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Racial Inequality and Minimum Wages in Frictional Labor Markets

Jesse Wursten* and Michael Reich**

We examine how the racial patchwork of federal and state minimum wage changes between 1990 and 2019 has affected racial wage gaps, with specific attention to effects on labor market frictions. Black workers on average are less likely to live in high-wage states that have raised their wage floors. The effect of state minimum wages on the national racial wage gap is thus not self-evident.

Using five different causal specifications, including the "bunching" estimator of Cengiz et al. (2019), and data from the CPS and the QWI, we find that minimum wage changes since 1990 did reduce the 2019 racial wage gaps, by 12 percent among all workers and 60 percent among less-educated workers. The reductions are greater among black women and among black prime age workers. The gains for black workers are concentrated well above the new minimum wage, beyond the usual spillover estimates. Earnings of all race/ethnic/gender groups grew, with larger effects among black workers. We do not find disemployment effects for any group.

Surprisingly, racial differences in initial wages do not explain the reduction in the racial wage gap. Rather, minimum wages expand job opportunities for black workers more than for white workers. We present a model in which minimum wages assist the job search of workers who do not own automobiles and who live farther from jobs. Our causal results using the ACS show that minimum wages increase commuting via automobile among black workers, supporting our model. Minimum wages also reduce racial gaps in separations and hires, further suggesting the policies especially enhance job opportunities for black workers.

Keywords: racial wage gaps, minimum wages, black wages, Hispanic wages, labor markets

JEL codes: J7, J15, J31, J38

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1. Introduction

Racial wage and employment gaps have increased steadily since the late 1970s, both overall and separately among males and females (Daly, Hobijn and Pedtke 2017; Miller 2018). This widening has occurred despite reductions in black-white educational attainment and achievement score gaps and the implementation of numerous policies to remedy labor market inequality.[1] The fraction of the racial wage gap that cannot be explained by state of residence, years of schooling, age, job type, industry and occupation has increased in this period (Daly et al. 2017).

In this paper, we consider the effects of minimum wage policy on these racial wage gaps. The Fair Labor Standards Act was extended to more industries and occupations, especially in the latter 1960s, and with only limited further extensions since. And since the federal minimum wage level was last increased in 2009, its real value in 2020 is no higher than in the 1990s.

Federal and state minimum wage policies began to diverge in the late 1980s, as 29 states and the District of Columbia have enacted their own, higher, minimum wage standards. These states tend to be higher cost and higher wage states, while those that remain at the federal level consist mainly of low-wage states, many located in the Old South. These southern states contain disproportionately large concentrations of black workers. In other words, minimum wage policy in the U.S. has evolved in a manner that, despite the 1966 extensions, has increasingly left behind black workers in low-wage states. This patchwork system of federal and state minimum wage policies may therefore have *increased* national racial wage gaps, despite potentially narrowing them in individual states.[2]

Our causal analyses find that the intermittent federal minimum wage increases and the growth of state minimum wage policies since the 1990s have narrowed racial wage inequality. Our counterfactual simulation indicates that, absent the policies, the gap in hourly wages between black and white workers would have been 12 percent larger among all workers and 60 percent larger among those with at most a high school diploma. These results are consistent with the magnitudes in Derenoncourt and Montialoux (2021), who find that the minimum wage reforms of that era reduced racial inequality by about 10 percent. They are inconsistent with the estimates for subsequent years reported in Cengiz et al. (2019).[3]

The reduction in racial wage inequality results less from pre-existing racial wage differentials among the most exposed workers, and more from black workers' reacting more strongly to

[1]Reardon et al. 2014. The remaining racial gaps in educational attainment and achievement are increasingly accounted for by increasing differences in parental income and education, as Reardon et al. show. As a result, these racial educational gaps have not narrowed since 2002 (de Brey et al. 2019).

[2] Black workers represent 6.7 percent of all employment in the [unweighted] median state-quarter in the period since 1990, versus 4.9 percent in the state-quarters with at least one state minimum wage event (defined as per Cengiz et al. 2019), a 27 percent difference (calculations based on the CPS).

[3] Appendix A shows this possibility by comparing minimum wage effects in Mississippi and Washington State. Cengiz et al. 2019 (Table I, column 2) report an average wage increase of 6.8 percent among all workers and a wage increase of only 4.4 percent for the sample of black or Hispanic workers (Table II, column 5; Cengiz et al. do not report wage effects for blacks or Hispanics with a high school degree or less.) This discrepancy with our results may arise from compositional effects (e.g., minimum wage changes were smaller in states with larger concentrations of black workers); our specifications are robust to such confounding factors. Their bunching estimator requires large sample sizes, greater than the sample size among blacks with a high school degree or less. In Figure *6* below we show that our finding of disproportionate wage gains for black workers is supported by a simplified bunching estimator. The estimator is simplified in the sense that we reduce the number of coefficients to be estimated and harmonize the control variables with the treatment indicators.

minimum wage changes. Using an entropy balancing design that reweights observations to equalize initial wages, we show that the reduction in the racial wage gap cannot be explained by differences in initial wages. Moreover, our bunching estimates (Cengiz et al. 2019) indicate that black wage gains are concentrated among workers (eventually) earning $4 above the new minimum wage, well above previous spillover effect ranges.

We present a model in which these disproportionate wage gains arise through the indirect effect of minimum wage policies on the job opportunities of low-wage black workers.[4] Higher minimum wages expand the financial resources at their disposal, improve their credit ratings and thus their access to automobile financing and to expanded commuting options. In turn, the expanded commuting options allow black workers to reach better paying jobs and find better matches. This mechanism is consistent with earlier findings on the mismatch between the location of black workers' residential locations and higher-paying job opportunities (Raphael and Riker 1999; Miller 2018) and minimum wage effects on credit access and car loans (Cooper, Luengo-Prado and Parker 2020; Aaronson et al. 2012). We then use commuting data from the ACS Journey to Work files and employment flow data from the Quarterly Workforce Indicators dataset to verify empirically that minimum wage increases lead to higher rates of automobile commuting and lower job turnover for black workers. These effects are either absent or considerably smaller for white workers.

The gains for black workers do not crowd out those of white or Hispanic workers. Rather, minimum wages increase earnings for all race/age/gender groups; they simply increase more for black workers and women in general. We do not find any disemployment effects among race/ethnicity and gender groups. On the contrary, black workers are *less* likely to lose their jobs after minimum wage changes. The wage gains for all groups without disemployment effects makes sense if these policies reduce labor market frictions.[5]

Relation to the literature The voluminous minimum wage literature includes numerous estimates of the effects of the policy on black and white employment, but very few estimates of the separate effects on black and white workers' wages. The lion's share of these studies focuses only on racial gaps in employment effects. Card and Krueger (1995), who devote one chapter to wage effects, report (p. 282) only that nonwhite workers are more exposed to minimum wages; they do not estimate the policy's effects on racial wage differentials. The same is true of the extensive and more recent survey of the minimum wage literature by Belman and Wolfson (2014). Allegretto, Dube and Reich (2011) show that minimum wages increase wages of black teens more than wages of white teens. As we discuss below, teens constitute only 8 percent of all workers. Our analysis focuses on workers with a high school diploma or less, who constitute 49 percent of respondents in our sample, thereby providing more generalizability.

Minimum wage policies may act directly upon the racial wage gap as well as indirectly through the amelioration of other factors that generate racial inequality. As we have noted, our results indicate that the direct mechanism alone cannot explain the reduction of the racial wage gap. Indirect mechanisms that have been previously studied include effects on labor market

[4] Manning (2020, 2021) reviews the relevance of labor market frictions in explaining the absence of disemployment effects.

[5] The minimum wage literature also shows that minimum wages are partly absorbed by productivity gains (Ruffini 2020) and small price increases in some industries (Cooper, Luengo-Prado and Parker 2020). Moreover, studies of noncompliance suggest that minimum wage increases do not always reach their targets (Levine 2018) and that noncompliance differs by racial groups (Cooper and Kroeger 2017). This literature is beyond our scope here.

discrimination (Charles and Guryan 2008; Pager and Pedulla 2015), discrimination in tasks or skills (Hurst et al. 2020), job search frictions (Raphael and Riker 1999; Johnson 2006; Stoll and Covington 2012) and through interactions with other markets, such as housing, that constrain black job opportunities (Ihlanfeldt and Sjoquist 1998; Bergmann et al. 2020).[6] Our finding of racial differences in the effects of minimum wages on commuting modes and employment flows lend support to an explanation based on reductions in search frictions for black workers.

Our paper is most related to Derenoncourt and Montialoux (2021) and Bailey, DiNardo and Stuart (2020), both of whom examine the effects of the 1966 minimum wage reform on different racial groups. Our paper uses subsequent policy changes and makes use of the longitudinal feature of the CPS MORG data, which have rarely been used to study minimum wage effects. Our paper is also related to the literature on heterogeneous effects of minimum wages (Cengiz et al. 2019; Wursten 2020; Godoey and Reich forthcoming) as well as to an earlier literature on the political economy of racial inequality by economists (Reich 1978, 1981; Alesina et al. 2001) and more recent work by political scientists (such as Lopez 2014). This literature suggested that union power and the extent of the welfare state are weakened by racial divisions, hurting white workers as well as black workers. Finally, our paper is also closely related to studies that show how access to automobiles affects labor market outcomes for disadvantaged workers. For example, Ong (2002) finds that predicted car ownership improved employment among TANF recipients and Cho (2020) finds that state-level reforms since 2013 on driver's licenses improved employment opportunities among undocumented workers.

Methods and results Our analyses focus on workers with a high school diploma or less. We begin by using descriptive evidence to demonstrate that minimum wages continue to have the potential to reduce racial inequality. Next, we use four causal estimation strategies to determine whether minimum wages raise earnings more for black workers. We start with standard panel models based on the CPS MORG, in line with Allegretto et al. (2017).

Second, to control more tightly for individual characteristics and better target our sample to affected workers, we exploit that respondents report their earnings twice over a twelve-month period. This setup closely resembles a difference in differences design, in which the pre- and post-period are respectively the first and second wave of interviews; we determine treated vs control status by the presence of a minimum wage change in the twelve months between interviews. This approach also allows us to test whether minimum wage increases are race-neutral; that is, do black and white workers at the same initial wage receive the same wage increase? Using an entropy-balancing technique to reweight the black wage distribution, we find that minimum wage increases are not at all race-neutral: our observed greater wage increases for black workers do not diminish when we compare workers with similar initial wages.

Third, we run an event study at the state-quarter level to test for robustness to confounding time trends (Cengiz et al. 2019, Godoey et al. 2019). Fourth, we implement a bunching estimator

[6] These mechanisms are not mutually exclusive. For example, greater search frictions for black workers can arise from employer discrimination as well as from spatial disparities between black neighborhoods and the location of jobs. Much of the empirical debate about "race versus space" is based on cross-sectional data that cannot identify causal effects (Glaeser, Hanushek and Quigley 2014). For two studies with a causal research design, see Stoll and Raphael (2000) and Miller (2018). For a more detailed discussion of race and labor market frictions, see Lang and Spitzer (2020).

(Cengiz et al. 2019), which provides additional information on the spread of wage gains throughout the wage distribution.

Since we find that the direct mechanical effects of minimum wage increases do not fully explain the reduction in the racial wage gap, we then turn to possible indirect mechanisms that might be at play. We develop a model motivated by previous studies that find that minimum wages have large effects on credit ratings and on acquisition of automobiles. In the model, minimum wage increases indirectly allow workers to switch from low to high outside option type. This improvement of their bargaining position in turn leads to a new wage that can exceed the new minimum wage. This channel is less relevant to white workers because they are more likely to be situated in a location with good outside options and have higher starting wealth.

We test the model by examining the effects of minimum wages on the probability of commuting to work by automobile instead of public transit, using the American Community Survey Journey to Work files.[7] We find that higher minimum wages lead to increased automobile commuting for young (ages 26-35) black workers in poor households. In line with our expectations, the effects are smaller to non-existent for workers from richer households, workers 21 to 25 (who do not easily qualify for car loans) and older workers (most of whom already own a car; see Appendix Figure A3).

Additionally, we estimate the effect of minimum wages on job stability using the Quarterly Workforce Indicator dataset. Here we follow the panel setup of Dube et al. (2016) and find that turnover rates of food services workers decline twice as much among black workers, regardless of specification. In line with our expectations, these effects are even stronger in low-wage counties within states (see also Wursten 2020). They are also robust to using the Cross Border County Pair method introduced in Dube et al. (2010).

Finally, we use our results to estimate the size of the racial inequality reduction that can be attributed to the minimum wage. For workers with at most a high school education, minimum wage increases since 1995 reduced the 2019 racial wage gap by 60 percent, from five percentage points to two percentage points. They also reduced the overall racial wage gap by 12 percent and reduced the growth of the racial wage gap by 20 percent.

Additional results We also find that minimum *wages* especially benefit female workers of all race/ethnicities. Our findings for different age groups indicate that minimum wages have greater wage effects on young (16-24) white workers than on young black workers. But they have larger effects on black workers in prime age groups (25-54) than among white workers in those age groups.

Some studies suggest that white workers believe that improvements for black workers must come at the expense of the white workers (Craig and Richeson 2014). However, Lempinen shows that explaining to white workers how black and white workers share common interests lead to different results (Lempinen, 2020). Our finding that minimum wage policies increase white workers' wages indicates that the policies do not constitute a zero-sum game between blacks and whites.[8] This result explains the broad appeal of minimum wage policies to white workers.

[7] We are the first to study of the effects of minimum wages on commuting modes. Public transit systems outside central city limits generally are much slower per mile than commuting by automobile.

[8] These findings are like those in Reich (1978, 1981).

We conclude that minimum wage increases continue to be a powerful tool for ameliorating racial inequality, especially for workers at the bottom of the wage distribution.

Our paper proceeds as follows. Section 2 discusses the data and institutional context. Section 3 presents our descriptive and causal evidence. Section 4 examines indirect effects. Section 5 provides our counterfactual simulation of how much minimum wage increases since 1995 have remedied racial wage inequality. Section 6 concludes.

2. Data and institutional context

Our main analysis is based on three datasets: Current Population Survey Monthly Outgoing Rotation Group (CPS-MORG)[9] files for individual-level characteristics and hourly wages, the Quarterly Workforce Indicator (QWI) dataset for county-level employment stocks and flows (hires, separations) and the regularly updated state minimum wage levels dataset described by Vaghul and Zipperer (2019).

CPS Table 1 provides descriptive statistics for the 1990-2019 CPS sample. We exclude the self-employed, those in the armed forces and unpaid family workers. 71 percent of the remaining respondents are non-Hispanic whites, 10 percent are black and 11 percent are Hispanic. We group the remaining race and ethnicities as well as mixed races in the restgroup *Other*.[10] Teens make up 9 percent of the full sample, increasing to 16 percent when we consider only those with a high school degree or less (HSOL), and to 19 percent of those earning less than 1.5 times the 24-month smoothed minimum wage (< 1.5 MW). They represent just 2 percent of workers earning between 1.5 and 2.5 times the smoothed minimum wage.

A similar pattern emerges for those identifying as black or Hispanic. They are overrepresented in the high school or less and < 1.5 MW subsamples and less present in the higher wage groups (e.g. black workers make up just 6 percent of the > 2.5 MW group). The opposite holds for white workers, providing suggestive evidence of an (unadjusted) racial earnings gap. Employment rates differ substantially by race and ethnicity. They are highest for non-Hispanic white respondents (72 percent), dropping to 65 and 61 percent for Hispanic and black respondents, respectively. Rates are lower for those without a college degree. By construction, the initial employment rate is 100 percent in the last three columns, as these are based on a wage criterion.

Hourly pay (in 2019 dollars) averaged $18 over the sample period, $0.74 higher for (non-Hispanic) white workers and $3.20 and $2.09 lower for Hispanic and black workers. These differences are less pronounced within low-wage groups, e.g. Hispanic workers earn more than their white counterparts in the < 1.5 MW sample. On the other hand, white workers do earn an average of $1.71 more per hour in the top wage group (> 2.5 MW). This difference in patterns between the bottom group and the top wage group could result from the equalizing effects of minimum wage policy at the bottom. The small differences in hourly pay by race and ethnicity for workers in the <1.5 MW sample suggest any further minimum wage increases would have limited direct effects on racial wage gaps.

[9] Obtained through IPUMS (Flood et al, 2020).

[10] Hispanics can be of any race. The racial identity of Hispanics has changed in recent decades, from predominantly white to more multi-racial (Parker et al. 2015). The overlap between the Hispanic and black categories has therefore grown over time. We ignore this overlap in this paper. We must also ignore other groups, such as Asian Americans and Native Americans, because of sample size issues.

QWI The publicly available Quarterly Workforce Indicators (QWI) dataset is based on administrative Longitudinal Employer-Household Dynamics data and has employment stocks and flows for most U.S. counties.[11] In recent years the QWI has incorporated race, gender and ethnicity variables. The dataset is available in different endpoints, which split the data into different population groups. We start from the 'rh' endpoint, which splits workers by their race and ethnicity. We define hiring, separation and turnover rates as per Dube et al. (2016), where each rate is defined as the new flows divided by employment at the start of the quarter (E_{t0}), e.g. the quarterly hiring rate $H_t = \frac{hires_t}{E_{t0}}$ and the separation rate $S_t = \frac{separations_t}{E_{t0}}$. The turnover rate is the average of the hiring and separation rate. We restrict all analysis to the food services sector (NAICS 722).

Table 1 CPS descriptive statistics

	Full Sample	HSOL	< 1.5 MW	1.5-2.5 MW	>2.5 MW
Sample shares	1.00 (0.00)	0.49 (0.50)	0.01 (0.08)	0.01 (0.09)	0.01 (0.09)
Teen (16-19)	0.09 (0.28)	0.16 (0.36)	0.19 (0.39)	0.02 (0.14)	0.00 (0.04)
Hispanic	0.11 (0.32)	0.16 (0.36)	0.16 (0.36)	0.10 (0.30)	0.05 (0.23)
Black	0.10 (0.31)	0.12 (0.33)	0.10 (0.30)	0.09 (0.29)	0.06 (0.24)
White	0.71 (0.45)	0.66 (0.47)	0.68 (0.47)	0.75 (0.43)	0.83 (0.37)
Other	0.07 (0.25)	0.06 (0.24)	0.06 (0.24)	0.05 (0.23)	0.05 (0.21)
Employed	0.70 (0.46)	0.60 (0.49)	1.00 (0.00)	1.00 (0.00)	1.00 (0.00)
Hispanic	0.65 (0.48)	0.60 (0.49)	1.00 (0.00)	1.00 (0.00)	1.00 (0.00)
Black	0.61 (0.49)	0.51 (0.50)	1.00 (0.00)	1.00 (0.00)	1.00 (0.00)
White	0.72 (0.45)	0.62 (0.49)	1.00 (0.00)	1.00 (0.00)	1.00 (0.00)
Hourly wage (2019$)	17.92 (10.72)	15.17 (7.76)	9.58 (1.54)	15.15 (2.57)	28.99 (9.68)
Hispanic	14.72 (7.85)	13.41 (6.17)	9.84 (1.62)	15.12 (2.68)	27.41 (8.55)
Black	15.83 (8.74)	13.80 (6.65)	9.42 (1.52)	14.82 (2.46)	27.42 (8.30)
White	18.66 (11.15)	15.81 (8.14)	9.52 (1.51)	15.17 (2.55)	29.12 (9.75)

Notes: White refers to non-Hispanic white workers. Table reports means, with standard deviations in parentheses. Statistics are unweighted. Hourly wages are in 2019 dollars, deflated using the CPI-U. HSOL refers to the high school or less sample. < 1.5 MW is the sample of workers earning less than 1.5 times the 24-month smoothed minimum wage. 1.5-2.5 and > 2.5 MW are then the samples earning respectively between 1.5 and 2.5 and more than 2.5 times the 24-month smoothed minimum wage. These three groups are by definition employed. Period: 1990-2019. Data obtained through IPUMS.

Table 2 provides descriptive QWI statistics for 1990-2017. The average county employed about 500 black workers, 550 Hispanic workers and almost 3000 non-Hispanic white workers in the food services sector (with large standard deviations). Quarterly hiring and separation rates are high among white workers (40 percent) and higher still among black and Hispanic workers (over 50 percent), suggesting considerable workforce churn in this sector. Average monthly earnings for black workers are also considerably lower than for their Hispanic and white coworkers.

