Labour Economics 82 (2023) 102344

Contents lists available at ScienceDirect

Labour Economics

journal homepage: www.elsevier.com/locate/labeco

Racial inequality in frictional labor markets: Evidence from minimum wages[☆]

Jesse Wursten[a,*], Michael Reich[b]

[a] KU Leuven, Naamsestraat 69, Leuven 3000, Belgium
[b] University of California, Berkeley, United States

ARTICLE INFO

JEL classification:
J7
J15
J31
J38

Keywords:
Racial wage gaps
Minimum wages
Black wages
Hispanic wages
Labor markets

ABSTRACT

We provide the first causal analysis of how state and federal minimum wage policies in the U.S. have affected labor market frictions and racial wage gaps. Using stacked event studies, binned difference-in-differences estimators, within-person analyses and classic panel methods, we find that minimum wages increased wages of black workers between 16 and 64% more than among white workers and reduced the overall black-white wage gap by 10% (and by 56% among workers most affected by the policies). Racial differences in initial wages cannot explain this differential effect. Rather, minimum wages expand job opportunities for black workers more than for white workers. We present a model with labor market frictions in which minimum wages expand the job search radius 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 but not among white 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.

In this paper we examine how minimum wage policies have affected labor market frictions and racial wage gaps in the U.S. These gaps have increased steadily since the late 1970s, both overall and separately among males and females (Daly et al., 2017; Miller, 2018), despite reductions in black-white gaps in educational attainment, achievement scores and the implementation of numerous policies to remedy labor market inequality (Reardon, 2016).[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).

It is not obvious whether minimum wage policies since the 1980s have narrowed or broadened racial wage gaps. In the late 1980s, federal and state minimum wage policies began to diverge; today, 29 states and the District of Columbia have enacted their own, higher, minimum wage standards. In seven states, nominal minimum wage levels are, or will soon become, more than twice as high as the federal level that obtains in 21 states. Adoption of higher minimum wages is not random. States with higher standards tend to be higher cost and higher wage states; those that remain at the federal minimum wage level consist mainly of low-wage states, many located in the Old South. These southern states contain disproportionately large concentrations of black workers. As a result, minimum wage policy in the U.S. has evolved in a manner that has increasingly left behind black workers in low-wage states.

The patchwork system of federal and state minimum wage policies that has emerged since the 1980s may therefore have increased national racial wage gaps, despite potentially narrowing them in individual states. Indeed, the landmark study by Cengiz et al. (2019) finds that state minimum wage policies have led to greater wage increases for white workers than for black/Hispanic workers.[2,3]

[2] Appendix A illustrates this possibility by comparing minimum wage effects in Mississippi and Washington State. And Black et al. (2013) find that racial wage disparities are greater when location is taken into account. 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] The federal-state unemployment insurance system has a similar patchwork structure. Kuka and Stuart (2021) find that this system benefits white workers more than black workers.

☆ We thank our universities for research support and Joslyn Jingjing Fu for excellent research assistance. Benjamin Bowser, David Card, Heepyung Cho, David Cooper, Arindrajit Dube, Anna Godoey, Joslyn Jingling Fu, Lawrence Katz, Patrick Kline, María J. Luengo-Prado, Steven Raphael and William Spriggs, as well as participants in the Berkeley Labor Lunch, 2021 SOLE, LERA and EALE meetings and the 2022 AEA meeting provided helpful suggestions.

* Corresponding author.
E-mail addresses: jesse.wursten@kuleuven.be (J. Wursten), mreich@econ.berkeley.edu (M. Reich).

[1] The remaining racial gaps in educational attainment and achievement are increasingly accounted for by increasing differences in parental income and education (Reardon, 2016). As a result, these racial educational gaps have not narrowed since 2002 (de Brey et al., 2019).

https://doi.org/10.1016/j.labeco.2023.102344
Received 2 July 2021; Received in revised form 23 December 2022; Accepted 17 February 2023
Available online 25 February 2023
0927-5371/

Our causal analyses find that the intermittent federal minimum wage increases and the steady growth of state minimum wage policies since 1990 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 10 percent larger among all workers and 56 percent larger among those with at most a high school diploma.

The reduction in racial wage inequality results less from pre-existing racial wage differentials among the most exposed workers, and more from black workers' becoming more able to overcome labor market frictions that stem from the mismatch between residential and employment locations and employer discrimination. Our bin-by-bin estimates (cf. Cengiz et al., 2019) indicate that black (and Hispanic) wage gains exceed the ranges expected in simple wage top-up scenarios. We show that the reduction in the racial wage gap cannot be explained by differences in initial wages, using both a graphical approach and a more formal entropy balancing design that re-weights observations to equalize average initial wages.

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. Higher minimum wages expand the financial resources at their disposal, improve their credit ratings and thus their access to automobile financing and expanded commuting options. Since the speed of automobile trips typically greatly exceeds those using public transit, the expanded commuting options allow black workers to search more extensively and to obtain better paying jobs, potentially at less discriminatory firms. This mechanism is consistent with earlier findings on the mismatch between black workers' residential locations and higher-paying job opportunities (Miller, 2018; Raphael and Riker, 1999), and minimum wage effects on credit access and car loans (Aaronson et al., 2012; Cooper et al., 2020). 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 among 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, nor do they have negative effects on employment and hours of any of these groups. Rather, minimum wages increase earnings for all race, ethnic, age and gender groups; they simply increase more for black workers. The finding of wage gains for all groups without disemployment effects makes sense if these policies reduce labor market frictions.[4]

Relation to the literature The voluminous minimum wage literature includes numerous estimates of the effects of the policies on black and white employment, but very few estimates of the separate effects on black and white workers' wages. For example, Card and Krueger (1995, p. 282), who devote one chapter to wage effects, report 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 et al. (2011) show that minimum wages increase wages of black teens more than wages of white teens. Our paper is related to the literature on heterogeneous effects of minimum wages (Cengiz et al., 2019; Godøy et al., 2021; Wursten, 2020). Derenoncourt and Montialoux (2020) find similar effects to ours for the period of the late 1960s, when federal minimum wage coverage was expanded, but no states had yet implemented their own standards. Our analysis considers multiple types of minimum wage workers and a more recent time period, thereby providing more generalizability.

Minimum wage policies may act directly upon the racial wage gap, as well as indirectly, by ameliorating other factors that generate racial inequality. As we have noted, our results indicate that the direct mechanism alone does not explain the reduction of the racial wage gap. Previously studied mechanisms include job search frictions (Johnson, 2006; Raphael and Riker, 1999; Stoll and Covington, 2012) and interactions with other markets such as housing, that constrain black job opportunities (Andersson et al., 2018; Bergman et al., 2019; Ihlanfeldt and Sjoquist, 1998).[5] Our finding of racial differences in the effects of minimum wages on commuting modes and employment flows lend support to a causal explanation based on reductions in search frictions for 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 (2019) finds that state-level driver's license reforms increased commuting by automobile and thereby improved employment opportunities among undocumented workers. Using 1990 Census data on 242 metro areas, Raphael and Stoll (2001) examine the effects of car ownership on employment rates among white and black workers. They find larger effects on black employment than on white employment, especially where racial residential segregation is greater. These results suggest that enlarging the search radius allows black workers to find more employers who are less discriminatory. Raphael and Stoll do not, however, examine effects on racial wage differentials.

Data Our primary data source consists of the 1982–2019 Current Population Survey (CPS), accessed through IPUMS (Flood et al., 2020). The CPS tracks millions of U.S. residents over a sixteen-month time frame and records (among others) demographic and work-related characteristics. In the fourth and sixteenth month it also asks for wage information. Our main analyses combine that data with the evolution of minimum wage policy at both the state and federal level (retrieved through Vaghul and Zipperer, 2019). We use the Quarterly Workforce Indicator dataset (QWI) and the American Community Survey (ACS), respectively, for the sector-level analysis and the commuting mode regressions.

Wage effects by race In line with recent developments on staggered treatment designs (including Callaway and Sant'Anna, 2021; de Chaisemartin and d'Haultfoeuille, 2020; de Chaisemartin and D'Haultfœuille, 2021; Goodman-Bacon, 2021) we adopt a stacked event study at the event-state-quarter level as our baseline method. We follow closely the design principles of Cengiz et al. (2019) and adapt the estimator to a setting with a continuous treatment variable.[6] As the name suggests, the estimator *stacks* multiple event studies. Each event study accounts for the dynamics of any treatment effect and is adjusted for any events happening in the control states. They are then combined to maximize power and minimize the idiosyncratic effects of particular events. We find that wage elasticities are 63% larger for black workers than for white workers (0.15 vs. 0.09) when we restrict the sample to those with at most a high school diploma and exclude high wage earners (more than $20 in 2019 dollars). In the food services sector (NAICS 722) the difference is smaller, at 16%.

We also apply the binned difference-in-differences estimator of Cengiz et al. (2019) to gain further insight into the distribution of wage

[4] The minimum wage literature also shows that minimum wages are partly absorbed by small price increases in some industries (Cooper et al., 2020).

[5] 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 et al., 2004). For studies with a causal research design, see Andersson et al. (2018); Miller (2018); Stoll and Raphael (2000).

[6] In the Cengiz et al. (2019) setting, observations are wage bins which are either in the relevant wage range of a minimum wage policy or not. Their treatment indicators and controls for confounding events are thus a set of dummies. In our context, both the treatment variable and the controls are appropriately scaled by the size of the minimum wage change.

Table 1
Wage elasticities by race for different methods and populations.

Dataset - Method	Black	White	Relative difference
CPS - Stacked event study (HSOL, <$20)	0.15 (0.05)	0.09 (0.01)	+63%
QWI - Stacked event study (food services)	0.17 (0.02)	0.15 (0.02)	+16%
CPS - Binned estimator (MW ± $4)	0.68 (0.12)†	0.46 (0.07)	+48%
CPS - Within individual (< 1.5× MW)	0.22 (0.08)	0.13 (0.03)	+64%
CPS - Classic panel (HSOL, <$20)	0.18 (0.03)	0.13 (0.01)	+41%

Notes: White refers to non-Hispanic white workers. Table reports wage elasticities for white and black workers as well as the relative difference (*black* − *white*) / *white*. The stacked event study and binned estimator analyses are described in Section 2. The within individual methodology is introduced in Section 3, the classic panel method in Section 5. Standard errors are clustered at the state level and shown in parentheses. HSOL refers to workers with a high school diploma or less, food services to NAICS 722. Periods are 1982–2019 for CPS-based results and 1990–2020 for QWI analyses. Replication tag: #table-results-summary-wage-elasticities. The replication tag is mirrored in the code and makes it easy to link exhibits in the paper to the code used to generate it (and is more stable than Table and Figure numbers which are subject to change). † The binned estimator pools black and Hispanic workers due to sample size constraints, see Appendix B.

effects.[7] This method counts workers in 25 cent wage bins and can thus map the impact of minimum wage policy at different relative wage levels without ex ante restricting the sample of affected workers. We find that for white workers, most effects occur close to the minimum wage (jobs paying within $1 of the new minimum wage), whereas for black and Hispanic workers we also see gains further up the wage distribution (up to $4 above the new minimum wage).

In a novel approach in the minimum wage literature, we exploit the longitudinal nature of the CPS to show that these differences cannot be explained by initial wage differences. In this *within-person* analysis we use the first interview (month four) to select all workers earning less than 1.5 times the smoothed minimum wage and then estimate whether subsequent wage growth was affected by minimum wage policy and by race/ethnicity. We control for initial characteristics and allow counterfactual wage growth to differ by race/ethnicity. We find that (a) initial wages are very similar within the affected group (only 1.4% lower for black workers on average), (b) wage growth is nonetheless 64% more responsive for black workers, and (c) that the wage growth differential persists if we reweight observations to balance average wages between racial and ethnic groups.

Mechanism 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. The model we develop is 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 and the stacked event study methodology.[8] 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 E1).

Additionally, we estimate the effect of minimum wages on job stability using the Quarterly Workforce Indicator dataset (see also Dube et al., 2016). Our stacked event study finds that turnover rates of food services workers decline 30% more among black workers than among white workers.

Counterfactual analysis We round off the paper by quantifying the impact of minimum wage policies on the black-white earnings gap. We start by estimating a classic two-way fixed effects model with state-specific time trends (Allegretto et al., 2011) to identify the long-run impact of minimum wage policies on workers with at most a high school diploma, and earning less than $20 per hour. We find wage elasticities of $+0.18$ for black workers and $+0.13$ for white workers. Then, we predict the implied wages in these groups had minimum wages not changed after 1982. We find that the actual wage gap in the affected population is 56% smaller than it would have been (3.4% vs. 7.7%). In the overall economy, this finding corresponds to a 10% reduction in the racial wage gap (22.8% vs. 25.3%, assuming all other workers were unaffected by minimum wage policy).

Summary table We summarize our empirical evidence on wage effect heterogeneity in Table 1. The overall trend is clear: wage elasticities are substantially larger for black workers; this result holds not just in different datasets and with different methods, but also in different subpopulations.

Roadmap Our paper proceeds as follows. Section 1 briefly describes the data used to estimate wage elasticities by race in Section 2. We show that initial wage disparities cannot explain the differences in Section 3. We motivate and empirically assess the commute and turnover mechanisms in Section 4. Section 5 describes how we calculate the impact on the hourly wage gap. Section 6 provides results along gender, age and employment dimensions. Section 7 concludes.

