0.21)	(0.41)	(0.10)	(0.15)	(0.13)	(0.20)
ΔMW This Year X Routine Share	0.00	0.08	0.17	0.18	-0.02	-0.07	0.08	0.24
	(0.17)	(0.19)	(0.19)	(0.37)	(0.08)	(0.12)	(0.10)	(0.20)
ΔMW Last Year X Routine Share	-0.09	0.05	-0.02	-0.08	0.18**	0.21*	0.22**	0.10
	(0.16)	(0.26)	(0.30)	(0.37)	(0.08)	(0.13)	(0.10)	(0.15)
ΔMW 2Yrs Ago X Routine Share	-0.55***	-0.47	-0.43	-0.95*	-0.26***	0.00	-0.07	-0.57**
	(0.18)	(0.34)	(0.38)	(0.56)	(0.10)	(0.15)	(0.15)	(0.24)
Panel C: Employment Effects by Interpersonal Tasks								
ΔMW Next Year X Interpersonal Share	-0.14	-0.24	-0.11	-0.45	0.13	0.00	0.18	0.37*
	(0.15)	(0.25)	(0.25)	(0.44)	(0.12)	(0.12)	(0.12)	(0.21)
ΔMW This Year X Interpersonal Share	0.01	-0.08	-0.13	-0.09	0.06	0.12	-0.03	-0.30
	(0.18)	(0.26)	(0.28)	(0.40)	(0.11)	(0.10)	(0.09)	(0.21)
ΔMW Last Year X Interpersonal Share	0.18	-0.01	-0.02	0.35	-0.21**	-0.20	-0.30***	-0.04
	(0.17)	(0.30)	(0.32)	(0.45)	(0.09)	(0.12)	(0.10)	(0.16)
ΔMW 2Yrs Ago X Interpersonal Share	0.49**	0.48	0.41	0.94	0.34***	0.02	0.17	0.63***
	(0.21)	(0.37)	(0.38)	(0.59)	(0.09)	(0.13)	(0.14)	(0.16)
N	80,233	80,233	74,649	17,078	220,914	220,914	189,995	17,667

Notes: This table reports estimates from a series of empirical specifications examining the differences between estimates that use and do not use state-year-occupation employment level weights in the ACS. Each column within each panel represents the Wage Group 1 estimates from a different regression. Among the different unweighted estimates "Large Employment Changes" are employment changes larger than 1 log point and "Large Occupations" are occupations with employment levels greater than/less than 100. While there are many fewer "Large Occupations," there are large Wage Group 1 occupations in each state and they represent more than 60 percent of the total employment among Wage Group 1 occupations. *p<0.1; **p<0.05; and *** p<0.01