Minimum Wages We observe 550 changes in federal and state minimum wages between 1990 and 2019, with an average size of $0.50 (8.4 percent).[12] The bottom line in Figure 1 represents

[11] The QWI fuzzes certain data cells to protect confidentiality. Moreover, entry into the QWI program was staggered and non-random. In our baseline specification, we use all data provided in the QWI as is. All results are robust to excluding heavily distorted cells and limiting the sample to 2000+, at which point most states had entered the program.

[12] We include small cost-of-living changes as they affect wages. We exclude local minimum wage changes because of the limited CPS sample size.

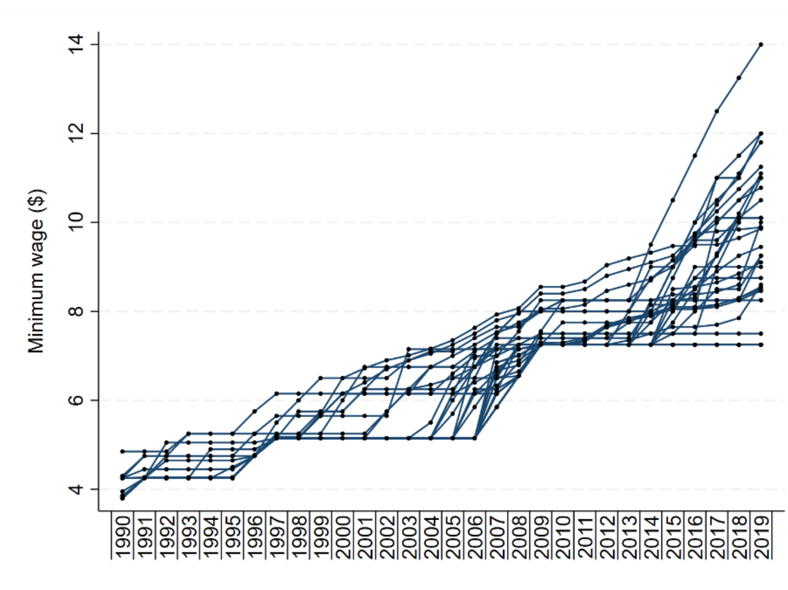
the federal minimum wage floor, the lines above show states that decided to exceed the federal floor. In 2019, the District of Columbia had the highest minimum wage, at $14 per hour.

Table 2 QWI descriptive statistics

	Black	Hispanic	White
Employment	499 (1596)	550 (2688)	2732 (6579)
Hiring rate (%)	55 (36)	50 (41)	40 (16)
Separation rate (%)	52 (28)	47 (29)	39 (13)
Turnover rate (%)	54 (31)	49 (33)	40 (13)
Monthly earnings ($)	816 (726)	913 (344)	924 (289)

Notes: White refers to non-Hispanic white workers. Table reports means by county, with standard deviations in parentheses. Statistics are unweighted. Hiring rate is defined as new quarterly hires divided by start-of-quarter employment, analogously for separations. Turnover rate is the average of the two. Period: 1990-2017. Includes only counties with data on all three groups. Food services sector only (NAICS 722).

Figure 1 Minimum wage evolution by state, 1990-2019



Source: Vaghul and Zipperer 2019

3. Descriptive and causal results

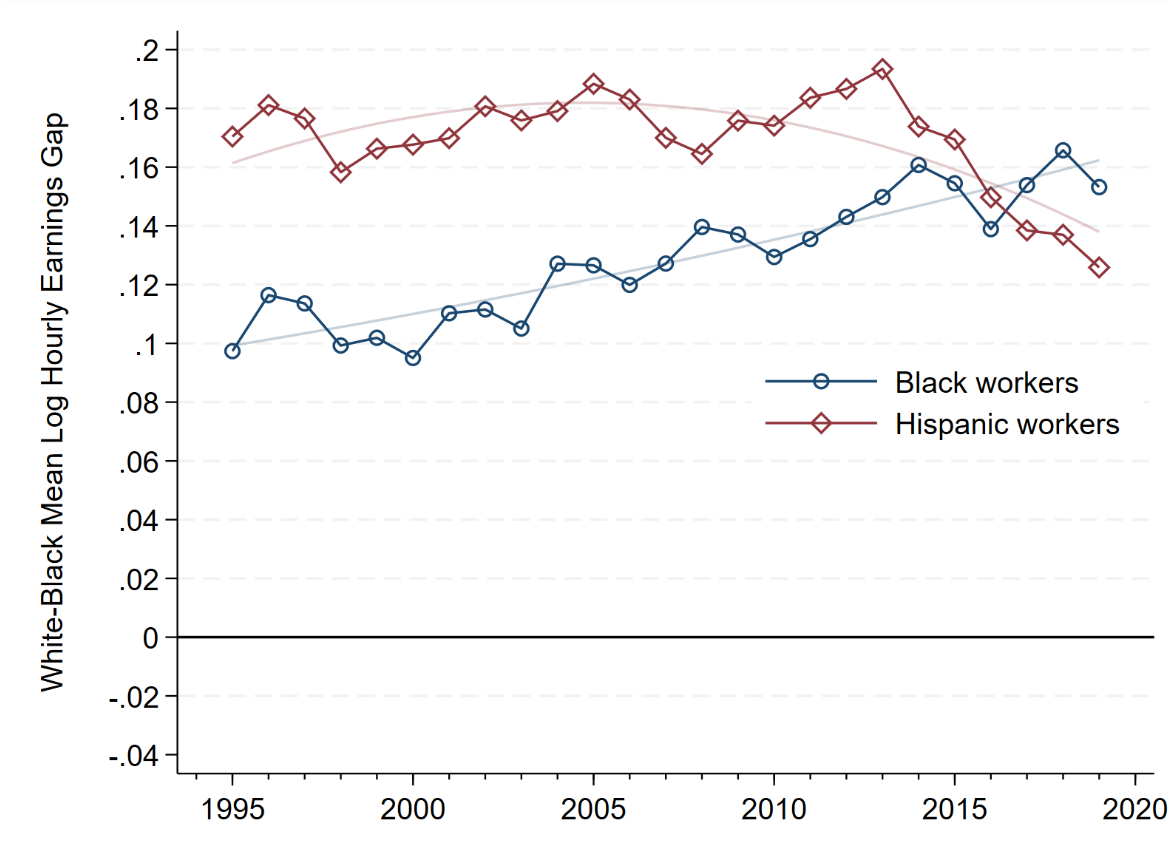
In the first part of this results section, we present descriptive evidence of the continued existence of racial earnings gaps. These differences illustrate why minimum wage policy could be a useful tool to close those gaps. In the second part, we estimate causal models using the CPS-MORG to show that minimum wages indeed raise wages more for black and Hispanic workers and that these racial disparities cannot be explained by differences in initial wages.

3.1. Descriptive results

In this section we present trends in unadjusted and then adjusted racial earnings gaps since 1995. We then explore the relative exposures of black, Hispanic and non-Hispanic white workers to minimum wage increases as well as the predicted share of wage bill increases by race/ethnicity.

Unadjusted race/ethnicity earning gaps Figure 2 shows the unadjusted racial hourly earnings gap from 1995 to 2019 for black and Hispanic workers versus (non-Hispanic) white workers, using CPS-MORG individual level hourly wages. We aggregate log wages by year and race/ethnicity and then calculate differences relative to the average log wage of white workers. Over our sample period, black and Hispanic workers earn 10 to 20 percent less per hour than white workers.[13] This gap has widened in recent years for black workers (from 10 percent to 16 percent) and narrowed for Hispanic workers (from 17 percent to 12 percent).

Figure 2 Unadjusted racial earnings gaps, 1995-2019



Notes: Based on CPS-MORG hourly wages. Weighted using MORG weights. Includes all earners above 16 years of age. Log wages are aggregated by race/ethnicity and year. Gaps can be roughly interpreted as percentage point differences. The lightly colored lines represent a quadratic fit of the yearly differences.

Adjusted race/ethnicity earnings gaps. Figure 2 above does not account for demographic, geographic and other compositional differences among racial groups. In Table 3 we present race/ethnicity earnings gaps adjusted for state, time, age, gender and marital status differences as per Equation (1).

$$hourlyWage_{it} = race_i + state_{it} + quarter_t + age_{it} + gender_{it} + married_{it} + \epsilon_{it} \tag{1}$$

[13] The black-white earnings gap in Derenoncourt and Montialoux (2021, figure 1) is somewhat larger than ours (25 percent versus 16 percent). They display annual earnings, for workers 25 to 64 only, which are affected by differences in hours worked per year as well as by differences in hourly wages. And their measure abstracts from the large declines in the proportion of young workers, who are more exposed to minimum wages. Our concern is with hourly pay among workers of all ages, including young workers. We examine age differences in minimum wage effects below.

The dependent variable is the log hourly wage in 2019 dollars of individual i in month t. We include state, quarter, age, gender and married status fixed effects. We do not control for education or industry as these might themselves reflect race/ethnicity gaps.

Table 3 shows the coefficients on the race/ethnicity dummies $race_i$. These coefficients estimate the earnings gap relative to white workers, controlling for individual characteristics.

Table 3 Adjusted racial earnings gaps, 1990-2019

	Earnings gap
Hispanic	-0.24***
	(0.02)
Black	-0.12***
	(0.01)
N	2,482,159

Notes: The reference category is non-Hispanic whites. Period: 1990-2019. Data: CPS-MORG. Replication tag: #adjRacialGap. Standard errors in parentheses, clustered at state level. Stars: * p<0.1, ** p<0.05, *** p<0.01

The average adjusted earnings gap of 12 percentage points for black workers is similar to the unadjusted ratio in Figure 2. In contrast, the adjusted earnings gap of 24 percentage points for Hispanic workers is much greater than the unadjusted gap of 12 to 18 percentage points.

Minimum wage exposure by race/ethnicity The lower wages earned by black and Hispanic workers are reflected in the probability that they are affected by minimum wages. We estimate Equation 2,

$$wageBelowMW_{it}^{1.25x} = \alpha + race_i + \epsilon_{it} \tag{2}$$

where the dependent variable is an indicator variable equal to one if the individual earned less than 1.25 times the minimum wage in month t.[14] The coefficient on the race dummies $race_i$ are then an estimate of the percentage point difference in the probability of being affected by the minimum wage relative to white workers (the omitted group).

Table 4 Probability of being a minimum wage worker, 1990-2019

Race	Percentage point	Percentage
Hispanic	1.5*** (0.07)	16.9
Black	1.8*** (0.08)	20.9
N	2,482,159	

Notes: reference category is non-Hispanic whites, of which 8.8 percent earn less than 1.25 times the minimum wage. Period: 1990-2019 Data: CPS-MORG. Replication tag: #probMwWorker. Standard errors in parentheses, clustered at state level, * p<0.1, ** p<0.05, *** p<0.01.

[14] The 1.25 cutoff allows for spillover effects and is standard in the minimum wage literature.

As Table 4 shows, Hispanic and black workers are respectively 1.5 and 1.8 percentage points more likely to be affected by the minimum wage. As 8.8 percent of white workers earn less than 1.25 times the minimum wage, this estimate corresponds to 17 and 21 percent increases respectively, in the probability of being a minimum wage worker.

Predicted share of wage bill increase by race/ethnicity Additionally, we examine whether a disproportionate share of the wage bill increase goes to black and Hispanic workers if compliance is perfect and there are no wage spillovers. For each year *y* and ethnicity *r* we calculate the total weekly wage bill,

$$wb_{ry} = \sum_{it\in(r,y)} hourlyWage_{it} * hoursPerWeek_{it} \tag{3}$$

Then we calculate how much the hourly wage of worker *i* would need to change to comply with minimum wage regulation six months in the future. Multiplied by initial hours worked, this gives us the wage bill increase by year and ethnicity,

$$wbi_{ry} = \sum_{it\in(r,y)} \max\left[\left(MW_{i,t+6} - hourlyWage_{it}\right) * hoursPerWeek_{it}, 0\right] \tag{4}$$

For each ethnicity we then calculate its share of the wage bill (5a), its share in the wage bill increases (5b) and the ratio thereof (5c):

$$wbShare_{ry} = \frac{wb_{ry}}{\sum_R wb_{ry}} \tag{5a}$$

$$wbiShare_{ry} = \frac{wbi_{ry}}{\sum_R wbi_{ry}} \tag{5b}$$

$$shareRatio_{ry} = \frac{wbiShare_{ry}}{wbShare_{ry}} \tag{5c}$$

where R collects the relevant ethnicities studied. If wages are identically distributed among race/ethnic groups (and minimum wage effects are homogeneous), then we would expect $shareRatio_{ry}$ to equal one for all race/ethnic groups. If individual race/ethnic groups stand to benefit more from minimum wage increases, we would expect their ratios to exceed one, which is what we find in Figure 3.

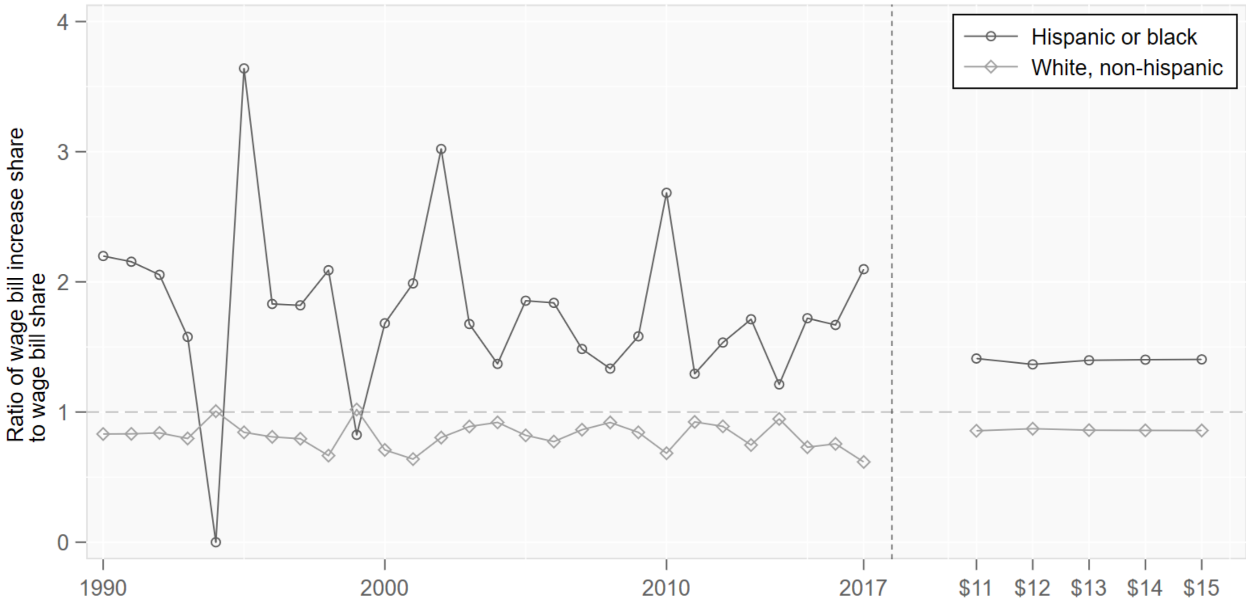
In all years except 1994 and 1999, the projected share of Hispanics and black workers in the total wage bill increase exceeds their share of the total wage bill (ratio >1).[15] The right panel of Figure 3 shows that this would continue to hold if the federal minimum were increased to $15 per hour.[16]

In summary, these descriptive statistics suggest that minimum wage increases should have a modest direct effect on reducing racial and ethnic wage inequality.

[15] Vermont was the only state with a projected minimum wage increase in 1994; and was 99.2 percent white at the time.

[16] We combined the two minorities for readability. Appendix Figure A1 shows that the results hold when we plot the results for Hispanic and black workers separately, albeit with more noise.

Figure 3 Ratio of share in wage bill increase to share in wage bill, by race/ethnicity



Notes: Based on CPS-MORG hourly wages. Left panel shows ratio of wage bill increase share relative wage bill share by racial group. A value above one indicates that the group receives a larger share of the wage bill increase due to minimum wage changes than their initial share in the total wage bill. We use actual minimum wages, but calculate wage bill increases as they would be in a world of perfect compliance but zero wage spillovers or employment effects.

3.2. Causal estimates

In this section, we present our causal estimates. We begin with our main results, which exploit between-state variation in minimum wage changes to examine whether wage effects vary by race and ethnicity. We then extend this analysis to incorporate differences by gender and age. Next, we examine within-individual effects, exploiting the longitudinal feature of the CPS MORGs. We then use entropy balancing to test whether the effects of minimum wage changes result from differences in initial wages. This test addresses the racial neutrality of the policy.

Between-state variation—main results Using the CPS, we leverage state-level variation in minimum wages to evaluate whether minimum wages raise wages and whether such wage effects are heterogeneous by race and ethnicity. We perform separate regressions by race, as per Equation 3, on a sample of workers with a high school diploma or less. The dependent variable y_{it} is either the log hourly wage in 2019 dollars or an employment dummy (one: employed; zero: not employed). The variable of interest is the log minimum wage mw_{st} in state *s*. We add the monthly state unemployment rate $uRate_{st}$ as well as state, age, gender, education and married status dummies to control for individual characteristics.

$$y_{it} = \beta * mw_{st} + uRate_{st} + state_{it} + age_{it} + gender_{it} + education_{it} + married_{it} + spec_{it} + \epsilon_{it}$$

$$\text{(6a) 2FE} \qquad spec_{it} = month_t \qquad (6)$$

$$\text{(6b) Trends} \qquad spec_{it} = month_t + state_{it} * month_t$$

We show the results in Table 5. The earnings coefficients are elasticities (percent change in hourly wage due to a one percent change in minimum wage). Panels A and B display results for the two-way fixed effects (2FE) model, which includes state and month fixed effects. Panels C and D show comparable results for the Trends model, which adds linear state time trends and, following Allegretto et al. (2017), is our preferred model. The wage cutoff, at $20 in 2019 dollars, reduces the share of workers in the sample unaffected by minimum wage policies.

We display results for both the entire sample period 1990-2019 and for the sample period 2002 to 2019. Previous research (Cengiz et al. 2019) has demonstrated that welfare reform and the introduction of the Earned Income Tax Credit in the 1990s and some peculiarities of the 2001 recession can confound minimum wage estimates, especially for teens. We therefore regard the period since 2002 as our preferred sample.

In the 2FE model with all years and no wage cutoff, earnings effects are smaller for black workers. This result reverses if we consider our preferred sample from 2002-2019. Including the wage cutoff in the 2FE model raises the wage elasticities but does not result in any difference among race/ethnicity groups (Panel B).

Table 5 Minimum wage effects on wages, by race/ethnicity

Log hourly wage		All years			2002 to 2019		
		(1)	(2)	(3)	(4)	(5)	(6)
		White	Hispanic	Black	White	Hispanic	Black
A. 2FE	no wage cutoff	0.07***	0.10**	0.04	0.06**	0.06**	0.08**
		(0.03)	(0.04)	(0.04)	(0.02)	(0.03)	(0.04)
	N	942065	209448	138326	352850	115665	49522
B. 2FE	with wage cutoff	0.12***	0.12***	0.10***	0.12***	0.10***	0.12***
		(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
	N	724813	186130	119471	277012	103966	44357
C. Trends	no wage cutoff	0.09***	0.10**	0.18***	0.06**	0.11**	0.10***
		(0.02)	(0.05)	(0.04)	(0.02)	(0.05)	(0.03)
	N	942065	209448	138326	352850	115665	49522
D. Trends	with wage cutoff	0.12***	0.10**	0.17***	0.11***	0.13***	0.13***
		(0.02)	(0.04)	(0.03)	(0.02)	(0.04)	(0.03)
	N	724813	186130	119471	277012	103966	44357

Notes: White refers to non-Hispanic white workers. Wage cutoff specifications exclude workers earning more than $20 in 2019 dollars. Weighted by the outgoing rotation group person-weights. Sample years include either 1990-2019 or 2002-2019. Restgroup consisting of other race/ethnicities not shown in table. Restricted to workers with at most a high school degree. See Appendix Figure A2 for an analysis of pre-existing trends. Data: CPS-MORG. Replication tag: #ptr_microState_base. Standard errors in parentheses, clustered at state level. Stars: * p<0.1, ** p<0.05, *** p<0.01

In our preferred state-time trends models (Panels C and D), all specifications show larger elasticities than in the 2FE models, as well as greater differences in elasticities among all race/ethnic groups. The elasticities for black and Hispanic workers' wages increase more than non-Hispanic white workers' wages. Including the wage cutoff raises the wage elasticities for non-Hispanic white workers without removing the racial differential. In Appendix Table A1 we show that none of the specifications result in negative employment semi-elasticities for any race/ethnicity. We do not find significant pre-trends for any race/ethnicity when we include leads of the minimum wage variable (Appendix Figure A2).[17]

Results by gender and age Table 6 and Table 7 further split our results by gender and age groups for our preferred Trends specification. The wage effects in Table 6 are uniformly larger for women than for men, regardless of sample (Panels A-C) or race (columns 1-3). Moreover, the elasticities for black workers are larger for both genders, indicating that our results are not driven by racial differences in gender composition in the workforce.