1. Data

Our analyses draws from four datasets: Current Population Survey Outgoing Rotation Group (CPS[-ORG]) files for individual-level characteristics and hourly wages, the Quarterly Workforce Indicator (QWI) dataset for state-level employment stocks and flows (hires, separations), the regularly updated state minimum wage levels dataset described by Vaghul and Zipperer (2019), and the American Community Survey's Journey To Work files.

CPS Table 2 provides descriptive statistics for the 1982–2019 CPS sample. We exclude the self-employed, those in the armed forces and unpaid family workers. 74 percent of the remaining respondents are non-Hispanic whites, 11 percent are black and 8 percent are Hispanic.[9]

[7] This estimator is also known as the bunching estimator and the distributional difference-in-differences estimator.

[8] We are the first to study the effects of minimum wages on commuting modes. Public bus and light rail systems generally are much slower than commuting by automobile, especially outside central city limits. Commuter rail systems provide more rapid commutes, but their services generally are oriented to in-commuting from affluent suburbs (Parks, 2016).

[9] 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

Table 2
CPS descriptive statistics.

	Full sample	HSOL	HSOL, < \$20	< 1.5 MW | 1	< 1.5 MW | 2
Sample shares	1	0.50 (0.50)	0.20 (0.40)	0.03 (0.17)	0.03 (0.16)
Teen (16–19)	0.11 (0.31)	0.19 (0.39)	0.16 (0.36)	0.20 (0.40)	0.15 (0.36)
Hispanic	0.08 (0.28)	0.12 (0.32)	0.12 (0.33)	0.11 (0.32)	0.12 (0.32)
Black	0.11 (0.31)	0.13 (0.33)	0.11 (0.32)	0.11 (0.31)	0.11 (0.32)
White	0.74 (0.44)	0.69 (0.46)	0.71 (0.45)	0.72 (0.45)	0.71 (0.45)
Other	0.07 (0.25)	0.06 (0.24)	0.06 (0.23)	0.06 (0.24)	0.06 (0.24)
Employed	0.72 (0.45)	0.64 (0.48)	1	1	1
Hispanic	0.65 (0.48)	0.60 (0.49)	1	1	1
Black	0.63 (0.48)	0.53 (0.50)	1	1	1
White	0.75 (0.43)	0.67 (0.47)	1	1	1
Hourly wage (2019\$)	20.93 (13.03)	16.20 (8.82)	12.17 (3.79)	9.62 (1.62)	9.68 (1.60)
Hispanic	16.25 (9.80)	13.98 (7.08)	11.77 (3.47)	9.83 (1.70)	9.92 (1.69)
Black	17.83 (10.63)	14.56 (7.47)	11.84 (3.61)	9.53 (1.60)	9.55 (1.60)
White	21.66 (13.29)	16.80 (9.16)	12.30 (3.87)	9.59 (1.60)	9.64 (1.57)

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. HSOL, <\$20 excludes those earning more than \$20 (2019 dollars). <1.5 MW refers to the sample of workers earning less than 1.5 times the 24-month smoothed minimum wage in their first interview. The | 1 and | 2 columns refer to their situation at the first (month four) and second (month sixteen) interview respectively. These three groups are by definition employed. Period: 1982–2019. Data obtained through IPUMS. Replication tag: #table-cps-sumstats.

Teens make up 11 percent of the full sample, increasing to 19 percent when we consider only those with a high school degree or less (HSOL), and to 20 percent of those earning less than 1.5 times the 24-month smoothed minimum wage in their first interview (<1.5 MW | 1).

A similar pattern emerges for workers identifying as black or Hispanic, who are also over-represented in the HSOL and <1.5 MW subsamples. Employment rates differ substantially by race and ethnicity. They are highest for non-Hispanic white respondents (75 percent), dropping to 65 and 63 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 \$21 over the sample period, \$0.7 higher for (non-Hispanic) white workers versus \$4.7 and \$3.1 lower for Hispanic and black workers. These differences do not persist within the low-wage group, e.g. Hispanic workers earn more than their white counterparts in the <1.5 MW sample. This difference in patterns between the bottom group and the average could result from the equalizing effects of minimum wage policy at the bottom.

QWI The Quarterly Workforce Indicators (QWI) dataset, which is based on administrative Longitudinal Employer-Household Dynamics data, has employment stocks and flows for most U.S. states.[10] In recent years the QWI has incorporated race, gender and ethnicity variables. The dataset is available through 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 the QWI-based analyses to the food services sector (NAICS 722) as the minimum wage is considerably more binding in that sector than in the overall economy. Around 25% of all minimum wage workers are active in the food services sector (Wursten, 2020).

Table 3
QWI descriptive statistics (NAICS 722).

	Hispanic	Black	White
Weekly earnings (2019\$)	314 (51)	277 (44)	327 (63)
Employment (in thousand)	34 (77)	26 (31)	116 (102)
Hiring rate (%)	41 (15)	49 (16)	36 (10)
Separation rate (%)	40 (14)	47 (15)	36 (9)
Turnover rate (%)	41 (14)	48 (15)	36 (9)

Notes: White refers to non-Hispanic white workers. Table reports means by state, 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: 1990–2020. Food services sector only (NAICS 722). Replication tag: #table-qwi-sumstats.

Table 3 provides descriptive QWI statistics for 1990–2020. The average state employed about 26 thousand black workers, 34 thousand Hispanic workers and almost 116 thousand non-Hispanic white workers in the food services sector (with large standard deviations). Quarterly hiring and separation rates are high among white workers (36 percent) and higher still among Hispanic (40 percent) and particularly black workers (48 percent), suggesting considerable workforce churn in this sector.[11] Average weekly earnings for black workers are also considerably lower than for their Hispanic and white coworkers.

Minimum wages We observe 580 changes in federal and state minimum wages between 1982 and 2019, with an average size of \$0.48 (8.4 percent). The bottom line in Fig. 1 represents 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.

2. Wage elasticities by race/ethnicity

To determine whether minimum wages affect the racial wage gap, we first estimate whether the policies have different wage effects on different racial/ethnic groups. The minimum wage setting is complex

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.

[10] The QWI fuzzes certain data cells to protect confidentiality. However, this only happens at the county level, whereas our QWI-based analyses are on the state level. Entry of states into the QWI program was staggered. In our baseline specification, we use all data as provided in the QWI.

[11] Note that the hiring rates are relative to employment, not population. A high hiring rate implies that many black employees are new hires, not that many black persons find a job in this sector.

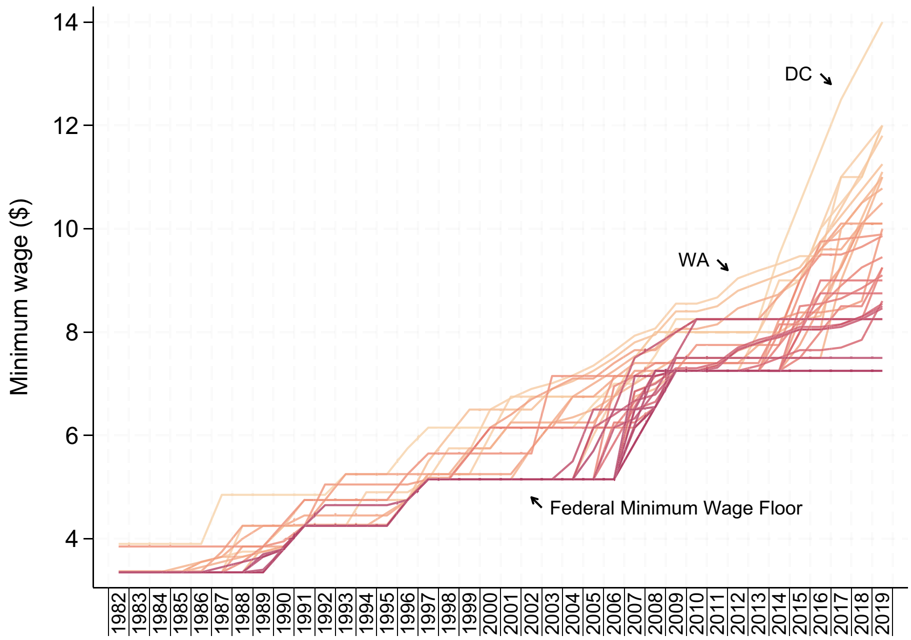

Fig. 1. Minimum wage evolution by state, 1982–2019. *Notes*: Figure shows evolution of effective minimum wages by state. The bottom line is the federal minimum wage floor, all lines above show state-level activity. Replication tag: #figure-mw-overtime.

from an econometric standpoint because minimum wage changes are both staggered (they do not all occur at the same time) and repeated (the same state can increase the minimum wage multiple times). As a result, both standard fixed effects models and standard staggered treatment effects models (e.g., Callaway and Sant'Anna, 2021; de Chaisemartin and d'Haultfoeuille, 2020) struggle to accommodate the dynamics of the minimum wage.

Stacked event study Instead, we base our preferred method on the stacked event study approach described in Cengiz et al. (2019, Online Appendix D) and adapted to a setting with a frequently changed and continuous treatment variable. The idea is to consider each minimum wage change as a separate event study, where the changing state is the treated unit and all other states are controls. Time is always defined relative to the event studied. The wrinkle is that those control states might experience one or more minimum wage changes during the event window. Therefore, we add control variables that accumulate any minimum wage changes over the event window, separately for small and large changes.[12] Stacking all events leads to following regression equation,

$$y_{sqe} = \sum_{\tau=-2}^{4} \alpha_\tau I^{\tau}_{sqe} \Delta mw_{sqe} + \mu_{se} + \mu_{qe} + \omega_{sqe} + \epsilon_{sqe} \tag{1}$$

where y_{sqe} is the average hourly wage (2019$) in state s, quarter q, duplicated for each event e (if quarter q is in the window of event e). I^{τ}_{sqe} indicates whether the event e happened in state s and if quarter $q \in [q_e + 4(\tau - 1), q_e + 4\tau)$, where q_e is the event quarter. For example, if $\tau = 1$, then the indicator variable will be one during the event and the subsequent three quarters. This leads to three pre-treatment and four post-treatment years, with separate treatment effects α_τ each. We omit the indicator for the first pre-treatment year α_{-1}. Combined with the event-specific state and time fixed effects $\mu_{se} + \mu_{qe}$ this step ensures that all estimates are relative to that pre-treatment year.

We scale the treatment indicators with the size of the minimum wage change Δmw_{sqe} (log difference), as it is possible that larger changes have stronger effects.[13] Finally, ω_{sqe} controls for confounding events, which we split into regular and small events. The small event control is the running sum of all small minimum wage changes (defined as less than five percent) over the event window per state. The regular event control is the running sum of all other minimum wage changes, excluding the studied event e.

In Appendix Figure E2 we model the log minimum wage itself and show that we perfectly filter out confounding events: the *log minimum wage* to *change in the log minimum wage* elasticity is exactly zero before the event hits and exactly one after.

We consider all minimum wage changes larger than five percent (in nominal terms) between 1978–2019 as events and control separately for all smaller changes. We do not differentiate between state and federal events, but due to our setup federal events are implicitly omitted whenever there is no state variation in the effective change in the minimum wage.[14] Appendix Figure E3 shows all events by state, quarter and type.

We apply the stacked event study described in Eq. (1) separately in each racial/ethnic group and in two complementary settings: workers with at most a high school diploma earning less than $20 per hour (CPS), shown in Fig. 2; and workers in the food services sector (NAICS 722, QWI) in Fig. 3.

In both figures we find that wage elasticities are larger for both black and Hispanic workers than for their white counterparts. The post-treatment wage elasticities (averaged over all post periods) for black workers are respectively +0.15 (s.e. 0.05) and + 0.17 (0.02) compared to +0.09 (0.01) and +0.15 (0.02) for white workers.[15] These estimates imply a relative difference of +63 percent in the high school or less sam-

[12] Cengiz et al. (2019) employ the same distinction, but in their context the controls are dummy variables which do not accumulate over multiple changes. They also consider federal events as separate, which is not required in our approach. See also footnote [14].

[13] This scaling is not present in Cengiz et al. (2019) because their observations are wage cells, for example [$7.25, $7.50) and whether a cell is affected by minimum wage policy depends on the level of the new minimum wage, not the size of the change.

[14] Our conclusions remain unchanged if we consider federal events as control events. Cengiz et al. (2019) omit federal events which makes sense in their binned setup (which requires variation in the level of the minimum wage between states). However, as our analysis is based on the *change* in the minimum wage, we can still extract information from federal minimum wage changes even if some states only had different minimum wage levels *before* the change.

[15] The minimum wage literature finds similar elasticities among all affected workers. As we report in Appendix Figures E7 and E8, we do not detect significant employment or hours effects.