The age-specific results in Table 7 follow the expected pattern, with wage effects largest for teens/young adults (16-24 years old) and older workers (55-100). Nevertheless, for black and Hispanic workers we also see meaningful wage increases for adult workers (25-…-54), indicating the gains from minimum wages are widely distributed.

Table 6 Minimum wage effects on wages, by race/ethnicity and gender

Log hourly wage		(1) White	(2) Hispanic	(3) Black
A. All	Male	0.04* (0.02)	0.09* (0.05)	0.13*** (0.04)
	Female	0.14*** (0.02)	0.12** (0.05)	0.22*** (0.04)
	N	942,065	209,448	138,326
B. Wage cutoff	Male	0.08*** (0.02)	0.09** (0.04)	0.14*** (0.03)
	Female	0.14*** (0.02)	0.11*** (0.04)	0.20*** (0.03)
	N	724,813	186,130	119,471
C. Wage cutoff Post 2002	Male	0.07*** (0.02)	0.08** (0.03)	0.08** (0.03)
	Female	0.14*** (0.02)	0.20*** (0.05)	0.16*** (0.03)
	N	277,012	103,966	44,357

Notes: Trends specification only, restricted to workers with at most a high school degree. White refers to non-Hispanic white workers. Wage cutoff specifications exclude workers earning more than $20 in 2019 dollars. Weighted by the outgoing rotation group person-weights. Sample years include either 1990-2019 or 2002-2019. Restgroup consisting of other race/ethnicities not shown in table. Data: CPS-MORG. Replication tag: ptr_microState_gender. Standard errors in parentheses, clustered at state level. Stars: * p<0.1, ** p<0.05, *** p<0.0

Within-individual analysis As an additional test, we leverage the four months in, eight months out, four months in, structure of the CPS MORGs.[18] The earnings questions are always asked in the last of each four months in the survey. As a result, for most respondents we observe wages twice, with a twelve-month gap between the two observations.[19]

[17] A sizeable difference between the earnings elasticities of white (+0.13) and black (+0.17) workers also remains when we include the lead minimum wage variables. For Hispanic workers we find some visual indications of pre-existing trends, although they are never statistically significant at 5 percent.

[18] Ours is the first study to use the longitudinal feature of the CPS MORG data to study minimum wage effects. Less than a handful of studies have used other datasets, such as the Social Security Detailed Earnings Records and SIPP, to conduct such studies. Using linked survey and administrative data, Rinz and Voorhies (2018) find that earnings of low wage workers continue to grow up to seven years after a minimum wage event. However, they do not report results separately by race or ethnicity. They also point to severe measurement issues at the bottom of the wage distribution in SIPP data, reducing its usefulness for minimum wage studies.

[19] By definition, we do not observe their wage if they do not have a job that month.

Table 7 Minimum wage effects on wages, by race/ethnicity and age group

Log hourly wage		(1) White	(2) Hispanic	(3) Black
A. All	16-24	0.16*** (0.02)	0.16*** (0.04)	0.27*** (0.04)
	25-34	0.03 (0.02)	0.10* (0.05)	0.18*** (0.04)
	35-44	0.04 (0.02)	0.07 (0.05)	0.12*** (0.04)
	45-54	0.05* (0.03)	0.09 (0.06)	0.09* (0.05)
	55-100	0.14*** (0.03)	0.12* (0.06)	0.26*** (0.04)
	N	942,065	209,448	138,326
B. Wage cutoff	16-24	0.14*** (0.02)	0.13*** (0.04)	0.20*** (0.03)
	25-34	0.05*** (0.02)	0.09** (0.04)	0.16*** (0.03)
	35-44	0.08*** (0.02)	0.07 (0.04)	0.15*** (0.03)
	45-54	0.10*** (0.02)	0.10** (0.05)	0.12*** (0.04)
	55-100	0.18*** (0.02)	0.12** (0.05)	0.25*** (0.04)
	N	724,813	186,130	119,471
C. Wage cutoff Post 2002	16-24	0.17*** (0.02)	0.18*** (0.04)	0.15*** (0.03)
	25-34	0.05** (0.02)	0.12*** (0.03)	0.09** (0.04)
	35-44	0.06*** (0.02)	0.10** (0.04)	0.13*** (0.03)
	45-54	0.05** (0.02)	0.12*** (0.04)	0.11** (0.04)
	55-100	0.14*** (0.02)	0.16*** (0.05)	0.18*** (0.03)
	N	277,012	103,966	44,357

Notes: Trends specification only, restricted to workers with at most a high school degree. White refers to non-Hispanic white workers. Wage cutoff specifications exclude workers earning more than $20 in 2019 dollars. Weighted by the outgoing rotation group person-weights. Sample years include either 1990-2019 or 2002-2019. Restgroup consisting of other race/ethnicities not shown in table. Data: CPS-MORG. Replication tag: #ptr_microState_age. Standard errors in parentheses, clustered at state level. Stars: * p<0.1, ** p<0.05, *** p<0.0

We compare the wage growth and employment status of workers who experienced a minimum wage change between the wage surveys to those that did not, controlling for their individual characteristics. As this design is based on changes in labor market conditions at the individual level, it is less susceptible to confounding trends than the levels-based approach above. Equation 7 shows the regression setup.

$$\begin{aligned}(hourlyWage_{it}^{B} - hourlyWage_{it}^{A}) = \beta_r * (mw_{st}^{B} - mw_{st}^{A}) \\ + \frac{hourlyWage_{it}^{A}}{medianWage_{st}^{A}} + \left(\frac{hourlyWage_{it}^{A}}{medianWage_{st}^{A}}\right)^2 + race_i \\ + state_{irt} + age_{5irt} + gender_{irt} + married_{irt} + educ_{8irt} + spec_{irt} + \epsilon_{it} \\ \text{(7a) 2FE} \quad spec_{irt} = quarter_{rt} \\ \text{(7b) Trends} \quad spec_{irt} = quarter_{rt} + state_{irt} * month_{rt}\end{aligned} \tag{7}$$

The dependent variable is the log difference in the deflated hourly wages for an individual *i* first observed in month *t*. The superscripts A and B refer respectively to the first observation (in month t) and the second observation (in month t+12). The variable of interest is the change in the minimum wage between the first and second observation. The coefficient has a subscript *r* to indicate we allow the effect to differ by race (non-Hispanic white, black or Hispanic). This notation also applies to the control variables and fixed effects.

The first control variable is the ratio of the individual's initial hourly wage to the state median that month, which enters linearly and squared. This variable captures that workers at different locations in the wage distribution may experience different wage growth even in the absence of a minimum wage change.[20]

The race/ethnicity, state, age, gender, married and education dummies control for individual characteristics of the worker. The $spec_{irt}$ variable adds quarter dummies (2FE model) or quarter dummies plus linear state time trends (Trends model).

Minimum wages affect only those earning low wages. In our main regressions we therefore restrict the sample to workers earning up to 1.5 times the 24-month average minimum wage in the baseline period.[21] We perform placebo tests on workers for whom the ratio falls between 1.5 and 2.5, as well as on workers for whom the ratio exceeds 2.5. These results test the robustness of our results to using different estimation methods and different sample selection criteria.

Table 8 Minimum wage effects on wages and employment, by race/ethnicity
Within-individual estimates

DV: Difference window B - window A		Trends			2FE		
		(1)	(2)	(3)	(4)	(5)	(6)
		< 1.5	1.5-2.5	> 2.5	< 1.5	1.5-2.5	> 2.5
A. Wages	White	0.10***	0.04	0.03	0.11***	0.05*	0.04
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
	Hispanic	0.19***	-0.01	0.00	0.25***	0.04	0.02
		(0.06)	(0.03)	(0.06)	(0.06)	(0.03)	(0.07)
	Black	0.25**	-0.05	-0.00	0.18*	-0.04	-0.01
		(0.10)	(0.06)	(0.05)	(0.10)	(0.06)	(0.06)
	N	155,005	217,282	208,909	155,005	217,282	208,909
B. Employment	White	0.04	-0.01	0.00	0.03	-0.00	0.01
		(0.02)	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)
	Hispanic	0.03	0.05	-0.00	0.01	0.04	0.02
		(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)
	Black	0.31***	0.01	-0.02	0.30***	0.01	-0.02
		(0.11)	(0.07)	(0.04)	(0.10)	(0.07)	(0.04)
	N	230,213	306,494	297,419	230,213	306,494	297,419

Notes: White refers to non-Hispanic white workers. Weighted by the outgoing rotations group person weights. Restgroup of other race/ethnicities not shown in table. The dependent variable in Panel B is a dummy indicating whether the individual *i* was employed at their second observation (month 16 of the CPS). As a result, the dependent variable will be 0 if the respondent is employed in both periods and -1 if they lose their job. Over the entire sample, 18.9% of black workers lost employment in the 12-month window between interviews. Replication tag: #ptr_microInd_baseline. Standard errors in parentheses, clustered at state level, * p < 0.1, ** p < 0.05, *** p < 0.01.

We present earnings results for this within-individuals estimation in the top panel of Table 8. For the sample with wages <1.5 times the minimum wage, we obtain positive earnings elasticities for all race/ethnic groups, both in the Trends (column 1) and 2FE models (column 4). The elasticities are smallest for white workers and up to 2.5 times larger for black and Hispanic workers. For example, in the Trends model, a 10 percent increase in the minimum wage corresponds to one percentage point more wage growth for white workers earning less than 1.5 times the minimum

[20] Our results remain unchanged if we control for the initial wage to median wage ratio using a ten-knot spline instead of the quadratic specification of Equation (7). Results available on request.

[21] $hourlyWage_{it}^{A} < 1.5 * mw_{it}^{A}$, results are similar if we use a different threshold (1.25x) or different smoothing windows (0, 12 or 24 months); see Appendix Table A2.

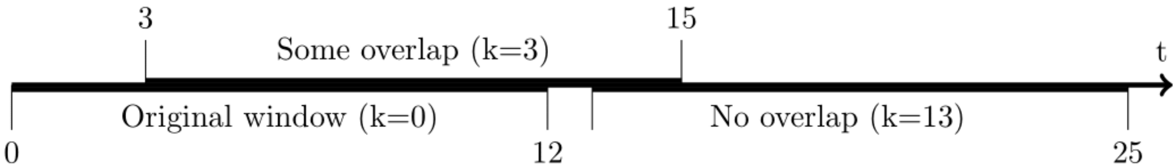
wage, versus 2.5 percentage points more wage growth for black workers. Reassuringly, these effects do not persist as we move up the wage distribution (columns 2, 3, 5 and 6).

The bottom panel of Table 8 displays the probability of remaining employed over the one-year interval. The dependent variable is $\left(employed_{it}^{B} - 1\right)$, where $employed_{it}^{B}$ is a dummy indicating whether the individual *i* was employed at their second observation (month 16 of the CPS MORG). As a result, the dependent variable will be 0 if the respondent is employed in both periods and -1 if they lose their job. We cannot evaluate whether more workers become employed as we select the samples on baseline wages.

Minimum wage increases do not affect the probability of remaining employed for white and Hispanic workers (columns 1 and 4). But for low-wage black workers, a 10 percent increase in the minimum wage *increases* their probability of remaining employed by 3.1 percentage points. We do not find any effects on higher wage workers (columns 2, 3, 5 and 6).[22]

We further test the earnings estimates of the within-individual analysis through a time placebo test, by using leads of the minimum wage change variable to predict wage growth. We replace the variable of interest in Equation (4), $\beta_r * (mw_{st}^{B} - mw_{st}^{A})$ by $\beta_{rk} * \left(mw_{s,t+k}^{B} - mw_{s,t+k}^{A}\right)$ for $k \in 1,2,..,14,15$. If β_r is a causal estimate of the minimum wage effect, then we would expect β_{rk} to be decreasing in k for $k \in [1,12]$ and stable around zero afterwards. We illustrate this concept in Figure 4 where we show the original minimum wage window (k=0), as well as one placebo window with some overlap (k=3) and one without overlap (k=13).

Figure 4 Time placebo illustration



Notes: Illustration of the time placebo concept. Original minimum wage window is shown for t=[0,12]. At k=3, the time placebo still overlaps the original window and could thus pick up actual effects. At k = 13 there is no overlap anywhere and we would expect a zero effect (as we do not expect future minimum wages to affect contemporaneous wages and employment).

As Figure 5 shows, we indeed find positive wage effects for each racial group at the original window (k=0, cf. Table 8), which gradually decline as we shift the window forward. The gaps then stabilize around an insignificant effect that is close to zero, after a shift of 8 to 10 months. These results suggest the within-individual results are not affected by confounding time trends.

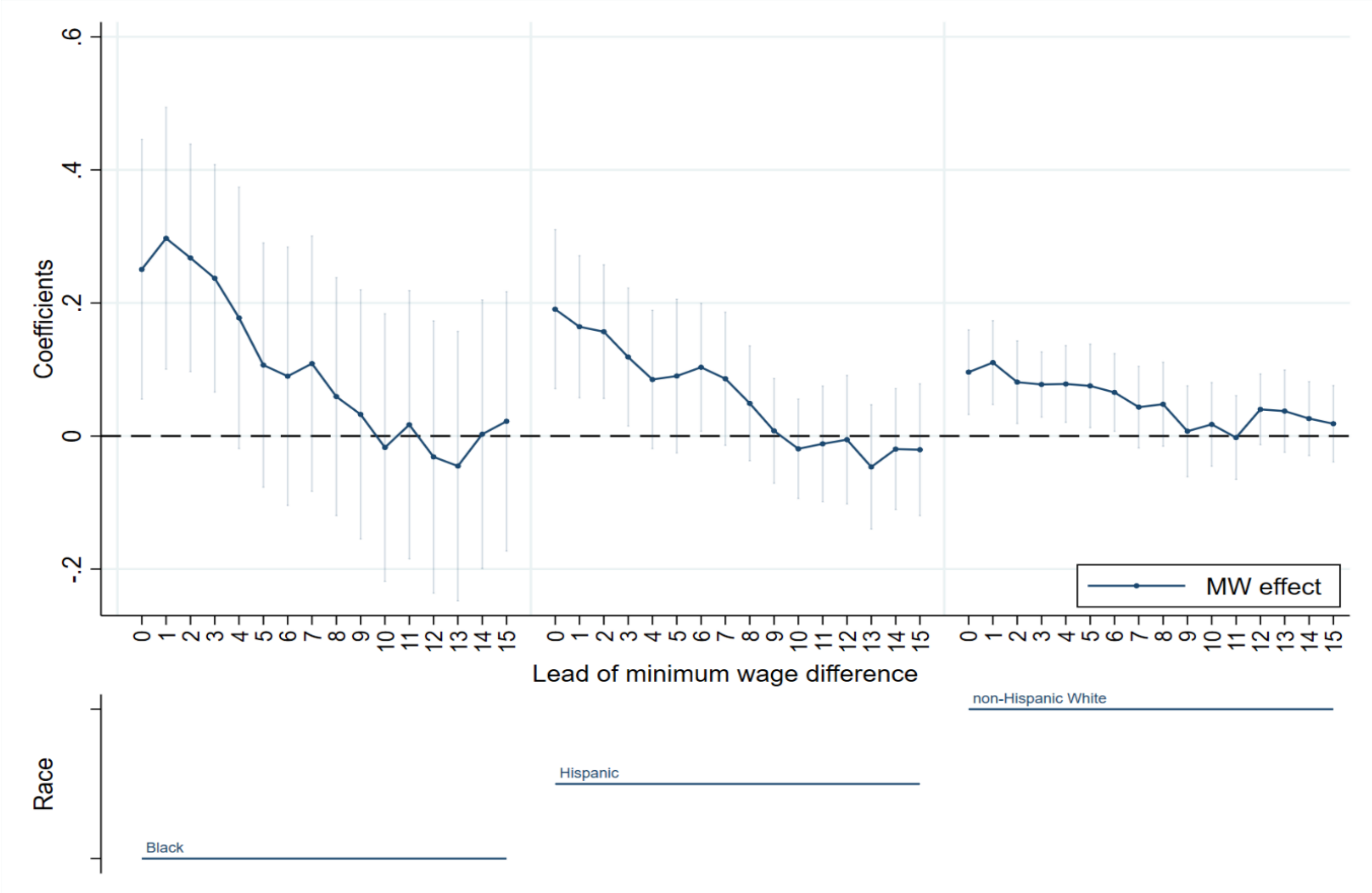
Race neutrality of the minimum wage Table 9 shows that the racial differences in wage gains cannot be explained purely by initial racial wage differences. Column (1) repeats the baseline estimate of Table 8. In column (2) we use entropy balancing (Hainmueller and Xu 2013) to remove differences in average wages among the three racial groups.[23] The sample for columns (3) and (4) excludes race-state-year cells with fewer than five observations so that we can use

[22] Appendix Table A4 shows that these results largely hold when we restrict the sample to 2002-2019, although the positive employment effect for black workers becomes less significant and there are some minor significant effects in the placebo samples.

[23] Specifically, we reweight the Hispanic and black worker observations such that their average initial wage equals that of the white workers in the sample. We do not balance the other covariates as they are either the variable of interest (minimum wage changes) or because our baseline specification is already very flexible in that dimension (e.g. small age group dummies with race-specific coefficients).

entropy balancing to remove average differences at the state-year level. Column (3) shows the results using the initial outgoing rotation group person weights for this reduced sample and column (4) uses the adjusted weights generated by the entropy-balancing routine.

Figure 5 Time placebo test of within-individual earnings effects by race/ethnicity



Notes: Figure shows time placebo test for the estimates of column (1) in Table 8 (within-individual estimates of the effect of minimum wages on earnings growth over one year). The first panel shows the effect for black workers, the second for Hispanic workers and the final panel for non-Hispanic white workers. The x-axis shows the k in $\left(mw^B_{s,t+k} - mw^A_{s,t+k}\right)$. Because we cover a one-year span, positive earnings effects up to 12 months ahead are in line with expectations, whereas effects beyond that would indicate the presence of spurious correlations. The vertical bars show 95 percent confidence intervals (standard errors clustered at the state level).

If the racial differences in wage gains resulted primarily mechanically from racial differences in initial wages, we would expect to find much smaller effects after we balance initial wages by race. Instead, we find that the size and significance of the white-black differential is basically unchanged. This finding holds whether we balance the overall sample or each state-year cell. In other words, minimum wage policy particularly benefits black workers beyond what we would expect, based on the initial wage distribution of black and white workers. These results suggest that black workers receive indirect benefits from the policy.[24] We discuss these indirect effects further in Section 3.3.

[24] This pattern remains if we restrict our earnings measure to either the hourly measure (which excludes tips) or the weekly earnings (divided by usual hours worked) measure, which includes tips. We also conducted entropy-balancing reweighting on subsamples, such as by gender, industry and for High School or Less. The results remained the same.

Table 9 Race neutrality of minimum wage effects on wages

Within-individual estimates

Dependent variable: Wages	(1) Full sample Base	(2) Full sample Overall	(3) State year sample Unadjusted	(4) State year sample Adjusted
White	0.10***	0.10***	0.10***	0.10***
	(0.03)	(0.03)	(0.03)	(0.03)
Hispanic	0.19***	0.20***	0.20***	0.08
	(0.06)	(0.06)	(0.06)	(0.07)
Black	0.25**	0.25**	0.28***	0.24**
	(0.10)	(0.10)	(0.10)	(0.10)
N	155,005	155,005	152,018	152,018

Notes: White refers to non-Hispanic white workers. Column (1) repeats the baseline estimate of Table 8. All columns are based on the trends specification and limited to workers earning < 1.5 times the minimum wage. In column (2) we use entropy balancing to ensure the average wage of all racial groups equals that of white workers. In column (4) we use entropy balancing to ensure this holds per state and year. As some state-year combinations have none or very few observations for certain racial groups, we exclude workers of that race for those state-year combinations. Column (3) shows unbalanced (= weighted by the outgoing rotation group person weights) estimates for that reduced sample. Restgroup of other race/ethnicities not shown in table. Replication tag: #ptr_microInd_raceNeutrality. Standard errors in parentheses, clustered at state level, ∗ p < 0.1, ∗∗ p < 0.05, ∗∗∗ p < 0.01.

Table 10 Minimum wage effects on wages, by race/ethnicity and age

Within-individual estimates, three age groups. 1990-2019

		A. Wages			B. Employment		
		(1) < 1.5	(2) 1.5-2.5	(3) > 2.5	(4) < 1.5	(5) 1.5-2.5	(6) > 2.5
16-24	White	0.12***	0.08	-0.04	0.09***	-0.01	-0.12
		(0.04)	(0.06)	(0.11)	(0.03)	(0.04)	(0.08)
	Hispanic	0.27***	-0.14*	0.17	-0.03	0.08	-0.15
		(0.06)	(0.08)	(0.19)	(0.08)	(0.11)	(0.24)
	Black	0.08	-0.04	-0.33	0.28*	0.17	0.30
		(0.10)	(0.15)	(0.40)	(0.14)	(0.14)	(0.25)
25-54	White	0.09**	0.03	0.03	-0.00	0.00	0.02
		(0.04)	(0.03)	(0.04)	(0.03)	(0.02)	(0.01)
	Hispanic	0.13*	0.02	-0.02	0.06	0.06	0.02
		(0.07)	(0.03)	(0.07)	(0.05)	(0.04)	(0.06)
	Black	0.33***	-0.04	0.05	0.36***	0.01	-0.02
		(0.11)	(0.06)	(0.06)	(0.13)	(0.07)	(0.05)
55-100	White	0.05	0.04	-0.01	-0.03	-0.05	-0.07***
		(0.05)	(0.03)	(0.03)	(0.05)	(0.04)	(0.02)
	Hispanic	0.31***	-0.09	0.11	0.05	-0.01	-0.13
		(0.11)	(0.07)	(0.14)	(0.10)	(0.12)	(0.09)
	Black	0.31	-0.14	-0.15	0.23	-0.15	-0.09
		(0.21)	(0.14)	(0.11)	(0.17)	(0.12)	(0.07)
	N	155005	217282	208909	230213	306494	297419

Notes: White refers to non-Hispanic white workers. Weighted by the outgoing rotations group person weights. Restgroup of other race/ethnicities not shown in table. The dependent variable in Panel B is a dummy indicating whether the individual *i* was employed at their second observation (month 16 of the CPS). As a result, the dependent variable will be 0 if the respondent is employed in both periods and -1 if they lose their job. Controls are interacted by age group, fixed are pooled across age groups. Period: 1990-2019, data: CPS-MORG. Replication tag: #ptr_microInd_age3. Standard errors in parentheses, clustered at state level, ∗ p < 0.1, ∗∗ p < 0.05, ∗∗∗ p < 0.01.

Within-individual effects by gender and age Appendix Table A7 shows that the within-individual minimum wage effects are similar across genders, with two exceptions. The positive earnings effect for white workers appears concentrated among women (+0.11 vs +0.07), whereas the positive employment effect for black workers is particularly pronounced for men (+0.41 vs +0.25).[25]

Table 10 shows within-individual results by age groups for our preferred Trends specification. We collapse ages to three groups (youth: 16-24, adults: 25-54, older adults: 55-100) due to sample size constraints.[26] Again, we find that adult black workers particularly benefit from minimum wage increases, whereas among white workers the benefits are concentrated among younger workers. We find positive employment retention effects for black workers of all ages (at differing significance levels) and for white youths.

Event study We also implement an event study analysis at the state-quarter level. We use the outgoing rotation group person weights to aggregate the individual level CPS data, restricting the sample to workers with at most a high school diploma who earn less than $20 (2019 dollars). The event study design follows Cengiz et al. (2019) and Godoey et al. (2019). We define treatment events as all minimum wage increases exceeding five percent that do not coincide with federal minimum wage increases. The control states for each event are then those states that do not experience such a qualifying minimum wage increase during the event window (3 years prior, 4 years post).

This procedure yields 143 events between 1990 and 2019. For each event e, we define the event moment as t_e and group the event time indicators by year relative to the event:

$$\begin{array}{lrl} & \tau_{-12} = & 1\ if\ t \in [t_e - 12, t_e - 9] \\ \text{Pre} & \tau_{-8} = & 1\ if\ t \in [t_e - 8, t_e - 5] \\ & \tau_{-4} = & 1\ if\ t \in [t_e - 4, t_e - 1] \\ \text{Contemporaneous} & \tau_0 = & 1\ if\ t \in [t_e, t_e + 3] \\ & \tau_{+4} = & 1\ if\ t \in [t_e + 4, t_e + 7] \\ \text{Post} & \tau_{+8} = & 1\ if\ t \in [t_e + 8, t_e + 11] \\ & \tau_{+12} = & 1\ if\ t \in [t_e + 12, t_e + 15] \end{array} \tag{8a}$$

We interact those event time indicators with the size of the log minimum wage increase, Δmw_e, and an indicator variable identifying the treated state in each event, I_{es}. We include event specific state and quarter fixed effects:

$$y_{est} = \delta_{es} + \delta_{et} + \sum_k \left(\beta_k * \tau_{ke} * \Delta_{\mathrm{mw}_e} * I_{es}\right) + controls_{est} + \epsilon_{est} \tag{8b}$$

where k iterates over the event time indicators defined in Equation (8a). All variables include a subscript e to indicate we stack observations per event and $controls_{est}$ includes education, age, married status and gender shares as well as three indicator variables equal to one if in quarter t state s has experienced respectively a qualifying, small or federal minimum wage change in the past 4 years.[27] We omit τ_{-4}, as the coefficients β_k are only identified relative to each other. The

[25] Based on Appendix Table A7, where we allow for gender-specific coefficients for the minimum wage and the control variables (unemployment rate and the wage to median wage ratios) but pool the fixed effects to avoid saturating the model entirely. We show complete split sample results in Appendix Table A8 and Appendix Table A9. The results are qualitatively similar, except for the placebo tests. For example, we find significant wage effects in the 1.5-2.5 group for black workers.

[26] Appendix Table A10 shows that results based on five groups are too noisy to be useful.

[27] We include an indicator for qualifying minimum wage events to account for treated states that experience multiple qualifying events in one event window. A small minimum wage change is one that does not meet the 5 percent threshold.

dependent variable y_{est} is either the average log hourly wage or the share of the sample that is employed in state s at quarter t.

Table 11 Minimum wage effects on wages and employment.
Event study estimates. 1990-2019

		White	Hispanic	Black
Wages	Pre	0.02 (0.03)	0.06 (0.07)	0.05 (0.09)
	Post	0.11 (0.05)**	0.15 (0.07)**	0.15 (0.08)**
	N	115353	108605	101191
Employment	Pre	0.00 (0.03)	-0.04 (0.06)	-0.06 (0.09)
	Post	-0.02 (0.03)	-0.02 (0.08)	0.03 (0.09)
	N	115413	113038	109337

Notes: White refers to non-Hispanic white workers. Coefficients show the average effect over respectively the pre- and post- period. Results are based on 143 events and include controls for education, age, marital status and gender shares as well as three indicator variables equal to one if in quarter t state i has experienced respectively a qualifying, small or federal minimum wage change in the past 4 years. Period: 1990-2019, data: CPS-MORG. Replication tag: #ptr_eventStudy. Standard errors in parentheses, clustered at the state level, * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 11 shows results averaged over respectively the pre- and post-periods.[28] Reassuringly, we do not find any significant effects in the pre period, not do we find any negative employment effects. In line with previous results, we do find that wages increase for all racial groups and that they increase more for Hispanic (+0.15, se 0.07) and black workers (+0.15, se 0.08) than for white workers (+0.11, se 0.05).[29]

Bunching estimator Finally, we adapt the bunching estimator described in Cengiz et al. (2019) to illustrate differences in the pattern of earnings gains per race. Figure 6 shows the estimated employment and wage bill effects by wage bins. The underlying data are the same CPS-MORG dataset that we used in our previous analyses, but now binned into 25 cent groups, from $1.25 to $30 and two endpoint bins: [0, 1.25] and [30, +∞].

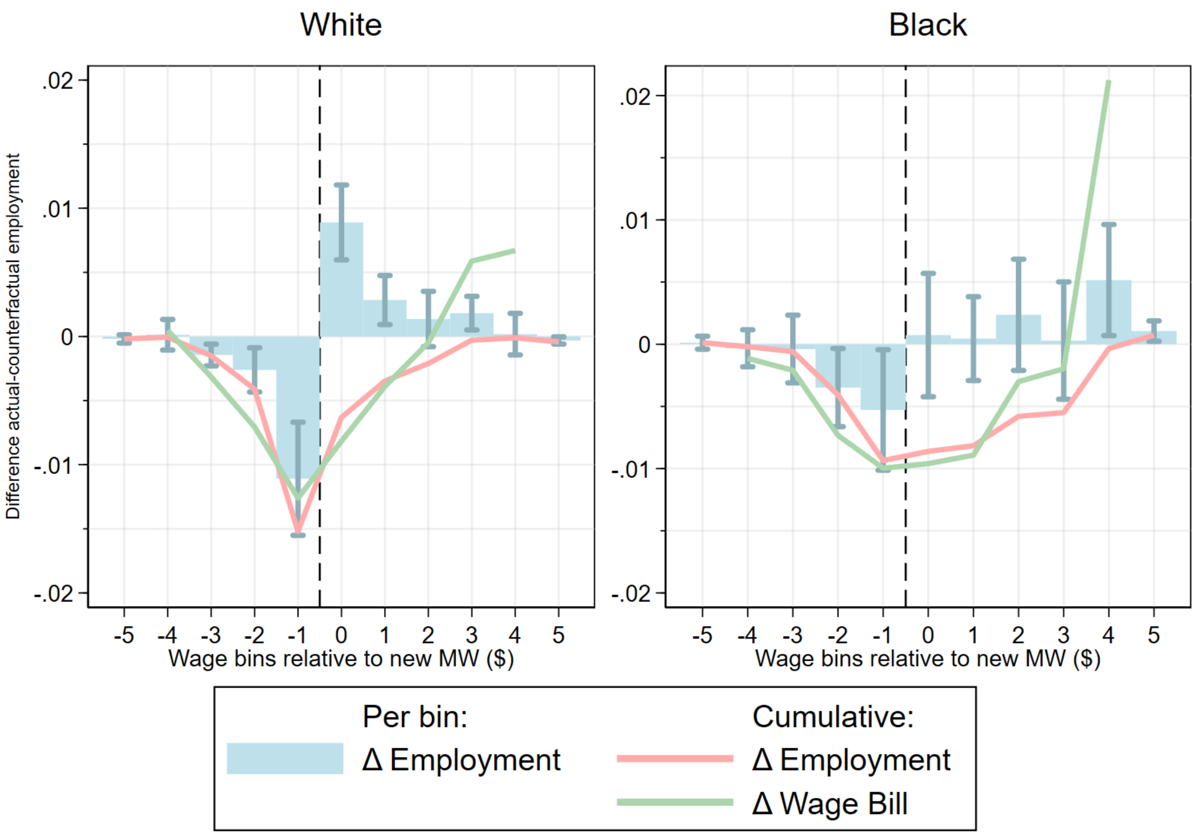
We generate treatment dummies that turn to one if bin j is within k dollars of a new state minimum wage. We follow the Cengiz et al. (2019) definition of suitable minimum wage events, but restrict k to [-4, 4[rather than [-5, 17[to improve efficiency (see also Equation 1 in their paper). We control for federal and small minimum wage effects and include bin-by-quarter and bin-by-state fixed effects. We omit the pre-treatment year dummies; thus, all effects are measured relative to the year before the event. For further details, see the notes to Figure 6 and Cengiz et al. (2019).

We plot the coefficients averaged over the entire post period. These coefficients can be interpreted as the percentage point difference (relative to the average over the control states) in the change (relative to the previous year) of the employment-population ratio for each bin. The red line shows the cumulative employment effects over the bins.

The green line shows the contribution of each relative wage bin to the total wage bill change, normalized by population (see Appendix B and the notes to Figure 6 for more details). For white workers we find similar results to Cengiz et al. 2019, with missing jobs concentrated in the dollar bin just below the new minimum wage, and excess jobs in the dollar bin just above. The total employment effect is indistinguishable from zero.

[28] The post period includes the contemporaneous effect.
[29] See Appendix Figure A4 for results per event time.

Figure 6 Employment and wage effects by bin



Notes:
Specification
This figure shows estimated employment and wage bill effects by wage bins. The underlying data are the same CPS-MORG dataset used in our other analyses, but now binned into 25cent groups, from $1.25 to $30 and two endpoint bins: [0, 1.25[and [30, +∞[. We generate treatment dummies that turn to one if bin *j* is within *k* dollars of a new state minimum wage. We follow the Cengiz et al. (2019) definition of suitable minimum wage events, but restrict *k* to [-4, 4[rather than [-5, 17[to improve efficiency (see also Equation 1 in their paper). We add dummies -5 and 5 to collect bins outside this range. We include up to three year leads and four-year lags to capture dynamic effects (graph available on request, pre-treatment trends are absent). We control for federal and small minimum wage effects as in Cengiz et al. (2019), with the distinction that we include separate control dummies for the -5, [-4, 0[, [0, 4[and 5 groups rather than just the middle bins. We follow their specification by including bin-by-quarter and bin-by-state fixed effects as well as dummies for the number of treatment events occurring over the [-3, 4] year window. We omit the pre-treatment year dummies, thus all effects are measured relative to the year before the event. We plot the coefficients averaged over the entire post period, multiplied by four (as the underlying bins are 1/4th of a dollar) but do not divide by pre-treatment employment-population ratios as they do. As such, the coefficients are to be interpreted as the percentage point difference (relative to the average over the control states) in the change (relative to the previous year) of the employment-population ratio for each bin. The red line shows the cumulative employment effects over the bins, the green line the cumulative effect on the wage bill, where the wage bill effect is defined as the wage at the midpoint of the bin times the employment-population ratio change (this line is not included in Cengiz et al, 2019 and differs from their wage bill calculations; we exclude the endpoint bins from these calculations).

Results
For white workers, we find very similar results as Cengiz et al. 2019, with missing jobs concentrated in the dollar bin just below the new minimum wage and excess jobs in the dollar bin just above. The total employment effect is indistinguishable from zero. For black workers, the missing jobs are spread over the two bins below the new minimum wage and the gains are focused on the higher +$4 bin. As a result, the wage bill effect is much larger for black workers, in line with our findings using other methods. The total employment effect remains zero. Note that if the minimum wage were wage neutral, we would expect to find the same bin pattern for white and black workers. By contrast, we observe a concentration of excess jobs in the high relative bin for black workers, consistent with the findings and mechanisms described in Section 3.2.

For black workers, the missing jobs are spread over the two bins below the new minimum wage and the gains are focused on the higher +$4 bin. As a result, the wage bill effect is much larger for black workers, in line with our findings using other methods. The total employment effect remains zero. Note that if the minimum wage were race neutral, we would expect to find the same bin pattern for white and black workers. By contrast, we observe a concentration of excess jobs in the high relative bin for black workers, consistent with the findings and mechanisms described before.

Summary of causal results Overall, we find that minimum wages policies have stronger earnings effects for black workers than for white workers and that these differences cannot be explained by initial wage differences.

4. Indirect Effects

Overview In this section we argue that these disproportionate gains are driven by higher minimum wages improving the financial situation of black workers, increasing their credit ratings and access to car loans. In turn, car ownership allows them to expand their search/commute radius, leading to more favorable and stable matches with employers.

We formalize this mechanism through a wage determination model with two types of workers, in the style of Card, Cardoso, Henning and Kline (2018). We present this model in Appendix C. The two worker types differ in their outside options, which are themselves a function of their job mobility possibilities. Job mobility options are determined by distance and disutility of commuting to potential jobs and by employer discrimination. Black workers are disproportionately low outside option workers, as they live predominantly in central cities and have fewer mobility options (Miller 2018), whereas higher quality job opportunities are concentrated in the suburbs. Minimum wage increases allow some of them to buy a car, turning those into high outside option workers with even larger earnings increases.

To motivate the pertinency of this model, we first review existing literature on the racial disparities in commuting behavior and access to credit. We then present new empirical evidence on the effect of minimum wage policies on commuting modes and job stability by race.

Literature A series of studies has shown that black workers are more likely to live in central cities, where wages are lower, less likely to own automobiles and therefore more constrained in their job searches. In the 2000 Census blacks constituted 20 percent of central city residents and 9 percent of suburban residents (Albouey and Lue 2015). Moreover, predicted wages (holding education, race, gender, occupation, industry, veteran, marital and immigrant status constant) were 4 percent below average in central cities and 4 percent above average in suburban areas. The 8 percent wage central city-suburban wage differential is likely a lower bound for black workers. According to Raphael and Stoll (2001), in 1995 only five percent of white households did not possess an automobile, compared to 24 percent of black households. Kawabata (2003) finds that commute times averaged 24 minutes by automobile versus 48 minutes by public transit. Job accessibility via automobiles, as measured in commute times is thus greater than when measured by distance. As a result, white males' search distance for jobs averages twice as far as for black males, yet black males spend longer traveling to work than do white males (Holzer et al. 1994).

Minimum wages help overcome these transportation constraints. Cooper, Luengo-Prada and Parker (2020), using data on 28 metro areas, find that minimum wages led workers to acquire automobiles, including by relaxing credit constraints, with larger effects among the credit-constrained. They also find that a 10 percent increase in the minimum wage generated a substantial increase in successful credit card applications, and an 8.6 percent increase in automobile debt (which reverses in subsequent years), confirming the results in Aaronson et al. (2012). Minimum wage increases also reduced debt among sub-prime borrowers. Relatedly, Baum (2009) shows that vehicle ownership for single mothers with a high school education or less reduced spatial isolation from employment opportunities and thereby improved employment outcomes. Cho (2020) examines the effects of state-level reforms since 2013 that permitted undocumented immigrants to acquire driver's licenses.[30]

The increased earnings from minimum wages and the resulting improved credit ratings allow black workers to buy vehicles and become less geographically limited in their job search. This increase in geographic mobility improves their set of outside options and thus their bargaining position, allowing them to extract a larger share of the surplus they create (Raphael and Riker 1999; Johnson 2006; Stoll and Covington 2012). As such, the minimum wage allows black workers to escape the poverty trap created by their lack of access to decent outside options. [31]

Empirical evidence on commute patterns We supplement these findings from the literature with new evidence from the American Community Survey (ACS) on the minimum wage effects on commuting patterns. Table 12 shows the effect of minimum wages on the share of workers that commute by car for ages 26-35. [32] We restrict the sample to workers with at most a high school diploma. Each cell represents a separate regression of the form,

$$\begin{aligned} &carShare_{sy}^{aqr} = \beta^{aqr} * mw_{sy} + \gamma * uRate_{sy} + spec_{sy}^{aqr} + \epsilon_{sy}^{aqr} \\ &\text{(8a) 2FE} \qquad spec_{sy} = year_y \\ &\text{(8b) Trends} \qquad spec_{sy} = year_y + state_s * year_y \end{aligned} \tag{8}$$

where $carShare_{sy}^{aqr}$ is the share of workers in age group a, household income quartile q and race r that commutes by car in state s and year y. We obtain individual transportation mode data from the American Community Survey's Journey to Work component (accessed through Flood et al. 2020), which we collapse to the share commuting by car per age, income quartile, race group, state and year. Income quartiles are based on total household income and calculated separately per state and year (but joint over age and race groups). We exclude those who work from home. The minimum wage enters in logarithms and is defined as the highest effective minimum wage occurring per state-year.

We find significant and positive coefficients for the poorest black workers, of +0.06* and +0.10** for the 2FE and Trends specifications, respectively. This latter result implies that a ten percent increase in the minimum wage leads to a one percentage point increase in the share of relevant workers commuting by car. This is a sizeable effect, since on average 18 percent of these workers

[30] Cho first shows the reforms increased vehicle ownership and increased the probability of commuting by car. He finds large increases in employment rates. especially after two years. Then, using occupational data, he constructs an index of car-dependency by job and shows that such jobs are higher paying. A 10 percent increased probability of a car commute increases wages by about 10 percent, in part because immigrants obtain more of the car-dependent jobs than before.