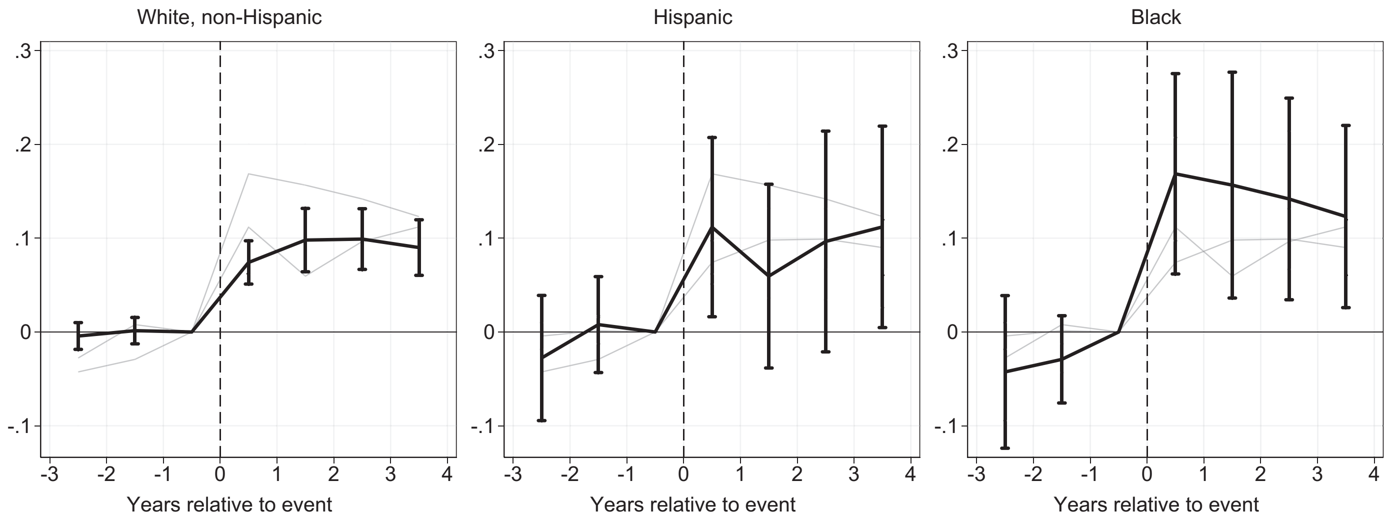

Fig. 2. Stacked event study, average hourly wage as dependent variable. Workers with at most a high school diploma earning less than \$20 per hour. 1982–2019. *Notes*: Figure shows wage elasticities by racial/ethnic group for workers with at most a high school diploma earning less than \$20 per hour. Faded lines show estimate of the other groups. Elasticities are larger for both Hispanic and black workers than for their white counterparts, but with considerable noise. Analysis at the event-quarter-state level, data based on CPS data. Handles show 95 percent confidence intervals with standard errors clustered at the state level. Replication tag: #figure-ses-cps-earnings.

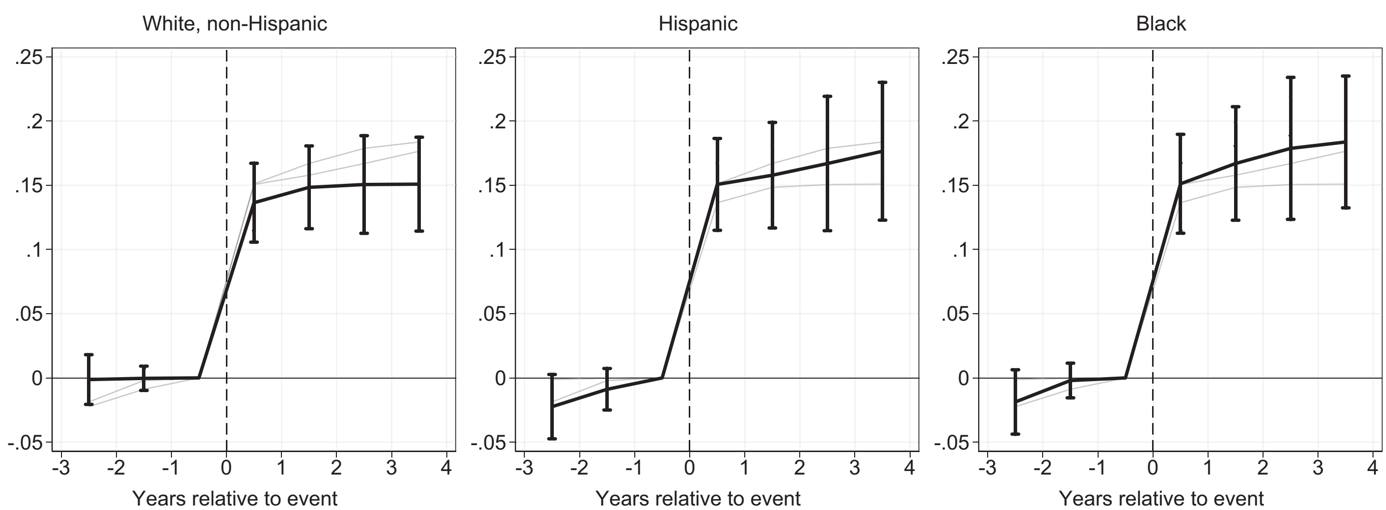

Fig. 3. Stacked event study, average weekly wage as dependent variable. Food services sector (NAICS 722), 1990–2020. *Notes*: Figure shows wage elasticities by racial/ethnic group for workers in the food services sector (NAICS 722). Faded lines show estimate of the other groups. Elasticities are larger for both Hispanic and black workers than for their white counterparts. Analysis at the event-quarter-state level, data based on QWI data. Handles show 95 percent confidence intervals with standard errors clustered at the state level. Replication tag: #figure-ses-qwi-earnings.

ple and +16 percent in food services. We do not detect pre-treatment trends in any of the scenarios.[16]

Binned estimator One downside of the stacked event study methodology is that it requires an *ex ante* definition of the potentially affected population, with the standard trade-offs between recall (capturing a meaningful share of affected persons) and precision (excluding unaffected persons).[17] Cengiz et al. (2019) also offer an alternative approach: zoom in on employment counts around the minimum wage level and track employment and wage effects over the entire relative wage distribution.[18]

We start by allocating all workers to 25 cent real wage bins, from $[1.25, 30]$ and count employment by bin.[19] Dividing by total population gives the dependent variable of Eq. (2),

$$\frac{E_{sqj}}{N_{sq}} = \sum_{\tau=-2}^{5} \sum_{k=-5}^{5} \alpha_{\tau k} * I_{sqj}^{\tau k} + \mu_{sj} + \rho_{qj} + \Omega_{sqj} + u_{sqj} \tag{2}$$

where s and q still index states and quarters and j indicates the wage bins. The treatment indicators $I_{sqj}^{\tau k}$ activate when bin j is within k dollars of a new minimum wage effective in state s since $[q + 4(\tau - 1), q + 4\tau)$.

[16] Appendix Figures E9 and E10 show that our results also hold if federal events are not considered treatment events, but that this reduction in the number of events considerably reduces the efficiency of the estimates.

[17] See Cengiz et al. (2021) for an extensive discussion on the precision and recall of various subpopulations.

[18] This *alternative approach* constitutes the main method put forward by that paper. It is also known as the bunching estimator and the distributional difference-in-differences estimator. *Relative* wage distribution here refers to wages relative to the new minimum wage set by each event.

[19] The two endpoint bins capture any workers outside that range. The bins are constructed separately by racial/ethnic group. We adjust the number of workers with the QCEW multiplier to improve precision, as described in Cengiz et al. (2019).

We again omit the pre-treatment year, such that the $\alpha_{\tau k}$ coefficients indicate how the employment-to-population ratio in bins within k dollars of the new minimum wage change relative to their pre-treatment year values. In the baseline we focus on the $[-4, 4]$ range and include endpoint coefficients for bins outside that range.[20] We include bin-by-state fixed effects μ_{sj} and bin-by-quarter fixed effects ρ_{qj}. Ω_{sqj} controls for small and federal increases.[21] The wage bin setting requires an adapted event definition: events qualify only if they change the real minimum wage by at least 25 cents; and we omit federal events (but control for them through Ω_{sqj}).

Figure 4 shows results over the relative wage distribution, averaged over the post period. We provide the intuition behind the calculation of each result here and refer to Cengiz et al. (2019, pp. 1417–1419) for the minutiae.[22,23]

There are so few low wage black workers in the CPS that over 50% of state-quarter-wage bins are imputed zeroes. As a result, the estimates for black workers are highly imprecise and attenuated towards zero (Panel A of Fig. 4). In Appendix B we provide more details on the lack of low wage black worker observations in the CPS, as well as the impact that has on the binned estimator.

In order to still gain some, albeit only suggestive, insight on the racial heterogeneity of minimum wage effects throughout the wage distribution we focus on the pooled results of black and Hispanic workers for the bunching estimator analysis (Panel D). We then turn to the split results to differentiate the contribution of each group.

The impact of the minimum wage on employment is visualised through the blue bars, which show the minimum wage induced change in the employment-to-population ratio of each relative wage group relative to the pre-treatment year period. Each bar is the average of five $\alpha_{\tau k}$ coefficients (the number of post-treatment years) and combines data from twenty wage bin observations (five times the four bins per value of k, here depicted on the x axis).

For both white workers and the pooled *Black or Hispanic* group we observe the expected pattern of a decline in the jobs paying less than the new minimum wage and an increase in jobs paying up to $4 dollars more.[24] However, for white workers these gains are concentrated on jobs paying up to $1 more, whereas for black and Hispanic workers we also see more higher-paying jobs appear (3 to 4 dollars above the new minimum wage).[25]

The green line shows the cumulative change to the average wage.[26] It evolves similarly for both groups in the initial part of the relative wage distribution, but where it then levels off for white workers, we see another bump for black and Hispanic workers due to the increase in workers earning $3-$4 more than the new minimum wage.

The split results in Panels A and B show that this bump is likely to be driven by black rather than Hispanic workers as the bump is particularly pronounced for black workers and it persists for black workers when we widen the relative wage groups as described in footnote [24].

As a result of this surge in higher wage jobs, the increase in the average wage of affected workers is considerably larger for black and Hispanic workers (7.1 percent, s.e. 1.2 percent) than for white workers (4.8 percent, s.e. 0.8 percent). These average wage increases correspond to wage elasticities with respect to the minimum wage of respectively 0.68 (s.e. 0.12) and 0.46 (s.e. 0.07).[27] These elasticities are considerably larger than those found in the other methods (see Table 1), mainly because the share of affected workers is particularly large in this approach.

Overall, we find considerably larger wage elasticities for black workers in all approaches, despite their differences in studied subpopulations, time periods and methods.

3. Race neutrality

The binned estimator results (Fig. 4) suggest that the larger wage gains for black or Hispanic workers derive from them acquiring higher wage jobs, rather than from topping up wages among extant low wage jobs to the new minimum wage. This finding implies that initial wages might not explain the large differences in wage elasticities between white and black workers. In this section, we test whether minimum wage policy is race neutral.

To do so, we exploit the longitudinal nature of the CPS Outgoing Rotation Group files, which report wage information twice for each respondent, twelve months apart. We reduce the panel to a pooled cross-section at the individual level, allowing us to relate wage changes to minimum wage policy and, crucially, to the person's wage at the first interview.[28] Eq. (3) describes the regression setup,

$$\begin{aligned}\left(hourlyWage_{it}^{\mathbf{B}} - hourlyWage_{it}^{A}\right) &= \beta \times \left(mw_{st}^{\mathbf{B}} - mw_{st}^{A}\right) \\ &\quad + \gamma_1 \frac{hourlyWage_{it}^{A}}{medianWage_{st}^{A}} + \gamma_2 \left(\frac{hourlyWage_{it}^{A}}{medianWage_{st}^{A}}\right)^2 \\ &\quad + \theta_s^A + \theta_t^A + \phi_s^A \times t^A + \text{individual controls}_{it}^A + \epsilon_{it}\end{aligned}$$

[20] As a robustness check, we also show results when we extend this range to $[-4, 16]$ with adjusted endpoint coefficients in Appendix Figure E4. In line with expectations we find that employment effects in the further-away wage groups are close to zero and uniformly insignificant.

[21] These are finer than specified in Cengiz et al. (2019). They interact three timing indicators: EARLY (three to two years ahead), PRE (one year ahead) and POST (up to four years past) with two wage bin indicators (four dollars above or below new minimum wage). We retain the timing indicators, but include two more wage indicators for the bins outside the four dollars above/below range to be consistent with the specification of k.

[22] We maintain their notation, but refer to quarters q instead of t as later analyses in this paper are at the yearly or monthly level.

[23] Appendix Figure E11 shows the binned estimator results over time rather than over the relative wage distribution. We do not detect any pre-trends.

[24] This pattern is absent for the black-only results. However, we show in Appendix Fig. B2 that it returns to visibility if we widen the relative wage groups to $2 groups, thus reducing the sample size issues present for black workers.

[25] The discrepancies do not arise from differences in estimation methods; our binned results are identical to theirs when we apply our estimation code to their data. Sample differences might explain the different binned results in the two papers, as many cells for black workers are very small. Differences in the classification and codification of treatment and confounding events may be more important. Figure A.1 in Cengiz et al. (2019) indicates that they classify certain large minimum wage events (>$0.25, some larger than $1) that affect relatively few workers as small events and exclude those from their main analysis. Moreover, their treatment and control dummies frequently take on values larger than one (despite being described as 0/1 variables in Cengiz et al., 2019, page 1415). Their main control for small and federal minimum wage events exceeds one in 40% of relevant cases. We follow the paper's description and generate dummies that are either zero or one. When we use their treatment and control variables with our data, we obtain quantitatively different treatment effects, suggesting the impact of these variables is not trivial and could explain why we obtain different results for black and Hispanic workers. We would like to stress that our other outcomes are consistent with theirs: minimum wages significantly increase wages at the bottom of the wage distribution among all racial and ethnic groups and do not meaningfully affect employment. In any case, we rely more on the ensemble of our results (Table 1) rather than just the binned estimator. These other methods can factor in federal changes which are disproportionately relevant for black workers and more than double the number of usable minimum wage events.