[31] One might expect minimum wage increases to also permit low-wage workers to relocate to better residences or to neighborhoods with greater job opportunities. However, Cooper et al. do not examine this channel. More generally, the Moving to Opportunity literature (Bergmann et al. 2020) does not find that such moves affect adult employment outcomes.

[32] We show results for ages 26-35 as this is the group most likely to contain a sizeable number of workers that would like to own a car, but cannot currently afford one, and that are sensitive to limited changes in financial and borrowing conditions. Results for broader age groups are available in Appendix Table A5 and Appendix Table A6. Including younger workers leads to similar, but muted patterns (ages 21-35). Among older workers the effect of minimum wages on commuting patterns is too weak to be detected (ages 21-65) as the group of workers who already own cars outweighs those sensitive to the policy (see Appendix Figure A3).

do not commute by car. The effect decreases in size and significance for black workers in higher family income quartiles. We do not find any significant effects for white workers, consistent with our hypothesis that this channel is mainly relevant for black workers.

Table 12 Minimum wage effects on share of workers that commutes by car

Ages 26-35; based on American Community Survey, 2000-2019

	Black		White	
	(1)	(2)	(3)	(4)
	2FE	Trends	2FE	Trends
Poorest Quartile	0.06* (0.03)	0.10** (0.04)	0.00 (0.01)	0.03 (0.02)
3rd Quartile	0.05 (0.05)	0.08* (0.04)	-0.00 (0.01)	0.01 (0.02)
2nd Quartile	-0.03 (0.05)	0.01 (0.06)	-0.01 (0.01)	0.01 (0.01)
Richest Quartile	-0.25 (0.19)	0.01 (0.15)	-0.04 (0.02)	0.01 (0.03)

Notes: White refers to non-Hispanic white workers. Weighted by the ACS person weights. Each number originates from a separate regression. Observation numbers omitted for brevity, ranging from 724-1020 state-year level observations (some regressions have fewer than 1020 observations because there are no workers of that age, race and wealth level for some state-years). Quartile refers to the household income quartile, defined separately for each state and year. 2FE refers to a two-way fixed effects specification with state and year fixed effects, the Trends specification adds linear state-specific time trends. We control for the unemployment rate at the state and year level. Period: 2000-2019, data: ACS. Replication tag: #ptr_acs_2635. Standard errors in parentheses, clustered at state level, ∗ p < 0.1, ∗∗ p < 0.05, ∗∗∗ p < 0.01.

Empirical evidence on job stability If minimum wage increases lead to improved matches between black workers and jobs, we expect to find separation rates to fall. We turn to the Quarterly Workforce Indicators dataset to test whether minimum wage increases lead to more stable matches by evaluating their impact on hiring and separation rates. The QWI dataset provides employment stocks, earnings, and employment flows per county, quarter and race/ethnicity. We base our regression equations on Dube et al. (2016),

$$y_{ct} = \beta * mw_{st} + \gamma_0 * pop_{ct} + \gamma_1 * emp_{ct}^{TOT} + \gamma_2 * y_{ct}^{TOT} + county_c + spec_{ct} + \epsilon_{ct}$$
$$\begin{array}{ll} \text{2FE} & spec_{ct} = quarter_t \\ \text{Trends} & spec_{ct} = quarter_t + state_s * quarter_t \\ \text{CBCP} & spec_{ct} = quarter_t * pair_{ipt} \end{array} \quad (9)$$

where y_{ct} equals the natural log of average monthly earnings, employment headcount, hiring rate, separation rate or turnover rate for county c in quarter t in the food services sector (NAICS 722). pop_{ct} is the log of county population, emp_{ct}^{TOT} the log of total private sector employment, y_{ct}^{TOT} the total private sector equivalent of the dependent variable (e.g. log of total private sector hiring rate) and $county_c$ are county dummies.[33] As before, we test two specifications, the two-way fixed effects model (2FE), which adds quarter dummies and the trends model (Trends) which further adds linear state specific time trends. In Appendix Table A11 we also show results for the Cross Border County Pair (CBCP) model (cf. Dube et al. 2016), which limits identification to comparisons of counties across borders through county pair-quarter dummies. This also modifies the sample as each county-quarter observation is duplicated for each pair it belongs to.

We show elasticities with respect to the minimum wage in Table 13. We restrict our discussion to the results from the Trends model (columns 1-3) and show effects for the 2FE model in columns 4 to 6 for completeness. As before, we find that earnings of black and Hispanic workers

[33] To remain consistent with the other approaches we do not incorporate local minimum wages in the minimum wage variable in our baseline results. They are nevertheless fully robust when they are included.

react more strongly to the minimum wage than those of non-Hispanic white workers (+0.31 and +0.26 vs +0.21, Panel A). We do not find any employment effects in the Trends model.[34] We do find strong responses of the hiring, separation and turnover rates, which decline for all workers, but almost twice as much for black workers (Panels C-E).[35] For example, a 10 percent increase in the minimum wage reduces the separation rate of non-Hispanic white workers by 1.8 percent versus a 4.1 percent reduction for black workers.[36] Reduced turnover among black workers is consistent with studies that suggest enhanced job matching among black workers when they can extend their searches in distance and time (Raphael and Riker 1999; Pager and Pedulla 2015).

Table 13 QWI-based estimates of minimum wage effects on earnings, employment and employment flows, by race/ethnicity, 1990-2017

		Trends			2FE		
		(1) White	(2) Hispanic	(3) Black	(4) White	(5) Hispanic	(6) Black
A. Earnings	MW	0.21***	0.26***	0.31***	0.26***	0.26***	0.30***
		(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
	N	184766	184766	184766	184766	184766	184766
B. Employment	MW	-0.04	-0.02	-0.01	-0.16***	-0.21	0.08
		(0.02)	(0.04)	(0.04)	(0.05)	(0.16)	(0.07)
	N	184965	184965	184965	184965	184965	184965
C. Hiring rate	MW	-0.18***	-0.20**	-0.41***	-0.22***	-0.33**	-0.51***
		(0.06)	(0.08)	(0.10)	(0.05)	(0.13)	(0.11)
	N	159053	155376	154903	159053	155376	154903
D. Separation rate	MW	-0.21***	-0.19**	-0.40***	-0.25***	-0.33**	-0.53***
		(0.07)	(0.09)	(0.10)	(0.06)	(0.13)	(0.11)
	N	158471	154726	155054	158471	154726	155054
E. Turnover rate	MW	-0.19***	-0.19**	-0.40***	-0.23***	-0.32**	-0.51***
		(0.07)	(0.08)	(0.10)	(0.06)	(0.13)	(0.11)
	N	152227	152220	152219	152227	152220	152219

Notes: White refers to non-Hispanic white workers. Unweighted. Restgroup of other race/ethnicities not shown in table. All dependent variables are in logs. Replication tag: #ptr_qwi_base. Standard errors in parentheses, clustered at state level, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The strong decline in the separation rate accords with the positive employment effects found using the individual CPS MORG data (Table 8), which found that black workers were more likely to remain employed after minimum wage increases. The absence of an employment effect in Table 13 is compatible with those results as it is more sensitive to the proportionate decline in the hiring rate than the CPS MORG estimates.

[34] See Dube et al. (2016) for a discussion of the significantly negative effect found in the 2FE model.

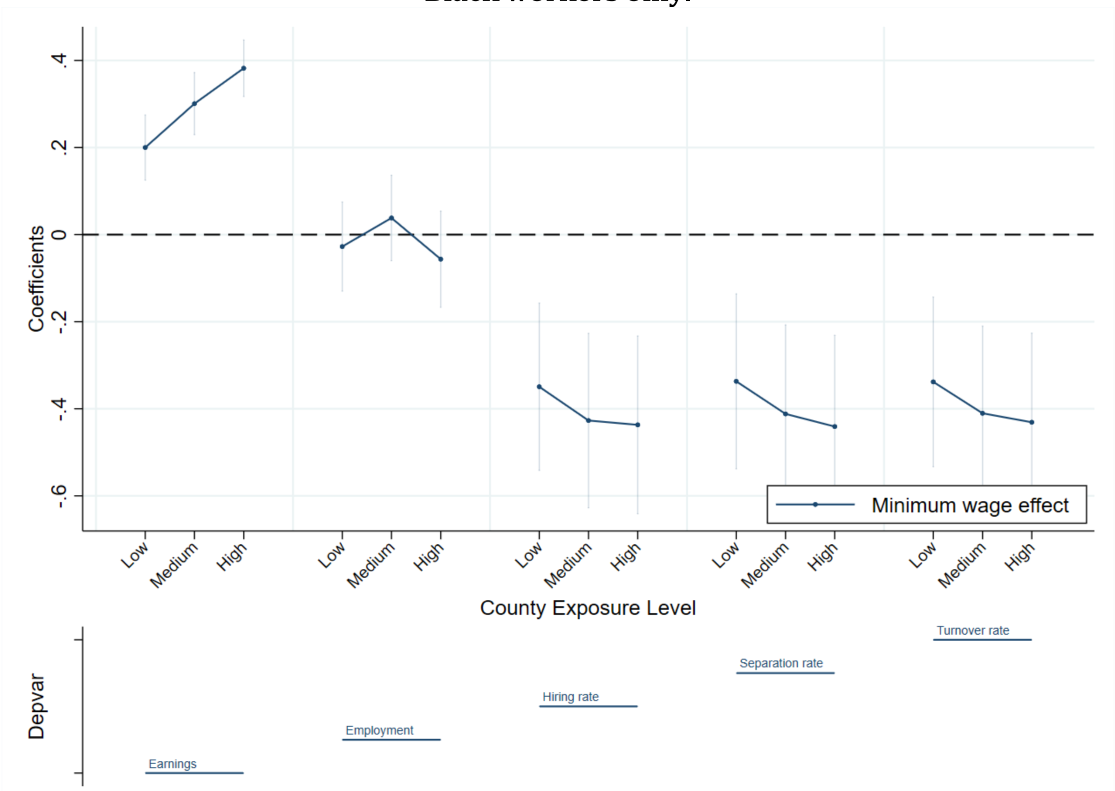
[35] The wage, hiring and separation elasticities for white workers in Table 13 are virtually identical to the findings in Dube, Lester and Reich (2016), Table 3, for teens and restaurant workers.

[36] In Appendix Table A11 we show that these patterns also hold when we compare counties across borders. The exception is that we do find a significant disemployment effect for Hispanics (-0.14, se 0.06), which is at odds with the results from the other methods.

As a robustness check, we interact the minimum wage variable with the county's potential effects from minimum wage policy (Figure 7, black workers only). Wursten (2020) defines three exposure levels based on the estimated share of food services workers (NAICS 722) that would be affected by a hypothetical 10 percent increase in the minimum wage: a low group (0-15 percent), a medium group (15-50 percent) and a high exposure group (50-100 percent). As expected, we find that earnings, hiring, separation and turnover effects are stronger in more exposed counties whereas employment effects are flat over the exposure distribution. Appendix Figure A5 shows that elasticities for black workers are also larger within each exposure group, indicating these results are not driven by differences in potential impact levels between white and black workers.

Summary Overall, these findings support our hypothesis that a sizeable share of low-wage black workers find themselves stuck in a poverty trap due to their inability to reach better paying jobs. Minimum wage policies improve their financial situation, allowing them to escape this trap by increasing their commute options and thus giving them access to a wider range of outside options. This is reflected in wage increases that exceed the mechanical effect of the wage floor, an increasing share of low wage black workers that commutes by car and a reduction in job turnover.

Figure 7 QWI-based estimates of minimum wage effects by county susceptibility. Black workers only.



Notes: Figure shows estimates of column (3) in Table 13 by county exposure levels. Low exposure refers to counties where we would expect less than 15 percent of food services workers to be affected by a hypothetical 10 percent minimum wage increase, versus 15-50 percent [medium] and 50-100 percent [high]. We refer to Wursten (2020) for more details. White refers to non-Hispanic white workers. The vertical bars show 95 percent confidence intervals (standard errors clustered at the state level).

Regression equation$y_{ct} = \sum_e \beta_e * I[E_c = e] * MW_{st} + controls_{ct} + \epsilon_{ct}$ where E_c is the exposure level of county c.

5. Counterfactual design

We next conduct a counterfactual simulation, based on the between-state analysis. The results from Table 5 allow us to estimate how minimum wages affect the hourly wage gap between white and black workers as per Equation 10c,

$$Actual\ gap_y = \sum_{\substack{i\in W \\ t=y}} E[hourlyWage_{it}|X, MW_{it} = MW_{it}] - \sum_{\substack{i\in B \\ t=y}} E[hourlyWage_{it}|X, MW_{it} = MW_{it}] \tag{10a}$$

$$Counterfactual\ gap_y = \sum_{\substack{i\in W \\ t=y}} E\big[hourlyWage_{it}\big|X, MW_{it} = MW_{i,1995}\big] - \sum_{\substack{i\in B \\ t=y}} E\big[hourlyWage_{it}\big|X, MW_{it} = MW_{i,1995}\big] \tag{10b}$$

$$Effect\ MW\ on\ gap_y = Counterfactual\ gap_y - Actual\ gap_y \tag{10c}$$

where W collects all white workers, B collects all black workers and X collects all worker characteristics except the minimum wage. $Actual\ gap_y$ is the estimated hourly wage gap between white and black workers in year y at actual minimum wage evolutions [37], $Counterfactual\ gap_y$ is the gap if minimum wages had remained at 1995 levels and $Effect\ MW\ on\ gap_y$ is the difference between the two.[38]

The bottom half of Figure 8 shows the evolution of the actual and counterfactual gap for workers with at most a high school diploma and hourly wages below \$20 (2019 dollars), the same sample observed in Table 5. In 1995, white workers in this selected sample earned 4.4 percent more than black workers with similar characteristics. By 2019, this gap has halved to 2.2 percent. Without minimum wage policy the gap would actually have increased, to 5.2 percent. We also simulate a second scenario in which the federal minimum wage follows California's path to \$12.

The top half of Figure 8 shows how the decline in the gap in this selected sample translates to the sample of all workers. In 1995, the hourly wage gap between all white and black workers was 9.7 percent. By 2019, the gap widened to 15.3 percentage points. Without minimum wage policies, we estimate the gap would have been 17.4 percentage points (fourteen percent larger). This estimate is based on the conservative assumption that workers outside the selected sample are completely unaffected by minimum wage policy.[39] If instead this group is also affected and shows similar race/ethnicity heterogeneity patterns, the inequality-reducing effect of the minimum wage would have been even larger. The size of the effect is similar to the 16 percent reduction found in Derenoncourt and Montialoux (2021). The second scenario shows that the gap would have been even smaller had the federal wage been increased to \$12.

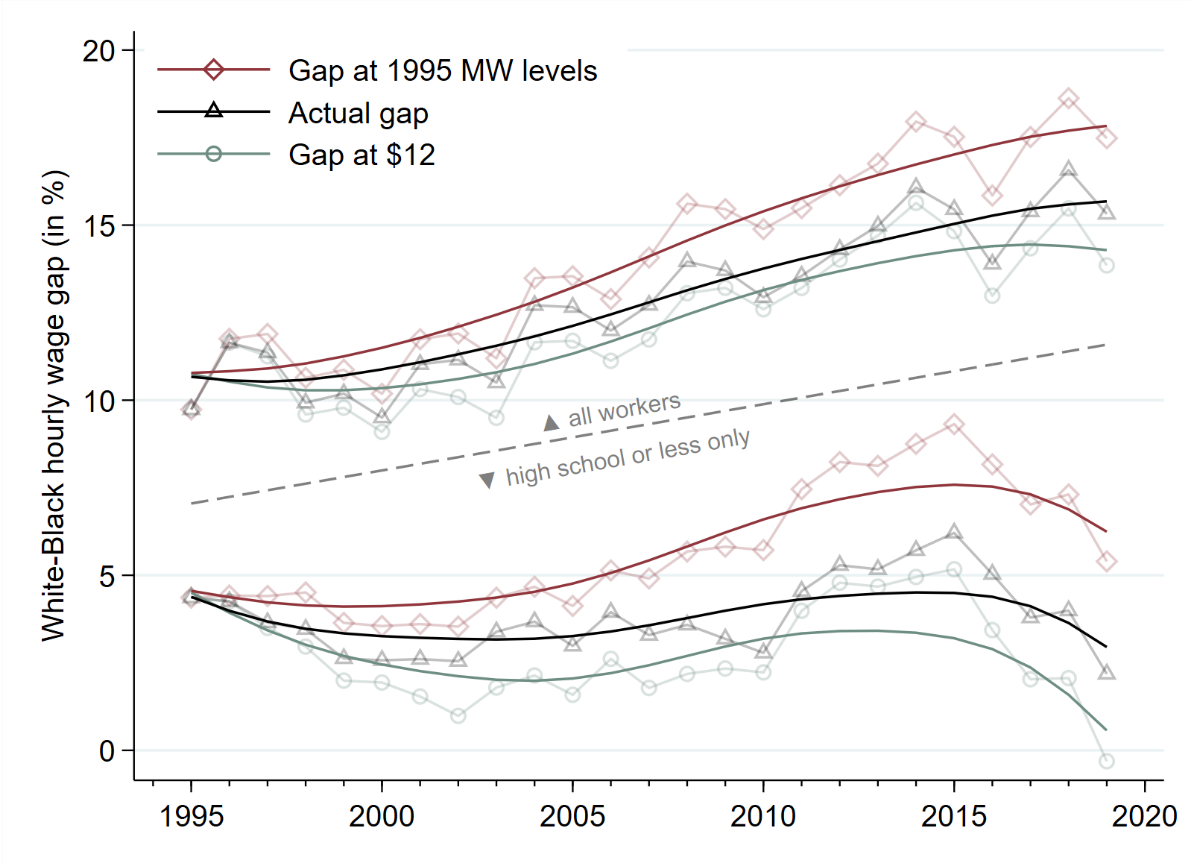
In Appendix Figure A6 we simulate a third scenario in which the federal minimum wage follows the "Raise the Wage Act of 2019" schedule, increasing to \$15 by 2025. We assume worker characteristics do not change relative to 2019 and that all groups have the same real wage growth

[37] Because of the mean zero error assumption in OLS and the inclusion of racial intercepts, the actual gap based on the regression predictions equals the actual gap based on observed values.

[38] We begin in 1995 because of unusual noise in our results for 1990 through 1994, caused perhaps by the CPS revisions of the educational attainment and employment questions during that period.

[39] The unaffected group includes those with at least some college education or an hourly wage above \$20 (2019 dollars).

Figure 8 Counterfactual white vs black worker hourly wage gap.
Based on between-state regressions.



Notes: Figure shows observed (black) and two counterfactual (red, teal) hourly wage gaps between white and black workers over 1995-2019. The first counterfactual gap predicts hourly wages for white and black workers as if there had been no changes in the minimum wage from 1995 onwards (red, diamonds). The second counterfactual gap assumes federal minimum wages follow California's path to $12. The bottom sample is workers with at most a high school degree and an hourly wage below $20 (2019 dollars), the top sample is all workers. We took the difference between the mean of log wages by race/ethnicity – the y-axis can be interpreted as the percentage gap of black workers versus white workers. Replication tag: #cf_history.

except for the minimum wage effect. As before, all changes occur through the HSOL sample. We find an additional 2.2 percentage point reduction in the overall wage gap by 2025.