[26] We define the cumulative change in the average wage w_k^r as $\frac{WB_k}{E_k}/\bar{w}_{-1}$ where $\bar{w}_{-1}$ is the average pre-treatment wage of workers earning less than the new minimum wage, $WB_k = \sum^k \overline{WB}_{-1} + \alpha^k * (k + \overline{MW})$ is the running sum of the wage bill and $E_k = \sum^k \bar{E}_{-1} + \alpha^k$ is the running sum of affected employment counts. α^k is the post-treatment average of the $\alpha_{\tau k}$ coefficients. Fewer jobs below the initial average wage increase the average wage, fewer jobs *above* the initial average wage will decrease the average wage (even if they are *below* the new minimum wage).

[27] Appendix Table E1 shows that this result continues to hold when we widen the wage window considered up to six dollars above the minimum wage.

[28] To our knowledge, this is the first time the longitudinal nature of the CPS ORG files have been used to study minimum wage effects.

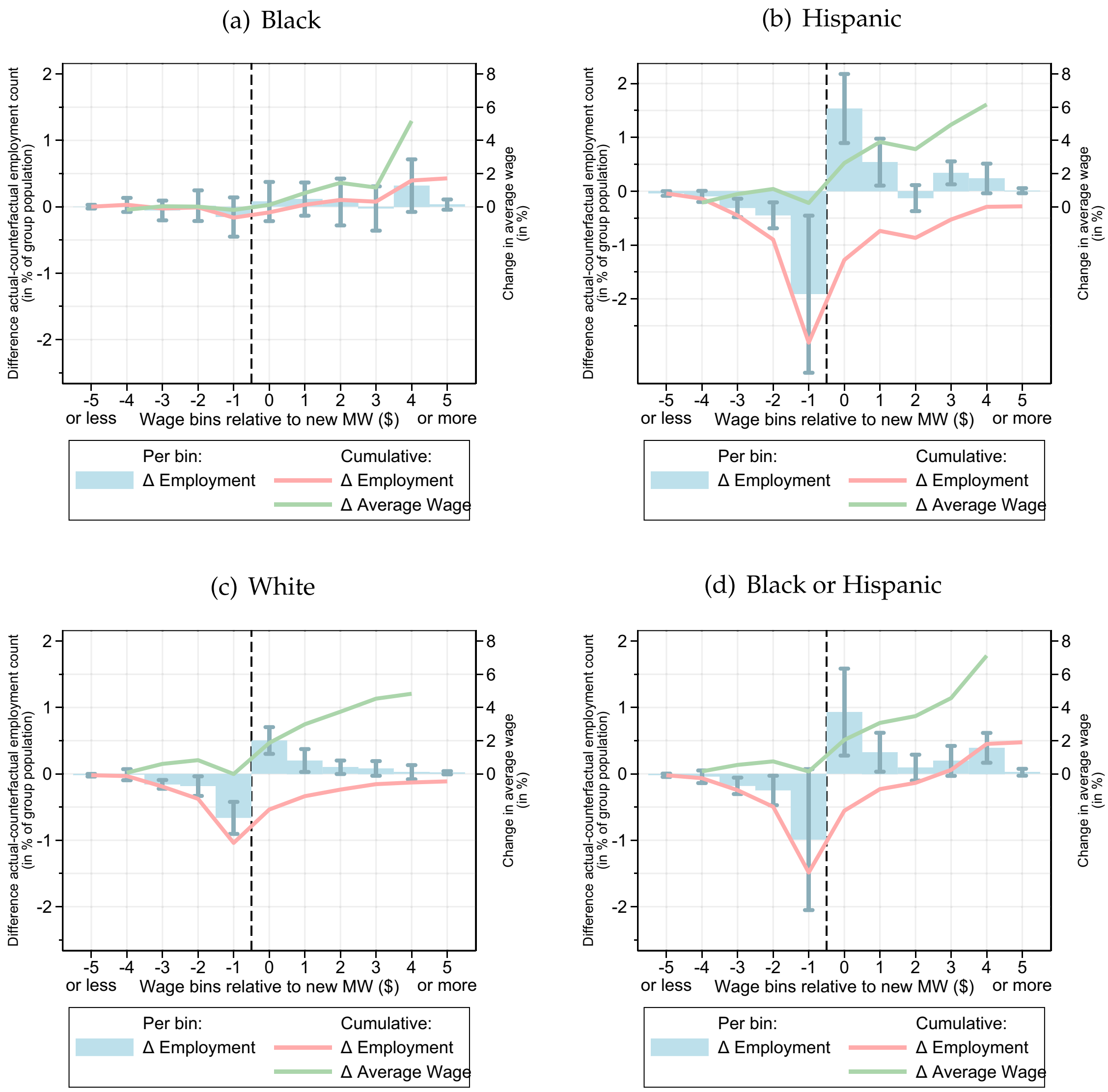

Fig. 4. Binned estimator, employment and wage effects. 1982–2019. *Notes*: Figure shows wage and employment effects by relative wage and racial/ethnic group. Bin-specific and cumulative employment effects are on the left axis, the changes in average wage on the right axis. Regressions based on Eq. (2). Wage effects are larger for Hispanic and black workers than for their white counterparts, mainly due to employment gains further up the relative wage distribution. Analysis at the bin-quarter-state level, data based on the CPS. Handles show 95 percent confidence intervals with standard errors clustered at the state level. Replication tag: #figure-binned-hisp-black-uniformaxis.

where the superscripts A and B indicate that values are respectively from the first or second interview. The dependent variable is the log change in the deflated hourly wage of individual i between the first and second interview (at respectively month t and $t+12$). The variable of interest is the change in the log minimum wage; β thus measures the wage elasticity.

We control flexibly for the separate earnings profiles of black and white workers by including the ratio of the individual's initial hourly wage to the state median wage that month, both linearly and squared. We include this control because workers at different locations in the wage distribution may experience different wage growth even in the absence of minimum wage changes (Dustmann et al., 2022, Figure 2).[29] We estimate all regressions separately for each racial/ethnic group, thereby filtering out racial differences in wage growth patterns.[30] We include state fixed effects θ_s^A, month fixed effects θ_t^A and state-specific time trends $\phi_s^A \times t^A$. Finally, we control for individual characteristics through age, gender, married status and educational achievement dummies.

We restrict the sample to workers earning up to 1.5 times the minimum wage during their initial interview, using the 24-month average of the minimum wage to avoid contaminating results through this sample selection method. We perform placebo tests on workers initially earning 1.5–2.5 times the averaged minimum wage.

Table 4 shows that wage elasticities continue to be larger for black and Hispanic workers than for white workers (0.22 and 0.20 vs. 0.13). Estimates are insignificant and close to zero in the placebo group of higher paid workers.[31]

[29] 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 Eq. (3). Results available on request.

[30] This approach thus allows that some white workers are in low-wage bins only temporarily.

[31] Appendix D describes how we test for pre-existing trends in this setting.

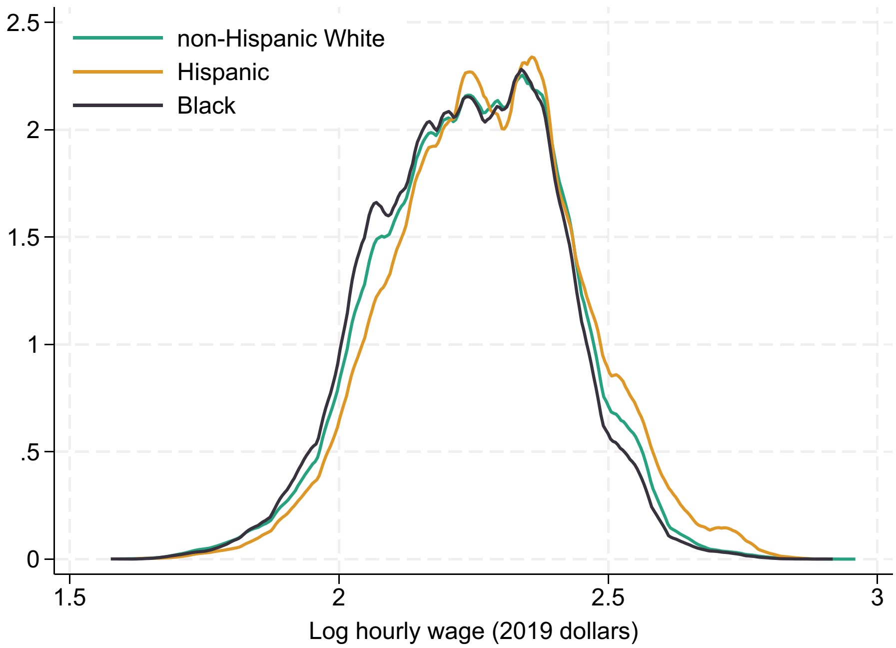

Fig. 5. Kernel density plot of initial wages of workers earning less than 1.5 times the minimum wage. 1982–2019. *Notes*: Figure shows the distribution of initial wages by race/ethnic group for workers earning less than 1.5 times the averaged minimum wage, which is the sample studied in Tables 4 and 5. Weighted by the CPS earnings weights. Kernel bandwidth: 0.02. We find that wages are very similar between groups, with wages for black workers only 1.4 percent lower and for Hispanic workers even 2.7 percent larger than for white workers. Replication tag: #figure-cpsLong-wageDistribution-sub150.

Table 4
Wage elasticities by race, pooled cross-sectional analysis. 1982–2019.

DV: Difference in real log wage	Initial wage < 1.5× MW			... between 1.5 and 2.5× MW		
	White	Hispanic	Black	White	Hispanic	Black
Log minimum wage	0.13	0.20	0.22	0.02	0.03	−0.01
	(0.03)	(0.06)	(0.08)	(0.02)	(0.04)	(0.06)
N	205,393	30,243	30,853	340,063	30,343	39,889

Notes: White refers to non-Hispanic white workers. Left panel shows results for plausibly affected workers (earning less than 1.5 times the 24-month averaged minimum wage at their initial interview), right panel is a placebo test on plausibly unaffected workers (earning 1.5-2.5 times the averaged minimum wage). Regressions are run separately by racial/ethnic group and control for individual characteristics and the initial location of the individual within the state-month specific wage distribution (see Eq. (3)). We find considerably larger wage elasticities for black and Hispanic workers than for white workers. Reassuringly, the placebo sample is entirely unaffected by minimum wage policy. Weighted using the CPS earnings weights. Standard errors are clustered at the state level and shown in parentheses. Replication tag: #table-results-cpsLong-baseline.

Table 5
Wage elasticities by race, pooled cross-sectional analysis, including entropy balanced results. 1982–2019.

DV: Difference in real log wage	Baseline (< 1.5 × MW)			Entropy balanced (idem)		
	White	Hispanic	Black	White	Hispanic	Black
Log minimum wage	0.13	0.20	0.22	0.13	0.21	0.22
	(0.03)	(0.06)	(0.08)	(0.03)	(0.06)	(0.08)
N	205,393	30,243	30,853	205,393	30,243	30,853

Notes: See Table 4. The entropy balanced (Hainmueller and Xu, 2013) results use weights generated such that the weighted average of initial wages of black and Hispanic workers is equalised to that of white workers. Replication tag: #table-results-cpsLong-reweighted.

Next, we apply an entropy balancing algorithm (Hainmueller and Xu, 2013) to balance average initial wages. The entropy balancing algorithm generates new regression weights w_i such that the weighted average of initial wages is equalised among the three racial/ethnic groups (using non-Hispanic white workers as the target group). Table 5 shows that the results remain unchanged, indicating that the different wage elasticities are not driven by differences in initial wages.

To explore this result further, Fig. 5 shows the kernel density plot of initial wages separately by racial/ethnic group for workers earning less than 1.5 times the minimum wage. The three wage distributions are highly similar, explaining why the entropy balancing exercise had almost no impact: within this selected sample, the distribution of black, white and Hispanic wages were already very close together. But the share of workers that belongs to this selected sample does differ substantially by group: from 18 percent of all white workers to respectively 27 and 36 percent of black and Hispanic workers.

4. Mechanisms

Overview The previous sections showed that black workers benefit more from minimum wage policy than white workers and that this difference cannot be explained by differences in initial wage levels.[32]

Figure 4 shows the wage gains for black/Hispanic workers are in the range of \$3 and \$4 above the new minimum wage (unlike the case for white workers). Gains this high above the minimum wage are not

[32] In the rest of this paper we focus on the mechanisms in play for black workers, leaving an analysis of the Hispanic context to future research.

likely to result from wage spillovers. Instead, we propose that higher minimum wages enable black workers to overcome spatial and racial labor market frictions. By spatial labor market friction, we refer to the distances between residences and jobs. The labor market mismatch literature has documented that these distances are much greater for black workers than for white workers. By racial labor market frictions, we refer to employer discrimination against black workers.

In this section we argue that higher minimum wages improve 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 potentially less discriminatory employers.[33]

We formalize this mechanism through a wage determination model with two types of workers, in the style of Card et al. (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. Black workers are disproportionately low outside option workers, as they live further from well-paying jobs and have fewer mobility options (Miller, 2018). Minimum wage increases allow more black workers to buy a new or used car, thereby turning them into high outside option workers with larger earnings increases.

To motivate this model, we first review the existing literature on racial disparities in spatial access to jobs and in commuting modes. We then present new empirical evidence on the effect of minimum wage policies on commuting modes and job stability by race. We conclude by providing back-of-the-envelope estimates of the magnitudes involved.

Literature A series of studies has shown that black workers are more likely to live in segregated neighborhoods in central cities, where wages are lower, and that they are less likely to own automobiles and therefore more constrained in their job searches. In the 2000 Census twenty percent of central city residents were black, versus nine percent of suburban residents (Albouy and Lue, 2015). Predicted wages (holding education, race, gender, occupation, industry, veteran, marital and immigrant status constant) in central cities were four percent below average, compared to four percent above average in suburban areas. As a result, and as Raphael and Riker (1999) and Miller (2018) have documented, there is a substantial spatial mismatch between the location of black workers' residences and the location of better-paying jobs. Moreover, although central cities have gentrified in recent decades, shifting some of the black population to suburbs, the spatial distance between jobs and workers continues to be greater for black workers (Urban Institute, 2020). For example, De la Roca et al. (2014) show that during the 2010 Census, black residents were still 78 percent more likely to live in central cities than white residents. Likewise, Schuetz et al. (2018) use 2010–14 American Community Survey data to document that this discrepancy also holds for low-income subgroups.

At the same time, black workers are more likely than white workers to use bus transit systems and less likely to use private vehicles to commute to work. In 1993–94, according to SIPP data cited by Raphael and Stoll (2001), 80 percent of white 25–34 year olds owned a car, compared to 49 percent of black 25–34 year olds. Across all ages, Dettling et al. (2017) note that in 2016 only ten percent of white households did not possess an automobile, compared to 27 percent of black households.

Because of fixed schedules and routes as multiple stops, commuting by bus is much slower than by private automobile. Burd et al. (2021) finds that commuting via public bus and subway systems takes twice as long (around 46 min) as commuting by private vehicle (26 min). The advantage in job accessibility via automobiles is thus much greater when measured in commute time than by distance. As a result, black males still spend more time traveling to work than do white males, despite white males' search distance for jobs averaging twice as far as for black males (Holzer et al., 1994).[34]

Table 6
Effect of minimum wage policy on the share of workers that commutes by car, by race. Workers with at most a high school diploma, aged 26–35. 2000–2019.

DV: Share commutes by car	White	Black
Poorest quartile	0.02 (0.02)	0.12 (0.05)
3rd quartile	0.00 (0.02)	0.11 (0.09)
2nd quartile	−0.01 (0.02)	−0.03 (0.11)
Richest quartile	0.01 (0.03)	−0.19 (0.15)

Notes: White refers to non-Hispanic white workers. The coefficients are based on stacked event studies ran separately by racial/ethnic group and by income quartile (cf. Eq. (1)). We find a significant increase in car commute shares for black workers of the bottom quartile (and an insignificant increase in the 3rd quartile). The richer quartiles are unaffected, as are white workers of all *income* quartiles. Analysis at the state-year level. Weighted using the ACS person weights. Appendix Figure E5 shows event study graphs for the two lowest income quartiles of black workers, unfortunately the samples are too small to accurately characterise effect dynamics. Standard errors are clustered at the state level and shown in parentheses. Replication tag: #table-results-acs-carCommute.

Minimum wages help overcome these transportation constraints. Cooper et al. (2020), using data on 28 metro areas, find that minimum wages led workers to acquire new or used automobiles, mainly by relaxing credit constraints, with larger effects among the credit-constrained. They also find that a ten 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 (2019) finds an increase in commuting by automobile and employment opportunities for undocumented immigrants after state-level reforms since 2013 that permit them to acquire driver's licenses.

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 outside options and thus their bargaining position, allowing them to obtain a larger share of the surplus they create (Raphael and Riker, 1999; Stoll and Covington, 2012). The minimum wage thus allows black workers to escape the poverty trap created by their lack of access to better outside options.[35]

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 6 shows the effect of minimum wages on the share of workers that commute by car for ages 26–35, by racial group and income quartile.[36] We restrict the sample to workers with at most a high school

[33] Automobile ownership can also improve choices for childcare services, which in turn can improve employment opportunities (Godøy et al., 2021).

[34] Bus systems are also less reliable than cars. Traditional commuter rail is faster than commuting by automobile, but such systems are oriented to commuting to central cities from outlying suburbs (Parks, 2016).

[35] 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. (2020) do not examine this channel. More generally, the Moving to Opportunity literature (Bergman et al., 2019) does not find that such moves affect adult employment outcomes.

[36] Income quartiles are defined at the state-year level, but not differentiated by racial group. We show results for ages 26–35 as this is the group most likely to

diploma. Each cell represents a separate stacked event study as described in Eq. (1), with the only difference that the ACS is yearly rather than quarterly.[37]

As Table 6 shows, minimum wages increase the share of black workers who commute by car in the poorest quartile (+0.12, s.e. 0.05). This effect becomes smaller and insignificant in the higher income quartiles. The effect on the poorest quartile is substantial, implying that a ten percent increase in the minimum wage raises the share that commutes by car by 1.2 percentage points.[38] We do not find any significant effects for white workers, consistent with our hypothesis that this channel is mainly relevant for black workers.

We also find– results available upon request– that commute times for black workers are not affected. The transportation literature that we cite above documents that travel speeds for most commuting trips are twice as high by car as by public transit. Our commuting time results thus suggest that black workers increase their commuting distances substantially.[39]

Our conclusion differs from Dustmann et al. (2022), who find that the introduction of a national German minimum wage increased commuting distance as minimum wage workers lost access to nearby low-wage jobs and are forced to turn to higher paid jobs further away. They thus interpret the increased commute as a negative element that offsets part of the utility of the minimum wage introduction, whereas we treat it as part of a virtuous cycle boosting wages and mobility.

A potential explanation is that the German context is very different from that in the U.S. The German public transportation system primarily involves commuting on a well-run rail system that allows for fast intra- and inter-city transport, whereas in the U.S., low-wage workers without a car mainly rely on slow and unreliable bus networks. Rail and subway public transit systems are less common, less dense and tend to serve professionals commuting in from affluent suburbs. For example, Buehler and Pucher (2012, Figure 5) show that in Germany, 60 percent of public transit trips happen by rail, versus 30 percent in the U.S.

In our setting, low wage workers shift from slow public transit to fast car transportation, which allows them to widen their search radius substantially, while keeping travel time constant or even reducing it. Higher commute distances thus do not constitute a negative effect, as the worker cares about commute *time* rather than distance.

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 extend the analysis of Fig. 3 to hiring, separation and turnover rates as dependent variables. These rates are defined relative to employment rather than population, such that for example the hiring rate equals the ratio of *new hires in a particular quarter* to *total employment at the start of that quarter*.

In Table 7 we summarize the results of these stacked event studies through the average post-treatment effect. Hiring, separation and turnover rates in the food services sector (NAICS 722) decrease for all workers after a minimum wage event (in line with findings in Dube et al., 2016) but the decline is larger for black workers. The difference is sizeable, with effects ~30% larger for black workers (e.g. a 10 percent increase in the minimum wage reduces turnover rates for black workers by 3.2 percent versus only 2.4 percent for white workers).[40]

Table 7
Stacked event study, multiple dependent variables. Food services sector (NAICS 722), 1990–2020.

	DV →	W	E	H	S	T
White	Log minimum wage	0.15	−0.02	−0.26	−0.23	−0.24
		(0.02)	(0.03)	(0.06)	(0.06)	(0.06)
Black	Log minimum wage	0.17	0.04	−0.35	−0.29	−0.32
		(0.02)	(0.05)	(0.10)	(0.10)	(0.10)
	N	347,658 events × states × quarters				

Notes: White refers to non-Hispanic white workers. Dependent variable W refers to the weekly wage, E to employment, H the hiring rate, S the separation rate and T the turnover rate. All dependent variables are in logs. Analysis at the event-state-quarter level, data based on QWI data. We find that churn decreases more for black workers, with a stronger effect of minimum wage policy on hiring, separation and turnover rates of black workers. Standard errors are clustered at the state level and shown in parentheses. Replication tag: #table-results-qwi-wehst.

Intuitively, one might expect that workers moving to better jobs would imply a turnover rate increase rather than a decrease. However, given the high initial turnover rate of low wage jobs (see Table 3), these moves rather replace bad-to-bad jobs moves with bad-to-good job moves. As previous minimum wage studies have found, improved job satisfaction and more thorough employer screening then reduce incentives for voluntary and involuntary separations, leading to lower turnover rates (Dube et al., 2016).

Magnitudes Fig. 4 suggests the disproportionate wage gains of black workers are driven by a modest surge of black workers earning around $3-4 above the minimum wage. We argue above that part of this excess wage gain is driven by increases in search radii. Below, we provide back-of-the-envelope calculations to illustrate that these kind of wage gains can be explained by our proposed mechanisms.

Although the literature on intra-urban wage differentials is scant, some evidence suggests that wages for similar workers with longer commutes are up to 20 percent higher (Ihlanfeldt, 1992; Raphael and Riker, 1999). Commuting by car to more distant jobs could thus already explain an additional wage increase of around $2 (half the excess gain).

Black workers benefit extra from expanded search radii as this also allows them to select non-discriminatory employers.[41] The extent of racial wage discrimination is often measured as the residual racial wage difference; the wage difference that cannot be explained by differences in education, age and experience, as in a Mincer-style regression. Black et al. (2013), who also control for test scores, urban residence and region in the U.S., provide the best study of the extent of racial wage discrimination (among men). Using decennial Census data, they find a residual of 24.8 percent in 1990 and 22.6 percent in 2000. If a minimum wage increase allows black workers to search more broadly and find non-discriminatory employers, that could likewise explain an additional wage increase of $2.

Adding the potential $2 gain from a longer commute to a better job and a potential $2 gain from finding a non-discriminatory employer makes an increase of $4 for a fraction of black workers plausible. Of course, there is likely some overlap between the racial discrimination and spatial labor market frictions. And minimum wage increases are not likely to entirely eliminate these frictions. On the other hand, the bars at $3 and $4 in Fig. 4 are modest in height, and are consistent with some decline in these frictions.

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 other age groups are available on request (they are all insignificant).

[37] The coefficients in Table 6 are the average over the post period. Graphs for the two lowest income quartiles of black workers are shown in Appendix Figure E5, others are available on request.

[38] On average, 18 percent of these workers do not commute by car, which makes a one percentage point change fairly sizeable.

[39] Commuting times is measured in the ACS, but commuting distance is not.

[40] We find a similar result in Appendix Table E3, where we estimate the probability workers remain employed in their second interview (cf. Eq. (3), with an employment dummy as dependent variable). We find that black workers are much more likely to remain employed after a minimum wage event (10 percent minimum wage increase leads to a two percentage point gain in the probability of remaining employed).

[41] Kline et al. (2021) finds large disparities in discriminatory behaviour between firms.

The advantages of owning a car can also be compared to the costs. If commuting by car results in a $4 per hour pay raise, full-time workers would receive an additional $8,000 per year (assuming they worked year-round). This increase in income more than offsets the cost of acquiring and operating a small vehicle and using it primarily to commute to work.

In 2019, the average round-trip automobile commute in the U.S. took 52 min. Since the average urban automobile commuting speed is 25 miles per hour, the average round-trip commuting distance is about 22 miles per workday, or 5600 miles per year for a year-round worker. Using the IRS standard mileage expense of 55 cents per mile, the average annual cost of driving a small sedan for this distance is about $3,100. This back-of-the-envelope calculation illustrates that a worker is better off acquiring a car for commuting to a job that pays $2 to $4 more than the minimum wage, compared to staying in their same job and using public transit.

The advantages of acquiring or more intensively using a private vehicle are even greater for parents of young children, whose commute includes both a childcare location and a job location. Indeed, Godøy et al. (2021) establishes that minimum wage increases lead to more use of formal childcare and to higher maternal employment rates.

Summary Overall, these findings support our hypothesis that a sizeable share of low-wage black workers are unable 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 mechanism is reflected in wage increases that exceed the mechanical effect of the wage floor, in the increasing share of low wage black workers that commute by car and in a reduction of job turnover.

5. Counterfactual estimates of effect sizes

In this section we place the wage elasticities into perspective by simulating the black-white hourly wage gap under three scenarios:

1. the actual minimum wage regime,
2. freezing minimum wages in 1982,[42]
3. minimum wage policy following California's path to $12.

Our baseline stacked event studies provide only short-term estimates of minimum wage policy. To estimate longer run effects, we therefore base this counterfactual exercise on a classic two-way fixed effect model with state-specific time trends:

$$y_{it} = \beta * mw_{st} + uRate_{st} + \theta_s + \theta_t + \phi_s \times t + \text{individual controls}_{it} + \epsilon_{it} \tag{4}$$

where y_{it} is the deflated log hourly wage of individual i in month t. We include the state-level unemployment rate $uRate_{st}$ to control for business cycle dynamics. We retain the same sample as in our baseline regressions (at most high school diploma, earning less than $20 per hour). As before, we find a higher wage elasticity for black workers (0.18, s.e. 0.03) than for white workers (0.13, s.e. 0.01) and overall results in line with the stacked event study models.[43] We present these results in Appendix Table E2.[44]

We generate wages for the three scenarios by predicting wages at, respectively, the actual minimum wage level, 1982 minimum wage levels and minimum wages equal to the California level.[45] The hourly wage gap is then the difference between the log average hourly wage, which we show in the bottom half of Fig. 6.

We find that minimum wage policy was instrumental in keeping this gap small (black line): the gap would have increased to 7.7 percent by the end of the sample had minimum wages been frozen in 1982 (red line), versus rising to just 3.4 percent under the actual minimum wage regime. Under the California to $12 policy, the gap would have dropped to 1.5 percent.