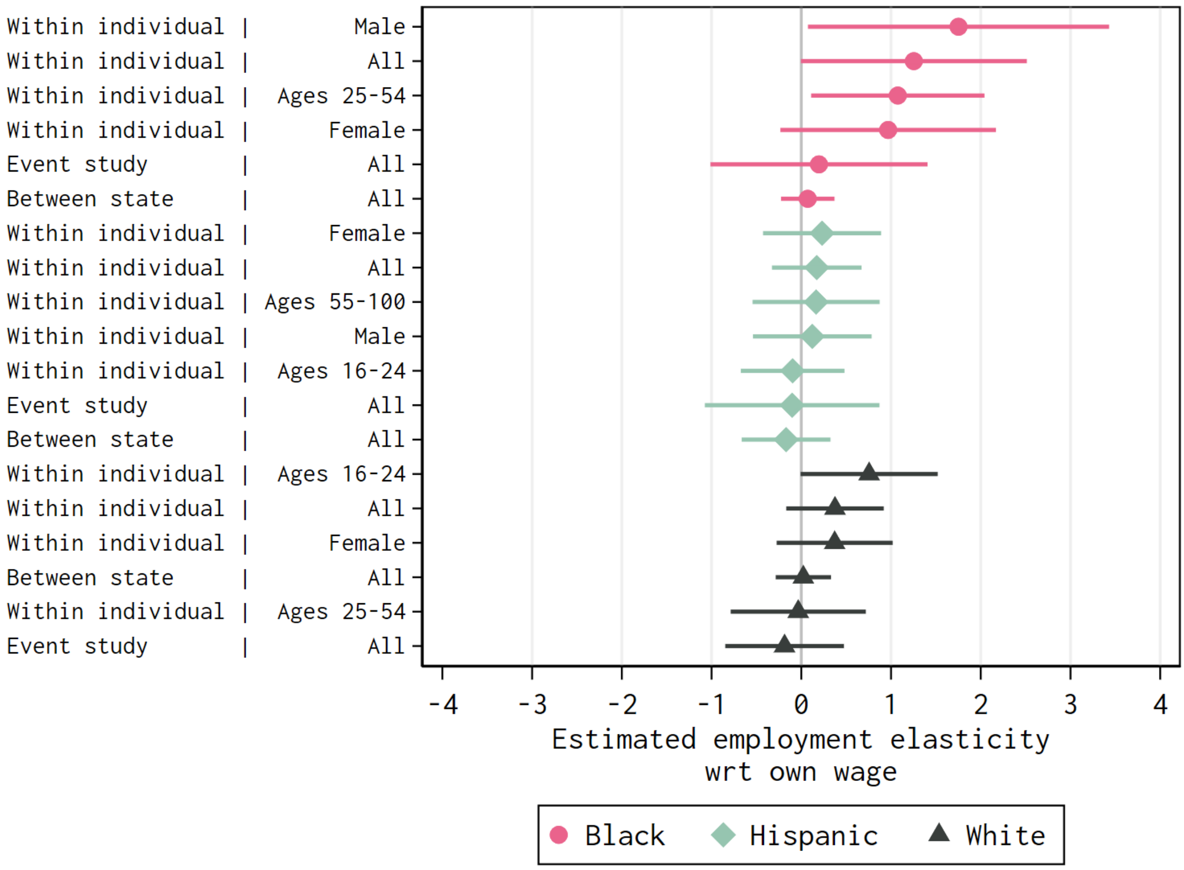
6. Conclusion

Racial wage inequality has increased since the 1990s. Our causal analysis indicates that minimum wage policies have reduced this racial wage gap. Moreover, the indirect benefits of these policies have led to wage gains for black workers that exceed the mechanical effect implied by strict policy compliance. This result is consistent with previous studies showing that affected workers largely spend their increased earnings to acquire a car. We demonstrate that the direct earnings effects of minimum wage policy are amplified as more black workers commute by car, allowing them to reach better paying jobs outside their previous search radius.

Our results hold whether we conduct our analysis at the individual, county or state level, and whether we identify treated workers by their educational attainment or by their initial wage conditions. Our results cannot be fully explained by lower initial wages among black workers relative to white workers. Instead, we find that the disproportionate wage gains of black workers are consistent with reduced search frictions among black workers.

In our preferred specification, minimum wage increases since 1995 reduced the black-white wage gap by 12 percent overall and by 60 percent among workers with a high school degree or less. Moreover, minimum wages reduced racial differences in separation rates, strongly improving the stability of black workers' employment.

Figure 9 Own Wage Employment Elasticities



Notes: Figure shows the own wage employment elasticities for our results of Table 5, Table 8, Table 10, Table 11 and Appendix Table A7, as well as their 95 percent CIs. The within individual estimates are based on a sample of workers earning at most 1.5 times the minimum wage, the between state and event study estimates are based on workers with at most a high school diploma. We show results from the Trends specification, 2FE results are available on request (and not qualitatively different). We omit specifications without significant wage effects as these lead to biased and imprecise own wage employment elasticities.

Figure 9 summarizes our findings graphically, arraying own wage employment elasticities for our four specifications, our preferred Trends model, and by race, ethnicity, gender and age. The own-wage elasticity measures the employment response to a one percent change in the average wage. It thus allows comparisons among groups with different minimum wage bites and different wage effects. As Figure 9 shows, the own-wage elasticities range between slightly negative (-0.18) to nearly +2. Reassuringly, the elasticities for whites and Hispanics fall in the range found in numerous other studies, as documented by Dube (2019). However, the own-wage elasticities in Figure 9 are much higher for blacks than for Hispanics of whites.

Our counterfactual analysis suggests that the adjusted hourly wage gap between *all* white and black workers would have been two percentage points higher had minimum wages not changed since 1995. This result suggests that the strong equalizing effect of the late 1960s minimum wage changes uncovered by Derenoncourt and Montialoux (2021) persists well into the twenty-first century. Moreover, minimum wage policy simultaneously reduces racial inequality and raises the wages of white workers, supporting a political economy view in which reducing racial disparities benefits both black and white workers.

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Appendix tables

Appendix Table A1 Minimum wage effects on wages, by race/ethnicity, employment results

DV: Employment dummy		All years			2002 to 2019		
		(1) White	(2) Hispanic	(3) Black	(4) White	(5) Hispanic	(6) Black
A. 2FE	MW	0.01	-0.00	0.06**	0.00	0.01	0.02
		(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
	N	2274454	550788	430056	1130044	363176	230010
B. Trends	MW	0.00	-0.02	0.01	0.02	-0.01	0.01
		(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)
	N	2274454	550788	430056	1130044	363176	230010

Notes: White refers to non-Hispanic white workers. No wage cut off results as this would require the respondent to have a job. Weighted by the outgoing rotation group person-weights. Sample years include either 1990-2019 or 2002-2019. Restgroup consisting of other race/ethnicities not shown in table. Restricted to workers with at most a high school degree. Data: CPS-MORG. Replication tag: #ptr_microState_employment. Standard errors in parentheses, clustered at state level. Stars: * p<0.1, ** p<0.05, *** p<0.0

Appendix Table A2 Minimum wage effects on wages, by race/ethnicity and age group, employment results

DV: Employment dummy		All years			2002 to 2019		
		(1) White	(2) Hispanic	(3) Black	(4) White	(5) Hispanic	(6) Black
A. 2FE	16-24	-0.18***	-0.19***	0.01	-0.09***	-0.14***	0.01
		(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
	25-34	-0.06***	-0.02	0.07**	0.05**	0.02	0.01
		(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
	35-44	-0.07***	-0.00	0.04	0.02	0.01	0.03
		(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
	45-54	-0.04*	0.05**	0.00	0.02	0.08***	0.04*
		(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)
	55-100	0.39***	0.23***	0.21***	0.05*	0.12***	0.04
		(0.03)	(0.04)	(0.03)	(0.02)	(0.03)	(0.03)
	N	2274454	550788	430056	1130044	363178	230013
B. Trends	16-24	-0.19***	-0.19***	-0.04	-0.08***	-0.15***	-0.01
		(0.01)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
	25-34	-0.07***	-0.03	0.02	0.06***	0.01	-0.01
		(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
	35-44	-0.07***	-0.01	-0.01	0.03*	0.00	0.02
		(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
	45-54	-0.04**	0.04*	-0.05	0.03*	0.07***	0.02
		(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
	55-100	0.38***	0.22***	0.16***	0.06**	0.10***	0.02
		(0.02)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)
	N	2274454	550788	430056	1130044	363178	230013

Notes: White refers to non-Hispanic white workers. No wage cut off results as this would require the respondent to have a job. Weighted by the outgoing rotation group person-weights. Sample years include either 1990-2019 or 2002-2019. Restgroup consisting of other race/ethnicities not shown in table. Restricted to workers with at most a high school degree. Data: CPS-MORG. Replication tag: #ptr_microState_employment. Standard errors in parentheses, clustered at state level. Stars: * p<0.1, ** p<0.05, *** p<0.0

Appendix Table A3 Minimum wage effects on wages, by race/ethnicity
Within-individual estimates. Different thresholds

DV: Change in log earnings	(1) Current MW, 1.5x	(2) 12 month smoothed, 1.5x	(3) 24 month smoothed, 1.25x	(4) 24 month smoothed, 1.5x
White	0.12***	0.10***	0.18***	0.10***
	(0.04)	(0.03)	(0.05)	(0.03)
Hispanic	0.21***	0.20***	0.25***	0.19***
	(0.06)	(0.06)	(0.06)	(0.06)
Black	0.20**	0.25**	0.31***	0.25**
	(0.10)	(0.11)	(0.11)	(0.10)
N	155662	155169	88588	155005

Notes: White refers to non-Hispanic white workers. Weighted by the outgoing rotations group person weights. Restgroup of other race/ethnicities not shown in table. Trends specification, dependent variable is log hourly earnings. Period: 1990-2019, data: CPS-MORG. Replication tag: #ptr_microInd_diffThresholds. Standard errors in parentheses, clustered at state level, ∗ $p < 0.1$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$.

Appendix Table A4 Minimum wage effects on wages, by race/ethnicity
Within-individual estimates. 2002-2019

		Trends			2FE		
		(1) < 1.5	(2) 1.5-2.5	(3) > 2.5	(4) < 1.5	(5) 1.5-2.5	(6) > 2.5
A. Wages	White	0.11***	0.02	-0.04*	0.12***	0.02	-0.04*
		(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
	Hispanic	0.13***	0.05	-0.08	0.21***	0.06	-0.08
		(0.05)	(0.04)	(0.08)	(0.06)	(0.04)	(0.07)
	Black	0.20*	-0.12*	-0.13	0.25**	-0.10	-0.15
		(0.11)	(0.07)	(0.11)	(0.12)	(0.06)	(0.13)
	N	65856	89930	68590	65856	89930	68590
B. Employment	White	0.05	0.01	0.03	-0.00	0.01	0.01
		(0.04)	(0.02)	(0.04)	(0.02)	(0.01)	(0.01)
	Hispanic	-0.07	0.03	-0.10*	0.03	-0.08	-0.12**
		(0.04)	(0.05)	(0.05)	(0.04)	(0.06)	(0.05)
	Black	0.23	0.05	0.28*	0.03	0.01	0.02
		(0.14)	(0.07)	(0.15)	(0.07)	(0.05)	(0.05)
	N	109212	141518	109212	141518	111908	111908

Notes: Hisp refers to Hispanic workers; White refers to non-Hispanic white workers. Weighted by the outgoing rotations group person weights. Restgroup of other race/ethnicities not shown in table. The dependent variable in Panel B is a dummy indicating whether the individual *i* was employed at their second observation (month 16 of the CPS). As a result, the dependent variable will be 0 if the respondent is employed in both periods and -1 if they lose their job. Period: 2002-2019, data: CPS-MORG. Replication tag: #ptr_microInd_02. Standard errors in parentheses, clustered at state level, ∗ $p < 0.1$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$.

Appendix Table A5 Minimum wage effects on share of workers that commutes by car

Ages *21-35*; based on American Community Survey, 2000-2019

	Black		White	
	(1)	(2)	(3)	(4)
	2FE	Trends	2FE	Trends
Poorest Quartile	0.01 (0.03)	0.07** (0.03)	0.01 (0.01)	0.03* (0.02)
3rd Quartile	0.03 (0.04)	0.05 (0.04)	0.00 (0.01)	0.01 (0.01)
2nd Quartile	-0.00 (0.04)	0.02 (0.05)	-0.01 (0.01)	-0.01 (0.01)
Richest Quartile	-0.13 (0.11)	0.12 (0.11)	-0.00 (0.02)	0.04 (0.03)

Notes: White refers to non-Hispanic white workers. Weighted by the ACS person weights. Each number originates from a separate regression. Observation numbers omitted for brevity, ranging from 1425-2039 state-year level observations (some regressions have fewer than 2039 observations because there are no workers of that age, race and wealth level for some state-years). Quartile refers to the household income quartile, defined separately for each state and year. 2FE refers to a twoway fixed effects specification with state and year fixed effects, the Trends specification adds linear state-specific time trends. We control for the unemployment rate at the state and year level. Period: 2000-2019, data: ACS. Replication tag: #ptr_acs_2135. Standard errors in parentheses, clustered at state level, ∗ $p < 0.1$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$.

Appendix Table A6 Minimum wage effects on share of workers that commutes by car

Ages *26-65*; Based on American Community Survey, 2000-2019

	Black		White	
	(1)	(2)	(3)	(4)
	2FE	Trends	2FE	Trends
Poorest Quartile	0.03 (0.03)	0.04 (0.03)	-0.00 (0.01)	0.01* (0.01)
3rd Quartile	0.02 (0.03)	0.01 (0.02)	0.00 (0.01)	0.00 (0.01)
2nd Quartile	0.01 (0.01)	0.04* (0.02)	-0.00 (0.00)	-0.00 (0.01)
Richest Quartile	0.00 (0.05)	0.07 (0.05)	-0.02*** (0.01)	-0.00 (0.01)

Notes: White refers to non-Hispanic white workers. Weighted by the ACS person weights. Each number originates from a separate regression. Observation numbers omitted for brevity, ranging from 1610-2040 state-year level observations (some regressions have fewer than 2039 observations because there are no workers of that age, race and wealth level for some state-years). Quartile refers to the household income quartile, defined separately for each state and year. 2FE refers to a twoway fixed effects specification with state and year fixed effects, the Trends specification adds linear state-specific time trends. We control for the unemployment rate at the state and year level. Period: 2000-2019, data: ACS. Replication tag: #ptr_acs_2665. Standard errors in parentheses, clustered at state level, ∗ $p < 0.1$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$.

Appendix Table A7 Minimum wage effects on wages, by race/ethnicity and gender

Within-individual estimate, fixed effects pooled by gender. 1990-2019.

			Trends			2FE		
			(1)	(2)	(3)	(4)	(5)	(6)
			< 1.5	1.5-2.5	> 2.5	< 1.5	1.5-2.5	> 2.5
A. Wages	White	Male	0.07*	0.07**	0.04	0.08**	0.08**	0.06
			(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
		Female	0.11***	0.02	0.01	0.12***	0.03	0.02
			(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)
	Hispanic	Male	0.19**	-0.04	-0.00	0.25***	0.01	0.02
			(0.07)	(0.02)	(0.07)	(0.08)	(0.03)	(0.08)
		Female	0.20***	0.04	0.01	0.25***	0.09*	0.01
			(0.07)	(0.05)	(0.10)	(0.06)	(0.05)	(0.12)
	Black	Male	0.24**	-0.06	0.02	0.18	-0.05	0.01
			(0.11)	(0.07)	(0.06)	(0.12)	(0.06)	(0.07)
		Female	0.26**	-0.05	-0.02	0.19*	-0.04	-0.03
			(0.11)	(0.09)	(0.05)	(0.11)	(0.09)	(0.06)
		N	155005	217282	208909	155005	217282	208909
B. Employment	White	Male	0.03	-0.02	-0.01	0.02	-0.02	-0.00
			(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.01)
		Female	0.04	0.00	0.02	0.03	0.01	0.03*
			(0.03)	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)
	Hispanic	Male	0.02	0.05	-0.00	0.00	0.04	0.01
			(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.06)
		Female	0.05	0.06	0.00	0.02	0.05	0.02
			(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.06)
	Black	Male	0.41***	-0.05	-0.01	0.40***	-0.05	-0.01
			(0.12)	(0.08)	(0.05)	(0.11)	(0.08)	(0.05)
		Female	0.25**	0.06	-0.03	0.24**	0.05	-0.03
			(0.12)	(0.08)	(0.05)	(0.11)	(0.08)	(0.05)
		N	230213	306494	297419	230213	306494	297419

Notes: White refers to non-Hispanic white workers. Weighted by the outgoing rotations group person weights. Restgroup of other race/ethnicities not shown in table. The dependent variable in Panel B is a dummy indicating whether the individual *i* was employed at their second observation (month 16 of the CPS). As a result, the dependent variable will be 0 if the respondent is employed in both periods and -1 if they lose their job. Period: 1990-2019, data: CPS-MORG, pooled fixed effects. Replication tag: #ptr_microInd_genderPooled. Standard errors in parentheses, clustered at state level, ∗ $p < 0.1$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$.

Appendix Table A8 Minimum wage effects on wages, by race/ethnicity

Within-individual estimates, *men only*. 1990-2019

		Trends			2FE		
		(1)	(2)	(3)	(4)	(5)	(6)
		< 1.5	1.5-2.5	> 2.5	< 1.5	1.5-2.5	> 2.5
A. Wages	White	0.03	0.04	0.03	0.05	0.05	0.04
		(0.04)	(0.04)	(0.04)	(0.03)	(0.05)	(0.04)
	Hispanic	0.17*	-0.00	-0.03	0.21**	0.06	-0.00
		(0.09)	(0.04)	(0.08)	(0.09)	(0.05)	(0.08)
	Black	0.16	-0.21***	0.02	0.13	-0.21***	-0.00
		(0.16)	(0.07)	(0.11)	(0.16)	(0.07)	(0.12)
	N	58425	95424	127621	58425	95424	127621
B. Employment	White	0.06	0.02	-0.01	0.04	0.02	0.00
		(0.05)	(0.03)	(0.02)	(0.05)	(0.03)	(0.01)
	Hispanic	-0.07	0.05	-0.01	-0.06	0.05	0.01
		(0.07)	(0.08)	(0.06)	(0.06)	(0.07)	(0.06)
	Black	0.38***	-0.02	0.04	0.35***	-0.03	0.03
		(0.12)	(0.09)	(0.06)	(0.12)	(0.09)	(0.06)
	N	88380	135562	179692	88380	135562	179692

Notes: White refers to non-Hispanic white workers. Weighted by the outgoing rotations group person weights. Restgroup of other race/ethnicities not shown in table. The dependent variable in Panel B is a dummy indicating whether the individual *i* was employed at their second observation (month 16 of the CPS). As a result, the dependent variable will be 0 if the respondent is employed in both periods and -1 if they lose their job. Period: 1990-2019, data: CPS-MORG, men only. Replication tag: #ptr_microInd_splitmale. Standard errors in parentheses, clustered at state level, $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Appendix Table A9 Minimum wage effects on wages, by race/ethnicity
Within-individual estimates, wo*men only*. 1990-2019

		Trends			2FE		
		(1)	(2)	(3)	(4)	(5)	(6)
		< 1.5	1.5-2.5	> 2.5	< 1.5	1.5-2.5	> 2.5
A. Wages	White	0.14***	0.04*	0.03	0.15***	0.05*	0.05
		(0.04)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)
	Hispanic	0.20***	-0.02	0.16	0.28***	-0.00	0.14
		(0.07)	(0.09)	(0.10)	(0.06)	(0.08)	(0.10)
	Black	0.27***	0.04	0.02	0.19*	0.07	0.01
		(0.09)	(0.11)	(0.10)	(0.10)	(0.12)	(0.10)
	N	96579	121858	81284	96579	121858	81284
B. Employment	White	0.02	-0.03	0.02	0.01	-0.03	0.02
		(0.03)	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)
	Hispanic	0.11*	0.07	0.03	0.07	0.06	0.04
		(0.06)	(0.05)	(0.06)	(0.06)	(0.05)	(0.07)
	Black	0.31**	0.03	-0.05	0.32**	0.03	-0.03
		(0.13)	(0.08)	(0.06)	(0.13)	(0.09)	(0.06)
	N	141833	170932	117726	141833	170932	117726

Notes: White refers to non-Hispanic white workers. Weighted by the outgoing rotations group person weights. Restgroup of other race/ethnicities not shown in table. The dependent variable in Panel B is a dummy indicating whether the individual i was employed at their second observation (month 16 of the CPS). As a result, the dependent variable will be 0 if the respondent is employed in both periods and -1 if they lose their job. Period: 1990-2019, data: CPS-MORG, women only. Replication tag: #ptr_microInd_splitfemale. Standard errors in parentheses, clustered at state level, ∗ $p < 0.1$, ∗∗ $p < 0.05$, ∗∗∗ $p < 0.01$.

Appendix Table A10 Minimum wage effects on wages, by race/ethnicity and age Within-individual estimates, five age groups. 1990-2019.

		A. Wages			B. Employment		
		(1)	(2)	(3)	(4)	(5)	(6)
		< 1.5	1.5-2.5	> 2.5	< 1.5	1.5-2.5	> 2.5
16-24	White	0.12***	0.08	-0.04	0.09**	-0.02	-0.12
		(0.04)	(0.06)	(0.11)	(0.03)	(0.04)	(0.08)
	Hispanic	0.27***	-0.14	0.18	-0.03	0.08	-0.15
		(0.06)	(0.08)	(0.19)	(0.08)	(0.11)	(0.24)
	Black	0.09	-0.04	-0.33	0.28*	0.17	0.30
		(0.10)	(0.15)	(0.39)	(0.14)	(0.14)	(0.25)
25-34	White	0.05	0.03	0.03	-0.04	-0.01	0.04
		(0.06)	(0.04)	(0.04)	(0.06)	(0.03)	(0.03)
	Hispanic	0.08	0.00	-0.04	0.06	0.10*	-0.00
		(0.07)	(0.05)	(0.09)	(0.07)	(0.06)	(0.05)
	Black	0.24*	-0.13*	-0.05	0.33**	0.05	0.03
		(0.13)	(0.07)	(0.10)	(0.15)	(0.09)	(0.08)
35-44	White	0.06	0.05	0.06	0.03	0.02	0.04**
		(0.07)	(0.04)	(0.05)	(0.05)	(0.03)	(0.02)
	Hispanic	0.22*	0.06	-0.04	-0.01	0.05	-0.02
		(0.11)	(0.06)	(0.13)	(0.08)	(0.05)	(0.07)
	Black	0.52***	0.04	0.01	0.43***	-0.06	0.02
		(0.18)	(0.10)	(0.08)	(0.16)	(0.09)	(0.06)
45-54	White	0.18***	0.01	0.02	0.02	0.00	-0.01
		(0.06)	(0.03)	(0.04)	(0.07)	(0.02)	(0.02)
	Hispanic	0.10	0.03	0.04	0.15*	-0.00	0.11
		(0.07)	(0.07)	(0.10)	(0.08)	(0.08)	(0.07)
	Black	0.23	0.02	0.19*	0.32*	0.04	-0.10
		(0.20)	(0.09)	(0.11)	(0.16)	(0.10)	(0.07)
55-100	White	0.04	0.04	-0.01	-0.03	-0.04	-0.07***
		(0.05)	(0.03)	(0.03)	(0.05)	(0.04)	(0.03)
	Hispanic	0.31***	-0.09	0.09	0.05	-0.02	-0.12
		(0.11)	(0.07)	(0.14)	(0.10)	(0.12)	(0.08)
	Black	0.29	-0.16	-0.16	0.22	-0.16	-0.09
		(0.22)	(0.15)	(0.11)	(0.17)	(0.12)	(0.07)
	N	155005	217282	208909	230213	306494	297419

Notes: White refers to non-Hispanic white workers. Weighted by the outgoing rotations group person weights. Restgroup of other race/ethnicities not shown in table. The dependent variable in Panel B is a dummy indicating whether the individual *i* was employed at their second observation (month 16 of the CPS). As a result, the dependent variable will be 0 if the respondent is employed in both periods and -1 if they lose their job. Controls are interacted by age group, fixed are pooled across age groups. Period: 1990-2019, data: CPS-MORG. Replication tag: #ptr_microInd_age5. Standard errors in parentheses, clustered at state level, $*$ p < 0.1, $**$ p < 0.05, $***$ p < 0.01.

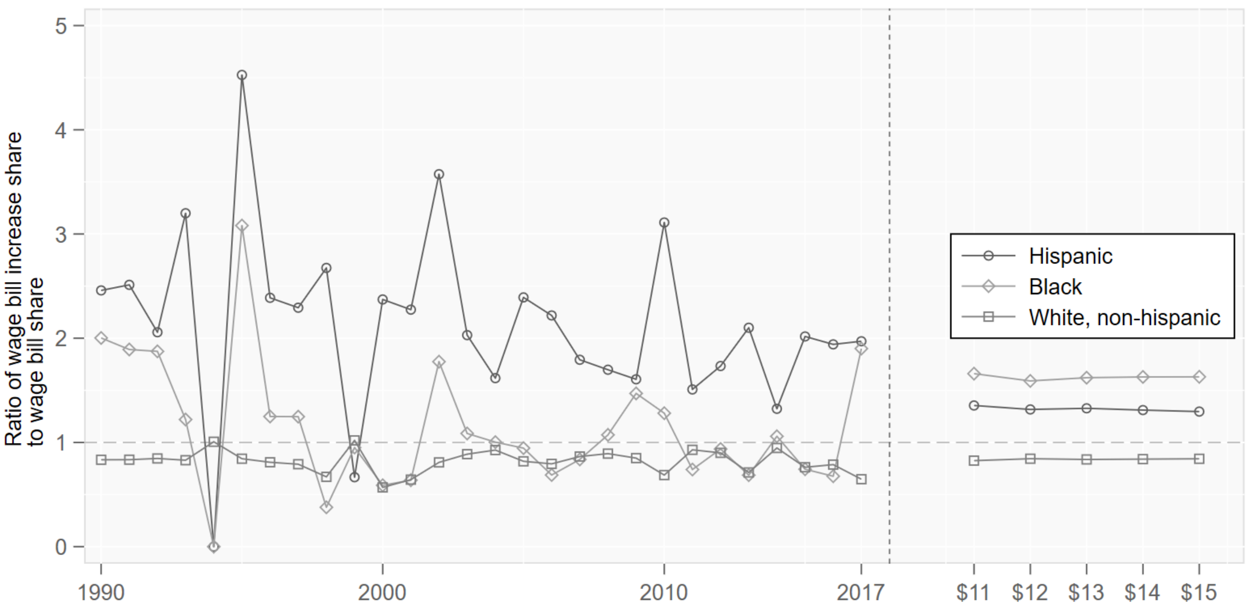
Appendix Table A11 QWI based estimates of minimum wage effects on earnings, employment and employment flows, by race/ethnicity, 1990-2017, CBCP results

		CBCP		
		(1)	(2)	(3)
		White	Hispanic	Black
A. Earnings	MW	0.20***	0.22***	0.28***
		(0.02)	(0.07)	(0.06)
	N	109852	109852	109850
B. Employment	MW	0.07	-0.14**	-0.04
		(0.04)	(0.06)	(0.08)
	N	110054	110054	110052
C. Hiring rate	MW	-0.20***	-0.24***	-0.28***
		(0.04)	(0.05)	(0.07)
	N	85606	82136	82252
D. Separation rate	MW	-0.19***	-0.19***	-0.25***
		(0.03)	(0.05)	(0.07)
	N	85216	81592	82564
E. Turnover rate	MW	-0.19***	-0.20***	-0.27***
		(0.03)	(0.05)	(0.07)
	N	79698	79698	79698

Notes: White refers to non-Hispanic white workers. Unweighted. Restgroup of other race/ethnicities not shown in table. All dependent variables are in logs. CBCP stands for cross border county pair analysis. Replication tag: #ptr_qwi_cbcp_base. Standard errors in parentheses, clustered at state level, * p < 0.1, ** p < 0.05, *** p < 0.01.

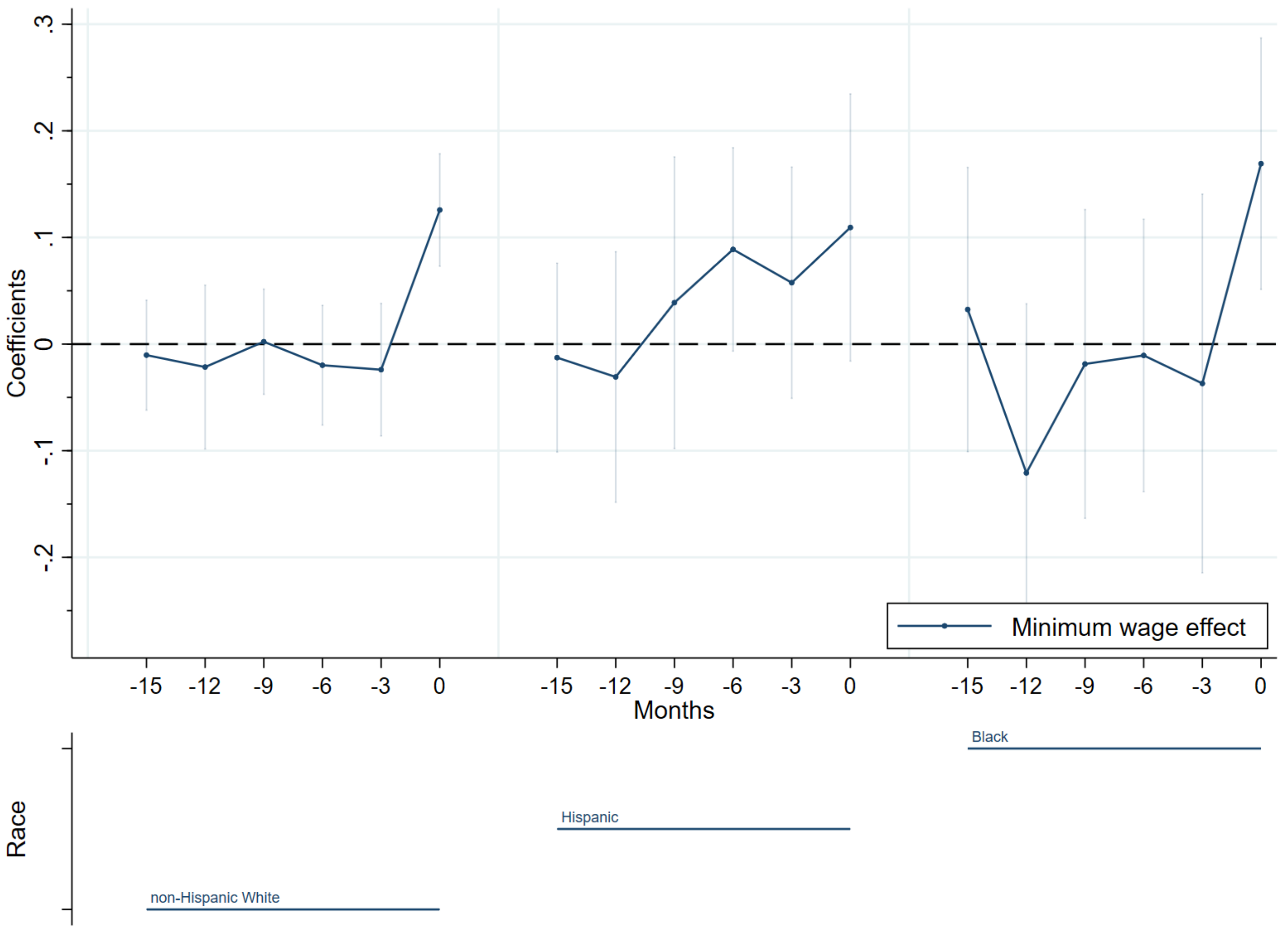
Appendix figures

Appendix Figure A1 Ratio of share in wage bill increase to share in wage bill, by race/ethnicity



Notes: Based on CPS-MORG hourly wages. Left panel shows ratio of wage bill increase share relative wage bill share by racial group. Values above one indicate that the group receives a larger share of the wage bill increase due to minimum wage changes than their initial share in the total wage bill. We use actual minimum wages, but calculate wage bill increases as they would be in a world of perfect compliance but zero wage spillovers or employment effects.

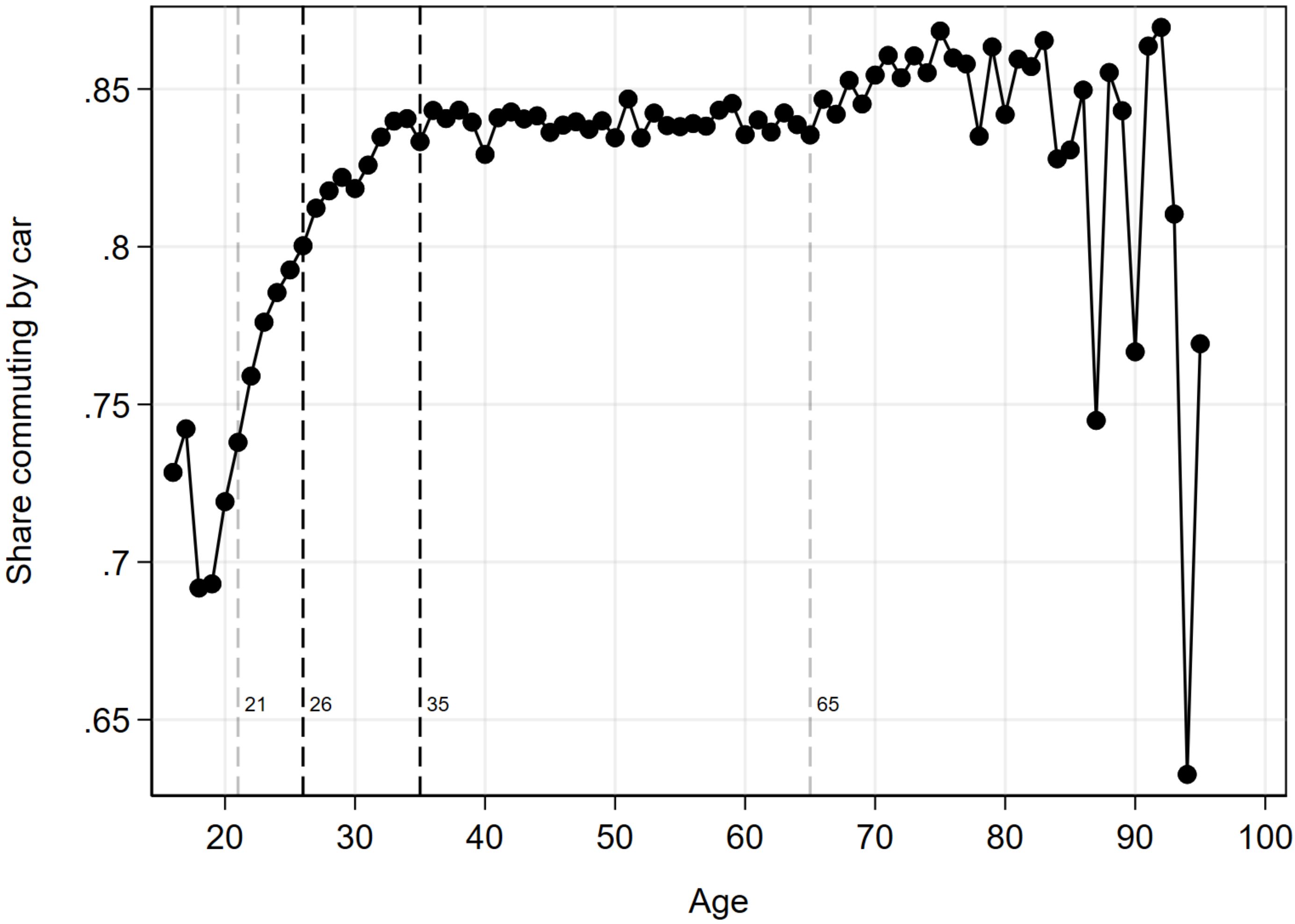
Appendix Figure A2 Distributed lag results for the state variation based results



Notes: Figure shows time placebo test for the estimates of columns (1)-(3), panel D in Table 5 (state variation based panel regressions on the CPS, trends model, with wage cutoff, all years). The first panel shows the effect for non-Hispanic white workers, the second for Hispanic workers and the final panel for black workers. The axis shows the cumulative coefficient of leads of the minimum wage variable. Replication tag: #ptg_microStatePretrends. The vertical bars show 95% confidence intervals (standard errors clustered at the state level).

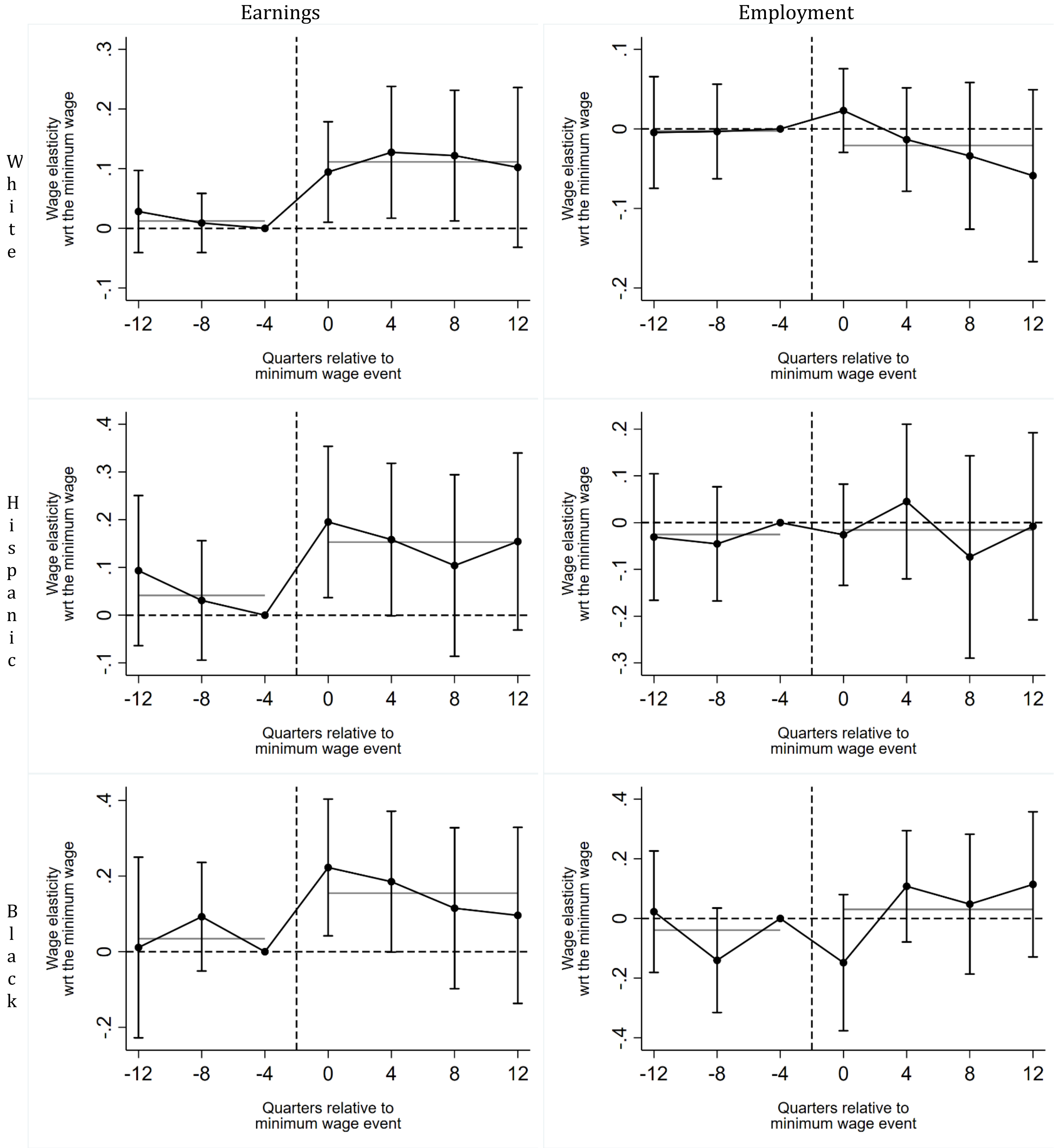
Regression equation: $y_{it} = \sum_k \beta_k * mw_{i,t+k} + controls_{it} + \epsilon_{it}$ for $k \in 0,3,6,9,12,15$.