Next, we extrapolate these *affected workers* gaps to the entire economy by assuming that unaffected workers are indeed entirely unaffected. Their wages thus remain identical in all three scenarios. The top half of Fig. 6 shows racial hourly wage gaps for the entire economy under this assumption. Again, minimum wage policy substantially reduces inequality: wage gaps reach 25.3 percent without minimum wage policy versus 22.8 percent under the actual regime (−2.5 percentage points, a 10 percent drop). The California to $12 path further reduces the gap to 21.8 percent (−3.5 percentage points, a 14 percent drop).

In Appendix Figure E6 we simulate the "Raise the Wage Act of 2019" schedule, which increases the federal 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. As before, all changes occur through the at most a high school diploma, less than $20 per hour sample. This policy would further reduce the hourly wage gap to 18.2 percent.

6. Other dimensions

Throughout this paper we have focused on the wage effects of minimum wage policy on workers among different racial/ethnic groups. Here, we briefly discuss results along other dimensions: other outcome measures, such as employment and for sub-populations by age and gender.

We do not detect any disemployment effects in the stacked event studies (Appendix Figure E7), nor with the binned estimator (red lines of Fig. 4), nor in the pooled cross-sectional analysis (Appendix Table E3, where we even find an increase in the probability of remaining employed for black workers, in line with the turnover results in Table 7)[46] and not in the classic two-way fixed effects model with state time trends (Appendix Table E2).[47] We also do not detect any effects on hours worked per week (see Appendix Figure E8 and Appendix Table E2).

Appendix Table E5 shows stacked event study results by age, gender and racial/ethnic group. Wage elasticities are largest for young workers (ages 16–24) but remain positive for older workers, albeit at various levels of significance. We do not detect any differences between genders, except for Hispanics where the gains appear entirely driven by female workers. We leave a detailed analysis of this latter result for future research as this is likely to be driven by context-specific factors, such as ethnicity-specific gender structure in occupational choices.[48]

7. Summary and conclusion

Racial wage inequality has increased since the 1980s; in the same period a patchwork of state minimum wages has developed, with lower minimum wages in states with higher proportions of black workers. Our

[42] We choose 1982 because our CPS data start then.

[43] The classic fixed effects model is less reliable when minimum wages have heterogeneous effects, as some of the implicit control units will also be treated. On the other hand, the stacked event study explicitly limits the horizon studied, potentially masking (very) long term effects.

[44] Appendix D describes how we test for pre-existing trends in this setting. We do not find any evidence for pre-trends for white and black workers. For Hispanic workers we do not find a significant pretrend in terms of statistical significance, but the pattern does raise questions on the validity of the long term Hispanic estimate.

[45] For the programming-minded, this corresponds to a sequence of `regress`, followed by `replace` and `predict, xb` in Stata syntax.

[46] See also footnote [40].

[47] We report own wage employment elasticities in Appendix Table E4.

[48] One caveat for all these regressions: the number of workers in each age-race-state-quarter or gender-race-state-quarter cell can be limited, especially for the minority groups.

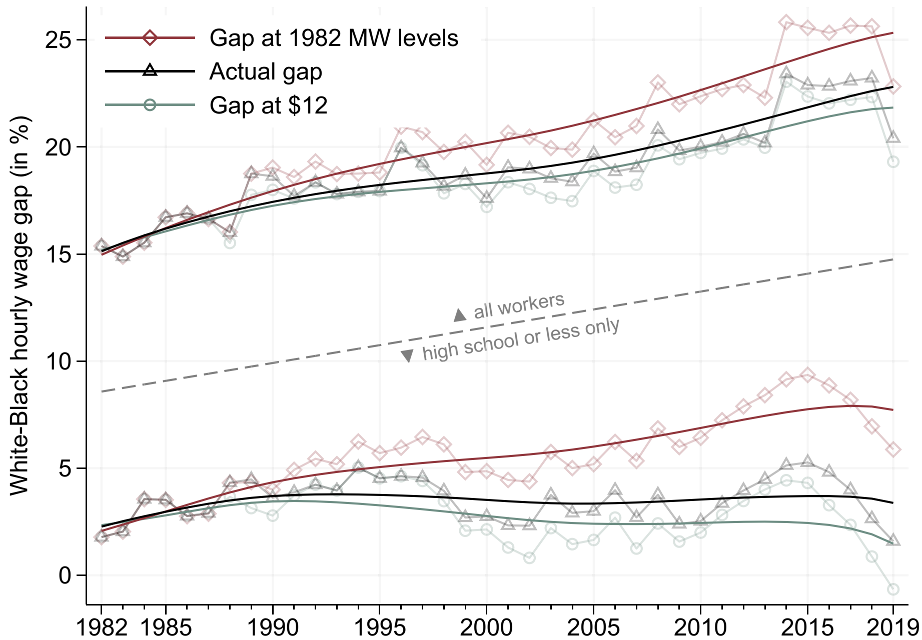

Fig. 6. Counterfactual and actual white-black hourly wage gaps over time. *Notes*: Figure shows observed (black triangle) and two counterfactual hourly wage gaps between white and black workers between 1982–2019. The first counterfactual gap predicts hourly wages for white and black workers as if there had been no changes in the minimum wage from 1982 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 earning less than $20 (2019 dollars), the top sample is all workers. Replication tag: #figure-counterfactual-actual1982cali.

causal analysis indicates that minimum wage policies have nonetheless reduced the racial wage gap (−10 percent in the overall economy, −56 percent for workers with at most a high school diploma earning less than $20 per hour). 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 in different datasets, with different methods and when analysing different sub-populations. They cannot be explained solely 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 as they start commuting more by car and as their job turnover decreases.

Data availability

Data will be made available on request.

Acknowledgement

The author gratefully acknowledges financial support of the Research Foundation Flanders (FWO) under grant numbers 1125818N and 12B1722N.

Appendix A. Washington - Mississippi 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. Precision issues in the binned estimator

The bunching estimator is demanding in terms of sample sizes because it aims to map wage- and employment effects throughout the wage distribution. Cengiz et al. (2019, p. 1421) note that in the overall sample, there are on average 5.5 observations per state-quarter-wage bin. However, this drops to less than half an observation per bin when restricted to black workers (own calculations).

The heatmaps in Fig. B1 demonstrate the stark difference between observation counts of white versus black workers earning less than $15. There is extensive coverage of low wage white workers in almost all states and quarters. The average number of observations per state-quarter exceeds thirty in all states but Hawaii and the federal District of Columbia. By contrast, there are 31 states with fewer than thirty black worker observations (< $15) and 21 even have fewer than ten black workers. The number of black worker observations also drops nationwide in the later half of the sample.[49]

This is reflected in the share of empty state-quarter-wage cells in the most relevant [$5, $15] range. Where only 15% of those cells are empty

[49] These heatmaps look the same when we use the data provided by Cengiz et al. (2019) in their replication package. Available on request.

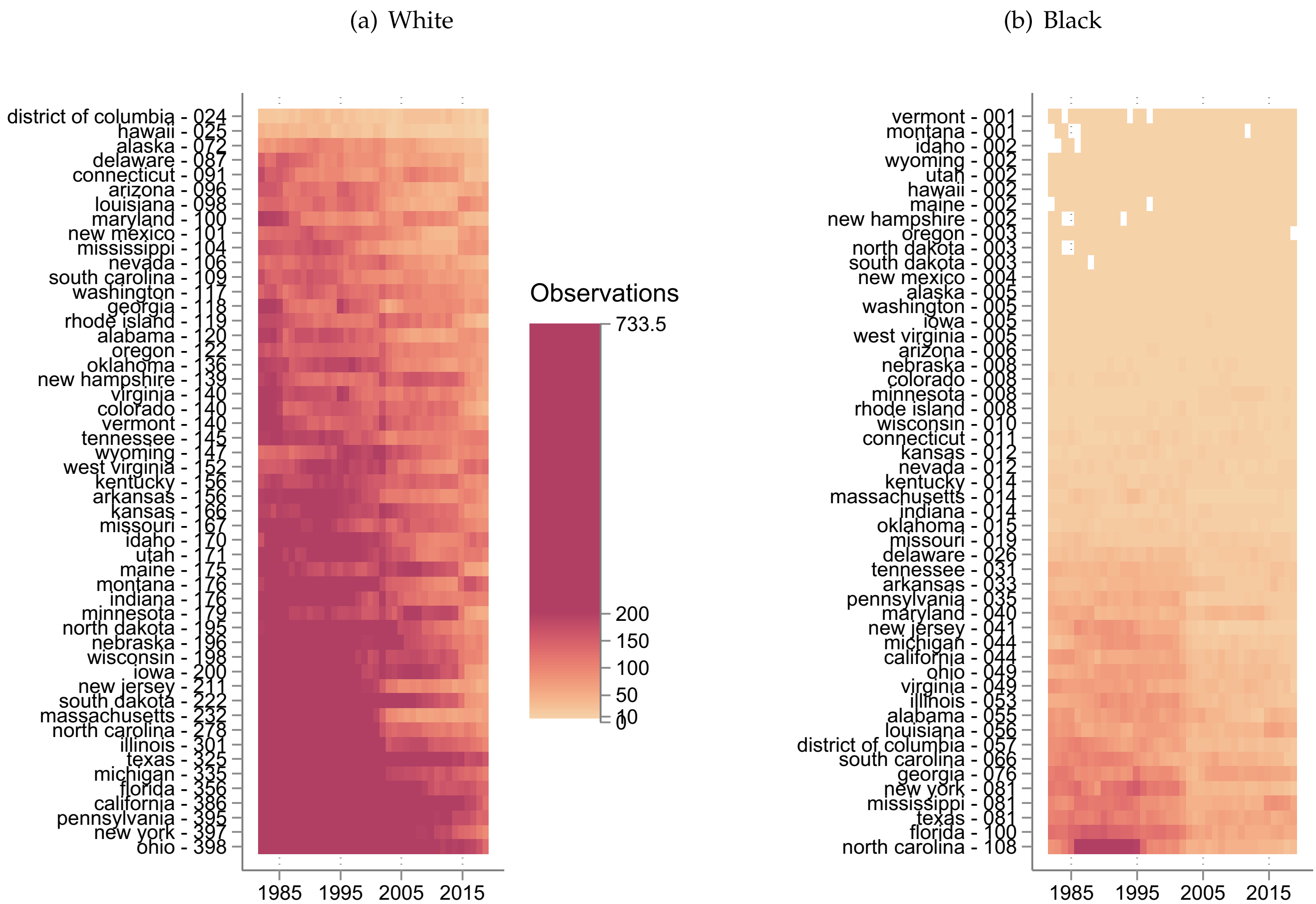

Fig. B1. Heatmap of observations per state and quarter, by race. 1982–2019. *Notes*: Heatmap shows the (unweighted) number of observations per state and quarter. States are ordered by the average number of quarterly observations, listed after the state name. The scale is identical in both heatmaps. Replication tags: #figure-heatmap-white-sub15 and #figure-heatmap-black-sub15.

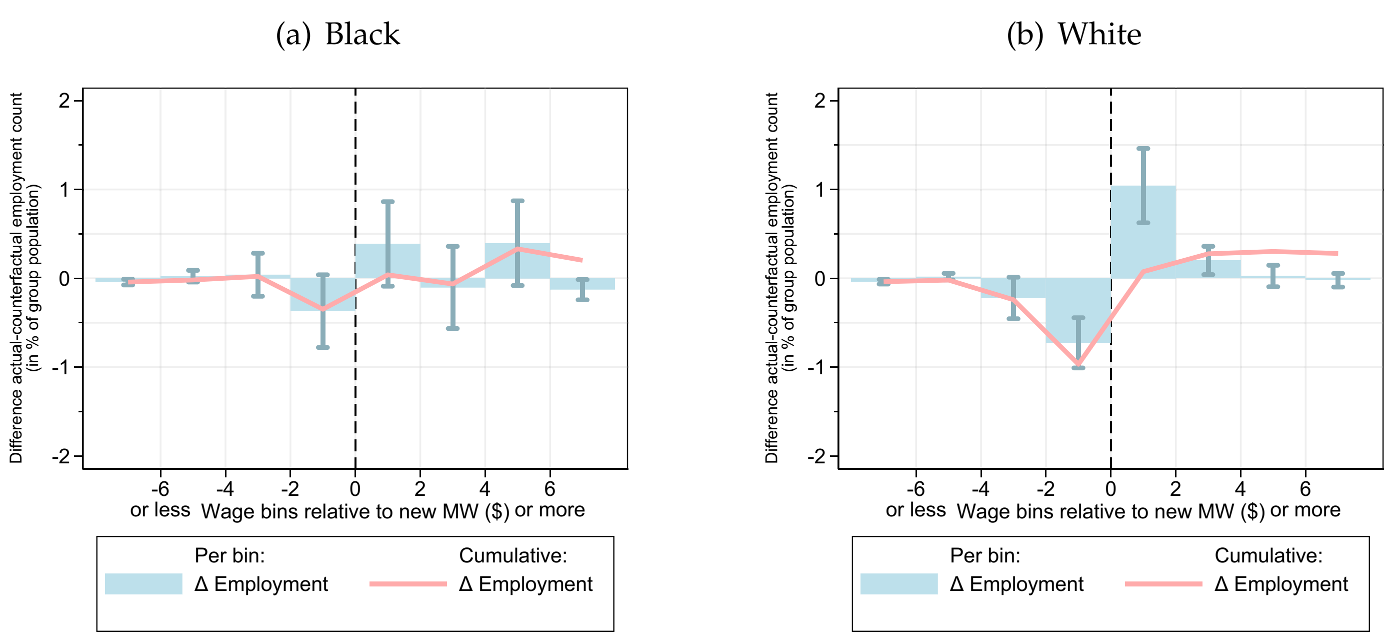

Fig. B2. Binned estimator, employment and wage effects, *wider relative wage groups*. 1982–2019. *Notes*: Figure shows employment effects by relative wage and racial/ethnic group. Regressions based on Eq. (2), but the indicator $I_{sqj}^{\tau k}$ now activates when bin j is within $[k, k+2)$ dollars of a new minimum wage. Analysis at the bin-quarter-state level, data based on the CPS. Handles show 95 percent confidence intervals with standard errors clustered at the state level. Replication tag: #figure-binned-white-black-doubleSized.

for white workers, more than 50% are empty for black workers.[50] In line with Cengiz et al. (2019), we impute zero employment in these empty cells.

Consistent with classic measurement error theory, this leads to attenuation bias towards zero as potential minimum wage effects are not picked up in zero-imputed cells. Indeed, the coefficients for black workers in Panel A of Fig. 4 are indistinguishable from zero, with the exception of the surge at $4. The results for white workers in Panel C do follow the classic pattern, with the largest effects in jobs around the minimum wage, but without the bump at $4.

Pooling black and Hispanic workers reduces the share of zero-imputed cells from 50% to 35%, bridging half the gap with white workers. The pooled results in Panel D of Fig. 4 show both the classic pattern of large effects around the minimum wage, and the bump at $4 present in the black-only results. The absence of the bump in the Hispanic-only results of Panel D shows that this excess effect is driven by black rather than Hispanic workers.

An alternative approach to reduce the sample requirements of the binned estimator is to widen the relative wage groups to $2 (versus $1 in Eq. (2)). Panel A of Fig. B2 shows a relatively classic employment pattern for black workers with employment gains and losses concentrated around the new minimum wage. Nevertheless, it also exhibits the exceptional bump in the $4 region, supporting the previous findings. The pattern for white workers in Panel B remains qualitatively unchanged.

Appendix C. Wage determination 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 observe without cost. 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

$$u_{ij} = \beta ln(w_{ij} - b_i) + v_{ij} \tag{C1}$$

where v_{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.

$$b_i = w_i^b - \alpha_{S_i} * d_i^b \tag{C2}$$

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

$$p_{ij} = P\left(\underset{k\in 1,\dots,J}{\arg\max}\, u_{ik} = j\right) = \frac{e^{\beta ln(w_{ij}-b_i)}}{\sum_{k=1}^{J} e^{\beta ln(w_{ik}-b_i)}} \tag{C3}$$

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

$$p_{ij} \approx \lambda_i e^{\beta ln(w_{ij}-b_i)} \tag{C4}$$

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

$$L_{ij}(w_{ij}) = p_{ij} = \lambda e^{\beta ln(w_{ij}-b_i)} \tag{C5}$$

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

$$\epsilon_{ij} = \frac{\beta w_{ij}}{w_{ij} - b_i} \tag{C6}$$

Demand side The firms solve the following cost minimisation problem

$$\min_W C_j = \sum_{i=1}^{K} w_{ij} * L(w_{ij}) \qquad s.t. \quad T_j f[L(W)] \geq Y \tag{C7}$$

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 $L(W) = \{L_{1j}(w_{1j}, \dots, L_{Kj}(w_{Kj})\}$.[51] For simplicity, we ignore capital and intermediate inputs.

The K first order conditions of this optimisation problem can be written as

$$w_{ij}\frac{1+\epsilon_{ij}}{\epsilon_{ij}} = T_j f_i \mu_j \tag{C8}$$

where μ_j represents the marginal cost of production which the firm will equate to marginal revenue at an optimal choice for Y.[52] f_i is the derivative of f with respect to L_{ij}.

Equilibrium Combining the demand-side Eq. (C8) with the supply-side Eq. (C6) provides following expression for the equilibrium wage w_{ij}

$$w_{ij} = \frac{\beta}{1+\beta} T_j f_i \mu_j + \frac{b_i}{1+\beta} \tag{C9}$$

$$= \frac{\beta}{1+\beta} T_j f_i \mu_j + \frac{w_i^b - \alpha_{S_i} * d_i^b}{1+\beta} \tag{C10}$$

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 ($S_i = L$).[53] 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 sufficiently 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,[54]

$$P(S_i = H | w_i) = P(w_i + e_i > \delta) \tag{C11}$$

where δ is some threshold to becoming more mobile and e_i bundles any relevant individual characteristics. We can interpret Eq. (C11) as the

50 Weighted by state population to be consistent with the regression setup. Replication tag: #number-emptycells

51 We assume f is twice differentiable.

52 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.

53 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.

54 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.

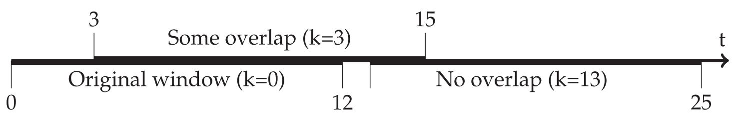

Fig. D1. Within individual estimator. Time placebo illustration. *Notes*: Illustration of the time placebo concept. The initial minimum wage window of Eq. (3) is $t \in [0, 12]$. At $k = 3$, the time placebo still overlaps the original window and can thus pick up actual effects. At $k = 13$ there is no overlap left and we would expect a zero effect (as we do not expect future minimum wages to affect contemporaneous wages).

reduced form of a budget constraint,

$$\text{wealth}(w_i, e_i) + \text{credit}(w_i, e_i) > P_{car} \tag{C12}$$

which states that the individual will only buy a car if she currently has sufficient wealth and credit options to pay for it.[55] 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 $b_i = w_i^b - \boldsymbol{\alpha}_{\mathbf{H}} * d_i^b$. In turn, this allows her to renegotiate her wage to

$$w_{ij} = \frac{\beta}{1+\beta} T_j f_i \mu_j + \frac{w_i^b - \boldsymbol{\alpha}_{\mathbf{H}} * d_i^b}{1+\beta} \tag{C13}$$

because the firm can now appropriate less of its proximity-based rent.[56]

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 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 $w_0 < MW$:

$$\Delta w^{HH}(w_0) = MW - w_0 \tag{C14}$$

$$\Delta w^{LL}(w_0) = MW - w_0 \tag{C15}$$

$$\Delta w^{LH}(w_0) = MW - w_0 \qquad \text{if } w_{ij}^H \leq MW \tag{C16}$$

$$\Delta w^{LH}(w_0) = w_{ij}^H - w_0 \qquad \text{if } w_{ij}^H > MW \tag{C17}$$

where we add a superscript H to w_{ij} in Eqs. (C16) and (C17) 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 Eq. (C11)) because they tend to live in areas with fewer job opportunities (cf. Bergman et al., 2019; Stoll and Covington, 2012), have to exert more search effort due to discrimination in the labour market (Kline et al., 2021) and have lower initial wealth and credit access (Dettling et al., 2017).

Appendix D. Pretrend analysis in the within-individual and classic panel models

Within individual estimator In Section 3 we employ a within individual estimator to establish the race non-neutrality of the minimum wage. Here, we use a time placebo test to verify the estimator is free of pre-existing trends. We adapt Eq. (3) to:

$$\begin{aligned}
&-5pt\left(hourlyWage_{it}^{\mathbf{B}} - hourlyWage_{it}^{A}\right) \\
&= \beta_k \times \left(mw_{s,t+k}^{\mathbf{B}} - mw_{s,t+k}^{A}\right) \\
&+\gamma_1 \frac{hourlyWage_{it}^{A}}{medianWage_{st}^{A}} + \gamma_2 \left(\frac{hourlyWage_{it}^{A}}{medianWage_{st}^{A}}\right)^2 \\
&+\theta_s^A + \theta_t^A + \phi_s^A \times t^A + \text{individual controls}_{it}^A + \epsilon_{it}
\end{aligned}$$

such that we estimate sixteen separate regressions, moving the minimum wage change variable $\left(mw_{s,t+k}^{B} - mw_{s,t+k}^{A}\right)$ forward one period each time. If the β of the initial Eq. (3) is a causal estimate of the minimum wage effect, then we would expect β_k to be decreasing in k for $k \in [1, 12]$ and stable around zero afterwards. We illustrate this concept in Fig. D1 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 ($k = 13$).

As Fig. D2 shows, we indeed find positive wage effects for each racial group at the original window ($k = 0$, cf. Table 4), 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 indicate the within individual results are not affected by confounding time trends.

Classic panel We estimate a two-way fixed effects model with state-specific time trends in Section 5 to reach a back-of-the-envelope estimate of the reduction in the racial wage gap due to minimum wage policy. We test for pretrends by adapting Eq. (4):

$$\begin{aligned}
y_{it} = &\sum_{k=0}^{5} \beta_k * mw_{s,t+3k} + uRate_{st} \\
&+ \theta_s + \theta_t + \phi_s \times t + \text{individual controls}_{it} + \epsilon_{it}
\end{aligned} \tag{D2}$$

where we include five leads of the minimum wage $mw_{s,t+3k}$ to capture whether hourly wages were on a different trajectory in quarters before the minimum wage increases. Figure D3 shows there are no significant pre-existing trends for any racial/ethnic group, but that the estimates are relatively noisy. Especially for Hispanic workers one could easily fit a trend through the confidence intervals, which might explain why that wage elasticity is inconsistent with other evidence (as it finds a lower wage elasticity for Hispanic workers than for white workers).

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

[56] 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.

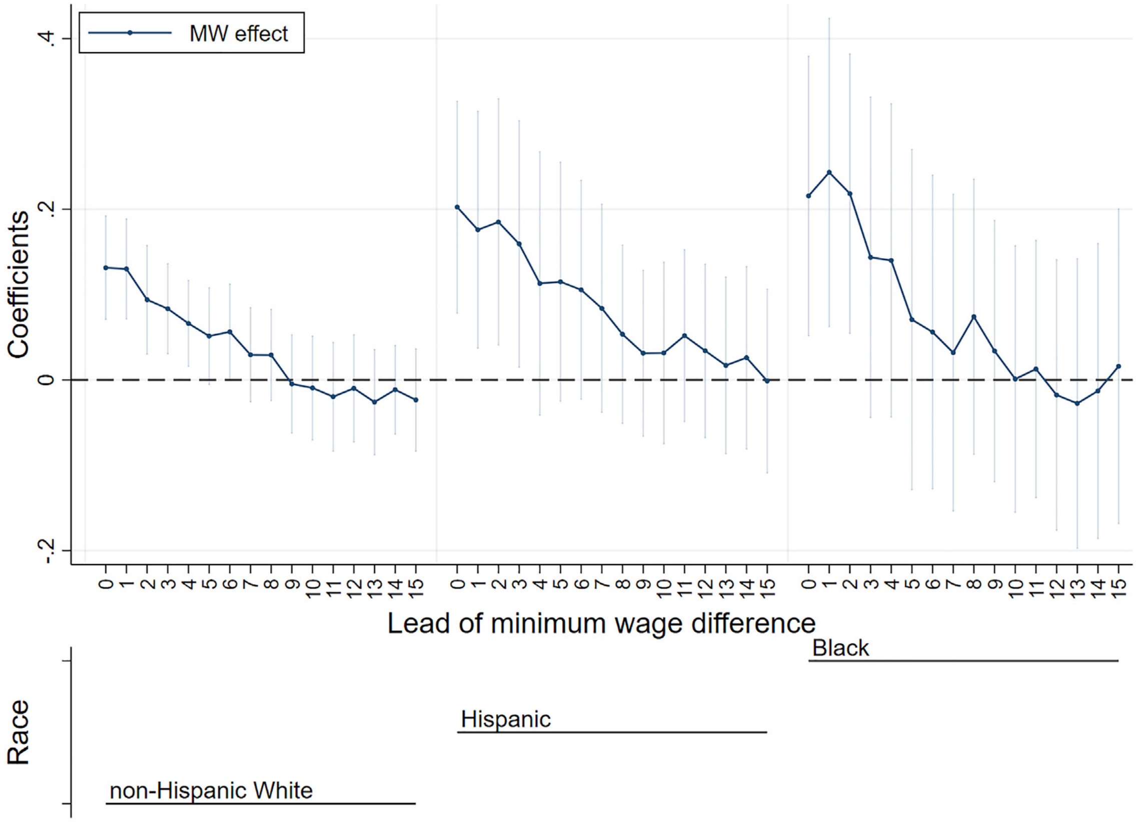

Fig. D2. Within individual estimator. Time placebo test. *Notes*: Figure shows time placebo test for the baseline estimates of Table 4 (within-individual estimates of the effect of minimum wages on earnings growth over one year, workers with initial wage < 1.5× the minimum wage). The first panel shows 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 $(mw_(s, t+k)^B - mw_(s, t+k)^A)$. 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 (see Fig. D1). The vertical bars show 95 percent confidence intervals with standard errors clustered at the state level. Replication tag: #figure-cpsLong-pretrends.