Appendix Figure A3 Car commute shares by age



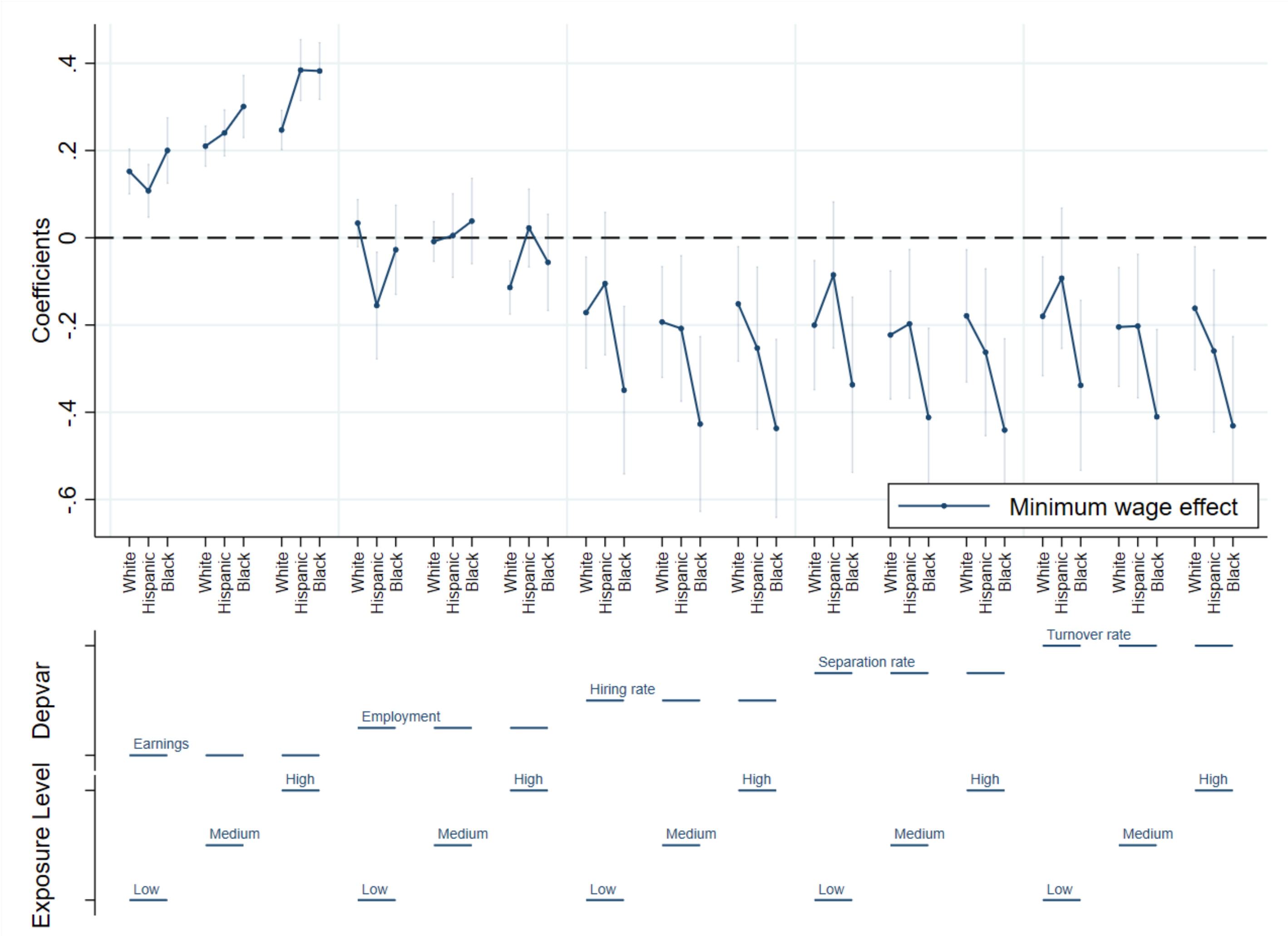
Notes: Figure shows the unweighted share of workers commuting by car averaged over all states and years for workers with at most a high school diploma. The main results are based on workers aged 26-35 (between black bars) as these are old enough to qualify for car loans, but young enough that many of them do not yet have a car. By contrast, the flat car commute share for older workers suggests that by that age, those who desire a car have obtained one. Replication tag: #ptg_car_byAge

Appendix Figure A4 Event study estimates by race



Notes: Each dot represents the joint effect over the four preceding quarters. For example, the dot at quarter 4 is the joint effect over quarters 1, 2, 3 and 4. All effects are relative to the period right before the minimum wage event (quarters -1 to -4, dot at -4). The horizontal gray lines show the average effect over the entire pre/post period. Handles show 95 percent confidence intervals. There are 143 treatment events. Regressions include controls for age, educ, married status and gender dummies; three indicator variables activating in the post window of qualifying, small and federal minimum wages; and event-specific state and time fixed effects. Data: CPS-MORG. Period: 1990-2019.

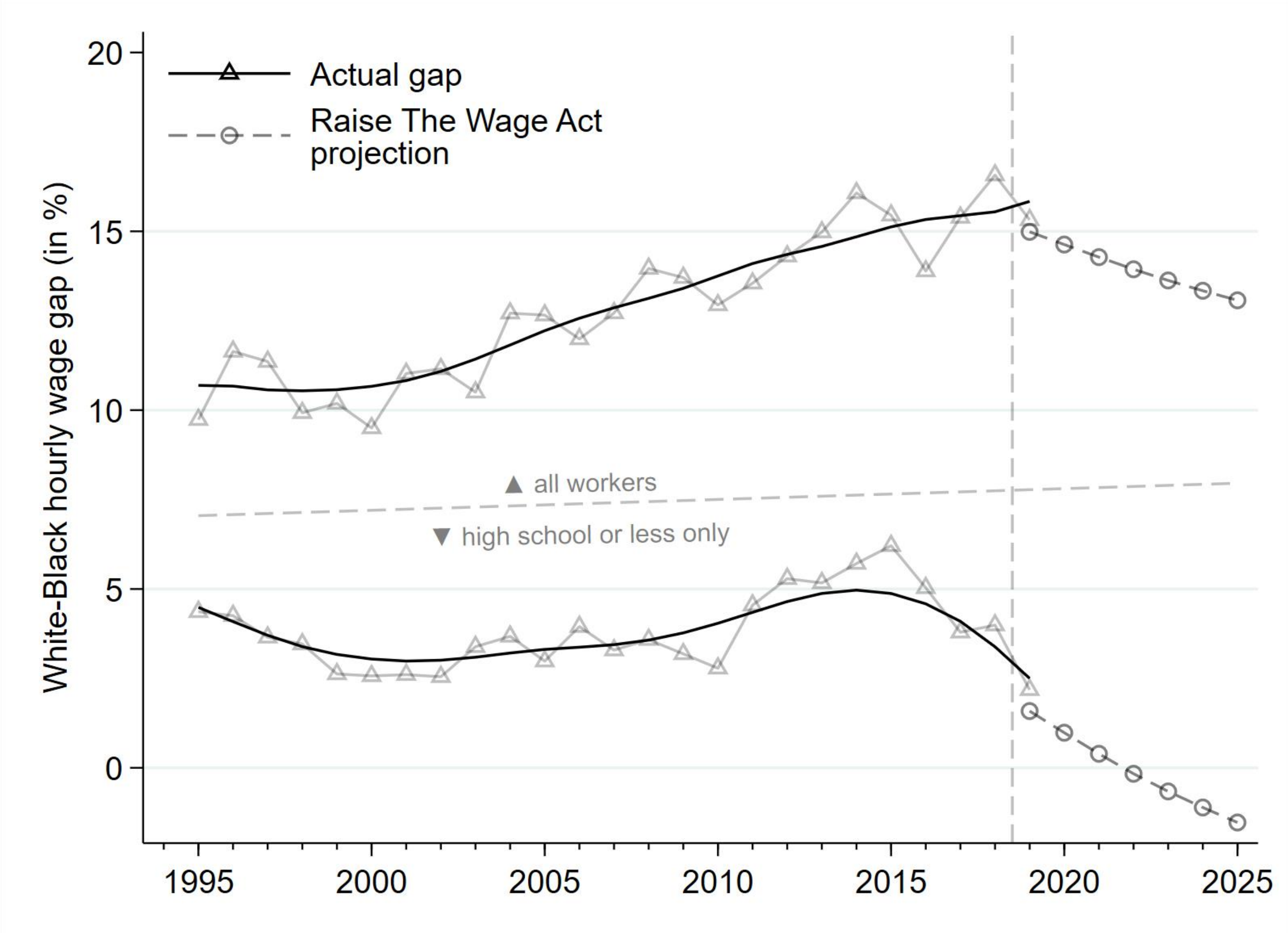
Appendix Figure A5 QWI-based estimates of minimum wage effects by county susceptibility



Notes: Figure shows estimates of column (1)-(3) in Table 13 (QWI based estimates of the effect of minimum wages on earnings, employment and employment flows) by county exposure levels. Low exposure refers to counties where we would expect less than 15 percent of food services workers to be affected by a hypothetical 10 percent minimum wage increase, versus 15-50 percent [medium] and 50-100% [high]. We refer to Wursten (2020) for more details. White refers to non-Hispanic white workers. The vertical bars show 95 percent confidence intervals (standard errors clustered at the state level).

Regression equation: $y_{ct} = \sum_e \quad \beta_e * I[E_c = e] * MW_{st} + controls_{ct} + \epsilon_{ct}$ where E_c is the exposure level of county c.

Appendix Figure A6 Counterfactual white vs black worker hourly wage gap
Based on between-state regressions and the Raise the Wage Act ($15) scenario



Notes: Figure shows observed (solid line) hourly wage gaps between white and black workers over 1995-2025, where values past 2019 are projected counterfactuals (dashed line). The counterfactual gap predicts hourly wages for white and black workers as if "Raise The Wage Act" had been implemented in 2019, which gradually raises the minimum wage to $15 by 2025. We assume worker characteristics do not change relative to 2019 and that all groups have the same real wage growth except for the minimum wage effect. The bottom sample is workers with at most a high school degree and an hourly wage below $20 (2019 dollars), the top sample is all workers. We took the difference between the mean of log wages by race/ethnicity – the y-axis can be interpreted as the percentage gap of black workers versus white workers. The gap is driven exclusively by workers with at most a high school degree and hourly wages below $20 (2019 dollars). Replication tag: #cf_future.

Appendix A: WA-MA Example

The concentration of state minimum wage increases in high wage states and the geographic concentration of black workers in $7.25 states suggest that the effect of minimum wages on the national racial wage gap may be ambiguous. Consider, for example, the contrasting cases of Washington and Mississippi. In 2019, about 133,000 black workers were employed in Washington State (4 percent of the state's 3.32 million workers). In the same year, about 418,000 black workers were employed in Mississippi (37 percent of all 1.13 million workers). These proportions remained stable throughout our sample period.

In 1995, the minimum wage was $4.90 in Washington and $4.25 in Mississippi. The State of Washington then increased its minimum wage to $12 by 2019, an increase of 145 percent over the 1995 level. Meanwhile, Mississippi's minimum wage only followed the federal level to $7.25 by 2019, an increase of 71 percent over the 1995 level. Washington State's policies thus raised the pay of a larger number of white low-wage workers, and by a greater amount, than the minimum wage changes in Mississippi raised low-wage black workers' pay. As a result, the two-state aggregate black-white wage gap for low-education workers grew between 1995 and 2019.

Washington and Mississippi are not representative of all states. This comparison between the two nonetheless reflects the differences between the groups of states that ever raised their minimum wage and the groups that did not. Indeed, the five states with the highest percentage of black workers-- Alabama, Georgia, Louisiana, Mississippi, and South Carolina, have never raised their state minimum wages.

Appendix B: Cumulative Wage Bill

Below we illustrate how we can infer the population-normalized wage bill increase from the relative bin wages and the employment-population ratio coefficients α_j (cf. Figure 6). The A and CF superscripts refer to the actual and counterfactual scenarios respectively, WB is the wage bill, E_j employment in bin j, w_j the average wage in bin j (approximated by its midpoint) and MW is the minimum wage.

$$\begin{aligned}
\Delta\frac{WB}{pop} &= \frac{WB^A}{pop} - \frac{WB^{CF}}{pop} \\
&= \sum_j w_j * \left(\frac{E_j}{pop}\right)^A - \sum_j w_j * \left(\frac{E_j}{pop}\right)^{CF} \\
&= \sum_j w_j * \left[\left(\frac{E_j}{pop}\right)^A - \left(\frac{E_j}{pop}\right)^{CF}\right] \\
&= \sum_j w_j * \alpha_j \\
&= \sum_j \left(w_j - MW\right) * \alpha_j + MW * \sum_j \alpha_j \\
&\approx \sum_j \left(w_j - MW\right) * \alpha_j
\end{aligned}$$

The approximation in the final step follows from our finding that employment over these wage bins remains identical in the actual and counterfactual scenario.

Appendix C: Model

In this section we describe a model with two types of workers to rationalise why certain groups might experience larger wage increases after minimum wage changes than can be explained through their initial wage (based on Card et al. 2018).

Setting Assume there are J firms and K workers. Each firm j posts worker-specific wages w_{ij} that workers costlessly observe. The firm will hire any worker i who is willing to accept a job at the posted wage.

Supply side Workers are of two types, with high or low outside options $S_i = H, L$, depending on their access to mobility options. Car ownership provides an intuitive distinction between these two groups, where those with a car have wider search options and less disutility of commuting. For worker i of outside option type S_i, the indirect utility of working at firm j is

$$u_{ij} = \beta ln(w_{ij} - b_i) + \nu_{ij} \tag{1}$$

where ν_{ij} captures idiosyncratic preferences for working at firm j that are unobservable to the firm. b_i is the wage-equivalent value of the worker's outside option, which is the difference of the outside wage w_i^b and the disutility from commuting there $\alpha_{S_i} * d_i^b$. This disutility is larger for workers of low outside option type ($\alpha_L > \alpha_H$) and increasing in the distance to the outside option d_i^b.

$$b_i = w_i^b - \alpha_{S_i} * d_i^b \tag{2}$$

The firm observes b_i and can thus extract rents from workers based on their location and mobility status. We assume the error term in the indirect utility of the worker ε_{ij} is made up of independent draws from a Type 1 Extreme Value distribution, which leads to logit choice probabilities of the form

$$p_{ij} = P\left(\arg\max_{k \in 1,\ldots,J} u_{ik} = j\right) = \frac{e^{\beta ln(w_{ij} - b_i)}}{\sum_{k=1}^{J} e^{\beta ln(w_{ik} - b_i)}} \tag{3}$$

If the number of firms J is large, then these probabilities can be approximated by

$$p_{ij} \approx \lambda_i e^{\beta ln(w_{ij} - b_i)} \tag{4}$$

where λ_i is a constant common to all firms in the market. For large J, this leads to the approximate firm-specific labor supply function

$$L_{ij}(w_{ij}) = p_{ij} = \lambda e^{\beta ln(w_{ij} - b_i)} \tag{5}$$

which corresponds to following (firm-specific) labor supply elasticity

$$\varepsilon_{ij} = \frac{\beta w_{ij}}{w_{ij} - b_i} \tag{6}$$

Demand side The firms solve the following cost minimisation problem

$$\min_W C_j = \sum_{i=1}^{K} w_{ij} * L(w_{ij}) \qquad s.t. \quad T_j f[L(W)] \geq Y \tag{7}$$

where C_j is total cost, T_j is a firm-specific productivity shifter and the production function f exhibits constant returns to scale with respect to $L(W) = \{L_{1j}(w_{1j}, ..., L_{Kj}(w_{Kj})\}$.[1] For simplicity, we ignore capital and intermediate inputs. The K first order conditions of this optimisation problem can be written as

$$w_{ij}\frac{1+\varepsilon_{ij}}{\varepsilon_{ij}} = T_j f_i \mu_j \tag{8}$$

where μ_j represents the marginal cost of production which the firm will equate to marginal revenue at an optimal choice for Y.[2] f_i is the derivative of f with respect to L_{ij}.

Equilibrium Combining the demand-side Equation 8 with the supply-side Equation 6 provides following expression for the equilibrium wage w_{ij}

$$\begin{aligned} w_{ij} &= \frac{\beta}{1+\beta} T_j f_i \mu_j + \frac{b_i}{1+\beta} && (9) \\ &= \frac{\beta}{1+\beta} T_j f_i \mu_j + \frac{w_i^b - \alpha_{S_i} * d_i^b}{1+\beta} && (10) \end{aligned}$$

Note that the wage w_{ij} is decreasing in distance to the outside option d_i^b and the disutility of commuting α_{S_i}. Intuitively, a worker can negotiate a better wage if she lives closer to her outside option, or cares less about commuting distances.

Minimum wages The introduction of a minimum wage MW can lead to three major outcomes for workers of low outside option type ($S_i = L$).[3] In the worst case scenario, the minimum wage exceeds its equilibrium wage w_0 and the worker is insufficiently productive to be profitable at minimum wage rates, forcing the worker to turn to its outside option.

In the intermediate case, the minimum wage still exceeds the worker's equilibrium wage, but now they are suffi-

[1] We assume f is twice differentiable.

[2] See Card et al. (2018) for some examples using different production markets. For our purposes, the specific setting of the product market is not directly relevant.

[3] We describe the three interesting outcomes. A fourth occurs if the worker's initial wage w_0 exceeds the minimum wage. Then the worker will not be affected in this model without spillovers. High outside option type workers share the worst case scenario (turn to outside option) and the unaffected outcome.

ciently productive to remain profitable. The worker is then paid $w_{ij} = MW$ and remains employed. In the best case scenario from the worker's perspective, the increase in earnings allows him to become of high type.

Consider the simplest model of outside option types, which assumes mobility is the key element driving outside option type[4],

$$P(S_i = H|w_i) = P(w_i + e_i > \delta) \tag{11}$$

where δ is some threshold to becoming more mobile and e_i bundles any relevant individual characteristics. We can interpret Equation 11 as the reduced form of a budget constraint,

$$\text{wealth}(w_i, e_i) + \text{credit}(w_i, e_i) > P_{car} \tag{12}$$

which states that the individual will only buy a car if she currently has sufficient wealth and credit options to pay for it.[5] This condition is more likely to hold when the worker's wage is increased from its initial wage w_0 to the minimum wage MW, as both wealth and credit options are increasing in the wage. Indeed, Cooper et al. (2020) and Aaronson et al. (2012) find that minimum wage increases lead to increased access to credit and higher car debt in particular.

If $MW + e_i > \delta$ holds, then the worker becomes of high type, changing its outside option valuation to $b_i = w_i^b - \alpha_{\mathbf{H}} * d_i^b$. In turn, this allows her to renegotiate her wage to

$$w_{ij} = \frac{\beta}{1+\beta} T_j f_i \mu_j + \frac{w_i^b - \alpha_{\mathbf{H}} * d_i^b}{1+\beta} \tag{13}$$

because the firm can now appropriate less of its proximity-based rent.[6]

Now consider two workers with the same initial equilibrium wage $w_0 < MW < T_j f_i \mu_j$, but of different outside option types. When the minimum wage is introduced, the wage of the *high* type increases to $w^{HH} = MW$, where the superscripts denote the initial and final type of the worker. The wage of the initially *low* outside option type either increases to $w^{LL} = MW$, or to a new *high* type equilibrium wage $w^{LH} = w_{ij}$ if the increase in bargaining power exceeds the minimum wage increase: $(\alpha_H - \alpha_L) * d_i^b > MW - w_0$.

Relation to empirical results In our empirical exercise we estimated the minimum wage induced wage increase for workers who remain employed over the event horizon (12 months). We found that black workers experienced

[4]We choose mobility as an example mechanism because it is a relevant factor in determining the range of outside options and because there are substantial differences in mobility between the racial groups we study (Raphael and Riker, 1999). Moreover, Cooper et al. (2020) and Aaronson et al. (2012) show that purchasing used cars constitutes one of the main spending responses to minimum wage increases.

[5]We abstract from other goods the worker might consume, as modeling the utility function that generates the ideal mix between other goods and car ownership adds considerable complication without generating interesting new insights.

[6]Phrased intuitively, as the worker now owns a car, they are more willing to turn to the 'further away' outside option, increasing their bargaining power vis-a-vis the employer.

larger wage gains than white workers, even after controlling for differences in initial wages. Our model motivates that difference, since a) the potential wage gains in this model are larger for low outside options type workers and b) black workers are more likely to be of the low outside option type.

Consider the potential wage gains for workers of high and low types, conditional on remaining employed and starting at wage $w_0 < MW$:

$$\Delta w^{HH}(w_0) = MW - w_0 \tag{14}$$

$$\Delta w^{LL}(w_0) = MW - w_0 \tag{15}$$

$$\Delta w^{LH}(w_0) = MW - w_0 \qquad \text{if } w_{ij}^{H} \leq MW \tag{16}$$

$$\Delta w^{LH}(w_0) = w_{ij}^{H} - w_0 \qquad \text{if } w_{ij}^{H} > MW \tag{17}$$

where we add a superscript H to w_{ij} in Equations 16 and 17 to stress that these are the equilibrium wages for that worker after it becomes of high outside option type. Given that $\Delta w^{LH}(w_0) \geq \Delta w^{HH}(w_0)$ and $\Delta w^{HH}(w_0) = \Delta w^{LL}(w_0)$, workers of low type receive a wage increase that is larger or equal to the wage increase of high types.

Black workers are more likely to be of the low outside option type (through the e_i term in Equation 11) because they tend to live in areas with fewer job opportunities (cfr. Stoll and Covington, 2011 and Bergman et al., 2020), have to exert more search effort due to discrimination in the labour market (Kline and Walters, 2020) and have lower initial wealth and credit access (Dettling et al, 2017).