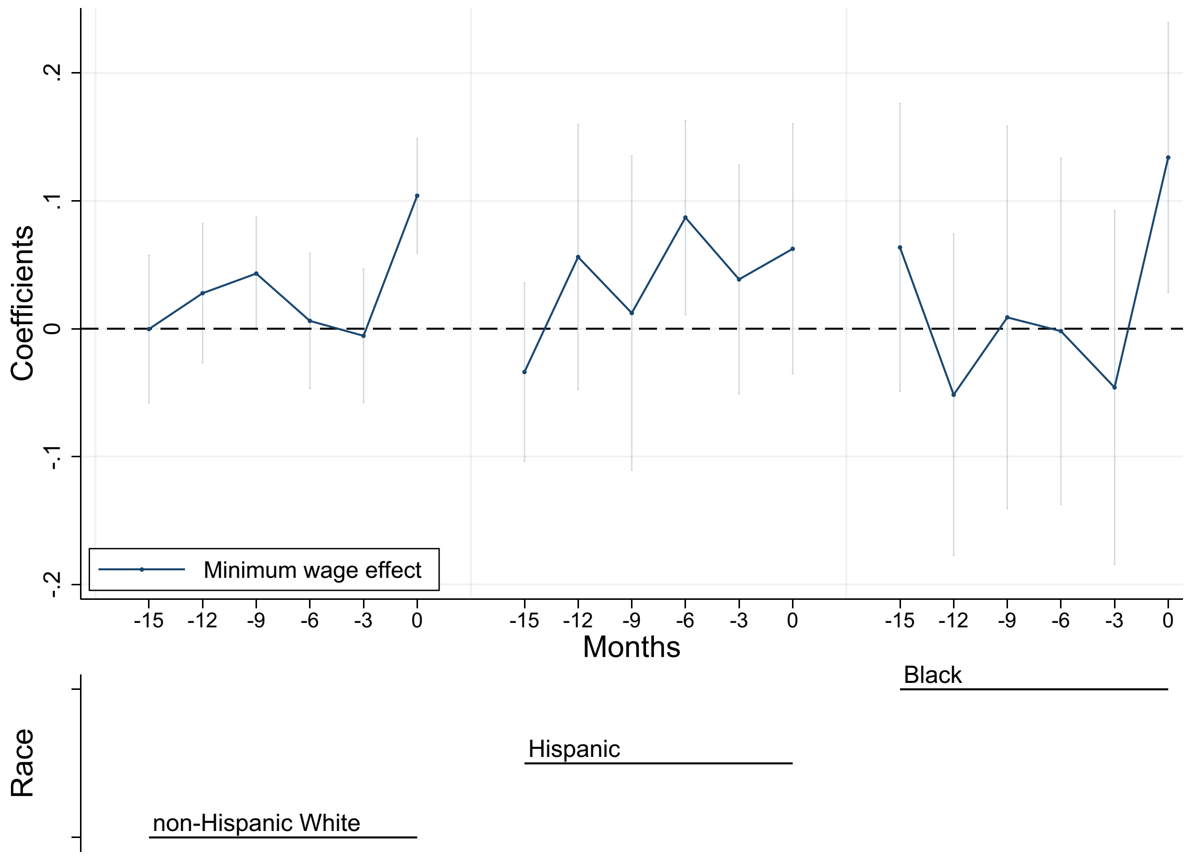

Fig. D3. Classic panel estimator with two-way fixed effects and state-specific time trends. Time placebo test. *Notes*: Figure shows time placebo test for the wage estimates of Table E2. The first panel shows the effect for non-Hispanic white workers, the second for Hispanic workers and the final panel for black workers. The axis shows the cumulative coefficient of leads of the minimum wage variable. The vertical bars show 95 percent confidence intervals with standard errors clustered at the state level). Replication tag: #figure-cpsPanel-pretrends.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.labeco.2023.102344.

References

Aaronson, D., Agarwal, S., French, E., 2012. The spending and debt response to minimum wage hikes. Am. Econ. Rev. 102 (7), 3111–3139.

Albouy, D., Lue, B., 2015. Driving to opportunity: local rents, wages, commuting, and sub-metropolitan quality of life. J. Urban Econ. 89, 74–92.

Allegretto, S.A., Dube, A., Reich, M., 2011. Do minimum wages really reduce teen employment? Accounting for heterogeneity and selectivity in state panel data. Ind. Relat. 50 (2), 205–240.

Andersson, F., Haltiwanger, J.C., Kutzbach, M.J., Pollakowski, H.O., Weinberg, D.H., 2018. Job displacement and the duration of joblessness: the role of spatial mismatch. Rev. Econ. Stat. 100 (2), 203–218.

Baum, C.L., 2009. The effects of vehicle ownership on employment. J. Urban Econ. 66 (3), 151–163.

Belman, D., Wolfson, P.J., 2014. What Does the Minimum Wage Do?. WE Upjohn Institute.

Bergman, P., Chetty, R., DeLuca, S., Hendren, N., Katz, L.F., Palmer, C., 2019. Creating Moves to Opportunity: Experimental Evidence on Barriers to Neighborhood Choice. Working Paper. National Bureau of Economic Research doi:10.3386/w26164. http://www.nber.org/papers/w26164

Black, D.A., Kolesnikova, N., Sanders, S.G., Taylor, L.J., 2013. The role of location in evaluating racial wage disparity. IZA J. Labor Econ. 2 (1), 1–18.

de Brey, C., Musu, L., McFarland, J., Wilkinson-Flicker, S., Diliberti, M., Zhang, A., Branstetter, C., Wang, X., 2019. Status and Trends in the Education of Racial and Ethnic Groups 2018. NCES 2019-038. National Center for Education Statistics.

Buehler, R., Pucher, J., 2012. Demand for public transport in Germany and the USA: an analysis of rider characteristics. Transp. Rev. 32 (5), 541–567.

Burd, C., Burrows, M., McKenzie, B., 2021. Travel Time to Work in the United States: 2019. American Community Survey Reports. United States Census Bureau.

Callaway, B., Sant'Anna, P.H., 2021. Difference-in-differences with multiple time periods. J. Econom. 225 (2), 200–230.

Card, D., Cardoso, A.R., Heining, J., Kline, P., 2018. Firms and labor market inequality: evidence and some theory. J. Labor Econ. 36 (S1), S13–S70.

Card, D., Krueger, A.B., 1995. Myth and Measurement: The New Economics of the Minimum Wage. Princeton University Press.

Cengiz, D., Dube, A., Lindner, A., Zipperer, B., 2019. The effect of minimum wages on low-wage jobs. Q. J. Econ. 134 (3), 1405–1454. doi:10.1093/qje/qjz014. https://academic.oup.com/qje/article-pdf/134/3/1405/29173920/qjz014.pdf

Cengiz, D., Dube, A., Lindner, A.S., Zentler-Munro, D., 2021. Seeing Beyond the Trees: Using Machine Learning to Estimate the Impact of Minimum Wages on Labor Market Outcomes. Working Paper. National Bureau of Economic Research doi:10.3386/w28399. http://www.nber.org/papers/w28399

de Chaisemartin, C., d'Haultfoeuille, X., 2020. Two-way fixed effects estimators with heterogeneous treatment effects. Am. Econ. Rev. 110 (9), 2964–2996.

de Chaisemartin, C., D'Haultfœuille, X., 2021. Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: a survey. Available at SSRN.

Cho, H., 2019. Driver's license reforms and job accessibility among undocumented immigrants. Available at SSRN 3356102.

Cooper, D., Luengo-Prado, M.J., Parker, J.A., 2020. The local aggregate effects of minimum wage increases. J. Money, Credit Bank. 52 (1), 5–35.

Daly, M., Hobijn, B., Pedtke, J.H., et al., 2017. Disappointing facts about the black-white wage gap. FRBSF Econ. Lett. 26, 1–5.

De la Roca, J., Ellen, I.G., O'Regan, K.M., 2014. Race and neighborhoods in the 21st century: what does segregation mean today? Reg. Sci. Urban Econ. 47, 138–151. doi:10.1016/j.regsciurbeco.2013.09.006. SI: Tribute to John Quigley. https://www.sciencedirect.com/science/article/pii/S0166046213000720

Derenoncourt, E., Montialoux, C., 2020. Minimum wages and racial inequality. Q. J. Econ. 136 (1), 169–228. doi:10.1093/qje/qjaa031. https://academic.oup.com/qje/article-pdf/136/1/169/35073220/qjaa031.pdf

Dettling, L., Hsu, J., Jacobs, L., Moore, K., Thompson, J., 2017. Recent trends in wealth-holding by race and ethnicity: evidence from the survey of consumer finances. FEDS Notes 2017. 10.17016/2380-7172.2083

Dube, A., Lester, T.W., Reich, M., 2016. Minimum wage shocks, employment flows, and labor market frictions. J. Labor Econ. 34 (3), 663–704. doi:10.1086/685449.

Dustmann, C., Lindner, A., Schönberg, U., Umkehrer, M., vom Berge, P., 2022. Reallocation effects of the minimum wage. Q. J. Econ. 137 (1), 267–328. doi:10.1093/qje/qjab028. https://academic.oup.com/qje/article-pdf/137/1/267/41929667/qjab028.pdf

Flood, S., King, M., Rodgers, R., Ruggles, S., Warren, J. R., 2020. Integrated public use microdata series, current population survey: Version 7.0 [dataset]. https://cps.ipums.org/cps/index.shtml. 10.18128/D030.V7.0

Glaeser, E.L., Hanushek, E.A., Quigley, J.M., 2004. Opportunities, race, and urban location: the influence of John Kain. J. Urban Econ. 56 (1), 70–79.

Godøy, A., Reich, M., Wursten, J., Allegretto, S., 2021. Parental labor supply: evidence from minimum wage changes. J. Hum. Resour. Forthcoming. 1119-10540R2

Goodman-Bacon, A., 2021. Difference-in-differences with variation in treatment timing. J. Econom. 225 (2), 254–277. doi:10.1016/j.jeconom.2021.03.014. Themed Issue: Treatment Effect 1. https://www.sciencedirect.com/science/article/pii/S0304407621001445

Hainmueller, J., Xu, Y., 2013. Ebalance: a stata package for entropy balancing. J. Stat. Softw. 54 (7), 1–18. doi:10.18637/jss.v054.i07.

Holzer, H.J., Ihlanfeldt, K.R., Sjoquist, D.L., 1994. Work, search, and travel among white and black youth. J. Urban Econ. 35 (3), 320–345.

Ihlanfeldt, K.R., 1992. Intraurban wage gradients: evidence by race, gender, occupational class, and sector. J. Urban Econ. 32 (1), 70–91.

Ihlanfeldt, K.R., Sjoquist, D.L., 1998. The spatial mismatch hypothesis: a review of recent studies and their implications for welfare reform. Hous. Policy Debate 9 (4), 849–892.

Johnson, R.C., 2006. Landing a job in urban space: the extent and effects of spatial mismatch. Reg. Sci. Urban Econ. 36 (3), 331–372.

Kline, P.M., Rose, E.K., Walters, C.R., 2021. Systemic Discrimination Among Large U.S. Employers. Working Paper. National Bureau of Economic Research doi:10.3386/w29053. http://www.nber.org/papers/w29053

Kuka, E., Stuart, B.A., 2021. Racial Inequality in Unemployment Insurance Receipt and Take-Up. Working Paper. National Bureau of Economic Research doi:10.3386/w29595. http://www.nber.org/papers/w29595

Miller, C., 2018. When Work Moves: Job Suburbanization and Black Employment. Working Paper. National Bureau of Economic Research doi:10.3386/w24728. http://www.nber.org/papers/w24728

Ong, P.M., 2002. Car ownership and welfare-to-work. J. Policy Anal. Manag. 21 (2), 239–252.

Parker, K., Menasce, J., Morin, R., Lopez, M., 2015. Chapter 7: The Many Dimensions of Hispanic Racial Identity. Pew Research Center.

Parks, V., 2016. Rosa parks redux: racial mobility projects on the journey to work. Ann. Am. Assoc. Geogr. 106 (2), 292–299.

Raphael, S., Riker, D.A., 1999. Geographic mobility, race, and wage differentials. J. Urban Econ. 45 (1), 17–46.

Raphael, S., Stoll, M.A., 2001. Can boosting minority car-ownership rates narrow inter-racial employment gaps? In: Brookings-Wharton Papers on Urban Affairs, pp. 99–145.

Reardon, S.F., 2016. School segregation and racial academic achievement gaps. RSF: Russell Sage Found. J. Soc. Sci. 2 (5), 34–57.

Schuetz, J., Larrimore, J., Merry, E.A., Robles, B.J., Tranfaglia, A., Gonzalez, A., 2018. Are central cities poor and non-white? J. Hous. Econ. 40, 83–94. doi:10.1016/j.jhe.2017.11.001. Race and the City. https://www.sciencedirect.com/science/article/pii/S1051137717300347

Stoll, M.A., Covington, K., 2012. Explaining racial/ethnic gaps in spatial mismatch in the US: the primacy of racial segregation. Urban Stud. 49 (11), 2501–2521.

Stoll, M.A., Raphael, S., 2000. Racial differences in spatial job search patterns: exploring the causes and consequences. Econ. Geogr. 76 (3), 201–223.

Urban Institute, 2020. The unequal commute, examining inequities in four metro areas' transportation systems. https://web.archive.org/web/20211203162120/, https://www.urban.org/features/unequal-commute. Accessed: 2022-01-11.

Vaghul, K., Zipperer, B., 2019. Historical State and Sub-State Minimum Wage Data. Washington Center for Equitable Growth. https://github.com/benzipperer/historicalminwage

Wursten, J., 2020. Estimating the Earnings and Employment Effects of the Minimum Wage Through Differences in Exposure Across US Counties. Working Papers Department of Managerial Economics, Strategy and Innovation (MSI). KU Leuven, Faculty of Economics and Business, Department of Managerial Economics, Strategy and Innovation (MSI).